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ABSTRACT

Using 1979-2019 Current Population Survey data, we study the effect of state and federal minimum wage policies on gender, race, and ethnic inequality. We find that minimum wages substantially reduce intergroup wage inequality at least up to the 20th wage percentile, with no evidence of adverse employment effects. We conduct counterfactual simulations of between-group inequality due to minimum wage changes since 1979. Declines in the real minimum wage in the 1980s slowed progress in narrowing between-group inequality. Relatively small changes in minimum wages during 1989-1998 and 1998-2007 meant little role for the minimum wage over those time spans. Since 2007, several states have steeply raised their minimum wages, especially raising Hispanics' relative wages, because they earn low wages and reside disproportionately in those states. Finally, we find that raising the federal minimum wage to $12/hour in 2020 dollars ($14.49 in 2025Q2 dollars) would reduce existing between-group wage gaps below the 15th percentile by 25-50%.

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An online appendix is available at http://www.nber.org/data-appendix/w31725

Introduction

Economists have long been interested in how minimum wage policies affect worker earnings and employment at the bottom of the wage distribution. However, minimum wage policies may also play a role in narrowing wage disparities between groups that are differentially located in the wage distribution, though these effects remain relatively unexplored in the contemporary US context. During the period from 1979 to 2019, minimum wages were, on average across states, binding at about the 4th percentile of the wage distribution, i.e., about 4 percent of workers earned the prevailing minimum wage in their jurisdiction,[1] but this average masks considerable heterogeneity across subgroups. Because lower-wage subgroups, such as Black, Hispanic, or female workers, have wages closer to the minimum, it is likely that increases in the minimum wage will have larger effects on wages for these subgroups than for others. This should be the case even when minimum wage effects spill over to wages above the minimum (e.g., Fortin, Lemieux, and Lloyd 2021), since the effect remains strongest for low wage workers. Thus, although the minimum wage is not explicitly designed to reduce intergroup wage inequality, it should still have that effect.

In this paper, we ask: To what extent do minimum wage policies reduce between-group wage gaps at different points along the wage distribution? That is, we study the causal effect of minimum wage changes on female-male, Black-White, and Hispanic-White wage gaps, with a particular focus on gaps below the 20th percentile of each group's wage distribution, where the minimum wage is likely to have the largest effect. We use 1979-2019 data on hourly wages from the Current Population Survey Merged Outgoing Rotation Group (CPS MORG) and apply two empirical methodologies to investigate this question.

[1] Values are calculated using data from the CPS MORG described in greater detail below.

First, we directly estimate the effect of minimum wage policies on various measures of between-group inequality using state-by-year panel regressions. Here, we largely follow the set-up of Autor, Manning, and Smith (2012), who study the effect of the minimum wage on within-group inequality. Second, we conduct counterfactual simulations to explore the dual role of federal and state minimum wage policies in reducing between-group wage gaps since 1979. Our simulation exercises study the effect of observed minimum wage policies during four intervals: 1979-1989, 1989-1998, 1998-2007, and 2007-2019. These periods represent four epochs in the history of US minimum wage policy: the first, a sustained decline in the real (inflation-adjusted) federal minimum over the 1980s; the second, two increases in the federal minimum during the 1990s; the third, a short period of inflationary decline during the early 2000s; and the fourth, a major federal increase concurrent with increases at the state level during the late 2000s and 2010s. A novel part of our analysis is to study the effect of the geographic distribution of subgroups, since Black and Hispanic workers are on average located in different states with different minimum wage policies than White workers.

We conclude by simulating the effect of a larger federal minimum wage on contemporary levels of between-group inequality. As we write, the federal minimum wage is $7.25 per hour. Using data on wages from recent years of the CPS MORG, we predict the effects of a federal minimum wage of $12 (in 2020 dollars) on between-group wage inequality at the national level, based on a pooled 2015-2019 sample of workers.[2] Note that $12 in 2020 dollars is equivalent to $14.49 in 2025Q2 dollars.[3] This is close to the $15 figure often referenced in the contemporary minimum wage policy debate.

[2] For most states, minimum wages exceeding $12 (in 2020 dollars) lie well outside the historical variation available in our sample, thus limiting the credibility of simulations exploring even higher minimum wages.
[3] This conversion uses the personal consumption expenditures deflator from the Bureau of Economic Analysis.

As mentioned above, the role of minimum wages in narrowing wage disparities between groups in the United States is relatively unexplored. Derenoncourt and Montialoux (2021) have established an important role for the expansion of the minimum wage in the late 1960s and early 1970s in reducing the Black-White earnings gap,[4] but only Wursten and Reich (2023) have explored the impact of minimum wages on Black-White gaps in the current context.[5] To our knowledge, the effect of minimum wages in the United States on the gender wage gap or the wage gap between White and Hispanic workers remains unexamined. Furthermore, we find new evidence on the causes of intergroup wage differentials, highlighting why between-group wage convergence varies across the wage distribution.

Background, Related Literature, and Our Contribution

Studying the effects of minimum wages on wage gaps between groups is especially important because progress in closing between-group disparities has slowed, stalled, or even reversed. In 2025, at the median, women earned 18 percent less than men, while Black and Hispanic workers earned 19 and 23 percent less than White workers, respectively (Bureau of Labor Statistics 2025). Convergence in the mean gender gap has slowed since the 1990s (Blau and Kahn 2017; Blau and Winkler 2022, Figure 7-2); there has been little consistent progress in narrowing the mean Black-White earnings gap since the mid-1970s (Blau and Winkler 2022, Table 7-8); and the mean Hispanic-White gap has risen since the mid-1970s (Blau and Winkler 2022, Table 7-8). At the bottom of the wage distribution, where the impact of the minimum wage is expected to be

[4] See also Bailey, DiNardo, and Stuart (2021).
[5] Some authors provide heterogeneity analyses that include these subgroups, for example, Cengiz, Dube, Lindner, and Zipperer (2019) for Black and Hispanic individuals, and women; and Wursten and Reich (2023) for Hispanic individuals and women.

concentrated, inequality between groups has been similarly persistent, as described in detail below.

The relationship between minimum wage policies and inequality between groups is also a pressing question because raising the minimum wage is an active policy area. Since 2014, 30 states have raised their minimum wage (Economic Policy Institute 2025), with many of those states implementing $12 and $15 minimum wages (National Conference of State Legislatures 2022). More than a dozen municipalities have also passed legislation to raise local minimum wages above the federal level (Cengiz et al. 2019). Further, at the national level, there has been a push by some in Congress to enact a $15 federal minimum wage.[6]

This paper makes several contributions to the minimum wage literature. First, we adopt a distributional approach to study between-group inequality, following in the footsteps of several minimum wage papers mainly focused on within-group inequality (DiNardo, Fortin, and Lemieux 1996; Lee 1999; Autor et al. 2012; Dube 2019; Fortin et al. 2021). Outside this work, the minimum wage literature primarily estimates mean effects for groups of workers likely to be disproportionately affected by the minimum wage, such as teens or service workers (e.g., Neumark, Salas, and Wascher 2014; Allegretto et al. 2017; Wursten and Reich 2023). Most recently, Wursten and Reich (2023) use data and policy variation similar to ours to study mean racial wage gaps within such disproportionately affected subgroups. In contrast, we focus on effects of minimum wage policies along the entire wage distribution, allowing us to directly explore the economy-wide role of minimum wages in shaping between-group wage inequality, without the need for *a priori* assumptions about who is affected by the minimum wage.[7] Our

[6] See, for example, H.R. 582 in the 116th Congress, which was passed by the House of Representatives in 2019.
[7] For example, Wursten and Reich (2023) assume workers outside their focal sample are entirely unaffected by the minimum wage.

methodology is a natural extension of Autor et al. (2012) and Lee (1999), who study the effect of minimum wages on within-group inequality; we apply their regression frameworks to a direct analysis of between-group inequality. Generally, we find that minimum wages reduce between-group wage gaps at the bottom of the distribution, with effects fading out at higher wage percentiles. We then confirm these estimates using new event study methods that are transparent regarding the sources of minimum wage variation and the existence (or not) of bias due to non-parallel pre-trends and heterogeneous treatment effects (Cengiz et al. 2019). While we focus on wage effects, we use these same empirical designs to explore concerns that minimum wage policies may reduce employment at the extensive or intensive margins for low-wage individuals, but we are unable to detect such effects.

As a second contribution, we evaluate the historical importance of the minimum wage using counterfactual simulations over key periods of fluctuation in federal and state policies from 1979 to 2019. The strongest prior evidence on Black-White inequality, for example, exploits unique policies legislated during the 1960s (Derenoncourt and Montialoux 2021) or only presents estimates pooled across several decades (Wursten and Reich 2023), whereas we document substantial variation in effect sizes across key minimum wage policy regimes during the 1979 to 2019 period. These simulations highlight how large heterogeneity in the value of the real minimum wage across time and space has worked to shape national-level inequality between groups.[8]

Third, we provide counterfactual predictions about the effects of a $12 federal minimum wage (in 2020 dollars, or $14.49 in 2025Q2 dollars) on national-level inequality between

[8] Other papers have studied heterogeneity in the effect of the minimum wage for women or Hispanic individuals (e.g., Cengiz et al. 2019; Wursten and Reich 2023) but have not directly examined how these effects translate to inequality between groups at the national level.

demographic groups. Our estimates imply that such a sizeable increase in the federal minimum wage would lead to significant and economically important reductions in gender, race, and ethnic inequality at the bottom of the national wage distribution. Finally, we estimate the impact of the geographic distribution of demographic groups on exposure to state minimum wage policies, which, to our knowledge, has not been previously studied. This factor proves to be important particularly for Hispanic-White wage differentials.

Our paper also contributes to the broader literature on group wage differentials. Recent work finds that wage convergence by gender and race does not necessarily progress evenly across the wage distribution—for gender gaps, see e.g., Blau and Kahn (2017) and Blau et al. (2024); for racial gaps, see e.g., Bayer and Charles (2018). These differences may be partially explained by policy. Indeed, we find that the impact of the minimum wage is concentrated at the bottom of the distribution, with little effect at the median.

Data

Our minimum wage variable is the effective minimum wage (i.e., the maximum of state and federal), measured as the annual modal value by state, which we obtain using a dataset made available by David Neumark (Neumark 2021). Our 1979-2019 wage data come from the CPS MORG.[9] We construct a sample of workers between ages 18 and 64, excluding the self-employed.[10] We use the reported hourly wage when available. Otherwise, we compute an individual's hourly wage as weekly earnings divided by hours worked at all jobs in the prior

[9] We obtained these data from the National Bureau of Economic Research's Public Data Use Archive. Note that our sample period is prior to the COVID-19 pandemic and thus not affected by the unusual circumstances associated with it.
[10] We exclude teens (ages 16-17) because our focus is on inequality within the broader adult labor force. Our results are robust to including teens and older individuals ages 65-74.

week.[11] All wages are converted into 2020 dollars using the personal consumption expenditures (PCE) deflator published by the Bureau of Economic Analysis (BEA).[12] We multiply top-coded values by 1.5 and then winsorize our wage variable to the 97th percentile at the state-by-year level.[13] We drop any observations with imputed earnings, as well as those with wages (reported or computed) below $1 per hour in 2020 dollars.[14]

For our analysis of racial/ethnic inequality, we focus on three groups: non-Hispanic White individuals, non-Hispanic Black individuals, and Hispanic individuals of any race. Due to sample size limitations in the CPS MORG at the state-year level, we do not analyze racial/ethnic inequality separately by sex. For similar reasons, we do not study groups comprised of individuals in other racial categories (e.g., Asians or other races) separately, but such individuals are included in our analyses of gender inequality. However, we show robustness to controlling for state-year racial/ethnic composition in analyses of gender gaps (and vice versa). We weight our analyses using CPS sampling weights multiplied by weekly hours worked.[15] In all specifications, we also adjust the weights so that each year is weighted equally to account for secular increases in population.

Our main regression approaches use state-year panel datasets, constructed by collecting *group-specific* wage quantiles within state-by-year-by-group cells. This allows us to directly

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[11] Because the CPS hourly wage variable (EARNHRE) does not contain tips, but the weekly earnings variable (EARNWKE) does, we show our results are robust to dropping individuals in occupations likely to contain many tipped workers.
[12] The PCE index is more effective than the CPI index at taking into account changes in the market basket of consumption because it updates annually, while the CPI updates only once every several years. For this reason, following Katz and Murphy (1992), we use the PCE index.
[13] In specifications that use group-specific percentiles, we winsorize wages at the state-by-year-by-group level. This decision, which follows Autor et al. (2012), has no bearing on our quantile-specific results, but slightly affects estimates for mean wage gaps.
[14] We judge that wages in this range are unlikely to reflect an individual's actual wage rate, and our results are robust to dropping workers with wages below $2 per hour.
[15] Using work hours to adjust the sampling weights is standard in the literature (e.g., Autor et al., 2012). Our results are robust to weighting all workers equally.

compare, e.g., wages at the 5th percentile of the female wage distribution to wages at the 5th percentile of the male wage distribution. We also collect mean wages within cells. Our gender wage gap estimates use every available year and state (excluding Washington, DC) contained in the CPS MORG between 1979 and 2019, which provides us with 2050 state-year observations. When we examine effects on Black-White and Hispanic-White wage gaps, however, we impose bin-size restrictions to reduce noise induced by using small cells to compute within-group state-year wage percentiles. (Small cell size is not an issue for the female and male samples.) Our main results for racial and ethnic wage gaps are estimated on balanced panels in which every state-year bin contains at least 50 individual wage observations for each group (White, Black, or Hispanic individuals).[16] This yields 1066 observations (in 26 states) for Black-White comparisons and 451 observations (in 11 states) for Hispanic-White comparisons. We emphasize that, although this bin-size restriction substantially restricts our sample size and narrows the geographic scope of our results for these groups, the selected sample reflects their actual geographic distribution.

Motivating Trends

In this section, we briefly review the trajectory of federal and state minimum wage policies since 1979. We also describe trends in gender, racial, and ethnic wage inequality observed in the CPS MORG from 1979 to 2019 at percentiles which may have plausibly been affected by the minimum wage.

Figure 1 shows the evolution of different measures of minimum wages in real terms (2020 dollars) since 1979. We include time series for the federal minimum, the mean effective

[16] Our main results are robust to estimates from unbalanced panels that simply drop any state-year cells that do not satisfy this 50-observation requirement.

minimum (i.e., maximum of state/federal), the mean effective minimum among states with policies exceeding the federal minimum, and the maximum effective minimum.[17] The real federal minimum wage peaked in 1979, fell until 1989, and has since risen with three discrete policy events, each followed by a period of inflationary decline. The federal minimum has been in inflationary decline since 2010. State minimum wages rarely superseded the federal minimum wage until about 1998, when several states raised their minimum wages after a prolonged decline in the real value of the federal minimum. Notably for our study, which uses data on wages out to 2019, the maximum effective minimum wage has steeply increased since 2015 as a growing number of states have passed large statutory increases. Online Appendix Figure B1b recreates Figure 1 using the minimum wage bite, which is our primary explanatory variable and defined as the difference between the log minimum wage and the log median wage (estimated at the state-year level). While trends in the bite largely match Figure 1 trends for the federal minimum wage and mean effective minimum wage, the minimum wage bite is much flatter than the real values for the mean minimum wage conditional on being higher than the federal minimum wage. This suggests that states that increased minimum wages above the federal level also tended to be those that experienced larger growth in median wages.[18]

Next, we present trends in between-group wage gaps, which are the primary focus of this paper. Figures 2a-c display the evolution of these gaps during our sample period at the 5th, 10th, 15th, and 20th wage percentiles, juxtaposed with changes in the average bite of the effective minimum wage. Figure 2a shows trends in gender wage gaps, which have generally declined

[17] Figure 1 weights states using total employment in labor hours, though Appendix Figure B1a shows that trends appear quite similar when weighting states equally.
[18] This is also suggested by Online Appendix Figure B1c, which shows the evolution of binding percentiles of the minimum wage.

since 1979, except at the 5th percentile, where little change is seen.[19] Racial and ethnic wage gaps demonstrate an opposite pattern. Figure 2b shows a slight upward trend in observed lower-tail wage gaps between Black and White workers, across all four focal wage percentiles. Figure 2c, which presents White-Hispanic wage gaps, shows similar increases in inequality prior to 2000, followed by plateaus over the 2000s and declines over the 2010s (although not back down to 1980s levels). We overlay the path of the average minimum wage bite in Figures 2a-c, which provides suggestive evidence that wage gaps at the bottom of the distribution have moved in opposition to changes in the minimum wage (i.e., between-group inequality often rises as minimum wages fall).

Online Appendix Figure B2 shows the share of hours at or below the effective minimum wage for the pooled sample and separately by sex, race, and ethnicity. This figure shows that men and White workers face less direct exposure to minimum wage policies than female, Black, and Hispanic workers. Racial, ethnic, and gender gaps in minimum wage exposure tended to be larger during the 1980s and have since become much smaller, with the main exception being the Hispanic-White exposure gap, reflecting Hispanic individuals' particularly low wages as well as their location in high minimum wage states.[20]

[19] In the CPS MORG, the gender wage gap slightly increases at the 5th and 10th percentiles during the 1980s. This contrasts with the convergence at the 10th percentile found by Blau and Kahn (1997) for 1979-1988 using the March CPS and Panel Study of Income Dynamics (PSID). We found this difference between the CPS MORG and March CPS to be robust to different sample restrictions, including dropping very low wage observations (less than $1 per hour either in 1983 dollars or 2020 dollars) and including only full-time workers. It was also not accounted for by the fact that the CPS MORG contains reported hourly wages, while, in the March CPS and PSID, an hourly wage must be inferred from weekly earnings divided by reported hours or from annual earnings divided by hours multiplied by weeks. The slight increase in the CPS MORG gender wage gap at the 5th and 10th percentiles over the 1980s persists even when using a similarly constructed hourly wage.

[20] Non-compliance with the minimum wage is also more pronounced for Hispanic individuals versus other groups. As shown in Online Appendix Table A1, Hispanic workers tend to have the highest share (in percentage points) of labor hours paid strictly below the minimum wage, especially after 2000.

Methods

We begin our analysis by directly estimating the effect of minimum wage policies on between-group inequality, using various inequality measures as dependent variables in our regressions. In a subsequent section, we use similar regression approaches to conduct a microsimulation analysis of the effects of minimum wage policies.

AMS/Lee Approach

The first regression approach we employ leverages minimum wage variation in a two-way fixed effects (TWFE) regression framework, as used by Autor et al. (2012), henceforth AMS, and Lee (1999) in their studies of aggregate and within-group (male, female) inequality. This approach is appealing because it does not make any assumptions about minimum wage compliance, spillovers, or employment effects. Wages are allowed to evolve freely through any of these channels. On the other hand, this approach imposes a strong functional form restriction on the relationship between the minimum wage and the wage distribution.

The original AMS/Lee method is to regress a measure of within-group wage inequality (e.g., the 50-10 log wage gap for men) on the bite of the minimum wage, which is defined as the effective log minimum wage minus the log median wage. The regressions are estimated using state-year panel datasets and include state and year fixed effects to account for level differences and aggregate trends, as well as state-specific linear trends to account for state trends unrelated to minimum wage policies. The minimum wage bite enters non-linearly as a quadratic term. We adapt the AMS/Lee method by using between-group inequality measures as outcome variables in the regression.

Let $y_{st}(p)$ represent some function of percentiles p of the log wage distribution in state s at time t. The minimum wage bite is $w_{st}^{mw} - w_{st}(50)$, where w_{st}^{mw} represents the log effective minimum wage in state s at time t and $w_{st}(50)$ represents the log median wage in state s at time t, measured among all groups in the state-year cell. We then estimate the following equation:

$$y_{st}(p) = \beta_1(p)[w_{st}^{mw} - w_{st}(50)] + \beta_2(p)[w_{st}^{mw} - w_{st}(50)]^2$$
$$+\sigma_s(p) + \gamma_t(p) + \sigma_s(p) \times t + \varepsilon_{st}(p). \quad (1)$$

When using the approach to directly relate minimum wage policies to between-group inequality, we set $y_{st}(p)$ to be $w_{st}^{g}(p) - w_{st}^{g'}(p)$, the log wage gap between two distinct demographic groups g and g' (e.g., men and women) at some percentile of each group's wage distribution p (e.g., the 5th percentile of the male and female distributions, respectively). The β coefficients (β_1 and β_2) measure the impact of the minimum wage bite on the male-female, White-Black, or White-Hispanic gap at percentile p.[21] As in AMS, we report marginal effects at the weighted mean of the bite; a negative effect means that the gap has been reduced.[22] Our main specification includes state and year fixed effects ($\sigma_s(p)$ and $\gamma_t(p)$, respectively) as well as state-specific linear trends ($\sigma_s(p) \times t$).[23]

In the original AMS specification, the state-year median wage is included (via subtraction) on both the left- and right-hand sides of equation (1), because wage inequality and the minimum wage bite are both defined relative to a state-year's median log wage. This introduces a mechanical upward bias in estimated marginal effects, which can be purged via two-

[21] AMS argue that the quadratic term in the bite is necessary to allow effects to increase as minimum wages grow closer to the median wage, since a one log point increase in the real minimum wage will likely "sweep up" more workers when it cuts at, for example, the 40th percentile rather than the 5th percentile of a state's wage distribution. This is because the wage distribution is denser near the median than in the left tail.
[22] That is, we report the weighted mean over all states and years of $\beta_{st}'(p) \equiv \beta_1(p) + 2\beta_2(p)[w_{st}^{mw} - w_{st}(50)]$.
[23] The inclusion of these state-specific linear trends does not meaningfully affect our estimates at the most important low percentiles but eliminates a few spurious "placebo" effects at higher percentiles such as the 70th or 80th, the absence of which serves as an important specification check.

stage least squares regression (TSLS) with a set of instrumental variables. For our between-group regressions, this IV procedure is less necessary, since the state-year median wage is no longer on the left-hand side of the regression. However, we still prefer TSLS estimates due to concerns about endogeneity bias arising from omitted factors related to both state-year median wages and the relative positions of different groups at lower segments of the wage distribution, one example factor being labor demand.[24] Therefore, following AMS, we instrument for the minimum wage bite and its square with (*i*) the log statutory minimum wage, (*ii*) the square of the log statutory minimum wage, and (*iii*) the interaction of the log statutory minimum wage and the weighted mean of the median log wage (estimated over all states and years).[25] We cluster standard errors at the state level.

In our main approach, we estimate equation (1) using TSLS in levels. For comparison, in the online appendix we provide results from ordinary least squares regressions (OLS) and regressions estimated in first differences. We prioritize the TSLS estimates due to aforementioned endogeneity concerns and focus on the results for levels, rather than first differences, in part because this is standard in the literature. However, results are broadly consistent across the specifications.

Stacked Difference-in-Differences (SDD)

[24] The TSLS specification may have yet another advantage, which is that it purges the OLS specification of classical measurement error in the bite of the minimum wage, as suggested by AMS. Such measurement error could be present because, for example, the minimum wage often changes in the middle of calendar years, and our measure of the minimum wage in each state-year cell is the mode of effective monthly minimum wages. In addition, there may be measurement error in the estimated median wage.

[25] Contrary to the discussion in Fortin et al. (2021), the AMS specification does exploit variation arising from changes—both inflationary declines and policy increases—in the federal minimum wage. This is because the third instrument mentioned above (i.e., the interaction of the log statutory minimum wage with the weighted mean of the median log wage for each state over the sample period) varies across states even when there is only a federal minimum wage increase.

Our second regression approach leverages discrete minimum wage policy changes in a difference-in-differences framework. This has two advantages. First, it allows us to explore the dynamics of minimum wage effects using an event study regression framework: i.e., how long does it take for minimum wage effects to appear, and how enduring are the effects? Second, it allows us to implement specifications for overcoming biases that may be present in TWFE models with staggered policy adoption (e.g., Goodman-Bacon 2019). Specifically, we implement the stacked difference-in-differences (SDD) regression framework of Cengiz et al. (2019). This method guards against bias created by the presence of heterogeneous treatment effects, eliminates the negative weighting problem that arises when already-treated states are used as controls in the presence of treatment effects that vary over time (Goodman-Bacon 2019), and allows us to assess directly the credibility of the parallel trends assumption that is required for identification in this setting. Relative to other recently proposed difference-in-differences specifications, the SDD approach is preferable in our setting because it can accommodate multiple minimum wage policy changes in the same state and allows for flexible selection of control units for each event.[26] To facilitate comparability between the SDD and TWFE results, we scale effects by the magnitude of the minimum wage change averaged over the post-treatment period.[27]

As in any difference-in-differences research design, we must define a discrete treatment event. In our primary specification, we define a minimum wage increase event as a year-over-year 3 percent or higher increase in the real effective minimum wage in 2020 dollars.[28] However,

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[26] Most other methods assume that treatment is an absorbing state, i.e., that each unit is treated either once or never (Wooldridge 2021). This is clearly not true in the case of minimum wage policy changes, which are frequent and can be of different magnitudes.

[27] We also present unscaled estimates in Online Appendix Table A2a.

[28] This 3 percent threshold corresponds very closely to the minimum size of the non-trivial events selected in Cengiz et al. (2019), who used 25 cents real changes (in 2016 dollars).

since we wish to trace out minimum wage effects for up to five years after such events, we prefer not to treat minimum wage increases that occur in repeated succession as separate events (for example, an increase in 1992, and then an increase in 1994), because it is not clear how to distinguish a delayed effect of an initial increase from an immediate effect of a subsequent increase. Instead, we designate such occurrences as a single event during which treatment intensity increases over time. We use a straightforward rule to eliminate such successive increases: we do not consider any events that occur less than five years after some other event.[29] This sort of rule-based selection of uncontaminated events is in line with the growing literature on event study designs (Miller 2023).

After defining treatment events, we match each state-event pair to a set of contemporary "clean control" states, which exclude any states with treatment events during the focal state's event window, which we define as 3 years before and 5 years after an event. These clean control states constitute "never-treated" units within the event window, though they may have minimum wage increases outside the window. Finally, because we require each event to have a non-empty set of clean controls, we exclude minimum wage increase events that occur simultaneously with federal minimum wage increases. Our selection rule ultimately results in a set of 69 minimum wage increase events occurring in 33 unique states. On average, we associate each event with about 30 clean control states (20 at minimum, 44 at maximum). Online Appendix Figure B3 displays the set of events that we analyze.

[29] For example, if our treatment definition produces events in 1992, 1994, and 1998, we drop the 1994 event because it occurs only 2 years after 1992 and we drop the 1998 event because it occurs only 4 years after the 1994 event. That is, we consider the 1992 change as a single event that increased in intensity over time. However, if our approach produces events in 1992, 2005, and 2012, we would still estimate effects of the 2005 and 2012 changes, since they occur more than five years after the previous event. Cengiz et al. (2019) do not take this approach and instead use all potentially overlapping events. Our results are robust to using all events, of which there are 140 that meet our 3 percent threshold, compared to 69 under our primary approach; see Online Appendix Table A2b.

Each treated state and matched set of clean control states constitutes its own nine-year panel dataset. To implement the SDD design, we then stack these datasets and estimate the following event-study regression:

$$y_{dst}(p) = \sum_{k=-3}^{k=5} \beta_k(p)\, D_{d,s,t+k} + \gamma_{dt}(p) + \sigma_{ds}(p) + \sigma_s(p) \times t + \varepsilon_{dst}(p) \qquad (2)$$

where datasets are denoted by d, calendar time is denoted by t, and event time is denoted by k. The event-time treatment dummies, $D_{d,s,t+k}$, indicate whether state s in calendar year t was treated k periods ago. These are set equal to 1 if, within dataset d, there was a minimum wage increase event in state s at time $t-k$; otherwise, $D_{d,s,t+k}=0$. We omit the dummy variable associated with $k=-1$, so that coefficients can be interpreted relative to the period immediately prior to treatment. We always include state-by-dataset and year-by-dataset fixed effects ($\sigma_{ds}(p)$ and $\gamma_{dt}(p)$, respectively), so the coefficients of interest are identified entirely from within-dataset variation between a once-treated state and a collection of never-treated states (the clean controls). Our main specification includes state-specific linear trends ($\sigma_s(p) \times t$), which are estimated across all calendar years at once and are not dataset-specific.[30] Following Cengiz et al. (2019), we cluster standard errors at the state-by-dataset level.

While the full event study specification in equation (2) is useful for tracing out minimum wage treatment effects over time within the event windows, estimating each event-time k

[30] Including these trends is consistent with our baseline TWFE specification and the original AMS specification, but our main SDD results are robust to excluding these trends; see Online Appendix Table A2c. Additionally, note that there may be a concern that treated and control states meaningfully differ in ways that are correlated with minimum wage policies. Online Appendix Table A2d shows the weighted average of the minimum wage bite, the log pooled 5th percentile wage, and the log pooled median wage for the treated state of each event and its set of clean control states, including only pre-treatment years within each event window. Generally, minimum wage bites are more negative in the treated versus the control states before treatment. Treated states also tend to have higher incomes, which is reflected in higher 5th percentile and median wages. However, we view it as relatively more important that the treated and control states exhibit similar pre-trends, and we test for this below. We also note that, as described below, our main TWFE specification results are robust to including a rich set of state-year time-varying controls measuring the demographic and education composition of the labor force.

coefficient comes at the sacrifice of statistical power. Hence, we also pool post-treatment effects by estimating a version of equation (2) in which we average all the post-treatment effects by setting $\beta_k(p) = 0$ for $k < 0$ and $\beta_k(p) = \beta(p)$ for $k \geq 0$. Further, estimates of $\beta(p)$ and $\beta_k(p)$ are not easily interpretable due to variation in the size of minimum wage reforms. Therefore, we also estimate versions of equation (2) that scale treatment effects by the size of the minimum wage increases, allowing for better comparability to our estimates from the AMS-style regressions. To do so, we replace the left-hand side of equation (2) with the minimum wage bite ($w_{st}^{mw} - w_{st}(50)$) and estimate a "first stage" effect separately by dataset (extracting every $\hat{\beta}_d^{FS}(p)$).[31] Then, we replace the event study indicators in equation (2) with $D_{d,s,t+k}^{*} = \hat{\beta}_d^{FS}(p) \times D_{d,s,t+k}$ and re-estimate the equation to yield new "dosage" coefficients, which we call $\beta^{Dosage}(p)$. These scaled coefficients may be interpreted as the effect of a standardized 1 log point change in the bite of the minimum wage.[32]

Whether as a difference-in-differences equation or a full-fledged event study, the specification in (2) has advantages and disadvantages compared to the two-way fixed effects specification in (1). Identification in the stacked event study is cleaner because events are defined as binary on-off switches, and it is clear what comparisons are being made between treatment and control states. Because the key independent variables in the TWFE specification of equation (1) are continuous, the exact nature of the implicit parallel trends assumption is not entirely transparent. In the stacked event study, the parallel trends assumption is tractable and testable, and any pre-trends are readily observable. However, one disadvantage of the event

[31] Note that the dataset-specific first stage regressions do not include state-specific linear trends. This is so that the first stage coefficients represent the simple non-detrended average difference in difference (post- versus pre-treatment versus control) of the relevant minimum wages for each event. The second stage includes state-specific linear trends that are estimated simultaneously across all the event-specific datasets.
[32] To be clear, each event receives its own first stage scaling coefficient, defined based on the size of the minimum wage change estimated over the 5-year post part of each event window.

study approach is that it cannot capture non-linearities in the effect of interest because the treatment variable is binary (or linearly scaled by the first stage). Another disadvantage is that the stacked event study exploits less minimum wage variation than TWFE because (*i*) it cannot incorporate federal minimum wage variation into the main effects (as there are no available control states), (*ii*) it does not use variation arising from the decline in the real minimum wage due to inflation (as featured prominently between 1979 and 1988), and (*iii*) its sample is narrowed by the requirement that changes in the minimum wage be big enough to meet our event definition. The smaller sample leads to noisier estimates, especially in the event study specification.

Direct Effects of Minimum Wage Policies on Between-Group Inequality

In this section, we discuss the results from our two methods used to directly estimate the effects of the minimum wage policies on between-group inequality.

Two-Way Fixed Effects and Stacked Difference-in-Differences Results

Figures 3a-c summarize graphically the key results from our main TWFE and SDD specifications. The estimates in these figures represent the effect of a 1 log point change in the bite of the minimum wage on log wage inequality between groups at a given percentile. Our main TWFE effects are the marginal effects estimated by TSLS in levels, and our main SDD effects are the dosage estimates described above. Both methods yield estimates showing that minimum wage increases substantially reduce between-group inequality at the bottom of the wage distribution. The point estimates that underlie these figures are displayed in Table 1.[33] The

[33] Estimating $\beta^{Dosage}(p)$ involves generated regressors. We account for uncertainty in the generated regressors by using state-by-dataset clustered bootstraps to obtain standard errors (starting from the main stacked event study

results from alternative TWFE specifications (TSLS in first differences, OLS in levels, and OLS in first differences) are shown in Online Appendix Table A3; results are broadly consistent across these specifications.[34]

Figure 3a shows results for gender wage inequality. The estimates are significantly negative at the 5th, 10th, 15th, and 20th wage percentiles for our TWFE specification; the point estimates are also negative at these percentiles in the SDD specification (although significant only at the 5th and 20th percentiles). There are no significant results above the 20th percentile or at the mean. Figures 3b and 3c show results for racial/ethnic wage inequality. For Black-White wage inequality, there are significant negative effects at the 5th, 10th, and 20th percentiles, with the 15th percentile estimate significantly negative for the TWFE specification and negative but barely missing statistical significance at the 5% level for the SDD specification. For Hispanic-White inequality, there are significant negative effects at these percentiles and additionally at the 30th percentile. Notably, there are small, statistically significant negative effects at the mean for both Black-White and Hispanic-White wage inequality in the TWFE specifications, with similar point estimates in the SDD specification, although the results are not quite significant at the 5% level.

As for the magnitudes of our effects, it is useful to briefly consider a historical episode: the 1980s. (Later, in our simulations, we will return to a more in-depth discussion of magnitudes.) The average minimum wage bite fell by 0.31 log points (about 27 percent) in real terms from 1979 to 1989. At the 5th percentile of each group's distribution, using our TWFE

panel). These bootstrapped standard errors are shown in Table 1 and Figures 3a-c. They are generally about 5% larger than the analytic standard errors (which we do not report), suggesting uncertainty in the generated regressors is not very important.

[34] We also present several robustness checks related to the SDD specification in Online Appendix Tables A2a through A2c; the motivations for these checks were discussed in the section on "Methods."

estimates, this implies a 0.037 log point (4 percent) increase in the gender wage gap; a 0.040 log point (4 percent) increase in the Black-White wage gap; and a 0.087 log point (9 percent) increase in the Hispanic-White wage gap. Results are similar using our SDD estimates, again at the 5th percentile of each group's distribution: a 0.056 log point (6 percent) increase in the gender wage gap; a 0.056 log point (6 percent) increase in the Black-White wage gap; and a 0.115 log point (12 percent) increase in the Hispanic-White wage gap. These magnitudes are substantial relative to the observed trends at the 5th percentile from 1979 to 1989: a 0.01 log point (1 percent) increase in the gender wage gap, a 0.04 log point (4 percent) increase in the Black-White wage gap, and a 0.05 log point (5 percent) increase in the Hispanic-White wage gap. In other words, our estimates imply that minimum wages are more than sufficient to account for these observed increases in between-group inequality during the 1980s at the 5th percentile.

Our estimates from both the TWFE and SDD specifications also confirm that minimum wage effects are typically decreasing in magnitude to roughly zero as we proceed from the bottom of the wage distribution to the median and higher percentiles. These upper percentiles act as placebo percentiles, as we should not expect any effects of the minimum wage that far up into the wage distribution.

We performed several robustness checks on our main TWFE specification, which is estimated by TSLS in levels.[35] Our results are robust to excluding individuals working in tipped occupations; broadening our age restriction to 16-74 (rather than 18-64); dropping individual wage observations below $2 per hour (rather than below $1 per hour); using only person weights (rather than person weights multiplied by labor hours); working with unbalanced state-year panels (rather than balanced panels, so as to allow more state-year cells for our analyses of

[35] Online Appendix Table A4 shows first stage diagnostic statistics for our TSLS levels and first difference specifications. Our core specifications survive standard under- and weak-identification tests.

Black-White and Hispanic-White inequality); and including a rich set of state-year time-varying controls measuring the demographic and education composition of the labor force.[36] Online Appendix Figures B4a-c display, for each of these robustness checks, our between-group coefficient estimates from our main TWFE specification. Our results are highly consistent across specifications.

Stacked Event Study Results

Our stacked event study specifications allow us to trace out the dynamic effects of the minimum wage on between-group inequality. Furthermore, they allow us to test the parallel trends assumption. This assumption is necessary for our estimates to be interpreted as causal.[37] We briefly note our results here, but we defer associated figures to the Online Appendix. First, we find a strong first-stage relationship between minimum wage events and the minimum wage bite, with no evidence of any pre-trend (Appendix Figure B5). Second, considering the estimated minimum wage effects on the inequality measures, the assumption of parallel pre-trends (in log wage percentiles) largely holds up to scrutiny across the wage distribution (Appendix Figures B6a-c). None of the pre-treatment coefficients are jointly significant at $p<0.01$, although a few are marginally significant at $p<0.05$ or $p<0.10$.[38]

[36] Control variables include, in each state-year cell, the labor force shares of age groups 18-25, 26-54, and 55-64; the Black shares of men and women; the Hispanic shares of men and women; the union coverage share; the unemployment rate; and the college-educated share of men and women, and Black and Hispanic individuals. Data on union coverage come from Hirsch, Macpherson, and Vroman (2001).
[37] The parallel trends assumption in our setting is that, in the absence of minimum wage increases, log wage gaps would have proceeded in parallel across treatment and control states.
[38] There is some evidence that gender inequality may have been trending upward at the 10th percentile in treated states relative to control states (with an F-test *p*-value of 0.07 on the pre-treatment coefficients), suggesting that the SDD coefficient may underestimate the actual reduction in 10th percentile gender inequality (indeed, we do not find a significant reduction at the 10th, while we do find significant reductions at the 5th and 20th). For race and ethnic inequality, the strongest signs of pre-trends are the negative pre-trends at the 10th percentile for Black-White and Hispanic-White inequality (with F-test *p*-values for the pre-treatment coefficients of 0.02 and 0.03, respectively). This suggests that we may be overestimating racial/ethnic convergence due to the minimum wage at the 10th

Broadly, the event study results give us confidence in the results from the SDD specification, which we prefer for drawing conclusions about the magnitude of effects in our setting because it brings with it more statistical power than our coefficient-by-coefficient event studies. The dynamic event study results are also visually consistent with those from the SDD specification. Moreover, when the post-treatment coefficients are significant, as happens in the lower-tail, our event studies show that the minimum wage's effects on between-group inequality tend to be long-lasting, persisting up to 5 years after the minimum wage is increased (which is the furthest out we estimate effects).[39]

Employment and Weekly Earnings Effects

Though we find substantial evidence that minimum wage increases can improve the relative wages of disadvantaged subgroups, if minimum wages disproportionately reduce employment for these groups, total welfare effects would be ambiguous. Thus, although employment effects are not our main focus, we apply AMS-style regressions to analyze the effect of minimum wages on employment throughout the wage distribution. Here, we focus on within-group employment effects, rather than between-group inequality in employment outcomes, as we believe this margin to be more policy-relevant. For example, if minimum wage policies reduce Black and White employment equally (resulting in no apparent increase in Black-White employment inequality), policymakers would still likely consider this a negative outcome that would trade off against reductions in Black-White wage inequality.

percentile, but we remain largely confident in our findings for race and ethnic inequality generally at other lower percentiles of the wage distribution.

[39] While not our main focus, we also followed earlier literature by estimating the effects of minimum wages on within-group inequality. Online Appendix Tables A5 and A6 and Figures B7a-c show that, consistent with our between-group results, the minimum wage lowers inequality at the bottom of the distribution by more for low wage groups such women, Black or Hispanic workers than for male or White workers.

Methodological Approach

We estimate the effect of minimum wage policies on weekly earnings (including zeros for the non-employed) and employment rates. The first dependent variable incorporates effects operating through changes in wages, and employment at the intensive margin (through hours worked) and the extensive margin (through employment). However, we note that consideration of the weekly earnings variable in the CPS MORG would not capture effects on *annual* earnings or possible effects on non-pecuniary amenities such as fringe benefits (see, e.g., Clemens, Kahn, and Meer 2018 and Clemens 2021).

To analyze employment effects, we must estimate where non-employed workers would place in the wage distribution had they worked. To do this, we take a standard wage index approach (first appearing in the minimum wage literature in Card and Krueger 1995) by using a linear equation with several demographic and human capital predictors to rank individuals, within each state-year cell, along a latent wage distribution.[40] We estimate this model within each state-year cell on employed workers with observed wages and obtain a predicted wage index for every individual (with or without wages).[41] We then assign all individuals to wage index bins (e.g., all workers below the 30th percentile of the wage index, or all workers between the 10th and 20th percentile of the wage index). Within these assigned bins, we compute three measures to use as dependent variables in our employment analysis: median log wages (only among the employed), log of mean employment, and log of mean weekly earnings (including

[40] Our predictors include indicators for sex and race/ethnicity, a quadratic polynomial in years of schooling, quartic polynomials in age and potential experience, an indicator for ages 18-19, binned educational attainment (mutually exclusive dummies for high school or less, some college, bachelor's degrees, and advanced degrees), and linear interactions of potential experience with each education dummy.

[41] Our construction of these state-year cells is identical to that of our original data cleaning procedure. For weekly earnings, values below $40 (in 2020 dollars) are dropped.

zeros for the non-employed).[42] We conduct this procedure separately for the pooled sample (including all workers), the male and female samples, and each of the White, Black, and Hispanic samples. Considering each bin and each group, we then estimate the same AMS-style TWFE regressions that underpin our main analyses.

Results

Figures 4a-b contains our main results for employment.[43] They show for each group the effect of the minimum wage bite within predicted wage bins. The first entry is for the 30th percentile and below (where minimum wage effects are expected to be concentrated), followed by decile-specific bins centered at the percentiles indicated on the x-axis. First, we consider the results for log wages among the employed. These results constitute a check on the wage index approach. The results generally reproduce the conclusions of our earlier analyses of wage inequality. For each group, large positive effects of the minimum wage on wages are observed at the lowest part of the latent wage distribution, and these effects always fade out to non-significant null effects at the median and upper regions of the wage distribution.

We find that earnings effects are similar to hourly wage effects. Earnings effects are almost always positive and usually larger for the lower percentiles, although results for Hispanic individuals peak at the 20th percentile. Focusing on the summary results for those below the 30th percentile, effects are of a similar size for men and women and larger for Black and Hispanic individuals than White individuals. These effects are significant for the pooled sample, men, women, and Hispanic individuals. For employment, we do not find any significant negative

[42] We take the mean of employment (which can take only 0 or 1 values) and weekly earnings (which can take 0 or positive values) within each state-year-wage index bin and then log transform the resulting averages.
[43] Online Appendix Table A7 contains the point estimates displayed in this figure.

effects at the mean or for any of the percentile bins. Indeed, many of the estimated employment effects are positive, although generally not significant. These employment results suggest that our findings about the effect of minimum wages on relative hourly wages of demographic groups are not offset by employment losses.

The Simulated Effects of Minimum Wage Policies Since 1979

In this section, we report results from simulations of counterfactual between-group wage inequality from 1979 to 2019 using estimates from modified versions of our TWFE and SDD specifications.[44] These simulations allow us to assess the impact of federal/state minimum wage policies on national-level inequality between groups. We show for each of four intervals (1979-1989, 1989-1998, 1998-2007, and 2007-2019) how much higher (or lower) between-group inequality would have been in the end year if US state/federal minimum wages had been fixed at their levels in the start year. Each interval identifies a distinct period in the evolution of the minimum wage (see Figure 1).

Methodological Approach

We consider two groups g and g', a percentile p, and an interval extending from year t_0 to year t_1. Our objective is to compare the actual change from t_0 to t_1 in p-th percentile wage inequality between groups g and g' to the simulated change in inequality under the counterfactual that the minimum wage structure of year t_0 (including both state and federal

[44] Wursten and Reich (2023) also produce counterfactual estimates of the effects of the minimum wage on the race wage gap. However, their estimates are either for a subpopulation of workers with high school education or less and earning less than $20 per hour or (for economy-wide estimates) assume all other workers were unaffected by minimum wage policy.

policies) prevailed in year t_1. The simulated change minus the actual change provides the counterfactual effect of the minimum wage on the pay gap of the two groups at percentile p.

We broadly adopt the simulation approach devised by Lee (1999). Specifically, we increment individual wage observations by assigning them to integer wage percentiles within state-year cells and then adjusting their wages to account for the effect of the minimum wage using percentile-specific regression coefficients from the TWFE or SDD specifications. This requires us to use $w_{st}(p) - w_{st}(50)$ as dependent variables in equations (1) and (2) rather than $w_{st}^{g}(p) - w_{st}^{g'}(p)$, since there is no natural way to use wage gap estimates to increment individual wage observations.

Our main approach uses regression coefficients from pooled equations, i.e., that include all demographic groups, estimated for each percentile p on the full sample of 2050 state-year cells from 1979 to 2019 (or, in SDD, the full sample of state-year-dataset cells). These regression coefficients are used to increment wages, under counterfactual minimum wage policies, based on individual workers' assignments to pooled state-year wage percentiles. Using these counterfactual wages, we then obtain the resulting between-group wage differentials at each percentile. This approach assumes the effect of the minimum wage at each percentile (estimated from our pooled regressions) to be the same for individuals regardless of their gender, race, or ethnicity. Therefore, it only measures the effect operating through disadvantaged groups' tendency to be concentrated at lower percentiles.

It is possible, however, that responsiveness to the minimum wage at given wage levels differs across groups.[45] Thus, we also present results from group-specific regressions, limiting

[45] Wursten and Reich (2023) find that the impact of the minimum wage is more responsive for Black than White workers within the subsamples they consider.

the regression samples to balanced panels of states with at least 50 worker observations in every state-year-group cell. The results suggest that taking into account group-specific differences in coefficients would not affect our conclusions. Hence, we prefer the pooled regressions since the larger sample sizes facilitate more precise wage percentile estimates.[46] For more details on the counterfactual simulation method, see our online Methods Appendix.

Simulation Results

Figure 5 presents results from our counterfactual simulations, derived from the pooled TWFE specification, for each pair of demographic groups and for each interval. Rather than plotting the exact quantiles of the predicted counterfactual wage distribution (which are somewhat noisy), we plot a smoothed moving average of the percentile-specific predictions, using a centered window with a width of 5 percentiles. We obtain standard errors using state-clustered bootstraps. The blue shaded regions represent 95% confidence intervals. Point estimates and standard errors at selected percentiles are displayed in Online Appendix Table A8a.

Simulations based on the pooled TWFE estimates indicate that the decline in the real value of the minimum wage from 1979 to 1989 substantially increased gender, racial, and ethnic inequality at the bottom of the wage distribution. For example, had 1979 minimum wages remained in 1989, the wage gap between women and men at the 10th percentile would have been 0.05 log points (5 percent) smaller; between Black and White workers, 0.06 log points (6 percent) smaller; and between Hispanic and White workers, 0.02 log points (2 percent) smaller.

[46] Lee (1999) also uses pooled regressions to compute counterfactual wages.

Statistically significant effects on inequality by gender, race, and Hispanic ethnicity during this period are present up to approximately the median.

These point estimates are substantial when compared to both the level of lower-tail wage inequality between groups and the actual changes in inequality observed over this period. Between-group wage gaps below the 15th percentile are generally below 0.20 log points (22 percent), and typically around 0.10-0.15 log points (11-16 percent).[47] Observed trends are also relevant. For example, the 0.06 log point (6 percent) increase in gender inequality from 1979 to 1989 at the 10th percentile would have been almost entirely eliminated. And the 0.10 log point (11percent) Black-White inequality increase at the 10th percentile would have been substantially reduced. Effects for Hispanic-White inequality are more muted, but still non-negligible.

Turning to later periods, 1989-1998 shows smaller impacts for all three demographic comparisons, although at low wage percentiles up to about the median, effects are still statistically significant in the expected direction. Recall that this was a time when the minimum wage increased, so keeping minimum wages at the initial level would have increased inequality. The results in Figure 5 indicate that this increase reduced gender, racial, and ethnicity gaps at the lower end of the wage distribution.

From 1998 to 2019, changes in minimum wages had relatively little effect on gender or race inequality at the bottom of the distribution. However, for Hispanic-White inequality, while effects were very small between 1998 and 2007, after 2007 there were statistically significant and economically important effects at the bottom of the distribution. These effects are non-negligible in magnitude (particularly from the 10th to 25th percentiles, peaking at an effect of about 0.05 log points (5 percent) at the 18th percentile) and retain statistical significance up to

[47] An exception is the Hispanic-White gap at the 15th percentile that has often been as high as 0.3 log points. See Figures 2a-c, discussed above, for time series plots of these measures.

about the median. These unique results for Hispanic-White inequality since 2007 reflect both Hispanic individuals' continued disadvantaged position in the wage distribution (see Online Appendix Figure B2) as well as their concentration in states that legislated especially large minimum wage increases over this period.

Online Appendix Figure B8a plots simulation results from the pooled SDD specification, and online Appendix Table 8a provides point estimates at selected percentiles. These estimates have similar signs to the pooled TWFE results, though the pooled SDD estimates tend to be slightly smaller.

We also conduct simulations based on group-specific regression equations, estimated using the more restricted set of states described above. We compare group-specific to pooled regression results in Online Appendix Figure B8b and Online Appendix Table A8b, which show broadly similar results across the two approaches. Point estimates from group-specific regressions are somewhat larger in magnitude, particularly for the Black-White comparison in 1979-1989 and 1989-1998.[48] However, differences in estimated effects between the two approaches are small and never statistically significant.

The Disparate Impacts of Geographic Concentration

Next, we consider how the geographic concentration of racial and ethnic groups in different states interacts with sub-national minimum wages to create disparate impacts of minimum wage policies.[49] Table 2 shows the average bite of the minimum wage for each demographic group for the bookend years from each of our four periods between 1979 and 2019.

[48] This accords with Wursten and Reich's (2023) finding for the Black-White wage gap noted above.
[49] Online Appendix Figure B9 displays maps of racial and ethnic geographic concentration across US states. Online Appendix Figure B10 provides Duncan segregation indexes for measures of Black-White, Hispanic-White, and Black-Hispanic state-level segregation.

Not surprisingly, there is no observable gender gap in the bite since men and women are similarly distributed across states, although (especially) female labor force participation rates may vary. In contrast, White, Black, and Hispanic workers do exhibit two periods of noteworthy differences. First, in 1979, Black workers were disproportionately subject to a higher (less negative) minimum wage bite. Since the prevailing minimum wage in this period tended to be the federal minimum, this reflects higher Black concentration in southern states, which had lower median wages. Second, in 2019, Hispanic workers faced a disproportionately higher minimum wage bite. In this period, there was considerable variation in state minimum wages, and Hispanic workers disproportionately lived in states with high minimum wages.

To illustrate the implications of these geography-induced differences in minimum wage bite, we adjust the sample weights for Hispanic and Black individuals in our simulations so as to match the geographic distribution of White workers.[50] We then apply the original pooled TWFE coefficients to increment wages, but we use the adjusted weights to estimate the observed and counterfactual wage percentiles for each group and for each period. We interpret these reweighted results as minimum wage effects operating solely through the relative positions of Black and Hispanic workers in their state-year wage distributions. We subtract these reweighted results from our primary simulation results to infer the influence of differences in spatial distribution.

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[50] Specifically, within each period (e.g., 1979-1989) and considering the relevant groups (e.g., White and Hispanic workers), we use logistic regression to regress an indicator for White on U.S. state fixed effects; these logistic regressions are weighted using our standard hours weights (w_i). We then obtained fitted values from this regression ($\hat{p}_i$) and create adjusted hours weights (w_i^*) equal to w_i if person i is White and equal to $w_i \times \hat{p}_i/(1-\hat{p}_i)$ otherwise. These adjusted weights ensure that the spatial distribution of White and Hispanic (or Black) workers is approximately identical across U.S. states during the relevant sample period. Note that we did not use a multivariate reweighting procedure such as that in DiNardo, Fortin and Lemieux (1996) because we are interested in the overall location differences between ethnic groups rather than location differences between ethnic groups of, say, a given education and age structure.

Table 3 displays these spatial distribution effects, derived from the pooled TWFE specification, with results shown for the two salient periods identified above: Black-White inequality for 1979-1989 and Hispanic-White inequality for 2007-2019. Figure 6 shows smoothed estimates throughout the wage distribution for these two cases.[51] It is evident that geography played a role in each case. Had the spatial distribution of Black individuals matched White individuals from 1979 to 1989, the reduction in racial inequality associated with retaining the minimum wage at its higher 1979 level would have been smaller at wage percentiles below the median, and especially below about the 25th percentile. Had the spatial distribution of Hispanic individuals matched White individuals from 2007 to 2019, this period's increases in federal and (especially) state minimum wages would have reduced Hispanic-White inequality by less at wage percentiles below the median. As may be seen in Table 3 and Figure 6, the magnitudes of the geographic effects for both Black and Hispanic workers are substantial relative to the baseline (unadjusted) effects during the relevant period. Averaging across the percentiles below the median shown in Table 3, group differences in location accounted for 27 percent of the baseline effect for Black workers in the 1979-1989 period and 67 percent of the baseline effect for Hispanic workers in the 2007-2019 period.[52]

Counterfactual Effects of a $12 (in 2020 Dollars) Federal Minimum Wage

As of 2025, the federal minimum wage was $7.25. In this section, we assess the effects of a hypothetical federal minimum wage of $12 (in 2020 dollars), a substantial increase in the

[51] Online Appendix Figures B11a-d show the smoothed estimates at all percentiles for each type of racial and ethnic inequality and for all four periods. Online Appendix Table A9 displays the associated point estimates. As expected, in periods without important group differences in exposure, reweighted effects are very close to the original effects.
[52] This is calculated as the simple average of Remaining Effect/Baseline Effect.

minimum wage that falls within our estimation sample.[53] Note that $12 in 2020 is equivalent to $14.49 as of 2025Q2, close to the $15 minimum wage that has been proposed by some advocates. Table 4 displays inequality levels and the predicted effects of a $12 minimum wage using our pooled TWFE specification applied to wage data from 2015 to 2019.[54]

Our results show that raising the federal minimum wage to $12 would have significant and economically substantial effects on between-group inequality at the bottom of the wage distribution for all three demographic comparisons. Specifically, comparing the minimum wage effects and inequality levels in Table 4 indicates that a federal minimum wage of $12 could reduce lower-tail (i.e., beneath the 15th percentile) gender and racial/ethnic inequality by between a quarter to half of existing levels of inequality.[55] Finally, for all three demographic comparisons, statistically significant reductions in mean between-group inequality are also predicted, although the implied reductions are very small compared to pre-existing mean gaps.

Conclusion

We find that minimum wage policies played a significant role in the trajectory of inequality between demographic groups in the 1980s but that their importance has decreased over time. From 1979 to 1989, the large decline in the real federal minimum wage led to substantial increases in gender, racial, and ethnic wage gaps at the bottom of the wage distribution. After 1989, minimum wage policies had little effect on gender wage inequality. For

[53] Online Appendix Figure B12 shows the observed distribution of minimum wage bites over the period 2015-2019 together with counterfactual bites under federal minimum wages of $10, $12, and $15 (in 2020 dollars). This figure clearly demonstrates that a federal minimum wage above $12 (in 2020 dollars) would lead to minimum wage bites that would be far outside the existing variation in our 1979-2019 sample.
[54] See Online Appendix Table A10 for corresponding estimates based on the pooled SDD specification.
[55] For the Black-White and Hispanic-White gaps, absolute declines of comparable magnitudes—between 0.025 and 0.06 log points—are present up to the 40th percentile; however, since raw group wage gaps also increase in size as one moves from the lower tail to the median, these counterfactual changes become relatively less impactful.

racial/ethnic inequality, minimum wage policies continued to have an economically meaningful impact on Black-White inequality from 1989 to 1998 and on Hispanic-White inequality from 1989 to 1998 and from 2007 to 2019. Minimum wage policies have continued to exert an economically important impact on the national Hispanic-White wage gap, in large part because Hispanic individuals lived in states with higher minimum wages. Indeed, we further contribute to the existing literature by documenting how spatial variation in state minimum wage policies in the United States has important consequences for the national wage distribution. Moreover, we do not find any evidence of negative effects of minimum wages on employment.

While minimum wage increases since 2007 have not had discernible effects on gender inequality and inequality between Black and White workers, our evidence suggests this is largely because these changes have either been small or occurred in areas with broadly higher wages. Indeed, assuming the continued validity of our estimates of the minimum wage's effects, we show that raising the federal minimum wage to $12 in 2020 dollars ($14.49 in 2025Q2 dollars) would bring about economically meaningful reductions in lower-tail wage inequality by gender, race, and ethnicity.

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Tables

Table 1. Effects of the MW Bite on Between-Group Wage Gaps

	Panel A. Male-Female			*Panel B. White-Black*			*Panel C. White-Hispanic*		
	Raw Gap	TSLS Levels	SDD Dosage	Raw Gap	TSLS Levels	SDD Dosage	Raw Gap	TSLS Levels	SDD Dosage
Mean	0.24	-0.01	-0.04	0.22	-0.08	-0.10	0.40	-0.08	-0.07
		(0.02)	(0.02)		(0.03)	(0.05)		(0.03)	(0.06)
p5	0.09	-0.12	-0.18	0.05	-0.13	-0.18	0.12	-0.28	-0.37
		(0.03)	(0.05)		(0.05)	(0.07)		(0.04)	(0.16)
p10	0.13	-0.11	-0.03	0.09	-0.11	-0.16	0.21	-0.23	-0.22
		(0.02)	(0.05)		(0.05)	(0.07)		(0.04)	(0.07)
p15	0.16	-0.08	-0.03	0.13	-0.17	-0.15	0.27	-0.15	-0.13
		(0.02)	(0.03)		(0.04)	(0.08)		(0.04)	(0.05)
p20	0.18	-0.10	-0.09	0.16	-0.24	-0.16	0.32	-0.20	-0.20
		(0.03)	(0.04)		(0.04)	(0.07)		(0.04)	(0.06)
p30	0.21	0.00	-0.06	0.20	-0.03	-0.07	0.39	-0.09	-0.13
		(0.02)	(0.04)		(0.04)	(0.08)		(0.04)	(0.06)
p40	0.23	0.01	-0.03	0.22	-0.07	-0.15	0.43	-0.06	-0.09
		(0.03)	(0.04)		(0.04)	(0.07)		(0.02)	(0.06)
p50	0.25	0.02	-0.03	0.24	-0.05	-0.08	0.45	-0.03	-0.09
		(0.02)	(0.03)		(0.06)	(0.06)		(0.03)	(0.07)
p70	0.26	0.03	0.01	0.27	-0.02	-0.05	0.48	-0.01	0.05
		(0.02)	(0.03)		(0.05)	(0.07)		(0.04)	(0.08)
p90	0.30	0.02	0.02	0.30	-0.03	0.04	0.49	-0.09	0.09
		(0.04)	(0.04)		(0.09)	(0.09)		(0.09)	(0.12)
MW Bite			1.10			1.05			1.11
			(0.06)			(0.07)			(0.07)
Events			69			41			43
Observations	2050	2050	18629	1066	1066	11266	451	451	9659

Notes: This table shows point estimates at each percentile from our direct between-group specifications, in which a p-th percentile measure of between-group inequality is regressed on either a quadratic in the minimum wage bite or an event-study indicator for minimum wage increase events. Specifications include state and year fixed effects and state-specific linear trends. Specifications estimated by two-stage least squares (TSLS) in levels and by the stacked difference-in-difference (SDD) dosage estimator are shown. Standard errors are clustered at the state-level in the TSLS levels specification and at the state-by-dataset level in the SDD dosage specification (the latter are bootstrapped).

Table 2. Average MW Bites Experienced by Different Groups

	1979	1989	1998	2007	2019
National	-0.64	-0.94	-0.83	-0.93	-0.89
	(0.12)	(0.11)	(0.12)	(0.13)	(0.16)
Men	-0.64	-0.94	-0.83	-0.93	-0.89
	(0.12)	(0.11)	(0.12)	(0.13)	(0.16)
Women	-0.64	-0.94	-0.83	-0.93	-0.89
	(0.12)	(0.11)	(0.12)	(0.13)	(0.16)
White	-0.64	-0.95	-0.83	-0.93	-0.90
	(0.12)	(0.11)	(0.12)	(0.13)	(0.16)
Black	-0.60	-0.94	-0.82	-0.96	-0.93
	(0.14)	(0.12)	(0.13)	(0.13)	(0.14)
Hispanic	-0.65	-0.93	-0.81	-0.93	-0.85
	(0.11)	(0.10)	(0.11)	(0.11)	(0.17)

Notes: This table shows the average minimum wage bites experienced by gender, racial, and ethnic groups in several years from 1979 to 2019. Each row uses group-specific sample weights to compute national weighted means of the minimum wage bite experienced by each group in a given year. Empirical standard deviations (across states) are shown in parentheses.

Table 3. Decomposing the Effect of Geography on Between-Group Inequality

	Baseline Effect (Total)	Reweighted Effect (Wage/Positional)	Remaining Effect (Geographic = Total - Positional)
Panel A. Black-White Inequality, 1979-1989			
p5	-0.050	-0.028	-0.022
	(0.005)	(0.003)	(0.004)
p10	-0.056	-0.046	-0.009
	(0.005)	(0.005)	(0.002)
p15	-0.047	-0.023	-0.023
	(0.006)	(0.003)	(0.003)
p20	-0.018	-0.020	0.002
	(0.005)	(0.005)	(0.002)
p40	-0.017	-0.011	-0.006
	(0.005)	(0.003)	(0.002)
p50	-0.014	-0.011	-0.002
	(0.004)	(0.003)	(0.001)
Panel B. Hispanic-White Inequality, 2007-2019			
	Baseline Effect (Total)	Reweighted Effect (Wage/Positional)	Remaining Effect (Geographic = Total - Positional)
p5	0.015	0.003	0.012
	(0.003)	(0.002)	(0.002)
p10	0.006	-0.003	0.009
	(0.002)	(0.001)	(0.002)
p15	0.030	0.016	0.014
	(0.004)	(0.002)	(0.003)
p20	0.038	0.026	0.012
	(0.005)	(0.003)	(0.002)
p40	0.016	0.011	0.005
	(0.004)	(0.003)	(0.002)
p50	0.007	0.004	0.003
	(0.003)	(0.002)	(0.002)

Notes: This table shows baseline and reweighted effects for our pooled TWFE counterfactuals for Black-White inequality in 1979-1989 and Hispanic-White inequality in 2007-2019. The baseline effects are our smoothed counterfactual estimates from the pooled TWFE specification. The reweighted effects are smoothed counterfactual estimates that combine the pooled TWFE coefficients with sample weights (for computing wage percentiles) that are adjusted so that, during the relevant period, Blacks/Hispanics had the same residential distribution across US states as Whites. The remaining effect can be taken as an estimate of the effect of geography on the counterfactual estimates. Standard errors around the smoothed baseline and reweighted counterfactuals (and their difference) are bootstrapped using 100 sets of pooled TWFE coefficients. Here we show only these two cases as they are the salient cases of group differences in the bite of the minimum wage. Point estimates for all group gaps and all periods are shown in Appendix Table A9.

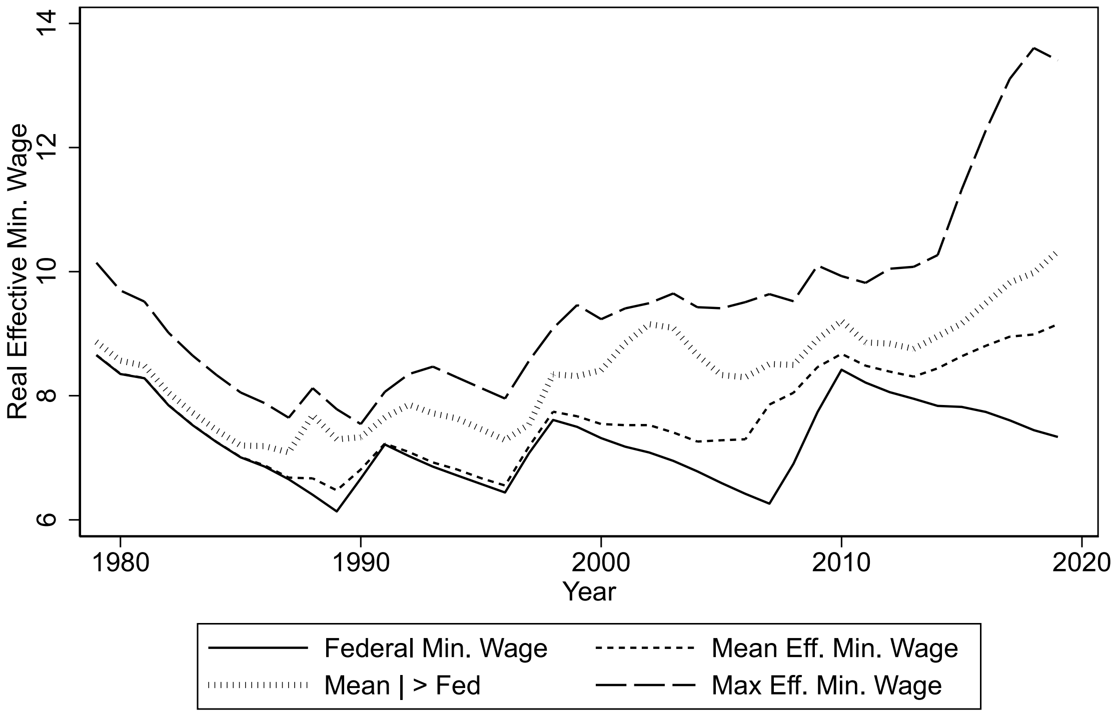
Table 4. Federal $12 (2020 Dollars) Minimum Counterfactuals Using TWFE, 2015-2019

	Male-Female Inequality		Black-White Inequality		Hispanic-White Inequality	
	Raw Gap	Predicted Change	Raw Gap	Predicted Change	Raw Gap	Predicted Change
Mean	0.18	-0.012	0.28	-0.020	0.35	-0.017
		(0.002)		(0.003)		(0.003)
p5	0.09	-0.047	0.12	-0.059	0.11	-0.032
		(0.004)		(0.004)		(0.006)
p10	0.08	-0.035	0.15	-0.052	0.13	-0.027
		(0.003)		(0.004)		(0.005)
p15	0.13	-0.039	0.20	-0.054	0.20	-0.038
		(0.006)		(0.008)		(0.007)
p20	0.13	-0.022	0.23	-0.047	0.26	-0.041
		(0.005)		(0.009)		(0.008)
p30	0.13	-0.009	0.26	-0.026	0.31	-0.024
		(0.005)		(0.008)		(0.007)
p40	0.17	-0.014	0.28	-0.023	0.35	-0.026
		(0.004)		(0.007)		(0.008)

Notes: This table shows counterfactuals of between-group inequality using the pooled TWFE estimates (for 1979-2019) for a $12 federal minimum wage (in 2020 dollars) using incremented data from 2015 to 2019. A $12 minimum wage in 2020 dollars is equivalent to a $14.49 minimum wage in 2025Q2 dollars. Standard errors computed by bootstrapping smoothed moving averages of the counterfactuals at each percentile are shown in parentheses.

Figures

Figure 1: Evolution of the real effective minimum wage



Notes: This figure plots the trajectory of four summary measures of state/federal minimum wage policies over time since 1979. Minimum wage data are from David Neumark's monthly panel dataset of state/federal minimum wage policies. Minimum wages are converted to 2020 dollars using the GDP PCE deflator from the Bureau of Labor Statistics. Time series are weighted means (by total employment in labor hours) across all US states within each year.

Figure 2a: Evolution of gender inequality

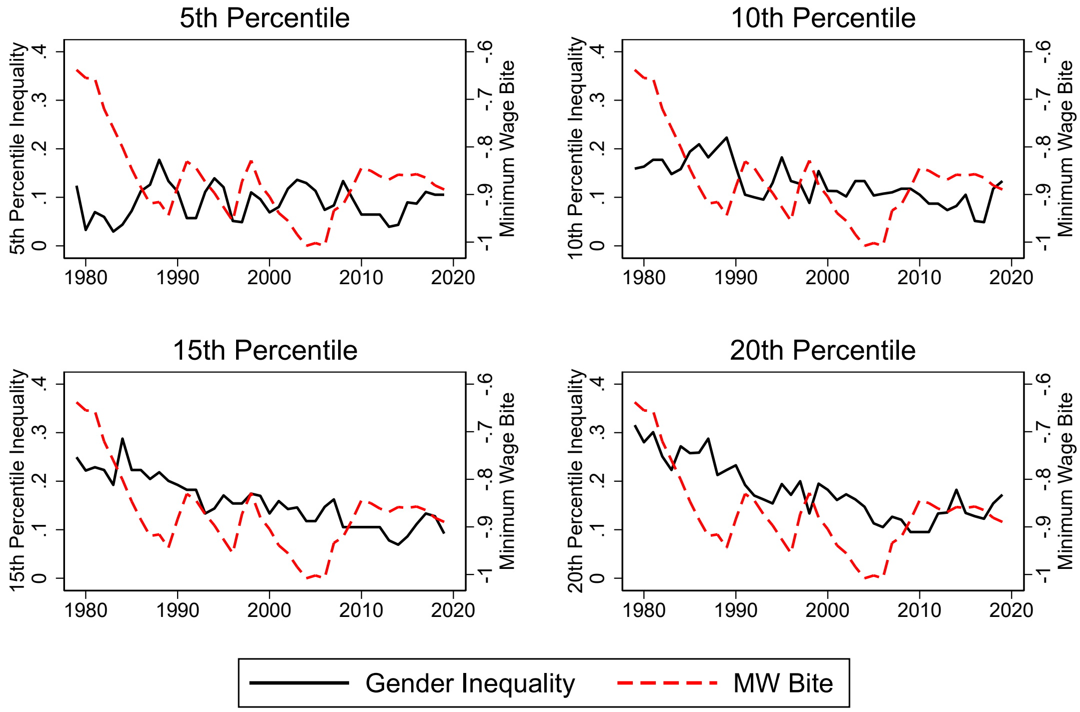


Figure 2b: Evolution of Black-White inequality

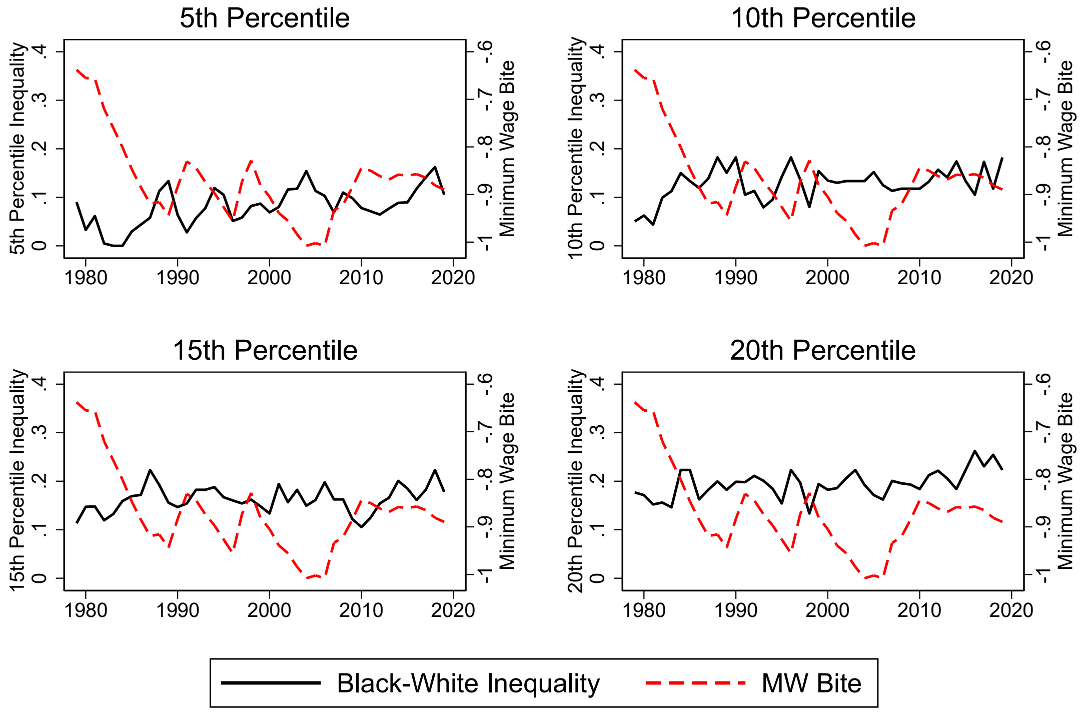
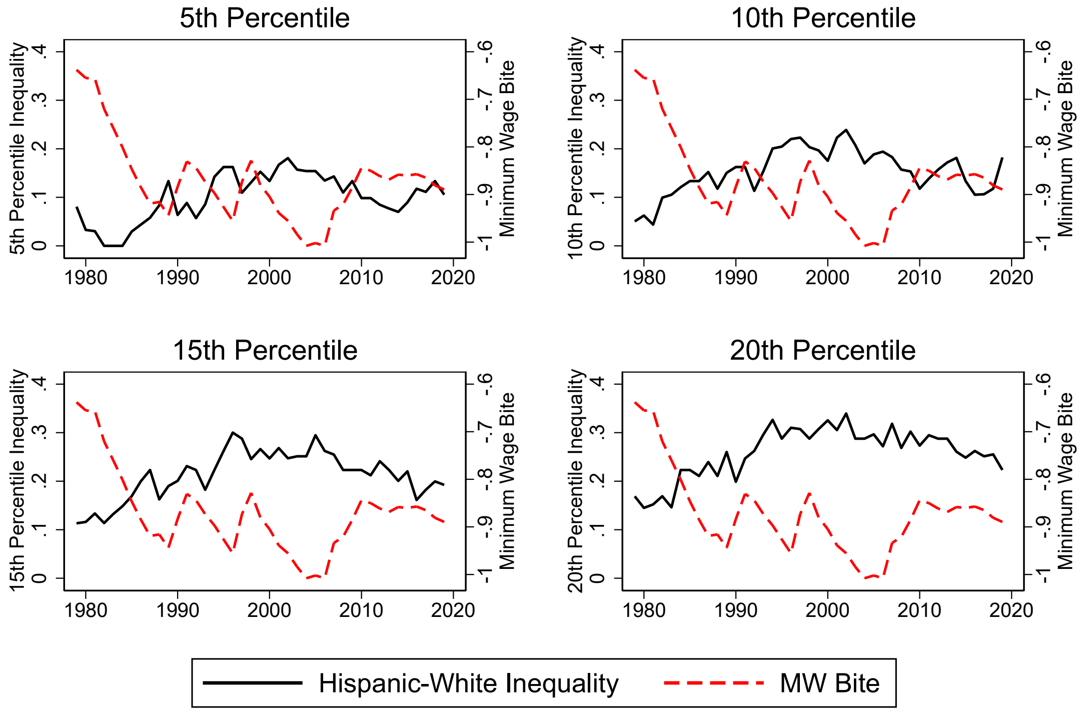


Figure 2c: Evolution of Hispanic-White inequality



Notes: This figure plots national-level log wage gaps between men and women (2a), Black and White Workers (2b), and Hispanic and White workers (2c) over time (solid black lines, left-hand axes) at the 5th, 10th, 15th, and 20th percentiles. The path of the bite of the minimum wage (long-dashed red line, right-hand axes) is superimposed. The log wage percentiles are computed using CPS MORG sample weights multiplied by reported work hours (our baseline weights in most specifications).

Figure 3a: Minimum wage effects for gender inequality

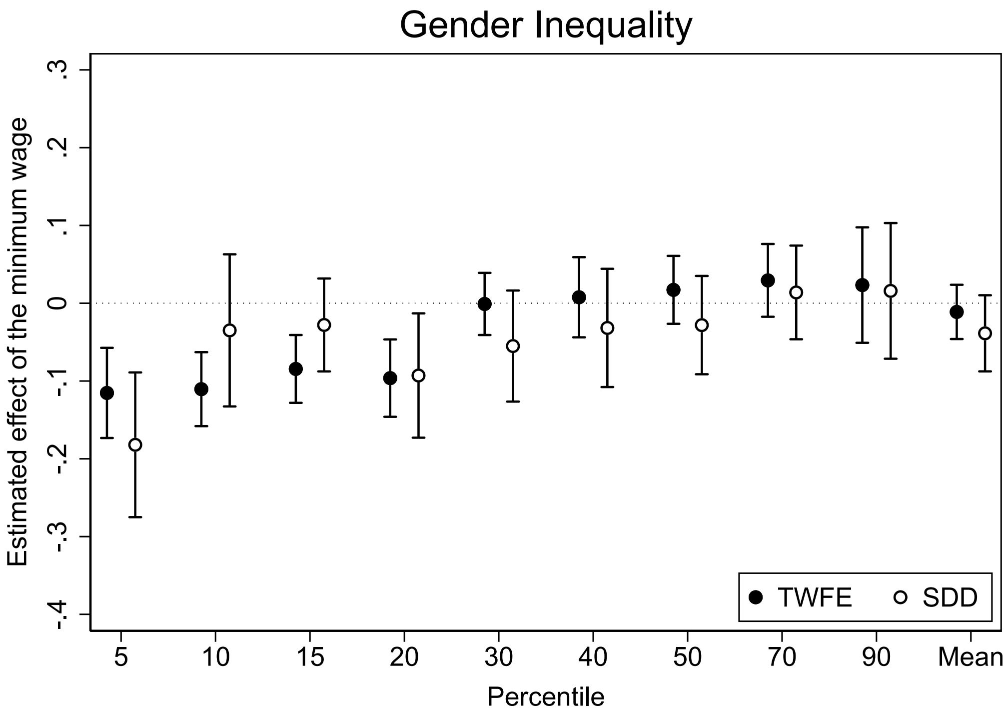


Figure 3b: Minimum wage effects on Black-White inequality

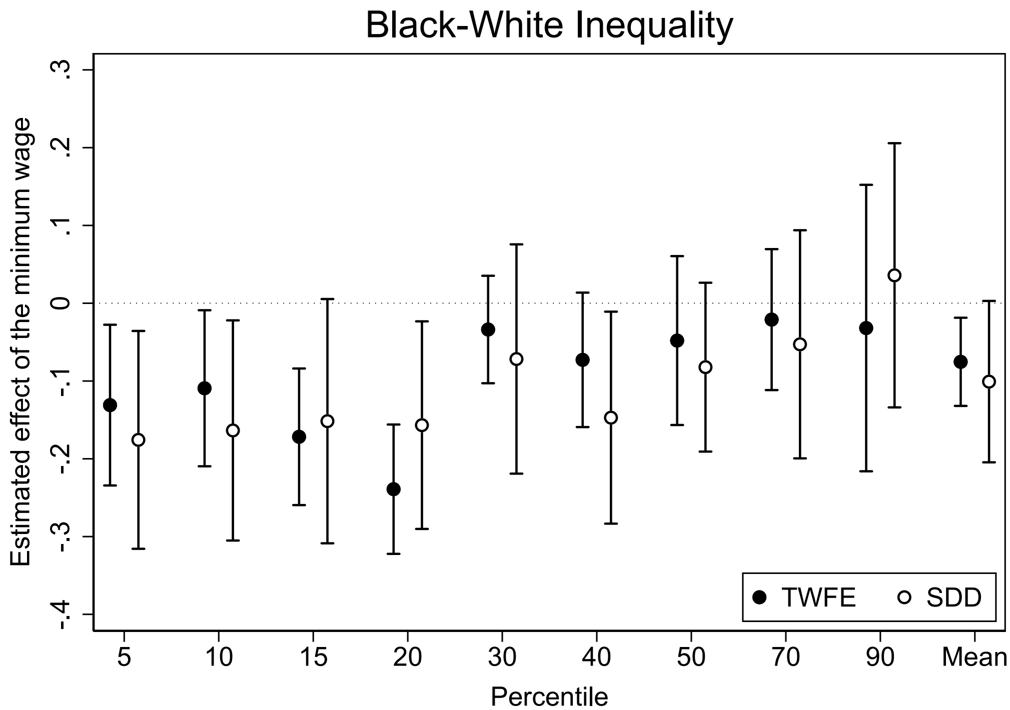
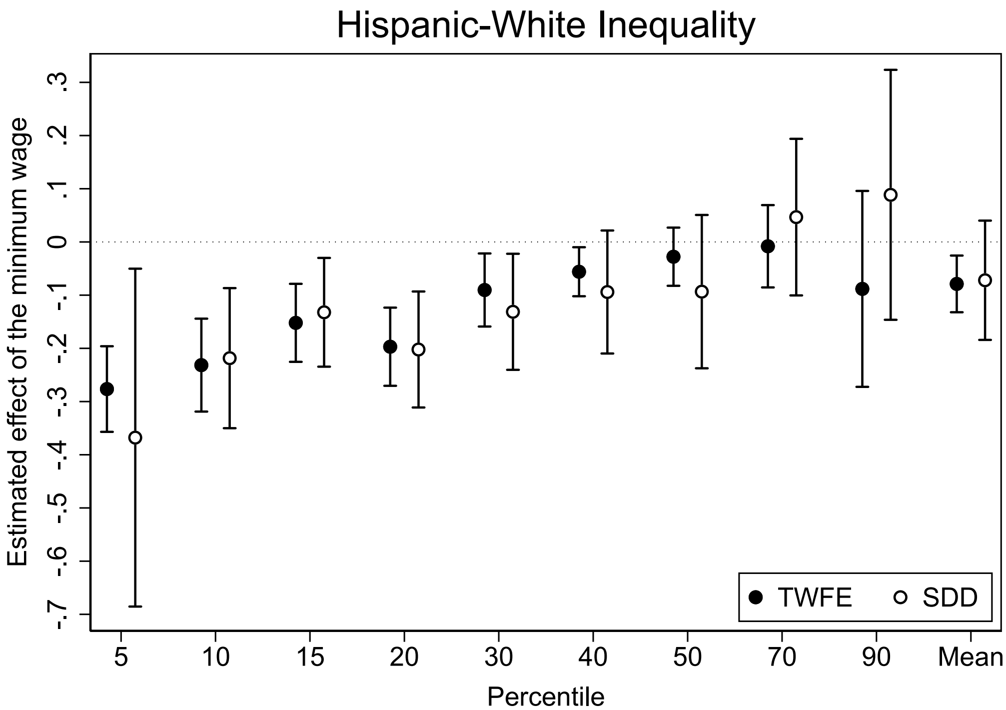


Figure 3c: Minimum wage effects for Hispanic-White inequality



Notes: This figure plots minimum wage effects from regressions that use inequality in log wages between men and women (3a), Black and White workers (3b), and Hispanic and White workers (3c) at various percentiles as the key dependent variable. At each percentile, as well as at the mean, we show effects from two-way fixed effects (TWFE) and stacked difference-in-differences (SDD) specifications. The confidence intervals in this figure are 95% confidence intervals constructed from standard errors that are clustered at the state-level (in the case of TWFE) or at the state-by-dataset level (bootstrapped in the case of SDD).

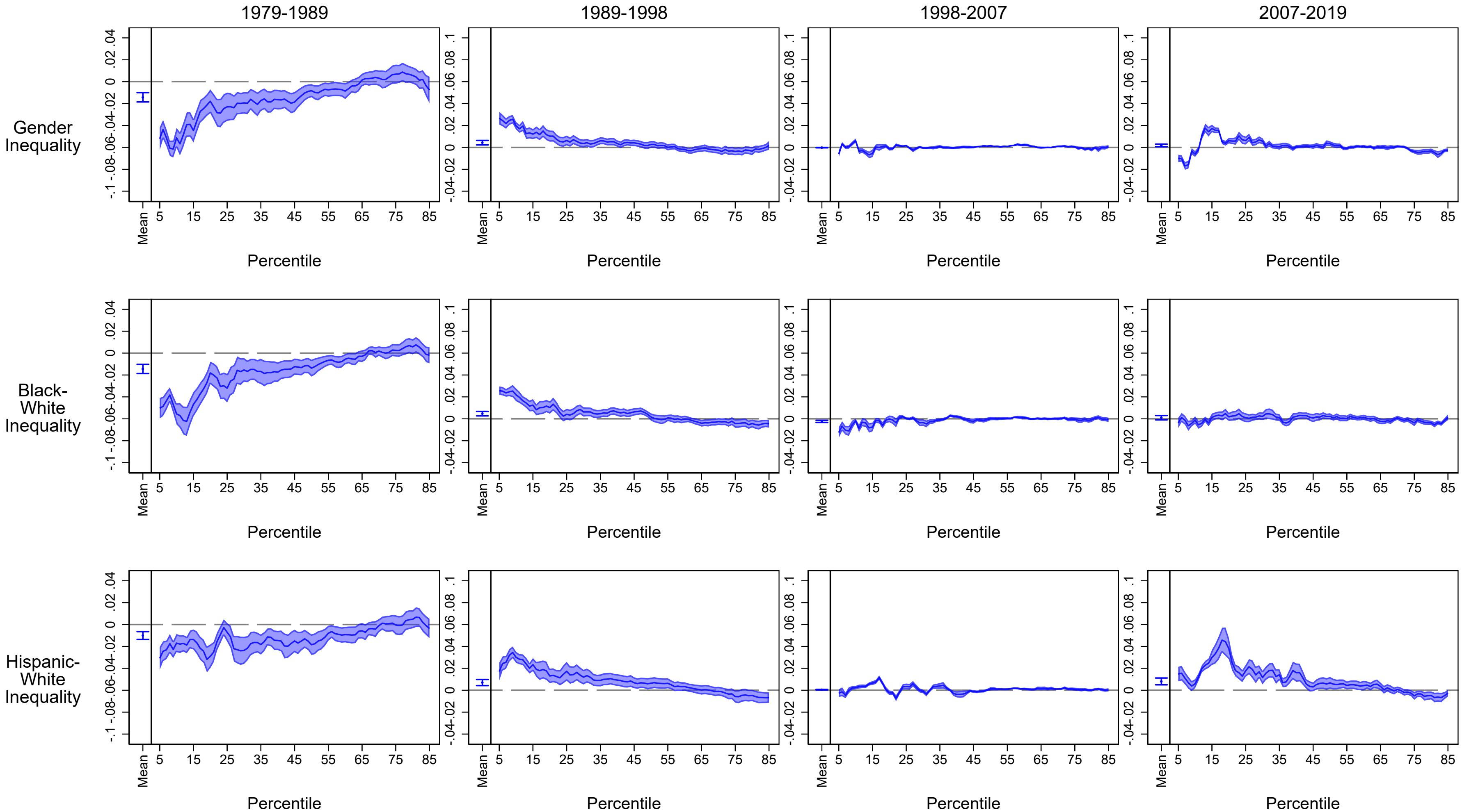
Figure 4a: Employment effects—Pooled, male, and female samples

Notes: These figures show estimates within predicted wage bins (i.e., 30th percentile and below as well as decile-specific bins centered at the percentiles indicated on the x-axis) for log median wages (for those with wages), log of mean weekly earnings (including zeros), and log of mean employment (including zeros). See the text for more details on how individuals are assigned to wage bins.

Figure 4b: Employment effects—White, Black, and Hispanic samples

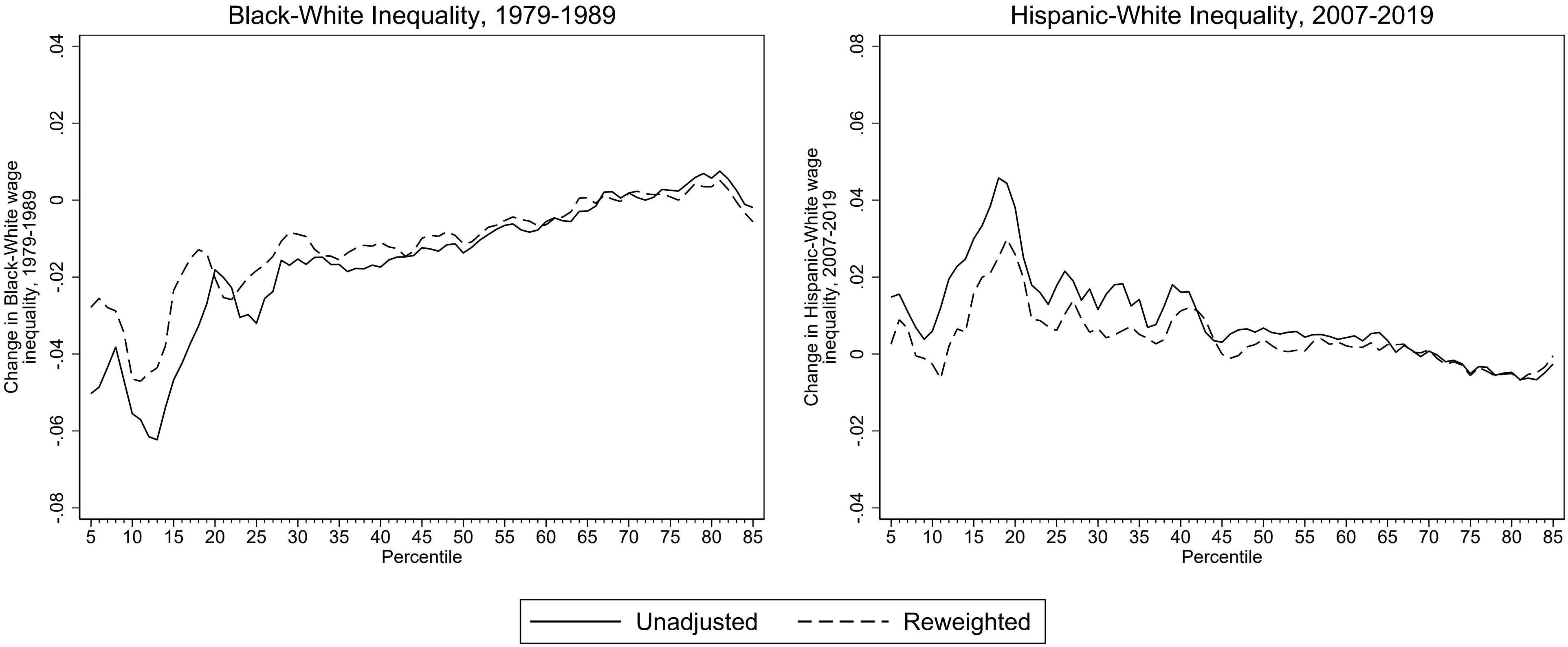
Notes: See notes to Figure 4a.

Figure 5: Counterfactuals using a pooled TWFE equation



Notes: These figures show smoothed moving averages of estimated counterfactual changes in between-group inequality for the pooled TWFE specification for each pair of groups and for each period. Note that the figures for 1979-1989 have a y-axis that runs from -0.10 to 0.04, whereas the figures for other periods have a y-axis that runs from -0.04 to 0.10. This difference in axis scale arises because counterfactual effects in the 1979-1989 period are expected to be negative, while effects in the later periods are expected to be positive. Standard errors shown are 95% bootstrapped confidence intervals around the smoothed (over percentiles) moving average of the counterfactual estimates.

Figure 6: Reweighting exercises for racial/ethnic inequality: two salient cases



Notes: In this figure, White workers are implicitly used as the numeraire group, and their spatial distribution across US states over the relevant period is never changed. The unadjusted lines display our main results, i.e., smoothed moving averages of predicted changes in between-group inequality over the period, assuming that the minimum wage structure of the initial year continued to prevail. The reweighted lines display these smoothed predicted changes after reweighting Black workers and Hispanic workers so that their distribution across US states was exactly equal to that of Whites workers over the period. Only reweighted effects for Black-White inequality from 1979-1989 and Hispanic-White inequality from 2007-2019 are displayed, as these are the periods with salient, non-negligible effects of geography in the lower-tail. All point estimates from racial/ethnic reweighting exercises are shown in Online Appendix Table A9.