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Minimum Wages and Race Disparities*

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Abstract

We provide a comprehensive analysis of the effects of minimum wages on blacks, and on the relative impacts on blacks vs. whites. We study not only teenagers – the focus of much of the minimum wage-employment literature – but also broader low-skill groups. We find no evidence that higher minimum wages reduce wage disparities. First, much of our evidence points to job loss effects from higher minimum wages are more evident for blacks – and more so for black men – but not detectable for whites. Second, in many of our analyses the effects of minimum wages are large enough to generate adverse effects on earnings (and relative earnings) of blacks. Third, given strong residential segregation by race in the United States, more adverse effects of minimum wages on blacks would fall on areas with a high black population share, and this appears to be accentuated by evidence that minimum wage effects are more adverse in areas where blacks are a very high share of the population, regardless of individual race.

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I. Introduction

The literature on the employment effects of minimum wages is vast. For example, a recent survey of U.S. evidence only was limited to studies published (or forthcoming) between 1992 and 2020, and imposed other criteria for inclusion, and still ended up covering 70 papers (Neumark and Shirley, 2022).[1] This literature amply documents that the strongest evidence of disemployment effects, and the larger effects, appear for the lowest-skilled groups – usually defined in terms of either age or education (see, e.g., Neumark and Shirley, 2022). Presumably the reason is that the minimum wage is more binding for these groups, and hence a larger share of workers among them ends up with marginal revenue product below the minimum wage, even after reallocation of inputs and other changes in firm operations that impact the productivity of labor or otherwise offset the higher cost of the minimum wage.[2] Yet this literature pays scant attention to the differential effects of minimum wages on employment of lower-skilled minority workers, even though minority groups also earn lower wages (as emphasized by Deere et al., 1995).[3]

Whether the lower wages among minorities reflect actual lower skills, or "discounting" of minority workers' productivity à la Becker (1957), minimum wages should be more binding for minorities. Thus, the competitive model of the labor market should predict more adverse minimum wage effects on minorities.[4] With regard to minimum wage effects on blacks, Milton Friedman put this most succinctly and provocatively in a 1966 op-ed in *Newsweek*: "I am convinced that the minimum-wage law is the most anti-Negro law on our statute books."[5] Another hypothesis is that even if skills and wages are similar for blacks and whites, employers choose to reduce employment among blacks more than among whites – behavior that could also be interpreted as discrimination if skill differences do not motivate this response.

In contrast, advocates for higher minimum wages claim that they are a critical tool for closing gaps between blacks and whites (Derenoncourt et al., 2020). This argument focuses on wages, which ignores the

[1] An earlier extensive survey of the minimum wage-employment literature emphasized (but did not limit to) U.S. studies, and covered over 100 papers (Neumark and Wascher, 2007).
[2] Manning (2021) and Schmitt (2015) discuss many of the other margins of adjustment to a higher minimum wage (although motivating their discussions of other margins based on inaccurate summaries of the research on employment effects as failing to detect job loss).
[3] See Section II for a discussion of the related literature.
[4] Some recent research puts forward evidence of monopsony-like power in labor markets (e.g., Azar et al., 2022; Rinz, 2022), and a couple of papers argue that this framework applies to low-wage labor markets and hence minimum wage effects (Azar et al., forthcoming; Corella, 2020). This paper is not the place to adjudicate this evidence. However, we would suggest caution in adopting this view. First, the literature on how labor market power might mediate minimum wage effects on employment is in its infancy, and there is debate over whether concentration measures capture employer labor market power (Yeh et al., 2022). Second, most evidence is in fact consistent with the competitive model (Neumark and Shirley, 2022), so even if labor market power reduces or eliminates the adverse employment effects of minimum wages in some markets, this does not happen broadly, and minimum wages would still be more binding for minority workers.
[5] He also referenced the adverse effects of minimum wages on teenagers, referring to the lower skills of both teenagers and blacks. However, as we have pointed out, the same prediction would apply if blacks do not have lower skills, but their productivity is discounted as in the employer discrimination model. Myrdal (1944) also warned of the potential for more adverse employment effects of minimum wages on blacks.

potential job loss that, as argued above, could be worse for blacks. The research underlying this argument, based on 1960s expansions of the minimum wage (Derenoncourt and Montialoux, 2021), reports that wages for blacks were increased relative to wages for whites, without an accompanying decline in employment for blacks. On the other hand, studying the same 1960s expansions, Bailey et al. (2021) find similar earnings effects but offsetting disemployment effects that were larger for black men (compared to the overall modest effects).[6] Regardless, the employment effects debated in these two papers are from many decades back.

Given the strong possibility of more adverse employment effects for blacks, and the dearth of evidence, in this paper we provide a comprehensive analysis of the effects of minimum wages on blacks, and on the relative impacts on blacks vs. whites. We study not only teenagers – the focus of much of the minimum wage-employment literature – but also broader low-skill groups. We focus on employment, which has been the prime concern with the minimum wage research literature. Moreover, employment effects are of first-order importance, as constraints on employment from a high minimum wage can potentially have both short-term adverse effects on earnings and longer-term adverse effects on human capital accumulation.[7] However, we also report evidence on earnings, as well as hours and own-wage elasticities, to present a comprehensive picture. We also subject our analysis to a wide battery of tests and alternative approaches suggested by past research on the employment effects of minimum wages and recent econometric work on difference-in-differences types of estimators.

We find a good deal of evidence that job loss effects from higher minimum wages are much more evident for blacks – especially black men – and in contrast not very detectable for whites. We also estimate impacts of the minimum wage on estimated wages, as well as on earnings. The evidence from these analyses further reinforces the conclusion that there tend to be adverse effects of minimum wages on blacks, and more so on black men.

Given extreme residential segregation by race in the United States, the race difference in the effects of minimum wages imply that much of the adverse impact of the minimum wage falls on areas with a high black population share. We explore additional evidence on whether minimum wage effects are also more adverse in black areas, regardless of individual race. We find evidence of this heterogeneity, which accentuates the concentration of the adverse effects of minimum wages in areas where blacks are a very high share of the population.

II. Related Literature

There are scattered exceptions to the general inattention to race in the minimum wage literature – by

[6] Bailey et al. suggest that the lack of employment impact in Derenoncourt and Montialoux depends on excluding from the model state-by-birth cohort effects and a GSP control, and using a reference week rather than annual employment measure (Table 2 and Appendix).

[7] Neumark and Nizalova (2007) find adverse effects of exposure to a higher minimum wage when young on later wages, employment, hours, and earnings. These effects appear to be stronger for blacks.

our count, five U.S. studies, only three of which have race differences as their focus, and none of which compellingly contrast the evidence for lower-skilled minorities and non-minorities. Neumark and Wascher (2011) report separate estimates of the effects of minimum wages, the EITC, and their interaction on less-educated Black or Hispanic men, but their focus is not on employment effects per se. Cengiz et al. (2019) briefly explore employment effects via their "binned wage/missing jobs" approach, and for blacks and Hispanics combined find a very small and insignificant disemployment effect, although they do not restrict attention to lower-skilled blacks or Hispanics.

Of the papers more directly focused on race differences in employment effects, Deere et al. (1995) study the effects of federal minimum wage increases in 1990 and 1991, identifying employment effects by comparing changes in employment for low- vs. high-wage groups. They report a higher fraction of low-wage workers among blacks than whites or Asians, for both women and men, and larger employment declines for black women and men. In regressions for teenagers and high school dropouts adjusting for cyclical changes, they report estimates for blacks but not other races, thus not providing comparisons for lower-skilled blacks vs. other races. Wursten and Reich (2023) use the Cengiz et al. binned data approach with Current Population Survey (CPS) data, finding no employment effect for blacks but reductions for Hispanics, although they too do not restrict to lower-skilled minorities. The discussion paper by Blau et al. (2025) uses CPS data and estimates employment (and earnings) effects by race (and ethnicity) for wage bins, using a predicted wage for those not working. Their results contrast with ours. Blau et al. do not find evidence of negative earnings or employment effects for lower-wage/predicted wage blacks, although their estimates are far less precise than ours, with confidence intervals for estimated employment effects for blacks at the lower-skill end of the wage distribution roughly 5-10 times larger than ours. The differences may be attributable to smaller sample sizes available with CPS data: they note that requiring at least 50 state-by-year observations on wages leaves only 26 states, and the cells for wage bins will of course be much smaller. In addition, they do not condition directly on skill measures, or disaggregate by gender (and we find stronger adverse results for black males).

The conflicting results and wide confidence intervals in existing research, the absence of research on race differences in minimum wage effects by gender despite evidence that black men may face particular challenges in the labor market, and the general dearth of evidence on race differences in the effects of minimum wages, imply that we have a lot more to learn about race and minimum wages. And in our view, it is clear that the American Community Survey data we use is invaluable in this effort, by providing the statistical power necessary to isolate effects for specific sub-groups by race, skill and gender. Moreover, we explore issues and topics that the existing research has not considered, including: a richer set of outcomes; econometrics issues – especially those arising out of the broader literature on minimum wages and employment; and spatial evidence and implications regarding race disparities in minimum wage effects.

III. Data

We use American Community Survey (ACS) data from 2005-2019. To keep the race comparisons straightforward, we focus only on blacks and non-Hispanic whites and study those aged 16-65.[8] The ACS data are invaluable in studying the effects of minimum wages on low-skilled blacks because other data sets used to study the employment effects of minimum wages generally – mainly the CPS – are likely to have very low observation counts for these groups in many states.[9]

The smallest unit of disaggregation available in the publicly available ACS micro data is the Public Use Microdata Area (PUMA). Per the Census Bureau's definition, PUMA boundaries are defined using three main criteria: 1) each PUMA must have a population of 100,000 or more at the time of delineation, and this population threshold must be maintained throughout the decade; 2) PUMAs are formed only by aggregating whole census tracts or counties and must not cross state boundaries; and 3) the building blocks for PUMAs must be contiguous or share a common border.[10] The Census Bureau updates PUMA boundaries every 10 years based on new population data from the Decennial Census. The 2012 ACS data files were the first to include PUMAs defined using the 2010 Census data. ACS data files from 2005-2011, which we also include in our analysis, use PUMAs defined after the 2000 Census. There are 2,071 PUMAs in our sample from 2005-11, and 2,351 PUMAs from 2012-19 (after the 2010 boundary changes). To define PUMAs over our entire sample period, we used the 2000-2010 PUMA crosswalk file, which identifies PUMAs that overlap across census years.[11] We treated two PUMAs as the same if their land areas overlapped by at least 99% between the two census years. Based on this criterion, 686 of the 2,071 original PUMAs remained the same between the 2000 and 2010 Censuses, while the others were redefined. We assigned new PUMA codes to the latter after 2012, and kept the original codes for the former. This leads to a total of 3,736 unique PUMAs across our full sample period, including 686 that remained the same, 1,385 that are identified through 2011, and 1,665 beginning in 2012.

We use state, city, and county level minimum wages for the years in our sample. We map these minimum wages to PUMAs for our PUMA-level analysis. To do so, we map cities within the boundaries of

[8] For our wage analysis, we additionally drop unpaid family workers (0.28%) and the self-employed (8.4%). The ACS oversamples units in areas with smaller populations (https://usa.ipums.org/usa/chapter2/chapter2.shtml#ACS). All individual-level data and estimates (in all tables and figures) are weighted by ACS person weights.

[9] For example, Wursten and Reich (2023) note that for their binned estimator more than 50% of cells for blacks are empty. They also note that this likely leads to attenuation towards zero in estimated effects for blacks, which could partly account for their findings. It is not feasible to try to replicate the binned data approach using ACS data, because wages have to be estimated from annual wage and salary information, as discussed in detail below.

It is possible that the small samples of blacks available in some states in the CPS have deterred a focus on race differences. And other datasets prominent in the minimum wage literature, like the QCEW and CPB, do not distinguish workers by race. The QWI does, however, and could potentially provide further evidence, although the QWI does not disaggregate by race and skill (age/education); see https://lehd.ces.census.gov/doc/QWI_101.pdf.

[10] Certain exceptions to these rules and further guidelines for creating PUMAs can be found here: https://www.census.gov/content/dam/Census/library/publications/2020/acs/acs_pums_handbook_2020_ch02.pdf.

[11] See: https://usa.ipums.org/usa/volii/pumas10.shtml.

each PUMA and assign the highest binding annual average minimum wage within a PUMA's boundaries as the PUMA's minimum wage. The average was generated based on the number of months a sub-PUMA jurisdiction spent at each minimum wage level.

Figure 1 displays information on the minimum wages assigned to PUMAs. The top panel shows levels and the bottom panel changes. The figure shows box-and-whisker plots for each year. The rectangle extends from the 25th to the 75th percentile, the upper horizontal lines show the 75th percentile plus 1.5 times the interquartile range, and the lower horizontal lines show the 25th percentile minus this value.[12] The top panel also shows the federal minimum wage values. These figures clearly illustrate the extent to which states and localities have advanced minimum wages well beyond the federal level in recent years.[13]

Although wages are not central to our analysis, we are interested in estimating wages, to assess the extent to which the bindingness of the minimum wage may vary between blacks and whites. The ACS does not report hourly wages, so they have to be estimated from information on annual wage and salary income and total hours worked. We drop those reporting zero hours. However, these are either unemployed or not in the labor force the entire year. Weeks worked last year is a categorical variable with ranges 1-13, 14-26, 27-39, 40-47, 48-49 and 50-52 weeks. We use the midpoints of these ranges. Hours are reported as usual hours worked per week, reported as 1-99, and top-coded at 99. We thus estimate hourly wages as (wage and salary income/{weeks x usual weekly hours}). This approach generates a handful of extreme outliers, with some maximum values in the tens of thousands of dollars, as well as some very low values.[14]

We first inflate all income and wage data to 2019 dollar values using the Consumer Price Index.[15] Next, we identify two types of wage outliers. At the low end are those reporting zero annual income (308 out of more than 16 million). Even if they are over-reporting hours, such as by adding an extra zero, their estimated wage would still be zero, so we do not try to correct these (they will be eventually dropped based on truncation rules discussed shortly). There are also some very high values; for example, the 99th percentile is $151. In many cases, these are associated with high annual incomes. For example, of those with hourly wages above the 99th percentile, 68.5% have annual wage and salary income above $297,000 – the 99th percentile of population wage and salary income distribution. When estimated hourly wages are high and reported wage and salary income is high, there is no obvious problem. These people generally work 40-60

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[12] The latter is often not shown because of bunching attributable to the federal minimum wage. Note that there are a few minimum wage declines owing to deflation for an indexed minimum wage (Colorado in 2010, https://staffscapes.com/colorado-s-minimum-wage-rate-for-2010/), legislation (Iowa, in 2017, https://www.johnsoncountyiowa.gov/wage), and a court ruling (Kentucky, in 2016, https://nkytribune.com/2016/10/kentucky-supreme-court-invalidates-minimum-wage-measures-passed-by-lexington-louisville/) overturning local minimum wages. Note also that some PUMA definitions change in 2012, in which case the first percent change can be in 2013.

[13] Most of the variation is at the state level rather than the local level, so the state versions of these figures look similar.

[14] There were 0.12% of observations with estimated wages $<$ \$1, 0.02% with wages $>$ \$1,000, and 0.0004% with wages $>$ \$10,000.)

[15] The source for this is https://fred.stlouisfed.org/series/CPIAUCSL#0.

hours per week (Figure 2, Panel A). In other cases, though, those with wages above the 99th percentile and income below the 99th percentile have low reported/estimated hours per week; they have much more hours mass below 20 hours per week and even below 5 hours per week (Figure 2, Panel B). And this is even more apparent if we restrict income to a lower value, like income below the 90th percentile while wages are still above the 99th percentile (Figure 2, Panel C). Thus, it seems likely that in many of these cases hours are reported or coded with a missing zero after the first digit. We thus added a zero to hours when hours were reported as a single digit and wages were above the 99th percentile. After doing this, we restrict wages to between ½ of the prevailing federal tipped minimum wage[16] and $130, in 2019 dollars. With these changes and restrictions imposed, the distribution of estimated hourly wages looks well-behaved (Figure 3).[17]

The main outcome on which we focus is employment in the reference week. We also present results for any employment in the past year (in appendix tables); the results are qualitatively similar. In addition, we present some analyses for wages, annual earnings, and annual hours.

IV. Descriptive Evidence

There are race differences in skills that would make minimum wages more binding for blacks. As shown in Figures 4A and 4B, blacks are younger and less educated. However, our constructed/estimated hourly wage data indicate lower wages for blacks conditional on age and education. As an example, Figure 5A shows these hourly wage differences by year for black and white males with at most a high school degree, who are younger than 30 years of age. If we condition on working full-time (40 hours a week) and full-year (50-52 weeks a year), the gap is somewhat larger (Figure 5B). In contrast, however, hourly wages for black teens are close to or slightly higher than for white teens (Figure 5C).[18]

In our analysis, we estimate employment regressions for subsets of the population distinguished by education and age (as well as by race and gender). Wage differences by race within these groups could reflect unobserved skill differences (stemming, for example, from lower school quality for blacks) or discrimination, but either way they might predict stronger disemployment effects for blacks when minimum wages are more binding.

We next examine evidence on whether minimum wages are more binding for blacks. Figure 6A shows that, for all workers, the spike in the wage distribution at the minimum wages is much more pronounced for blacks. This is sometimes the case, although less markedly so, for subgroups defined by

[16] The source for this is https://www.dol.gov/agencies/whd/state/minimum-wage/tipped/History.

[17] Nonetheless, as this discussion suggests, using the ACS data to estimate employment effects of minimum wages using a "wage bin" or a "fraction affected" approach would be ill-advised.

[18] Teenagers may be a quite heterogeneous group, ranging from high school dropouts to those who will eventually have very high education (for example, 59% of teenagers do not yet have a high school degree), and part-time as well as full-time workers. Moreover, these characteristics may differ by race. Thus, it is perhaps not surprising that the black wage shortfall we generally see does not appear for teenagers. We cannot observe future education in these data. However, if we condition on full-year, full-time workers there is somewhat more of an indication that wages are higher for white teenagers (Appendix Figure A1).

education, age, and gender, suggesting that the evidence in Figure 6A is not fully attributable to measurable differences between blacks and whites along these dimensions, although these differences clearly matter. For example, Figure 6B, for males with a high school degree or less, shows quite clearly a larger spike and more mass near the minimum for blacks than for whites. But in Figure 6C, for male high school dropouts only, the differences are much less pronounced – presumably because a larger share of blacks than whites, in Figure 6B, are high school dropouts. Similarly, Figure 6D is for high school dropouts under age 30; again, the race differences are less pronounced than in Figure 6B, because the age restriction accounts partly "controls" for blacks being younger. Consistent with Figure 5C, the distributions are not notably different for teens (Figure 6E). This descriptive evidence suggests that race differences in employment effects of minimum wages could be more pronounced when we condition on low education and relatively young age, but not necessarily teenagers. Even more clear from these figures, though, is the motivation for looking at less-educated and younger workers rather than all workers when studying the employment effects of minimum wages, because the minimum wage is binding for larger shares of these groups.

A potential concern with this bindingness analysis is that, unlike the CPS, the ACS does not directly ask about hourly earnings; instead, we construct an hourly wage from annual earnings, midpoints of weeks worked categories, and usual weekly hours, which could introduce measurement error that attenuates the apparent spike at the minimum wage and calls into question the wage-bin comparisons above. To assess whether this affects our conclusions, we replicate Figure 6 using CPS Outgoing Rotation Group (ORG) data, applying sample restrictions that mirror those used for the ACS: we again focus on blacks and non-Hispanic whites aged 16-65, drop those reporting zero hours, and exclude unpaid family workers and the self-employed. Because the CPS ORG identifies location only at the state level, we further restrict the sample to the 39 states with no sub-state minimum wage variation over our sample period, so that the applicable minimum wage is unambiguous. Within this sample we construct two separate wage measures: (1) the CPS ORG's directly reported hourly wage (available for workers paid hourly), and (2) an hourly wage constructed from usual weekly earnings and usual hours worked per week, analogous to our ACS construction. Reassuringly, the bindingness patterns evident in Figure 6 are replicated using both CPS ORG wage measures (Figure 7 and Figure 8); in each case, the spike in the wage distribution at the minimum wage is again markedly larger for blacks than for whites, and the same qualitative patterns across education, age, and gender subgroups documented above emerge. That the directly measured hourly wage and the constructed wage yield the same story, and that this story matches the ACS-based evidence in Figure 6, indicates that our ACS wage construction is not driving the racial differences in bindingness we document, and validates using the ACS's much larger sample sizes for the wage analysis that follows.

V. Employment Results

A. Baseline minimum wage-employment regressions

We first estimate some standard minimum wage-employment regressions, focusing on evidence of differential effects of minimum wages for different groups of workers, without yet distinguishing effects by race. We focus on those with a high school education or less, and under different age thresholds, because minimum wage effects for these groups can, on the one hand, do the most to boost incomes, but on the other hand can also have the most adverse labor demand effects. We also study combinations of low education/young age criteria, and in each case include estimates by gender as well. Our analysis is at the PUMA-level, wherein each observation is a PUMA cell defined by year and by the variables on which we condition the sample for our different analyses (race, age, education, and gender).

The initial PUMA-level regressions are of the form:

$$Y_{pt} = \alpha + \beta \cdot \ln(MW)_{pt} + \gamma_B \cdot Black_{pt} + X_{pt}\delta + D_p \cdot \lambda + D_t \cdot \tau + \varepsilon_{pt} \qquad (1)$$

Y is the mean employment rate. MW is the highest minimum wage prevailing in a PUMA. Black is a dummy variable equal to one for PUMA cell means for blacks and zero for PUMA cell means for whites. X is a vector of controls, including the proportion of males, average number of children, average age, and the proportion of people in each category of marital status and education, in each PUMA cell.[19] D_p and D_t are PUMA and year fixed effects. This type of regression is standard in the minimum wage-employment literature. Observations are at the PUMA (p) and year (t) level.

The results are reported in Table 1, for a large number of low-skilled groups. Note that we have not included any cyclical control. Some minimum wage studies include an unemployment rate – sometimes calculated for a more-educated and/or older group assumed to be unaffected by the minimum wage. However, given that we are estimating minimum wage effects for a number of age and education groups beyond the common focus on teenagers, it seemed inappropriate to assume we know which group's unemployment rate is unaffected by the minimum wage and hence a valid control. This issue is, a priori, less of a concern for our primary question of interest – differences in the effects of minimum wages on blacks vs. whites, although the business cycle may have different effects by race (e.g., Forsythe and Wu, 2021). Nonetheless, we estimated equation (1) for the same less-skilled subsamples we study in Table 1, but adding as a cyclical control the unemployment rate of prime-age, male, college-educated workers, and the results were not sensitive.

Turning to teens, the estimated effect of minimum wages on teen employment is negative but not

[19] Marital status has the following categories: married (spouse present), married (spouse absent), separated, divorced, widowed, and never married/single. Education has the following categories: no schooling, nursery school through grade 4, grade 5-8, grades 9-12 (without completion certificate), 12th grade completed (GED or regular high school diploma), some college experience, bachelor's degree, and master's degree or above. Ethnicity is not added as a control as it has little variation; only 2.6% of blacks report Hispanic ethnicity while the remaining 97.4% are non-Hispanics. For our analysis, we are only considering blacks and non-Hispanic whites, as noted earlier. All PUMA-level estimates (in all tables and figures) are weighted by the corresponding PUMA population in each year, constructed from the individual data.

significant, with an elasticity of −0.087. Broken out by gender, the elasticity is about twice as large for male teens (−0.112 vs. −0.065), and significant (at the 10% level) only for them. The remaining rows move away from the usual focus on teenagers, with the model estimated for those with less education (high school at most, or less than high school), younger ages (less than 25 or less than 30), and gender, and then the combinations of these.[20] None of the estimated minimum wage effects are significant at the 10% level. However, almost all of the estimates are negative: for teens (overall and by sex); for those under age 25 (overall and by sex); for high school dropouts (overall and by sex); and for all combinations of younger age and lower education (overall and by sex).

B. Differences in employment effects by race

We next turn to our primary analysis – estimation of differences in minimum wage-employment effects by race. We estimate equation (1) separately for each race:

$$Y_{pt} = \alpha + \beta\cdot\ln(MW)_{pt} + X_{pt}\delta + D_p\cdot\lambda + D_t\cdot\tau + \varepsilon_{pt} \qquad (2)$$

This is equivalent to estimating a fully interactive model with Black, where "Black" is a dummy variable for PUMA cell means for blacks. We also estimate this interactive model so that we can easily calculate the significance of the race differences in estimated employment effects. We can also view the interactive model as a triple-difference model, in which case one might interpret the effects for whites as reflecting both minimum wage effects and common shocks to whites and blacks, and the relative effects for blacks as the causal effects.

The race differences in estimated minimum wage effects, reported in Table 2, are striking. The estimated employment effects for whites are never statistically significant and they are small, although they are negative in most cases. However, the estimated employment effects for blacks are negative for *every* low-skill group we consider with only one exception, and considerably larger (in absolute value). Moreover, the estimated differences and the overall effects for blacks are statistically significant at the 5% or 10% levels for many groups. For the overall effects for blacks ("Black empl. effect"), these include: teens (all, male, and female); high school dropouts (all and male); under 25, high school dropouts (all, male, and female); under 25, with at most a high school education (all and male); under 30, high school dropouts (all, male, and female); and under 30, with at most a high school education (male). In addition, about as many estimates (and typically the same estimates) are significantly different between blacks and whites.[21] In

[20] Note that these low-skill groups have some overlap – e.g., there are many teenagers in the groups defined based on age below 25 or 30 and education less than high school or at most a high school degree. (For example, 78% of those with less than a high school degree and under 30 are teens, but on the other hand 41% of teens have a high school degree or more education.) Our goal was to define low-skill groups based on age, based on education, and based on both (the strictest definitions), rather than to study small mutually exclusive groups.

[21] The "daggers" in the second column report the statistical significance of the differences between the estimates for blacks and whites.

general, when we consider low education (high school dropouts), or combinations of low education (up to at most a high school education) and being young, there is clear evidence of adverse effects of minimum wages on black employment. Moreover, the adverse effects of minimum wages on blacks are larger and more strongly significant for black males (the estimate is also larger for black male teens compared with black female teens).[22]

In addition, when we look at elasticities, the race differences are even more pronounced, because for every group we consider, the employment rate is lower for blacks. As an example, looking at those with at most a high school education and under 30, the estimated minimum wage coefficient for whites is −0.001, vs. −0.044 for blacks. But because the employment rate is 0.369 for blacks and 0.500 for whites, the elasticity difference is accentuated (−0.002 for whites vs. −0.119 for blacks). In addition, there are some cases of quite large elasticities for low-skilled blacks: e.g., −0.337 for black male teens; −0.473 for black male high school dropouts under 30; and −0.585 for black male high school dropouts under 25. These are much larger disemployment elasticities than are typical of most of the research literature (Neumark and Shirley, 2022).[23,24]

C. Effects on hours

Although the focus of research on labor demand effects of minimum wages has been on employment, hours can also adjust both because of the employment changes, and because of intensive margin changes. Some recent work shows hours can be an important adjustment margin independent of employment effects – see, e.g., Borgschulte et al. (2026), who find that minimum wage increases reduce hours for immigrant workers with no corresponding employment loss. Table 3 reports estimates for annual hours. These provide even stronger evidence than the employment effects, with negative and significant estimates for blacks in the majority of cases, and many elasticities exceeding −0.2 or −0.3, and also a bit more evidence of significant negative effects for black females (e.g., less than high school, and under 25 and less than or equal to high school). Going forward, however, we focus on employment effects.

D. Potential econometric biases

[22] In Appendix Table A1 we report regressions for the same groups shown in Table 2, but excluding teenagers (and hence the first three rows of Table 2 do not appear). One motivation for this is that being low education for a teenager is not necessarily the same indicator of low skill as it is for non-teenagers, because teenagers can still be in school. We continue to find larger negative effects for blacks than for whites. Although the estimates for blacks are generally not significantly different from zero, there are quite a few cases – especially for males – where the race differences are significant. We can interpret this a couple of ways. First, viewed as triple-difference estimates, the relative effects for blacks are the causal effects. Second, if we view the direct evidence for blacks when teens are excluded as somewhat weaker, then of course the conclusion would be that black teenagers to some extent drive the stronger disemployment effects for blacks.

[23] Table 2 also reports the share of whites and blacks in each of the skill groups defined by age and education. Consistent with the descriptive information reported earlier (Figure 4), there is a larger proportion of blacks than whites in each of these lower-skill groups.

[24] Appendix Table A2 reports estimates paralleling Table 2, but defining employment as any weeks worked in the past year. These are qualitatively similar to the estimates in Table 2, although the elasticities are generally a bit smaller.

Recent econometric work has highlighted potential biases in panel data estimates when there are pre-trends or heterogeneous (dynamic) treatment effects (e.g., Callaway et al., 2024; Wooldridge, 2021). On a priori grounds, we are less concerned about these biases in this paper, because we are most interested in differences between the effects of minimum wages on blacks and whites. These comparisons likely net out any common shocks/changes for the low-skill groups we study. Still, the overall effects for each group are also clearly of interest. As noted in regard to Table 2, the adverse effects of minimum wages for blacks are stronger when we focus on lower-skilled groups, such as both young and less-educated. The alignment of these differences with how we would predict minimum wage effects to vary with skill makes it less likely they are spurious.[25] However, we do additional analyses that rule out meaningful biases.

In particular, we implement the two-stage difference-in-differences (2S-DID) method of Gardner et al. (2024). Unlike some recent methods developed to address potential biases in two-way fixed effects estimates, this method can be implemented when there is continuous treatment (as well as other complications). The basic idea is to estimate area and period fixed effects for any untreated observations available (which can be a subset of observations for treated observations, and excludes always treated observations). Under a parallel trends assumption, these are unbiased for the full sample. One can then residualize, and estimate "2nd-stage" regressions on residuals, leading to unbiased estimates (with the correct standard errors recovered from GMM).[26]

The 2S-DID method cannot be applied to the full 2005-19 sample period. Federal minimum wage increases of 2007-09 effectively imply that untreated observations in the first stage are largely confined to 2005-09, leaving insufficient years to identify period fixed effects for subsequent years. Consequently, second-stage estimates would draw almost exclusively on Great Recession variation, which is uninformative about minimum wage employment effects more generally. We therefore restrict the 2S-DID analysis to the 2012-19 period, where the availability of never-treated PUMAs and PUMAs treated after the first year of the sample provides adequate first-stage identification across all periods. We start this analysis in 2012, rather than 2010 or 2011, to put a few years between the last federal minimum wage increase and the end of peak labor market effects of the Great Recession (the unemployment rate peaked in 2009), and the start of the period we consider. Moreover, new PUMA definitions were established in 2012.[27] We first estimate the models from Table 2 for this sub-period and show that the results are very similar.[28] These results are

[25] In addition, prior research (Cengiz et al., 2019; Dube et al., 2024) has argued that the shocks to employment in the 1980s and 1990s that predated later minimum wage increases are what leads to bias in estimated employment effects of minimum wages, and Dube et al. (2024) argue that starting analyses in 2000 avoids this problem. This question is not settled, but regardless, our data start in 2005, so if one were concerned about contamination from these earlier shocks, it would not raise questions about our findings.
[26] Gardner et al. also show that this estimator performs better than other estimators when used on simulated data where the assumptions that make two-way fixed effects unbiased are not violated.
[27] There are 1,000 never-treated PUMAs and 1,351 ever-treated PUMAs in this sub-period.
[28] For completeness, Appendix Table A3 reports estimates for the earlier period, 2005-2011. The estimated employment

reported in the first two columns of Table 4.

The 2S-DID estimates are reported in the third and fourth columns of Table 4, for the same 2012-19 period. The results and conclusions are generally similar. The standard errors are a bit larger, as we would expect. But we still obtain a consistent pattern of negative estimated employment effects for blacks for every age and education group, with a number of them statistically significant – in particular for young, less educated, black males – although overall the estimates are a shade smaller in absolute value and some of the estimates become insignificant.[29] The similarity of the estimates is depicted in Figure 9, which plots the 2S-DID estimates against the two-way fixed effects estimates, by race, for each gender (including females and males together), age, and education group for which estimates are reported in Table 4. The two types of estimates for both whites and blacks correspond closely, with correlations of 0.92 and 0.94 respectively. The figure also indicates that although the estimates for blacks become slightly less negative, the estimates for whites similarly increase so that the differences between the estimates for blacks and whites are similar.

E. Sensitivity of findings to methods used in critiques of research finding negative employment effects

The Introduction cites an extensive survey of research on the employment effects of minimum wages in the United States that shows that most studies find negative employment effects, and that these are typically stronger in studies focusing on lower-skilled workers. However, studies are not unanimous, and there are some prominent studies challenging the conclusion that higher minimum wages in the United States reduce employment of lower-skilled workers. This naturally raises the question of whether our findings of much sharper disemployment effects for blacks are robust to the methods used in these studies that challenge the conclusions in the broader literature.

A core argument in these "revisionist" studies is contesting the commonly-used two-way fixed effects panel data approach, arguing that it leads to bias towards finding adverse employment effects because of "spatial heterogeneity" – correlations between unobserved negative shocks to low-skilled employment

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effects are again generally larger (negative) for blacks, although the statistical evidence is weaker. Thus, one could view the evidence of more adverse employment effects for minimum wages for blacks as stemming more from the later variation in the data, which is all state (and local) variation, whereas much of the variation in the earlier period is federal.

[29] Other recent papers in the new difference-in-differences literature have sometimes examined evidence on minimum wage effects, but by necessity using a discrete treatment (e.g., Callaway and Sant'Anna, 2021; Clemens and Strain, 2021, who use three dummy variables for different types of minimum wage increases – indexed, non-indexed large, and non-indexed small; and Hampton and Totty, 2023). Given that the Gardner et al. method can use continuous variation, there is no reason to discard such variation. Nonetheless, we look at event-study evidence, treating the first minimum wage increase in our sample period as the treatment. There was no evidence of violation of parallel trends; pre-treatment estimates going back up to three years are close to zero and statistically insignificant (results available from authors upon request, but for examples, see Appendix Figure A2). Interestingly, as these estimates indicate, and as we further document in estimates corresponding to Table 4 for this discrete definition of minimum wage treatment (Appendix Table A4), the estimated employment effects on blacks using this discrete definition are not distinguishable from zero. This suggest considerable caution in drawing substantive empirical conclusions from the application of methods that require defining what is actual continuous treatment as discrete treatment – at least in the minimum wage context.

and minimum wage increases (Allegretto et al., 2009, 2011; Dube et al., 2010; Dube, 2011). While these studies by no means encompass the entire literature challenging the conclusion that minimum wages reduce low-skilled employment in the United States, they do feature prominently. More importantly, they pertain most directly to the empirical approach used in this paper.

We therefore adapt, as directly as possible, the empirical approaches used in the studies emphasizing this spatial heterogeneity argument, and apply them to our analysis of race differences in the employment effects of minimum wages. The core question is whether our findings of adverse employment effects for blacks (and not whites) are robust to the approaches taken in these studies, or instead are fragile, with the employment effects for blacks disappearing when these approaches are adapted. To be clear, the approaches taken in these studies have been contested. (See especially Neumark et al., 2014; and Jha et al., 2024.) Our purpose here is not to re-litigate any of this debate, but instead to see whether the fragility of the evidence on overall low-skilled employment effects from using the methods in the revisionist papers extends to the evidence on employment effects for blacks. Note that for this exploration of whether the negative effects are robust, we focus on the cases for which we found the clearest evidence of negative effects for blacks: male teens, male < HS < 25, male ≤ HS < 25, male < HS < 30, and male ≤ HS < 30. However, we also convey information on all the groups studied thus far.

One set of analyses that are readily adaptable to our analysis are those in Allegretto et al. (2011), who make two modifications to the panel data estimator using CPS data by state and period. The first is to introduce state-specific linear time trends to account for the possibility that minimum wage increases are associated with different prior trends by state.[30] The second is to include dummy variables for each unique Census division-by-period combination – the idea being that these account for shocks that are common to states in the same Census division, and hence the within-period and Census division minimum wage variation provides more credible identification. We are able to adopt their approach exactly to our analysis, except of course using the ACS data for the sample period our analysis covers.[31]

A second type of analysis is to use a "cross-border" research design. The most prominent minimum wage paper using this design is Dube et al. (2010), who study the effect of the minimum wage on restaurant employment in the QCEW, introducing dummy variables for each unique "pair" of counties straddling a state border; the idea, paralleling Allegretto et al. (2011), is that these account for shocks that are common to the pairs of counties in the same cross-border area.[32] However, the same researchers, in an earlier

[30] Meer and West (2016) provide an interesting analysis of issues that arise in using state-specific linear trends.
[31] While they include an unemployment rate control, we follow our earlier analysis and exclude this; however, we verified that the results are not at all sensitive to this choice (although we checked this using an unemployment rate unlikely to be affected by the minimum wage – for 25-54 year-old college-educated males – whereas they used the aggregate rate).
[32] The Allegretto et al. (2011) approach is a version of a "cross-border" research design, where the "border" defines different states in the same Census division – and hence, strictly speaking, is not a border.

unpublished paper written the same time as these other two papers argue that it is preferable to use cross-border segments of multi-state commuting zones (MSCZs) rather than cross-border counties.[33] We report results both ways, first using cross-border counties, and then using cross-border commuting zones.[34]

The estimates replicating the Allegretto et al. (2011) analyses using state-specific linear trends and Census division-by-period dummies are reported in Table 5 and Figure 10. To parallel the Allegretto et al. (2011) analysis more closely, we use the data at the state-by-year level, using state minimum wages. As the table and even more so the figure makes clear, the estimates are very robust; the only real change in Table 5 is that the standard errors increase and hence the estimated effects become less statistically significant.

The estimates for the cross-border county design are reported in Table 6 and Figure 11, and the estimates using the MSCZs are reported in Table 7 and Figure 12. As shown in the tables, in both cases the cross-border design estimates are less precise, especially for blacks, which is not surprising. But we still get a clear pattern of more negative estimates for blacks. Figures 11 and 12 indicate that the cross-border estimates follow a similar pattern to the TWFE estimates, although with less precision the estimates for blacks are more widely dispersed around the 45-degree line. The concentration of the estimated effects for blacks at more negative values is clear in both figures; while some of the estimates for females are closer to or even above zero, the concentration of estimates for black males or both sexes combined are still clustered around negative values, whereas the estimate for whites are clustered around zero. Indeed, for the cross-border MSCZ analysis, in Table 7 and Figure 12, the race difference for males sharpens, generating considerably more adverse estimates for young and less-educated blacks.

A final approach we consider and adapt to our analysis is the "clean controls" event-study design used by Cengiz et al. (2019) in their Appendix D. Rather than pooling all minimum wage changes into a single staggered panel, Cengiz et al. argue for estimating event-specific treatment effects using only "clean" control states or areas (those experiencing no non-trivial minimum wage increase within the event window) on the grounds that this design is more robust to the problems that can arise with staggered treatment timing

[33] "…while DLR uses county pairs straddling state borders we use a more economically-motivated definition of local labor markets: commuting zones" (Allegretto et al., 2009, p. 5).

[34] For the cross-border county analysis, not every PUMA in the data matches to a single county, so to keep this as clean as possible we restrict the sample to PUMAs that map uniquely to one county. These PUMAs account for 71 percent of all PUMAs (2,671 out of 3,736), corresponding to 537 counties. Of these counties, 189 lie along a state border. We map these border counties into contiguous county pairs using DLR's replication package (file: county-pair-list.txt available at https://dataverse.harvard.edu/api/access/datafile/:persistentId?persistentId=doi:10.7910/DVN/L4DUZ7/8ODET4.), which provides contiguous pair mappings for all 1,139 U.S. counties located along state borders. The regression sample is restricted to the 189 border counties, although not all resulting county pairs exhibit minimum wage variation. Of the 189 border counties, 56 border one other county, 75 border two, 40 border three, 13 border four, two border five, two border six, and one borders seven counties. For the cross-border commuting zone analysis, not every PUMA in the data matches to a single commuting zone, so to keep this as clean as possible we only use PUMAs that do. These represent 82% of PUMAs, 303 out of 741 commuting zones, and 27 out of 137 MSCZs. Appendix Figure A3 shows two maps. The first displays all commuting zones, delineating the multi-state ones. The second displays the commuting zones included in our analysis.

and heterogeneous treatment effects. Our implementation is considerably simpler than theirs, as we do not stack multiple events; instead, we focus on a single, prominent event – the 2015 round of minimum wage increases – which lets us construct a clean panel of PUMAs with three pre-treatment and four post-treatment years (2012-2019). Of the 2,351 PUMAs in our sample, 177 experienced a minimum wage increase in 2015 and constitute our treated group, while 1,000 PUMAs with no minimum wage change over the full window serve as clean controls. We then estimate a standard two-way fixed effects event-study specification using this discrete treatment and clean-control sample, controlling for the usual set of individual characteristics like sex, age, education, and marital status. As reported in Figure 13, this design shows no evidence of differential pre-trends for blacks relative to whites in the lead-up to the 2015 increases, and, consistent with our main results, points to some evidence of negative employment effects for young and less-educated blacks (but not whites) following the minimum wage increase. The estimates are, however, considerably less precise than our baseline results, with wide confidence intervals, which is unsurprising given that this design relies on a single event and a substantially reduced, non-stacked sample of treated and control areas.[35]

A core issue motivating some of these alternative analyses is that may be pre-trends in the data that are correlated with minimum wage increases. Given our findings indicating that minimum wage increases reduce employment of low-skilled blacks (overall, and relative to whites), the general concern would be the possibility that black employment was declining in the ever-treated areas relative to never-treated areas before the minimum wage increases occurred. To formally check this, we estimate a distributed-lag/lead specification, following the approach in Jha et al. (2024):

$$Y_{pt} = \alpha + \sum_{k=-K}^{L-1} \beta_k \cdot \Delta \ln(MW)_{p,t-k} + \beta_L \cdot \ln(MW)_{p,t-L} + X_{pt} \cdot \delta + D_p \cdot \lambda + D_t \cdot \tau + \varepsilon_{pt} \quad (3)$$

We set K = 2 leads and L = 2, yielding two pre-treatment leads (β_{-2}, β_{-1}), a contemporaneous term (β_0), a one-period lag (β_1), and a term capturing the cumulative effect up to two years after a minimum wage increase (β_2). We estimate the specification separately by race and also test formally for race differences by interacting each lead/lag term (along with all other regressors) with an indicator for black workers.

Table 8 reports the estimates for the same five groups where we find the clearest evidence of adverse employment effects for blacks, which we examined in Tables 5-7 as well. The second lead (β_{-2}) is small and statistically insignificant for both races in every group, giving no indication of differential pre-existing trends two years ahead of a minimum wage increase. The first lead (β_{-1}) for blacks is positive in every group, and statistically significant for male teens and for male high school dropouts under 25. Critically, none of the black leads is negative and significant: if anything, the presence of a positive pre-trend works against finding an adverse effect, suggesting our employment estimates, if biased at all, understate rather than overstate the true negative effect on black employment. Consistent with our main results, the

[35] We do not show scatter plots here because there is not a TWFE estimate corresponding to each post period.

adverse effects for blacks show up in the post-treatment period, concentrated in the cumulative lag term (β_2), which is consistently negative for every group and statistically significant for male teens and for males high school dropouts under 30. This evidence reinforces the conclusion that minimum wage increases are associated with disproportionately adverse employment effects for young, less-educated black men, and that this pattern is not an artifact of differential pre-existing trends.

F. Possible Explanations of the Stronger Employment Effects of Minimum Wages on Blacks

We have documented considerably stronger effects of minimum wages in reducing employment of black low-skilled workers than white low-skilled workers. Indeed, while we find significant negative employment effects for blacks, with quite large elasticities, we find no statistically significant effects for whites (and correspondingly the elasticities are much closer to zero, although generally negative). In this section, we explore some evidence on why. To be clear, though, the main focus of this paper is on whether minimum wage effects differ by race; more definitive evidence on why the race differences we find exist await more and different kinds of research.

One explanation for the race difference is that minimum wages could be more binding for blacks. It is true that blacks are younger and less educated than whites (Figure 4), and overall minimum wages are more binding for blacks. But our regressions condition on young ages, low education, or both, and the spikes in the wage distribution for lower education and age groups were not that much more pronounced for blacks – although they were to some extent (Figure 6). The latter – i.e., lower wages for blacks even conditional on these observable skill measures – can occur because of lower unmeasured components of skill owing to early skill gaps (e.g., Carneiro et al., 2005), school quality differences (see, e.g., the evidence and other studies reviewed in Hanushek and Rivkin, 2009), or other pre-market factors (Neal and Johnson, 1996). Wages for blacks can also be lower conditional on age and education because these variables do not capture actual labor market or job tenure (nor do other variables in the ACS). Lower employment rates and higher unemployment rates for blacks would likely imply that their actual labor market experience is lower for the same potential experience, and that job tenure is also lower. Wages can also be lower for observationally similar blacks because of discrimination that results in lower wages for blacks.[36] Neal (2006) argues that the discrimination-driven race gap in pay is larger among less-skilled workers, because low-skill labor markets are disproportionately made up of smaller employers who are less likely to have professionalized HR departments, formal hiring and pay-setting procedures, and the internal compliance structures that make firms subject to meaningful scrutiny under anti-discrimination law. In addition, Title VII of the Civil Rights Act does not apply to firms with fewer than 15 employees (although state statutes may apply).[37]

[36] In both the Becker (1957) taste discrimination model, and search models with discriminatory tastes (Black, 1995), hiring discrimination against a group by some employers will lower market wages for that group, in the absence of a legal wage floor.

[37] See https://www.eeoc.gov/employers/small-business/small-business-requirements.

In addition, even if skills and wages are similar, when employers choose to cut back on employment in response to a higher minimum wage increase, the job loss could fall mainly on blacks. This is another form of discrimination, if there is no observed or unobserved skill difference that could justify such decisions. Given that employment adjustment to the minimum wage may come about mainly via slower hiring (e.g., Liu et al., 2016; Portugal and Cardoso, 2006), hiring discrimination may be the culprit.[38,39]

We assess whether minimum wages are more binding for blacks in two ways. First, we contrast the bindingness of the minimum wage for the different groups we study with the estimated employment elasticities. Second, and related, we compare estimated employment effects to estimated wage effects.

Figure 14 plots – for many of the groups we study in Table 2, again focusing on the groups for which we found the strongest adverse effects for blacks – the proportion below 110% of the minimum wage, and the estimated employment elasticities for blacks and whites. Differences in the shares below 110% of the minimum wage for blacks and whites are not apparent. To reiterate, though, these comparisons of bindingness can reflect a number of factors and not simply wage differences (or lack thereof) between otherwise identical blacks and whites.[40] Figure 14 also displays the estimated employment elasticities. As we saw in the earlier tables, the employment effects are considerably more adverse for blacks (and more so for black males). The figure emphasizes that these differences in employment effects emerge even though the bindingness of the minimum wage is very similar for blacks and whites. This is apparent, for example, for: high school dropouts (all), for teenagers (all, male, and female), and for and high school dropouts under 30 (all, male). For some groups like high school dropouts (female) and high school dropouts under 30 (female), the employment elasticities are more adverse for blacks even though the minimum wage is more binding for whites. Thus, this evidence does not suggest that the more adverse effects of minimum wages for black

[38] If the minimum wage has caused employers to adjust labor and other inputs so that many workers' marginal revenue products are equal to the minimum wage (consistent with spikes in the wage distribution at the minimum wage), then there is no cost to employers to discriminate against a particular group in reducing employment. (For an early version of this argument, see Stratton, 1993.) One still might expect some mitigation of discrimination from the threat of lawsuits. But the main adjustment may be via hiring, and U.S. discrimination law might do little to prevent hiring discrimination, both because damages are low (as workers get hired sometime later) and it hard to identify a class for a class action lawsuit (Bloch, 1994).

[39] Yet another explanation is that the lower-skilled or younger blacks and whites that we study work in different industries with different elasticities of labor demand. However, the industry distributions are very similar across blacks and whites. For example, for teens, high school dropouts, and high school dropouts under age 30, respectively, the correlations between NAICS two-digit industry employment shares for blacks and whites are 0.98, 0.99, and 0.99. Furthermore, both blacks and whites (in each of these sub-groups) have the highest representation in the two industries – Retail and Food & Accommodation – that have been the primary focus of industry-specific studies examining the negative impacts of minimum wage increases (e.g., Kim and Taylor, 1995; Dube et al., 2010; Jha et al., forthcoming). We thus think industry composition plays little role.

[40] We also note that minimum wages appear to be a bit more binding for females, as indicated by the slightly higher blue bars for them – in contrast to what we might have expected for blacks relative to whites. This may be because employment effects are more adverse for blacks but not for women, so that more women with lower wages are still working after minimum wage increases.

employment are attributable to minimum wages being more binding for blacks.[41]

We can get a somewhat different perspective from comparing wage and employment elasticities. The wage elasticities are estimated using the same regression as in equation (2), although for log wages. The results are presented in Figure 15, which plots the estimated wage elasticities and employment elasticities for each group. These figures provide a way to display more of these estimates compactly. There are a number of observations to take away from the figure. First, in all cases, the estimated wage elasticities are (mostly) positive (and range up to about 0.3). This is to be expected, although one might expect less precise estimates relative to results using measured hourly wages like in the CPS; nonetheless, the point estimates are in the same range as is usually found using CPS data.[42]

Second, overall and for women (Panels A and C), groups with higher wage elasticities also have larger negative employment elasticities. This is clear from the plotted estimates, as well as the simple bivariate regression lines fitted to these points. Evidence of this nature boosts the credibility of our employment estimates, in the sense that, within race, groups for whom wages are pushed up more by the minimum wage (for workers remaining employed) also experience larger job losses. However, for black males (Panel B) the scatter plot is more vertical, and hence a downward slope is not evident. This, again, suggests that the sharper employment effects for less-skilled black males are not explained by larger positive effects on their wages. Finally, echoing our core findings, the estimated employed effects for whites are small – which is made clear in the figure by keeping the vertical axis the same for blacks and whites, which shows how much closer the white employment elasticities are to zero.

Returning to our main inquiry, the third observation is that the wage elasticities are not larger for blacks than for whites, but rather are on average a bit lower (see the note to Figure 15). This is consistent with what Figure 14 showed – that minimum wages are not much more binding for blacks than for whites once we condition on education and/or age.

Fourth, for similar wage elasticities, the employment elasticities for blacks are considerably larger (in absolute value). This is of course related to the evidence from Figure 14, but here we can see that black employment declines following minimum wage increases are much larger than those experienced by whites despite similar or smaller effects on wages. Moreover, wage elasticities are estimated from the employed only, so if blacks experience more job loss, there may be more selection of low-wage blacks than of low-wage whites out of the samples used for the wage estimates. This would imply that wage elasticities for blacks could be biased upward, implying that the higher wage vs. fewer jobs tradeoff is even worse for

[41] Appendix Figure A4 includes the remaining groups covered in Table 2; the conclusions are similar.

[42] For example, looking at the less-educated or teenagers, Neumark and Wascher (2011) report estimates in the range of about 0.15 to 0.3. The measurement error of relevance here is in the dependent variable, which should just lead to imprecision in estimating the effect of the minimum wage, not bias (assuming the measurement error is classical).

blacks.[43]

Together, we interpret this evidence as indicating that the stronger adverse employment effects of minimum wages for blacks are not necessarily explained by lower skills of blacks, or lower wages (even unrelated to skills). These are likely part of the story, given that minimum wages are generally a bit more binding for them. However, the evidence on wage effects does not establish that blacks' wages would be pushed up more.

It thus seems likely that an additional factor is that employers simply choose to reduce employment of blacks more when reducing overall employment in response to minimum wage increases, whether through hiring or separations. There is a good deal of evidence of discrimination in hiring against blacks and other minority groups (Neumark, 2018), but little evidence on the role of minimum wages in this discrimination.[44] Radical/institutional economists, using a model of "queues" for jobs in which whites are higher in the queue, have noted that discrimination may be stronger when labor markets are slack (Thurow, 1975; Evans, 2013). Although this work does not pertain to the minimum wage directly, in the competitive model, a higher minimum wage creates slack (with labor supply exceeding demand), yielding the same prediction.[45] Perhaps consistent with this story, Biddle and Hamermesh (2013) find that the racial wage gap in the United States is procyclical (although it is less clear that the wage gap is relevant here, and the authors point out that part of this effect is compositional). More directly related to hiring, Lippens (2025) conducts a meta-analysis of correspondence study evidence on hiring discrimination. He finds that discrimination against racial and ethnic minorities is countercyclical (more discrimination in slack labor markets) in Europe, but not the United States.

Overall, then, prior research does not give a clear indication of whether a higher minimum wage does or would be expected to increase discrimination against blacks, although we regard this as a plausible hypothesis and one consistent with our findings. We view this as an important question for future research.

VI. Earnings Effects

Our estimated employment and wage elasticities provide information on the "own-wage elasticity" (OWE) of employment stemming from variation in the minimum wage. This parameter is of interest because

[43] The same would apply to Figure 14. On the other hand, there is a potentially offsetting source of bias. As noted earlier (and shown in Table 3), a higher minimum wage reduces hours for blacks. Given that we construct wages by dividing annual earnings by midpoints of weeks worked ranges, hours reductions for blacks are sometimes not captured by the midpoints we use (unless the range changes), implying downward bias in wages measured for blacks following minimum wage increases (although if the decline puts a person in a lower range but is smaller than the midpoint difference, the opposite could occur).

[44] One exception is Brandon et al.'s (2025) correspondence study evidence suggesting relative hiring of blacks increases after a minimum wage increase. However, this kind of field experiment analysis is only revealing about whether treatment of observationally equivalent blacks and whites changes, and does not account for changes in job posting, job search, etc. – and hence the evidence does not necessarily address what happens to actual hiring.

[45] In a sense, this parallels Becker's writing on competition and discrimination, as a higher minimum wage prevents paying less for the group that experiences discrimination, and hence instead leads to avoiding hiring from that group.

the larger it is (in absolute value, assuming the employment effect is negative), the less likely that a higher minimum wage raises earnings of the affected groups.[46] For whites, the wage elasticities are largely in the 0.05 to 0.15 range (Figure 15), and the employment elasticities smaller (in absolute value), implying employment-wage elasticities that can be quite close to zero, in which case higher minimum wages would increase earnings for white workers. For blacks, in contrast, in most cases the employment elasticity is larger in absolute value. In that case, black workers' earnings are likely to decline in response to higher minimum wages (even more so if the wage elasticities for blacks are biased upwards).

Table 9 collects the prior information on employment and wage elasticities in one table, and also reports the implied OWE's (when the estimated wage elasticity is positive). The table shows that there are in fact many implied OWE's for blacks that are negative and greater than one in absolute value.[47] (And, of course, when the estimated wage effects is not positive, the question of whether a higher minimum wage raises a group's earnings is moot – although the same caveat about estimation of wage effects noted earlier applies.)

Rather than speculate about effects on earnings from estimated employment and wage elasticities, in Table 10 we directly estimate effects on earnings (including zeros, so bias from selection into the sample with wages is now irrelevant). Note, also, that if the negative hours effects reported above also reflect negative intensive margin effects (which we cannot estimate with cross-sectional data), then earnings declines could be larger than implied by the OWE, which factors in only employment. The results are striking. For most definitions of low-skilled workers, the estimated earnings effects for whites are positive, and they are even positive and significant in a couple of cases (for under 25, overall and females). In sharp contrast, the estimated effects for blacks are much more likely to be negative, and significant in many cases (for nine groups at the 10% significance level or less): for teens (overall, males, and females); for high school dropouts under 25 (overall, and males); for high school dropouts under 30 (overall and males); and for those with at most a high school education under age 25 (overall and males). In addition, there are other sizable negative effects for blacks that are not significantly different from zero but are significantly different from the effects for whites (e.g., for males with at most a high school education under age 30). Moreover, some of the negative elasticities are sizable, ranging to as much as −0.73, with the estimated adverse impacts

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[46] Freeman (1996) interprets the elasticity of employment with respect to the minimum wage as the elasticity of demand for minimum wage workers. He notes: "[I]f the elasticity of demand for minimum wage workers exceeds one [*in absolute value*], the minimum wage will reduce rather than increase the share of earnings going to the low-paid" (p. 641, italicized text added). Unless one is looking only at workers paid the minimum wage, the wage elasticity with respect to the minimum wage is well below one (a common value in many studies is around 0.15-0.3, as noted earlier). Thus, to estimate the elasticity of demand for minimum wage workers and draw inferences about the effects of the minimum wage on earnings of minimum wage workers, one has to divide the employment elasticity by the wage elasticity.

[47] Recall the earlier caveat that the use of hours ranges in the data could potentially bias the estimated wage effect for blacks downward, in biasing the (absolute value of the) OWE upward.

sometimes considerably larger for black men. The conclusion appears to be that young and less-educated black men, in particular, experience lower earnings because of higher minimum wages.

Table 11 presents estimates from the same two-stage difference-in-differences method used in Table 4 (again, for 2012-19). The results in the first two columns also indicate that the results are robust to the shorter sample period – with even more evidence of adverse effects on blacks, again more so for black males. More important, the results are robust to the 2S-DID estimation. Finally, paralleling Figure 9, Figure 16 plots the 2S-DID estimates against the two-way fixed effects estimates for the larger set of gender, age, and education subgroups; the robustness of the estimates is clear.

VII. Spatial Implications and Analysis

A. Implications of residential racial segregation.

There is extensive residential racial segregation in the United States (e.g., Iceland and Weinberg, 2002; Logan, 2013). Figure 17 shows information on this segregation at the PUMA level, in our data. We plot the share of the population that is black in PUMAs in each decile (and at some other percentiles) of the share black at the PUMA level.[48] A horizontal line would indicate that the share black is the same everywhere. The relationship is not only steep, but convex, indicating sharp segregation of blacks by PUMA, with, for example, the share black increasing from 44.98% to 93.76% from the 90th to the 99th percentiles.

Given the residential concentration of blacks, the more adverse effects of minimum wages on black than on white employment imply that job loss from minimum wages will be sharply concentrated in black areas. In particular, combining the evidence on employment effects with the pattern of segregation in Figure 17, the low share black in the first seven or eight deciles of the share black implies that in most PUMAs, the overall effects of minimum wages will be minimal, whereas the overall effects would be quite strong in the highest deciles where the share black is much higher because of the sharply nonlinear nature of residential segregation.

B. Variation in effects with share black in PUMA

It is also possible that part of the explanation of race differences in the employment effects of minimum wages is that these effects differ across predominantly black areas and predominantly white areas. Given sharp residential segregation by race, these spatial differences in the employment effects of minimum wages could underlie the evidence we have reported based on the race of individuals. Thus, we turn to the question of whether the more adverse effects of minimum wages on blacks are attributable to more adverse effects on black individuals, or more adverse effects on neighborhoods with large black populations – or both.

[48] The first point in the graph corresponds to the 1st percentile and the last point corresponds to the 99th percentile of share black at the PUMA level. All deciles are computed as the weighted percentile of share black across PUMAs, where the weights correspond to the PUMA population in each year.

The employment effects of minimum wages could vary with the share black in the neighborhood. Blacks tend to live in poorer neighborhoods with a higher concentration of low-skilled and younger workers, and a higher share of blacks as compared to whites, as shown in Figure 18A. These differences of course partly reflect underlying individual-level differences between blacks and whites (consistent with the earlier observation that blacks are, on average, younger and less educated than whites). Figure 18B instead shows differences based on the share black, indicating that in areas where blacks are concentrated, families are poorer, and workers are typically lower skilled and younger. Again, this pattern may simply reflect the fact that such areas have a higher share of blacks, who themselves tend to have these characteristics.

In contrast to Figures 18A and 18B, Figure 18C shows average characteristics of those in PUMAs at the 10th and 95th percentile of share black, by race. We see that blacks who live in black areas tend to be better off than blacks who live in white areas (e.g., lower poverty and a bit older, although slightly less educated), while whites who live in white areas tend to be better off than whites who live in black areas (e.g., less likely to be poor, and a bit older). This suggests that predominantly black neighborhoods differ from predominantly white neighborhoods not only because of the characteristics and composition of black workers, but also because of differences between whites living in the different areas.

The lower skills of workers in black areas can imply sharper disemployment effects of minimum wages in those areas in part independent of individual race, and the higher poverty (and extreme poverty) rate may be associated with fewer job opportunities in the first place, different kinds of businesses in the area, etc. However, the relationship of these differences to whether disemployment effects will be larger in areas with larger concentrations of blacks is subtle. Our regressions condition on skill, so even though blacks live in areas where workers are on average less skilled, the regression effects need not differ by area. On the other hand, to the extent that minimum wage effects are more adverse for the less-skilled, on average minimum wage effects would be stronger for blacks because of their position in the skill distribution.

Effects can vary across neighborhoods even if workers are similar across neighborhoods, owing, for example, to the businesses or industries present in different neighborhoods (which may vary in sensitivity to minimum wages or present more or fewer product substitutes),[49] or differences in job density (including jobs available for minorities).[50]

Effects can also differ across neighborhoods if there is differential selection of black and white

[49] See, e.g., Moore and Diez Roux (2006) for evidence on differences in the distributions of different types of food stores across white and black neighborhoods.
[50] See, e.g., evidence on differences in “spatial mismatch” and “racial mismatch” across neighborhoods (Hellerstein et al., 2008). A related mechanism is that in predominantly black neighborhoods, decades of employment suburbanization have reduced local job density and increased the distance to alternative employment. When minimum wages reduce local jobs, the higher commuting costs faced by residents of these areas – compounded by housing discrimination that limits residential mobility – may deter job search and amplify disemployment effects relative to predominantly white neighborhoods where alternative employment is more geographically accessible (Holzer, 1991).

workers into neighborhoods depending on their racial mix, with unmeasured skill differences that could influence minimum wage impacts. In this case, differential effects on, say, black workers in more black vs. more white areas might reflect worker differences rather than neighborhood differences per se; nonetheless, the evidence would still tell us whether, e.g., effects on blacks are more adverse in black neighborhoods.[51,52]

For this analysis, because we are trying to disentangle the effect of an individual's race from the share black in the PUMA, we estimate the model at the individual level. We use a pooled interactive model, augmented to allow the effects of minimum wages to vary not only with race of the individual but also with the racial composition of the area – specifically, we include a dummy variable for whether the percent black is greater than or equal to the 95th percentile (%Black95), and the corresponding interactions:[53]

$$\begin{aligned} Y_{pt} = {} & \alpha + \beta\cdot\ln(MW)_{pt} + \beta_{B}\cdot\ln(MW)_{pt}\cdot Black_{pt} + \beta_{\%B}\cdot\ln(MW)_{pt}\cdot\%Black95_{pt} + \gamma_{B}\cdot Black_{pt} \\ & + \gamma_{\%B}\cdot\%Black95_{pt} + X_{pt}\delta + X_{pt}\cdot Black_{pt}\cdot\delta_{B} + X_{pt}\cdot\%Black95_{pt}\cdot\delta_{\%B} + D_{p}\lambda + D_{p}\cdot Black_{pt}\cdot\lambda_{B} \\ & + D_{p}\cdot\%Black95_{pt}\cdot\lambda_{\%B} + D_{t}\cdot\tau + D_{t}\cdot Black_{pt}\cdot\tau_{B} + D_{t}\cdot\%Black95_{pt}\cdot\tau_{\%B} + \varepsilon_{pt} \qquad (4) \end{aligned}$$

The model includes a full set of interactions with %Black95, including the fixed year and PUMA effects, to ensure that we isolate the effects of variation in %Black95 on the effect of the minimum wage, rather than other omitted interactions of control variables with %Black95.

In Table 12, we first report the estimated minimum-wage employment effect for whites, followed by the interactions with Black and %Black95. For the detailed results, like for the analysis of the fragility of the estimates to alternative methods used in the minimum wage literature, we focus on the groups for which we found the strongest adverse effects – young and less-educated black males. But we summarize more complete results afterwards. Looking at the estimates of β_B in Table 12, or their sums with the estimates of β – which gives us the overall effect for blacks in PUMAs below the 95th percentile black – we find negative interactions for blacks, significant at the 10% level in four out of five cases (all for young and less-educated). However, the sums are generally a bit smaller than the estimated black minimum wage effects in Table 2. Thus, the more adverse effects of minimum wages on young and less-educated blacks do not arise only in

[51] We cannot necessarily distinguish between individual and neighborhood effects by, e.g., comparing effects for black vs. white workers in black vs. white areas, because the selection can be similar across races.

[52] There could also be variation in labor market competition across neighborhoods. See Jha et al. (forthcoming) for differences in concentration in the restaurant sector between more rural and urban areas. Recent research has highlighted possible impacts of higher labor market concentration in mitigating the negative effects of minimum wages on employment (Azar et al., forthcoming; Corella, 2020). However, we examined data from the National Establishment Time Series (NETS), computing PUMA-level HHIs at both the firm and establishment level for a couple of specific low-wage sectors (retail, and food and accommodations), and for a broader seta of low-wage sectors (Arts, Entertainment, and Recreation; Administrative and Support and Waste Management; and Other Services except Public Administration). As shown in Appendix Table A5, there is not a clear relationship between the share black and concentration.

[53] For this analysis, these percentiles are estimated at the individual level. But the PUMA estimates were weighted, and hence they are very comparable. Results were qualitatively similar but a shade weaker using the 90th percentile. We chose to use the 95th percentile based on the evidence on segregation in Figure 17 indicating the strong nonlinearity that sets in between the 90th and 99th percentiles.

the areas with the highest percent black.

At the same time, there is evidence of spatial differences – with larger job loss in black areas. All of the estimated interactions between the minimum wage and the dummy for the percent black being at or above the 95th percentile are negative. And two – for male high school dropouts under either age 25 or age 30 – are significant at the 5% or 10% level. These estimated differences in employment effects combined with the sharply lower employment rates for both whites and blacks in heavily black areas imply considerably larger employment elasticities in black areas, especially for black males. For example, for black male teens we estimate an employment elasticity of −0.627 in heavily black areas, and for black male high school dropouts under age 30, an elasticity of −0.746.

The much larger employment effects in areas with a high share black is illustrated, by way of examples, in Table 13, where we use the estimates from Table 12, and from specifications with homogeneous minimum wages effects that parallel Table 2 but, like Table 12, are estimated at the individual level, to do simple simulations of the effects of a higher minimum wage. Specifically, we consider the effects of an increase from the federal minimum wage of $7.25 prevailing in 2019 (the end of our sample period) to the California minimum wage in that year of $12. We present estimates for male teenagers, and male high school dropouts under 30. Using the estimates from the specification with homogeneous effects, the estimated elasticity varies across PUMAs because the employment rate is different at different percentiles of the percent black (which are reported in Table 12). Using the estimates from Table 12, the estimated elasticity *also* varies across PUMAs because of the estimated differences in employment effects between heavily black areas and other areas.

For male teenagers, the white population share in the areas below the 95th percentile of the share black is over 80% (0.826), while the black population share is 0.853 at and above the 95th percentile of the share black. As shown in the third and fourth rows, employment rates of blacks are a good deal lower than employment rates of whites, and employment rates of both groups are lower in areas with a high share black. In this case, the difference between the white employment rate in white areas (below the 95th percentile of the share black) and the black employment rate in black areas (at or above the 95th percentile of the share black) is 0.183 (0.340 – 0.157), and the difference in the weighted employment rate between these areas is a bit smaller (0.148), but not by much. The other columns present similar estimates for male high school dropouts under 30.

Next, we use these figures along with the estimated employment effects. First, in Panel B, using the estimates from the specification with homogeneous effects, the minimum wage employment elasticities are larger in absolute value for blacks. As a result, the average employment elasticity (weighting by population shares) is smaller in white areas and much larger in black areas: for example, for male teens, −0.398 in heavily black areas vs. −0.116 in white areas. As a result, as shown in the final row of the table, the large minimum wage increase considered would only lower the employment rate of male teens in white areas by 2.1 percentage points. In contrast, the effect in areas at or above the 95th percentile of the share black would double – to a decline of 4.2 percentage points. Going through the same calculations for male high school dropouts under age 30, the differences are more dramatic; the large minimum wage increase considered would lower the employment rate of male high school dropouts under 30 in white areas by 1.5 percentage points, vs. 5 percentage points in areas at or above the 95th percentile of the share black.

These simulations, albeit simple, suggest that some of the larger minimum wage differences that now exist in the United States could account for a sizable share of the lower employment rate of less-skilled workers in areas with a high black population share – even without account for spatial heterogeneity in effects. Consider male high school dropouts under age 30. The simulation implies that minimum wages lower the employment rate in heavily black areas by 3.5 percentage points relative to white areas (5.0 – 1.5). This is about 27% of the weighted difference in employment rates between PUMAs at the 10th and 95th percentiles of the share black (0.298 – 0.169, or 12.9 percentage points). The difference in impacts in areas with a high share black arises because the adverse employment effects of higher minimum wages fall mainly on blacks.

Panel C takes this further, using the estimated heterogeneous minimum wage effects from Table 12. In each case shown, the implied difference in employment effects between heavily black areas and white areas is a good deal larger. For male teens, the large minimum wage increase considered would lower the employment rate of teens in white areas by 1.8 percentage points, vs. 6.2 percentage points in heavily black areas. This difference is equal to 29.7% of the 0.148 difference in weighted employment rates between these areas (0.318 – 0.170). And for male high school dropouts under age 30, the large minimum wage increase considered would lower the employment rate of teens in white areas by 1.2 percentage points, vs. 7.4 percentage points in heavily black areas. This difference is equal to nearly half (48.1%) of the 0.129 difference in employment rates between these areas.

The estimates covering more groups are shown in Figure 19. Here, we graph the results from the calculations just discussed based on Panels B and C of Table 13 – the percentage of the weighted difference in employment rates between areas with a high share white and a high share black that could potentially be accounted for by large minimum wage differences. For example, looking at the red bar for male high school dropouts under age 30, the height of the red bar is the same figure of 48.1% cited above. The red bars are based on heterogeneous minimum wage effects (Table 12 and Panel C), and the blue bars on homogeneous minimum wage effects (Panel B). The red bars are generally higher, and always for males, reflecting the negative estimated interactions between the minimum wage and the dummy variable for the share black being at or above the 95th percentile. Moreover, the heights of the red bars are sizable, near or above 30% for males, suggesting that high minimum wages may account for sizable shares of the employment rate differentials for lower-skilled workers between heavily black areas and other areas (which have a large share white) – owing both to more adverse effects of minimum wages for black individuals, and more adverse effects in heavily black areas.

VIII. Conclusions

There are a priori reasons to believe that the employment effects of minimum wages could be more adverse for black workers than for white workers. These more adverse effects could occur because of skill differences, Becker-type discrimination whereby employers devalue black workers' productivity and hence minimum wages are more binding, or because employers choose to reduce employment relatively more among blacks when responding to a higher minimum wage. Despite these possibilities, and despite the very large literature on employment effects of minimum wages for low-skilled workers, race differences in employment effects have received little attention.

In this paper, we turn to this question, using ACS data that provide very large samples of both blacks and whites. We estimate standard minimum wage-employment regressions, and then extend these analyses to the estimation of race differences in effects. Many of the estimates point to substantial disemployment effects for low-skilled black workers, with some elasticities in the −0.2 to −0.3 range or higher. Moreover, these effects are much larger than for whites, for whom we generally do not detect adverse employment effects of minimum wages. And the adverse effects for blacks are sharper for black men than for black women. We do a number of analyses that bolster a causal interpretation of these results. In addition, the evidence of adverse effects of minimum wages mainly on low-skilled blacks – and more so on low-skilled black men – is reinforced by our estimated effects of minimum wages on both wages and earnings.

We also explore whether lower skills or lower wages (whether because of unmeasured skill or discrimination) explain the more adverse employment effects of minimum wages for blacks. There is not very much evidence of this, although it is hard to be definitive because we can only estimate wage elasticities for those who remain employed. Another factor, which we regard as plausible, is that employers simply choose to reduce employment of blacks more when reducing overall employment in response to minimum wage increases – likely via the hiring channel.

Our analysis also compares employment and wage elasticities. Our comparisons suggest that the adverse employment effects of minimum wages on blacks are in some cases sufficiently large, relative to the positive wage effects, that minimum wages likely reduce earnings of black workers, while being more likely to increase earnings of white workers. We then confirm directly that minimum wages tend to reduce earnings of low-skilled blacks, but are more likely to increase earnings of low-skilled whites.

Given extensive residential segregation by race in the United States, any adverse minimum wage effects for blacks imply that the effects of minimum wages will be much more adverse in areas with a higher black population share. We report illustrative calculations suggesting that large minimum wage differences can account for a sizable share of the employment rate differences, for lower-skilled workers, between PUMAs at or above the 95th percentile of the share black vs. other areas, which amounts to a comparison of heavily black areas vs. predominantly white areas. When we estimate models that also allow employment effects to differ with the share black, we find evidence of more adverse employment effects in black areas, which accentuates the impact of more adverse employment effects on black individuals, and in some cases suggests that large minimum wage differences can account for around 40% of the employment rate differences, for lower-skilled workers, between PUMAs with very high and much lower black population shares.

Recall that Milton Friedman called the minimum wage "the most anti-Negro law on our statute books." We cannot compare the effects of the minimum wage to other laws that may adversely affect blacks. And we do not believe higher minimum wages are enacted to harm blacks, or with knowledge that the benefits may accrue mainly to whites. But our evidence indicates that – when it comes to the labor market impacts of the minimum wage – there is evidence that blacks appear to bear the cost, while whites bear very little cost and more likely benefit.

One could take the view that our evidence of adverse effects on blacks is not decisive because we do not find these effects for every low-skill group we consider, and the results are less likely to be statistically significant when subjected to more demanding analyses that use less data. Nonetheless, there is clearly some evidence of adverse effects for blacks (especially black men), and little or no evidence of adverse effects for whites and some evidence of positive effects on their earnings. At a minimum, this has to call into serious question the claim that a higher minimum wage narrows the gaps in labor market outcomes between blacks and whites. And our view of the evidence is that it makes us more likely to believe that a higher minimum wage instead widens race disparities.

In contrast, recent research on the Earned Income Tax Credit (EITC) indicates that it reduces after-tax income inequality between blacks and whites in the lower half of the income distribution, although not at the lower tail (the 10th percentile). This inequality-reducing effect comes from stronger positive extensive margin (employment) responses among blacks. Thus, combined with the evidence from this paper, it appears that the EITC is far more effective than the minimum wage at reducing race disparities in earnings. In our view, this conclusion complements broader research indicating that the EITC is far more effective than the minimum wage at reducing poverty and helping low-income families (Burkhauser, 2021).

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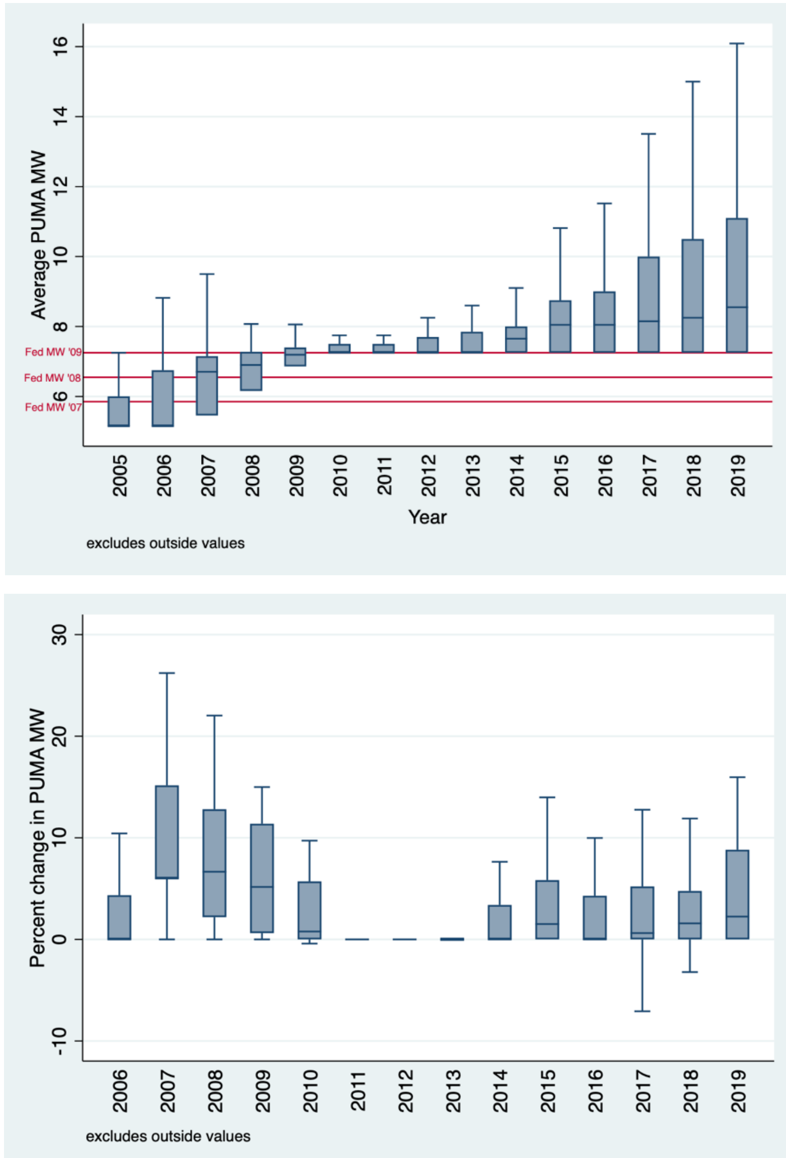
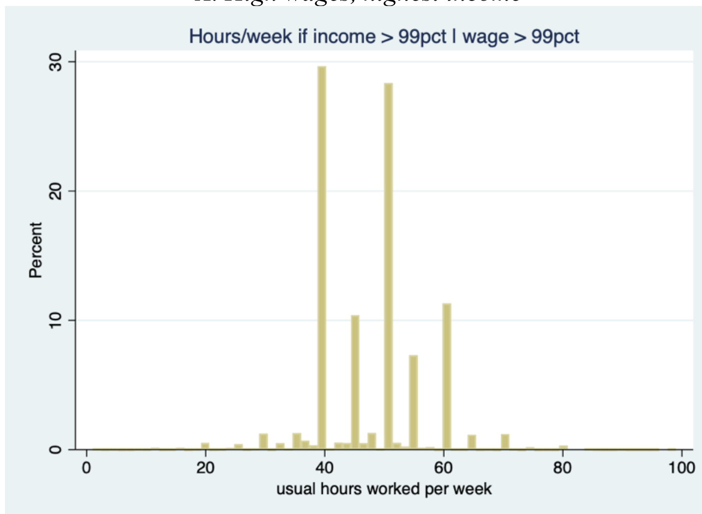
Figure 1: Box-and-Whisker Plots for Minimum Wages Levels and Percent Changes



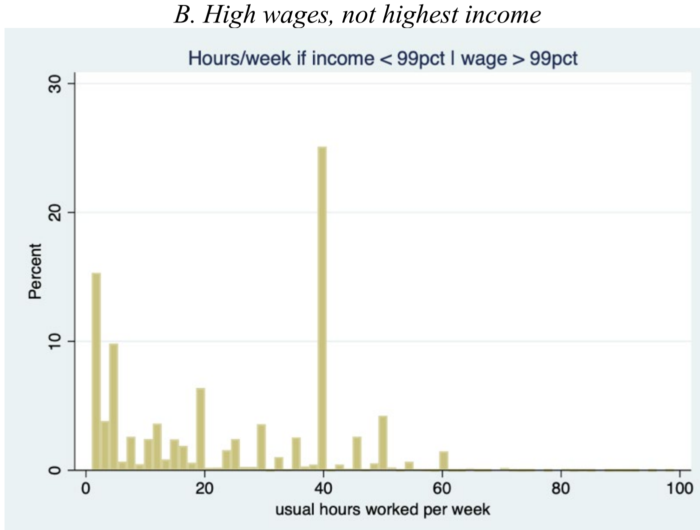
Notes: The rectangle extends from the 25th to the 75th percentile, the upper horizontal lines show the 75th percentile plus 1.5 times the interquartile range, and the lower horizontal lines show the 25th percentile minus this value. The latter is often not shown because of bunching attributable to the federal minimum wage. Note there are a few minimum wage declines owing to deflation for an indexed minimum wage (Colorado in 2010), legislation (Iowa, in 2017), and a court ruling (Kentucky, in 2016) overturning local minimum wages. Note also that some PUMA definitions change in 2012, in which case the first percent change can be in 2013.

Figure 2: Reported Hours Distributions for High-Income and Lower-Income High-Wage Workers

A. High wages, highest income



B. High wages, not highest income



C. High wages, lower income

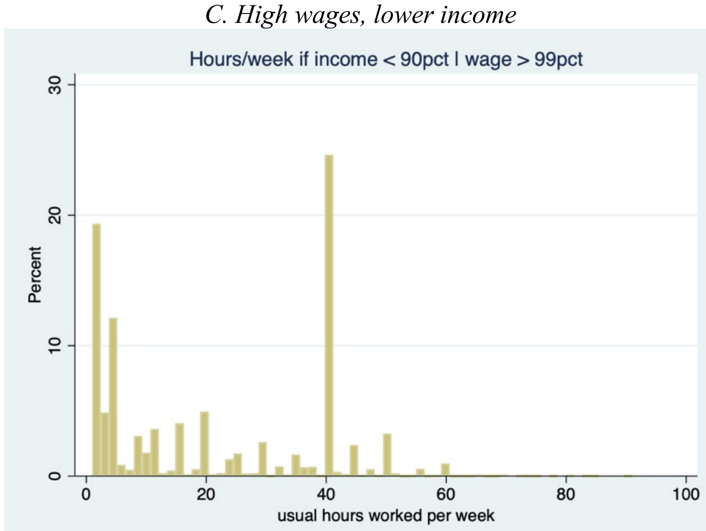


Figure 3: Distribution of Estimated Hourly Wages

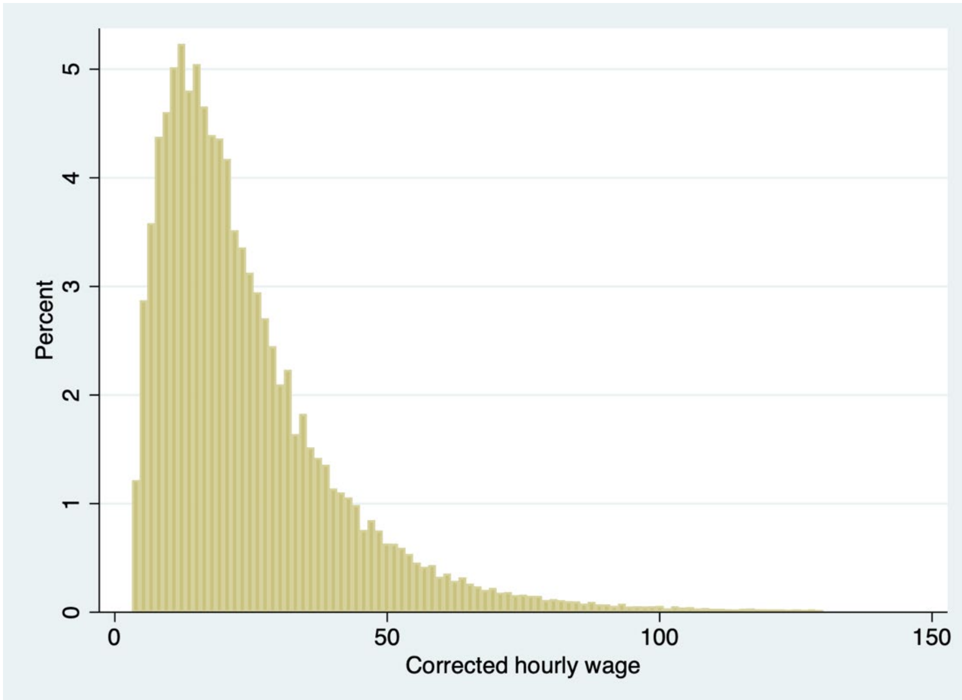
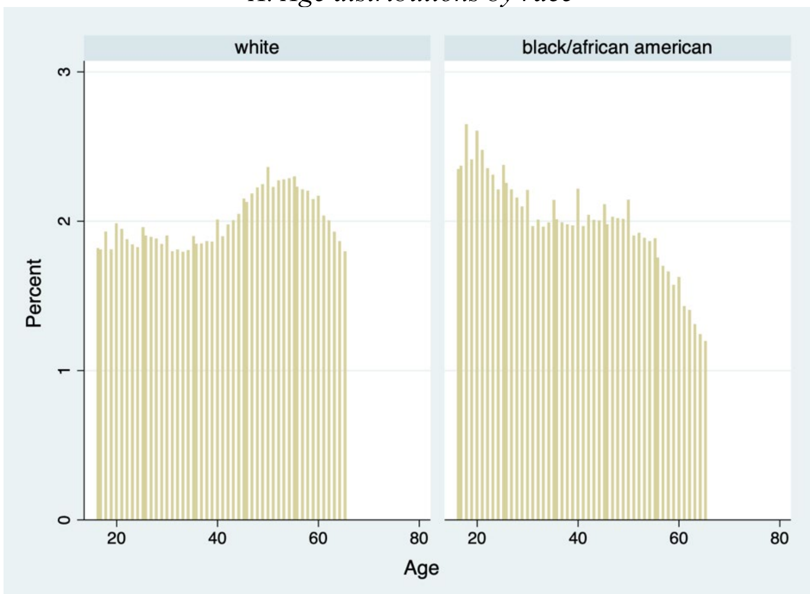


Figure 4: Race Differences in Age and Education

A. Age distributions by race



B. Education distributions by race

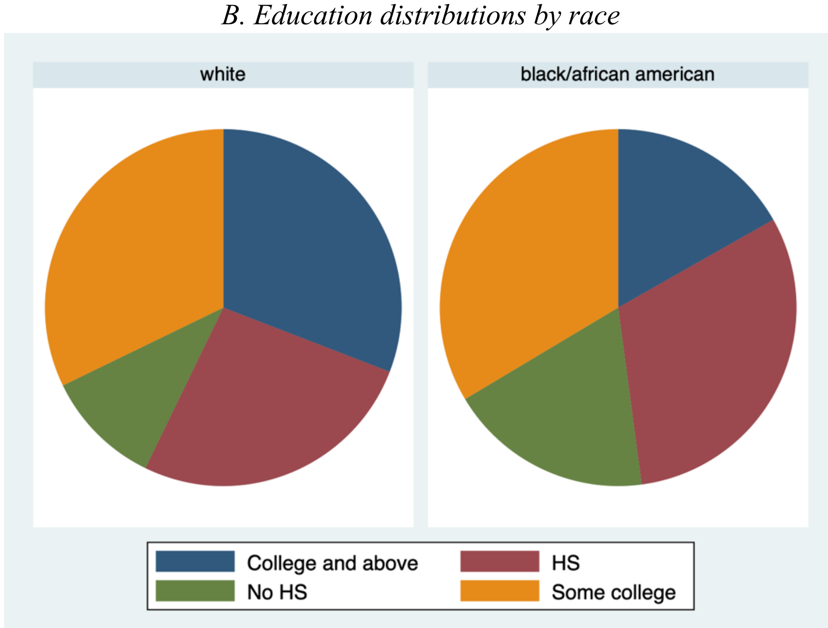
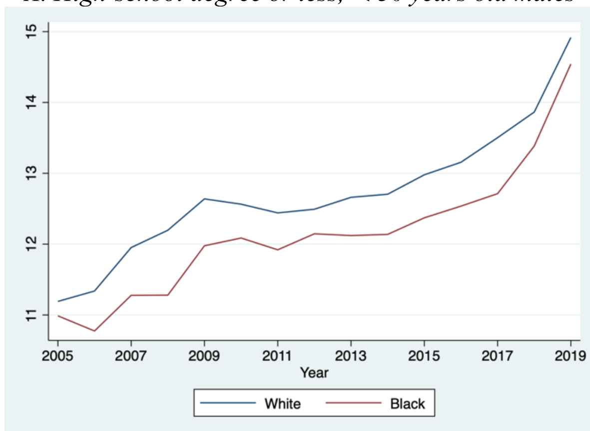
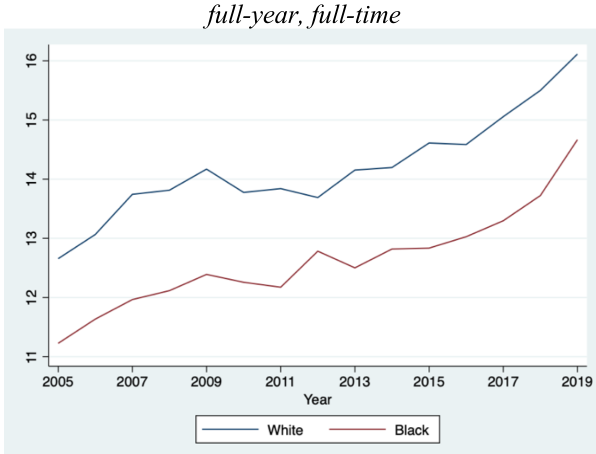


Figure 5: Hourly Wages by Year, Blacks and Whites

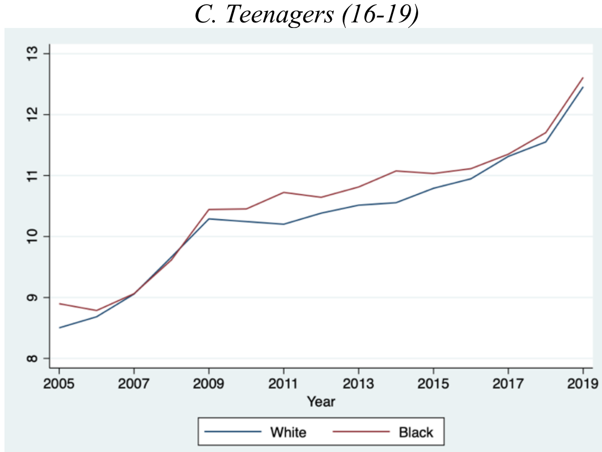
A. High-school degree or less, < 30 years old males



B. High-school degree or less, < 30 years old males, full-year, full-time

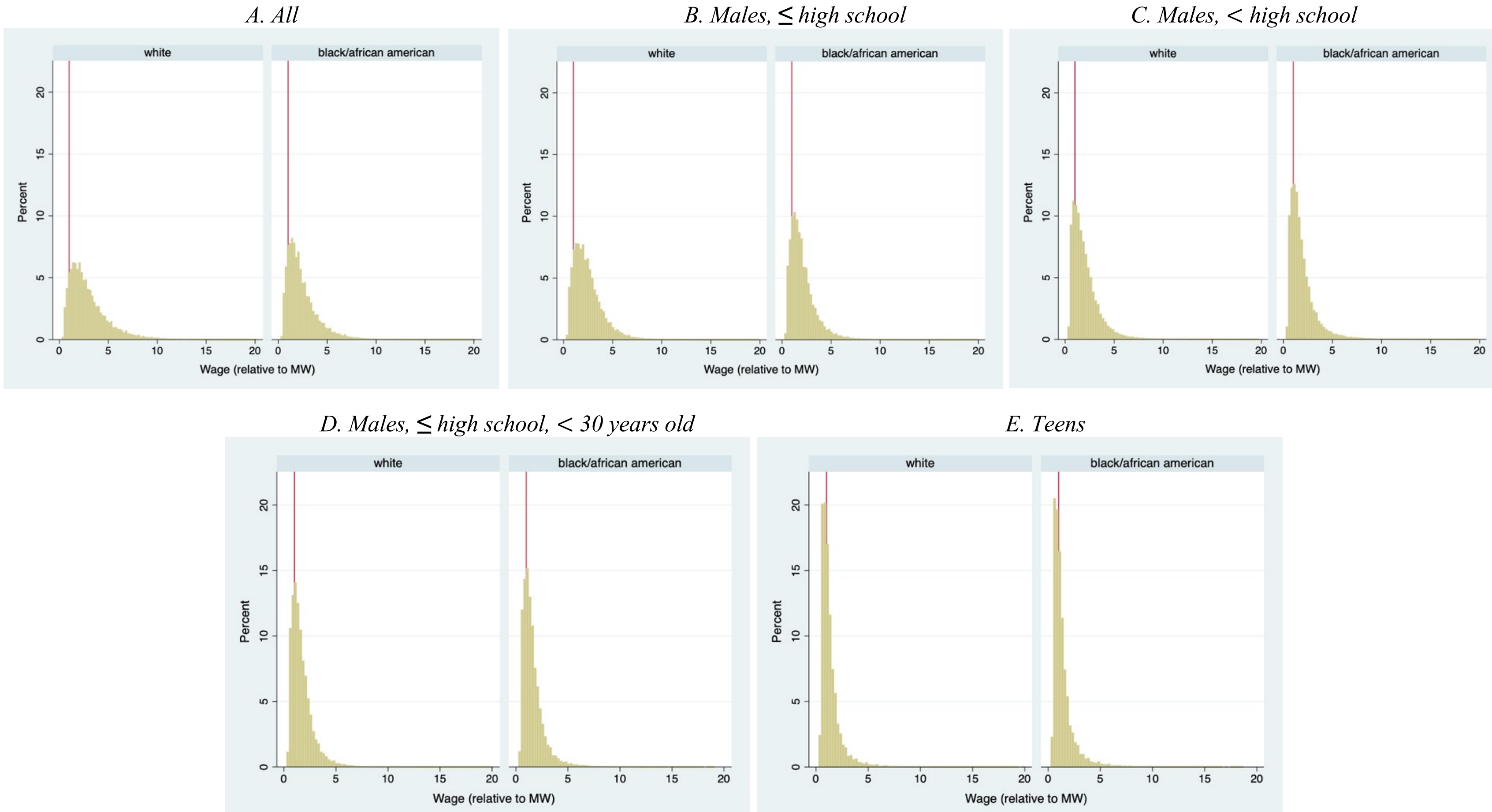


C. Teenagers (16-19)



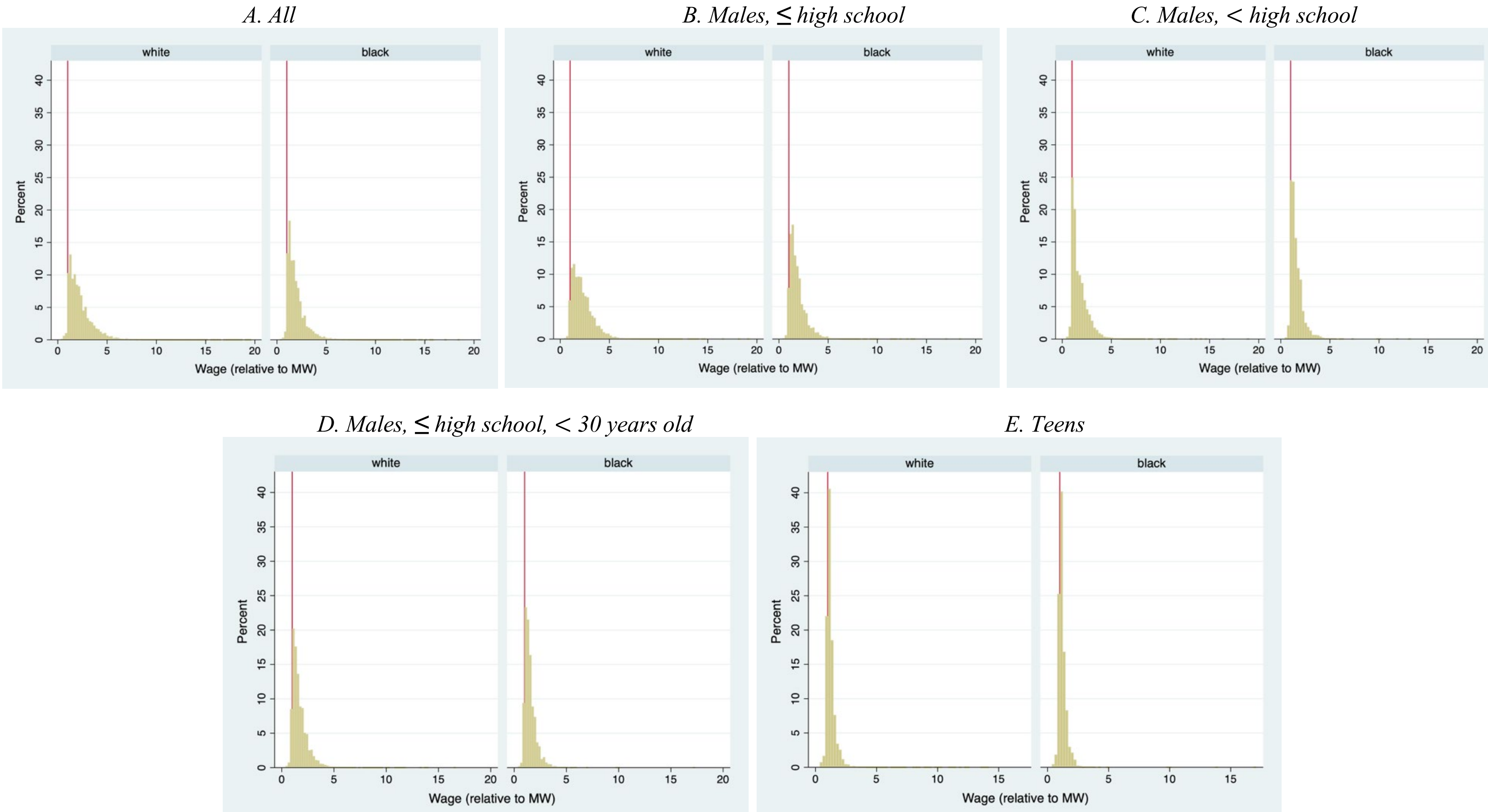
Note: Wages in each year are in nominal terms (in the respective year's dollar value) and weighted by individual person weights.

Figure 6: Wage Distributions of Blacks and Whites



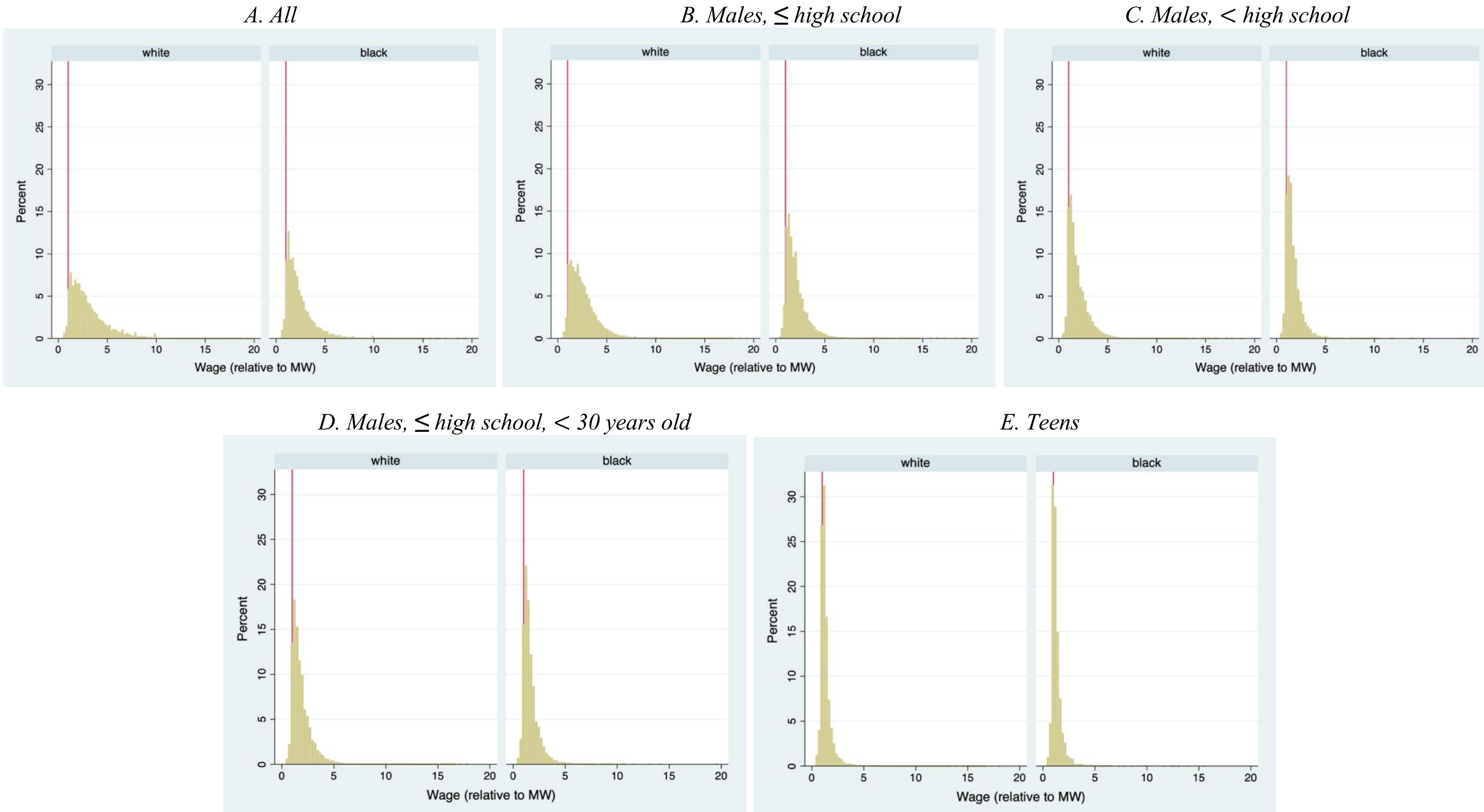
Note**:** Wages on the x-axis are defined as relative to minimum wage in each year, i.e., we construct wage/minimum wage and then pool across years. Thus, the red spike represents relative wage = 1 (or wage = minimum wage) in any year.

Figure 7: Wage Distributions of Blacks and Whites Using CPS Hourly Wage Data Reported for Hourly Workers



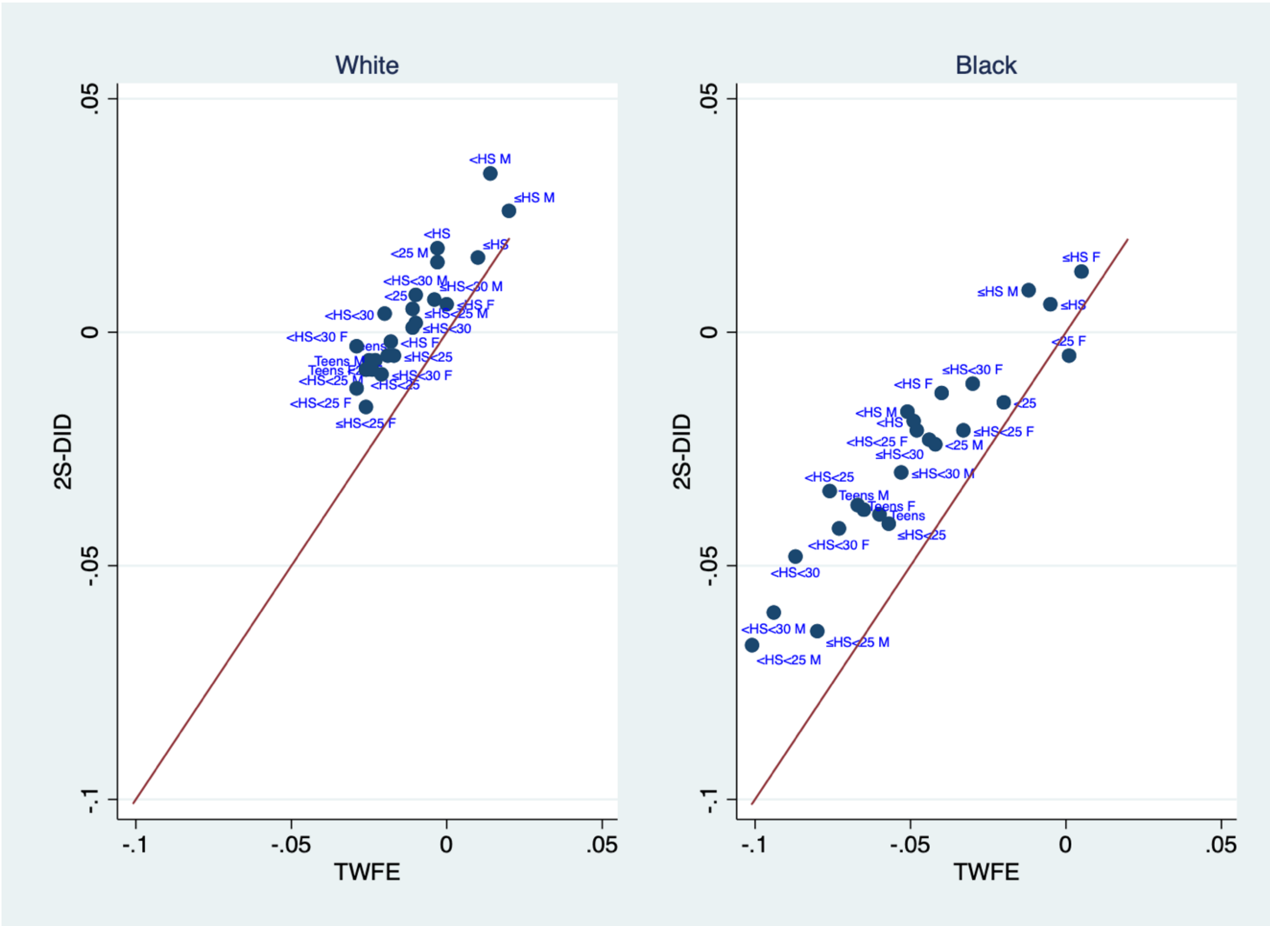
Note**:** Wages on the x-axis are defined as relative to minimum wage in each year, pooled across years. Thus, the red spike represents relative wage = 1 (or wage = minimum wage) in any year.

Figure 8: Wage Distributions of Blacks and Whites Using CPS Constructed Hourly Wage Data



Note**:** Wages on the x-axis are defined as relative to minimum wage in each year, pooled across years. Thus, the red spike represents relative wage = 1 (or wage = minimum wage) in any year.

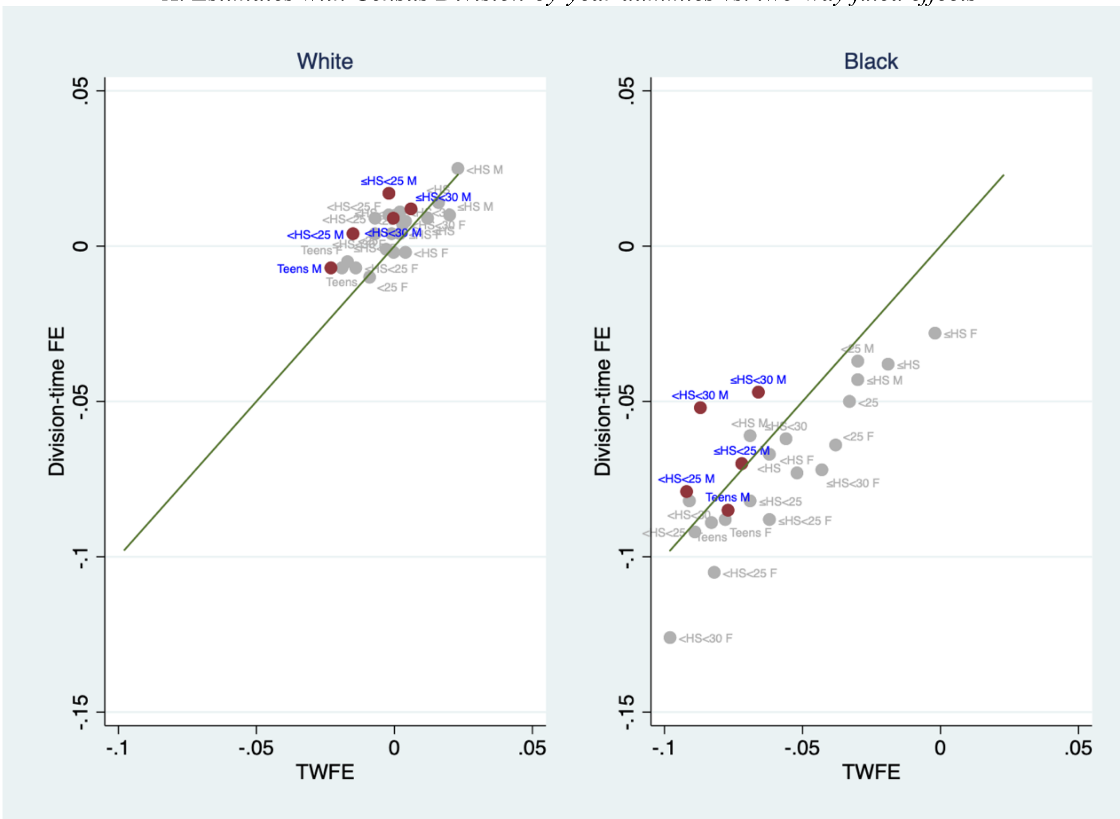
Figure 9: 2S-DID vs. Two-Way Fixed Effects Estimates, Employment



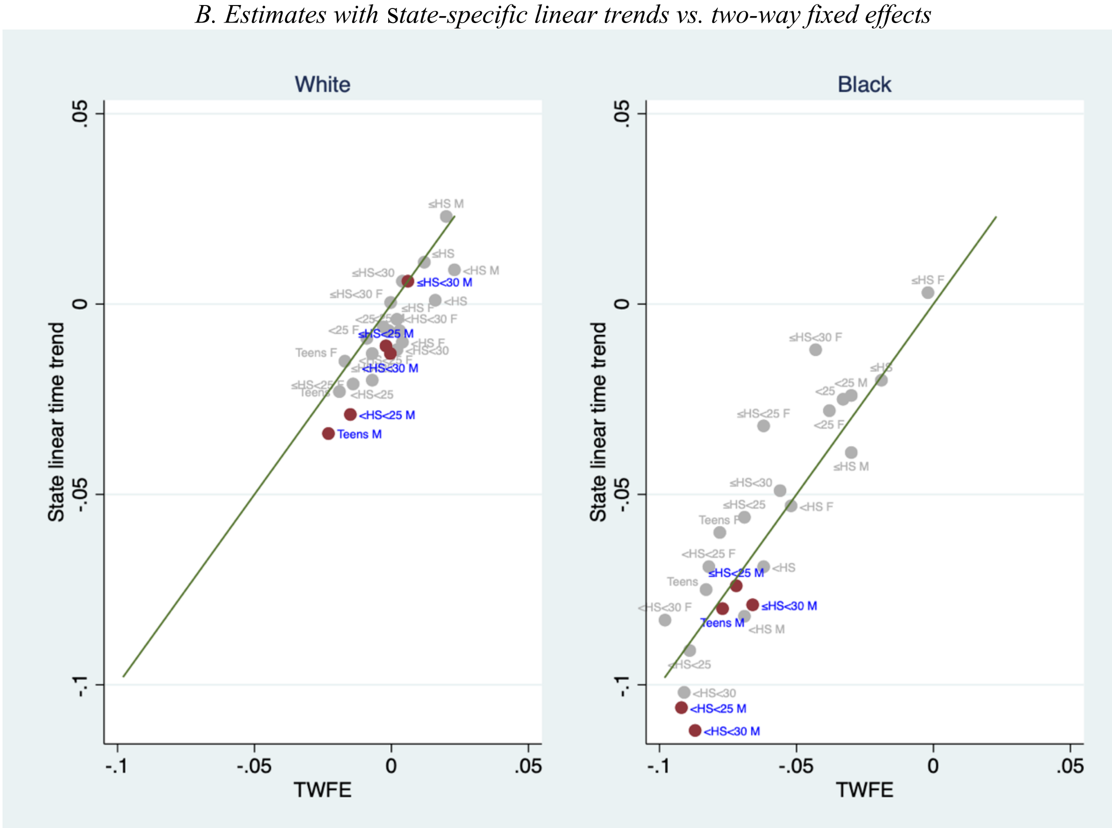
Note: Figure shows the estimates and 45-degree line. Estimates are shown for all gender, age, and education groups for which estimates are reported in Table 4. The correlation between estimates for Whites is 0.92, and for Blacks is 0.94.

Figure 10: Two-Way Fixed Effects Estimates vs. Estimates with Census Division-by-Year Dummies and State-Specific Linear Trends, Employment

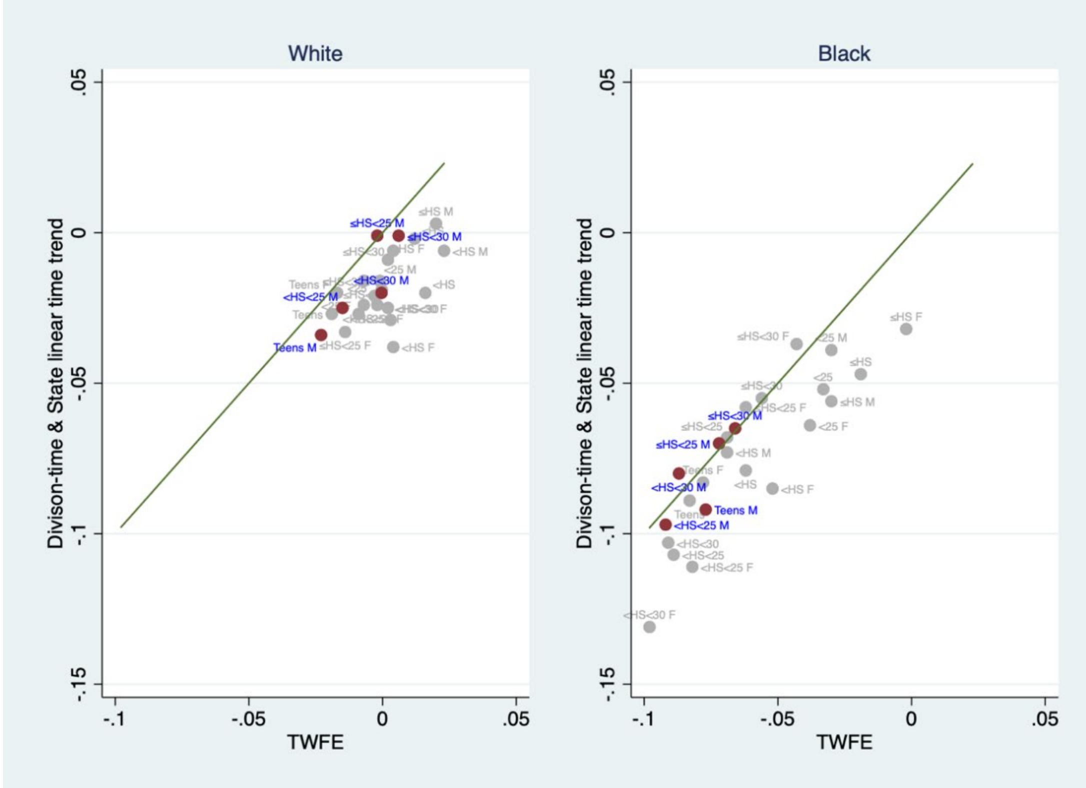
A. Estimates with Census Division-by-year dummies vs. two-way fixed effects



B. Estimates with State-specific linear trends vs. two-way fixed effects

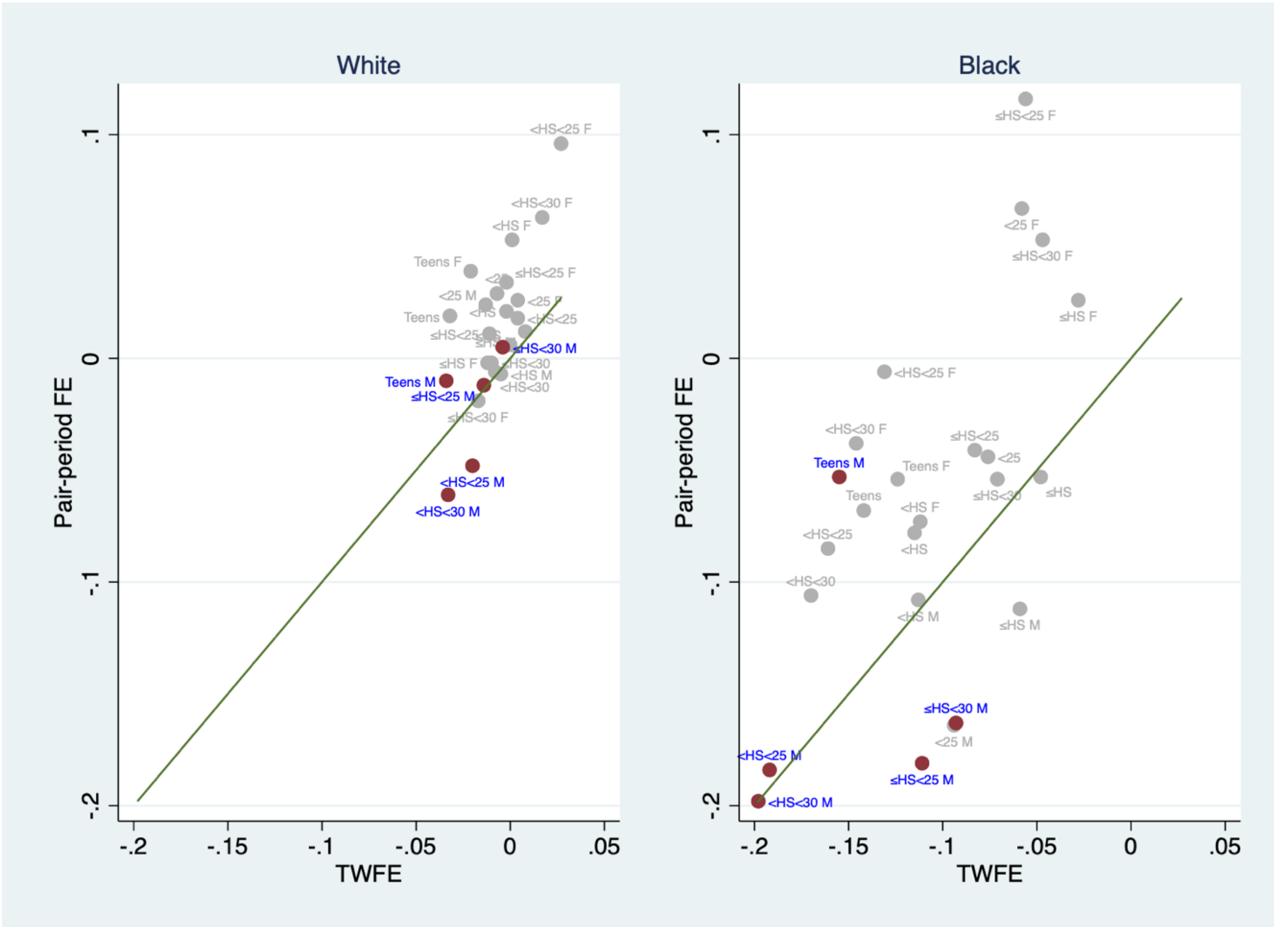


C. Estimates with Census Division-by-year dummies and state-specific linear trends vs. two-way fixed effects



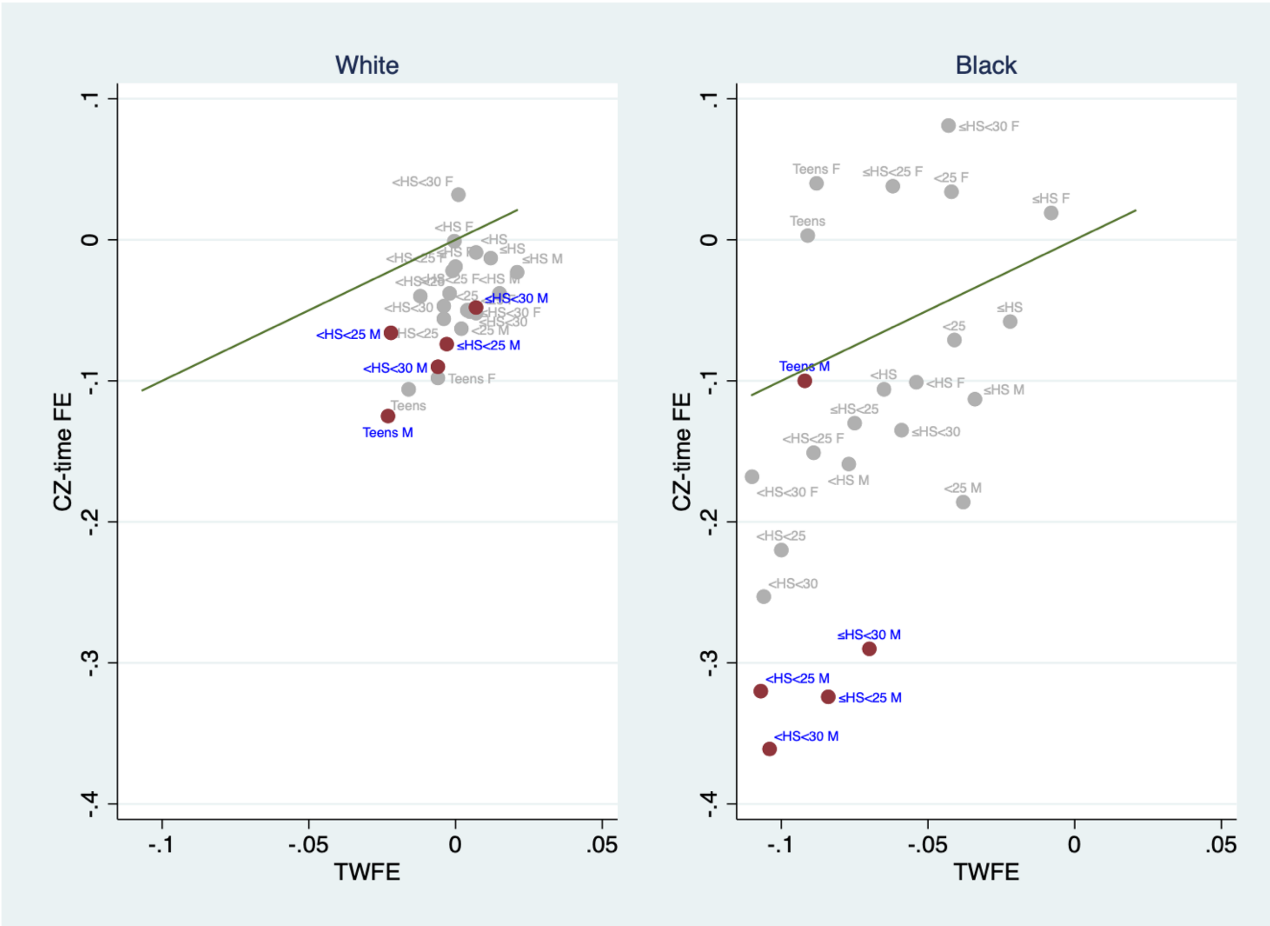
Note: Highlighted estimates correspond to Table 5, while shaded estimates are reported for the full set of race, gender, age, and schooling groups covered in Table 2. Figure shows the estimates and 45-degree line.

Figure 11: Two-Way Fixed Effects Estimates vs. Cross-Border Design Using Contiguous County Pairs, Employment



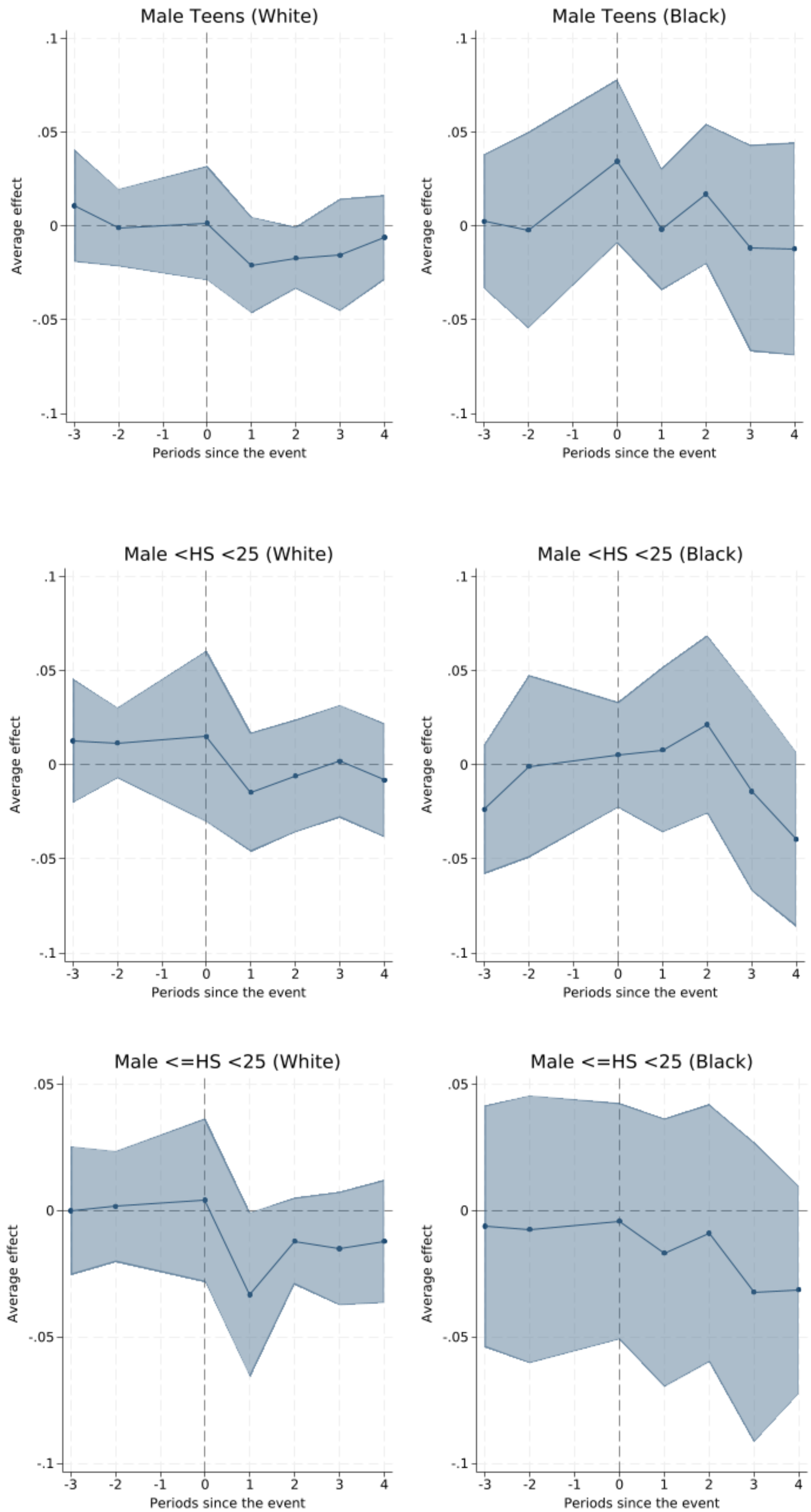
Note: Highlighted estimates correspond to Table 6, while shaded estimates are reported for the full set of race, gender, age, and schooling groups covered in Table 2. Figure shows the estimates and 45-degree line.

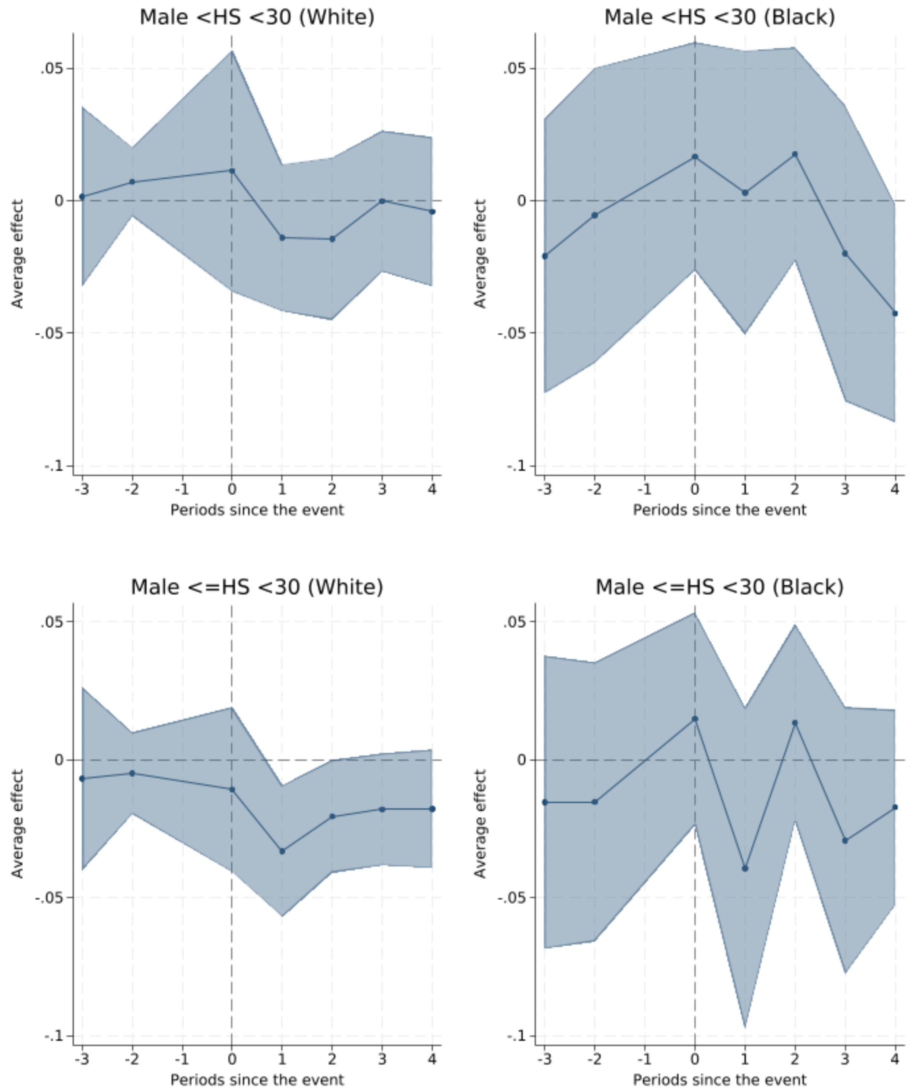
Figure 12: Two-Way Fixed Effects Estimates vs. Cross-Border Design Using Multi-State Commuting Zones, Employment



Note: Highlighted estimates correspond to Table 7, while shaded estimates are reported for the full set of race, gender, age, and schooling groups covered in Table 2. Figure shows the estimates and 45-degree line.

Figure 13: Event-Study Graphs for Select Groups (Clean Controls, Discrete Treatment, 2012-19)





Note: The event-study estimates exclude one year prior to treatment, thereby constraining the pre-treatment coefficients to zero. However, the graph displays a continuous line connecting all the estimates, which may give the appearance of a non-zero value in the pre-treatment year, even though no estimate is reported for that period.

Figure 14: Shares below 110% of the Minimum Wage and Estimated Employment Elasticities

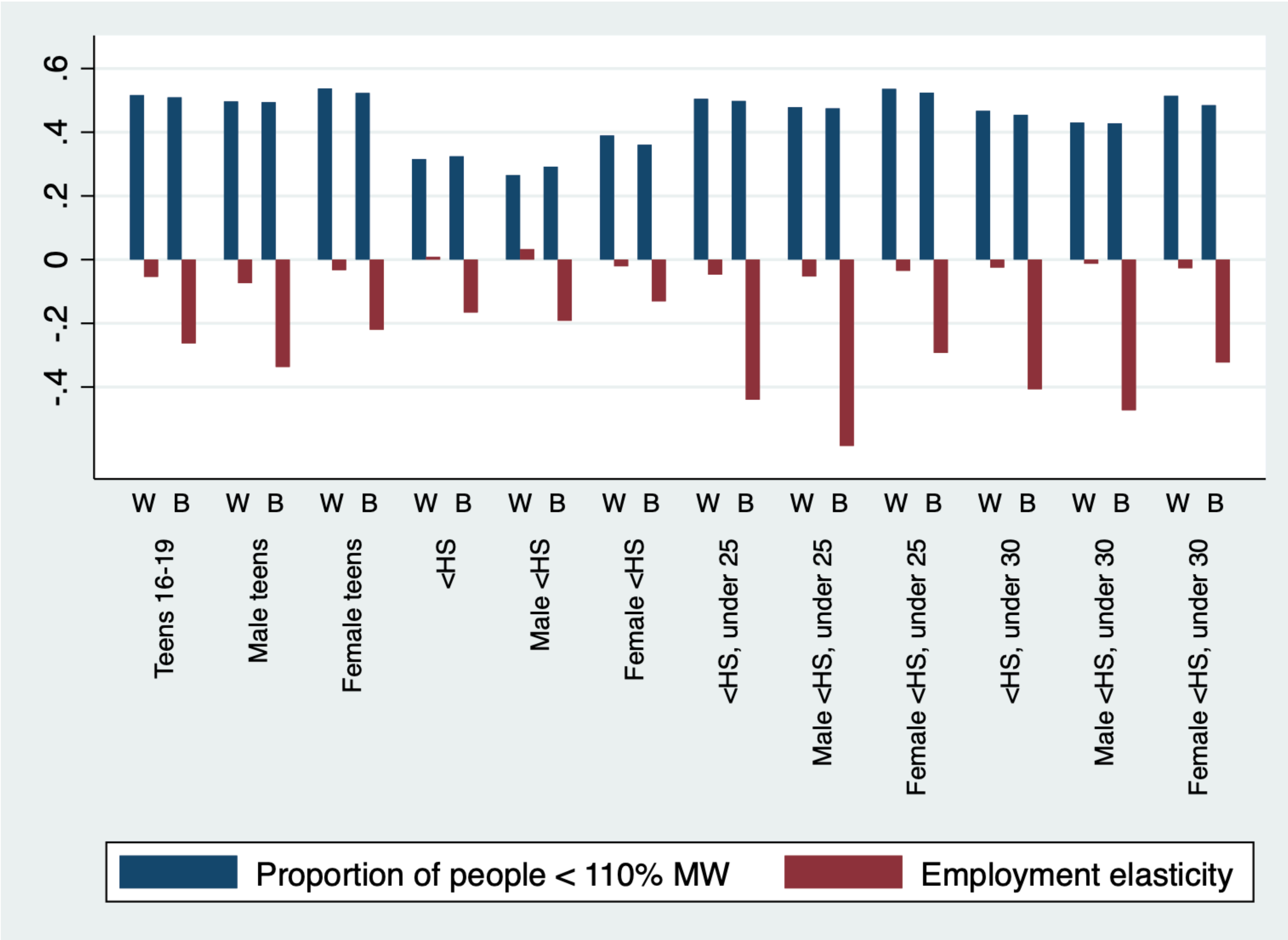
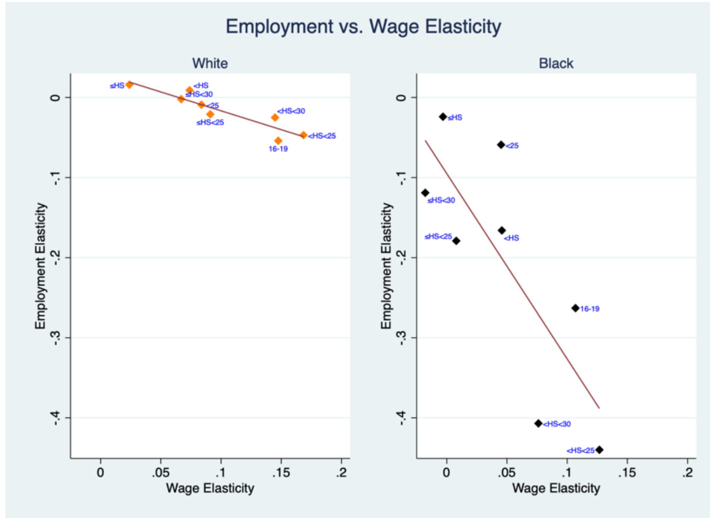
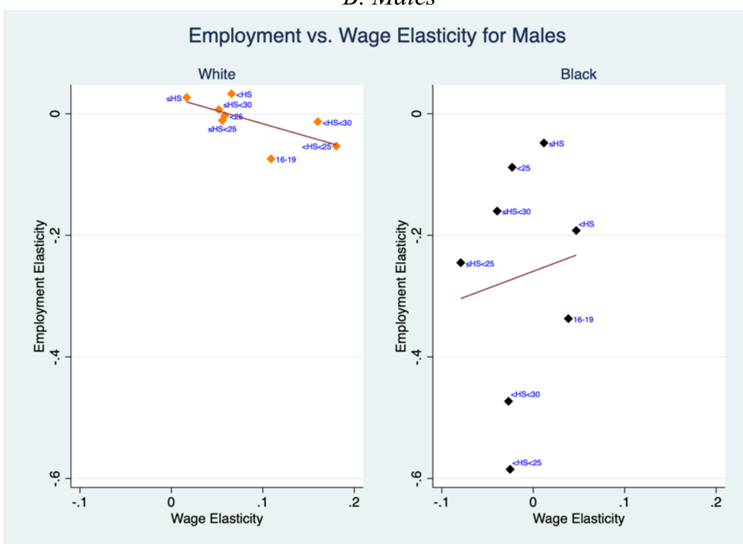


Figure 15: Employment and Wage Elasticities

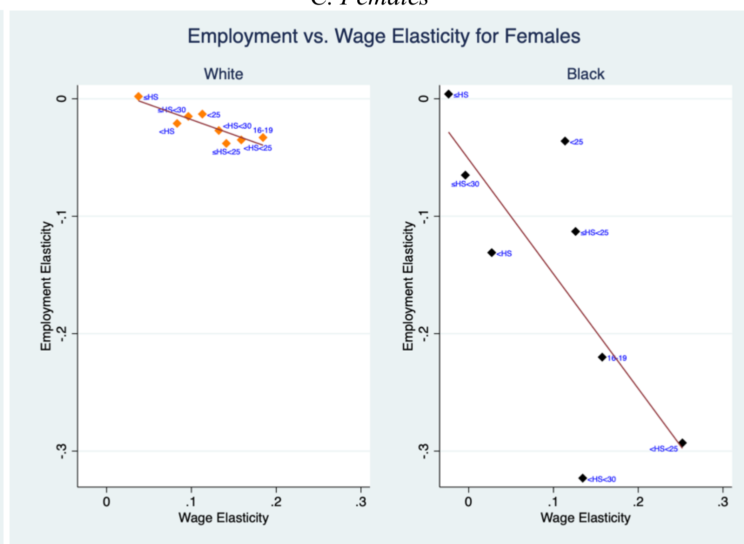
A. All



B. Males

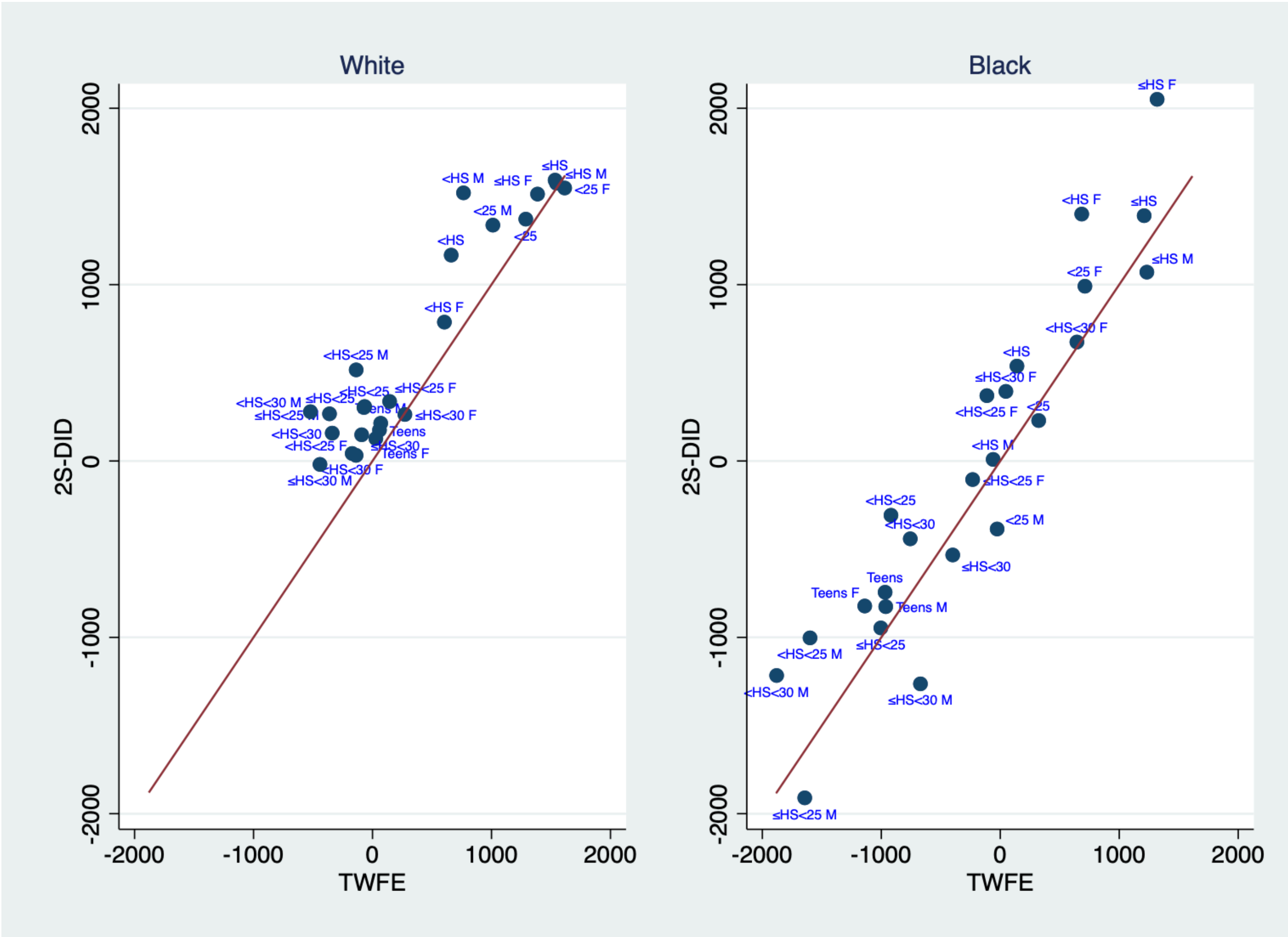


C. Females



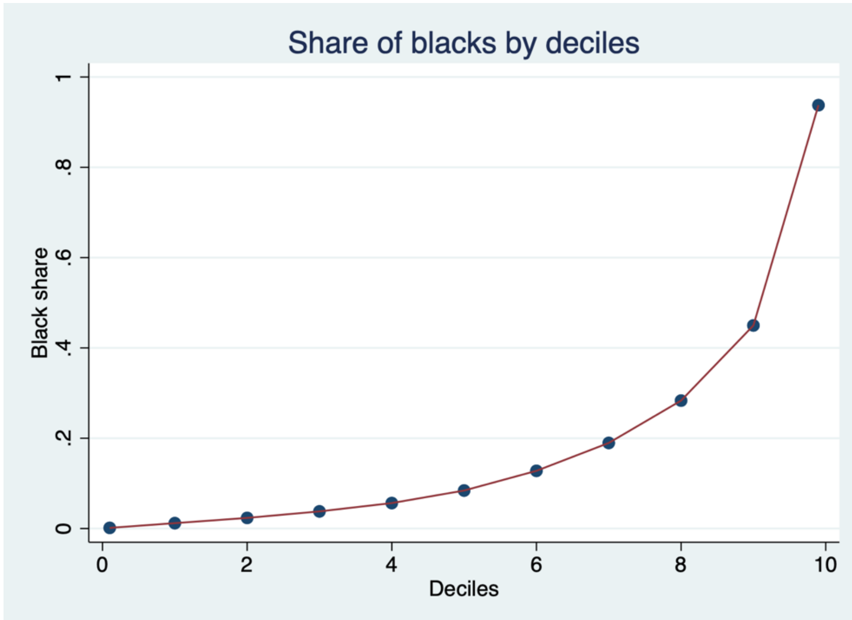
Note: Average wage elasticities for Panel A: 0.10 (whites), 0.05 (blacks); Panel B: 0.09 (whites), -0.01 (blacks); Panel C: 0.12 (whites), 0.10 (blacks).

Figure 16: 2S-DID vs. Two-Way Fixed Effects Estimates, Earnings



Note: Figure shows the estimates and 45-degree line. Estimates are shown for all gender, age, and education groups for which estimates are reported in Table 11. The correlation between estimates for Whites is 0.94, and for Blacks is 0.94 as well.

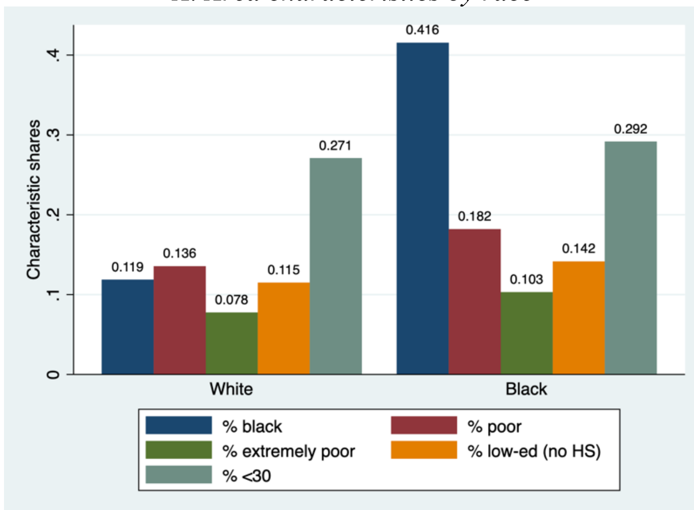
Figure 17: Share of Black Population by PUMA Deciles



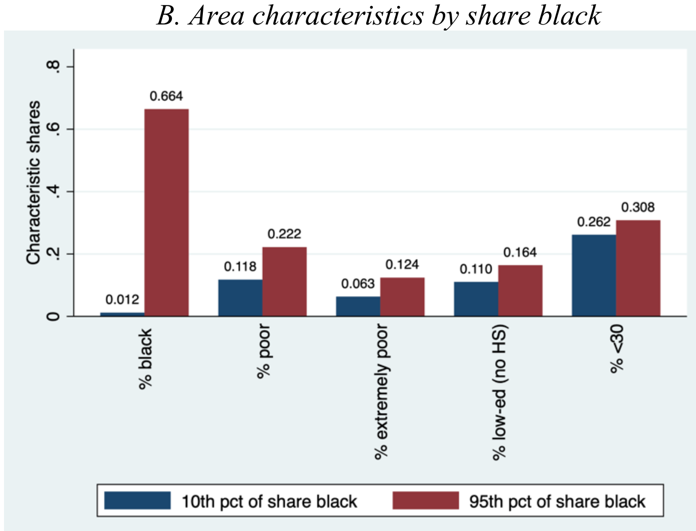
Notes: The first point in the graph corresponds to the 1st percentile and the last point corresponds to the 99th percentile of share black at the PUMA level. The other points are the deciles (10th, 20th, etc., percentiles). All deciles are computed as the weighted percentile of share black across PUMAs, where the weights correspond to the PUMA population every year.

Figure 18: PUMA Share Black, Poor, Extremely Poor, Low-Skilled, and Young by Race and Share Black

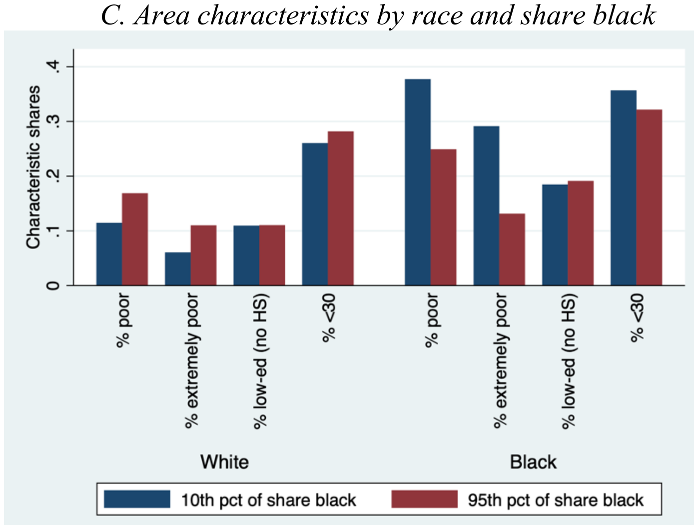
A. Area characteristics by race



B. Area characteristics by share black

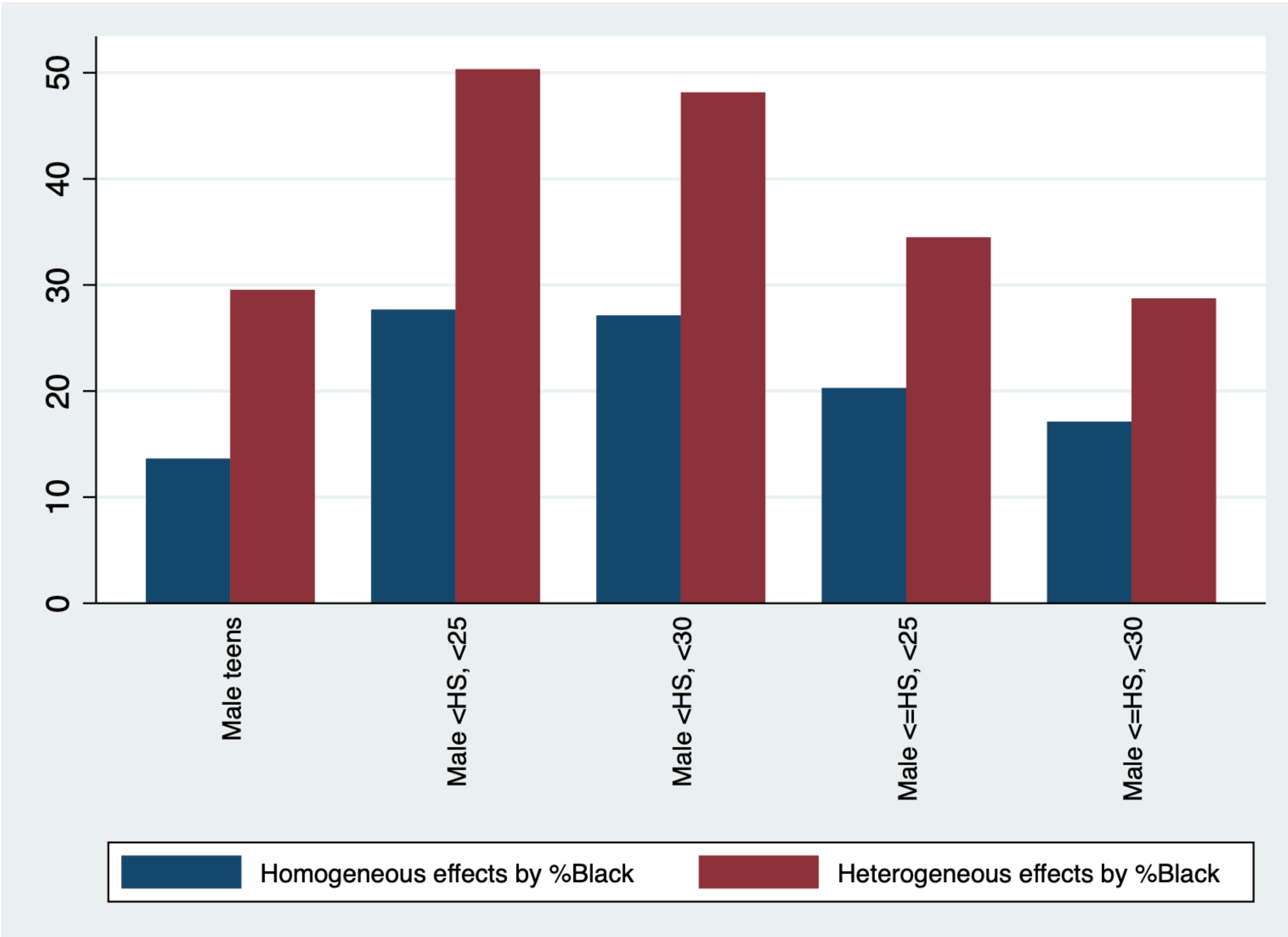


C. Area characteristics by race and share black



Note: All characteristic shares are estimated using individual-level data weighted by individual person weights. All shares are measured in a ± 5 percentile interval around the specified percentile; e.g., the share poor in the 10th percentile is computed as the weighted average between 5th and 15th percentiles of the share black.

Figure 19: Percent of Difference in Employment Rates in White Areas (black share < 95th percentile) vs. Black Areas (black share > 95th percentile) Explained by a Large Minimum Wage Increase ($7.25 to $12) – Homogeneous Minimum Wage Effects vs. Heterogeneous Effects



Note: The height of each bar represents the percentage of the employment rate gap between White and Black areas that is explained by a large minimum wage increase. The estimates used to determine the percentage of variation explained are derived from the calculations presented in Table 13.

Table 1: Baseline Minimum Wage-Employment Regressions

Population	Employment effect (β)	Black effect (γ_B)	Average Employment rate	Employment elasticity
Teens 16-19	-0.029	-0.080***	0.330	-0.087
	(0.019)	(0.003)		(0.057)
Male teens	-0.035	-0.088***	0.310	-0.112*
	(0.021)	(0.004)		(0.067)
Female teens	-0.023	-0.068***	0.350	-0.065
	(0.019)	(0.004)		(0.054)
< 25	-0.010	-0.079***	0.518	-0.020
	(0.014)	(0.003)		(0.028)
Male < 25	-0.009	-0.102***	0.507	-0.017
	(0.017)	(0.004)		(0.034)
Female < 25	-0.010	-0.054***	0.530	-0.020
	(0.014)	(0.003)		(0.026)
< HS	-0.012	-0.059***	0.368	-0.032
	(0.020)	(0.005)		(0.053)
Male < HS	-0.006	-0.098***	0.395	-0.016
	(0.022)	(0.005)		(0.057)
Female < HS	-0.016	-0.009	0.335	-0.049
	(0.018)	(0.006)		(0.054)
≤ HS	0.003	-0.027***	0.567	0.006
	(0.016)	(0.006)		(0.028)
Male ≤ HS	0.007	-0.082***	0.603	0.011
	(0.019)	(0.005)		(0.031)
Female ≤ HS	0.007	-0.082***	0.603	0.011
	(0.019)	(0.005)		(0.031)
< HS, < 25	-0.028	-0.095***	0.267	-0.105
	(0.021)	(0.004)		(0.078)
Male < HS, < 25	-0.034	-0.115***	0.258	-0.133
	(0.023)	(0.004)		(0.089)
Female < HS, < 25	-0.021	-0.071***	0.277	-0.076
	(0.023)	(0.004)		(0.083)
≤ HS, < 25	-0.020	-0.101***	0.414	-0.048
	(0.019)	(0.004)		(0.046)
Male ≤ HS, < 25	-0.022	-0.129***	0.421	-0.053
	(0.022)	(0.004)		(0.051)
Female ≤ HS, < 25	-0.019	-0.066***	0.405	-0.047
	(0.020)	(0.004)		(0.049)
< HS, < 30	-0.027	-0.096***	0.290	-0.094
	(0.023)	(0.004)		(0.079)
Male < HS, < 30	-0.027	-0.125***	0.290	-0.093
	(0.024)	(0.004)		(0.084)
Female < HS, < 30	-0.025	-0.059***	0.290	-0.087
	(0.025)	(0.004)		(0.087)
≤ HS, < 30	-0.011	-0.096***	0.468	-0.024
	(0.019)	(0.004)		(0.040)
Male ≤ HS, < 30	-0.012	-0.134***	0.488	-0.025
	(0.021)	(0.004)		(0.043)
Female ≤ HS, < 30	-0.010	-0.047***	0.444	-0.021
	(0.020)	(0.005)		(0.046)

Notes: The sample consists of ACS micro-data at the PUMA-by-year level from 2005-2019 restricting to those aged between 16 to 65. ACS person sampling weights are used while collapsing the micro-data. The demographic controls included are a dummy variable for PUMA cell means for blacks, and the proportion of males, average number of children, average age, proportion of people in each category of marital status and education in each PUMA cell. Fixed effects are at PUMA and year level. Minimum wages can vary across PUMAs over years. Employment regressions are weighted by PUMA population for each group in each year. Regressions are estimated in Stata using the reghdfe command. Employment elasticity for each population group is computed by dividing the employment effect (β) by the average employment rate of the group weighted by its PUMA population every year, using nlcom command for post-estimation. Reported standard errors are clustered at the state level. The coefficients are statistically significant at the ***1%, **5%, or *10% level.

Table 2: Minimum Wage-Employment Regressions with Separate Effects by Race

Population	White empl. effect	Black empl. effect	Avg white empl. rate	Avg black empl. rate	White empl. elas.	Black empl. elas.	White sub-pop. share	Black sub-pop. share
Teens 16-19	-0.019 (0.019)	$-0.059^{**\dagger\dagger}$ (0.025)	0.358	0.226	-0.054 (0.052)	-0.263^{**} (0.109)	0.074	0.098
Male teens	-0.025 (0.020)	$-0.069^{**\dagger\dagger}$ (0.027)	0.339	0.204	-0.074 (0.059)	-0.337^{**} (0.134)	0.038	0.050
Female teens	-0.012 (0.021)	$-0.055^{**\dagger}$ (0.026)	0.378	0.249	-0.033 (0.054)	-0.220^{**} (0.106)	0.036	0.048
< 25	-0.005 (0.014)	-0.024 (0.022)	0.547	0.408	-0.009 (0.026)	-0.059 (0.055)	0.169	0.217
Male < 25	-0.002 (0.017)	$-0.033^{\dagger}$ (0.025)	0.539	0.380	-0.003 (0.031)	-0.088 (0.066)	0.086	0.110
Female < 25	-0.007 (0.014)	-0.016 (0.024)	0.555	0.437	-0.013 (0.025)	-0.036 (0.055)	0.082	0.107
< HS	0.004 (0.018)	$-0.049^{**\dagger\dagger\dagger}$ (0.023)	0.393	0.297	0.009 (0.046)	-0.166^{**} (0.079)	0.106	0.185
Male < HS	0.014 (0.021)	$-0.054^{**\dagger\dagger\dagger}$ (0.024)	0.433	0.284	0.033 (0.049)	-0.192^{**} (0.086)	0.059	0.100
Female < HS	-0.007 (0.017)	-0.041 (0.028)	0.343	0.312	-0.021 (0.050)	-0.131 (0.090)	0.048	0.085
≤ HS	0.010 (0.016)	-0.012 (0.024)	0.591	0.476	0.016 (0.027)	-0.024 (0.050)	0.370	0.496
Male ≤ HS	0.018 (0.019)	$-0.022^{\dagger\dagger\dagger}$ (0.023)	0.640	0.467	0.027 (0.030)	-0.048 (0.050)	0.199	0.263
Female ≤ HS	0.001 (0.014)	0.002 (0.026)	0.535	0.486	0.002 (0.025)	0.004 (0.054)	0.170	0.233
< HS, < 25	-0.014 (0.022)	$-0.076^{***\dagger\dagger}$ (0.025)	0.296	0.173	-0.047 (0.073)	-0.440^{***} (0.146)	0.050	0.078
Male < HS, < 25	-0.015 (0.024)	$-0.092^{***\dagger\dagger\dagger}$ (0.026)	0.291	0.157	-0.053 (0.083)	-0.585^{***} (0.164)	0.027	0.043
Female < HS, < 25	-0.011 (0.025)	-0.057^{*} (0.030)	0.303	0.193	-0.035 (0.082)	-0.293^{*} (0.157)	0.023	0.035
≤ HS, < 25	-0.009 (0.019)	$-0.055^{**\dagger\dagger\dagger}$ (0.023)	0.445	0.310	-0.021 (0.043)	-0.179^{**} (0.075)	0.091	0.138
Male ≤ HS, < 25	-0.005 (0.022)	$-0.073^{**\dagger\dagger\dagger}$ (0.027)	0.459	0.296	-0.011 (0.048)	-0.245^{***} (0.091)	0.050	0.075
Female ≤ HS, < 25	-0.016 (0.020)	-0.037 (0.028)	0.429	0.327	-0.038 (0.047)	-0.113 (0.086)	0.041	0.063
< HS, < 30	-0.008 (0.023)	$-0.082^{***\dagger\dagger\dagger}$ (0.026)	0.319	0.201	-0.025 (0.071)	-0.407^{***} (0.130)	0.056	0.091
Male < HS, < 30	-0.004 (0.025)	$-0.087^{***\dagger\dagger\dagger}$ (0.025)	0.326	0.184	-0.013 (0.077)	-0.473^{***} (0.137)	0.030	0.051
Female < HS, < 30	-0.009	$-0.072^{**\dagger\dagger}$	0.311	0.222	-0.027	-0.323^{**}	0.026	0.041

Population	White empl. effect	Black empl. effect	Avg white empl. rate	Avg black empl. rate	White empl. elas.	Black empl. elas.	White sub-pop. share	Black sub-pop. share
	(0.025)	(0.033)			(0.081)	(0.150)		
≤ HS, < 30	-0.001	-0.044††	0.500	0.369	-0.002	-0.119*	0.119	0.186
	(0.018)	(0.026)			(0.036)	(0.071)		
Male ≤ HS, < 30	0.003	-0.057**†††	0.529	0.355	0.007	-0.160**	0.067	0.102
	(0.021)	(0.025)			(0.039)	(0.071)		
Female ≤ HS, < 30	-0.007	-0.025	0.462	0.386	-0.015	-0.065	0.053	0.084
	(0.019)	(0.035)			(0.041)	(0.090)		

Notes: Same as Table 1. The employment regressions are run separately by race. The sub-population shares represent the proportion of individuals within each race that belong to the specified sub-group. These shares are computed as the weighted average of sub-group shares across PUMAs, with weights based on the PUMA population in each year. The daggers represent the statistical significance of the differential effect of a Black PUMA, on employment. For this, the following model is estimated: $Y = \alpha + \beta\cdot\ln(MW) + \beta_B\cdot\ln(MW)\cdot Black + X\delta + X\cdot Black\cdot\delta_B + D_P\cdot\lambda + D_P\cdot Black\cdot\lambda_B + D_T\tau + D_T\cdot Black\cdot\tau_B + \varepsilon$, where Black is an indicator for PUMA cell means for blacks. The daggers represent the statistical significance of the black-minimum wage interaction estimate in a pooled model. The coefficients are statistically significant at the ***1%, **5%, or *10% level. The coefficients are statistically significantly different by race at the †††1%, ††5%, or †10% level.

Table 3: Minimum Wage-Annual Hours Regressions with Separate Effects by Race

Population	White empl. effect	Black empl. effect	Avg white empl. rate	Avg black empl. rate	White empl. elas.	Black empl. elas.
Teens 16-19	-33.004	-94.815**††	340.480	236.436	-0.097	-0.401***
	(25.909)	(35.706)			(0.076)	(0.151)
Male teens	-38.728	-93.983*†	349.646	223.944	-0.111	-0.420**
	(28.750)	(46.829)			(0.082)	(0.209)
Female teens	-27.100	-102.798***††	330.805	249.426	-0.082	-0.412***
	(26.514)	(30.313)			(0.080)	(0.122)
$<$ 25	-21.271	-46.875	799.215	616.308	-0.027	-0.076
	(28.868)	(42.606)			(0.036)	(0.069)
Male $<$ 25	-15.823	-40.740	851.370	603.499	-0.019	-0.068
	(37.529)	(48.361)			(0.044)	(0.080)
Female $<$ 25	-25.373	-51.095	744.297	629.449	-0.034	-0.081
	(24.638)	(42.910)			(0.033)	(0.068)
$<$ HS	-9.471	-92.709**†††	646.968	524.949	-0.015	-0.177**
	(36.147)	(45.870)			(0.056)	(0.087)
Male $<$ HS	-2.798	-97.097*††	792.380	536.366	-0.004	-0.181*
	(46.471)	(50.402)			(0.059)	(0.094)
Female $<$ HS	-14.232	-82.628*†	467.867	511.584	-0.030	-0.162*
	(29.039)	(49.221)			(0.062)	(0.096)
$\leq$ HS	4.227	-53.893††	1148.967	915.429	0.004	-0.059
	(36.892)	(55.787)			(0.032)	(0.061)
Male $\leq$ HS	14.075	-54.991††	1338.484	944.880	0.011	-0.058
	(46.896)	(61.792)			(0.035)	(0.065)
Female $\leq$ HS	-5.096	-50.820	927.393	882.093	-0.005	-0.058
	(26.521)	(49.858)			(0.029)	(0.057)
$<$ HS, $<$ 25	-19.891	-91.035***†††	271.917	199.480	-0.073	-0.456***
	(23.897)	(28.074)			(0.088)	(0.141)
Male $<$ HS, $<$ 25	-20.473	-109.501***†††	304.161	198.392	-0.067	-0.552***
	(29.222)	(34.862)			(0.096)	(0.176)
Female $<$ HS, $<$ 25	-20.542	-56.873*	234.667	200.580	-0.088	-0.284*
	(23.638)	(28.958)			(0.101)	(0.144)
$\leq$ HS, $<$ 25	-30.629	-99.926**††	614.279	465.354	-0.050	-0.215**
	(35.834)	(40.443)			(0.058)	(0.087)
Male $\leq$ HS, $<$ 25	-23.456	-114.114**†††	701.734	472.171	-0.033	-0.242**
	(44.873)	(49.806)			(0.064)	(0.105)
Female $\leq$ HS, $<$ 25	-43.603	-84.902**	507.443	457.154	-0.086	-0.186**
	(30.899)	(38.677)			(0.061)	(0.085)
$<$ HS, $<$ 30	-14.035	-108.878***†††	353.838	277.340	-0.040	-0.393***
	(30.112)	(35.116)			(0.085)	(0.127)
Male $<$ HS, $<$ 30	-7.845	-125.683***†††	416.808	277.659	-0.019	-0.453***
	(37.320)	(43.810)			(0.090)	(0.158)
Female $<$ HS, $<$ 30	-19.313	-73.754**	279.752	276.761	-0.069	-0.266**
	(27.544)	(36.655)			(0.098)	(0.132)
$\leq$ HS, $<$ 30	-23.718	-99.266*††	795.154	622.295	-0.030	-0.160*
	(35.164)	(53.287)			(0.044)	(0.086)
Male $\leq$ HS, $<$ 30	-21.514	-113.106*†††	927.063	634.173	-0.023	-0.178*
	(43.795)	(57.882)			(0.047)	(0.091)
Female $\leq$ HS, $<$ 30	-27.354	-76.461	628.054	607.843	-0.044	-0.126
	(31.900)	(56.496)			(0.051)	(0.093)

Notes: Same as Table 2. The employment measure is usual hours worked in the past 12 months.

Table 4: Minimum Wage-Employment Regressions by Race, TWFE and 2S-DID, 2012-19

Population	TWFE White	TWFE Black	2S-DID White	2S-DID Black
Teens 16-19	-0.025	-0.060***	-0.006	-0.039
	(0.018)	(0.019)	(0.018)	(0.028)
Male teens	-0.023	-0.065**	-0.006	-0.038
	(0.022)	(0.025)	(0.019)	(0.033)
Female teens	-0.026	-0.067***	-0.008	-0.037
	(0.020)	(0.025)	(0.020)	(0.034)
< 25	-0.011	-0.020	0.005	-0.015
	(0.013)	(0.017)	(0.015)	(0.021)
Male < 25	-0.003	-0.042**	0.015	-0.024
	(0.016)	(0.018)	(0.017)	(0.021)
Female < 25	-0.019	0.001	-0.005	-0.005
	(0.017)	(0.022)	(0.016)	(0.027)
< HS	-0.003	-0.049**	0.018	-0.019
	(0.020)	(0.019)	(0.017)	(0.028)
Male < HS	0.014	-0.051**	0.034*	-0.017
	(0.024)	(0.021)	(0.019)	(0.022)
Female < HS	-0.018	-0.040	-0.002	-0.013
	(0.019)	(0.027)	(0.017)	(0.039)
≤ HS	0.010	-0.005	0.016	0.006
	(0.017)	(0.022)	(0.013)	(0.025)
Male ≤ HS	0.020	-0.012	0.026*	0.009
	(0.018)	(0.020)	(0.014)	(0.024)
Female ≤ HS	-0.00004	0.005	0.006	0.013
	(0.017)	(0.026)	(0.012)	(0.029)
< HS, < 25	-0.026	-0.076***	-0.008	-0.034
	(0.021)	(0.023)	(0.021)	(0.031)
Male < HS, < 25	-0.024	-0.101***	-0.008	-0.067**
	(0.025)	(0.023)	(0.022)	(0.027)
Female < HS, < 25	-0.029	-0.048	-0.012	-0.021
	(0.025)	(0.029)	(0.024)	(0.042)
≤ HS, < 25	-0.017	-0.057***	-0.005	-0.041
	(0.020)	(0.019)	(0.018)	(0.030)
Male ≤ HS, < 25	-0.010	-0.080***	0.002	-0.064**
	(0.023)	(0.022)	(0.020)	(0.025)
Female ≤ HS, < 25	-0.026	-0.033	-0.016	-0.021
	(0.021)	(0.026)	(0.019)	(0.044)
< HS, < 30	-0.020	-0.087***	0.004	-0.048
	(0.023)	(0.023)	(0.022)	(0.034)
Male < HS, < 30	-0.010	-0.094***	0.008	-0.060**
	(0.028)	(0.023)	(0.023)	(0.029)
Female < HS, < 30	-0.029	-0.073**	-0.003	-0.042
	(0.025)	(0.036)	(0.025)	(0.052)
≤ HS, < 30	-0.011	-0.044*	0.001	-0.023
	(0.018)	(0.022)	(0.016)	(0.032)
Male ≤ HS, < 30	-0.004	-0.053**	0.007	-0.030
	(0.021)	(0.022)	(0.017)	(0.026)
Female ≤ HS, < 30	-0.021	-0.030	-0.009	-0.011
	(0.020)	(0.034)	(0.019)	(0.050)

Notes: Same as Table 1. The TWFE and 2S-DID regressions are run separately by race. The sample is restricted to 2012-19. The treatment considered in the second-stage in the 2S-DID regressions is the continuous variation in minimum wages across PUMAs over years, as in the preceding tables.

Table 5: Minimum Wage-Employment Regressions by Race, TWFE and Estimates with Census Division-by-Period Dummies and State-Specific Linear Trends

Population	TWFE White	TWFE Black	Division-time effects White	Division-time effects Black	State linear trends White	State linear trends Black	Division-time effects and state linear trends White	Division-time effects and state linear trends Black
Male teens	-0.023	-0.077*†	-0.007	-0.085*††	-0.034	-0.080	-0.034	-0.092
	(0.023)	(0.042)	(0.026)	(0.044)	(0.031)	(0.052)	(0.020)	(0.056)
Male < HS, < 25	-0.015	-0.092**††	0.004	-0.079*†	-0.029	-0.106**††	-0.025	-0.097*
	(0.026)	(0.038)	(0.032)	(0.045)	(0.029)	(0.045)	(0.026)	(0.057)
Male ≤ HS, < 25	-0.002	-0.072*††	0.017	-0.070††	-0.011	-0.074	-0.001	-0.070
	(0.024)	(0.038)	(0.026)	(0.042)	(0.030)	(0.047)	(0.025)	(0.052)
Male < HS, < 30	-0.0004	-0.087**††	0.009	-0.052	-0.013	-0.112**†††	-0.020	-0.080
	(0.026)	(0.038)	(0.032)	(0.047)	(0.029)	(0.042)	(0.026)	(0.064)
Male ≤ HS, < 30	0.006	-0.066*††	0.012	-0.047†	0.006	-0.079††	-0.001	-0.065
	(0.022)	(0.038)	(0.023)	(0.039)	(0.029)	(0.053)	(0.025)	(0.057)

Notes: Same as Table 1. The regressions are run separately by race, and the analysis is at the state level. In contrast to Allegretto et al. (2011), we do not include an unemployment rate control, but results were not sensitive to including the unemployment rate for 25-54 year-old college-educated males. The column headings explain the other modifications to the specification, following Allegretto et al. (2011). "Division-time effects" refers to interactions between year dummy variables and dummy variables for each of nine Census divisions. "State linear trends" refers to the inclusion of state specific linear trends based on year.

Table 6: Minimum Wage-Employment Regressions by Race, TWFE and Estimates with Cross-Border County Pair-by-Period Dummies

Population	TWFE effect-White	TWFE effect-Black	County pair-time effect- White	County pair-time effect- Black
Male teens	-0.034	$-0.155^{**\dagger\dagger}$	-0.010	-0.053
	(0.038)	(0.062)	(0.048)	(0.095)
Male < HS, < 25	-0.020	$-0.192^{***\dagger\dagger}$	-0.048	-0.184^{*}
	(0.046)	(0.052)	(0.052)	(0.100)
Male ≤ HS, < 25	-0.014	$-0.111^{**\dagger}$	-0.012	$-0.181^{***\dagger\dagger}$
	(0.036)	(0.053)	(0.038)	(0.056)
Male < HS, < 30	-0.033	$-0.198^{***\dagger\dagger}$	-0.061	-0.198
	(0.045)	(0.046)	(0.065)	(0.125)
Male ≤ HS, < 30	-0.004	-0.093^{*}	0.005	$-0.163^{**\dagger\dagger}$
	(0.034)	(0.048)	(0.045)	(0.062)

Notes: Same as Table 1.

Table 7: Minimum Wage-Employment Regressions by Race, TWFE and Estimates with Multi-State Commuting Zone-by-Period Dummies

Population	TWFE White	TWFE Black	CZ-time effects White	CZ-time effects Black
Male teens	-0.023	-0.092**††	-0.125	-0.100
	(0.026)	(0.042)	(0.093)	(0.123)
Male < HS, < 25	-0.022	-0.107***††	-0.066	-0.320***
	(0.027)	(0.036)	(0.086)	(0.105)
Male ≤ HS, < 25	-0.003	-0.084**††	-0.074	-0.324**†
	(0.025)	(0.038)	(0.067)	(0.123)
Male < HS, < 30	-0.006	-0.104***†††	-0.090	-0.361***
	(0.027)	(0.036)	(0.087)	(0.120)
Male ≤ HS, < 30	0.007	-0.070*†††	-0.048	-0.290**†
	(0.023)	(0.038)	(0.051)	(0.115)

Notes: Same as Table 1.

Table 8: Testing for Pre-trends in Race-Specific Employment Effects of Minimum Wage Changes

Population	White Second Lead (β_{-2})	Black Second Lead (β_{-2})	White First Lead (β_{-1})	Black First Lead (β_{-1})	White Current Effect (β_0)	Black Current Effect (β_0)	White First Lag (β_1)	Black First Lag (β_1)	White Cumulative Lag (β_2)	Black Cumulative Lag (β_2)
Male teens	0.060	0.005	-0.024	0.265***††	0.103*	0.112	-0.139**	-0.100	-0.032	-0.246***††
	(0.048)	(0.075)	(0.053)	(0.084)	(0.055)	(0.082)	(0.053)	(0.061)	(0.050)	(0.069)
Male < HS, < 25	-.0051	0.024	-0.138**	0.130*†††	0.073	0.039	-0.135**	-0.127	-0.085	-0.148
	(0.069)	(0.111)	(0.056)	(0.066)	(0.069)	(0.085)	(0.066)	(0.144)	(0.061)	(0.099)
Male ≤ HS, < 25	-0.003	-0.025	0.014	0.052	0.051	-0.013	-0.112*	-0.118	0.0004	-0.144
	(0.038)	(0.082)	(0.052)	(0.085)	(0.068)	(0.061)	(0.058)	(0.096)	(0.060)	(0.097)
Male < HS, < 30	-0.043	0.042	-0.111**	0.091††	0.080	0.093	-0.118*	-0.153	-0.091	-0.177*
	(0.074)	(0.099)	(0.053)	(0.064)	(0.076)	(0.102)	(0.065)	(0.136)	(0.056)	(0.096)
Male ≤ HS, < 30	-0.014	0.001	0.022	0.019	0.023	-0.047	-0.110*	-0.113	-0.018	-0.042
	(0.041)	(0.082)	(0.043)	(0.095)	(0.068)	(0.074)	(0.058)	(0.090)	(0.051)	(0.100)

Notes: This table reports the coefficient from the distributed-lag/lead specification described in the text, estimated separately by race. The daggers represent the statistical significance of the corresponding White–Black difference, obtained by estimating the same specification on the pooled sample with all regressors interacted with an indicator for black race. The coefficients are statistically significant at the ***1%, **5%, or *10% level. The coefficients are statistically significantly different by race at the †††1%, ††5%, or †10% level.

Table 9: Employment, Wage, and Own-Wage Elasticities Across Demographic Sub-groups

Population	White Employment elasticity	Black Employment elasticity	White Wage elasticity	Black Wage elasticity	White Own-wage elasticity	Black Own-wage elasticity
Teens 16-19	-0.054	-0.263	0.148	0.107	-0.366	-2.459
Male teens	-0.074	-0.337	0.109	0.038	-0.679	-8.767
Female teens	-0.033	-0.220	0.184	0.158	-0.179	-1.397
<25	-0.009	-0.059	0.084	0.045	-0.108	-1.307
Male <25	-0.003	-0.088	0.058	-0.023	-0.051	N/A
Female <25	-0.013	-0.036	0.113	0.114	-0.115	-0.317
< HS	0.009	-0.166	0.074	0.046	0.122	-3.626
Male < HS	0.033	-0.192	0.066	0.047	0.502	-4.077
Female < HS	-0.021	-0.131	0.083	0.027	-0.253	-4.817
≤ HS	0.016	-0.024	0.024	-0.003	0.671	N/A
Male ≤ HS	0.027	-0.048	0.017	0.012	1.611	-4.084
Female ≤ HS	0.002	0.004	0.038	-0.024	0.053	N/A
< HS, < 25	-0.047	-0.440	0.168	0.127	-0.279	-3.471
Male < HS, < 25	-0.053	-0.585	0.180	-0.025	-0.294	N/A
Female < HS, < 25	-0.035	-0.293	0.159	0.252	-0.220	-1.162
≤ HS, < 25	-0.021	-0.179	0.091	0.008	-0.231	-22.590
Male ≤ HS, < 25	-0.011	-0.245	0.056	-0.079	-0.197	N/A
Female ≤ HS, < 25	-0.038	-0.113	0.141	0.126	-0.269	-0.896
< HS, < 30	-0.025	-0.407	0.145	0.076	-0.173	-5.345
Male < HS, < 30	-0.013	-0.473	0.160	-0.027	-0.081	N/A
Female < HS, < 30	-0.027	-0.323	0.132	0.135	-0.204	-2.401
≤ HS, < 30	-0.002	-0.119	0.067	-0.018	-0.030	N/A
Male ≤ HS, < 30	0.007	-0.160	0.052	-0.039	0.135	N/A
Female ≤ HS, < 30	-0.015	-0.065	0.096	-0.004	-0.156	N/A

Notes: This table reports estimated employment elasticities, wage elasticities, and the resulting own-wage elasticities of employment (OWE). OWE is calculated as the ratio of the employment elasticity to the wage elasticity. Values for OWE are reported as N/A for subgroups where the estimated wage elasticity is non-positive, as a positive wage effect (a binding first-stage policy treatment) is required to identify the own-wage response.

Table 10: Minimum Wage-Earnings Regressions with Separate Effects by Race

Population	White earnings effect	Black earnings effect	Avg white earnings	Avg black earnings	White earnings elas.	Black earnings elas.
Teens 16-19	133.228	-807.539**†††	2640.006	1923.255	0.050	-0.420**
	(277.684)	(384.990)			(0.105)	(0.200)
Male teens	-17.329	-942.795*††	2822.201	1871.527	-0.006	-0.504*
	(331.104)	(529.015)			(0.117)	(0.283)
Female teens	301.056	-788.884**†††	2447.676	1976.453	0.123	-0.399**
	(274.964)	(331.449)			(0.112)	(0.168)
< 25	1258.763**	266.912†	8711.407	6210.252	0.144**	0.043
	(562.903)	(707.141)			(0.065)	(0.114)
Male < 25	1039.829	14.674†	9653.250	6246.818	0.108	0.002
	(688.045)	(812.109)			(0.071)	(0.130)
Female < 25	1520.116***	520.406	7719.673	6173.127	0.197***	0.084
	(476.669)	(664.003)			(0.062)	(0.108)
< HS	775.485	81.492	8744.003	6501.137	0.089	0.013
	(588.061)	(575.643)			(0.067)	(0.089)
Male < HS	1183.521	-64.842	11631.050	7067.748	0.102	-0.009
	(1048.049)	(693.022)			(0.090)	(0.098)
Female < HS	343.163	410.400	5188.055	5839.852	0.066	0.070
	(341.634)	(750.622)			(0.066)	(0.129)
≤ HS	1320.934	919.984	18763.500	12892.900	0.070	0.071
	(841.555)	(581.615)			(0.045)	(0.045)
Male ≤ HS	1778.575	1066.254	23751.160	14121.590	0.075	0.076
	(1140.949)	(680.530)			(0.048)	(0.048)
Female ≤ HS	725.609	833.329	12932.150	11502.650	0.056	0.072
	(502.276)	(622.930)			(0.039)	(0.054)
< HS, < 25	166.420	-730.862**††	2271.276	1709.716	0.073	-0.427**
	(317.538)	(289.600)			(0.140)	(0.169)
Male < HS, < 25	208.151	-1272.419***†††	2685.466	1748.244	0.078	-0.728***
	(436.233)	(345.296)			(0.162)	(0.198)
Female < HS, < 25	109.371	-52.013	1792.752	1661.097	0.061	-0.031
	(241.502)	(288.908)			(0.135)	(0.174)
≤ HS, < 25	105.937	-784.035*	5973.392	4336.447	0.018	-0.181*
	(565.534)	(467.019)			(0.095)	(0.108)
Male ≤ HS, < 25	-12.816	-1435.864**††	7236.296	4569.074	-0.002	-0.314**
	(737.607)	(587.318)			(0.102)	(0.129)
Female ≤ HS, < 25	219.252	-34.674	4430.594	4057.232	0.049	-0.009
	(435.275)	(535.195)			(0.098)	(0.132)
< HS, < 30	-13.838	-662.550*†	3373.879	2675.181	-0.004	-0.248*
	(386.192)	(348.643)			(0.114)	(0.130)
Male < HS, < 30	95.970	-1578.094***†††	4260.728	2769.691	0.023	-0.570***
	(568.774)	(487.229)			(0.133)	(0.176)
Female < HS, < 30	-99.412	512.982	2330.450	2557.319	-0.043	0.201
	(239.542)	(370.106)			(0.103)	(0.145)
≤ HS, < 30	302.215	-338.180	9073.687	6705.336	0.033	-0.050
	(625.470)	(558.156)			(0.069)	(0.083)
Male ≤ HS, < 30	353.522	-732.983††	11307.820	7141.284	0.031	-0.103
	(808.661)	(640.198)			(0.072)	(0.090)
Female ≤ HS, < 30	267.914	173.231	6243.546	6175.748	0.043	0.028
	(460.488)	(677.141)			(0.074)	(0.110)

Notes: Same as Table 2.

Table 11: Minimum Wage-Earnings Regressions by Race, TWFE and 2S-DID from 2012-19

Population	TWFE White	TWFE Black	2S-DID White	2S-DID Black
Teens 16-19	58.355	-968.256**	175.321	-744.338*
	(323.101)	(371.437)	(312.064)	(428.150)
Male teens	69.072	-962.913	213.864	-825.763
	(367.940)	(618.726)	(318.312)	(585.318)
Female teens	27.977	-1139.223***	127.073	-822.401**
	(350.304)	(340.293)	(384.203)	(333.096)
< 25	1288.224**	322.736	1371.291**	229.372
	(544.888)	(701.282)	(556.099)	(720.148)
Male < 25	1011.874	-26.491	1337.224**	-385.688
	(662.588)	(814.279)	(647.993)	(801.350)
Female < 25	1615.168***	711.538	1547.795***	990.892
	(481.491)	(745.695)	(477.725)	(676.120)
< HS	661.082	139.996	1167.313**	538.137
	(601.124)	(482.865)	(580.359)	(625.345)
Male < HS	764.887	-59.869	1520.27	8.928
	(1109.379)	(658.515)	(1006.751)	(892.728)
Female < HS	604.247	684.432	787.271*	1400.017**
	(462.448)	(749.301)	(418.102)	(638.152)
≤ HS	1534.616**	1209.18**	1592.488***	1390.849**
	(749.086)	(590.194)	(580.338)	(574.901)
Male ≤ HS	1546.743	1232.018*	1574.68*	1070.709
	(988.962)	(671.183)	(872.593)	(783.637)
Female ≤ HS	1387.146**	1318.188*	1513.405***	2050.701***
	(523.433)	(692.199)	(370.016)	(570.069)
< HS, < 25	-66.387	-918.981***	308.695	-307.870
	(354.928)	(237.130)	(321.539)	(347.536)
Male < HS, < 25	-137.608	-1599.174***	516.641	-1003.128***
	(484.031)	(288.353)	(424.926)	(332.490)
Female < HS, < 25	-138.250	-112.672	31.875	370.285
	(319.695)	(285.136)	(280.840)	(387.632)
≤ HS, < 25	-72.137	-1003.898**	303.298	-946.657
	(656.146)	(394.647)	(573.654)	(638.033)
Male ≤ HS, < 25	-361.728	-1643.551**	267.501	-1910.329**
	(875.073)	(634.685)	(736.799)	(761.150)
Female ≤ HS, < 25	144.118	-232.485	337.321	-105.267
	(514.927)	(470.697)	(487.527)	(521.194)
< HS, < 30	-338.905	-756.832***	157.967	-441.352
	(416.101)	(253.512)	(427.357)	(352.766)
Male < HS, < 30	-521.890	-1880.144***	279.432	-1216.295***
	(613.038)	(363.120)	(597.290)	(448.712)
Female < HS, < 30	-170.849	644.010	42.332	674.397
	(297.141)	(433.888)	(273.819)	(493.573)
≤ HS, < 30	-91.155	-399.544	148.530	-533.128
	(634.986)	(432.087)	(627.546)	(489.799)
Male ≤ HS, < 30	-441.813	-671.269	-19.454	-1263.665**
	(810.091)	(570.362)	(739.334)	(585.475)
Female ≤ HS, < 30	272.010	47.181	263.756	394.498
	(492.788)	(566.145)	(487.899)	(574.072)

Notes: Same as Table 4.

Table 12: Minimum Wage-Employment Regressions with Separate Effects by Race & an Indicator for Areas Above the 95th Percentile in Black Population Share

	Empl. effect, white (β)	Black-MW interaction ($β_B$)	%Black95-MW interaction ($β_{\%B}$)	Effect by percentile %Black	Avg. white empl. rate	Avg. black empl. rate	White empl. elas.	Black empl. elas.	Black − white empl. elas.
Teens Male	-0.024	-0.021	-0.053	<95th	0.340	0.218	-0.071	-0.209	-0.138
	(0.020)	(0.025)	(0.033)				(0.059)	(0.158)	
				>95th	0.245	0.157	-0.313**	-0.627***	-0.314
							(0.123)	(0.153)	
< HS, < 25, Males	-0.014	-0.052*	-0.063**	<95th	0.291	0.167	-0.047	-0.393**	-0.346
	(0.025)	(0.030)	(0.027)				(0.085)	(0.188)	
				>95th	0.219	0.128	-0.352**	-1.008***	-0.656
							(0.152)	(0.190)	
≤ HS, < 25, Males	-0.003	-0.053*	-0.046	<95th	0.459	0.307	-0.007	-0.181*	-0.174
	(0.022)	(0.029)	(0.030)				(0.049)	(0.104)	
				>95th	0.386	0.259	-0.127	-0.391***	-0.264
							(0.092)	(0.119)	
< HS, < 30, Males	-0.006	-0.055*	-0.057*	<95th	0.326	0.192	-0.020	-0.317**	-0.297
	(0.026)	(0.029)	(0.031)				(0.080)	(0.154)	
				>95th	0.269	0.158	-0.236	-0.746***	-0.509
							(0.153)	(0.187)	
≤ HS, < 30, Males	0.007	-0.048*	-0.033	<95th	0.530	0.362	0.013	-0.113	-0.127
	(0.022)	(0.026)	(0.029)				(0.042)	(0.078)	
				>95th	0.460	0.332	-0.057	-0.223**	-0.167
							(0.087)	(0.103)	

Notes: The sample consists of ACS micro-data from 2005-2019 restricting to those aged between 16 to 65. Employment estimates are from linear probability models for an indicator of employment. The demographic controls included are race, sex, number of children, marital status, age and education. Additional controls include an indicator for PUMAs with a Black population share above the 95th percentile in each year. Fixed effects are at PUMA and year level. All controls and fixed effects are interacted with race and high-black-share indicators. Percentiles of share black are computed using individual-level data weighted by individual person weights. Employment elasticity for each population group is computed by dividing the employment effect (β) by the average employment rate of the group. ACS person sampling weights are used in the regressions and employment rate calculations. Reported standard errors are clustered at the state level.

Table 13: "Simulated" Minimum Wage Effects on Employment in White Areas (black share < 95th percentile) vs. Black Areas (black share > 95th percentile), Increase from $7.25 (Federal Minimum Wage) to $12 (California Minimum Wage in 2019)

A. Population shares and employment descriptives

	Male Teens		Male < HS, < 30	
Share black percentile	**<95th**	**>95th**	**<95th**	**>95th**
White sub-population share	0.826	0.147	0.791	0.100
Black sub-population share	0.174	0.853	0.209	0.900
White employment rate	0.340	0.245	0.326	0.269
Black employment rate	0.218	0.157	0.192	0.158
Weighted employment rate	0.318	0.170	0.298	0.169

B. Homogeneous effects by black share

	Male Teens		Male < HS, < 30	
Share black percentile	**<95th**	**>95th**	**<95th**	**>95th**
White MW-empl. elas.	-0.073	-0.101	-0.021	-0.026
Black MW-empl. elas.	-0.323	-0.449	-0.438	-0.532
Weighted empl. elas.	**-0.116**	**-0.398**	**-0.108**	**-0.481**
Impact of MW increase ($7.25 to $12) on empl. rate	**-0.021**	**-0.042**	**-0.015**	**-0.050**

C. Heterogeneous effects by black share

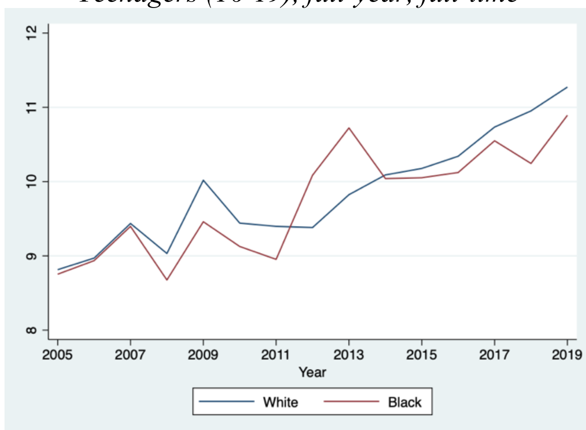
	Male Teens		Male < HS, < 30	
Share black percentile	**<95th**	**>95th**	**<95th**	**>95th**
White MW-empl. elas.	-0.071	-0.313	-0.020	-0.236
Black MW-empl. elas.	-0.209	-0.627	-0.317	-0.746
Weighted empl. elas.	**-0.095**	**-0.581**	**-0.082**	**-0.695**
Impact of MW increase ($7.25 to $12) on empl. rate	**-0.018**	**-0.062**	**-0.012**	**-0.074**

Notes: All measures and percentiles are based on individual-level data using ACS person weights. "Sub-population shares" include only blacks and white, and refer to shares among teens, male teens and high school dropouts under 30. Minimum wage-employment elasticities for homogeneous effect by Black share are based on estimates of the similar model as in Table 2, but estimated at the individual level, and for heterogeneous effect by Black share are based on estimates in Table 12. Weighted employment rate and weighted elasticities are based on the sub-population shares. The last row is computed using separate elasticities and employment rates by race.

Appendix: Figures and Tables

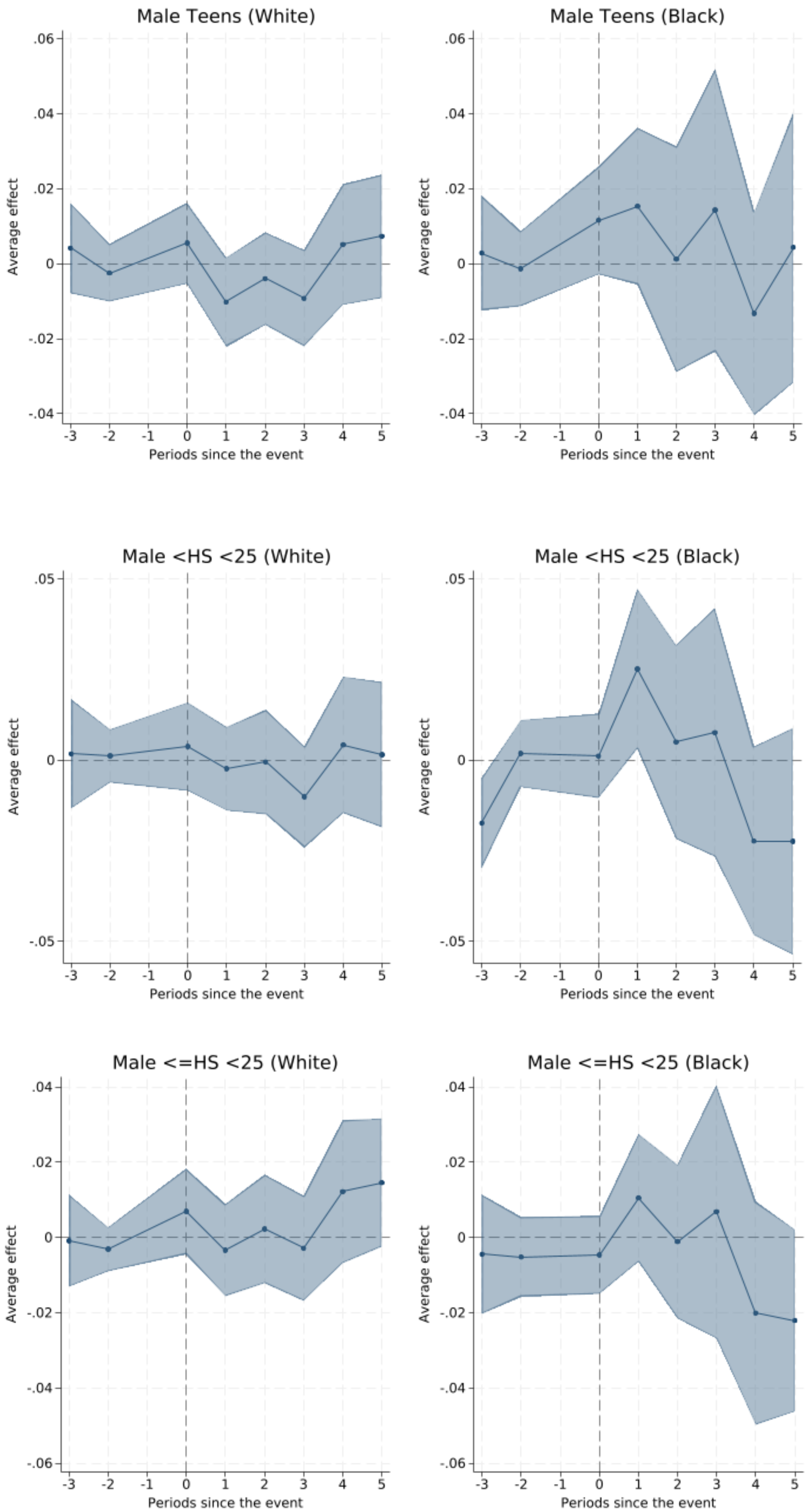
Appendix Figure A1: Hourly Wages by Year, Blacks and Whites

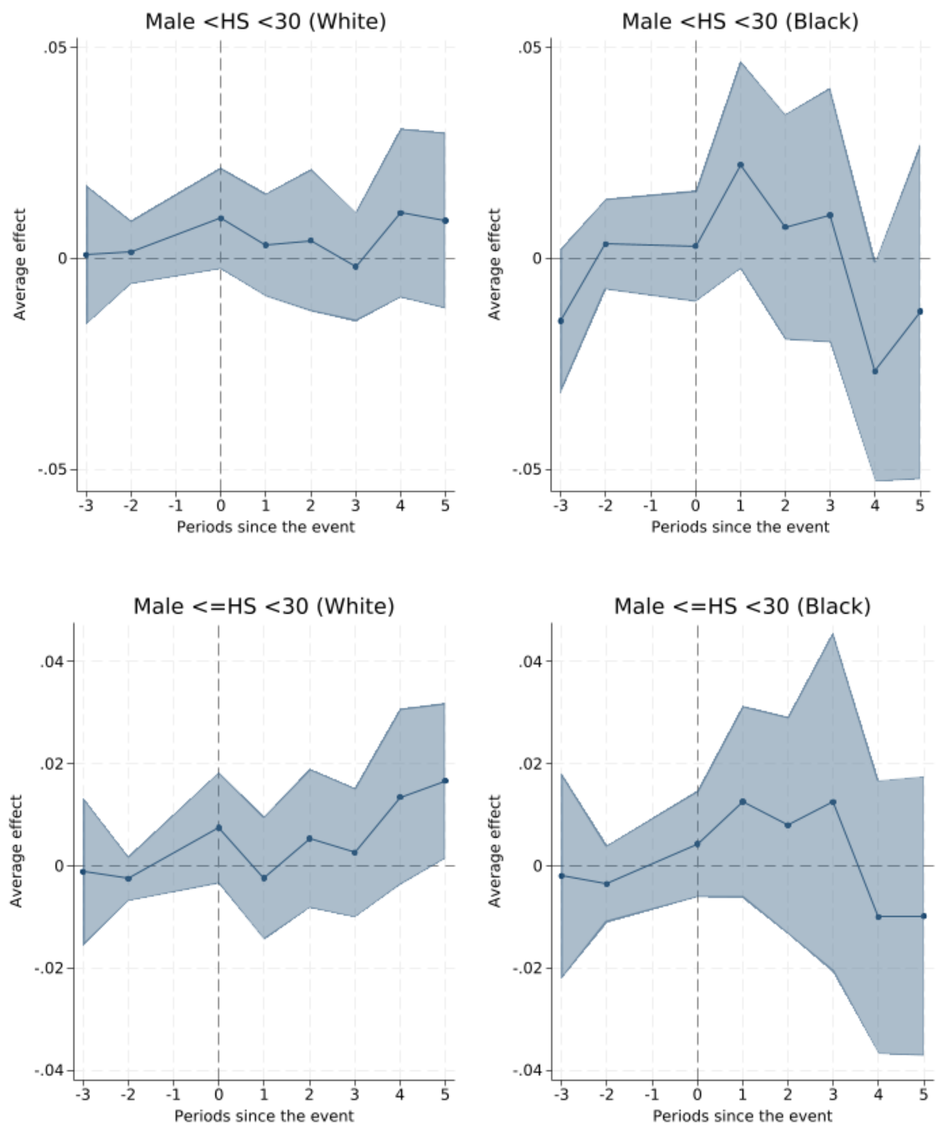
Teenagers (16-19), full-year, full-time



Note: Wages in each year are in nominal terms (in the respective year's dollar value) and weighted by individual person weights.

Appendix Figure A2: Event-study Graphs for Select Groups (Discrete Treatment, 2012-19)

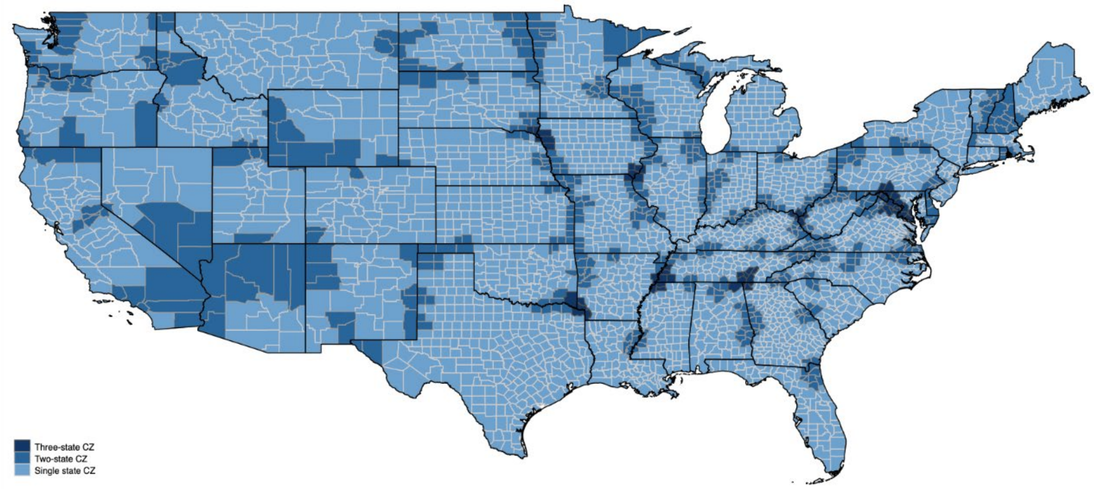




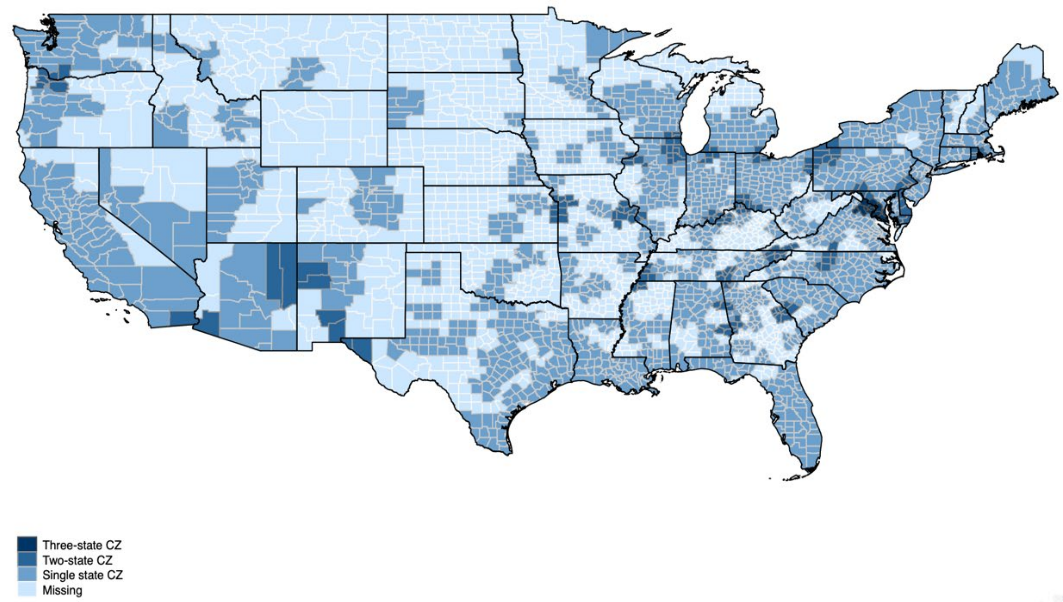
Note: The event-study estimates exclude one year prior to treatment, thereby constraining the pre-treatment coefficients to zero. However, the graph displays a continuous line connecting all the estimates, which may give the appearance of a non-zero value in the pre-treatment year, even though no estimate is reported for that period.

Appendix Figure A3: US Map with county and state boundaries showing single and multi-state commuting zones

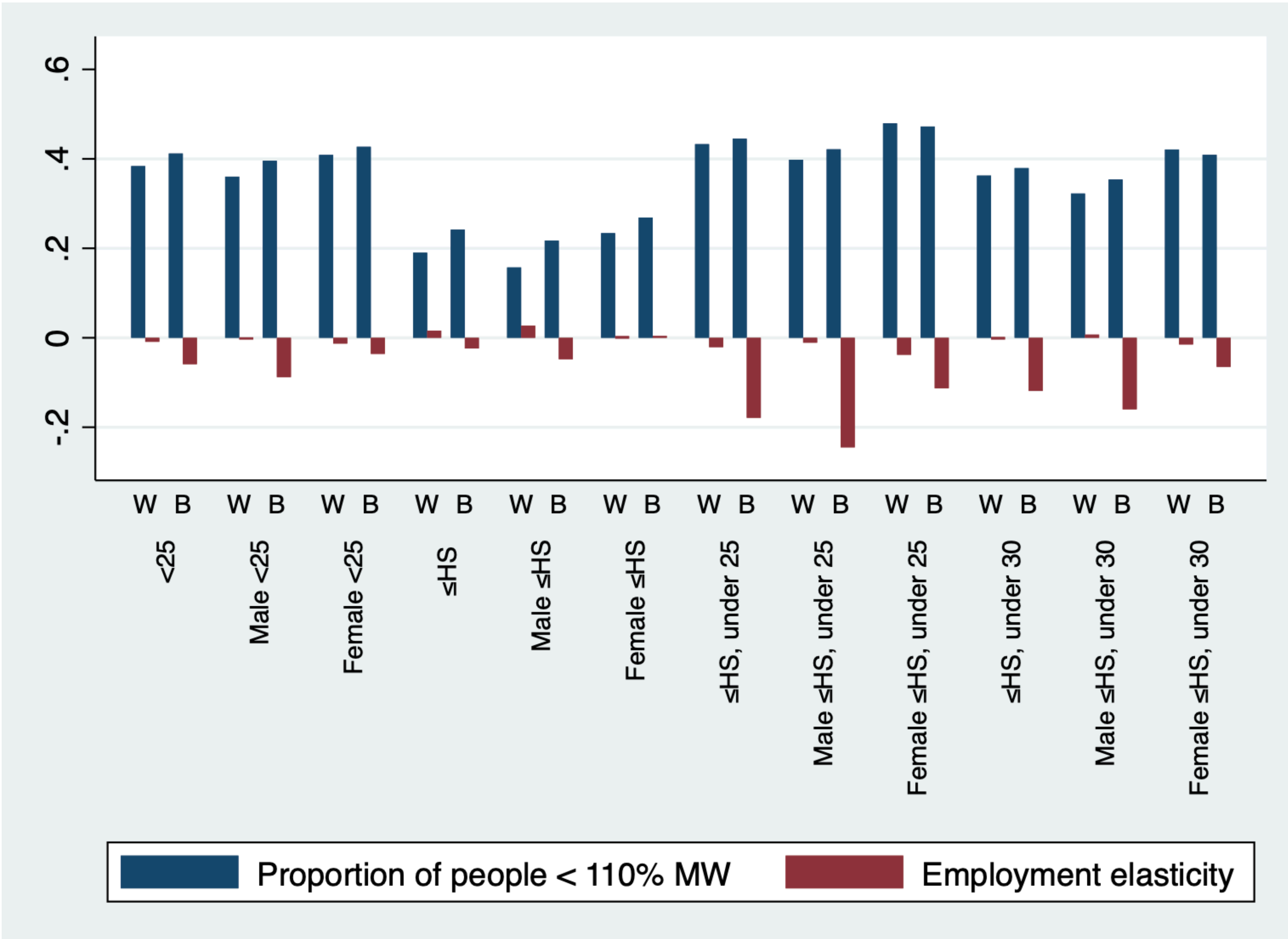
A. Areas with all commuting zones



B. Areas where each PUMA maps uniquely to a single commuting zone



Appendix Figure A4: Shares below 110% of the Minimum Wage and Estimated Employment Elasticities



Notes: This figure provides the same kind of information as in Figure 13, for the groups not covered in Figure 13.

Appendix Table A1: Minimum Wage-Employment Regressions with Separate Effects by Race, Subgroups excluding Teens

Population	White empl. effect	Black empl. effect	Avg white empl. rate	Avg black empl. rate	White empl. elas.	Black empl. elas.
< 25	0.007	-0.0003	0.694	0.557	0.011	-0.001
	(0.013)	(0.023)			(0.019)	(0.042)
Male < 25	0.017	-0.011	0.695	0.527	0.025	-0.022
	(0.017)	(0.027)			(0.025)	(0.051)
Female < 25	-0.002	0.008	0.693	0.588	-0.003	0.013
	(0.014)	(0.028)			(0.020)	(0.048)
< HS	0.013	-0.031††	0.480	0.374	0.028	-0.084
	(0.017)	(0.025)			(0.036)	(0.066)
Male < HS	0.034	-0.042†††	0.551	0.362	0.062	-0.117
	(0.025)	(0.028)			(0.046)	(0.078)
Female < HS	-0.013	-0.019	0.385	0.389	-0.034	-0.049
	(0.014)	(0.029)			(0.037)	(0.075)
≤ HS	0.016	-0.001	0.640	0.531	0.025	-0.002
	(0.015)	(0.025)			(0.023)	(0.048)
Male ≤ HS	0.025	-0.015†††	0.699	0.523	0.036	-0.029
	(0.019)	(0.025)			(0.028)	(0.048)
Female ≤ HS	0.005	0.016	0.572	0.540	0.009	0.031
	(0.011)	(0.028)			(0.020)	(0.051)
< HS, < 25	0.055	-0.014	0.489	0.296	0.113	-0.048
	(0.040)	(0.046)			(0.081)	(0.156)
Male < HS, < 25	0.059	-0.057†	0.535	0.265	0.110	-0.216
	(0.055)	(0.048)			(0.103)	(0.182)
Female < HS, < 25	0.056	0.009	0.424	0.341	0.133	0.025
	(0.056)	(0.092)			(0.133)	(0.270)
≤ HS, < 25	0.025	-0.023	0.652	0.476	0.039	-0.048
	(0.021)	(0.029)			(0.032)	(0.060)
Male ≤ HS, < 25	0.030	-0.042†	0.685	0.453	0.044	-0.093
	(0.030)	(0.035)			(0.044)	(0.078)
Female ≤ HS, < 25	0.012	-0.007	0.605	0.506	0.020	-0.014
	(0.026)	(0.047)			(0.042)	(0.094)
< HS, < 30	0.041	-0.061*††	0.499	0.325	0.083	-0.189*
	(0.035)	(0.035)			(0.070)	(0.106)
Male < HS, < 30	0.069	-0.067*†††	0.565	0.293	0.123	-0.230**
	(0.047)	(0.034)			(0.084)	(0.117)
Female < HS, < 30	0.008	-0.045	0.409	0.369	0.018	-0.123
	(0.037)	(0.048)			(0.090)	(0.129)
≤ HS, < 30	0.023	-0.018	0.662	0.504	0.034	-0.036
	(0.019)	(0.031)			(0.029)	(0.062)
Male ≤ HS, < 30	0.028	-0.034††	0.711	0.483	0.039	-0.070
	(0.026)	(0.029)			(0.036)	(0.059)
Female ≤ HS, < 30	0.015	0.007	0.593	0.531	0.025	0.014
	(0.022)	(0.048)			(0.038)	(0.090)

Notes: Same as Table 2.

Appendix Table A2: Minimum Wage-Annual Employment Regressions with Separate Effects by Race

Population	White empl. effect	Black empl. effect	Avg white empl. rate	Avg black empl. rate	White empl. elas.	Black empl. elas.
Teens 16-19	0.005	-0.050*†††	0.552	0.357	0.009	-0.139*
	(0.022)	(0.028)			(0.041)	(0.075)
Male teens	-0.004	-0.078**†††	0.539	0.333	-0.008	-0.235**
	(0.023)	(0.031)			(0.043)	(0.094)
Female teens	0.015	-0.021	0.565	0.381	0.026	-0.055
	(0.025)	(0.030)			(0.044)	(0.079)
< 25	0.014	-0.013	0.715	0.550	0.020	-0.023
	(0.016)	(0.025)			(0.022)	(0.045)
Male < 25	0.013	-0.032††	0.714	0.525	0.019	-0.062
	(0.017)	(0.026)			(0.024)	(0.050)
Female < 25	0.016	0.007	0.716	0.575	0.022	0.012
	(0.016)	(0.028)			(0.022)	(0.048)
< HS	0.014	-0.039††	0.506	0.398	0.029	-0.099
	(0.019)	(0.029)			(0.038)	(0.072)
Male < HS	0.023	-0.041†††	0.554	0.394	0.041	-0.103
	(0.021)	(0.030)			(0.037)	(0.077)
Female < HS	0.006	-0.037†	0.446	0.402	0.014	-0.091
	(0.021)	(0.031)			(0.047)	(0.077)
≤ HS	0.016	-0.008	0.682	0.573	0.023	-0.015
	(0.014)	(0.026)			(0.021)	(0.046)
Male ≤ HS	0.021	-0.016††	0.735	0.573	0.028	-0.027
	(0.015)	(0.027)			(0.021)	(0.047)
Female ≤ HS	0.010	-0.001	0.619	0.574	0.017	-0.001
	(0.013)	(0.026)			(0.022)	(0.046)
< HS, < 25	-0.002	-0.072**†††	0.447	0.285	-0.004	-0.252**
	(0.025)	(0.030)			(0.056)	(0.106)
Male < HS, < 25	0.001	-0.090**†††	0.451	0.274	0.001	-0.327***
	(0.027)	(0.034)			(0.060)	(0.123)
Female < HS, < 25	-0.001	-0.045	0.443	0.298	-0.003	-0.152
	(0.027)	(0.037)			(0.062)	(0.123)
≤ HS, < 25	0.012	-0.048*†††	0.604	0.444	0.020	-0.108*
	(0.021)	(0.027)			(0.035)	(0.061)
Male ≤ HS, < 25	0.013	-0.076***†††	0.622	0.433	0.021	-0.176***
	(0.022)	(0.028)			(0.036)	(0.064)
Female ≤ HS, < 25	0.010	-0.013	0.583	0.456	0.017	-0.028
	(0.023)	(0.034)			(0.040)	(0.075)
< HS, < 30	0.005	-0.073**†††	0.469	0.321	0.011	-0.227**
	(0.025)	(0.032)			(0.053)	(0.100)
Male < HS, < 30	0.009	-0.075**†††	0.484	0.309	0.018	-0.242**
	(0.026)	(0.036)			(0.053)	(0.116)
Female < HS, < 30	0.006	-0.066*††	0.450	0.335	0.014	-0.197*
	(0.028)	(0.037)			(0.063)	(0.110)
≤ HS, < 30	0.018	-0.041†††	0.646	0.503	0.029	-0.081
	(0.019)	(0.030)			(0.029)	(0.060)
Male ≤ HS, < 30	0.016	-0.059**†††	0.678	0.492	0.024	-0.120**
	(0.019)	(0.028)			(0.028)	(0.057)
Female ≤ HS, < 30	0.022	-0.015	0.605	0.515	0.037	-0.029
	(0.021)	(0.040)			(0.035)	(0.077)

Notes: Same as Table 2. The employment measure is annual employment, defined as individuals who reported working more than zero weeks in the past 12 months.

Appendix Table A3: Minimum Wage-Employment Regressions with Separate Effects by Race, 2005-11

Population	White empl. effect	Black empl. effect	Avg white empl. rate	Avg black empl. rate	White empl. elas.	Black empl. elas.
Teens 16-19	-0.007 (0.030)	-0.049 (0.062)	0.368	0.218	-0.019 (0.080)	-0.222 (0.283)
Male teens	-0.030 (0.034)	-0.082 (0.073)	0.349	0.199	-0.085 (0.096)	-0.411 (0.369)
Female teens	0.012 (0.032)	-0.003 (0.065)	0.389	0.239	0.031 (0.082)	-0.014 (0.274)
< 25	0.005 (0.026)	-0.015 (0.061)	0.547	0.385	0.010 (0.048)	-0.040 (0.157)
Male < 25	-0.002 (0.033)	-0.004 (0.071)	0.540	0.358	-0.003 (0.061)	-0.012 (0.199)
Female < 25	0.012 (0.025)	-0.031 (0.061)	0.554	0.412	0.022 (0.044)	-0.076 (0.149)
< HS	0.002 (0.028)	-0.059 (0.058)	0.406	0.299	0.004 (0.069)	-0.196 (0.195)
Male < HS	0.003 (0.037)	-0.084 (0.075)	0.449	0.288	0.006 (0.082)	-0.292 (0.259)
Female < HS	0.003 (0.027)	-0.034 (0.055)	0.354	0.311	0.008 (0.075)	-0.110 (0.175)
≤ HS	0.002 (0.025)	-0.031 (0.052)	0.599	0.471	0.004 (0.041)	-0.065 (0.110)
Male ≤ HS	0.013 (0.033)	-0.054 (0.063)	0.647	0.462	0.020 (0.051)	-0.117 (0.137)
Female ≤ HS	-0.010 (0.019)	-0.008 (0.045)	0.544	0.481	-0.018 (0.034)	-0.017 (0.095)
< HS, < 25	-0.006 (0.035)	-0.081† (0.061)	0.310	0.174	-0.018 (0.113)	-0.468 (0.351)
Male < HS, < 25	-0.024 (0.043)	-0.089 (0.074)	0.308	0.160	-0.079 (0.141)	-0.555 (0.462)
Female < HS, < 25	0.018 (0.038)	-0.076† (0.065)	0.314	0.191	0.058 (0.121)	-0.397 (0.342)
≤ HS, < 25	-0.002 (0.031)	-0.037 (0.060)	0.450	0.296	-0.005 (0.070)	-0.127 (0.202)
Male ≤ HS, < 25	-0.001 (0.040)	-0.058 (0.080)	0.464	0.284	-0.002 (0.086)	-0.205 (0.282)
Female ≤ HS, < 25	-0.005 (0.030)	-0.015 (0.056)	0.433	0.310	-0.011 (0.070)	-0.050 (0.181)
< HS, < 30	0.007 (0.035)	-0.080†† (0.061)	0.336	0.202	0.022 (0.103)	-0.396 (0.300)
Male < HS, < 30	-0.008 (0.042)	-0.099† (0.073)	0.347	0.186	-0.022 (0.122)	-0.529 (0.393)
Female < HS, < 30	0.029 (0.037)	-0.062 (0.065)	0.323	0.220	0.089 (0.115)	-0.282 (0.294)
≤ HS, < 30	0.018 (0.030)	-0.042 (0.064)	0.505	0.353	0.035 (0.060)	-0.120 (0.181)
Male ≤ HS, < 30	0.017 (0.040)	-0.080† (0.080)	0.536	0.340	0.032 (0.074)	-0.235 (0.234)
Female ≤ HS, < 30	0.019 (0.027)	0.005 (0.060)	0.467	0.367	0.040 (0.058)	0.012 (0.164)

Notes: Same as Table 2.

Appendix Table A4: Minimum Wage-Employment Regressions with Separate Effects by Race, TWFE and 2S-DID (Discrete Treatment) from 2012-19

Population	TWFE White	TWFE Black	2S-DID White	2S-DID Black
Teens 16-19	-0.005 (0.005)	0.001 (0.007)	0.0002 (0.005)	0.001 (0.010)
Male teens	-0.004 (0.005)	0.004 (0.009)	-0.0002 (0.005)	0.006 (0.012)
Female teens	-0.004 (0.006)	-0.005 (0.009)	-0.0001 (0.007)	-0.003 (0.013)
< 25	-0.002 (0.004)	-0.001 (0.006)	0.004 (0.004)	-0.002 (0.007)
Male < 25	0.001 (0.004)	-0.003 (0.006)	0.006 (0.005)	-0.001 (0.007)
Female < 25	-0.005 (0.004)	0.0003 (0.008)	0.001 (0.004)	-0.003 (0.009)
< HS	0.0003 (0.004)	0.002 (0.007)	0.006 (0.004)	0.002 (0.009)
Male < HS	0.004 (0.004)	-0.001 (0.007)	0.009^{**} (0.005)	0.002 (0.008)
Female < HS	-0.002 (0.004)	0.006 (0.009)	0.004 (0.005)	0.006 (0.013)
≤ HS	0.002 (0.003)	0.005 (0.006)	0.005 (0.003)	0.006 (0.008)
Male ≤ HS	0.005 (0.004)	0.006 (0.006)	0.009^{**} (0.004)	0.009 (0.008)
Female ≤ HS	-0.001 (0.003)	0.004 (0.007)	0.002 (0.003)	0.006 (0.009)
< HS, < 25	-0.004 (0.005)	0.0001 (0.007)	0.0001 (0.006)	0.004 (0.010)
Male < HS, < 25	-0.003 (0.005)	-0.001 (0.008)	0.001 (0.006)	-0.001 (0.009)
Female < HS, < 25	-0.004 (0.007)	0.0001 (0.011)	-0.0004 (0.007)	0.004 (0.016)
≤ HS, < 25	-0.003 (0.004)	-0.009 (0.007)	0.002 (0.005)	-0.006 (0.010)
Male ≤ HS, < 25	-0.001 (0.005)	-0.006 (0.006)	0.006 (0.007)	-0.006 (0.009)
Female ≤ HS, < 25	-0.005 (0.005)	-0.014 (0.011)	-0.002 (0.006)	-0.008 (0.014)
< HS, < 30	-0.0003 (0.005)	-0.002 (0.008)	0.006 (0.006)	0.002 (0.011)
Male < HS, < 30	0.001 (0.005)	-0.0004 (0.008)	0.008 (0.006)	0.001 (0.011)
Female < HS, < 30	-0.001 (0.007)	-0.004 (0.011)	0.004 (0.007)	0.002 (0.017)
≤ HS, < 30	-0.001 (0.005)	-0.003 (0.007)	0.005 (0.005)	0.001 (0.011)
Male ≤ HS, < 30	0.001 (0.005)	-0.0001 (0.007)	0.008 (0.006)	0.004 (0.010)
Female ≤ HS, < 30	-0.003 (0.005)	-0.008 (0.011)	0.002 (0.006)	-0.002 (0.015)

Notes: Same as Table 4. The treatment considered is discrete, defined as the first-time minimum wage increases in a PUMA.

Appendix Table A5: HHIs (for Various Industries) for PUMAs with 10th and 95th Percentiles of Share Black (in 2019)

A. 10th percentile of share black (each PUMA weighted by population)

	Retail (NAICS = 44,45)	**Food & Accommodation (NAICS = 72)**	**Low wage (NAICS = 44,45,71,72,56,81)**	**All**
HHI (estab.)	133.66	100.85	42.71	31.40
HHI (firm)	252.36	113.20	62.26	63.82
Count (estab.)	1005	478	4872	11313
Employment	8805	6649	27309	80613

B. 95th percentile of share black (each PUMA weighted by population)

	Retail (NAICS = 44,45)	**Food & Accommodation (NAICS = 72)**	**Low wage (NAICS = 44,45,71,72,56,81)**	**All**
HHI (estab.)	139.17	101.63	48.81	54.21
HHI (firm)	225.70	120.94	58.76	161.99
Count (estab.)	914	442	4832	9983
Employment	7381	6568	27107	84165

Notes: All measures (including percentiles of share black) are computed as weighted averages across PUMAs, where the weights correspond to the population of each PUMA in 2019. The HHI, establishment count, and employment are measured in a ± 5 percentile interval around the specified percentile— for e.g., the HHI for the 10th percentile is calculated by taking the weighted average HHI in the interval between 5th and 15th percentile of share black.