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ABSTRACT

We use wage data from the Current Population Survey Merged Outgoing Rotation Group (CPS MORG) to study the effect of state and federal minimum wage policies on gender, race, and ethnic inequality throughout the wage distribution, focusing on lower-tail inequality between men and women, Blacks and Whites, and Hispanics and Whites. We use estimates from three empirical strategies — two reduced-form, one structural — to provide counterfactual simulations of between-group inequality over four key "epochs" of minimum wage policy changes since 1979. Declines in the real minimum wage during the 1980s slowed progress in narrowing between-group inequality during that period. Fairly muted shifts in national and state policies from 1989 to 1998 and 1998 to 2007 meant that the minimum wage was less important over those time spans. Since 2007, several states have opted for steep minimum wage hikes, which we find have especially improved Hispanics' relative wages, both because they continue to earn low wages and because they reside disproportionately in those states. Finally, we make predictions about the effect of raising the federal minimum wage to $12. We find that a change of this magnitude would reduce existing between-group wage gaps below the 15th percentile by 25-50% and would therefore have an economically important impact on gender, racial, and ethnic inequality in the present day.

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

I. Introduction

Economists have long been interested in how minimum wage policies affect the shape of the aggregate wage distribution, and there have been several notable empirical advances on the topic in the last three decades (DiNardo, Fortin, and Lemieux 1996; Lee 1999; Autor, Manning, and Smith 2012; Dube 2019; Fortin, Lemieux, and Lloyd 2021). Minimum wages may also play a role in narrowing wage disparities across groups that are differentially located in the wage distribution. However, these effects remain relatively unexplored in the contemporary US context. 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,[1] but only Wursten and Reich (2021) have explored the impact of minimum wages on Black-White gaps in the current context.[2] To our knowledge, the effect of minimum wages in the United States on the gender pay gap or the wage gap between Whites and Hispanics remains relatively unexamined. Yet, given the disproportionate location of these groups at the lower end of the wage distribution, the minimum wage may be a useful policy instrument for narrowing these gaps, one that may supplement more "group-specific" approaches like anti-discrimination policies.[3]

Studying the effects of minimum wages on wage gaps between groups is all the more important because progress in closing such disparities has slowed, stalled, or even reversed.

[1] See also Bailey, DiNardo, and Stuart (2021).
[2] Some authors provide heterogeneity analyses that include these subgroups, for example, Cengiz et al. (2019) for Blacks and women; and Wursten and Reich (2021) for Hispanics. Derenoncourt, Gérard, Lagos, and Montialoux (2022) present interesting findings on the impact of a national minimum wage on racial earnings gaps in Brazil, a middle-income country with a large informal sector.
[3] Beginning with Blau and Kahn (1992), a number of studies based on international comparisons have found that the overall wage compression associated with higher union density and minimum wages helps to reduce the gender pay gap (see Blau and Kahn 2017 for a review). There have also been within country analyses. See, for example, Caliendo and Wittbrodt (2022) for a recent study on the impact of minimum wages on the gender wage gap in Germany.

Today, at the mean, women earn 18 percent less than men, while Blacks and Hispanics earn 21 and 25 percent less than Whites, respectively (Bureau of Labor Statistics 2022). 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 (Rodgers 2019; Blau and Winkler 2022, Table 7-8),[4] 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 concentrated, inequality between groups has been similarly persistent. Our Current Population Survey Merged Outgoing Rotation Group (CPS MORG) data, described in detail below, indicate that the log wage gap between men and women at the 5th percentile has been stable at about 0.10 log points since 1979, while it has declined at the 10th, 15th, and 20th percentiles, plateauing in each case to about 0.10 log points today. Log wage gaps at these lower-tail wage percentiles between Blacks and Whites and between Hispanics and Whites are also stable or have increased since 1979.

The extent to which minimum wage policies can reduce inequality between groups is also a pressing question because raising the minimum wage is currently an active policy area. Since 2014, 28 states have raised their minimum wage (Economic Policy Institute 2021), with many of those states implementing $12 and $15 minimum wages (National Conference of State Legislatures 2022). More than a dozen municipalities have passed legislation to raise their local minimum wage above the federal level (Cengiz et al. 2019). Further, at the national level, there has been a push by some Congressional Democrats to enact a $15 federal minimum wage.[5]

[4] An analysis of male trends by Bayer and Charles (2018) that takes the non-employed population into account finds that the Black-White wage gap remains as large as it was in the 1950s.
[5] See, for example, https://www.cnbc.com/2021/01/26/democrats-reintroduce-15-minimum-wage-bill-with-unified-control-of-congress.html, accessed March 1, 2023.

In this paper, we provide new estimates of the effect of state and federal minimum wages on inequality between demographic groups throughout the entire wage distribution. During the period from 1979 to 2019, effective minimum wages were binding at about the 4th percentile of the wage distribution, on average across states, with a standard deviation of 2.5 percentage points.[6] Although there is evidence of spillovers from the minimum wage to wages above the minimum (see, e.g., Fortin, Lemieux, and Lloyd 2021), the minimum wage is nonetheless likely to have its largest effect on lower-tail inequality with a more muted impact on inequality at the mean.[7]

We use data on hourly wages since 1979 from the CPS MORG and apply several empirical methodologies to investigate the effects of the minimum wage on between-group inequality throughout the wage distribution. In the first portion of our analysis, we directly estimate this relationship by placing various measures of between-group inequality on the left-hand side of state-by-year panel regressions. Here, we largely follow the set-up of Autor, Manning, and Smith (2012), henceforth AMS. They study within-group wage inequality and estimate a quadratic relationship between the "bite" of the minimum wage—measured as the difference between each state's minimum wage and its median wage—and inequality at various percentiles of the overall wage distribution. We extend their method to a direct analysis of between-group inequality. We then confirm these estimates using new event study methods that are highly transparent regarding the sources of minimum wage variation and the existence (or

[6] Again, values are calculated using wage data from the CPS MORG.

[7] It is rare for changes in minimum wage policies in the US to affect one demographic group relative to another strongly enough to produce effects at the mean. One important counterexample is the minimum wage policies that were legislated during the late 1960s and early 1970s when the US federal government extended the minimum wage to cover industries with a high fraction of black workers, thereby generating a substantial increase in the mean black wage (Derenoncourt and Montialoux 2021). However, estimates obtained from such a large and highly-targeted policy change are likely not generalizable to the broader incremental changes that characterize contemporary changes.

not) of bias due to non-parallel pre-trends and heterogeneous treatment effects (Cengiz et al. 2019). Using this "direct effect" analysis framework, we find that minimum wages lead to substantial wage increases at the bottom of the wage distribution that are substantially larger for women, Blacks, and Hispanics than for men and Whites. Further, we find that the larger minimum wage effects for these groups are not constrained to the very bottom of the wage distribution, where the minimum wage most immediately "bites," but extend at least up to the 20th wage percentile, with effects even higher up the distribution for some demographic groups in some specifications.

In the second portion of our analysis, we use estimates from three different empirical strategies to conduct a suite of counterfactual analyses of the effects of minimum wages on gender, racial, and ethnic wage gaps. Here, we use coefficients from the estimated relationship between the wage distribution and minimum wage policies to simulate counterfactual wage distributions from the CPS MORG microdata. We obtain coefficients from strategies analogous to the AMS and event study designs mentioned above, and we also apply the distribution regression approach of Fortin, Lemieux, and Lloyd (2021), henceforth FLL. We use these counterfactual distributions to explore the 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. The periods we study represent four discernible epochs in the history of US minimum wage policy: the first, a sustained decline in the real federal minimum over the 1980s; the second, two increases in the real federal minimum during the 1990s; the third, a short period of inflationary decline during the early 2000s; the fourth, a major federal policy increase concurrent with sustained increases at the state level during the late 2000s and 2010s.

We find that minimum wage policies played a key role in the trajectory of inequality between demographic groups in the 1980s, working to slow progress as the real minimum wage declined, 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 — relative to the counterfactual of constant 1979 minimum wages — 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 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 the present day to exert an economically important impact on the national Hispanic-White wage gap, in part because Hispanics in particular continue to fall disproportionately near the bottom of the wage distribution but also in part because of where Hispanics live. Indeed, we further contribute to the existing literature by documenting how spatial variation in state minimum wage policies in the US has led to disparate impacts across demographic groups. We show that, after 2007 especially, Hispanic workers were exposed to larger increases in the bite of the minimum wage than Black and White workers were, and that this improved Hispanics' position in the national wage distribution relative to Whites.

We conclude our paper by conducting a counterfactual analysis of how increases in the federal minimum wage would affect today's 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 on between-group wage inequality at the national level.[8] Our estimates imply that such a sizeable increase in the federal minimum wage would lead to significant and economically important reductions in inequality at the bottom of the national wage distribution between men and women, Blacks and Whites, and Hispanics and Whites.

This paper makes four primary contributions to the minimum wage literature. First, we adopt a distributional approach to study between-group inequality, following in the footsteps of a set of minimum wage papers that have mostly focused on within-group inequality (DiNardo, Fortin, and Lemieux 1996; Lee 1999; Autor, Manning, and Smith 2012; Dube 2019; Fortin, Lemieux, and Lloyd 2021). We use this approach to study race, ethnic, and gender wage gaps, and we also combine it with new, more transparent event study methods. Our focus on the wage distribution stands in contrast to much of the work in this literature, which focuses on select groups of workers that are likely to contain a high proportion of individuals affected by the minimum wage, such as teens (Card 1992; Neumark and Wascher 1992; Neumark, Salas, and Wascher 2014; Allegretto et al. 2017) or wage earners making less than some fixed ratio of the minimum wage (e.g., Wursten and Reich 2021). Estimating the effects of minimum wage policies on gender and race/ethnic wage gaps throughout the earnings distribution means that our estimates are generalizable beyond specific sub-groups such as teens or workers in specific industries, such as restaurants, and hence avoid external validity issues associated with such targeting.

Second, 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

[8] We do not make predictions about the results of federal minimum wages higher than $12 because, for most states, such a policy would lie well outside the historical variation available in our sample, thus limiting the credibility of our predictions.

strongest prior evidence on Black-White inequality, for example, exploits unique and targeted policies legislated during the 1960s (Derenoncourt and Montialoux 2021), whereas we study the past four decades using all minimum wage policy variation over that period.[9] These simulations show to what extent the US suite of minimum wage policies has worked to reduce national-level inequality between groups.[10] We also use these simulations to show that our results are broadly consistent across methodologies. Third, we provide counterfactual predictions about the effects of a $12 federal minimum wage on national-level inequality between demographic groups. 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 studied.

II. Data

Our primary minimum wage variable is the annual modal effective minimum wage (i.e., the maximum of state and federal), which we obtain using a state-level monthly dataset made available by David Neumark (Neumark 2021). Our wage data come from the CPS MORG.[11] We construct a primary wage sample of workers between ages 18 and 64, excluding the self-employed.[12] We use the reported hourly wage when available. Otherwise, we compute an individual's hourly wage as his/her weekly earnings divided by hours worked at all jobs in the prior week. All wage figures are converted into 2020 dollars using the personal consumption

[9] Wursten and Reich (2021) focus on the 1990-2019 period, which does not include the 1980s when there was a substantial decrease in the real minimum wage.

[10] Other papers have studied heterogeneity in the effect of the minimum wage for women or Hispanics (e.g., Cengiz et al. 2019; Wursten and Reich 2021) but have not directly examined how these effects translate to inequality at the national level. This is important because the impact of the minimum wage on national between-group inequality, particularly for race/ethnicity, is dependent on the geographic distribution of different groups (the geographic distribution of men and women is quite similar).

[11] We obtain these data from the National Bureau of Economic Research's Public Data Use Archive.

[12] 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 to including older individuals ages 65-74.

expenditures (PCE) deflator published by the Bureau of Economic Analysis (BEA). 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 wages, as well as those with wages (reported or computed) below $1 per hour in 2020 dollars.[14] For our analysis of race/ethnic inequality, we focus on three groups: non-Hispanic Whites, non-Hispanic Blacks, and Hispanics of any race. Owing to sample size limitations in the CPS MORG at the state-year level, we do not analyze results for minorities separately by sex. For similar reasons, we do not directly study groups comprised of individuals in other categories (e.g., Asians or "other" races), but such individuals are included in our analyses of gender inequality. 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 and changes in CPS sample sizes.

Two of our main regression approaches, which follow the set-up of AMS, use state-year panel datasets. We construct these panels by collapsing the CPS MORG wage data into state-by-year-by-group cells, collecting observed quantiles of group-specific wage distributions within each cell. Our estimates of the effect of minimum wages on gender wage gaps 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

[13] In specifications that use group-specific percentiles, we winsorize wages at the state-by-year-by-group level. This decision, which follows AMS, has no bearing on the majority of results we show, which are quantile-specific, but would slightly affect estimates for mean wage gaps.

[14] We judge that wages in this range are unlikely to reflect an individual's actual wage rate. Such wages may arise due to errors in reporting or coding. In a robustness check, we instead drop those with wages below $2 per hour, and results remain similar. Note that the CPS's reported hourly wage variable (EARNHRE) does not contain tips, but the weekly earnings variable (EARNWKE) does. Due to the ambiguity in measuring the earnings of tipped workers, in another robustness check described below, we drop individuals in occupations likely to contain many tipped workers. This also makes little difference to our results.

[15] Using work hours to adjust the sampling weights is standard in the literature (e.g., AMS 2012). Our results are robust to specifications that weight all workers equally by using person weights only.

Black-White and Hispanic-White wage gaps, however, we impose bin-size restrictions to reduce noise in our estimates induced by the occasionally small cells used to compute within-group state-year log wage percentiles. 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 (Whites, Blacks, or Hispanics).[16] This provides us with 1066 observations (in 26 states) for Black-White comparisons and 451 observations (in 11 states) for Hispanic-White comparisons.[17] We emphasize that, although this bin-size restriction substantially restricts our sample size and narrows the geographic scope of our results, it also reflects the actual geographic concentration of race/ethnic minorities in the United States. Finally, small sample sizes in the CPS MORG prevent us from analyzing men and women separately within racial/ethnic groups. However, in some analyses of gender gaps, we control for the race/ethnic composition in each state-year cell, and in some analyses of race/ethnicity differentials, we control for gender composition. Our results are robust to such controls.

Our other main regression approach, which follows the set-up of FLL (2021), uses individual-level data to estimate wage distribution regressions using a probit equation. This approach involves the inclusion of demographic variables such as education and potential labor market experience.[18] We reserve a detailed description of how this worker-level dataset is constructed to the methods section below.

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

[17] Our counterfactual simulations, however, are conducted using individual-level data that do not adopt this sample restriction, as we discuss in further detail below.

[18] Note that this method, in contrast to the reduced form specifications, could in principle allow for analyzing group-by-sex inequality, for example, the gender gap in earnings among Blacks, or the Hispanic-White earnings gap among men. However, we found that CPS MORG sample sizes were still too small to make such analyses statistically informative.

III. 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, race, and ethnic wage inequality observed in the CPS MORG from 1979 to 2019 that 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 minimum (i.e., maximum of state/federal), the mean effective minimum among states with policies exceeding the federal minimum, and the maximum effective minimum.[19] The real federal minimum wage peaked in 1979, fell until 1990, and has since risen with three discrete policy events, each followed by a period of inflationary decline. The federal minimum was been in inflationary decline since 2010. State minimum wages rarely superseded the federal minimum wage until about 1998, when several states chose to raise their minimum wages in the face of 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. Finally, although trends in real minimum wages are important, the key explanatory variable in our two reduced-form regression approaches is the "bite" of the minimum wage, that is, the difference (within a state-year cell) between the minimum wage and the median wage. The bite is, therefore, always negative, and a "higher" bite is less negative and closer to zero. The bite can reflect shifts in median wages as well as explicit state/federal minimum wage policies. Online Appendix Figure

[19] The annual means in Figure 1 represent weighted averages of all US states in each year (weighted by total employment in labor hours). Online Appendix Figure B1a is a version of this figure that uses unweighted means across US states. Weighted and unweighted trends are quite similar.

B1b shows the evolution of the average bite since 1979.[20] Trends in the minimum wage bite and the real minimum wage largely coincide for the federal minimum wage and mean effective minimum wage (seen in Figure 1). However, trends in the bite are much flatter than the real values for the mean minimum wage conditional on being higher than the federal minimum wage. This suggests that states which increased their minimum wages above the federal level also tend to be those that have had a larger growth in median wages.

Next, we present trends in gender, race, and ethnic wage gaps, which are the primary focus of this paper. Figures 2a-c display the evolution since 1979 of between-group disparities at the 5th, 10th, 15th, and 20th wage percentiles juxtaposed with changes in the average bite (across states) of the minimum wage. Figure 2a shows observed log wage gaps between men and women at the bottom of the distribution. These gaps have typically been declining since 1979, except for at the 5th percentile, where little change is seen.[21] Race and ethnic wage gaps demonstrate an opposite pattern. Figure 2b shows slight increases in observed lower-tail log wage gaps between Blacks and Whites across all wage percentiles. Figure 2c shows observed lower-tail log wage gaps between Whites and Hispanics have generally increased since 1979. However, their most salient feature is an inverted U-shaped pattern, with increases during the 1980s and 1990s followed by plateaus over the 2000s and declines over the 2010s (although not back down to 1980s levels). Various factors contributing to these trends, such as human capital convergence

[20] Online Appendix Figure B1c shows the evolution of binding percentiles of the minimum wage.

[21] In the CPS MORG, the gender wage gap slightly increases at the 5th and 10th percentiles during the 1980s. This stands in contrast to the convergence at the 10th percentile found by Blau and Kahn (1997), who studied the period from 1979 to 1988 using the March CPS and Panel Study of Income Dynamics (PSID). This difference between the CPS MORG and March CPS is quite 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. We also explored whether this difference could be accounted for by the fact that the CPS MORG contains reported hourly wages, while, in the March CPS and PSID, an hourly wage can only be inferred from weekly earnings divided by reported hours or from annual earnings divided by hours multiplied by weeks. However, the slight increase in the gender wage gap at the 5th and 10th percentiles over the 1980s persists in the CPS MORG even when using a similarly constructed hourly wage.

(or the slowing thereof), skill-biased technical change, and changing patterns of selection into the labor force (e.g., Bayer and Charles 2018; Blau and Kahn 2017; Blau, Kahn, Boboshko, and Comey forthcoming) have been considered in other work, and direct analysis of these factors lies outside the scope of this paper. We focus on the contribution of minimum wage policy, a critical labor market institution that is under policymakers' immediate control. To that end, we overlay the path of the average minimum wage bite in Figures 2a-c, which provides some suggestive evidence, particularly for minorities, that wage gaps at the bottom of the distribution have moved in opposition to changes in the minimum wage (i.e., between-group inequality tends to rise as minimum wages fall).

The most important reason that the minimum wage can be expected to reduce inequality between demographic groups, especially at the bottom of the wage distribution, is that direct exposure to the minimum wage varies between groups. 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 Whites face less direct exposure to minimum wage policies than women, Blacks, and Hispanics. Race, ethnic, and gender gaps in this measure of exposure tended to be larger during the 1980s and have since become much more similar—with the main exception of the Hispanic-White exposure gap, reflecting their particularly low wages as well as their location in high minimum wage states.[22]

IV. Methods

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

To estimate the effect of minimum wage policies on between-group inequality, we employ two analysis frameworks and three different regression approaches, each with advantages and disadvantages. In our first analysis framework, we directly estimate the effect of minimum wage policies on between-group inequality by putting various inequality measures on the left-hand side of our regressions. In the second analysis framework, we use coefficients from analogues of our regression approaches to conduct a microsimulation analysis of minimum wage policies. We detail the regression approaches below but defer details of the microsimulation approach to Section VI.

A. AMS/Lee Approach

The first regression approach we employ leverages minimum wage variation in a two-way fixed effects (TWFE) regression framework. This is the approach taken by Autor, Manning, and Smith (2012) and Lee (1999) in the context of aggregate and within-group (male, female) inequality. We extend these papers by applying their methods to estimate the effect of minimum wage policies on between-group inequality. The advantage of this approach is that 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. All regression variables are defined at the state-year level, and state and year fixed effects, as well as state-specific linear trends, are included to account for level differences in inequality within states and trends over time. The minimum wage bite enters non-linearly as a quadratic term. In the first portion of our analysis, we adapt the AMS/Lee method by using between-group inequality measures as outcome variables in the regression; we explain this analysis below. In the second portion of our analysis, we use various points along the pooled wage distribution as outcome variables and use the resulting coefficients to conduct counterfactual simulations using the CPS MORG microdata.

Let $y_{st}(p)$ represent some function 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:

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

In equation (1), as in AMS, the bite is included with a quadratic term.[23] Our main specification includes state and year fixed effects ($\sigma_s(p)$ and $\gamma_t(p)$, respectively) as well as state-specific linear trends ($\sigma_s(p) \times t$).[24] 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 wage distribution percentile p (e.g., the 5th percentile). Later, when using the approach to generate coefficients for

[23] In their analysis of pooled and within-gender wage inequality, AMS argue that the quadratic term in the bite is necessary to capture the idea that the same real minimum wage will have a greater effect on the shape of a state's wage distribution when it binds at a higher quantile of that distribution relative to the median. The logic is that a 1 log point rise in the real minimum wage will "sweep up" a greater fraction of individuals when it cuts at, for example, the 40th percentile rather than the 5th percentile of a state's wage distribution (see footnote 6 of AMS). This is likely to be the case since the wage distribution is likely to be thicker near the median than at the lower tail of the distribution. This logic works best for the lower half of the distribution since the density likely falls after a peak near the middle.

[24] 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.

our counterfactual simulations, we set $y_{st}(p)$ to be $w_{st}(p) - w_{st}(50)$, the log wage percentile p minus the median wage of the pooled wage distribution.

In the original AMS specification, the state-year median wage is always included on both the left- and right-hand sides of equation (1), because the wage inequality measure and the minimum wage bite are defined using a state-year's median log wage. This introduces an upward bias in the magnitude of estimated marginal effects, which can be purged via TSLS with a set of instrumental variables. For our primary between-group regressions, this IV procedure is less necessary, since the state-year median wage is no longer on both the left- and right-hand sides of the regression; hence, there is no purely "mechanical" bias in our main specifications. However, we still prefer TSLS estimates due to concerns that there may be bias arising from the endogeneity of labor demand or other factors that could affect both the state-year median wage and the relative positions of different groups at lower segments of the wage distribution.[25] Therefore, we follow AMS by instrumenting 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 with the weighted mean of the median log wage for each state over the entire sample period.[26] As in AMS, we report marginal effects for each percentile p at the weighted mean of the bite.[27] We cluster our standard errors at the state level.

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[25] 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 primary measure of the minimum wage in each state-year cell is the mode of effective monthly minimums. In addition, there can be errors in the median wage, which is part of the bite variable. We note, however, that since the instruments also are likely to have errors, measurement error may be present even in the TSLS specification.

[26] Contrary to the discussion in FLL (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.

[27] We report the weighted mean over all states and years of $\beta'_{st}(p) \equiv \beta_1(p) + 2\beta_2(p)[w_{st}^{mw} - w_{st}(50)]$.

In our main approach, we estimate equation (1) using two-stage least squares (TSLS) regressions in levels. For comparison, we provide results from OLS estimation and regressions estimated in first differences in the appendix. We emphasize the TSLS results for the reasons indicated above and focus on the results for levels, rather than first differences, in part because this is standard in the literature.[28] Results are broadly consistent across these estimates.

B. Stacked Difference-in-Differences (SDD)

Our next strategy is to estimate minimum wage effects using difference-in-differences regressions that leverage discrete minimum wage policy changes. 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 nest our analysis in modern difference-in-differences frameworks that overcome biases that may be present in TWFE models with staggered policy adoption (e.g., Goodman-Bacon 2019; De Chaisemartin and d'Haultfoeuille 2020; Baker et al. 2021). 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.

[28] In addition, as Wooldridge (2010) points out, there are some reasons to prefer fixed effects (FE) to first differences (FD). Specifically, when strict exogeneity fails but contemporaneous exogeneity holds, FE and FD will both be inconsistent, but the inconsistency bias of FE is proportional to 1/T, while the FD inconsistency is unrelated to T (see, pp. 321-323).

Like any difference-in-difference research design, this framework requires a definition of what constitutes a minimum wage increase "event." As a baseline, 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.[29] However, since we wish to trace out minimum wage effects for up to 5 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 may increase in intensity over time. We use a straightforward rule to eliminate such successive increases: we do not consider any events that occur less than 5 years after some other event.[30] This sort of rule-based selection of "uncontaminated" events is in line with the growing literature on event study designs (Miller 2022). We present results that scale effects by the magnitude of the minimum wage change averaged over the "post-treatment" period. This helps to account for any subsequent events that occur within 5 years of a focal event.

We attach each event to a set of "clean control" states, which are chosen to exclude states that experience a minimum wage increase event within the event window of the focal event. Such states are "never-treated" units within the event window but may experience a minimum wage increase event further away from the focal event. Finally, because we require each event to have a non-empty set of clean controls, we drop minimum wage increase events that occur

[29] 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 cent nominal changes (in 2016 dollars).
[30] For example, if a state has non-trivial 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 study only the 1992 event as a single minimum wage event.

simultaneously with increases in the federal minimum. Our selection rule ultimately results in a set of 69 minimum wage increase events occurring in 33 unique states. On average, we attach each event to about 30 clean control states (20 at minimum, 44 at maximum). Online Appendix Figure B3 displays the set of events that we analyze.

To implement the SDD design, we associate each minimum wage increase event with its own "dataset" that spans an event window (in our main specification, 3 years before and 5 years after the event) and includes the state in which the event occurred as well as any clean control states. We "stack" these datasets and estimate the following equation:

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

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 a state s in calendar year t was treated k periods ago. Formally, 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 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.[31] We cluster standard errors at the state-by-dataset level.[32]

[31] Including these trends is consistent with our baseline TWFE specification. However, their omission may be appropriate in the context of an event study, as researchers may not wish to identify effects "off of" parametric trends in wage inequality imposed throughout short event windows (e.g., Meer and West 2016). Our results at the bottom of the wage distribution are not sensitive to removing these trends, although a couple of upper percentile results (above the median) for Hispanic-White inequality become spuriously positive and significant. Because we see the absence of effects in these upper percentiles as a "specification test," we view this as a mark in favor of including the trends in our event studies. Note that the same upper percentile specification test is what motivates including these trends in AMS's original panel specification.

[32] This clustering is consistent with Cengiz et al. (2019) as in their Table D.1.

Our primary estimates come from 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 $k \geq 0$. This SDD equation has an advantage with respect to statistical power compared to estimating separate coefficients for each event-time k. Straightforward estimates of $\beta(p)$ are not easily interpretable, however, due to variation in the size of minimum wage reforms. Therefore, along with these basic estimates, we estimate two additional equations that scale the DD coefficients to make them comparable to estimates from the AMS-style regressions. The first method scales $\beta(p)$ by the average "first-stage" estimate (across all "datasets") of the effect of treatment on the bite. The second method scales the treatment indicators separately by dataset using dataset-specific first-stage estimates, and then estimates the resultant $\beta(p)$ using these scaled treatment indicators. To estimate the first-stage effects, we replace the left-hand side of equation (2) with the minimum wage bite ($w_{st}^{mw} - w_{st}(50)$), and then estimate the "first stage" either simultaneously (extracting a single $\beta^{FS}(p)$) or separately by dataset (extracting every $\beta_d^{FS}(p)$). The scaled OLS estimate is produced simply by computing $\beta^{Scaled}(p) = \beta(p)/\beta^{FS}(p)$. The dosage OLS estimate is produced by replacing the key event study indicators in equation (2) with $D^*_{d,s,t+k} = \beta_d^{FS}(p) \times D_{d,s,t+k}$ and re-estimating the equation to yield new coefficients that we call $\beta^{Dosage}(p)$. These scaled coefficients can be interpreted as the effect of a standardized 1 log point change in the bite of the minimum wage that could result, for example, from a state-level policy change.

We also estimate the full event study specification in equation (2) that includes separate coefficients for each event-time k. To do so, we impose the standard normalization that

$\beta_{-1}(p) = 0$. This full specification allows us to trace out minimum wage treatment effects over time within brief event windows.[33]

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 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 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 count as non-trivial discrete events. The smaller sample leads to noisier estimates, especially in the coefficient-by-coefficient dynamic event studies.

C. FLL Approach

[33] The full event study specification also requires the coefficients to be scaled so that they can be compared to the AMS-style results, and we conduct this scaling in the same way as in the SDD specification.

In addition to the previous two reduced form methods, we also evaluate the effects of minimum wages on inequality between groups using a distribution regression framework proposed by Fortin, Lemieux, and Lloyd (2021). As above, the goal is to analyze the impact of the minimum wage on between-group inequality throughout the entire wage distribution. However, this regression approach does not yield a direct estimate of the effect of minimum wages on between-group inequality. Instead, we use it as an input to our counterfactual simulations to infer the model's implications for between-group inequality.

In their original paper, FLL introduce a stacked probit specification that relates the probability that an individual's wage Y_i is located above a given wage cutoff y_k to that individual's characteristics and the distance between his/her wage and the effective minimum wage. The specification uses a fixed grid of K wage cut-offs so that, formally, the stacked equations are given by:

$$Pr(Y_i \geq y_k) = \Phi(X_i \beta_k) \text{ for } k = 1, 2, \dots, K.$$

Following FLL (2021), we implement this by jointly estimating the parametric stacked probit model:

$$Pr(Y_{ist} \geq y_k) = \Phi\left(Z_{istk}\beta + \sum_{m=-3}^{4} D_{kst}^{m}\,\phi_m - c_k\right) \tag{3}$$

where the dependent variable is an indicator that is equal to one if an individual i in state s and year t reports a wage above the cutoff y_k. The specification in equation (3) requires us to define a grid of wage cut-offs to form the wage bins in which individuals may be located. We use 57 wage cut-offs running from 1.6 to 4.4 log points (in 2020 dollars, these correspond to approximately \$5 and \$81). These cut-offs define 58 log wage bins that are 0.05 log points wide, except for the first and final bins, which are open-ended at the bottom and top ends. The c_k are wage cutoff fixed effects.[34]

This specification non-parametrically estimates the effects of the minimum wage on individuals' locations in the wage distribution using a set of dummy variables, D_{kst}^{m}, where $D_{kst}^{m} = \mathbf{1}[y_{k-m} \leq MW_{st}], m \in [-3,4]$. For example, D_{kst}^{0} $(for\ m = 0)$ turns on for wage bins at or below the minimum wage. Indicators above the minimum wage allow the model to account non-parametrically for spillover effects. Indicators below the minimum wage allow for non-parametric effects for non-compliance. Following FLL (2021), we use 3 bins below and 4 bins above an individual's observed position in the wage distribution (each of width 0.05 log points).

The model includes a rich set of demographic covariates in Z_{istk}:

$$Z_{itsk}\beta = X_{itsk}\beta_X + \theta_s + \omega_t + t \times \theta_s + y_k \times L_{its}$$

The controls in X_{itsk} include years of schooling, a quartic in potential experience, experience-by-education group dummies (16 discrete categories of combined education/experience levels), a continuous interaction between years of potential experience and years of schooling, 11 industry categories, 12 occupation categories, and marital status.[35] In our primary specification, we pool all groups together and include dummy variables for gender and mutually exclusive race/ethnic categories.. Furthermore, following FLL (2021), we control for wage heaping at nominal whole-dollar wages by including indicator variables for wages below five and ten dollars, as well as a continuous measure of nominal wage integer values between zero and ten.[36] In addition, θ_s are

[34] This wage grid closely resembles that in FLL (2021).
[35] Construction of these control variables closely follows that of FLL (2021). We found that our results were not typically sensitive to the definition or inclusion of additional controls.
[36] The indicator for wages below five dollars is only included in 1979-1989, while the indicator for below ten dollars is only included in 1989-1998, 1998-2007, and 2007-2019.

state fixed effects, ω_t are year fixed effects, and $t \times \theta_s$ are state-specific linear time trends. Finally, we interact the state and year fixed effects and the education-experience categories with the wage cutoffs, as indicated by the inclusion of $y_k \times L_{its}$.[37]

V. Direct Effects of Minimum Wage Policies on Between-Group Inequality

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

A. Two-Way Fixed Effects and Stacked Difference-in-Difference Results

Figures 3a-c summarize graphically the key results from our main TWFE and SDD specifications. Our main TWFE coefficients are those estimated by TSLS in levels, and our main SDD coefficients are the "dosage" estimates described in Section IV. The dosage SDD estimator is attractive because the coefficient estimate of interest is naturally weighted by the size of the minimum wage increases from each individual event. Our estimates based on both methods show that the minimum wage substantially reduces between-group inequality at the bottom of the wage distribution. The coefficient estimates plotted 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 of the wage distribution. The reduced-form point estimates that underlie these figures are displayed in Table 1. The results from alternative specifications for each approach are shown in Online Appendix Tables A2 and A3, respectively. The alternative specifications include TSLS

[37] L_{its} comprises the state and year fixed effects and education-experience categories.

in first differences,[38] OLS in levels and first differences, and scaled SDD estimates. Results are broadly consistent across these specifications.

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-c show results for race/ethnic wage inequality. With one exception, for both Black-White and Hispanic-White wage inequality, there are significant negative effects at the 5th, 10th, 15th, and 20th percentiles. The exception is Hispanic-White wage inequality at the 5th percentile, in which the point estimate is strongly negative but imprecisely estimated. Notably, there are small, statistically significant negative effects at the mean for Black-White wage inequality in both the TWFE and SDD specifications, while, for Hispanic-White wage inequality, the mean effect is only significantly negative in the TWFE specification.

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 in real terms from 1979 to 1989. At the 5th percentile, this implies a 0.036 log point increase in the gender wage gap, a 0.040 log point increase in the Black-White wage gap, and a 0.087 log point increase in the Hispanic-White wage gap based on our TWFE estimates; and a 0.068 log point increase in the gender wage gap, a 0.059 log point increase in the Black-White wage gap, and a 0.053 log point increase in the Hispanic-White wage gap based on our SDD estimates. These magnitudes are substantial relative to the observed trends at the 5th percentile from 1979 to 1989: a 0.01 log

[38] 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.

point increase in the gender wage gap, a 0.04 log point increase in the Black-White wage gap, and a 0.05 log point increase in the Hispanic-White wage gap. In other words, our estimates imply that minimum wage policy choices are more than sufficient to account for all of the increases observed over the 1980s in all three of our measures of 5th percentile between-group inequality.

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. Our results confirm these expectations.

We performed several robustness checks on our main TWFE specification, which is estimated by TSLS in levels. Across methods, our results are robust to excluding individuals working in tipped occupations; broadening our age restriction to 16-74 (rather than 18-64 in our baseline specification); 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); including a rich set of state-year time-varying controls (including the unemployment rate as well as measures of the demographic and educational composition of the labor force); and to working with unbalanced state-year panels (rather than balanced panels, so as to allow more state-year cells for our Black and Hispanic analyses). 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.

B. 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 effects of changes in the minimum wage bite on between-group wage inequality.[39] We discuss these results here, but we defer associated figures to the Online Appendix. Online Appendix Figure B5 plots the "first stage" relationship between the minimum wage increase events and the minimum wage bite. The plotted coefficients can be interpreted as the average size and time path of the set of "treatment" events that we analyze in our event studies and in the stacked DD specification.[40] Overall, the plot shows a strong first stage relationship and does not exhibit any pre-trend.

Online Appendix Figures B6a-c plot event study coefficients for the effect of the minimum wage on inequality between men and women, Whites and Blacks, and Whites and Hispanics at several wage percentiles and at the mean. Considering all estimates together, the assumption of parallel pre-trends (in log wage percentiles) largely holds up to scrutiny across the wage distribution. There is some evidence that gender inequality may have been trending upward at the 10th percentile in reforming states relative to control states, 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, we find two significant pre-treatment coefficients at the 10th percentile two periods before treatment in the cases of Black-White and Hispanic-White

[39] The parallel trends assumption in our setting is that, in the counterfactual absence of treatment, log wage percentiles would have proceeded in parallel across treatment and control states. This entails a functional form assumption (that the trend is in percentiles), but our empirical results suggest that such an assumption is not problematic (Roth and Sant'Anna 2023).
[40] The red lines overlaid on this graph and the rest of our event study plots indicate the SDD specification's point estimate (single solid red line) and 95 percent confidence interval (two dashed red lines).

inequality, and one which appears two periods before treatment at the 5th percentile for Hispanic-White inequality. Here, there is suggestive evidence that we may be overestimating convergence at the 10th percentile, but we remain largely confident in our findings at the 5th and 20th percentiles. 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).

Up to this point, we have discussed estimates from specifications that place direct measures of between-group inequality on the left-hand side of our estimating equations. Another way to infer that the minimum wage had effects on between-group inequality is to estimate within-group equations and observe that effects are larger for disadvantaged groups. While such specifications are not our main focus, we show results from these within-group equations in Online Appendix Tables A5 (for TWFE) and A6 (for SDD). Additionally, within-group event studies are shown in Online Appendix Figures B7a-c. Across the board, the point estimates in these tables and figures show that minimum wage effects are larger for these lower wage groups (women, Hispanics, and Blacks) than for Whites and men, thus lending strong plausibility to our main results.

VI. 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 a modified version of our TWFE and SDD specifications, as well as the FLL approach. We conduct these simulations for two key reasons. First, these simulations explicitly address the question of how impactful the US's historical suite of federal/state minimum wage policies have been in reducing national-level inequality between groups over the past four decades. Second, they allow us to compare magnitudes of the results across all three of our regression approaches, which differ in subtle ways (e.g., incorporating different sources of variation, different periods, discrete versus continuous definitions of treatment, and different functional forms). These exercises answer the following question over each period: How much higher (or lower) would between-group inequality have been in the end year if the US state/federal minimum wages had been fixed at their levels in the start year? We answer these questions using four intervals of time—1979 to 1989, 1989 to 1998, 1998 to 2007, and 2007 to 2019—to study between-group inequality throughout the wage distribution. These intervals are of particular interest because, as noted earlier, each identifies a distinct period in the evolution of the mean effective minimum wage.

An additional advantage of these historical simulations is that the minimum wage variation exploited is always firmly within-sample. It is challenging to simulate the effects of a minimum wage that is completely or predominantly outside the observed sample range. For example, while it would be interesting to estimate the effects of a very high federal minimum wage (such as the popularly proposed $15 minimum wage) on wage inequality, high real minimum wages have rarely been observed in the United States, and for the most part only very recently and in a small minority of states. This problem is exacerbated when considering variation in the bite of the minimum wage as the primary causal determinant of inequality (i.e., comparing the locally prevailing minimum to state median wages) because high minimum wages are typically located in high wage states.

A. Methodological Approach

To assess the effects of minimum wages on changes in demographic wage differentials by percentile, we consider two groups g and g', a percentile p, and an historic 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 policies) prevailed in year t_1.

For the reduced form methods, we broadly adopt the simulation approach devised by Lee (1999). This approach increments individual wage observations by assigning them to integer wage percentiles within state-year cells and then adjusting their wages using percentile-specific regression coefficients from the estimated reduced-form equations (in our case, either the TWFE or SDD equations). This procedure requires us to return to variants of the original AMS specification, in which the dependent variable in equation (1) is replaced with $w_{st}(p) - w_{st}(50)$. This is necessary because, although our estimates from the previous section reflect the "direct" effects of the minimum wage bite on between-group inequality, there is no natural way to use such direct estimates to increment individual wage observations and obtain counterfactual wage distributions.[41] We use the estimated coefficients from the pooled equation and the difference between the year t_0 and t_1 values of the minimum wage bite to increment wages for both groups, g and g', and obtain a simulated between-group pay gap at percentile p in year t_1.

[41] Lee (1999) also uses a pooled regression to compute counterfactual wages.

The simulated change in the gap at percentile p is the simulated gap in t_1 minus the actual gap in year t_0, and the simulated change minus the actual change then provides the effect of the minimum wage on the gender pay gap at this percentile. We use a completely analogous approach for the SDD specification.

A potential alternative would be to estimate within-group versions of the regression and use coefficients from those equations to increment individuals' wages on a group-specific basis. We do not employ this alternative mainly because the Lee (1999) method's initial step is to assign individuals to integer wage percentiles within their state-year wage distributions. When cell sizes drop below 50 observations, however, as is common with Blacks and Hispanics in the CPS MORG, this assignment procedure leaves many "missing percentiles." Thus, while this group-specific alternative would be a worthwhile exercise, it requires a larger dataset than the CPS MORG. Note that the larger cell sizes in the pooled approach allow us to include all state-years in the simulation.

For the FLL regression approach, we estimate regression models separately for our four key historical intervals (1979-1989, 1989-1998, 1998-2007, 2007-2019).[42] The choice to estimate the FLL models separately by period contrasts with our approach in the TWFE and SDD regressions, which are estimated once on all wage data from 1979 to 2019. There are two main reasons for this difference. The first reason is that the TWFE and SDD regressions work with many fewer observations by design because they are estimated on state-year panels. Splitting each TWFE or SDD regression into four sub-periods of time would lead to prohibitive reductions in sample size. The FLL regressions are estimated on samples of individual wage

[42] The probit estimates are displayed in Online Appendix Table A7. Our results are qualitatively similar to those presented in FLL (2021), although our coefficients tend to be slightly smaller than theirs. Standard errors are clustered at the state level, which accounts for autocorrelation across time and correlation across wage bins.

observations, and the CPS MORG is large enough that dividing the original 1979-2019 sample into period-by-group sub-samples does not pose a problem. The second reason is that, in the FLL models, the wage bins are fixed (in 2020 log dollars), so the meaning of a location in a specific wage bin is likely to change over time as the overall wage distribution shifts. There is no such concern in the TWFE and SDD analyses, which are focused on wage gaps at specific percentiles of the wage distribution. To estimate counterfactuals using the FLL-style regressions, we obtain simulated changes in between-group wage gaps by using the fitted probit equations to reweight observed wage distributions into counterfactual wage distributions at the end points of each of the epochs.

For more details on each simulation method, see the Appendix: Methods for Simulating the Effects of Minimum Wage Policies Since 1979. In this Appendix, we also discuss how we compute bootstrapped standard errors for each method's counterfactuals.

B. Simulation Results from the Pooled TWFE Specification

Figure 4 presents the pooled TWFE results from our counterfactual simulations for each pair of demographic groups and for each historical interval. (Online Appendix Figures B8 and B9 show analogues of Figure 4 for smoothed counterfactual microsimulations using the SDD and FLL methods.) Each subfigure corresponds to a pair of groups and one of the four historical periods. Each one displays the predicted change in between-group wage inequality throughout the wage distribution supposing that the federal/state minimum wage structure of the first year in each period continued to prevail through the last year in each period. 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 around these smoothed moving averages using state-clustered bootstraps (discussed in the methods Appendix). The blue shaded regions represent 95% confidence intervals. Point estimates of the moving averages and their standard errors at selected percentiles are also displayed in Online Appendix Table A8.[43]

Simulations based on the pooled TWFE estimates indicate that the decline in the value of the minimum wage from 1979 to 1989 worked to substantially increase gender, race, and ethnic inequality at the bottom of the wage distribution. For example, had 1979's minimum wage structure remained in 1989, the wage gap between women and men at the 10th percentile would have been 0.05 log points smaller; the wage gap between Blacks and Whites 0.06 log points smaller; and the wage gap between Hispanics and Whites 0.02 log points smaller. Statistically significant effects on inequality by gender, race, and Hispanic ethnicity during this period are present up to approximately the median.

Although such point estimates might appear small, they are non-negligible when contextualized against both the level of lower-tail wage inequality between groups as well as the actual changes in inequality observed over these periods. Between-group wage gaps below the 15th percentile are almost always below 0.20 log points, and typically around 0.10 log points.[44] This limits the size of the minimum wage effect on inequality between groups in the lower tail. Observed trends are also relevant. For example, in the CPS MORG, gender inequality increased from 1979 to 1989 by about 0.06 log points at the 5th and 10th percentiles. Our results show that keeping minimum wage policies at their 1979 levels would have almost entirely eliminated this increase in gender inequality at the bottom. Our results for Black-White inequality are of a similar magnitude, increasing from 1979 to 1989 by 0.06 and 0.11 log points at the 5th and 10th

[43] Online Appendix Table A8 also displays counterfactual estimates from our other methods, SDD and FLL.
[44] See Figures 2a-c, discussed above, for time series plots of these measures.

percentiles. These increases would have been only 0.01 and 0.05 log points, respectively, had minimum wage policies remained at their 1979 levels. 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. The results in Figure 4 indicate that this increase disproportionately benefited low wage groups, working to reduce gender, race, and ethnicity gaps at the lower end of the wage distribution. For example, at the 10th percentile, had the 1989 minimum wage structure prevailed in 1998, gender wage inequality would have been 0.02 log points higher; Black-White and Hispanic-White inequality would have been 0.02 and 0.03 log points higher, respectively. All these estimates are statistically significant at 95% confidence. There was little change in the mean effective minimum wage from 1998 to 2007 and shifts in minimum wage policies had virtually no discernible effects on between-group inequality for any of our three pairs of demographic groups. After 2007, there was little change in the mean effective minimum wage at the national level and little evidence of a minimum wage impact on gender or race inequality. Notably, however, there were statistically significant and economically important effects for Hispanic-White inequality at the bottom of the distribution. These effects on Hispanic-White inequality are non-negligible in magnitude (particularly from the 10th to 25th percentiles, peaking at an effect of 0.05 log points at the 18th percentile) and retain statistical significance up to about the median. These unique results for Hispanic-White inequality since 2007 reflect both Hispanics' continued disadvantaged position in the wage distribution (see Online Appendix Figure B2) as well as Hispanics' geographic concentration in states that legislated especially large minimum wage increases over this period. We highlight this latter fact below in Section VII.

We emphasize that these simulations reflect the historical effects of the *actually existing* suite of US federal/state minimum wage policies on our measures of between-group inequality. That we find smaller simulated effects during a particular period, for example, does not imply (necessarily) that the effect size of the minimum wage was smaller during that period.[45] Rather, for each period and for each type of between-group inequality, the results of our historical simulations reflect a combination of (*i*) the overall magnitude of minimum wage changes combined with (ii) the relative positions of each group in the wage distribution as well as (*iii*) the relative intensity of minimum wage policy changes experienced by both groups due to differences in geographic location. While the influence of the "positional" factor is obvious enough, we will highlight the "spatial" factor below in Section VII.

C. Consistency of Counterfactual Estimates across Methods

Figure 5 plots the counterfactual estimates from all of our three regression methods (see also Online Appendix Table A8)**.** To aid legibility, we do not show confidence intervals (these can be found in Online Appendix Figures B8 and B9). Our counterfactual estimates almost always have the correct sign: the counterfactual changes are more negative than the actual changes during 1979-1989 (when returning to the 1979 minimum wage structure would lead to a higher bite) and more positive than the actual changes during later periods (when returning to the

[45] Only the results from our FLL method could support claims of this sort (as its underlying coefficients are estimated separately by period).

initial minimum wage structure would lead to a lower bite).[46] The figure also shows that our three methods produce results with broadly consistent magnitudes. However, there is some notable variation across methods. Specifically, the FLL counterfactuals tend to produce the largest estimates, in particular from 1979 to 1989, while the pooled TWFE and pooled SDD counterfactuals tend to be closer together. However, the FLL counterfactuals are the most imprecisely estimated. Additionally, the pooled SDD estimates tend to be slightly smaller than the pooled TWFE estimates.

VII. The Disparate Impacts of Geographic Concentration

Next, we highlight 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 when considered nationally.[47] Table 2 shows the average bite of the minimum wage in the bookend years from each of our four periods between 1979 and 2019, with standard deviations in parentheses. These average bites are computed with group-specific sample weights, so each row of the table displays the bites experienced by each group in a given year. Unsurprisingly, there is no observable gender gap in the experienced bite since men and women are not differentially distributed across states, although (especially) female labor force participation rates may vary. In contrast, Whites, Blacks, and Hispanics do exhibit two periods of noteworthy differences. In 1979, Blacks were on average subject to a higher (less negative) minimum wage bite. Since the prevailing minimum wage in this period tended to be the federal

[46] There are occasional exceptions at the very bottom of the wage distribution. For example, the FLL method produces a wrong-signed estimate for the 1979-1989 period for Hispanic-White inequality at the very bottom of the wage distribution, at the 5th percentile.
[47] Online Appendix Figure B10 displays maps of racial and ethnic geographic concentration across US states. Online Appendix Figure B11 provides Duncan segregation indexes for measures of Black-White, Hispanic-White, and Black-Hispanic state-level segregation.

minimum, this reflects higher Black concentration in poorer southern states, which had lower medians, leading to higher bites. The other major group difference was for Hispanics in 2019, for which the minimum wage bite was much higher (less negative) than for the other two groups. In this period, there was considerable variation in state minimum wages, and Hispanics were more highly concentrated in states that passed relatively high minimum wages. These snapshot differences in the average levels of bites experienced by racial and ethnic groups have consequences for our simulations, which exploit historical changes in minimum wage policies to counterfactually shift the bite of the minimum wage. Group differences in bites imply that, in the counterfactual in which the 1979 minimum wage structure is imposed on observed data in 1989, Blacks would experience the largest average increase in the bite, whereas imposing 2007 minimum wages in 2019 would lead to a larger average decrease in the bite for Hispanics.

We use a series of reweighting exercises to illustrate how our findings are affected by these facts. In each period that we simulate, we adjust the sample weights for Hispanics and Blacks so that their geographic distribution across states during that interval resembles that of Whites. Then, using this reweighted data, we re-estimate the implied effect of changes in minimum wage policies on between-group inequality at each percentile. We take the difference between the original effect and the effect estimated on reweighted data to reflect the influence of differences in spatial distribution on the minimum wage's impact on racial/ethnic inequality, while we take the reweighted effect to reflect purely positional factors (i.e., the fact that Hispanics and Blacks tend to have lower wages than Whites within a state, and thus are more affected by the same change in the minimum wage).

Table 3 displays these effects using our pooled TWFE counterfactuals. This table shows these reweighting results for two key salient periods—Black-White inequality for 1979-1989 and

Hispanic-White inequality for 2007-2019. Figure 6 shows smoothed estimates at all percentiles for these two salient cases.[48] It is evident that geography played a role in each case. Had Blacks lived in the same places as Whites from 1979 to 1989, this period's fall in the federal minimum wage would have increased national Black-White inequality by slightly less at wage percentiles below the median, and especially below about the 25th percentile. Had Hispanics lived in the same places as Whites from 2007 to 2019, this period's increases in federal and state minimum wages would have reduced Hispanic-White inequality by slightly less at wage percentiles below the median. The latter period is especially notable because most minimum wage increases at this time were driven by state-level policies: Hispanics lived disproportionately in states that legislated substantial raises in their minimum wages and disproportionately reaped the resulting wage benefits.

VIII. Counterfactual Effects of a $12 Federal Minimum Wage

In considering the effect of the minimum wage, it is interesting to consider what the effect of a substantially higher minimum wage on inequality across groups would be. For example, a $15 minimum wage has been proposed by some members of Congress. However, it is difficult to predict the effects of a $15 minimum wage because such a high federal minimum would lead to bites at the state-level that would be far out of historical sample.[49] In this section,

[48] Online Appendix Figures B12a-d show the smoothed estimates at all percentiles for each type of race and ethnic inequality and for all four periods, not only those in which there were salient group differences in exposure to the bite of the minimum wage. Appendix Table A9 displays point estimates for these reweighting exercises as well as bootstrapped standard errors around the smoothed point estimates. As expected, in periods without important group differences in exposure, reweighted effects are very close to the original effects.

[49] Online Appendix Figure B13 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. We also display the distribution of bites observed in 1979, which represents a high point in the history of US minimum wage policies. This figure clearly demonstrates that a federal minimum wage above $12 would lead to minimum wage bites that would be far outside the existing variation in our 1979-2019 sample.

we make predictions about the effects of a hypothetical federal minimum wage of $12 (in 2020 dollars) on gender, racial, and ethnic inequality. Table 4 displays the predicted effects of this policy change on between-group wage inequality using coefficient estimates from our pooled TWFE specification applied to wage data from 2015 to 2019. See Online Appendix Table A10 for counterfactual predictions under a federal minimum of $12 using all three methods.

According to our point estimates, raising the federal minimum wage to $12 would have significant effects on between-group inequality at the bottom of the wage distribution for all three of our demographic comparisons. The implied decreases in inequality are economically substantial. Existing levels of between-group inequality during 2015-2019, which are also shown in the table, are useful for gauging the magnitude of these effects. For example, at the 5th percentile, during 2015-2019, there was a gender wage gap of about 0.09 log points. We predict that a $12 federal minimum wage would reduce gender inequality at this percentile by 0.05 log points. Log point reductions of similar magnitude are predicted up to about the 15th percentile in the case of gender inequality. Similarly, there were Black-White wage gaps of about 0.12 log points at the 5th percentile and 0.15 at the 10th percentile; these gaps would be reduced by 0.06 and 0.05 log points, respectively. We also predict that the existing Hispanic-White wage gaps of about 0.11 log points at the 5th percentile and 0.13 log points at the 10th percentile would fall by about 0.03 log points in each case. In other words, 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 up to a half of existing levels of inequality.[50] Finally, for all three demographic

[50] 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.

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.

IX. Conclusion

Taken together, our results show that the minimum wage binds more for low-wage groups such as women, Blacks, and Hispanics than it does for men and Whites. As a result, state and federal minimum wages can work to reduce inequality between these groups at the bottom of the wage distribution. Our results indicate that the large decline in the value of federal minimum wage over the 1980s retarded progress during that decade toward closing gender, race, and ethnic wage gaps at the bottom of the distribution. After the 1980s, over the period from 1989 to 2007, minimum wage policy changes were on average generally slight, and thus so was the minimum wage's impact on between-group inequality. On the other hand, since 2007, we find that Hispanics (relative to Whites) saw especially large, positive relative wage gains from state-level minimum wage changes. This is in part because Hispanics' earnings have distinctly continued to lag in wages, but also because Hispanics are disproportionately located in states that have elected to raise their minimum wages substantially. While minimum wage policies since 2007 have not had discernible effects on gender inequality and inequality between Blacks and Whites, our evidence suggests this is largely due to the nature of these recent policy changes—in particular, their limited size and geographic scope. Indeed, assuming the continued validity of our estimates of the minimum wage's effects, we show that raising the federal minimum wage to $12 would bring about economically meaningful reductions in lower-tail wage inequality by gender, race, and ethnicity. We should point out, however, that our results apply to employed workers, and the minimum wage could in principle have negative employment effects. The very large literature on these effects has not come to a consensus on whether the minimum wage reduces employment, but even those who find negative effects often report estimates that are modest in magnitude.[51] Thus, our results show that minimum wage policy is a potentially useful policy instrument in lessening lower-tail earnings inequality between demographic groups.

[51] For illustrative studies, see Allegretto, Dube, Reich, and Zipperer (2017) and Neumark, Salas, and Wascher (2014).

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Tables

Table 1: Reduced-Form 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	-0.12	0.40	-0.08	-0.03
		(0.02)	(0.02)		(0.03)	(0.04)		(0.03)	(0.05)
p5	0.09	-0.12	-0.18	0.05	-0.13	-0.19	0.12	-0.28	-0.17
		(0.03)	(0.05)		(0.05)	(0.06)		(0.04)	(0.13)
p10	0.13	-0.11	-0.03	0.09	-0.11	-0.19	0.21	-0.23	-0.21
		(0.02)	(0.05)		(0.05)	(0.07)		(0.04)	(0.07)
p15	0.16	-0.08	-0.03	0.13	-0.17	-0.18	0.27	-0.15	-0.12
		(0.02)	(0.03)		(0.04)	(0.08)		(0.04)	(0.05)
p20	0.18	-0.10	-0.09	0.16	-0.24	-0.19	0.32	-0.20	-0.19
		(0.03)	(0.04)		(0.04)	(0.07)		(0.04)	(0.06)
p30	0.21	0.00	-0.06	0.20	-0.03	-0.10	0.39	-0.09	-0.11
		(0.02)	(0.04)		(0.04)	(0.07)		(0.04)	(0.05)
p40	0.23	0.01	-0.03	0.22	-0.07	-0.17	0.43	-0.06	-0.05
		(0.03)	(0.04)		(0.04)	(0.06)		(0.02)	(0.06)
p50	0.25	0.02	-0.03	0.24	-0.05	-0.10	0.45	-0.03	-0.04
		(0.02)	(0.03)		(0.06)	(0.05)		(0.03)	(0.06)
p70	0.26	0.03	0.01	0.27	-0.02	-0.06	0.48	-0.01	0.10
		(0.02)	(0.03)		(0.05)	(0.06)		(0.04)	(0.06)
p90	0.30	0.02	0.02	0.30	-0.03	-0.02	0.49	-0.09	0.10
		(0.04)	(0.04)		(0.09)	(0.07)		(0.09)	(0.08)
MW Bite			1.10			1.05			1.10
			(0.05)			(0.06)			(0.07)
Events			69			69			69
Observations	2050	2050	18629	1066	1066	10740	451	451	4695

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 TSLS in levels and by the 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.

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

	Panel A. Black-White Inequality, 1979-1989		
	Baseline Effect (Total)	Reweighted Effect (Wage/Positional)	Remaining Effect (Geographic = Total – Positional)
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. We show only these two cases as they are the salient cases of group differences in the bite of the minimum wage. Standard errors around the smoothed baseline and reweighted counterfactuals (and their difference) are bootstrapped using 100 sets of pooled TWFE coefficients. Point estimates for all group gaps and all periods are shown in Appendix Table A9.

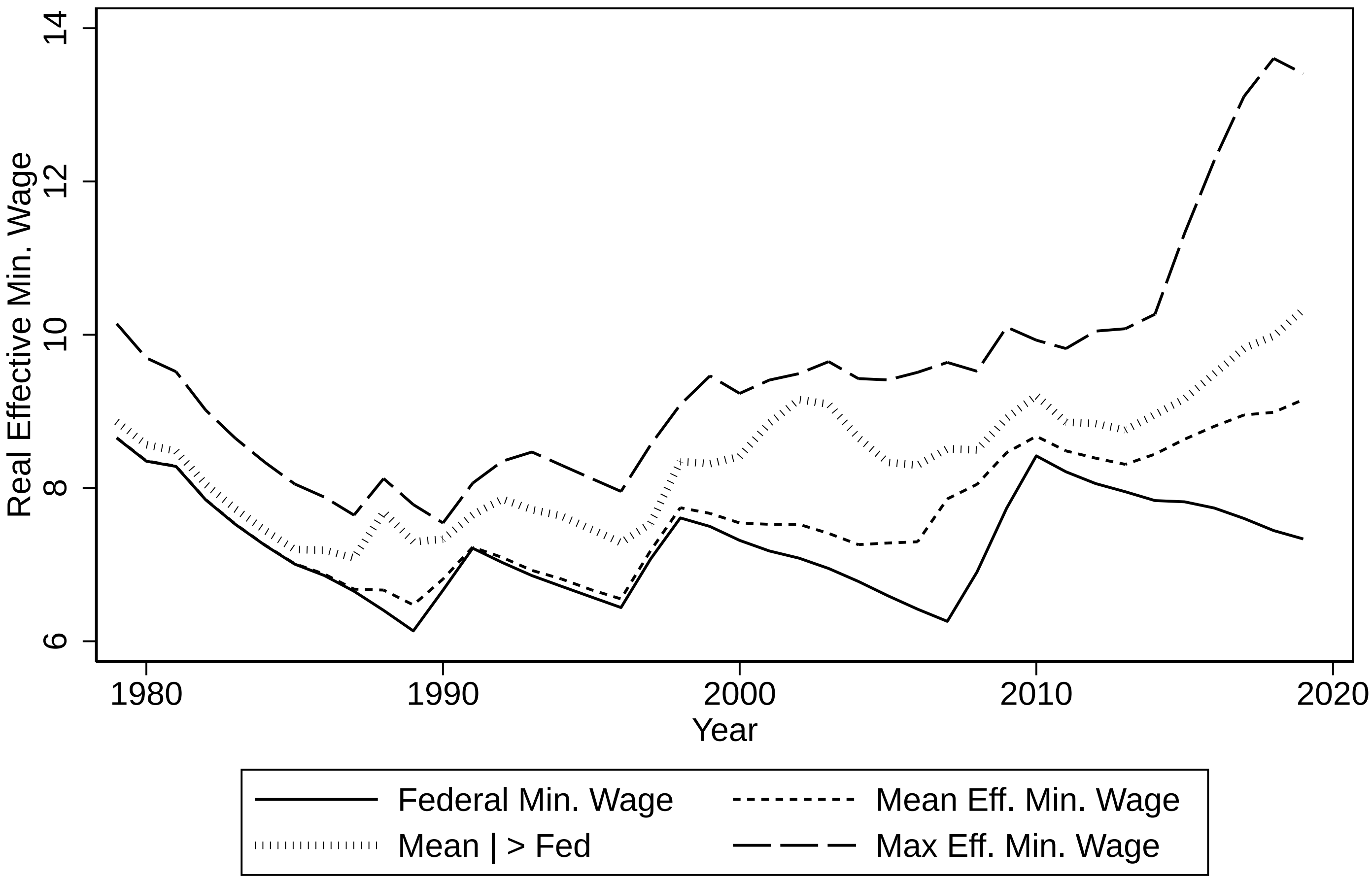
Table 4: Federal $12 Minimum Counterfactuals Using TWFE, 2015-2019

	Male-Female Inequality		Black-White Inequality		Hispanic-White Inequality	
	Raw Gap	Predicted Change	Raw Gap	Predicted Change	Raw Gap	Predicted Change
Mean	0.17	-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	0.07	-0.035	0.15	-0.052	0.13	-0.027
		(0.003)		(0.004)		(0.005)
p15	0.12	-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	0.25	-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	0.27	-0.023	0.36	-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 using incremented data from 2015 to 2019. Standard errors computed by bootstrapping smoothed moving averages of the counterfactuals at each percentile are shown in parentheses.

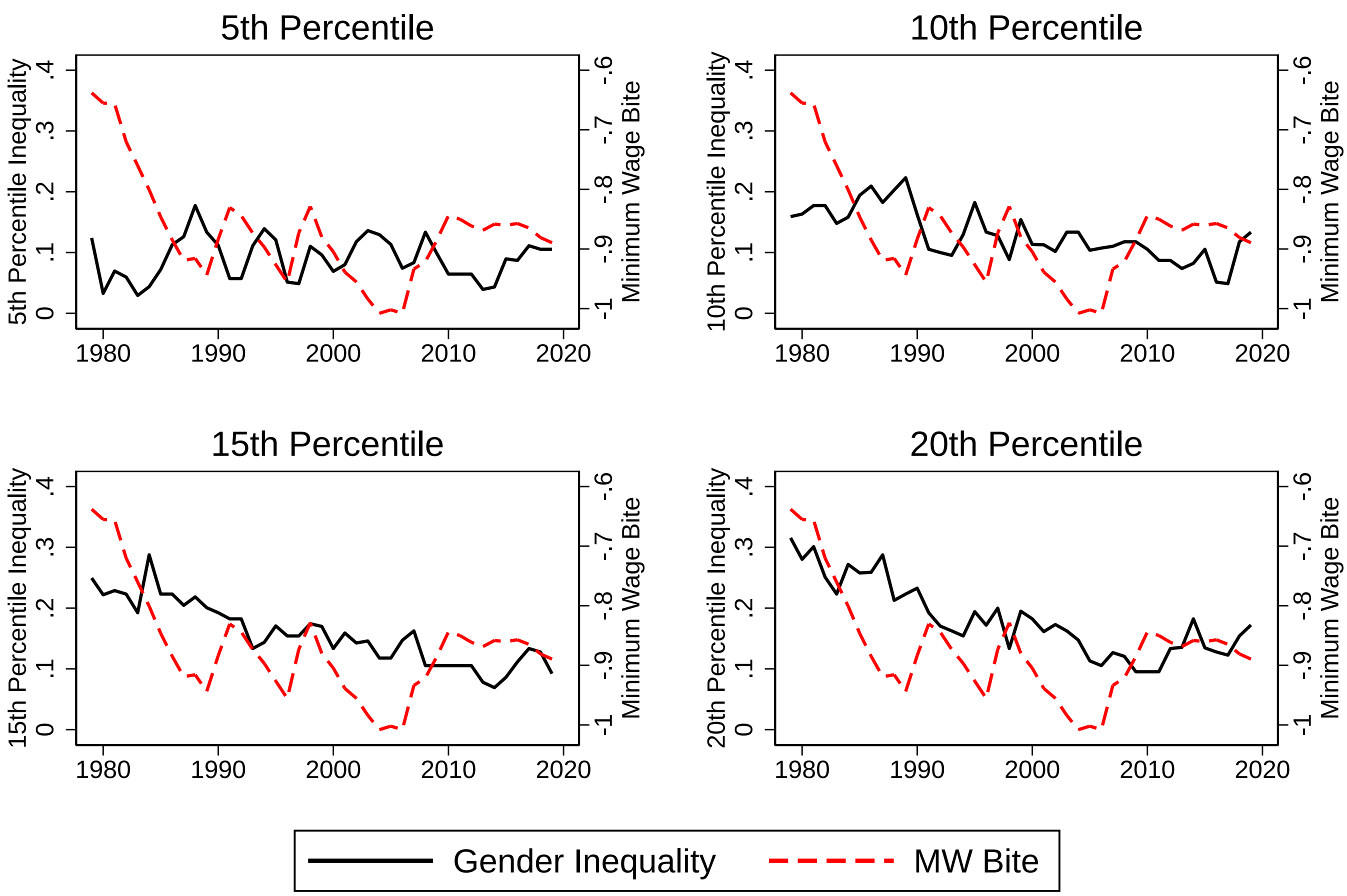
Figures

Figure 1: Evolution of the real effective minimum wage



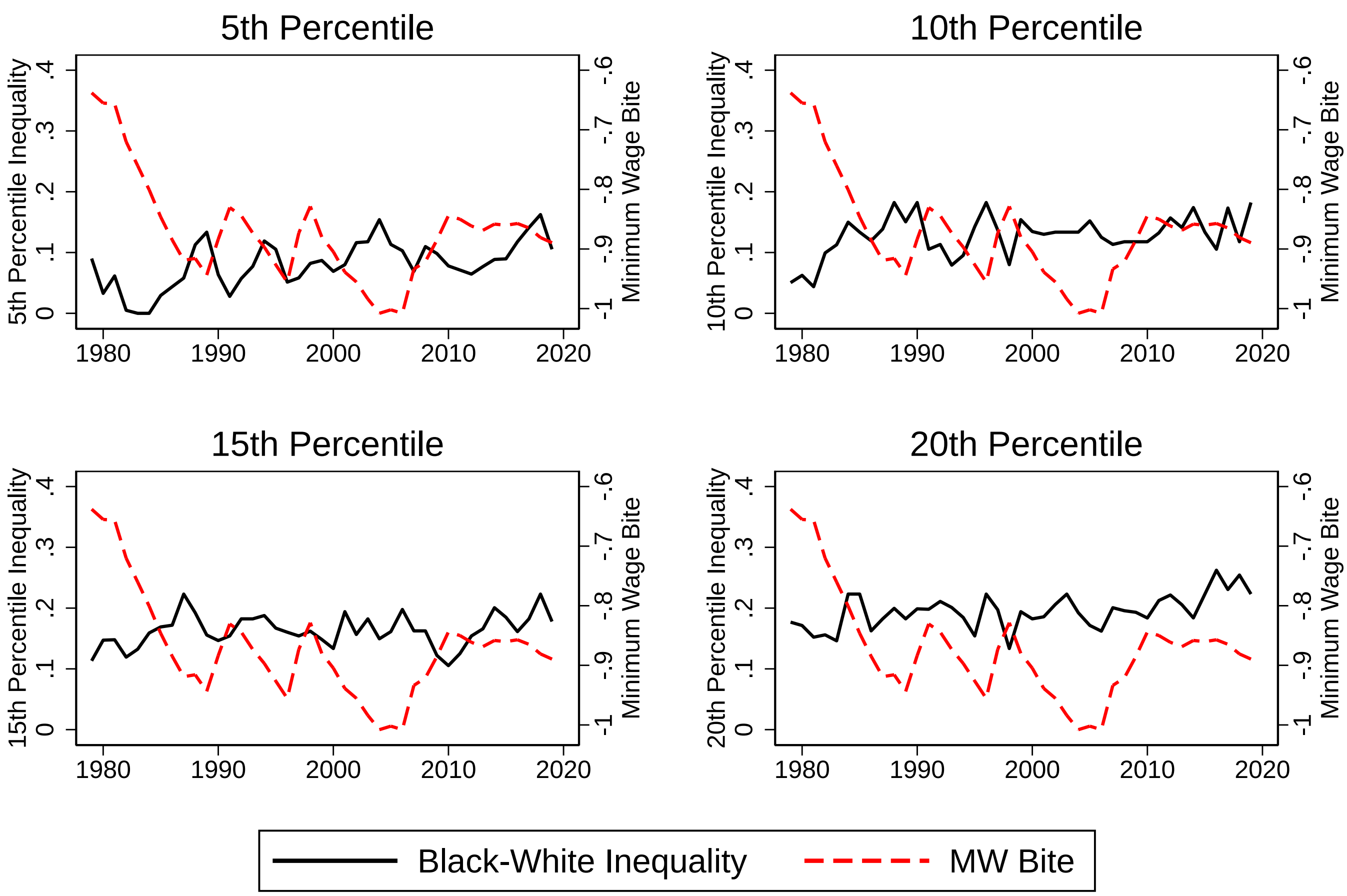
Notes: This figure plots the trajectory of four summary measures of state/federal minimum wage policies over time since 1979. Minimum wage data are from David Neumark's monthly panel dataset of state/federal minimum wage policies. Minimum wages are converted to 2020 dollars using the GDP PCE deflator 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



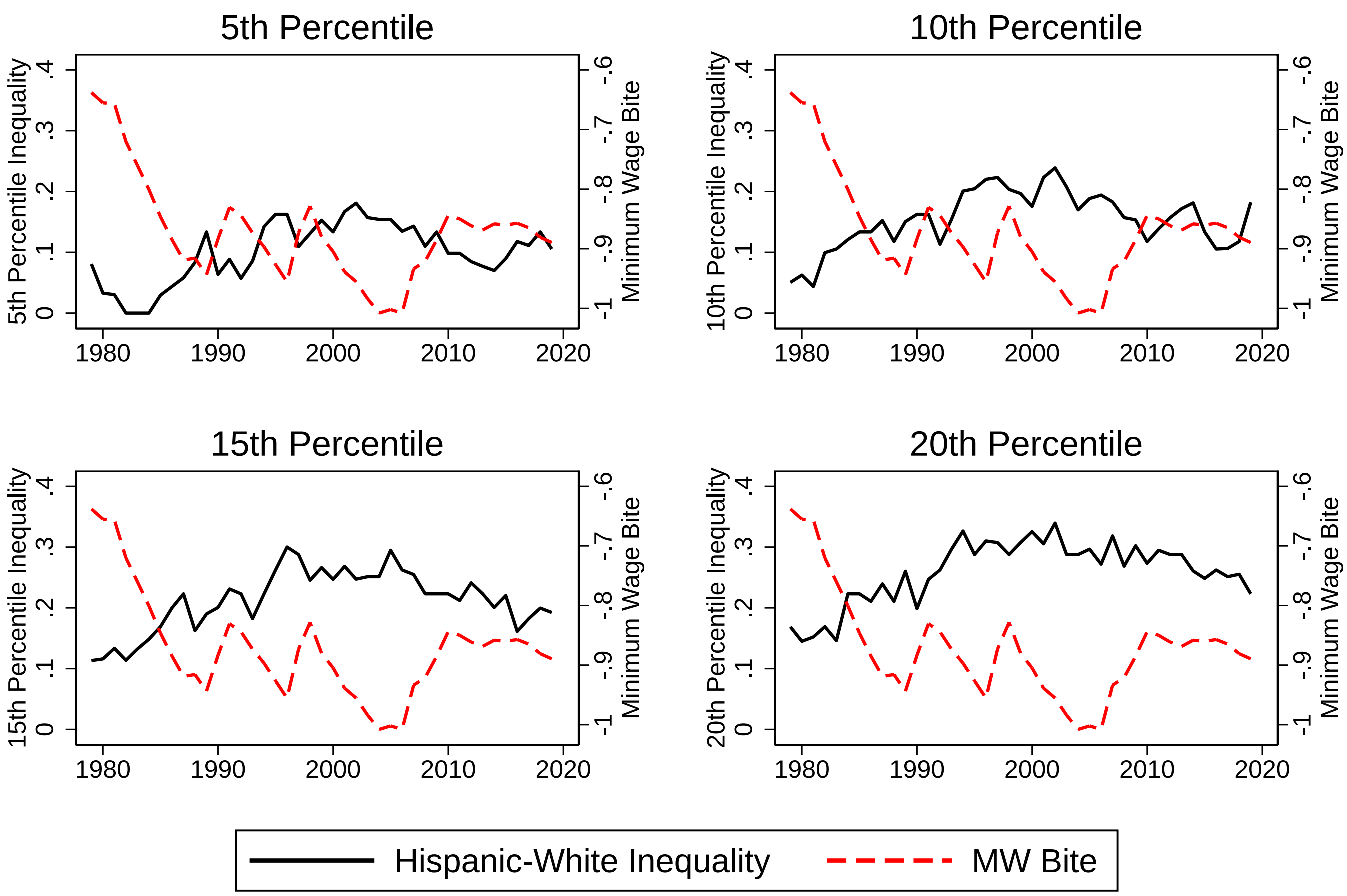
Notes: This figure plots national-level log wage gaps between men and women over time (solid black lines, left-hand axes) at the 5th, 10th, 15th, and 20th percentiles. The path of the bite of the minimum wage (long-dashed red line, right-hand axes) is superimposed. The log wage percentiles are computed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

Figure 2b: Evolution of Black-White inequality



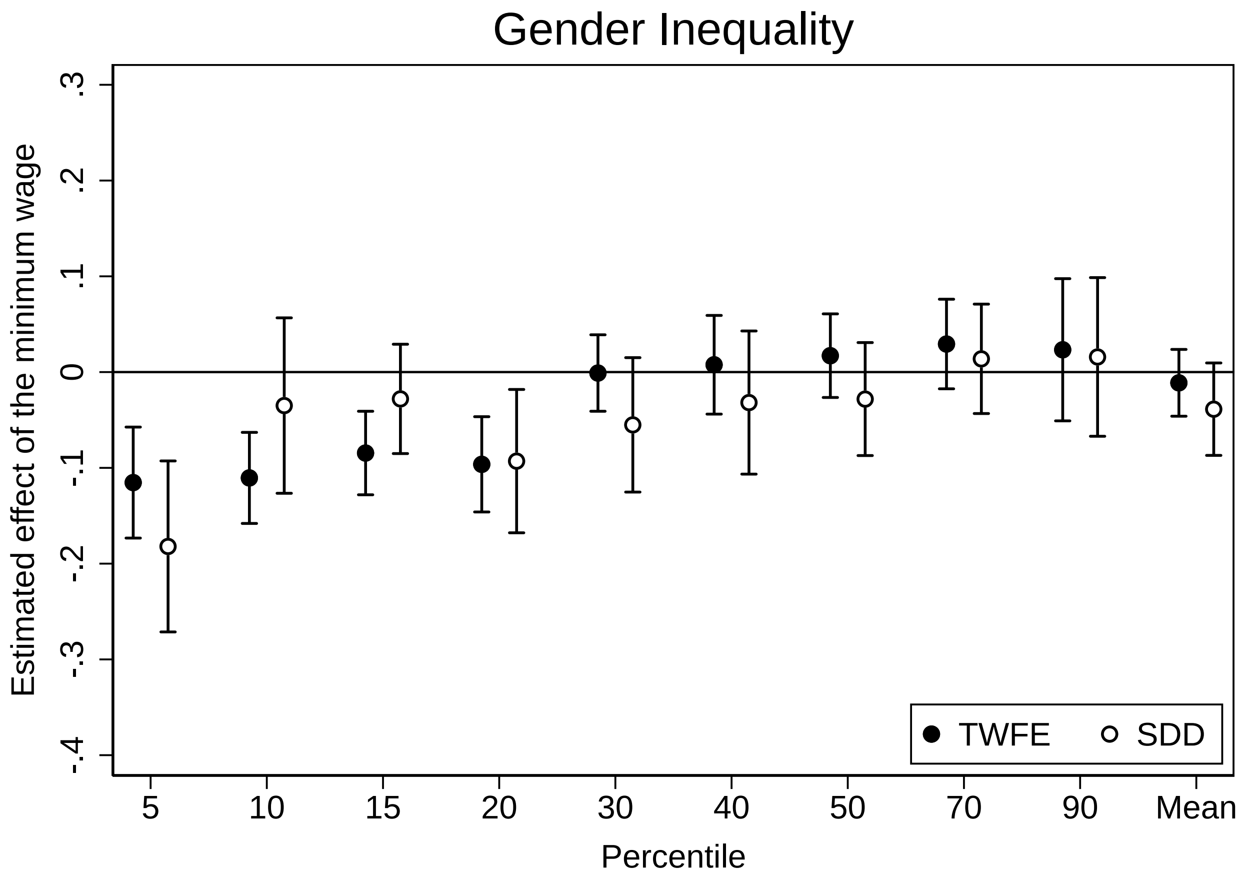
Notes: This figure plots national-level log wage gaps between Blacks and White over time (solid black lines, left-hand axes) at the 5th, 10th, 15th, and 20th percentiles. The path of the bite of the minimum wage (long-dashed red line, right-hand axes) is superimposed. The log wage percentiles are computed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

Figure 2c: Evolution of Hispanic-White inequality



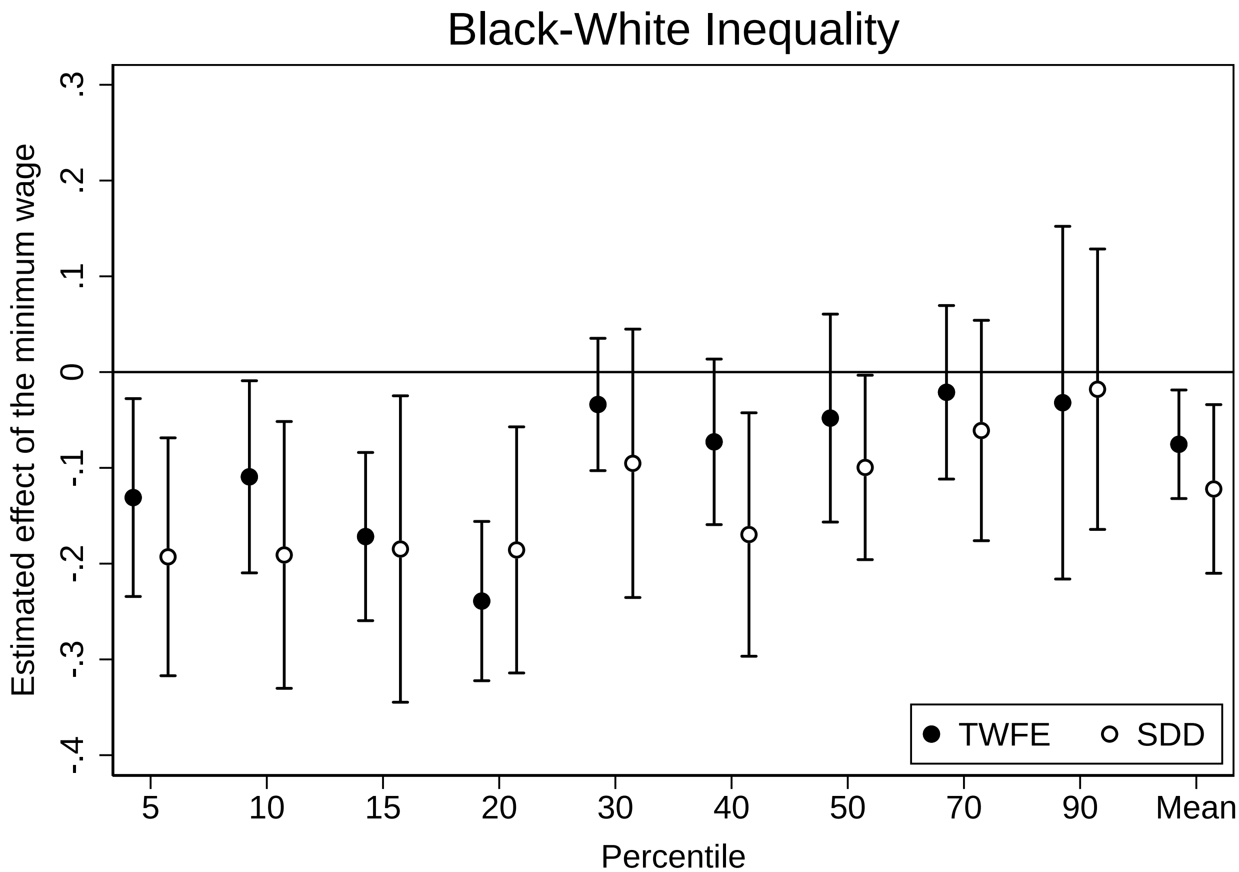
Notes: This figure plots national-level log wage gaps between Hispanics and Whites over time (solid black lines, left-hand axes) at the 5th, 10th, 15th, and 20th percentiles. The path of the bite of the minimum wage (long-dashed red line, right-hand axes) is superimposed. The log wage percentiles are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

Figure 3a: Reduced-form coefficients for gender inequality



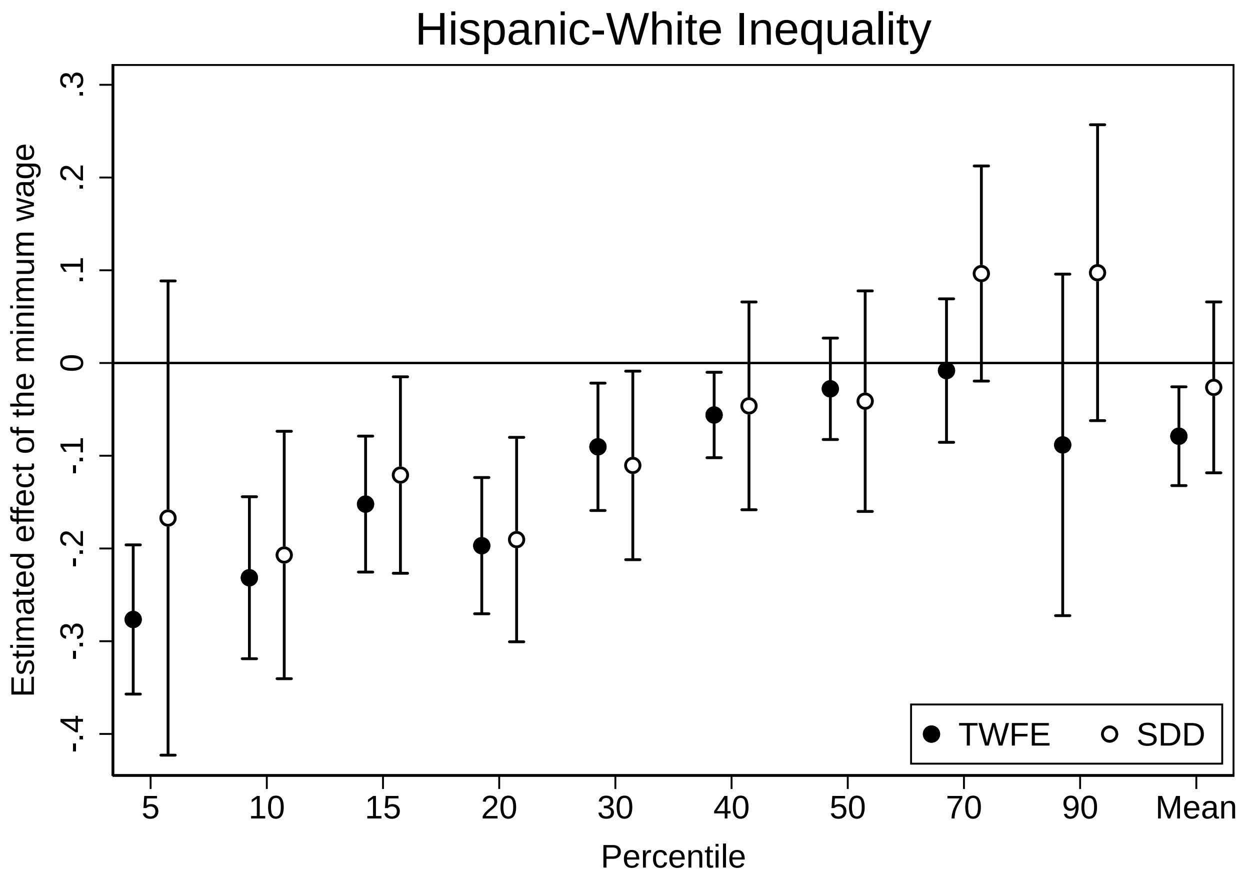
Notes: This figure plots our reduced-form coefficient results from regressions that use inequality in log wages between men and women at various percentiles as the key dependent variable. At each percentile, as well as at the mean, we show coefficients from our two-way fixed effects (TWFE) and our stacked 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 (in the case of SDD).

Figure 3b: Reduced-form coefficients for Black-White inequality



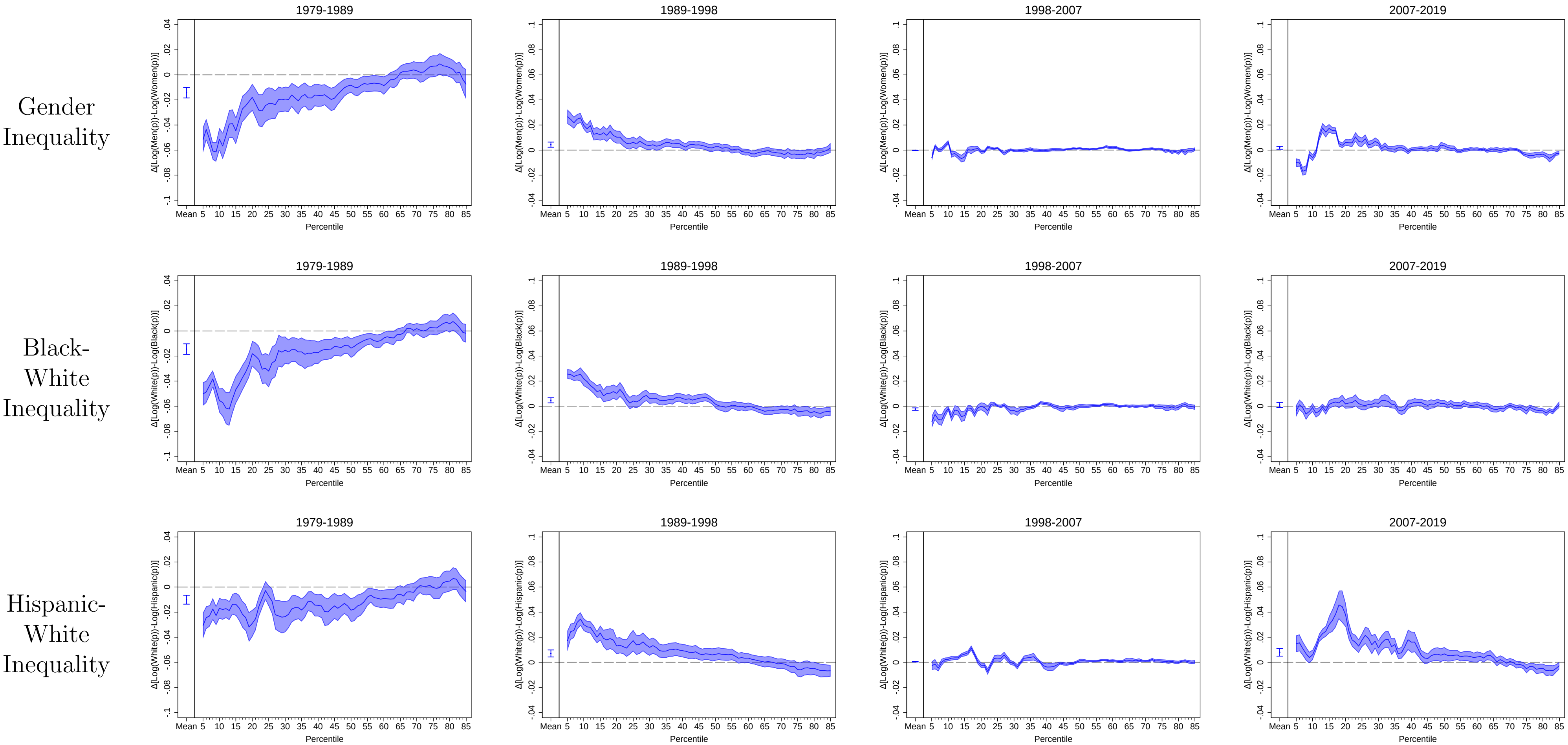
Notes: This figure plots our reduced-form coefficient results from regressions that use inequality in log wages between Blacks and Whites at various percentiles as the key dependent variable. At each percentile, as well as at the mean, we show coefficients from our two-way fixed effects (TWFE) and our 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 (in the case of SDD).

Figure 3c: Reduced-form coefficients for Hispanic-White inequality



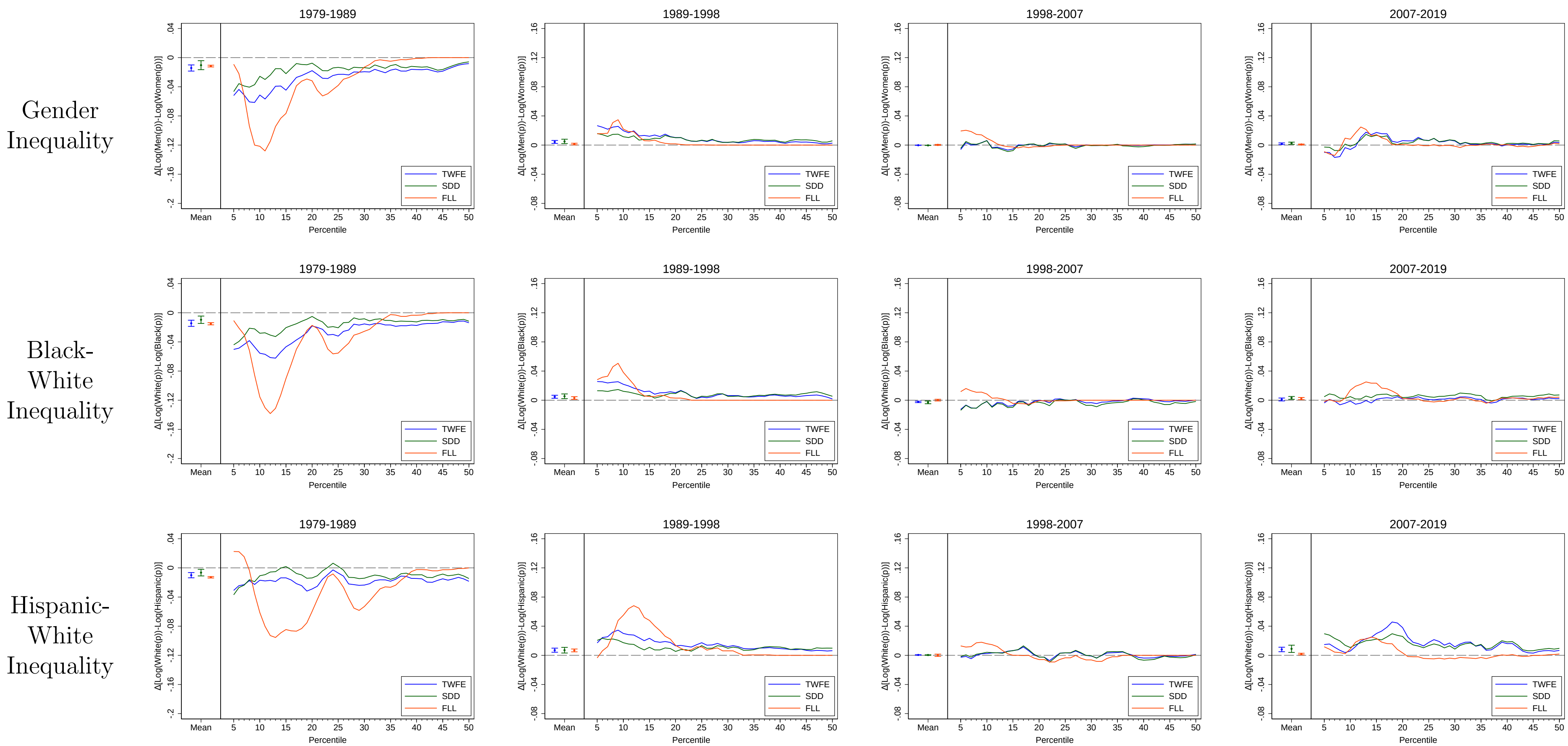
Notes: This figure plots our reduced-form coefficient results from regressions that use inequality in log wages between Hispanics and Whites at various percentiles as the key dependent variable. At each percentile, as well as at the mean, we show coefficients from our two-way fixed effects (TWFE) and our 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 (in the case of SDD).

Figure 4: Counterfactuals using a pooled TWFE equation



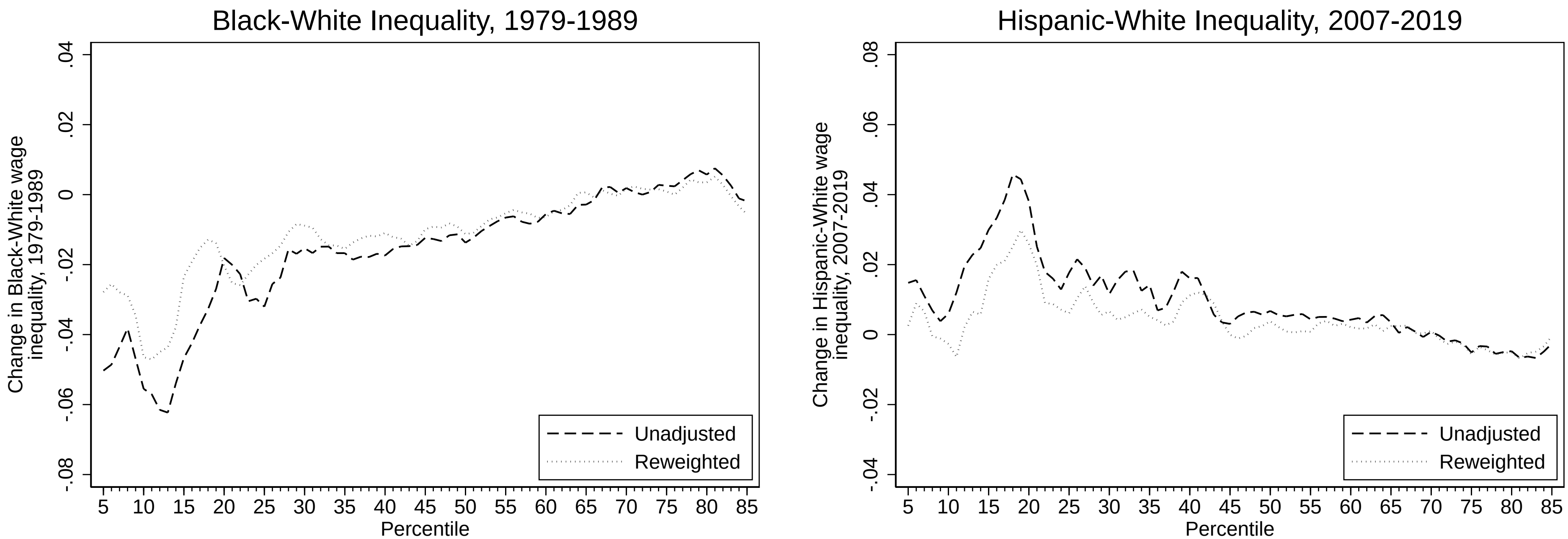
Notes: These figures show smoothed moving averages of estimated counterfactual changes in between-group inequality for the pooled TWFE specification for each pair of groups and for each period. Note that the figures for 1979-1989 have a y-axis that runs from -0.10 to 0.04, whereas the figures for other 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 5: Counterfactual changes using our three core methods



Notes: See the notes for Figure 4.

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



Notes: In this figure, Whites 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 historical period, assuming that the minimum wage structure of the initial year continued to prevail. The "reweighted" lines display these smoothed predicted changes after reweighting Blacks and Hispanics so that their distribution across US states was exactly equal to Whites' over the historical. 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 Appendix Table A5.