MINIMUM WAGES AND RACIAL INEQUALITY*

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The earnings difference between white and black workers fell dramatically in the United States in the late 1960s and early 1970s. This article shows that the expansion of the minimum wage played a critical role in this decline. The 1966 Fair Labor Standards Act extended federal minimum wage coverage to agriculture, restaurants, nursing homes, and other services that were previously uncovered and where nearly a third of black workers were employed. We digitize over 1,000 hourly wage distributions from Bureau of Labor Statistics industry wage reports and use CPS microdata to investigate the effects of this reform on wages, employment, and racial inequality. Using a cross-industry difference-in-differences design, we show that earnings rose sharply for workers in the newly covered industries. The impact was nearly twice as large for black workers as for white workers. Within treated industries, the racial gap adjusted for observables fell from 25 log points prereform to 0 afterward. We can rule out significant disemployment effects for black workers. Using a bunching design, we find no aggregate effect of the reform on employment. The 1967 extension of the minimum wage can explain more than 20% of the reduction in the racial earnings and income gap during the civil rights era. Our findings shed new light on the dynamics of labor market inequality in the United States and suggest that minimum wage policy can play a critical role in reducing racial economic disparities. *JEL* Codes: J38, J23, J15, J31

I. Introduction

One of the most striking dimensions of inequality in the United States is the persistence of large racial economic

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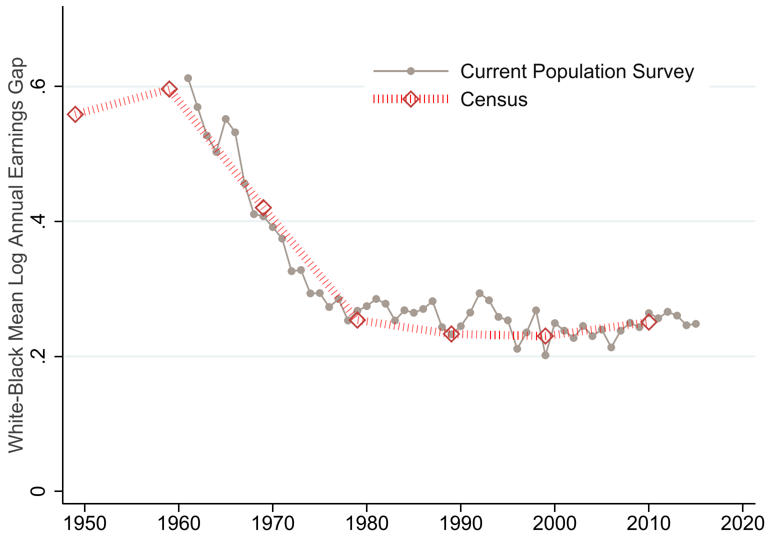


FIGURE I

Economy-Wide White-Black Unadjusted Wage Gap in the Long Run, in the CPS and in the Decennial Censuses

Annual Social and Economic Supplement of the Current Population Survey, 1962–2016; U.S. Census from 1950 to 2000, and American Community Survey data in 2010 and 2017. Sample: Adults 25–65, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. The economy-wide racial gap is defined here as the combination between the industries covered in 1938 and the industries covered in 1967.

disparities (Bayer and Charles 2018; Chetty et al. 2020). A major aspect of these disparities is the earnings difference between black and white workers. There is a 25% gap between the average annual earnings of white and African American workers today (see Figure I).[1] Over the past 70 years, this gap fell significantly only once, during the late 1960s and early 1970s, when it was reduced by a factor of about two. What made the white-black earnings gap fall? Understanding the factors behind

1. The racial earnings gap is measured here as the mean log annual earnings difference between white and black workers (i.e., conditional on working) using two data sources with information on earnings: decennial U.S. Census data, from which we measure earnings from 1949 onward; and an annual data source: the Annual Social and Economic Supplement of the Current Population Survey, from which we measure earnings from 1961 to 2015. Both data sources paint a consistent picture.

this historical improvement may offer insights for reducing the large racial disparities that still exist today.

A large literature has put forward various explanations for the decline in racial inequality during the 1960s and 1970s, including federal antidiscrimination legislation (Freeman 1973) and improvements in education (Smith and Welch 1989; Card and Krueger 1992). The magnitude of the decline, however, remains a puzzle (see Donohue and Heckman 1991, and our discussion of the related literature in Section II).

This article provides a new explanation for falling racial earnings gaps during this period: the extension of the federal minimum wage to new sectors of the economy. The Fair Labor Standards Act of 1966 introduced the federal minimum wage (as of February 1967) in sectors that were previously uncovered and where black workers were overrepresented: agriculture, hotels, restaurants, schools, hospitals, nursing homes, entertainment, and other services. These sectors employed about 20% of the total U.S. workforce and nearly a third of all black workers. Perhaps surprisingly, the role of this major reform in the much studied decline in racial inequality during the civil rights era has not been analyzed before. We show that it had large positive effects on wages for low-wage workers and that the effects were more than twice as large for black workers as they were for white workers. Our estimates suggest that the 1967 extension of the minimum wage can explain more than 20% of the decline in the racial earnings gap between 1965 and 1980. Moreover, we find that this reform did not have large adverse employment effects on either black or white workers. The extension of the minimum wage thus reduced not only the racial earnings gap (the difference in earnings for employed individuals) but also the racial income gap (the difference in income between black and white individuals, whether working or not). To our knowledge, our article provides the first causal evidence on how minimum wage policy affects racial income disparities.

Our contribution in this article is twofold. First, we provide an in-depth analysis of the causal effect of the 1967 extension of the minimum wage—a large natural quasi-experiment—on the dynamics of wages and employment. To conduct this analysis, we use a variety of data sources and research designs that paint a consistent picture. A key data contribution is to assemble a novel data set on hourly wages by industry, occupation, gender, and region. In the 1960s, 1970s, and 1980s, the Bureau of Labor Statistics (BLS) published regular industry wage reports with detailed information on the distribution of hourly wages by 5- and 10-cent bins, including the number of workers employed in each of these bins. For the purposes of this research, we digitized more than 1,000 of these tabulations. This new data source allows us to provide transparent and robust evidence on the effects of the 1967 minimum wage extension on wages and employment. We also rely on microdata from the March Current Population Survey (CPS), which allow us to investigate how the effects of the reform vary with race and other socioeconomic characteristics such as education. Taken together, the CPS and BLS data enable us to provide consistent and clear graphical evidence of the short- and medium-term effects of the extension of the minimum wage.

The analysis proceeds in two steps. First, we show that the 1967 reform had a large effect on wages for workers at the bottom of the earnings distribution. Our newly digitized BLS data reveal clear evidence of an immediate and sharp hourly wage increase for low-paid workers: a large mass of workers paid below $1 in 1966 (the level of the minimum wage introduced in 1967) bunches at $1 in 1967. To quantify the magnitude of the wage effect, our baseline empirical approach is a cross-industry difference-in-differences research design: we compare the dynamics of wages in the newly versus previously covered industries, before and after 1967. In the CPS data, the average annual earnings of workers in the industries covered in 1967 (our treated group) evolve in parallel with the annual earnings of workers in the industries covered in 1938 (our control group) before the reform. In 1967, they jump by 5.3% relative to the control industries and the effect persists through the late 1970s. The magnitude of the increase is consistent with the predicted effect of the minimum wage hike estimated using the prereform CPS. We obtain a similar increase in the average hourly wage in the newly covered industries using the BLS data. We estimate that 16% of workers in the treated industries are affected by the reform and that they receive a 34% wage increase on average in 1967. The wage effect on treated workers is large because before 1967, many of them (predominantly black workers) were employed at wages far below the federal minimum wage of $1 introduced in 1967. The wage increase in the newly covered industries is concentrated among workers with a low level of education. The magnitude of the wage effect is robust to a series of tests and to controlling for a wide range of observable characteristics and time trends.

In a second step, we study the effect of the 1967 minimum wage extension on employment. We first estimate employment effects using geographic variation in the bite of the reform. Just as today, some states had their own minimum wage laws (on top of the federal minimum wage) in the 1960s while others did not. This variation made the 1967 reform more or less binding across states. We build a minimum wage database by state, industry, and gender spanning the 1950–2016 period. We compare states without a state minimum wage law as of January 1966 (strongly treated) to other states (weakly treated). Because the federal minimum wage was high in the late 1960s (much higher than today relative to the median wage), the 1967 reform is a particularly large shock in the strongly treated states. Using this research design, we show that the 1967 reform had a near-zero effect on employment. We are able to rule out employment elasticities with respect to average wages greater (in absolute sense) than −0.16. The results hold for black workers in isolation, for whom employment elasticities greater than −0.24 can be ruled out.

We build on these analyses by using our BLS data and implementing a bunching estimator (following Harasztosi and Lindner 2019; Cengiz et al. 2019). Within treated industries, we compare the number of workers paid strictly below the minimum wage and those paid at or slightly above the minimum wage in the observed 1967 wage distribution to those in a counterfactual distribution with no minimum wage reform. We first present estimates of the employment effect of the reform for an important case study—laundries in the U.S. South—where the reform was particularly binding (over one-third of workers were paid below the minimum wage prior to the reform) and where black workers were overrepresented (40% of the workforce). We document a near-zero effect on employment in this sector and region. We demonstrate that this near-zero effect holds across many industry and region subgroups. Overall, our bunching results suggest low employment responses in treated industries in the United States as a whole. Our findings are robust to considering alternative assumptions on the extent of spillover effects from the minimum wage.[2]

2. Under the assumption of spillovers up to 115% (120%) of the minimum wage, we calculate an employment elasticity of 0.06 (−0.21) in the treated industries as a whole, qualitatively similar to our CPS estimates and well in the range of those in the broader minimum wage literature. See Online Appendix Figure E5.

The second—and most important—contribution of the article is to uncover the key role of minimum wage policies in the dynamics of racial inequality. We show that the extension of the minimum wage during the civil rights era can explain more than 20% of the decline in the unadjusted black-white earnings gap observed during this critical period of time. The reform reduced the gap through two channels. First, the gap between the average wage in the treated industries and the rest of the economy fell. Because black workers were overrepresented in the treated industries, this between-industry convergence reduced the nationwide racial gap. Second, within the newly covered industries, the wage increase is much larger for black than for white workers, and hence the reform sharply reduced the unadjusted racial gap within the treated industries. This within-industry effect accounts for more than 80% of the impact of the reform on the economy-wide racial gap. The reform also sharply reduced the adjusted racial earnings gap (i.e., the difference in earnings between black and white workers conditional on observable characteristics) within the treated industries, from 25 log points prior to 1967 to about 0 after. That is, within agriculture, laundries, and so on, black workers were paid 25 log points less than white workers with similar observables (such as education, experience, number of hours worked) when the federal minimum wage did not apply, and this difference falls to close to 0 after the introduction of the federal minimum wage. Combined with the evidence of limited effects on black employment, these results suggest that the 1967 reform was effective at advancing black economic status.

Conceptually, our results are consistent with competitive models of the labor market characterized by low elasticity of demand for workers in the newly covered industries and inelastic demand for black workers, in particular.[3] We provide evidence that substitution toward white workers was extremely limited in the newly covered industries after the reform. This may stem in part from the high degree of occupational segregation prevalent in the labor market at the time. Black workers were concentrated in low-status jobs throughout our period of analysis, and white workers may have been unwilling to assume these positions at the wages prevailing postreform. Under these conditions,

3. Our results are also consistent with monopsonistic models of the labor market in which the minimum wage falls above the monopsonist's but below the perfect competitor's wage.

the minimum wage can improve black workers' relative wages without resulting in their significant relative disemployment.

The remainder of the article is organized as follows. We start by relating our work to the literature in Section II. Section III presents background information on the 1966 amendments to the Fair Labor Standards Act and describes the datasets used in this research. We present the effects of the reform on wages in Section IV and its effects on employment in Section V. Section VI quantifies the role of the 1967 extension of the minimum wage in the decline of the racial earnings and income gap and discusses potential explanations for our findings. Section VII concludes. An Online Appendix supplements the article. The data and programs used in this article are available at clairemontialoux.com/flsa.

II. Related Literature

Our article lies at the intersection of two core literatures in labor economics: racial inequality and the economic effects of the minimum wage.

II.A. Literature on Racial Inequality and the Civil Rights Movement

A large body of work seeks to understand what caused the decline in the racial earnings gap during the civil rights era, a period that saw major policy and economic changes. Two explanations have been advanced: changes in the demand versus supply side of the labor market.

A number of studies investigate whether antidiscrimination policies increased the relative demand for black workers (Freeman 1973; Freeman et al. 1973; Vroman 1974; Wallace 1975; Butler and Heckman 1977; Freeman 1981; Brown 1984; Smith and Welch 1986).[4] This literature focuses on employment outcomes rather than on the racial gap itself. Other studies (see, e.g., Donohue and Heckman 1991; Wright 2015; Aneja and Avenancio-Leon 2019; Johnson 2019) consider the role of the Voting Rights Act of 1962 and 1965 and other federal initiatives (e.g., school desegregation) in narrowing the racial gap. One

4. A cornerstone of the civil rights movement, Title VII of the 1964 Civil Rights Act prohibited both employment and wage discrimination based on race, sex, color, religion, and national origin. It was enforced by the Equal Employment Opportunity Commission (EEOC), created in 1965.

key difficulty faced in this literature is that federal government policies affected the nation as a whole, making it difficult to identify their causal impact.[5] It is also difficult to obtain good measures of government antidiscrimination activity. Most of the literature used either sparse intercensal wage data or aggregated time series, making it difficult to isolate the contribution of these policy changes at the macro level.[6]

On the supply side, the literature has identified two important developments contributing to the decline in the racial gap. First, educational outcomes improved for African Americans. Lillard, Smith, and Welch (1986) and Smith and Welch (1989) emphasize the relative increase in the number of years of schooling for black workers. They concluded that an increase in school quantity can explain about 20%–25% of the narrowing of the black-white wage gap in the late 1960s. Card and Krueger (1992, 1993) find that about 15%–20% of the reduction in the racial wage gap owes itself to improvements in school quality for black children.[7] Second, the increase in income transfers in the context of President Johnson's Great Society may have led to a reduction in the labor force participation of black workers with low levels of education (Butler and Heckman 1977). Donohue and Heckman (1991) find that this specific factor can explain about 10%–20% of black-white wage convergence while other supply-side factors can explain about 55% of the decline during the civil rights era.[8]

Our study pushes the literature forward in two directions. First, our article is the first to highlight the role played by the 1967 minimum wage extension in the decline of racial inequality.

5. The identification problem is particularly acute for studies of the role of the EEOC, as Title VII covers all firms in the economy. Heckman and Wolpin (1976) also show that it is difficult to assess the causal effect of the Office of Federal Contract Compliance as the contract status of a firm is endogenous (government contracts are awarded to less discriminatory firms).

6. A notable exception is Heckman and Payner (1989), who focus on the textile manufacturing industry in South Carolina. They were, however, unable to infer economy-wide estimates based on this study.

7. Card and Krueger (1992) do not find evidence of any contribution of the relative increase in school quantity to the reduction in the racial earnings gap in the late 1960s.

8. Other supply shift stories, such as the northern migration of African Americans over the twentieth century, have been found to play a minor role. Smith and Welch (1986) note that northern migration actually slowed in the mid-1960s; their Table 18 shows that the percentage of black men living in the South was 74.8 in 1940, 57.5 in 1960, and 53.1 in 1980.

This factor turns out to be quantitatively important, comparable in size to the impact of relative school quality improvements found by Card and Krueger (1992) and school quantity improvements found by Smith and Welch (1986). Our article moves us closer to a full quantitative understanding of what caused the decline in the racial earnings gap in the 1960s.

Second, our study solves a key puzzle in the literature on the dynamics of racial inequality. Figure II, Panel A plots the evolution of the unadjusted racial earnings gap since the early 1960s, measured as the mean log difference in annual earnings between white and black workers. As is apparent from this figure, almost half of the decline happened in just two years: 1967 and 1968.[9] Neither the demand nor supply factors described above can easily explain the specific timing of the reduction in the racial earnings gap. Antidiscrimination policies were rolled out gradually from 1964 onward, with enforcement powers gradually increasing over time (Wallace 1975; Butler and Heckman 1977).[10] Similarly, there is no sudden change in schooling quantity or quality for African Americans in 1967; educational improvements occurred gradually. Income transfers also rose progressively throughout the 1960s and 1970s.[11] By contrast, the 1967 extension of the minimum wage can explain why the decline in the racial earnings gap is particularly pronounced in 1967. Figure II, Panel B shows indeed that the unadjusted racial earnings gap fell sharply in the newly covered industries relative to the previously covered ones precisely in 1967.

9. The unadjusted racial gap was 53 log points in 1966, and it fell to 46 in 1967 and 41 in 1968. In 1979, it was down to 27 log points.

10. Only in 1972 was the EEOC given the power to initiate litigation. Before 1972, it could not file lawsuits to enforce Title VII and could only refer cases to the Justice Department or briefs as "friends of the court," see Brown (1982). The EEOC's backlog of complaints increased gradually over the late 1960s and 1970s (see, e.g., U.S. Civil Rights Commission, 1977, 211: https://www2.law.umaryland.edu/marshall/usccr/documents/cr12en22977.pdf).

11. Medicare and Medicaid were introduced in 1966, but were initially small (1.7% of all government transfers in 1966) before gradually increasing to 4.8% of all transfers in 1970, 6.4% in 1975, and 8.2% in 1980. See Table II-C3b in Piketty, Saez, and Zucman (2018), available at http://gabriel-zucman.eu/usdina/. Food stamps were introduced in 1964, then rolled out across counties. It was only in 1975 that all counties were mandated to offer a food stamps program (Hoynes and Schanzenbach 2009). Aid to Families with Dependent Children (AFDC) expanded cash benefits in the early 1970s (U.S. Department of Health & Human Services 2001). Taken together, all transfers accounted for 24% of the national income per adult in 1961, 24% in 1966, 28% in 1970, and 32% in 1975. See Table II-C3b in Piketty, Saez, and Zucman (2018), available at http://gabriel-zucman.eu/usdina/.

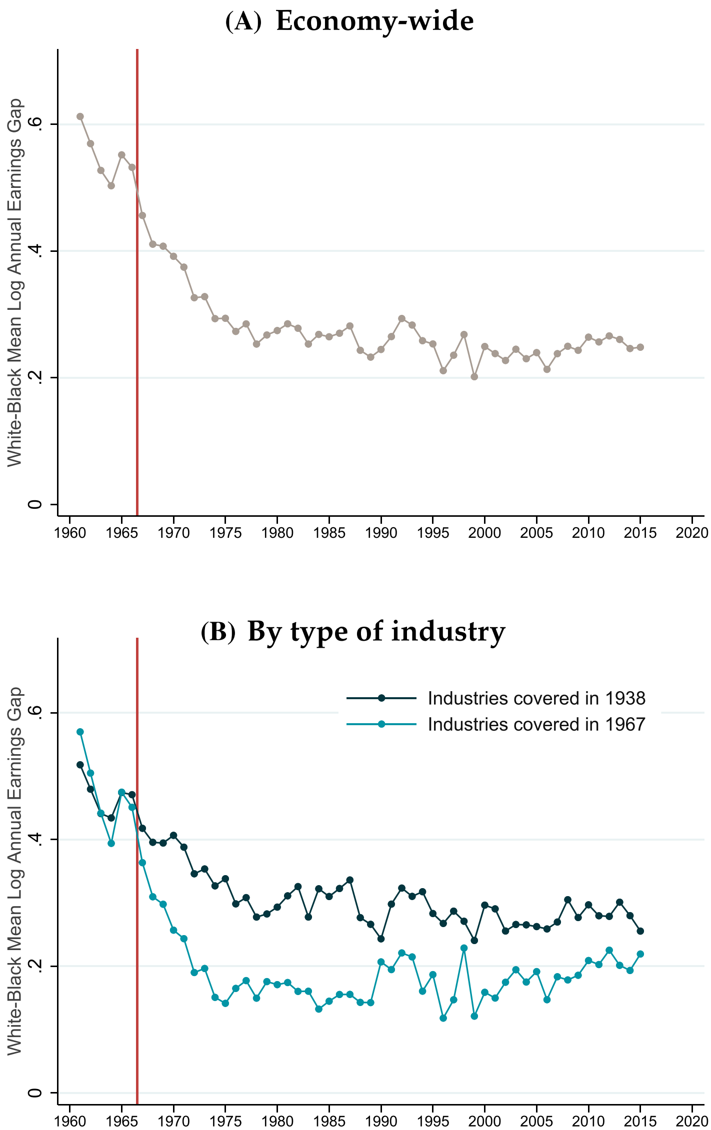


FIGURE II

White-Black Unadjusted Wage Gap in the Long Run

Annual Social and Economic Supplement of the Current Population Survey, 1962–2016. Sample: Adults 25–65, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. The economy-wide racial gap is defined here as the combination between the industries covered in 1938 and the industries covered in 1967. Color version of figures available online.

II.B. Minimum Wage Literature

Our article contributes in several ways to an expansive literature on the economic effects of the minimum wage. First, our study is the first to provide causal evidence on how minimum wage policy can affect racial economic disparities. A large body of work discusses the efficiency costs of the minimum wage and focuses on employment effects (see, e.g., Card 1992; Neumark and Wascher 1992, 2008; Card, Katz, and Krueger 1993; Card and Krueger 1995; Dube, Lester, and Reich 2010; Cengiz et al. 2019). The literature also examines effects on wage inequality (see, e.g., Blackburn, Bloom, and Freeman 1990; DiNardo, Fortin, and Lemieux 1996; Lee 1999; Autor, Manning, and Smith 2016) and family incomes (Gramlich 1976; Congressional Budget Office 2014; Dube 2019b). To date, however, the interplay between the minimum wage and racial inequality has not been investigated using a causal research design.

Second, our article provides evidence on the economic effects of very large minimum wage increases. The 1967 reform was a large shock to treated industries in states that did not have a state minimum wage—in these states, the wage floor moved from 0 to the prevailing federal minimum wage, at a high level in the late 1960s.[12] Bailey, Di Nardo, and Stuart (2020) investigate how the high nationwide minimum wage mandated by the 1966 FLSA affected employment, exploiting state-level differences in the bite of a national minimum wage due to differences in standards of living. Consistent with our estimates, they found little evidence of disemployment effects, neither overall nor for particular subgroups of the population.[13] Because our article focuses on different questions (the impact of the minimum wage on the black-white income gap and the effect of the 1967 reform on the newly covered industries), uses different research designs (cross-industry difference-in-differences and bunching),

12. In addition to expanding coverage, the 1966 FLSA increased the federal minimum wage from $1.25 in 1966 to $1.40 in 1967 and $1.60 from 1968 on (the equivalent of $9.91 in 2017 dollars, i.e., its historical peak).

13. When using an alternative measure of employment—employed at any point during the year, as opposed to the standard definition of employment, that is, employed during the reference week—Bailey, Di Nardo, and Stuart (2020) find small disemployment effects among black men. This result arises only with this nonstandard measure of employment. We further contextualize and discuss this result in Online Appendix E.6.

and relies in part on different data (our newly digitized BLS tabulations), we view our projects as complementary.[14]

More broadly, we contribute to a recent literature that analyzes sharp changes in the minimum wage, either in the United States at the city level (see, e.g., Jardim et al. 2018) or in foreign countries (e.g., Engbom and Moser 2018; Harasztosi and Lindner 2019), and to a burgeoning literature on bunching estimation applied to the minimum wage (Cengiz et al. 2019).[15] Our evidence of substantial wage effects and small employment effects from the 1967 reform is highly consistent with this literature on recent policy changes. Our study reflects the specific context of the late 1960s United States, characterized by rapid economic growth and high levels of occupational segregation. Taken together, however, the literature on large hikes sheds light on current policy discussions in the United States, where a number of local and federal policy makers are implementing or considering large increases in minimum wages.

Finally, we contribute a new database of minimum wage legislation by state, industry, and gender spanning the 1950–2016 period. Looking forward, this database could be used to exploit historical changes in minimum wage legislation across industries or gender groups (in contrast to the bulk of the literature that focuses on cross-state variation).

III. The 1967 Extension of the Minimum Wage and Data

III.A. *The 1966 Fair Labor Standards Act*

1. *Political Economy of the Reform.* The Fair Labor Standards Act (FLSA) of 1938 introduced the federal minimum wage

14. In addition to the papers mentioned here, an older study by Castillo-Freeman and Freeman (1992) analyzed the effect of federal minimum wage policy in Puerto Rico in the 1970s, where the bite was extremely high. Using cross-industry, time-series evidence, the authors show the minimum wage reduced the employment-to-population ratio, resulted in reallocation of labor from low-wage to high-wage industries, and increased migration to the mainland by workers with low levels of education.

15. A key advantage of the bunching approach is that it offers transparent graphical evidence on the employment effects of minimum wage hikes in the affected part of the wage distribution. By contrast, prior literature has focused on strongly affected subgroups, such as teens, or workers in specific industries, typically restaurants (Abowd et al. 2000; Neumark, Salas, and Wascher 2014; Allegretto et al. 2017).

in the United States. Millions of workers became subject to a wage floor. The coverage of the act, however, was incomplete: a number of sectors were excluded. The 1938 FLSA covered about 54% of the U.S. workforce (see Figure IV, Panel A) in the manufacturing, transportation and communication, wholesale trade, finance, and real estate sectors (see the complete list of covered sectors in Figure III). President Roosevelt intended to cover the economy as a whole but faced resistance in Congress, particularly from Southern Democrats (Phelps 1939). The law enacted in 1938 stipulates that only employees engaged in interstate commerce or the production of goods for interstate commerce be covered (Daugherty 1939). In practice, this meant that a number of sectors where black workers were overrepresented, such as agriculture, were excluded. The 1938 FLSA, like a number of other programs passed in the 1930s and 1940s, thus had a discriminatory dimension (Mettler 1994; Katznelson 2006; Rothstein 2017).

Over time, a series of amendments to the 1938 FLSA extended the minimum wage to the rest of the economy. In this article, we focus on the 1966 FLSA amendments, the largest expansion of the federal minimum wage.[16] The 1966 FLSA amendments introduced the federal minimum wage (as of February 1, 1967) in the following sectors: agriculture, nursing homes, laundries, hotels, restaurants, schools, and hospitals. These sectors employed about 8 million workers (see Figure IV, Panel A) in 1967, or 21% of the U.S. workforce. Critically, nearly a third of all U.S. black workers worked in the sectors covered for the first time in 1967, compared with about 18% of all U.S. white workers. The extension of the minimum wage to previously uncovered sectors of the economy was one of the 10 demands formulated by the civil rights movement during the March on Washington for Jobs and Freedom in August 1963.[17] President Johnson was also conscious of this imbalance and declared when signing the

16. Using CPS data, we estimate that 54% of the U.S. workforce was covered by the 1938 FLSA as of 1966, an additional 16% was covered by the 1961 amendments (which introduced the minimum wage in retail trade and construction), and an additional 21% by the 1966 amendments, which are the focus of this research. The remaining 9% of the workforce (domestic workers and workers in public administration) were covered after 1966. We refer to this extension of the minimum wage as the "1967 reform" throughout the article.

17. The ninth demand is formulated as follows: "[We demand] a broadened Fair Labor Standards Act to include all areas of employment that are presently excluded"; see Online Appendix Figure H1.

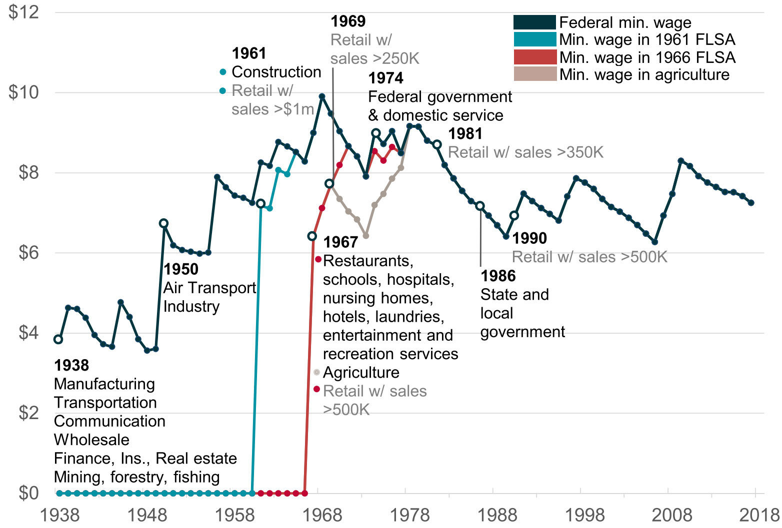
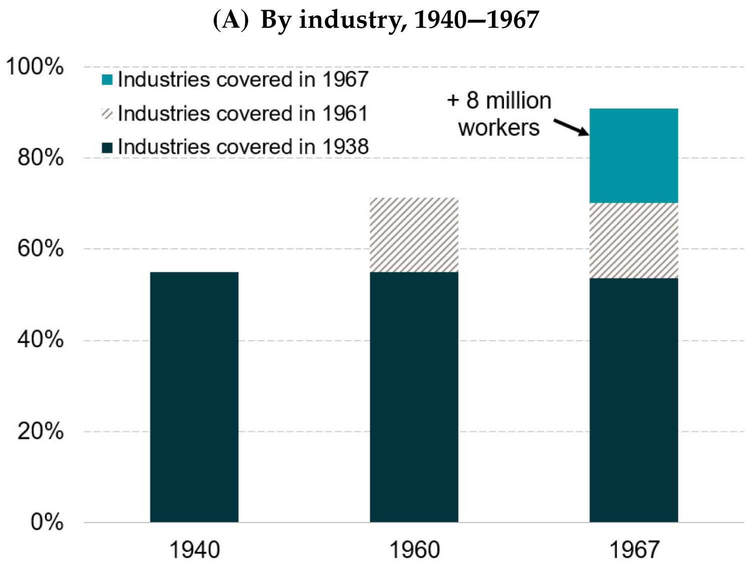


FIGURE III

Expansions in Minimum Wage Coverage, and Real Values of the Minimum Wage, 1938–2018 ($2017)

For the breakdown by industry: see our analysis of the Fair Labor Standards Act (FLSA) in Online Appendix A. For the values of the minimum wage, see Department of Labor, Wage and Hour Division, History of Federal Minimum Wage Rates Under the Fair Labor Standards Act, 1938–2009, available at: https://www.dol.gov/whd/minwage/chart.htm. The 1938 Fair Labor Standards Act introduced the federal minimum wage in manufacturing, transportation, communication, wholesale trade, finance, insurance and real estate, mining, forestry, and fishing. In 1950, the federal minimum wage was expanded to the air transport industry. In 1961, the minimum wage coverage was extended to all employees of retail trade enterprises with sales over $1 million and to construction enterprises with sales over $350,000. In 1967, the minimum wage was extended to agriculture, restaurants, hotels, schools, hospitals, nursing homes, and other services, and was introduced at $1 in nominal terms (i.e., $6.43 in $2017). This corresponded to 71% of the federal minimum wage that year. It increased gradually over the following years. It took 4 years for the minimum wage in the 1967 industries (except agriculture) to converge to the federal minimum wage. It took 11 years for the minimum wage in agriculture to converge to the federal minimum wage. Minimum wages series are deflated using CPI-U-RS ($2017). For more details on the sales threshold that applied to the retail sector starting in 1961, see Online Appendix A.

amendments that: “[The minimum wage law] will help minority groups who are helpless in the face of prejudice that exists. This law, with its increased minimum, with its expanded coverage will prevent much of th[e] exploitation of the defenseless—the workers who are in serious need” (Johnson 1966).



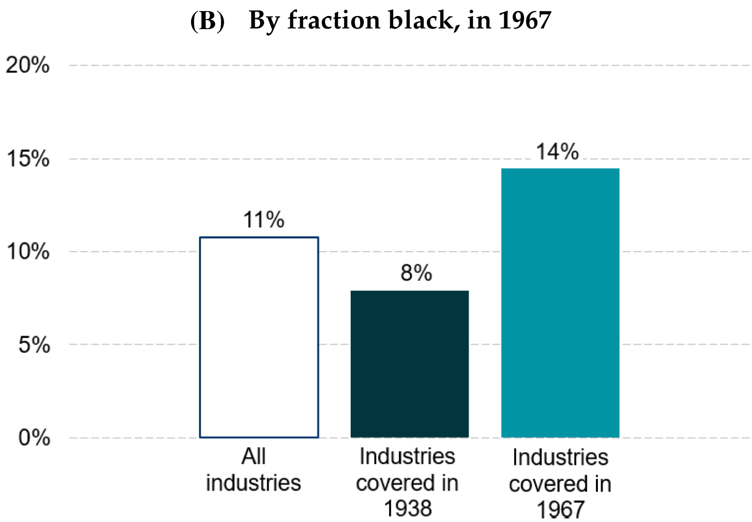


FIGURE IV

Share of Workers Covered by the Minimum Wage

U.S. Censuses 1940 and 1960. March CPS 1967. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. Coverage by federal minimum wage.

2. *A Sharp Change in Minimum Wage Policy.* The 1967 extension of the minimum wage represented a sharp increase in the minimum wage in many sectors of the economy. The ratio between the federal minimum wage and the median wage rose from 0% to 38% in 1967 in the newly covered industries.[18] The Kaitz index exhibits a jump in 1967 as well (see Online Appendix Figure A1). The minimum wage introduced in these sectors in 1967 ($1 in nominal terms) was initially below the federal minimum wage but converged to the level of the federal minimum wage by 1971, except in agriculture where convergence was only complete in 1977.[19] As a result, the ratio between the federal minimum wage and the median wage continued to increase in the newly covered sectors over time and reached 40%–50% during the 1970s, a level close to the one seen in the industries that were covered in 1938.

III.B. Data Used in Our Analysis

We use four data sources to study the 1967 extension of the minimum wage: industry wage reports published by the Bureau of Labor Statistics that we digitized; Current Population Survey micro files going back to 1962; U.S. decennial census data; and data on state minimum wage legislation by industry and gender. All the data are available at clairemontialoux.com/flsa; see Online Appendix I.

1. *BLS Industry Wage Reports.* The BLS conducted regular establishment surveys, starting in the 1930s through the 1980s, to monitor the implementation of the FLSA of 1938 and its amendments.[20] The surveys were requested by the Department of Labor's wage and public contracts divisions. The BLS reports are provided for detailed industries (often at the three-digit Standard Industrial Classification level), with a broad

18. This sharp change in the minimum wage to median ratio is also visible when taking into account the state minimum wage laws varying at the state × industry × gender level; see Online Appendix Figure E1.

19. In all sectors except agriculture, the minimum wage was introduced at $1 an hour in February 1967. Then the minimum wage was raised annually in 15-cent-an-hour increments, effective each February 1 through 1971, to $1.60 an hour.

20. The BLS establishment surveys started in 1934, after the outbreak of a general strike in the cotton textile industry. Several surveys were then undertaken in cooperation with the Works Progress Administration to monitor working conditions in these industries. For a history of BLS statistics from the nineteenth century to the 1980s, see Douty (1984).

coverage of the manufacturing and the nonmanufacturing sectors nationwide.[21]

The BLS focused on collecting information on the distribution of employer-paid hourly earnings, based on employer payroll records.[22] Hourly earnings exclude premium pay for overtime, work on weekends, holidays, and late shifts. Our data come in the form of tabulations that provide detailed distributions of hourly earnings by 5- and 10-cent bins and the number of workers in each bin. The hourly wage distributions are available for the United States as a whole and for different regions (South, Midwest, Northeast, and West), occupations (e.g., tipped workers versus nontipped workers for the restaurant and hotel industries; inside-plant workers versus office workers in laundries; and bus drivers, clerical employees, food servers, custodial employees, or maintenance employees in schools, etc.), gender, and type of area (metropolitan versus nonmetropolitan). One strength of the BLS data is to allow us to transparently study the evolution of the hourly wage distributions in each sector over time and investigate the heterogeneity in the impact of the 1967 reform across several dimensions, such as a more detailed sectoral breakdown than in the 1962–1967 CPS files.

For the purposes of this project, we digitized over 1,000 hourly wage distributions from every year available between 1961 to 1970. We built a database of hourly wage distributions for the industries covered in 1967 and for both durable and nondurable 1938 industries.[23]

2. *CPS Data.* The Census Bureau and the BLS have conducted the CPS—a monthly household survey—since the 1940s. However, public use files are only available for the years 1962 and onward. We use data from the March CPS, more precisely the Integrated Public Use Microdata Series (IPUMS) from 1962 to 1980.[24] IPUMS released the 1962–1967 files with a harmonized

21. For more details on the representativeness of the BLS Industry Wage reports and how the industries were selected, see Kanninen (1959).

22. In addition, the BLS collected information on weekly hours of work and supplementary wage practices, such as paid holidays and vacation, health insurance, and pension plans.

23. For a list of BLS reports we digitized for 1938 and 1967 industries, see Online Appendix Figure C1. Altogether, the reports we digitized cover over 80% of all BLS industry wage surveys published between 1961 and 1970.

24. Downloaded from https://cps.ipums.org/cps-action/samples, see Flood et al. (2018).

industry variable in 2009. Because incomes in the March CPS of year t refer to incomes earned in calendar year $t-1$, we can track annual earnings from 1961 onward (e.g., starting six years before the 1967 extension of the minimum wage). We study earnings through to 1980, two years after the full convergence of the minimum wage in agriculture to the federal minimum wage level.

One advantage of the CPS over the BLS tabulations is that it provides individual worker–level data, and information on education and race (not available in the BLS data). We harmonized industry classifications across years; our harmonized industry variable includes 23 different industries.[25] This is more detailed than the two-digit NAICS code but a bit coarser than the three-digit NAICS code. For instance, we are able to separate restaurants from the rest of the retail sector, but we cannot separate hotels and lodging places from laundries and other professional services due to data limitations in the 1962–1967 CPS. The BLS industry wage reports have hourly wage information for more detailed sectors.

There are three main limitations involved in using March CPS data to analyze the 1967 reform. First, we only directly observe annual earnings in the CPS files of the 1960s and early 1970s, not hourly wages.[26] In the CPS regressions shown below, our main outcome of interest will be annual wages, as we will control for the number of weeks worked and the number of hours worked in a week. As we show in the next section, the wage effects of the reform estimated using the CPS will turn out to be very consistent with the effect on hourly wages estimated using the BLS industry wage reports.

Second, pre-1968 CPS micro files have fewer observations than in later years,[27] which increases the level of noise compared to more recent years. There is a difference in employment counts between the 1960 decennial census data and the early CPS

25. We used the information contained in the original industry variable from 1962 to 1967 and in the industry variable created by IPUMS from 1968 onward that recodes industry information into the 1950 Census Bureau industrial classification system. For more information about the construction of the integrated industry codes in IPUMS starting in 1968, see http://usa.ipums.org/usa/chapter4/chapter4.shtml.

26. The CPS started to collect information on hourly and weekly earnings in 1973 in the May supplement of the survey. Starting in 1979, the earnings questions were asked each month for people in the outgoing rotation groups.

27. There are about 15,000 observations in our sample in March CPS 1962–1965, then around 30,000 through the mid-1970s (see Online Appendix Table B2).

files. However, conditioning on being employed, annual earnings in the March CPS and census are perfectly in line (see Figure I).[28] However, the employment shares by industry and race match the information contained in the census. Furthermore, we have checked that CPS employment is consistent in both levels and shares with the 1970 and 1980 censuses. The limitation of the CPS in the early 1960s does not affect our cross-industry or cross-state difference-in-differences point estimates, but it increases standard errors for the years 1962–1967.

Third, from 1968 to 1976, the IPUMS data report information by state groups as opposed to states. We have information for 21 state groups across all years. The states that were grouped together were small (e.g., large states such as California and New York are always one single state) and geographically close to each other (see Online Appendix Figure B2). We checked that the borders of the state groups do not cross region or division lines. Importantly, the states in each group have similar state minimum wage policies. Thus this data limitation is unlikely to be a threat to our cross-state empirical strategy. For simplicity, in our analysis using CPS data, we use the term "states" to refer to "state groups."

3. *U.S. Census Data.* We use the 1–100 national random sample of the population from the 1950, 1960, 1970, and 1980 decennial censuses to compute the share of workers covered by the FLSA of 1938 and its subsequent amendments.[29] We also use census data to show that the employment shares by industry, gender, and race in 1960 are consistent with the early CPS files (see Online Appendix Table B2).

4. *Minimum Wage Database.* We use the report of the Minimum Wage Study Commission published in 1981 (O'Hara 1981) to build our minimum wage database by state, gender, and

28. Online Appendix Table B2 shows that our estimated number of employed persons in March CPS 1962 and 1963 in our sample is lower (average of 23,181,837 over those two years) than the estimate we get in 1960 in census data (33,244,820). Starting in March 1964, the number of people employed is in line with Census data. The black-white and men-women employment shares, however, are similar in March CPS 1962 and 1963 and census 1960.

29. Census data were accessed from the IPUMS website at https://usa.ipums.org/usa-action/samples, with variables—in particular the industry variable—harmonized with the CPS files, see Ruggles et al. (2018).

industry.[30] We cross-check the information in the Minimum Wage Study Commission (O'Hara 1981) with the information contained in the Department of Labor Handbook on women workers published in 1965 (Wirtz 1965).[31] In 1965, 31 states and the District of Columbia had minimum wage laws (for more details on how the database was constructed, see Online Appendix A).

IV. The Wage Effects of the 1967 Reform

IV.A. *Identification Strategy, Sample, and Summary Statistics*

We start by studying the effect of the 1967 extension of the minimum wage on the dynamics of annual wages in the CPS, before studying the effect of the reform on hourly wages in the BLS data. In what follows, when we use the term wages in discussing results from the CPS, we refer to annual wages; when we use the term wages in discussing results from the BLS data, we refer to hourly wages.[32] Throughout the text, we use the term annual (hourly) earnings interchangeably with annual (hourly) wages. Our baseline empirical approach is a cross-industry difference-in-differences research design: we compare the dynamics of wages in the newly versus previously covered industries, before and after 1967. The identification assumption is that absent the 1967 reform, wages in the 1967 industries (treated) and in the 1938 industries (control) would have evolved similarly. We provide graphical evidence that wages in the two groups evolved in parallel before 1967, lending support to our identification assumption (see Figure V). As discussed below, our effects are robust to the inclusion of a wide range of controls and time-varying effects, making it unlikely that our effects are confounded by contemporaneous changes differentially affecting workers in the treated versus control industries.

30. The report was downloaded from https://cpb-us-e1.wpmucdn.com/blogs.rice.edu/dist/f/3154/files/2015/11/Minimum-Wage-Study-1983-Carter-Administration-1hkd1cv.pdf.

31. Accessible at https://fraser.stlouisfed.org/files/docs/publications/women/b0290_dolwb_1965.pdf.

32. The precise variable in the CPS, "INCWAGE," includes wage and salary income (see https://cps.ipums.org/cps-action/variables/INCWAGE#description_section). Because we are focused on workers from the lower part of the earnings distribution where income most likely comes from wages, because our baseline specification controls for hours worked last week (in Section V), and we show no systematic selection on this margin, we believe the term "annual wages" best describes our primary earnings outcome in the CPS.

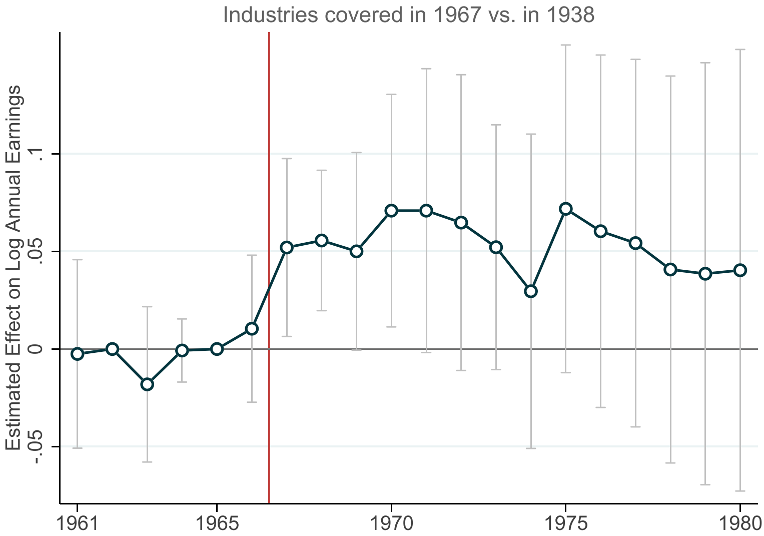


FIGURE V

Impact of the 1967 Reform on Annual Earnings

March CPS 1962–1981. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. This regression uses a cross-industry design and controls for gender, race, years of schooling, a cubic in experience, full-time/part-time status, number of weeks and hours worked, occupation, and marital status. Includes industry and time fixed effects. The year 1962 is excluded and set to zero. Standard errors are clustered at the industry level. Annual earnings are in $2017, deflated using annual CPI-U-RS series.

Our sample includes all prime-age workers, that is, aged 25 to 55. Workers younger than 21 were subject to a different, lower minimum wage that is not the focus of our study. Workers younger than 25 may have been of draft age (aged 18 to 25).[33] We also exclude the self-employed, workers in group quarters, unpaid family workers, and individuals working less than 13 weeks a year and less than three hours a week (to remove noise generated by very low annual earnings). Throughout the analysis, control industries include all industries that were covered in 1938 (that is,

33. The inclusion of men aged 18–25 might in particular lead to negative biases in the overall employment results if enrollment during the Vietnam War is contemporaneous with the implementation of the minimum wage reform and if enrollment rates are higher in states also strongly affected by the reform.

we exclude from the analysis the industries added in 1961, 1974, and 1986, which together employed about 25% of the workforce, see Online Appendix Table B3). As shown by Table I, our results are not sensitive to the inclusion of 1961 industries (i.e. construction and retail trade) in the control group. All wages are converted to 2017 dollars, using the CPI-U-RS price index from the BLS.

Table II presents summary statistics; the data are averaged over 1965 and 1966. On the eve of the 1967 extension of the minimum wage, workers in the 1967 industries (our treated group) were paid 30% less on average than workers in the 1938 industries (control). The difference in average annual earnings between black and white workers was the same in both groups of industries. Female workers were overrepresented in the industries covered in 1967, among both white and black workers.[34] In both the control and treated industries, black workers were less educated than white on average (around 40%–45% have more than 11 years of schooling versus 65%–75% for white workers). The distribution of white individuals across regions is the same in the treatment and control groups. Black workers were predominantly in the South, and those working in the treated industries were more concentrated in the South (56%) than those working in the control industries (44%). White and black workers were employed in different occupations. Finally, the majority of workers worked full-time, full-year. However, the share of workers that were full-time, full-year was higher in the treated industries (87% for white and 79% for black workers) than in the control industries (68% for white and 67% for black workers).

We estimate the following difference-in-differences model:

$$\log w_{ijst} = \alpha + \sum_{k=1961}^{1980} \beta_k \text{Covered } 1967_j \times \mathbb{1}[t = k] + \delta_j + \delta_t + \mathbb{X}'_{ijst}\Gamma + \varepsilon_{ijst}, \tag{1}$$

34. In this article, we focus on the contribution of the 1967 reform to the decline in the racial earnings gap. We choose not to focus on the gender earnings gap, despite the fact that women were overrepresented in the treated industries, for two reasons. First, there is no sharp decline in the gender earnings gap in the late 1960s and early 1970s. The gender annual and weekly earnings gap begins declining sharply in the 1980s after a long period of stability (Blau and Kahn 2017). Second, we find no evidence of heterogeneity in the effect of the reform by gender. One reason the reform may not have generated a reduction in the gender earnings gap is because of the large increases in female labor force participation over this period. An increase in the relative supply of women may have counterbalanced increases in their relative wage.

TABLE I

WAGE EFFECT: MAIN RESULTS AND ROBUSTNESS CHECKS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Covered in 1967 × 1967–1972	0.065** (0.025)	0.060** (0.024)	0.061** (0.025)	0.056** (0.022)	0.065** (0.023)	0.058** (0.025)	0.063** (0.023)	0.065** (0.025)
Observations	407,823	407,823	407,823	401,171	375,393	490,183	407,823	407,823
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y
State linear trends	N	Y	N	N	N	N	N	N
State-by-year FE	N	N	Y	N	N	N	N	N
W/o agriculture	N	N	N	Y	N	N	N	N
Full-time only	N	N	N	N	Y	N	N	N
1961 ind. in control group	N	N	N	N	N	Y	N	N
Winsorized data	N	N	N	N	N	N	Y	N
Two-way clusters	N	N	N	N	N	N	N	Y

Source: March CPS 1962–1981.

Notes. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

The outcome variable is log annual earnings (in \$2017, deflated using annual CPI-U-RS). Individual-level controls are gender, race, years of schooling, a cubic in experience, full-time/part-time status, no. of weeks and hours worked, occupation, and marital status. In column (6), we include retail trade and construction in the control group, two industries that started to be covered by the 1961 FLSA (see Online Appendix A). In column (7), log annual earnings and individual-level controls are winsorized at the 5% level. In columns (1)–(7), standard errors are clustered at the industry level. In column (8), standard errors are clustered at the industry and state levels. ** $p < .05$.

TABLE II

WORKERS' CHARACTERISTICS, 1965–66

	Control group		Treatment group	
	White	Black	White	Black
Annual earnings (in $2017)	45,809	28,870	32,848	20,854
Age	39.8	38.8	39.9	39.0
Gender				
Male	0.76	0.80	0.43	0.39
Female	0.24	0.20	0.57	0.61
Education				
11 years of schooling or less	0.38	0.64	0.26	0.51
More than 11 years of schooling	0.62	0.35	0.74	0.48
Marital status				
Married	0.86	0.77	0.77	0.65
Single	0.13	0.15	0.22	0.22
Region				
North Central	0.29	0.26	0.28	0.18
North East	0.30	0.23	0.26	0.17
South	0.26	0.44	0.26	0.56
West	0.15	0.08	0.20	0.08
Occupation				
Operatives	0.33	0.52	0.04	0.12
Craftsmen	0.20	0.12	0.03	0.01
Clerical and kindred	0.16	0.07	0.14	0.06
Managers, officials, and proprietors	0.11	0.01	0.06	0.01
Professional and technical	0.10	0.02	0.42	0.21
Sales worker	0.05	0.00	0.00	0.00
Service worker	0.01	0.08	0.30	0.56
Other	0.03	0.17	0.01	0.02
Full-time/part-time status				
Full-time, full-year	0.87	0.79	0.68	0.67
Part-time	0.13	0.21	0.32	0.33

Source: March CPS 1966–67.
Notes. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. Because the CPS collects information on earnings received during the previous calendar year, annual earnings reported in this table were earned in 1965–66. Annual earnings are in $2017, deflated using annual CPI-U-RS series. The other demographic characteristics were collected in 1966–1967.

where $\log w_{ijst}$ denotes the log annual earnings of worker i in industry j, state s, in year t.[35] The dummy variable Covered 1967_j equals 1 if worker i works in an industry covered in 1967 and 0 if they work in an industry covered in 1938. t is the year the reform

35. Year t corresponds to the calendar year during which income was earned, that is, 1961 in CPS 1962, 1962 in CPS 1963, and so on.

was implemented (1967), and δ_j and δ_t are industry and year fixed effects, respectively. The coefficient of interest, β_k, measures the effect of the 1967 reform *k* years after the baseline year (1965 in what follows). In all our analyses, we control for the following worker-level characteristics contained in the vector $\mathbb{X}_{ijst}$: gender, race, experience, experience squared and cubed, number of years of schooling, occupation, marital status, and part-time or full-time status. We also control for the number of weeks worked and the number of hours worked.[36] In Section V, we show that the reform did not affect the number of hours worked per year conditional on working (see Figure VIII, Panel A and Online Appendix Table E4).[37] More generally, adding individual-level controls doesn't affect our results suggesting that sorting on observables is not part of the response to the 1967 reform, at least in the medium run (see Online Appendix Figure D1 showing the wage effect with all controls, all controls except number of weeks and hours worked, and no controls). Adding them increases the precision of our estimates, however.[38] We report standard errors clustered at the industry level to allow for arbitrary dependence of ε_{ijst} across year *t* within industry *j*. We view clustering here mainly as an experimental design issue where the assignment is correlated within the clusters (see Abadie et al. 2017). This is why we cluster by industry in our main specification and not by other dimensions across which there may be unobserved heterogeneity within clusters. The clustering is at the industry rather than at the industry-year level to account for serial correlation across years (Bertrand, Duflo, and Mullainathan 2004).

36. The CPS contains information on the number of weeks worked last year, by categories: 1–13 weeks, 14–26 weeks, 27–39 weeks, 40–47 weeks, 48–49 weeks, and 50–52 weeks. The CPS contains information on the number of hours worked last week.

37. The annual number of hours worked is constructed as the product of the number of hours worked a week and the number of weeks worked a year. Because the number of weeks worked is only available by intervals, we multiplied the number of hours worked per week by the midpoint of each weeks-worked interval, and smoothed this measure by adding or subtracting to it a random number generated from a uniform distribution.

38. Adding or not adding individual-level controls has no effect on our medium-run point estimates as shown in Online Appendix Figure D1. Starting in 1971, the point estimates with all the individual-level controls are slightly higher than the point estimates in our baseline specification. One possibility is that the extension of the minimum wage has a positive effect on the number of years of schooling in the medium and long run.

IV.B. Baseline Estimates of the Effect of the 1967 Reform on Wages

Figure V shows the effect of the 1967 reform on the log annual earnings of treated workers relative to control workers. Before the implementation of the reform in February 1967, the annual earnings of workers in the treated versus control industries evolved in parallel: the point estimates for 1961–1966 are centered around 0 and are not statistically different from 0.

Starting in 1967, annual earnings increased substantially—by about 5%—for workers in the newly covered industries relative to workers in the control industries. Relative wages continued to increase after 1967 through to 1971 when the treatment effect peaks (+6.7%). This pattern of increase is consistent with the fact that in the newly covered industries, the minimum wage was first introduced in 1967 at a level ($1 in nominal terms) below the prevailing federal minimum wage ($1.25), before gradually converging to the level of the federal minimum wage over the 1967–1971 period (except in agriculture); see Figure III. After 1971, the point estimates stabilize and the wage increase persists over time. Overall, the average wage of workers in the newly covered industries is 6.5 log points (i.e., 6.7%) higher relative to the average wage of workers in control industries in 1967–1972 compared with the preperiod 1961–1966; see Table I, column (1). These effects are statistically different from zero at the 5% level.

1. *Actual versus Predicted Effects.* The magnitude of the wage estimates are consistent with the predicted wage increase obtained from assigning the 1967 minimum wage to workers in the treated industries who were below the 1967 minimum wage in 1966. We compare the actual effects of the reform to the predicted effects of the reform under the following three assumptions: first, there is perfect compliance with the reform; second, there is no employment effect; and finally, there are spillovers up to 115% of the 1967 minimum wage.[39]

We start from the distribution of hourly wages in the 1966 CPS (constructed using the information available on annual earnings, the number of weeks worked, and the number of hours worked; see note 37). From there, we estimate that 16% of

39. We tested alternative assumptions on spillover effects and found small quantitative effects. The average predicted wage increase is 5.4%, 4.9%, and 6.0% for spillover effects up to 115%, 120%, and 110%, respectively.

TABLE III
PREDICTED WAGE EFFECT

	Share of workers at or below the MW (%) (1)	Avg increase in earnings for MW workers (%) (2)	Predicted increase in earnings (%) (3) = (1) × (2)	Estimated increase in earnings (%) (4)
All	16.1	33.5	5.4	5.3
By education				
Low education	31.1	32.7	10.2	10.1
High education	9.6	34.2	3.3	2.5
By race				
Black	28.8	38.2	11.0	8.0
White	13.9	32.0	4.5	4.3

Source: March CPS 1962–1981.
Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: Minimum wage workers = those at or below 1967 min. wage. Estimates in columns (3) and (4) for 1967 only.

workers in the treated industries were below the 1967 minimum wage in 1966; see column (1) in Table III). For these workers, the average increase resulting from moving straight to the $1 nominal minimal wage introduced in 1967 is 34%; see column (2). The predicted wage effect in 1967 for all workers in the treated industries is 16% × 34% = 5.4%; see column (3). This is close to the estimated effect of 5.3% found in our wage regression in 1967.[40] The predicted wage effect is slightly larger than the observed effect (5.4% versus 5.3%). This could be due to several factors. There is measurement error in hourly wages, there may be imperfect compliance with the reform, and there may be effects of the reform on employment. We explore the latter in Section V.

2. *Effects by Education.* The wage effect shows up primarily where one would expect to see it, that is, for workers with low levels of education. We separately estimate the above model for workers with 11 years of schooling or less versus those with more than 11 years of schooling; see Figure VI, Panel A.[41] For workers with low levels of education, wages increased by 10.1% in 1967

40. Because we make predictions for 1967 alone, we compare the predicted effects to our wage coefficient obtained for 1967 alone (see Figure V rather than to the pooled estimate for 1967–1972 presented in Table II).

41. There is a similar pattern among black and white workers (see Online Appendix Figures D4a and D4b).

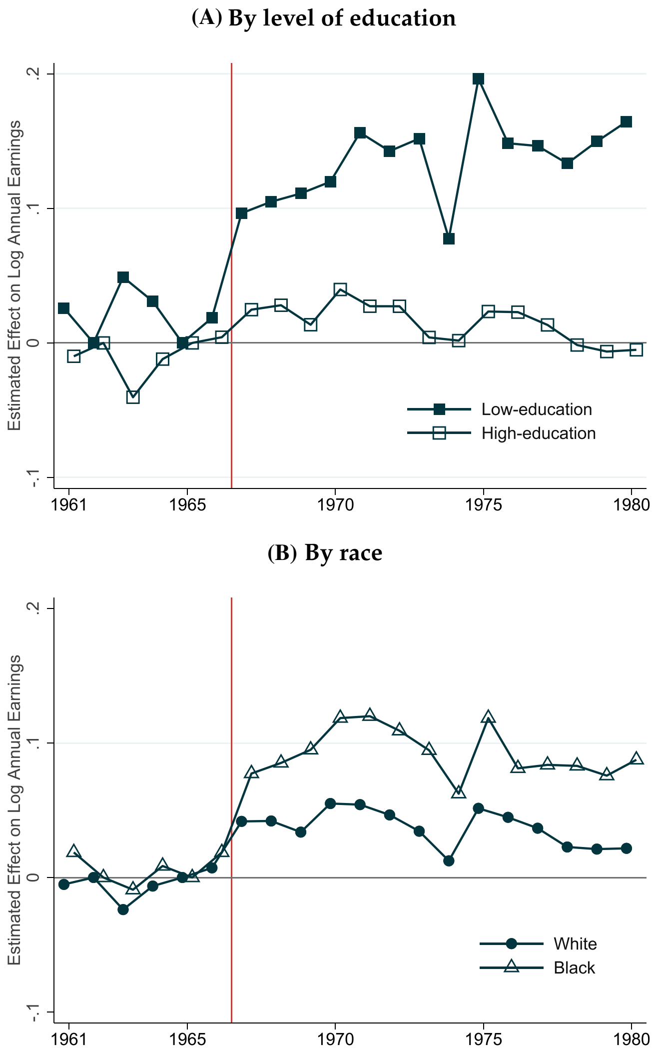


FIGURE VI

Heterogeneity in the Wage Effect of the 1967 Reform

March CPS 1962–1981. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. These regressions use a cross-industry design and control for gender, race (Panel A only), years of schooling, experience, quadratic and cubic in age, full-time/part-time status, number of weeks and hours worked, occupation, and marital status. Includes industry and time fixed effects. Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling. The year 1962 is excluded and set to zero. Standard errors are clustered at the industry level. Annual earnings are in \$2017, deflated using annual CPI-U-RS series.

in the newly covered industries, above and beyond wage growth in the previously covered industries. The effect is much smaller (2.5% in 1967) among highly educated workers. These results are consistent with the idea that our empirical design captures the effect of the extension of the minimum wage in 1967 and not a general trend affecting all workers (e.g., including the highly skilled) in the 1967 industries. These estimated effects are well in line with our predictions, as shown in Table III.

3. *Effects by Quartiles.* As expected, the wage effect is concentrated in the lowest quartile of the 1966 distribution (+7.0%). This is true whether we look at all workers, at white workers only, or at black workers only. We report these results in Online Appendix Table D1.

4. *Wage Effects Using Hourly Wage BLS Data.* We confirm our wage results using the BLS industry wage reports instead of the CPS data. We implement the same cross-industry difference-in-differences research design: we compare the dynamics of wages in the newly versus previously covered industries, before and after 1967. Control industries here include manufacturing industries (see Online Appendix Figure C1 for the list of industries we digitized and years available), which were covered by the minimum wage in 1938.[42] We adapt our cross-industry design to the nature of the BLS data and estimate two models: (i) a similar difference-in-differences model as described in equation (1); and (ii) a triple difference-in-differences model defined as follows:

$$\begin{aligned} y_{jrt} = \alpha &+ \beta_1 \text{Covered 1967}_j \times \text{Post}_t \times \text{South}_r \\ &+ \beta_2 \text{Covered 1967}_j \times \text{Post}_t + \beta_3 \text{Post}_t \times \text{South}_r \\ &+ \beta_4 \text{Covered 1967}_j \times \text{South}_r + \nu_j + \eta_r + \lambda_t + \varepsilon_{jrt}, \end{aligned} \tag{2}$$

42. We included all reports published between 1961 and 1970 for industries covered in 1938 and in 1967 whose reports met the following criteria: the report contained hourly earnings data, a pre- and postreform report for that industry was available; and occupational, gender, and geographic categories could be harmonized for that industry across years. Eighty percent of the industry reports published between 1961 and 1970 met these criteria. We added to this sample movie theaters and schools, two newly covered industries with reports only in the pre- or postperiod. Results are robust to excluding these industries and years where only newly covered or previously covered industries' reports were available.

TABLE IV
HOURLY WAGE EFFECT USING BLS DATA

	Cross-industry DinD		Cross-industry triple DinD	
	Full sample	Strict sample	Full sample	Strict sample
Covered in 1967 ×				
1967–1969	0.083***	0.117***	0.066**	0.098***
	(0.025)	(0.032)	(0.025)	(0.034)
1967–1969 × South			0.075***	0.081**
			(0.018)	(0.040)
Observations	337	194	337	194
Time FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Region FE	Y	Y	Y	Y

Source: BLS Industry Wage Reports. See Online Appendix Figure C1 for the set of tabulations digitized.
Notes. Sample: All nonsupervisory employees. The "full" sample contains industries listed in Online Appendix Figure C1. The "strict" sample excludes movie theaters and schools (only available pre- or postreform) as well as years 1961–1962, 1964, and 1970 where only treatment or control industries are available. Standard errors are clustered at the industry level. *** $p < .01$; ** $p < .05$.

where y_{jrt} denotes log hourly wages in industry j, region r, and year t; Covered 1967_j indicates whether an industry was covered in 1967; ν_j, η_r, and λ_t are industry, region, and year fixed effects. Our standard errors are clustered at the industry level. In addition, $\hat{\beta}_4$ in this specification allows us to investigate whether the wage effects are larger in the South—where black workers were concentrated. This regression is run on two samples: a strict sample that only includes industries with both pre- and postreform data and years with both control and treatment industries and a full sample including all our digitized data.

Table IV shows that in the difference-in-differences model, wages in the newly covered industries jump by 8.6% (8.3 log points) relative to wages in nondurable manufacturing after the reform (1967–1969) relative to before (see columns (1) and (2)). This magnitude is slightly higher than the 6.7% wage increase estimated using CPS data. This small difference in the magnitude could be due to differences in the measure of the outcome (hourly wages in the BLS versus annual wages in the CPS), in the sample (BLS data are focused on nonsupervisory workers, a lower-skilled subgroup of workers than workers overall), differences in the set of industries compared in the control and the treatment groups, or differences in the time period.[43]

43. We note that in the triple difference-in-differences model, the wage increase is higher for treated industries in the South relative to all previously covered

IV.C. Robustness Tests and Other Estimation Strategies

The main threat to our baseline identification strategy is shocks happening in 1967 that differentially affect workers in treated versus control industries. In what follows we present a number of checks and tests for the wage effects we estimate. We first consider two types of shocks—state shocks and sectoral shocks—before considering additional checks and studying alternative research designs.

1. *Robustness to State Linear Trends and State Shocks.* If treated industries were concentrated in the South, for example, then convergence in wages between workers in the South and in the North could explain some of our wage effect. To address this concern, in column (2) of Table I, we add state linear trends to the controls of our baseline model. Table I, column (3) includes controls for state-specific shocks to address any state-specific policy changes during this period. The inclusion of these controls does not change the magnitude or the pattern of the estimated wage effect. This suggests regional wage convergence or state-specific shocks are unlikely to bias our estimates.

2. *Robustness to Sectoral Shocks.* One might be concerned about shocks happening in certain treated industries, such as agriculture (e.g., mechanization). In Table I, column (4), we exclude agriculture from our sample to see whether the results still hold. We find that the magnitude of the wage effect (5.8%) is only a bit lower than when agriculture is included (6.7%). One interpretation is that there is some heterogeneity in the wage response across industries. This interpretation would be consistent with the fact that the bite of the minimum wage is higher in agriculture than in the other newly covered sectors.

3. *Additional Robustness Tests.* We report the following additional robustness tests. First, we vary the sample selection criteria. In Table I, column (5), we restrict the sample to full-time workers only. The point estimate (6.5 log points) is similar to the baseline estimate reported in column (1). This result suggests that

industries in the non-South (+7.8% in the full sample, see column (3); +8.4% in the strict sample, see column (4)). Although we do not observe wage distributions separately by race in the BLS data, these results are consistent with larger effects on black workers who made up a large share of the Southern workforce.

the 1967 reform did not affect full-time and part-time workers differentially. In column (7), we winsorize the top and the bottom of the distribution of the outcome and the control variables at the 5% level; the point estimate remains unchanged (6.3 log points). This result shows that outliers (in particular at the bottom of the distribution) do not drive our results. In column (8), we test whether the precision of our results is robust to alternative ways of clustering standard errors. Because the intensity of the treatment varies by state and as there is reason to believe that unobserved components of the annual wage for workers are correlated within states, we implement two-way clustering (at the industry and state levels). The precision of our results is unchanged.[44] Finally, following Cameron, Gelbach, and Miller (2008) we implement a wild bootstrap approach to clustering standard errors, as we have a small number of clusters whether by industry (16) or state (21). Wild bootstrap slightly improves the precision of our estimates.

More generally, one might be concerned that following the 1967 minimum wage coverage extension, workers in the control industries were willing to work in the newly treated industries and switch jobs. We do not believe that this sorting effect could have been substantial for two reasons, one theoretical and the other empirical. First, as mentioned already, the extension of minimum wage coverage was gradual, and wages in the treated industries were much lower than in the control industries on average; the wage compensating differentials between the two types of industries would have to have been very large to be consistent with consequential sorting effects. Second, we do not find evidence of large reallocations of workers from the control to the treated industries in the years following the 1967 reform (see Online Appendix Figure B3a).

One might also be concerned that the 1967 extension of the minimum wage led to spillover increases in wages in the control industries. We plot a version of Figure V in levels with no controls (see Online Appendix Figure D2). We show that there is wage growth in both types of industries before the reform and that the mean log annual earnings evolve in parallel in the years

44. Along with the fact that the standard errors are much lower when the clustering is implemented at the state level rather than at the industry level, this result indicates that the correlation in the unobserved components of workers' wages within industries is higher than the correlation in the unobserved components of workers' wages within states.

TABLE V
WAGE EFFECT BY RACE

Model	Baseline		Robustness checks			
	(1)		(2)		(3)	
	Black	White	Black	White	Black	White
Covered in 1967 ×						
1967–1972	0.095***	0.054**	0.074***	0.051**	0.074**	0.048**
	(0.022)	(0.023)	(0.024)	(0.023)	(0.030)	(0.022)
1973–1980	0.078*	0.036	0.049	0.033	0.043	0.035
	(0.037)	(0.042)	(0.039)	(0.041)	(0.043)	(0.041)
Observations	37,770	370,053	37,770	370,053	36,895	370,053
Controls	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
State FE	N	N	Y	Y	N	N
State-by-year FE	N	N	N	N	Y	Y

Source: March CPS 1962–1981.
Notes. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. The outcome variable is log annual earnings (in \$2017, deflated using annual CPI-U-RS). Individual-level controls are gender, years of schooling, a cubic in experience, full-time/part-time status, number of weeks and hours worked, occupation, and marital status. Standard errors are clustered at the industry level. *** $p < .01$; ** $p < .05$; * $p < .1$.

leading up to the reform. In 1967, there is wage growth in the treated industries above and beyond wage growth in the control industries. We therefore do not find evidence of large spillover increases in wages in the control industries for black and white workers taken all together. We discuss in the next subsection why such spillover increases might be higher among black workers.

IV.D. Wage Effects by Race

We now turn to our second key finding: the magnitude of the wage response to the 1967 reform was much larger for black workers (10%) than for white workers (5.5%).

To establish this fact, we run the same regression as in our benchmark cross-industry design, but for white and black workers separately (see Table V, columns (1) and (2)). That is, we compare white workers in the treated industries to white workers in the control industries, before versus after 1967 (see Figure VI, Panel B). Similarly, we compare black workers in the treated industries to black workers in the control industries (see Figure VI, Panel B), controlling for observables as in our benchmark specification. Strikingly, black workers in the treated industries saw their wages rise 10% more than black workers in the control industries starting in 1967. Because the wages of black workers in the control industries were themselves rising faster than the wages of white workers in the control industries, the wage of black workers in the treated industries rose much faster (+20%) than average (black plus white) wages in the control industries (see Online Appendix Figure D3). This effect on black workers, in addition to the precise timing of the change in wages, provides additional support that we pick up the effects of the 1967 reform on the racial wage gap as opposed to, for example, the effects of the 1964 Civil Rights Act. One might be concerned that the wage effect which we find among black workers still reflects civil rights era antidiscrimination policies that primarily affected Southern states with a large black population. We have checked that the wage response is robust to the inclusion of state-by-year fixed effects (see Table V columns (5) and (6)), which control for any state-specific shocks occurring over this period.[45]

Finally, we note that the magnitude of the wage response measured in 1967 using the cross-industry design is broadly consistent with our predicted wage effects by race (see Table III). The estimated wage effect among black workers (+8%), however, is somewhat smaller than the predicted one (+11%). There are several potential reasons for this. In particular, it is possible that the 1967 extension of the minimum wage led to spillover increases in wages in the control industries (as black workers were concentrated in the South where treated industries were also concentrated). If this is the case, and if such general equilibrium effects are present, they are not captured in our cross-industry design (Nakamura and Steinsson 2018). In this case, our estimated wage increase for black workers is biased downward, which could explain why it is smaller than the predicted wage effect for this group.

45. Although we isolate the effect of the 1967 minimum wage reform, we believe this reform and the Civil Rights Act acted in a complementary manner to reduce racial inequality over this time period. The Civil Rights Act sought to eliminate discrimination in hiring and promotion of black workers into jobs they were barred from in segregated firms. Meanwhile, the 1967 minimum wage lifted wages in exactly those jobs. Given that the concentration of black workers in low-wage jobs persisted (see Online Appendix Table E10)—whether due to imperfect CRA enforcement or continuing education inequality—the minimum wage appears to have been an important additional force reducing racial inequality over this time period.

IV.E. Wage Effect in a Cross-State Research Design

Our final analysis of wage effects of the reform considers an alternative research design that leverages geographic variation in the bite of the reform. We use this as our baseline design to estimate the employment effects of the 1967 reform in the following section. This is because this approach does not require knowing the industry of an unemployed worker, which is unobserved in repeated cross-sections of the CPS.[46] In this alternative design, we leverage the fact that just like today, many states had their own minimum wage law in the 1960s, thus already covering the industries that became covered by the federal law in 1967. We compare workers in states that already had a minimum wage law before the reform (weakly treated) to workers in states that did not (strongly treated). Figure VII shows that states with no minimum wage law as of 1966 were concentrated in the South, but not exclusively; they are also present in the Midwest. Our identification assumption is that absent the 1967 reform, wages in the weakly and strongly treated states would have followed the same trend. We estimate the following difference-in-differences model, pooling together our estimates over each period k, with $k \in \{[1961\text{–}1966], [1967\text{–}1972], [1973\text{–}1980]\}$:

$$\log w_{ist} = \alpha + \sum_k \beta_k \text{ Strongly treated state}_s \times \delta_{t+k} + \mathbb{X}'_{ist}\Gamma + \nu_s + \delta_k + \varepsilon_{ist}, \tag{3}$$

where Strongly treated state$_s$ is an indicator for a state with no minimum wage law as of January 1966. The coefficient of interest, β_k, measures the effect of the 1967 extension of the federal minimum wage k years after or before the year chosen as a baseline (1965 in this case). We control for the same workers' characteristics as in our cross-industry design. Standard errors are clustered at the state level. We find that wages in the strongly treated states grew on average by 4.1% more than in weakly treated states just after the reform and over the period 1967–1972

46. We do show that our employment effects are robust to aggregating our results to the state-industry level and using cross-industry variation in coverage to estimate the employment effects of the reform (see Online Appendix E.1). Still, we lose statistical power collapsing the CPS data in this way. We therefore use geographic variation in bite and our bunching analysis in the BLS data to provide our primary evidence on employment.

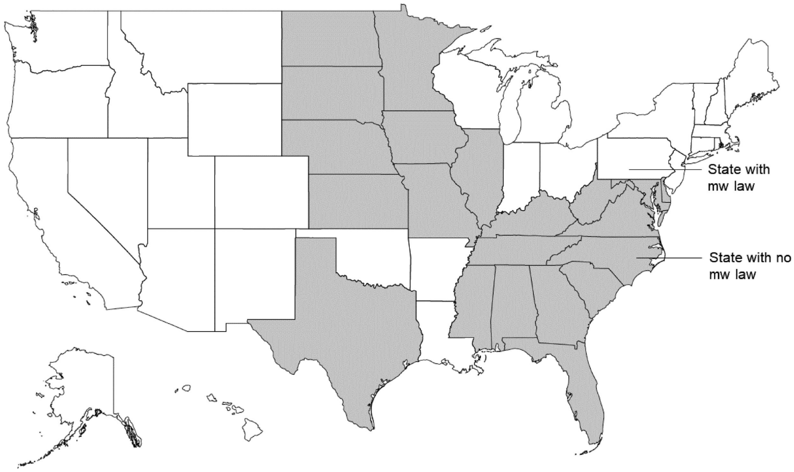


FIGURE VII

States with No Minimum Wage Laws as of January 1966

Authors' minimum wage database 1950–2016. More details provided in Online Appendices A and E.2. The strongly treated state groups are Florida, Illinois, Texas, Alabama-Mississippi, North Carolina–South Carolina–Georgia, Kentucky–Tennessee, Iowa–North Dakota–South Dakota–Nebraska–Kansas–Minnesota–Missouri, Delaware–Maryland–Virginia–West Virginia, Arkansas-Louisiana-Oklahoma.

(see Online Appendix Table E2). As in our cross-industry design, the effect is concentrated on workers with low levels of education.

V. The Employment Effects of the 1967 Reform

We analyze the employment effects of the reform in two stages. First, we follow the exact form of the wage analysis above to estimate the effects of the 1967 reform on employment using geographic variation in preexisting state minimum wage laws. This source of variation captures both extension of coverage to new industries and increases in the national federal minimum wage. To understand the employment effects of coverage extension specifically, we implement a bunching estimator with our newly digitized BLS industry wage reports, comparing employment in the newly covered sectors in specific wage bins (separately by region) to that under a counterfactual distribution with no minimum wage.

V.A. Employment Effects in the CPS

Using the same cross-state design as implemented for wages in Section IV.E, we compare employment outcomes in states that had no minimum wage law as of January 1966 (strongly treated) versus states that did (weakly treated). We provide graphical evidence that employment outcomes evolved in parallel in strongly versus weakly treated states before the reform.

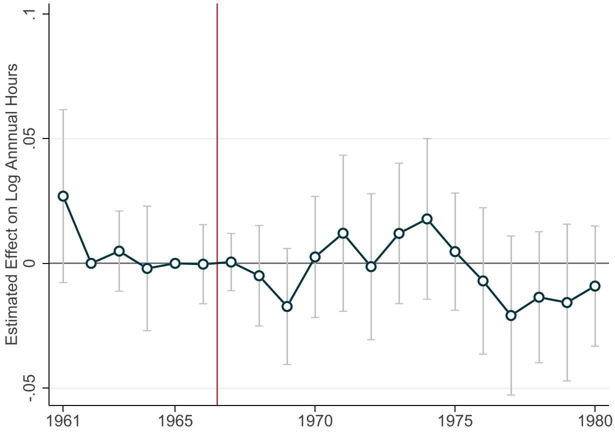
1. *Intensive Margin.* Starting with the reform's effect on annual hours worked, we estimate a difference-in-differences model similar to that in Section IV.C, except that the outcome is log annual hours.[47] Figure VIII, Panel A shows that before 1967 annual hours evolved similarly in strongly versus weakly treated states. There is no detectable change following the reform, neither for white nor for black workers (see Online Appendix Table E4). We can rule out annual hours elasticities with respect to the average wage lower than −0.16 for all workers (−0.21 for black workers) over 1967–1972.[48]

2. *Extensive Margin.* Next we investigate the reform's impact on the probability of being employed versus unemployed. As shown in Table VI, the reform does not appear to affect the probability of being employed versus being unemployed in 1967–1972, with a zero point estimate for the difference-in-differences coefficient of interest. The effect is precisely estimated. We are able to rule out a reduction in employment probability of more than 0.5 percentage points. Because average earnings in strongly treated states grew by 4.0% above and beyond earnings growth in weakly treated states, the lower-bound employment elasticity with respect to earnings is −0.16 at the 95% confidence interval. As shown by Online Appendix Figure E5, this estimate is in the range of elasticities found in the minimum wage literature. The point estimate on the probability of being employed versus unemployed or not in the labor force—an outcome that captures potential effects of the reform on labor force participation—is slightly positive, although not statistically different from 0. Using this metric, the lower-bound employment elasticity is very similar, at −0.25.

47. The number of annual hours worked is not directly available in the March CPS. We imputed the number of annual hours worked by multiplying the number of weeks worked a year (only available by intervals) and the number of hours worked a week. See also note 37.

48. The number of hours worked in the strongly treated states declined over 1973–1980, but the estimates are not statistically different from zero.

(A) Impact on annual number of hours worked (intensive margin)



(B) Impact on probability of being employed (vs. unemployed) (extensive margin)

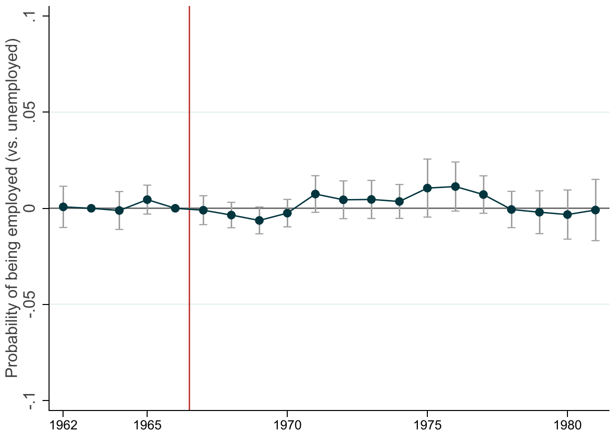


FIGURE VIII

Impact of the 1966 FLSA on Employment

March CPS 1962–1981. Sample: Panel A: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. Panel B: Adults 25–55, black or white, employed or unemployed. Panel A regression uses a cross-industry design and controls for gender, race, years of schooling, a cubic in experience, occupation, and marital status. Panel B regression uses a cross-state design and controls for years of schooling, a quadratic in age, and marital status. Includes industry (Panel A) or state (Panel B) and time fixed effects. The year 1962 is excluded and set to zero. Standard errors are clustered at the industry level (Panel A) or state level (Panel B).

3. *Heterogeneity by Race.* We estimate the model for black and white individuals separately. The results show no significant disemployment effects for either group. As reported in Table VI, because average wages increased 13.1% (12 log points) for black workers in strongly treated versus weakly treated states, the lower-bound employment elasticity is −0.24 for black persons in this setting—again well in the range of elasticities found in the literature (Online Appendix Figure E5). Results are similar when looking at the probability of being employed versus unemployed or not in the labor force (see Online Appendix Table E4, where we can rule out employment elasticities of more than −0.17 percentage points among black persons). Because the 1967 reform had large positive effects on wages but small employment effects (with lower bounds only slightly negative), it reduced not only the racial earnings gap (i.e., the difference in earnings between employed individuals) but also the racial income gap (i.e., including nonworkers).[49]

We also show in Online Appendix Table E4 that the employment elasticity (when the employment outcome is defined as the probability of being employed versus unemployed) is not statistically significant from zero for a number of other subgroups (men and women, low-education and high-education workers, and by cohort). We note that the employment elasticity is slightly positive for low-education workers when the employment outcome is defined as the probability of being employed versus unemployed or not in the labor force, suggesting possible positive effects of the minimum wage reform on labor force participation in this group.

4. *Heterogeneity in Employment Effects by Initial Labor Market Tightness and Geography.* Our small unemployment effects suggest low labor demand elasticity.[50] We examine heterogeneity in these effects using variation in initial labor market tightness and across regions with differing bite. Online Appendix E.3 reports these results. Overall unemployment effects and those for white individuals do not differ across states with different unemployment rates prereform. For black workers, however, we do observe greater disemployment in labor markets with above

49. We formally show this result in Online Appendix Table D2 by presenting statistically significant positive estimates of the impact of the 1967 reform on earnings unconditional on working. We refer to Online Appendix D for a detailed discussion of these results.

50. We discuss the conceptual implications of our results in Section VI.C.

TABLE VI

MAIN EFFECTS OF 1966 FLSA ON EMPLOYMENT AND ROBUSTNESS CHECKS USING CROSS-STATE DESIGNS

	Baseline cross-state design Strongly vs. weakly treated states			Alternative design 1 Kaitz index			Alternative design 2 Fraction of affected workers		
	All	Black	White	All	Black	White	All	Black	White
Treatment var. × 1967–1972									
Emp. (vs. unemp)	−0.001	−0.012	−0.001	−0.001	−0.008*	−0.000	−0.001	−0.010**	0.000
	(0.002)	(0.009)	(0.002)	(0.001)	(0.004)	(0.001)	(0.001)	(0.004)	(0.001)
Emp. (vs. unemp/nilf)	0.002	0.007	0.003	0.001	−0.003	0.002	0.001	−0.004	0.003
	(0.004)	(0.011)	(0.005)	(0.002)	(0.005)	(0.002)	(0.002)	(0.005)	(0.002)
Earnings	0.040***	0.123***	0.025***	0.014***	0.051***	0.006	0.022***	0.064***	0.012***
	(0.010)	(0.025)	(0.008)	(0.005)	(0.013)	(0.004)	(0.004)	(0.012)	(0.004)
Observations	534,977	51,666	483,311	534,977	51,666	483,311	534,977	51,666	483,311
Emp. (vs. unemp) elast.	−0.03	−0.10	−0.04	−0.09	−0.16**	−0.09	−0.03	−0.17**	0.01
Std. err.	(0.06)	(0.07)	(0.10)	(0.07)	(0.08)	(0.19)	(0.05)	(0.06)	(0.10)
Lower bound	−0.16	−0.24	−0.24	−0.24	−0.31	−0.47	−0.13	−0.28	−0.19
Upper bound	0.09	0.04	0.16	0.06	−0.01	0.29	0.08	−0.06	0.21
Emp. (vs. unemp/nilf) elast.	0.06	0.09	0.15	0.09	−0.09	0.44	0.06	−0.08	0.30
Std. err.	(0.16)	(0.13)	(0.26)	(0.23)	(0.14)	(0.59)	(0.16)	(0.11)	(0.30)
Lower bound	−0.25	−0.17	−0.37	−0.36	−0.37	−0.72	−0.24	−0.31	−0.29
Upper bound	0.38	0.34	0.66	0.54	0.19	1.61	0.37	0.14	0.88
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Source: CPS 1962–1981. *Notes.* Sample: For regressions on probability of being employed versus unemployed: adults 25–55, black or white, employed or unemployed. For regressions on log annual earnings: adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. The three treatment variables used are respectively: strongly treated state versus weakly treated state, the Kaitz index in 1966 at the state level, and the share of workers working below $1.60 in 1966. Further details are provided in Online Appendix E.2. The effect on employment and earnings using the two alternative designs is the effect of a one standard deviation increase in the treatment variable. For the design using the 1966 Kaitz index, the mean is 0.35 and the standard deviation is 0.048 in both the employment and the earnings samples. For the design using the fraction of affected workers, the mean is 0.17 and the standard deviation is 0.08 in both the employment and the earnings samples. Controls for employment regressions are gender, race, years of schooling, age, age squared, and marital status. Controls for earnings regression are gender, race, years of schooling, a cubic in experience, full-time/part-time status, number of weeks and hours worked, occupation, and marital status. Standard errors are clustered at the state level. $^{***}p < .01$; $^{**}p < .05$.

median initial unemployment. We find some evidence of greater disemployment for black workers in the South; however, these results are not robust across specifications.

5. *Robustness of Our Main Cross-State Design to Alternative Cross-State Designs.* Finally, we test whether our employment results using our baseline cross-state design are robust to alternative definitions of cross-state designs. Specifically, we develop two alternative cross-state designs for capturing variation in intensity of the treatment across states: (i) the state-level Kaitz index in 1966 and (ii) the fraction of affected workers in each state in 1966. The Kaitz index is a weighted minimum-to-median wage ratio that takes state-, demographic- and industry-specific minimum wages and the composition of the workforce into account. As a result, it captures both the pretreatment variation in state minimum wage laws by gender and industry, as well as variation in the sectoral composition of the workforce in each state.[51] The fraction of affected workers is defined as the number of workers with wages below $1.60 in 1966 in each state.[52]

We show the effect of an increase of one standard deviation in the treatment variable on annual wages and on the probability of being employed (versus unemployed) in Table VI. The pattern of the results we obtain with these two alternative cross-state designs is consistent with the results obtained from our main cross-state design: large, positive effects on earnings and small-to-negligible effects on employment.[53] We are able to rule out employment elasticities of more than −0.24 using the 1966 Kaitz

51. It is formally measured as: $\text{Kaitz Index}_{s1966} = \sum_j \frac{N_{sj1966}}{N_{s1966}} * \frac{\text{min.wage}_{sj1966}}{\text{median wage economy}_{1966}}$ with N_{sj1966} the number of workers working full-time and full-year in our sample by industry type j (i.e., either industries covered in 1938 or industries covered in 1967) in state s, N_{s1966} the number of workers working full-time, full-year in all industries in 1966 in state s, min.wage_{sj1966} the minimum wage law that applies at the state level in industry type j (i.e., taking into account all the differences in minimum wage legislation at the industry × state × gender level) in 1966, and median wage economy$_{1966}$ the economy-wide median wage for full-time, full-year workers in our sample. We provide the values of this state-level Kaitz index and the 1961–1980 evolution of the minimum-wage-to-median-ratio taking state minimum wage laws into account in Online Appendix E.2.

52. We follow Bailey, Di Nardo, and Stuart (2020), see their Table 1 p.28.

53. Dube (2019a, 27) offers the following heuristic for values of own-wage elasticities (OWEs): "While all categorizations are inherently arbitrary, we can

index measure, and more than −0.13 using the 1966 fraction of affected workers. Our results using the main cross-state design are also robust across racial groups: in particular, we are able to rule out employment elasticities of more than −0.31 for black persons using the 1966 Kaitz index measure and more than −0.28 using the fraction of affected workers (see Table VI).[54] Our results using the main cross-state design are also robust across gender groups and levels of education (see Online Appendix Tables E4, E5, E6).

V.B. Bunching Estimator

1. *Methodology.* To directly examine how introducing a minimum wage affected employment in the newly covered industries, we use the BLS industry wage reports. We follow recent developments in the literature that infer employment effects from changes in bunching in the affected part of the wage distribution (Cengiz et al. 2019; Harasztosi and Lindner 2019).

More precisely, we compare bunching in the observed 1967 wage distribution in treated industries to a counterfactual distribution absent the minimum wage reform. To construct the counterfactual distribution, we inflate nominal 1966 wages by the nominal 1966–1967 growth rate of per adult U.S. national income (+4.4%).[55] We then compute the number of workers employed below the minimum wage in the observed 1967 distribution and in the counterfactual 1967 distribution. The difference between these numbers is our estimate of the effect of the reform on subminimum wage employment, which we refer to as the "missing jobs" postreform. Following the notation of Cengiz et al. (2019), we denote the missing jobs postreform as $\Delta b = Emp^1[w < MW] - Emp^0[w < MW]$, where $Emp^1[w < MW]$ and $Emp^0[w < MW]$ represent the observed and counterfactual distributions, respectively.[56] We implement this procedure within each treated industry × region cell available in the data.

roughly think of an OWE less negative than −0.4 as small in magnitude, between −0.4 and −0.8 as medium, and more negative than −0.8 as large."

54. The respective elasticities using the probability of being employed versus unemployed or not in the labor force as the employment outcome are of similar magnitudes: −0.37 (Kaitz index) and −0.31 (fraction of affected workers).

55. For nursing homes, we use national income per capita growth from 1965 to 1967 (12.4%) to construct the counterfactual distribution of wages in 1967, as we only have data for 1965 and 1967.

56. We follow Harasztosi and Lindner (2019) and develop a counterfactual distribution based on national income growth. Our approach differs from

We repeat this procedure for jobs paying at or slightly above the minimum wage. In our baseline estimate, we assume that the part of the low-wage distribution affected by the minimum wage is the entire distribution up to 115% of the minimum wage, that is, up to $1.15 in 1967, consistent with spillover effects estimated in the literature (see, e.g., Dube, Giuliano, and Leonard 2019).[57] The difference in the number of jobs between the observed and counterfactual distributions is our estimate of the effect of the reform on employment at or slightly above the new minimum wage, which we refer to as the "excess jobs" postreform. We denote the excess jobs postreform as $\Delta a = Emp^1[MW \leqslant w < \bar{W}] - Emp^0[MW \leqslant w < \bar{W}]$.

We take the difference between excess and missing jobs as the total effect of the 1967 reform on low-wage employment: $\Delta e = \Delta b + \Delta a$. We normalize this difference by total 1966 employment (by treated industry $\times$ region) to estimate the percent change in the number of low-wage jobs. Taking Δe as the effect of the reform on employment, we calculate and report the following employment elasticity with respect to the average wage for each industry-by-region group and for all industries in the United States as a whole:

$$\text{(4)} \qquad \text{Employment elasticity w.r.t. the avg wage} = \frac{\Delta e}{\Delta w}.$$

The percent change in the average wage, Δw is defined as the difference between the observed and counterfactual average wage divided by the counterfactual average wage. To calculate the average wage in each industry-by-region group, we divide the total wage bill by the total number of workers in that group.[58]

Our identification assumption is that in the absence of the reform, wages would have evolved according to national income per capita growth between 1966 and 1967. We then attribute observed deviations from this counterfactual distribution to the causal impact of the reform on low-wage employment.

Cengiz et al. (2019), who exploit state-level minimum wage changes to construct a counterfactual evolution of the wage distribution.

57. In Online Appendix F.2, we show robustness to alternative cutoffs.

58. In our data, the wage bill is calculated by taking the average wage per bin, which we assume to be the midpoint of each bin, multiplying it by the total number of workers in that bin, and summing the resulting bin-level wage bills across all bins.

2. *Case Study: Laundries in the South.* We first illustrate our methodology graphically using the distribution of wages in laundries in the South. This case study is illustrative because wages in the South were very low in this industry, 40% of the workforce was black (compared with 14% at the national level for treated industries), and finally, few Southern states had preexisting minimum wage laws, making the 1967 reform a large shock.[59]

Figure IX, Panel A illustrates our bunching approach. We plot the observed frequency distribution of wages in 1967 against a counterfactual distribution with no minimum wage reform. After the minimum wage was introduced at \$1 in 1967, a large spike appears at \$1, indicating bunching around the minimum wage. The thin black line indicates the difference in employment between the observed and counterfactual distributions. The difference runs negative below \$1, jumps above 0 at exactly \$1, and then converges to 0. The figure concisely illustrates how excess jobs at or slightly above \$1 replace missing jobs below \$1. The area above the difference curve below \$1 represents our estimate of missing jobs ($|\Delta b|$) while the area under the curve from \$1 to \$1.15 represents our baseline estimate of excess jobs (Δa). As shown by Table VII, our estimates imply an employment elasticity of 0.02 (assuming spillovers up to 115% of the minimum wage, column (4)) and 0.16 (assuming spillovers up to 120% of the minimum wage, column (5)) for laundries in the South.

3. *Generalized Bunching Estimates.* We generalize our approach to the 16 treated industry $\times$ region cells for which we have sufficient data to conduct the estimation: four industries (laundries, hotels, restaurants, and nursing homes) across four census regions (South, Midwest, Northeast, and West).[60] Each

59. In 1963, 85% of Southern laundry workers were paid less than \$1.25 (the federal minimum wage in sectors covered in 1938; a sizable share were paid below \$0.50 an hour. Racial shares for laundries in the South are provided in Table VII, column (3)). See Figure IV, Panel B for national treated versus control industry racial shares.

60. See Online Appendix Figure C1. We have data for all four industries in 1967, and we have 1966 data for laundries, hotels, and restaurants. For nursing homes, prereform data are only available in 1965. Due to this limitation, we must impose additional assumptions to include nursing homes in the analysis. The aggregate number of workers in nursing homes increased by more than 40% between 1965 and 1967. This rapid growth may be due to the introduction of Medicare, which was signed into law by President Johnson in 1965 and launched in

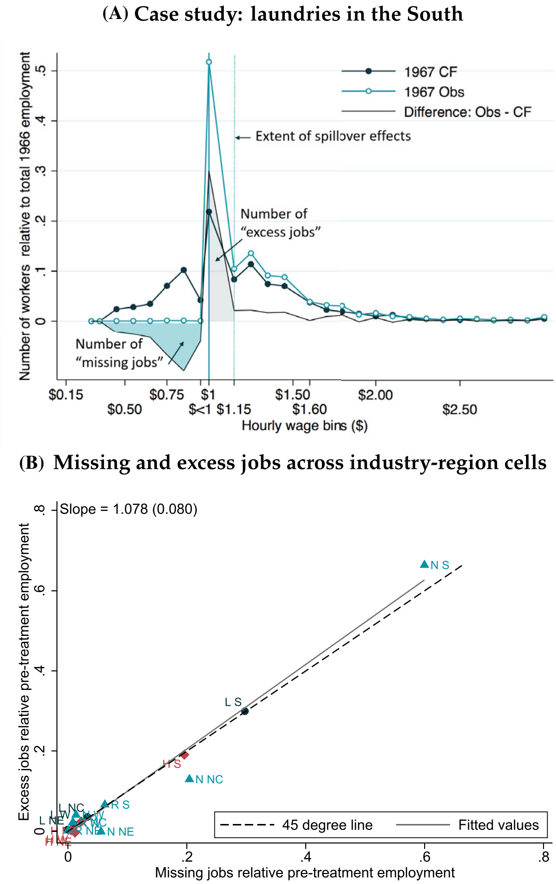


FIGURE IX

Bunching Estimation of Employment Effects in Treated Industries

BLS Industry Wage Reports. Sample: All nonsupervisory workers, excl. routemen (laundries) and tipped (e.g., in hotels and motels). The minimum wage was introduced at $1 in 1967. It reached $1.15 in 1968. In Panel A, the light blue line (with open circle markers; color version available online) plots the observed 1967 hourly wage distribution in laundries in the South. The dark blue line (with filled circle markers) plots the 1967 counterfactual distribution. The counterfactual distribution is constructed by inflating the observed 1966 hourly wage distribution by 1966–67 national income per capita growth (+4.4%); see Section V.B and Online Appendix F for more details. The dark line is the difference between the observed and counterfactual distributions for each bin. Panel B shows the number of excess (missing) jobs, relative to pretreatment total employment, above (below) the minimum wage for each industry-region cell. The black dashed 45-degree line indicates where excess jobs exactly equal missing jobs—a zero employment effect; points above (below) indicate positive (negative) effects. Industries and regions: laundries (L), hotels (H), restaurants (R); South (S), Midwest (denoted "NC" for "North Central" as in the original BLS reports), Northeast (NE), and West (W).

TABLE VII

EMPLOYMENT ELASTICITIES BY INDUSTRY AND REGION USING BASELINE BUNCHING METHODOLOGY

	Employment counts	Workers below $1 (%)	Black share (%)	Emp. elasticity wrt average wage 1.15 × MW	Emp. elasticity wrt average wage 1.20 × MW
	(1)	(2)	(3)	(4)	(5)
Laundries					
South	142,358	0.33	0.38	0.02	0.16
Midwest	107,127	0.04	0.19	0.40	0.34
Northeast	97,395	0.00	0.41	0.10	0.01
West	50,835	0.01	0.15	−0.45	−0.60
Hotels					
South	113,529	0.39	0.44	−0.10	−0.07
Midwest	83,277	0.11	0.30	−0.11	−0.07
Northeast	80,764	0.05	0.18	n.a.	n.a.
West	66,898	0.04	0.12	0.16	0.18
Restaurants					
South	271,757	0.35	0.27	n.a.	n.a.
Midwest	303,807	0.13	0.07	−0.70	0.70
Northeast	250,141	0.04	0.14	−0.22	0.76
West	185,977	0.03	0.05	−0.63	−0.36
Nursing Homes					
South	70,584	0.69	0.11	0.26	0.36
Midwest	110,199	0.32	0.06	−0.48	−0.59
Northeast	83,748	0.09	0.11	−0.41	−0.48
West	52,662	0.03	0.06	0.45	0.66
All industries					
United States	2,071,056	0.17	0.17	0.06	−0.21

Sources: BLS Industry Wage Reports for columns (1), (2), (4), and (5). 1968 March CPS for the share of black workers by industry-region groups.

Notes. Sample: All industries are composed of laundries, restaurants (nontipped workers) and hotels (non-tipped workers), and nursing homes. Column (2) measures the fraction of workers with hourly wages strictly below $1 in 1966. Column (3) uses the 1968 March CPS to assess the share of black workers by industry × region groups, as the BLS industry wage reports do not contain information on race. The 1968 March CPS is also the first year in the CPS that contains a sufficiently detailed industry code (with three-digit codes, as opposed to two-digit codes in March CPS 1962–1967) to separate out, say, laundries from hotels and other personal services. Column (4) (respectively, (5)) takes 115% (respectively, 120%) × the minimum wage as the threshold up to which the reform affects employment. The employment elasticity is calculated by dividing the percentage change in employment by the percentage change in the average wage (see Section V.B and equation (4)).

BLS industry wage report provides data on the number of workers in fine hourly wage bins in each of these 16 treated industry × region cells.

1966. We attribute 50% of this aggregate growth to the period 1966 to 1967 and increase the number of workers in each 1965 wage bin by the aggregate growth rate, so as not to include potential treatment effects of the reform in the generation of the 1967 counterfactual wage distribution for nursing homes.

Figure IX, Panel B plots the number of excess jobs against the number of missing jobs, both normalized by pretreatment employment, for our 16 treated industry-by-region groups. The 45-degree line marks the points where excess jobs exactly equal missing jobs, that is, where there is no effect on employment.[61] As the figure shows, across industry and region subgroups, the difference between excess and missing jobs is close to zero, and the fitted line across all points falls close to the 45-degree line.[62] There is, however, some heterogeneity in the employment effect across industries and especially across regions. For example, nursing homes in the Midwest have a slight decline in employment with the number of excess jobs slightly below that of missing jobs. The plot also illustrates stark differences in the bite of the reform. Swings in employment around the minimum wage were larger in the South, with 60% of nursing home jobs (relative to pretreatment total employment) moving from below the minimum wage to at or just above the minimum wage, and 30% in laundries. Hotels and restaurants were less affected, but relatively more affected in the South than in other regions.

In Table VII we report the employment elasticities implied by the missing and excess jobs plotted in Figure IX, Panel B. Column (4) reports elasticities using our baseline assumption of spillovers up to 115% of the minimum wage. Across industry-by-region groups, elasticities range from −0.7 to +0.45, well within the bounds of recent elasticities reported in the literature (see Online Appendix Figure E5).[63] Aggregating across sectors and regions, we find a small, slightly positive elasticity of 0.06. Elasticities are not higher in industry-by-region groups where the share of black workers is higher than average (column (3)). For instance, for hotels in the Midwest, where 30% of workers

61. Online Appendix Figure F2 shows the same plot when we assume spillovers up to 120% of the minimum wage.

62. A slope slightly greater than one indicates a small positive effect on employment on average.

63. In two cases, for hotels in the Northeast and restaurants in the South, we cannot report an elasticity due to a precise zero wage effect for that industry-by-region group. A precise zero effect on wages can arise in our methodology if the counterfactual distribution, which is generated by inflating wages by the aggregate 1966–1967 national income per capita growth rate, has wages close to the observed 1967 distribution. For example, workers paid just under $1 in 1966 nominal terms may earn more than $1 in the counterfactual, leading to a small implied effect of the reform on average wages.

were black, the elasticity is −0.11 and even smaller in laundries in the South (0.02), where the black share of employment is 38%. Column (5) reports the implied elasticities when we allow for spillovers up to 120% of the minimum wage. This alternative assumption leads to similar elasticities (with the exception of restaurants in the Midwest and Northeast).[64]

VI. Effects of the 1967 Reform on Racial Earnings Gaps

This section quantifies the contribution of the 1967 minimum wage extension to the decline in racial earnings inequality observed in the late 1960s and early 1970s.

VI.A. *Unadjusted Racial Gap*

We start by investigating how the reform affected the economy-wide unadjusted racial gap. To simplify the analysis, we only include the industries covered in 1938 and in 1967, that is, we disregard the industries covered in 1961, 1974, and 1986. The two sets of industries we consider include about 75% of all workers in 1966. Recall that the unadjusted racial earnings gap (in the 1938 and 1967 industries combined) fell by 25 log points between 1965 and 1980 (Figure II, Panel A).

The economy-wide racial gap can be expressed as a function of the racial gap in the 1938 industries (G^c), the racial gap in the 1967 industries (G^t), the average log earnings difference between black workers in the control versus treated industries G_b^{ct}, and the shares of black and white workers in the treatment and control industries:

$$G^{\text{total}} = s_w^c G^c + s_w^t G^t + G_b^{ct}\left(s_w^c - s_b^c\right), \tag{5}$$

with s_w^c (resp. s_b^c) the share of white (resp. black) workers working in the control industries; s_w^t (respectively s_b^t) the share of white (resp. black) workers working in the treated ones; $s_w^c + s_w^t = s_b^c + s_b^t = 1$. By 1980, we have $s_w^c = 64\%$; $s_w^t = 36\%$; $s_b^c = 56\%$; $s_b^t = 44\%$.[65]

64. Because of the localized bunching approach used to estimate the employment effects of the reform, these fluctuations in the employment elasticity can arise from idiosyncratic differences in the number of workers paid between \$1.15 and \$1.20 across the observed and counterfactual distributions.

65. See Online Appendix G for a derivation of the decomposition.

Using this decomposition, we estimate how the unadjusted racial earnings gap would have evolved if the minimum wage had not been extended in 1967. Our counterfactual scenario relies on two assumptions: first, that absent the reform the racial earnings gap in the treatment group, G^t, would have evolved as in the control group (as was the case before the reform); second, that the control-treatment earnings gap for black workers G_b^{ct} would have evolved as for white workers (as was the case before the reform). We calculate a counterfactual for G^t (resp. G_b^{ct}) by averaging the difference in the pretrends of the racial earnings gap (resp. control-treatment gaps) between 1961 and 1966 and adding this constant to the racial earnings gap in the control group (resp. control-treatment gap for whites) for each year after 1966. Specifically, we compute $G^t_{k,\text{counterfactual}}$ as:

$$(6) \quad \begin{cases} \forall k \leqslant 1966 : G^t_{k,\text{counterfactual}} = G^t_{k,\text{observed}} \\ \forall k > 1966 : G^t_{k,\text{counterfactual}} \\ \qquad = G^c_{k,\text{observed}} - \frac{1}{N}\sum_{k=1961}^{1966}\left(G^c_{k,\text{observed}} - G^t_{k,\text{observed}}\right). \end{cases}$$

As shown by Figure X, the 1967 minimum wage extension can explain around 20% of the decline in the racial earnings gap between 1965 and 1980. The unadjusted racial earnings gap would have been 31 log points instead of 25 log points in 1980. Eighty-two percent of this 6 log point difference owes itself to a reduction in the racial earnings gap within the treated industries (i.e., within-industry convergence). The remaining 18% owes itself to a reduction in the control-treatment earnings gap for black workers (i.e., between-industry convergence).

The contribution of the minimum wage to the decline in the unadjusted racial earnings gap (20%) is comparable in size to the effect of relative school quality improvements documented by Card and Krueger (1992).[66] To what extent does our estimated contribution of coverage extension understate or overstate the

66. There are some differences, however, between our calculations and Card and Krueger (1992)'s calculations that prevent a precise straightforward comparison. In particular, Card and Krueger (1992) calculate the contribution of relative improvements in schooling quality to the decline of the unadjusted racial wage gap measured as the mean log weekly (versus annual in our calculation) wage difference between white and black workers aged 21–60 (versus 25–55 in our calculations), for the whole economy (versus our treatment and control industries combined), and from 1960 to 1980 as measured in the U.S. censuses (versus from 1965 to 1980 measured in the CPS).

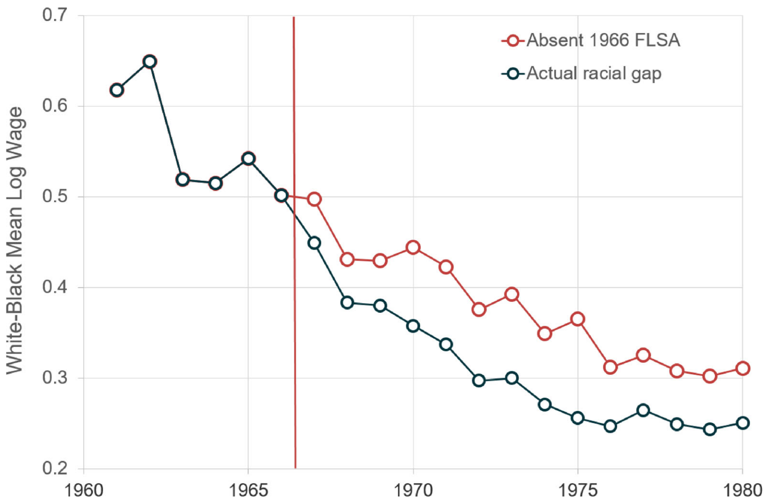


FIGURE X

1967 Reform Reduced Economy-Wide Racial Gap by ~20%

March CPS 1962–1981. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. The racial gap is calculated as the difference in the average log annual earnings of black workers and the average log annual earnings of white workers. There is no adjustment for any observables. The CPS collects information on earnings received during the previous calendar year. Therefore, we report estimates of the racial gap, for example, in the 1962 CPS in 1961 above. The economy-wide racial gap is defined here as the combination between the industries covered in 1938 and the industries covered in 1967.

contribution of minimum wage policy to the reduction in the racial earnings gap during this period? We underestimate the true impact of minimum wage policy on the racial earnings gap in the late 1960s because the 1967 reform not only extended coverage to new industries but also raised the level of the existing federal minimum wage. Black workers in the control industries likely experienced relative earnings gains as a result of the overall increase in the minimum wage, given their greater concentration in the lower part of the earnings distribution. Thus, from this point of view, our estimated contribution of 20% understates the true effect of the reform on racial inequality.

One potential concern is that we may overstate the contribution of the reform and minimum wage policy to the reduction in the racial earnings gap and in the racial income gap, in particular, if the reform had disemployment effects. As reported in Section V.A, the largest negative employment elasticity consistent with our results is −0.16. A portion of the reduction in the earnings gap may therefore reflect greater loss of black employment in the lower part of the productivity distribution relative to white. This would generate a selection effect on earnings—the black workers remaining employed would be higher productivity and have higher average earnings compared to the group of black workers employed prior to the reform. However, given the small disemployment effects implied by even the largest negative employment elasticity we estimate, we do not believe that we significantly overestimate the contribution of the reform to the decline in racial inequality.

VI.B. Adjusted Racial Gaps

Next we investigate the role of the 1967 reform in the evolution of the adjusted racial gap (i.e., controlling for observables). We estimate the following equation for workers in the treated and control sectors separately:

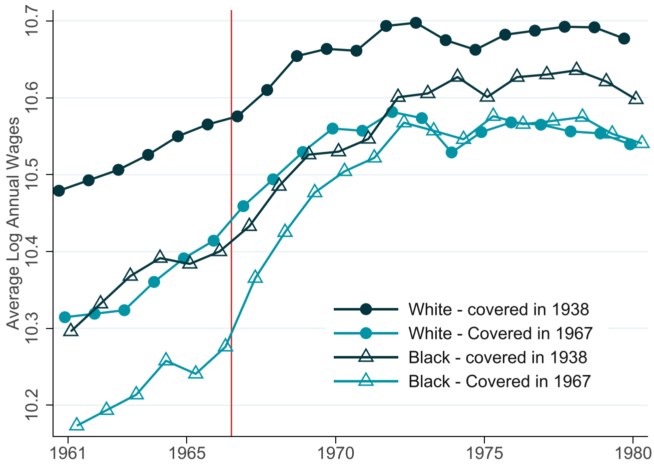
$$\log w_{ijt} = \alpha + \gamma \text{Black}_i + \sum_k \beta_k \text{Black}_i \times \delta_{t+k} + \mathbb{X}'_{ijt}\Gamma + \nu_j + \delta_k + \varepsilon_{ist}, \tag{7}$$

where Black_i is a dummy for being a black worker; the set of individual-level controls $\mathbb{X}'_{ijt}$ is the same as in the wage regression.

Figure XI, Panel A uses this equation to show the evolution of the average wage of black and white workers in the treated and control industries. Conditional on observable characteristics, black workers in the treated industries earned about 12% less than black workers in the control industries before the reform. The wages of these two groups of workers evolved in parallel. In 1967, the wage gap between black workers in control versus treated industries fell dramatically, to less than 5% in the years after the reform. Strikingly, within the treated industries the earnings of black workers entirely caught up with those of white workers. Average earnings (for both white and black workers) remained lower in the treated industries than in the control industries postreform.

We plot the corresponding adjusted racial gaps (i.e., $\gamma + \beta_k$, k in [1961,1980]) for the control and treated industries in Figure XI, Panel B. Before the reform, and conditional on observable characteristics, white workers were paid 20%–25%

(A) Wage effects in levels by race and treatment status



(B) Adjusted racial earnings gaps, by treatment status

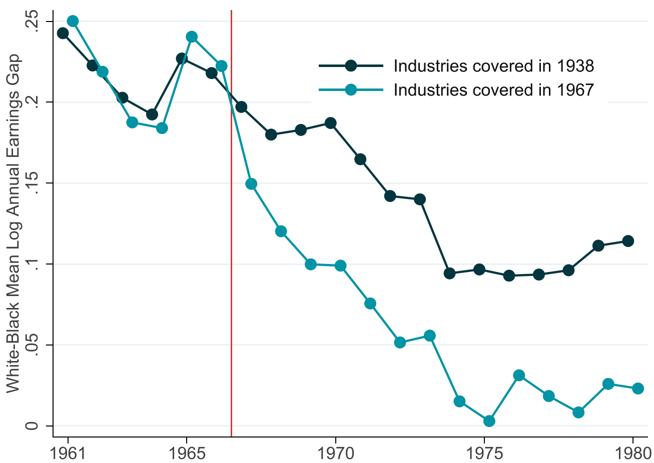


FIGURE XI

Adjusted Racial Wage Gaps

March CPS 1962–1981. Sample: Adults 25–55, black or white, who worked more than 13 weeks last year and three hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. Racial earnings gap measures are adjusted for gender, race (Panel B only), number of years of schooling, experience, full-time or part-time status, number of weeks and hours worked, industry, occupation, and marital status. In Panel A, the reference group is a male worker in 1965, with 12 years of schooling, married, in professional and technical occupations, working full-time, full-year. In the bottom panel, the reference category is male workers working full time, with 12 years of schooling, 5 years of experience, and working in Business and Repair Services.

more than black workers. This is true in both the treated and control industries. The adjusted racial earnings gap also evolved in parallel before the reform.

Starting in 1967, the adjusted racial earnings gap declined in both the treated and control industries. However, it fell much more in the treated ones. By the mid-1970s the adjusted racial gap vanished in the treated industries (see light blue lines in Figure XI, Panel A), while a 10% difference in wages between similar black and white workers in the control industries remained. One interpretation of the positive racial earnings gap in the control industries (despite the presence of a high minimum wage) is that the gap is driven by wage differences conditional on observables among medium- or high-skill workers. By contrast, because the industries in the treatment group are low wage, the adjusted racial earnings gap may be close to zero if a large fraction of the workers are paid around the minimum wage.

Last, we have checked that the decline in the adjusted racial gap is concentrated among low-education workers in the treated industries (see Online Appendix Figure D5a) and that there is no change in trend for high-education workers. By contrast, the decline in the adjusted racial earnings gap is smooth for both high- and low-education workers in the control industries (see Online Appendix Figure D5b). These results further suggest that the extension of the minimum wage (and not some other confounding shock) is the true driving force behind the decline in the adjusted racial earnings gap in the treated industries.

The impact of the 1967 minimum wage reform on the evolution of the racial earnings gap is consistent with the patterns documented by Bayer and Charles (2018), who note that distributional forces (those affecting any worker at a particular point in the earnings distribution), rather than positional forces (those specifically affecting black workers relative to white), have driven racial earnings convergence since 1950. Furthermore, our findings raise the possibility that the declining real federal minimum wage of recent decades has contributed to the contemporaneous stalling of racial convergence. Such a mechanism would also be consistent with the long-run patterns described in Bayer and Charles (2018).

VI.C. Discussion

How can we explain the large wage and small disemployment effects of the 1967 reform? Empirically, our findings with respect to employment are highly consistent with the recent minimum wage literature studying more modern reforms. For example, Cengiz et al. (2019) examine evidence from state increases in the United States from 1979 to 2016 and find limited employment effects, even in subgroups where the minimum-to-median wage ratio is as high as 59%.[67] In our period of study, the minimum-to-median wage ratio for all workers in newly covered industries peaks at about 50%. Conceptually, such a result is possible in a competitive labor market if labor demand is inelastic. This is the case when there is perfect complementarity between factors of production (between high-skilled and low-skilled labor or between labor and capital) or in tight labor markets, as was the case in the 1960s (Friedman 1962; Tobin 1965; Osborne 1966). In a monopsony model, an increase in the minimum wage leads to positive employment effects if the new minimum wage falls between the wage paid by a monopsonist and the wage paid by a perfect competitor (Stigler 1946). This is consistent with our empirical evidence in certain sectors (e.g., laundries in the South).[68]

How can we explain why the 1967 reform did not hurt black workers vis-à-vis white workers? The relative wage gains black workers made as a result of the reform could have induced employers to substitute toward white workers, even if aggregate employment is unaffected. However, we find no evidence that this substitution took place. In Online Appendix Table E9, we directly estimate the effect of the reform on relative earnings and employment of white workers. Across all specifications we document positive but near zero labor-labor substitution. Historical analyses of U.S. labor markets in the 1960s document a clear separation of black and white workers into "back-of-the-house" and "front-of-the-house" jobs, respectively.[69] It is possible,

67. See Dube (2019a) for an international review of the evidence, which also finds low employment responses to minimum wage increases across a variety of contexts.

68. At the same time, the minimum wage can lead to disemployment under monopsony if set to a level higher than the wage paid by a perfect competitor—this could be the case in other sectors where we observe small disemployment effects. In theory, price responses to the 1967 reform in product markets could be used to understand the importance of monopsony power in these sectors and regions during this historical period (Aaronson, French, and MacDonald 2008). However, there is a lack of data on sectoral prices by states during these years. Neither the Bureau of Economic Analysis nor the BLS collected price indices at the state × sector level in a systematic way in the 1960s and 1970s.

69. Self (2005) describes the employment line in the service sector in Oakland in the postwar period, where customer-facing and better-remunerated positions

therefore, that even if employers sought to substitute toward white workers, the latter may have been loath to take up low-status jobs traditionally associated with black workers (or work alongside them in these positions). In Online Appendix Table E10, we provide descriptive evidence on occupational segregation using the decennial 1960–1980 U.S. Censuses. Occupational segregation remained high in both treated and control industries over this period. These pieces of evidence, combined with the qualitative literature, support a story where low labor–labor substitutability made demand for black workers relatively inelastic, paving the way for the minimum wage to reduce racial inequality.

VII. Conclusion

This article studies the causal effect of the 1967 extension of the U.S. federal minimum wage—a large natural quasi-experiment—on wages, employment, and the dynamics of racial inequality in the United States. We uncover the critical role of the minimum wage in the reduction of the racial earnings gap during the civil rights era. The 1966 FLSA extended minimum wage coverage to sectors that employed 20% of the U.S. workforce. Drawing on a variety of data sources—including newly digitized BLS industry wage reports—and research designs, we show that the 1967 reform dramatically increased wages in the newly covered industries. The reform contributed to reducing the economy-wide racial gap in two ways: first, by reducing the wage gap between the treated industries (where black workers were overrepresented) and the rest of the economy; second, by reducing the racial earnings gap in the treated industries, as the wages of black workers increased faster than those of white workers. We can rule out large disemployment effects, including among black workers. Overall, the 1967 extension of the minimum wage can explain more than 20% of the decline in the racial gap observed during the late 1960s and 1970s—the only period of time after World War II during which the black-white earnings gap fell significantly. To our knowledge, our article provides the first causal evidence on how minimum wage policy affects racial

were exclusively held by white workers. Cobble (2005) describes similarly strict delineations in employment and long-lasting campaigns to open up better-paying service sector jobs to black women.

income disparities and sheds new light on the dynamics of labor market inequality in the United States.

While we focus on the effect of the 1967 extension of the minimum wage to new sectors of the economy, it is likely that the minimum wage affected racial inequality more broadly. The late 1960s were a time when the federal minimum wage reached its historical peak in real terms, following a series of hikes in 1961, 1963, 1967, and 1968. To the extent that black workers were overrepresented at or just below the minimum wage, these increases may have contributed to reducing the racial earnings gap above and beyond the 1967 reform. In future research, we plan to investigate how the decline in the federal minimum wage starting in the 1970s may have contributed to the stagnation of racial earnings convergence over the last several decades. Another fruitful venue for future work involves studying the consequences of recent local state minimum wage increases on gender and racial earnings gaps today.

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

Data and code replicating the tables and figures in this article can be found at Derenoncourt and Montialoux (2020), in the Harvard Dataverse, doi: 10.7910/DVN/MHNS1S.

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