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Minimum Wages and Racial Inequality

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Minimum Wages and Racial Inequality*

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Abstract

The earnings difference between white and black workers fell dramatically in the United States in the late 1960s and early 1970s. This paper shows that the extension 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 which 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 micro-data to investigate the effects of this reform on wages, employment, and racial inequality. Using a cross-industry difference-in-differences design, we show that wages rose sharply for workers in the newly covered industries. The impact was nearly twice as large for black workers as for white. Within treated industries, the racial gap adjusted for observables fell from 25 log points pre-reform to zero afterwards. Using a bunching design, we find no effect of the reform on employment. We can rule out significant dis-employment effects for black workers. 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.

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1 Introduction

One of the most striking dimensions of inequality in the United States is the persistence of large racial economic disparities (Bayer and Charles, 2018; Chetty et al., 2018). 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 1).[1] Over the last 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 this historical improvement may provide 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 anti-discrimination legislation (Freeman, 1973) and improvements in education (Card and Krueger, 1992; Smith and Welch, 1989). The magnitude of the decline, however, remains a puzzle (see Donohue and Heckman, 1991, and our discussion of the related literature in Section 2 below).

This paper 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 over-represented: 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 for white. Our estimates suggest that the 1967 extension of the minimum wage can explain more than 20% of the decline in the racial earnings gap during the late 1960s and early 1970s. Moreover, we find that this reform did not have detectable adverse employment effects on either black or white workers. The extension of the minimum wage thus not only reduced 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 paper provides the first causal

[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 onwards; 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.

evidence on how minimum wage policy affects racial income disparities.

Our contribution in this paper 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 of the paper is to assemble a novel dataset 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 cents 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 micro-data from the March Current Population Survey (CPS), which allow us to investigate how the effects of the reform vary with race and other socio-economic 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 impacts 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 vs. previously covered industries, before and after 1967. In the CPS data, the average annual earnings of workers in the industries covered in 1938 (our control group) evolve in parallel to the annual earnings of workers in the industries covered in 1967 (our treated group) before the reform. In 1967, they jump by 6% relative to the control industries and the effect is permanent through the late 1970s. The magnitude of the wage increase is consistent with the predicted effect of the minimum wage hike estimated using the pre-reform CPS. We obtain a similar increase in 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. Using our BLS data, we implement 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 US 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 over-represented (40% of the workforce). We document a near-zero effect on employment in this sector and region. We then demonstrate that this near-zero effect holds across many industry and region subgroups. Overall, the bunching estimator suggests low employment responses in treated industries in the United States as a whole. Our finding of small employment responses is robust to considering alternative assumptions on the extent of the spillover effects of the minimum wage.

We confirm our core results of large wage effects and small employment effects using a second research design. 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. In this research design, the 1967 reform has a precise zero effect on employment. We are able to rule out employment elasticities with respect to average wages greater than -0.1. The results hold for black workers in isolation, for whom employment elasticities greater than -0.2 can be ruled out.

The second—and most important—contribution of the paper 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 over-represented in the treated industries, this between-industry convergence reduced the U.S.-wide 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 reduces 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, etc., black workers were paid 25 log points less than white workers with similar observables (such as education, experience, number of hours worked, etc.) when the federal minimum wage did not apply, and this difference falls to close to zero after the introduction of the federal minimum wage.

Because the reform does not appear to have had significant adverse effects on black employment, the decline in the racial earnings gap translates into a similar decline in the racial income gap. The 1967 reform was thus effective at advancing black economic status.

We discuss potential explanations for the large effect of the minimum wage on racial inequality. One hypothesis is that prior to the reform, whites colluded to pay black workers low wages (below their average product) in the uncovered industries, particularly in the South. White collusion before 1967 could rationalize the low dis-employment effects of the reform. The introduction of the minimum wage reduced the possibilities of discrimination against black workers in agriculture, nursing homes, and other newly covered sectors. This insight potentially provides a new theoretical justification for minimum wage legislation when governments are concerned about forms of inequality that cannot be addressed directly through income-based tax and transfer policies.

The remainder of the paper is organized as follows. We start by relating our work to the literature in Section 2. Section 3 presents background information on the 1966 amendments to the Fair Labor Standards Act and describes the datasets used in this research. We study the effects of the reform on wages in Section 4 and its effects on employment in Section 5. Section 6 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 (e.g., white collusion). Section 7 concludes. An online appendix supplements the paper. The data and programs used in this paper are available online at: clairemontialoux.com/flsa.

2 Related Literature

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

2.1 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 types of explanations have been put forward: changes in the demand side of the labor market vs. changes in the supply side.

Demand side of the labor market. A cornerstone of the Civil Rights movement was the introduction of federal anti-discrimination policies. Title VII of the 1964 Civil Rights Act prohibited both employment and wage discrimination based on race.[2] It was enforced by the Equal Employment Opportunity Commission (EEOC) created in 1965.[3] Executive Order 11246, issued in 1965 and enforced by the Office of Federal Contract Compliance, required U.S. government contractors to prohibit discriminatory practices in hiring and employment and introduced affirmative action for government contractors (Ashenfelter and Heckman, 1976; Burman, 1973; Goldstein and Smith, 1976; Heckman and Wolpin, 1976).[4] The role of state fair-employment practices commissions was expanded, as the EEOC started referring cases to these commissions (Landes, 1968; Heckman, 1976).

A number of studies investigated whether these anti-discrimination policies increased the relative demand for black workers (Freeman, 1973; Freeman et al., 1973; Vroman, 1974; Freeman, 1981; Brown, 1984; Heckman and Payner, 1989; Smith and Welch, 1986; Wallace, 1975; Butler and Heckman, 1977). 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) also considered the role of the Voting Rights Act of 1962 and 1965, as well as other federal initiatives (such as school desegregation) in narrowing the racial gap.

[2] Title VII also prohibited employment and wage discrimination based on sex, color, religion and national origin.

[3] Most employers were covered by the Equal Employment Opportunity Commission, except firms with fewer than 100 employees (later reduced to 25 and then 15 employees), firms not engaged in interstate commerce, the self-employed, and state and local governments. Unions and employment agencies were covered.

[4] Discrimination on the basis of sex became part of the contract-compliance program in 1967. Affirmative action against sex discrimination was required in 1971.

One key difficulty faced in this literature is the fact 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 anti-discrimination activity. Most of the literature used either sparse intercensal wage data or aggregated time series that make it difficult to isolate the contribution of these policy changes at the macro level.[6]

Supply side of the labor market. 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. Smith and Welch (1989) and Lillard et al. (1986) 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] Moreover, a body of work argues theoretically that the returns to schooling could have increased for black workers during the 1960s as a result of the tightening of the labor market (Osborne, 1966; Tobin, 1965; Friedman, 1962). Heckman and Payner (1989) do not find empirical support for this theory, however.

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 factor can explain about 10%-20% of black-white wage convergence during the Civil Rights movement. Other supply shift stories, such as the northern migration of African Americans over the 20th century, have been found to play a minor role.[8] Overall, Donohue and Heckman (1991) find that supply-side factors can explain about 55% of the decline in the racial gap during the Civil Rights Era.

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

[5] The identification problem is particularly acute for studies of the role of the Equal Employment Commission, as Title VII covers all firms in the economy. Heckman and Wolpin (1976) also showed that it is difficult to assess the causal impact of the OFCC 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 focused 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] 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.

inequality. This factor turns out to be quantitatively important, comparable in size to the impact of school desegregation found by Card and Krueger (1992) and improvements in school quantity found by Smith and Welch (1986). Our paper 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. Appendix Figure G1a plots the evolution of the unadjusted racial earnings gap since the early 1960s, measured as the mean log difference in average annual earnings between white and black workers. As is apparent from this figure, much of the decline happened in just two years: 1967 and 1968. Neither the demand nor supply factors described above can easily explain the specific timing of the reduction in the racial earnings gap. Anti-discrimination policies were rolled out gradually from 1964 onwards; the enforcement powers of the Equal Employment Opportunity Commission gradually increased over time (Wallace, 1975; Butler and Heckman, 1977).[9] 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.[10] By contrast, the 1967 extension of the minimum wage can explain why a large portion of the decline in the racial earnings gap took place in 1967. Appendix Figure G1b shows indeed that the unadjusted racial earnings gap fell sharply in the newly covered industries relative to the previously covered ones precisely in 1967.

2.2 Minimum Wage Literature

An expansive literature studies the economic effects of the minimum wage. Our paper contributes to this literature in several ways.

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

[9] It is only in 1972 that the Equal Employment Opportunity Commission was 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., p. 211 of the U.S. Civil Rights Commission, 1977: https://www2.law.umaryland.edu/marshall/usccr/documents/cr12en22977.pdf).

[10] Medicare and Medicaid were introduced in 1966, but were initially small quantitatively (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 et al. (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 stamp 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 et al. (2018) available at http://gabriel-zucman.eu/usdina/.

minimum wage and focuses on its employment effects (see, e.g., Card, 1992; Card et al., 1993; Neumark and Washer, 1992; Card and Krueger, 1995; Neumark and Washer, 2008; Dube et al., 2010; Cengiz et al., 2019). The literature also studies effects on wage inequality (see, e.g., Blackburn et al., 1990; DiNardo et al., 1996; Lee, 1999; Autor et al., 2016) and family incomes (Gramlich, 1976; Congressional Budget Office, 2014; Dube, 2017). To date, however, the interplay between the minimum wage and racial inequality has not been investigated using a causal research design.

Second, our paper provides evidence on the economic effects of very large minimum wage increases. The 1967 reform was a large shock to the treated industries in states that did not have a state minimum wage — in these states, the wage floor moved from zero to the prevailing federal minimum wage, which was at a high level in the late 1960s. On top of extending the minimum wage to new sectors, 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). Bailey et al. (2018) investigate how the high nation-wide minimum wage mandated by the 1966 Fair Labor Standards Act affected employment, exploiting state-level differences in the bite of a national minimum wage due to differences in standards of living. Their results show little evidence of disemployment effects for men, consistent with our results. Because our paper 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) and relies in part on different data (our newly digitized BLS tabulations), we view our projects as complementary. 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., Harasztosi and Lindner, 2019; Engbom and Moser, 2018). Evidence about the effects of large hikes can help inform current policy discussions in the United States, where a number of both local and federal policy-makers are implementing or considering large increases in minimum wages.

Third, we add to the burgeoning literature on bunching estimation applied to the minimum wage. One of the advantages of the bunching approach is that it offers transparent graphical evidence on the employment effects of minimum wage hikes within large industries.[11] We are also able to track where in the wage distribution jobs were created or destroyed.

Finally, we contribute a new database of minimum wage legislation by state, industry,

[11] By contrast, the bulk of the literature has focused on teen employment or workers in specific industries, typically restaurants (Abowd et al., 2000; Allegretto et al., 2017; Neumark et al., 2014).

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).

3 The 1967 Extension of the Minimum Wage and Data

3.1 The 1966 Fair Labor Standards Act

Political economy of the reform. The Fair Labor Standards Act (FLSA) of 1938 introduced the federal minimum wage 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 53% of the U.S. workforce (see Figure 3a) in the manufacturing, transportation and communication, wholesale trade, finance and real estate sectors (see the complete list of covered sectors in Figure 2). 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 Fair Labor Standards Act, as a number of other programs passed in the 1930s and 1940s, thus had a discriminatory dimension (Katznelson, 2006; Mettler, 1994; Rothstein, 2017).

Over time, a series of amendments to the 1938 FLSA extended the minimum wage to the rest of the economy. In this paper, we focus on the 1966 FLSA amendments, the largest expansion of the federal minimum wage.[12] The 1966 FLSA amendments introduced the federal minimum wage (as of February 1st, 1967) in the following sectors: agriculture, nursing homes, laundries, hotels, restaurants, schools, and hospitals. These sectors employed about 8 million workers (see Figure 3a) in 1967, or about 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 to 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 of

[12] Using CPS data, we estimate that 53% 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 22% 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 paper.

1963.[13] President Johnson was also conscious of this imbalance, and declared when signing the 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).

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.[14] The Kaitz Index exhibits a jump in 1967 as well (see Figure 4). 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.[15]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.

3.2 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 online at: `clairemontialoux.com/flsa`; see Appendix I.

Bureau of Labor Statistics industry wage reports. The BLS conducted regular establishment surveys, starting in the 1930s through the 1980s to monitor the implementation of the Fair Labor Standards Act of 1938 and its amendments.[16] The surveys were requested by the

[13] The 9th demand is formulated as follows: "[We demand] a broadened Fair Labor Standards Act to include all areas of employment that are presently excluded", see Appendix Figure H1.

[14] 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 Appendix Figure E1.

[15] 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.

[16] 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 19th century to the 1980s, see Douty (1984).

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 coverage of the manufacturing and the non-manufacturing sectors nationwide.[17]

The BLS focused on collecting information on the distribution of employer-paid hourly earnings, based on employer payroll records.[18] 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 vs. non-tipped workers for the restaurant and hotel industries; inside-plant workers vs. 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 vs. non-metropolitan). The BLS data allow us to transparently study the evolution of the hourly wage distributions in each sector over time and to investigate any heterogeneity in the impact of the 1967 reform across many dimensions.

For the purposes of this project, we digitized over 1,000 hourly wage earnings distributions from every year available between 1961 to 1969. We built a database of hourly wage distributions for the industries covered in 1967, as well as for a set of industries covered in 1938—mainly from non-durable, low-wage manufacturing sectors.[19]

Current Population Survey data. The Census Bureau and the Bureau of Labor Statistics have conducted the Current Population Survey—a monthly household survey—since the 1940s. However, public use files are only available for the years 1962 and onwards. We use data from the March CPS, more precisely the Integrated Public Use Microdata Series (IPUMS) from 1962-1980.[20] IPUMS released the 1962-1967 files with a harmonized 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 onwards (e.g., starting six years before the 1967 extension of the minimum wage). We study earnings through to 1980, i.e., two years after the

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

[18] 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.

[19] More precisely, we digitized data for cigars, cotton textiles, flour and grain mills, hosiery, leather tanning, men's and boys' suits and coats, men's and women's footwear, men's and boys' shirts, miscellaneous plastic products, and wood household furniture. About 35 more industries are also available (see Appendix Figure D1a). More details on BLS of Labor Statistics industry wage reports are provided in Appendix D.1.

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

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 rich individual worker-level data, e.g., gender, race, and education levels (30 categories). We harmonized industry classifications across years; our harmonized industry variable includes 23 different industries.[21] This is more detailed than the 2-digit NAICS code but a bit coarser than the 3-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.[22] In the CPS regressions shown below, our main outcome of interest will thus be annual earnings, and we will control for the number of weeks worked and the number of hours worked within a week. As we shall see, the wage effects of the reform estimated using the CPS will turn out to be very consistent with the effect on hourly wages seen in the BLS industry wage reports.

Second, pre-1968 CPS micro files have fewer observations than in later years,[23] 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 files. However, conditioning on being employed, annual earnings in March CPS and Census are perfectly in line (see Figure 1).[24] However, the employment *shares* by industry and race match the information contained in the Census. Further, 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

[21] We used the information contained in the original industry variable from 1962 to 1967 and in the industry variable created by IPUMS from 1968 onwards 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 `usa.ipums.org/usa/chapter4/chapter4.shtml`.

[22] 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.

[23] About 15,000 observations in our sample in March CPS 1962-1965, then around 30,000 through the mid-1970s, see Appendix Table B2.

[24] 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 the men-women employment shares however are similar in March CPS 1962 and 1963 and Census 1960.

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 Appendix Figure B2). We checked that the borders of the state groups do not cross region or division lines. Importantly, the states within each group have similar state minimum wage policies. Thus this data limitation is unlikely to be a threat to our cross-state empirical strategy. In our analysis using CPS data, for simplicity we use the term "states" to refer to "state groups."

U.S. Census data. We use the 1-100 national random sample of the population from the 1940, 1950, 1960, 1970, and 1980 decennial censuses to compute the share of workers covered by the Fair Labor Standards Act of 1938 and its subsequent amendments.[25] 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 Appendix B.5).

Minimum wage database. We use the report of the Minimum Wage Study Commission published in 1981 (James G. O'Hara, 1981) to build our minimum wage database by state, gender, and industry.[26] We cross-check the information in the Minimum Wage Study Commission (1981) with the information contained in the Department of Labor Handbook on women workers published in 1965 (Willard Wirtz, 1965).[27] In 1965, 31 states and the District of Columbia had minimum wage laws (see Appendix A for more details).

4 The Wage Effects of the 1967 Reform

4.1 Identification Strategy, Sample, and Summary Statistics

We start by studying the effect of the 1967 extension of the minimum wage on the dynamics of wages – measured as log annual earnings – in the CPS, before studying the effect of the reform in the BLS data (with wages measured as hourly wages).[28] Our baseline empirical approach

[25] 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).

[26] 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.

[27] Accessible here: https://fraser.stlouisfed.org/files/docs/publications/women/b0290_dolwb_1965.pdf.

[28] In what follows, when we talk about wages in the CPS, we refer to log annual earnings, and when we talk about wages in the BLS data, we refer to hourly wages.

is a cross-industry difference-in-differences research design: we compare the dynamics of wages in the newly vs. 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 5).

As discussed below, our effects are robust to the inclusion of a wide range of controls and time-varying effects, such as state and industry, making it unlikely that our effects are confounded by contemporaneous changes differentially affecting workers in the treated vs. control industries.

Our sample includes all prime-age workers, i.e., 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).[29] 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 3 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, we exclude from the analysis the industries added in 1961, 1974, and 1986, which together employed about 25% of the workforce, see Appendix A). As shown by Table 3, our results are not sensitive to these sample restrictions. All wages are converted to 2017 dollars, using the CPI-U-RS price index from the Bureau of Labor Statistics.

Table 2 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.[30] In both the control and treated industries, black workers were less educated than white on average (around 40-45% have more than 11 years

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

[30] In this paper, we focus on the contribution of the 1967 reform on 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.

of schooling vs. 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 (42%). 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 (88% for white and 79% for black workers) than in the control industries (69% for white and 67% for black workers).

We estimate the following difference-in-differences model:

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

where $\log w_{ijst}$ denotes the log annual earnings of worker i in industry j, state s, in year t.[31] The dummy variable Covered 1967_j equals 1 if worker i works in an industry covered in 1967, 0 if they work in an industry covered in 1938. t is the year when the reform 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,[32] and the number of hours worked.[33] In section 5 below, we show that the reform did not affect the number of hours worked per year conditional on working (see Figure 10a and Appendix Table E3).[34] 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 Appendix Figure C1 showing the wage effect with all controls, all controls except number of weeks and hours worked,

[31] Year t corresponds to the calendar year during which income was earned, i.e. 1961 in CPS 1962, 1962 in CPS 1963, etc.

[32] 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.

[33] The CPS contains information on the number of hours worked last week.

[34] The annual number of hours worked is constructed as the ratio between the annual wage (as directly measured in the CPS) and the hourly wage (as re-constructed). We re-construct a measure of hourly wage by dividing the annual wage by the product of the number of hours worked per week and the number of weeks worked per week (measured as the midpoint of each weeks-worked interval). Because we do not observe the exact number of weeks worked per year, the variance of the measure of the hourly wage thus obtained is underestimated. Therefore, we further smoothed this hourly wage measure by adding or subtracting to it a random number generated from a uniform distribution over the interval[-\$0.25,\$0.25] (after converting our hourly wage measure to \$2017).

and no controls). It increases, however, the precision of our estimates.[35] 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 et al., 2004).

4.2 Baseline Estimates of the Effect of the 1967 Reform on Wages

Figure 5 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 vs. control industries evolved in parallel: the point estimates for the years 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 (+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 2. 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 0.066 log points (i.e., 7%) higher relative to the average wage of workers in control industries in 1967-1972 compared to 1966 and 0.051 log points (i.e., 6%) higher in 1973-1980 relative to 1966; see Table 3, column 1. These effects are statistically different from zero at the 5% level.

Actual vs. 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

[35] Adding or not adding individual-level controls has no effect on our medium-run point estimates as shown in Figure C1. 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.

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.

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 footnote 4.1 above). From there, we estimate that 16% of workers in the treated industries were below the 1967 minimum wage in 1966; see column (1) in Table 4). 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% $\times$ 34% = 5.5%; see column (4). This is close to the estimated effect of 5% found in our wage regression in 1967.[36] The predicted wage effect is slightly larger than the observed effect (5.5% vs. 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 5.

Effects by education. The wage effect shows up primarily where one would expect to see it, i.e., for workers with low levels of education. We separately estimate the above model for workers with 11 years of schooling or less vs. those with more than 11 years of schooling; see Figure 6a.[37] For workers with low levels of education, wages increased by 10% in 1967 in the newly covered industries, above and beyond wage growth in the previously covered industries. The effect is much smaller (4% 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 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 vs. previously covered industries, before and after 1967. Control industries here include non-durable manufacturing industries (see Figure D1a for the list of industries we digitized

[36] Because we make predictions for 1967 alone, we compare the predicted effects to our wage coefficient obtained for 1967 alone (see Figure 5 rather than to the pooled estimate for 1967-1972 presented in Table 3).

[37] There is a similar pattern among black and white workers (see Appendix Figures C3a and C3b).

and years available), which were covered by the minimum wage in 1938.[38] 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:

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

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 post-reform data and years with both control and treatment industries and a full sample including all our digitized data.

Table 5 shows that, in the difference-in-differences model, wages in the newly covered industries jump by 11% relative to wages in non-durable manufacturing after the reform (1967-1969) relative to before (see columns (1) and (2)). This magnitude is somewhat higher than the 7% wage increase estimated using CPS data. This difference in the magnitude could be due to differences in the measure of the outcome (hourly wages in the BLS vs. annual earnings in the CPS), in the sample (BLS data are focused on non-supervisory 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. We note that in the triple difference-in-differences model, the wage increase is higher for treated industries in the South relative to non-durable manufacturing industries in the non-South (+14% 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.

[38] Manufacturing represents more than 50% of all 1938 industries. Non-durable manufacturing represents about half of manufacturing in terms of the number of workers employed. In addition, wages in non-durable and durable manufacturing follow strictly similar trends, as can be seen in the CPS. We therefore believe that the subset of industries in the non-durable manufacturing industries form a good control group in this empirical setting.

4.3 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 vs. 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.

Robustness to 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 3 we add state fixed effects and state linear trends to the controls of our baseline model. The inclusion of state fixed effects and state linear trends does not change the magnitude or the pattern of the estimated wage effect. This suggests regional wage convergence is unlikely to bias our estimates.

Robustness to sectoral shocks. One might be concerned about shocks happening in certain treated industries, such as agriculture (e.g., mechanization). In column 3 of Table 3, we exclude agriculture from our sample to see whether the results still hold. We find that the magnitude of the wage effect (6%) is only a bit lower than when agriculture is included (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.

Additional robustness tests. We report the following additional robustness tests. First, we vary the sample selection criteria. In Column 4 of Table 3, we restrict the sample to full-time workers only. The point estimate (0.065 log points) is similar to the baseline estimate reported in column 1. This result suggests that the 1967 reform did not affect full-time and part-time workers differentially. In column 5, 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 (0.061 log points). This result shows that outliers (in particular at the bottom of the distribution) do not drive our results. In column 6, 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.[39] Finally, following Cameron et al. (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 (22). 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 are willing to work in the newly treated industries and change 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 above, the extension of the 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 appendix Figure B3a).

Wage effect in a cross-state research design. As a last robustness test, we consider another research design that leverages geographic variation in the bite of the reform. Just as 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 7 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 West and 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 three periods k, with k $\in$ [1961-1966], [1967-1972] & [1973-1980]:

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

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

[39] Together 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.

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% more than in weakly treated states just after the reform and over the period 1967-1972 (see Appendix Table E1). As in our cross-industry design, the effect is concentrated on workers with low levels of education.

4.4 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 (12%) than for white (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 columns (1) and (2) of Table 6). That is, we compare white workers in the treated industries to white workers in the control industries, before vs. after 1967 (blue line in Figure 6b). Similarly, we compare black workers in the treated industries to black workers in the control industries (dark line in Figure 6b), controlling for observables as in our benchmark specification. Strikingly, black workers in the treated industries saw their wages rise 12% 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 Appendix Figure C2). One might be concerned that the wage effect we find among black workers is confounded by other policies implemented during the 1960s (e.g. Civil Rights Era anti-discrimination policies) that primarily affected states with a large black population (particularly those in the South). We have checked that the wage response is robust to the inclusion of state-by-year fixed effects (see columns (3) and (4) of Table 6), which control for any state-specific shocks occurring over this period.

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 4). 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 downwards, which could explain why it is smaller than the predicted wage effect for this subgroup.

5 The Employment Effects of the 1967 Reform

5.1 Bunching Estimator

Methodology. We study the effect of the 1967 extension of the minimum wage on low-wage employment in the treated industries using the BLS industry wage reports. Using these data, we follow recent developments in the literature that infer employment effects from changes in bunching in the affected part of the wage distribution (Harasztosi and Lindner, 2019; Cengiz et al., 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%).[40] 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 two numbers is our estimate of the effect of the reform on sub-minimum wage employment, which we refer to as the "missing jobs" post reform. Following the notation of Cengiz et al. (2019), we denote the missing jobs post reform 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.[41] We implement this procedure within each treated industry $\times$ region cell available in the BLS data.

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, i.e., up to \$1.15 in 1967, consistent with spillover effects estimated in the literature (see, e.g., Dube et al., 2018a).[42] 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" post reform. We denote the excess jobs post

[40]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 the years 1965 and 1967.

[41]We follow Harasztosi and Lindner (2019) and develop a counterfactual distribution based on national income growth. Our approach differs from Cengiz et al. (2019) who exploit state-level minimum wage changes to construct a counterfactual evolution of the wage distribution.

[42]In Appendix D.3, we show robustness to alternative cutoffs.

reform as $\Delta a = Emp^1[MW \leq w < \bar{W}] - Emp^0[MW \leq w < \bar{W}]$.

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 then calculate and report the following employment elasticity with respect to average wage for each industry-by-region group and for all industries in the US as a whole:

$$\text{Employment elasticity wrt avg wage} = \frac{\Delta e}{\Delta w} \tag{4}$$

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.[43]

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.

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 interesting for three reasons. First, wages were very low in this industry, especially in the South: in 1963, 85% of the workforce was paid below \$1.25 (the federal minimum wage applicable in sectors covered since 1938), with a sizable share of workers paid below \$0.50 an hour. Second, black workers represented 40% of the workforce (see column (3) in Table 7) as opposed to 14% in the treated industries at the national level (see Figure 3b). Third, because southern states did not have any state minimum wage legislation, the 1967 reform was a large shock. If the 1967 extension of the minimum wage had large dis-employment effects, this should be visible in laundries in the South.

Figure 8a shows the raw hourly wage distributions in that sector and region from 1963 to 1968. In 1963 and 1966 the wage distribution is smooth, apart from spikes at round numbers, a well documented phenomenon (Kleven, 2016; Dube et al., 2018b). The shape of the wage

[43] In our data, the wage bill is calculated by taking 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. See Appendix D.3 for more details.

distributions is the same in 1963 and 1966, except that the distribution shifts to the right as the economy grew and prices increased. After the minimum wage was introduced at \$1 in 1967, by contrast, a large spike appears at \$1, indicating bunching in the earnings distribution around the minimum wage. The spike moves to the right in 1968 as the minimum wage increased to \$1.15.

The bottom panel, Figure 8b, illustrates our bunching approach. We plot the frequency distribution of wages as observed in 1967 against a counterfactual distribution with no minimum wage reform. The thin black line indicates the cumulative difference in employment between the observed and counterfactual distributions. The cumulative difference runs negative in the part of the distribution below \$1, jumps above zero at exactly \$1 and then converges to zero. The figure concisely illustrates how excess jobs appearing at or slightly above \$1 replace the missing jobs below \$1. Indeed, the area above the cumulative sum curve below \$1 represents the absolute value of the number of missing jobs ($|\Delta b|$), while the area under the curve from \$1 to to \$1.15 represents our baseline number of excess jobs (Δa).

As shown by Table 7, 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.

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).[44] Each BLS industry wage report provides data on the number of workers in fine hourly wage bins in each of these 16 treated industry $\times$ region cells.

Figure 9 plots the number of excess jobs against the number of missing jobs, both normalized by pre-treatment employment, for our 16 treated industry-by-region groups. The 45-degree line marks the points where the number of excess jobs exactly equals the number of missing jobs, i.e., where there is no effect on employment.[45] As the figure shows, across

[44]See Figure D1a. We have data for all four industries in 1967, and we have 1966 data for laundries, hotels, and restaurants. For nursing homes, pre-reform data is only available in 1965. Due to this data 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 1966. We attribute 50% of this aggregate growth to the 1965 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. We also report results excluding nursing homes from the analysis due to the rapid evolution of that industry in the late 1960s.

[45]Appendix Figure D13 shows the same plot when we assume spillovers up to 120% of the minimum wage.

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.[46] 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 across industry and region subgroups. Across all industries, the reform generated larger swings in employment around the minimum wage in the South. Nursing homes in the South were the most affected industry-by-region group, with approximately 60% of jobs (relative to pre-treatment total employment) moving from below the minimum wage to at or just above the minimum wage. Laundries in the South were second most affected, with a swing of 30%. Hotels and restaurants were less affected, but relatively more affected in the South than in other regions.

In Table 7 we report the employment elasticities implied by the missing and excess jobs plotted in Figure 9. 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 Figure 11).[47] Aggregating across sectors and regions, we find a small, slightly positive elasticity of 0.06. Elasticities are not higher in the 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 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 choice of thresholds leads to similar elasticities (with the exception of restaurants in the Midwest and Northeast).[48]

[46] A slope slightly greater than one indicates a small positive effect on employment on average.

[47] 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 1966-67 national income per capita growth rate, has wages close to the observed 1967 distribution. For example, workers paid at just under a dollar in 1966 nominal dollars may earn more than a dollar in the counterfactual, leading to a small implied effect of the reform on average wages.

[48] 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.

5.2 Employment Effects in the CPS

We supplement the bunching analysis with an investigation of the employment effects of the reform in the CPS. We use the same cross-state design as implemented for wages in section 4.3 above: we compare employment outcomes in states that had no minimum wage law as of January 1967 (strongly treated) vs. states that did (weakly treated).

We provide graphical evidence that employment outcomes evolved in parallel in strongly vs. weakly treated states before the reform.

Intensive margin. Starting with the effect of the reform on the annual number of hours worked, we estimate a difference-in-differences model similar to the one in Section 4.3, except that the outcome is log annual hours.[49]

Figure 10a shows that before 1967 annual hours evolved similarly in the strongly vs. weakly treated states. There is no detectable change following the reform, neither for white nor for black workers; see Appendix Table E3. We can rule out a decline in average hours worked of more than 3.8% over the 1967-1971 period (3.6% for black workers).[50]

Extensive margin. Next, we investigate the impact of the reform on the probability of being employed vs. unemployed.

As shown by Table 8, the reform does not appear to affect the probability of being employed vs. being unemployed in 1967-1972, with a 0 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 0.4 percentage points. Because average wages in the strongly treated states grew by 4% above and beyond wage growth in the weakly treated states, the lower bound employment elasticity with respect to the wage is -0.1 at the 95 percent confidence interval. As shown by Figure 11, this estimate is in the range of elasticities found in the minimum wage literature. The point estimate on the probability of being employed vs. unemployed or not in the labor force – an outcome that captures potential effects of the minimum wage reform on labor force participation (see Appendix Table E3, first column, second row) – is slightly positive, although not statistically different from 0. Using this metric, the lower bound employment elasticity is very similar, at -0.07.

[49] Annual hours are constructed as the ratio between annual wage (directly measured in the CPS) and the (re-constructed) hourly wage. See also footnote 34.

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

Heterogeneity by race. We estimate the model for black and white individuals separately. The results show no significant dis-employment effects for either group. As reported in Table 8, because average wages increased 12.3% for black workers in strongly treated vs. weakly treated states, the lower bound employment elasticity is -0.18 for black persons in this setting—still in the range of the elasticities found in the literature (Figure 11). The results are similar when looking at the probability of being employed vs. unemployed or not in the labor force (see Appendix Table E3, where we can rule out employment elasticities of more than -0.07 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 appears to have been effective at reducing not only the racial earnings gap (i.e., the difference in earnings between employed individuals) but also the racial income gap (i.e., including non-workers).

We also show in Appendix Table E3 that the employment elasticity (when the employment outcome is defined as the probability of being employed vs. unemployed) is not statistically significant from 0 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 vs. unemployed or not in the labor force, suggesting possible positive effects of the minimum wage reform on labor force participation in this group.

Robustness of our main cross-state design to alternative cross-state designs. We finally 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 that capture differently the variation in the intensity of the treatment across states: (i) a cross-state design using the state-level Kaitz Index in 1966 and (ii) a cross-state design that takes into account the fraction of affected workers in 1966. We provide a precise definition of these two treatment variables in Appendix E. We show that the effect of an increase of one standard deviation in the treatment variable on annual earnings and on the probability of being employed (vs. unemployed) in Table 8. 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 no effect on employment. We are able to rule out employment elasticities of more than -0.15 using the 1966 Kaitz Index measure, and more than -0.06 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 -0.24 for black persons using the 1966 Kaitz Index measure and more than -0.2 using the fraction of affected workers (see Table 8). Our results using the main cross-state design are also robust across gender groups and levels of education (see Appendix Tables E3, E4, E5).

6 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.

6.1 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, i.e., 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 (Appendix Figure G1a).

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 vs. treated industries G_b^{ct}, and the shares of black and white workers in the treatment and control industries:

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

with s_w^c (respectively 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%; and, s_b^c = 56% ; s_b^t = 44%. [51]

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

[51] See Appendix G for a derivation of the decomposition.

a counterfactual for G^t (resp. G^{ct}_b) by averaging the difference in the pre-trends 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:

$$\begin{cases} \forall k \leq 1966: G^t_{k,\text{counterfactual}} = G^t_{k,\text{observed}} \\ \forall k > 1966: G^t_{k,\text{counterfactual}} = G^c_{k,\text{observed}} - \frac{1}{N}\sum_{k=1961}^{1966}(G^c_{k,\text{observed}} - G^t_{k,\text{observed}}) \end{cases} \tag{6}$$

As shown by Figure 12, the 1967 minimum wage extension can explain around 20% of the decline in the racial earnings gap by 1980. The unadjusted racial earnings gap would have been 31 log points instead of 25 log points by 1980. 82% of this 6 log points 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 school desegregation documented by Card and Krueger (1992).[52] To what extent does our estimated contribution of coverage extension understate or overstate the 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 impact 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 5.2, the largest negative employment elasticity consistent with our results is -0.18. A portion of the reduction in the earnings gap may therefore reflect greater loss of black employment in the lower

[52] 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* (vs. annual in our calculation) wage difference between white and black workers *aged 21-60* (vs. 25-55 in our calculations), for the whole economy (vs. our treatment and control industries combined), and from 1960 to 1980 as measured in the U.S. Censuses (vs. from 1965 to 1980 measured in the CPS).

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 we significantly overestimate the contribution of the reform to the decline in racial inequality.

6.2 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 13a 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 vs. 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 post-reform.

We plot the corresponding adjusted racial gaps (i.e. γ+ β_k, k in [1961,1980]) for the control and treated industries in Figure 13b. Before the reform, and conditional on observable characteristics, white workers were paid 20%–25% 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 13a), 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 within the treated industries (see Appendix Figure C4a) 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 within the control industries (see Appendix Figure C4b). 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).

6.3 Discussion

How can we explain the large wage and small dis-employment effects of the minimum wage we obtain? In a competitive labor market, such a result is possible 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 the presence of tight labor markets, as was the case in the 1960s (Osborne, 1966; Tobin, 1965; Friedman, 1962). 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 model may be consistent with our empirical evidence in certain sectors (e.g. laundries in the South). In this model, the minimum wage increase leads to negative employment effects however if the minimum wage is set higher than wage paid by a perfect competitor – this could be the case in other sectors where we observe small dis-employment effects.[53]

How can we explain why the 1967 reform did not seem to hurt black workers more than white workers? One hypothesis is that before the reform, white employers colluded to pay black workers low wages in at least some of the treated industries and some regions (for example, laundries in the South). In the standard Becker (1957) model, taste-based discrimination is competed away if there are enough non-discriminating employers. However, in the context of agriculture, laundries, nursing homes, and other treated industries pre-1967, it is possible that there was no such competition but instead collective discrimination. Studying textile manufacturing in South Carolina in the mid-1960s, Heckman and Payner (1989) document a significant increase in the employment share of black workers following the introduction of federal anti-discrimination policy. They note that from 1915 to 1965, black workers had been excluded from the main operative and craftsman occupations of manufacturing in South Carolina by Jim Crow laws. There was white collusion to exclude black workers from employment. Our hypothesis is that a similar mechanism was at play in the treated industries, but affecting wages rather than quantities of labor employed as in Heckman and Payner (1989). This hypothesis potentially explains why wages rose sharply in 1967, but employment did not fall for black workers relative to white workers.

7 Conclusion

This paper 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 Fair Labor Standards Act 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 over-represented) and the rest of the economy; second, by reducing the racial earnings gap within the treated industries, as the wages of black workers increased faster than those

[53] Understanding the price responses to the 1967 reform on product markets could be used in theory to understand the importance of monopsony power in these sectors and regions during this historical period (Aaronson et al., 2008). However, there is a lack of data on sectoral prices by states during these years. Neither the Bureau of Economic Analysis nor the Bureau of Labor Statistics collected price indices at the state $\times$ sector level in a systematic way in the 1960s and 1970s.

of white workers. We can rule out large dis-employment 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 paper provides the first causal evidence on how minimum wage policy affects racial 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 over-represented 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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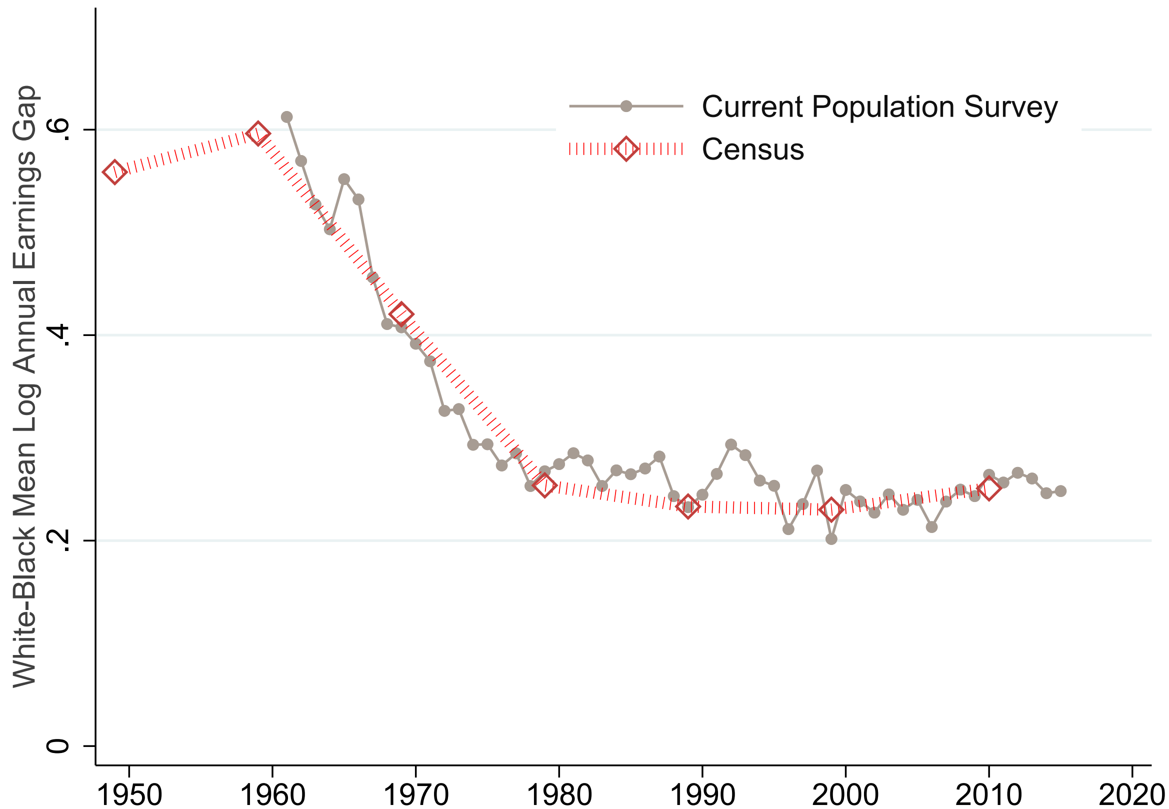
Figure 1: Economy-wide white-black unadjusted wage gap in the long-run, in the CPS and in the decennial Censuses

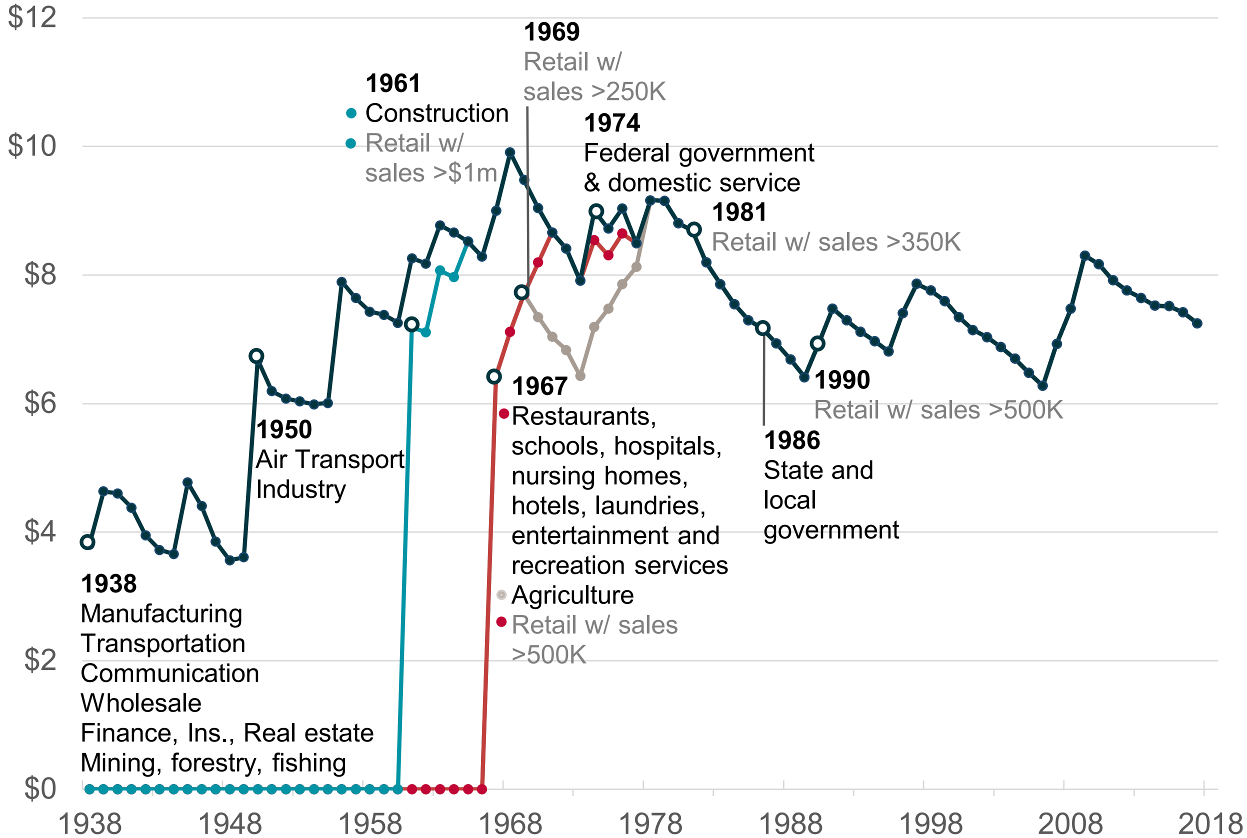


Source: Annual Social and Economic Supplement of the Current Population Survey, 1962-2016; US Census from 1950 to 2000 and American Community Survey data in 2010 and 2017.

Sample: Adults 25-65, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: 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 and the censuses collect information on earnings received during the previous calendar year. Therefore, we report estimates of the racial gap calculated using the 1950 Census in the year 1949 above and estimates calculated using the 1962 CPS in the year 1961. For the ACS, the reference period is the past 12 months, and we report estimates of the racial gap calculated using the ACS 2010 and 2017 in the current year. The economy-wide racial gap is defined here as the combination between the industries covered in 1938 and the industries covered in 1967. Annual earnings in $2017, deflated using annual CPI-U-RS series.

Figure 2: Expansions in minimum wage coverage, and real values of the minimum wage 1938-2018 ($2017)

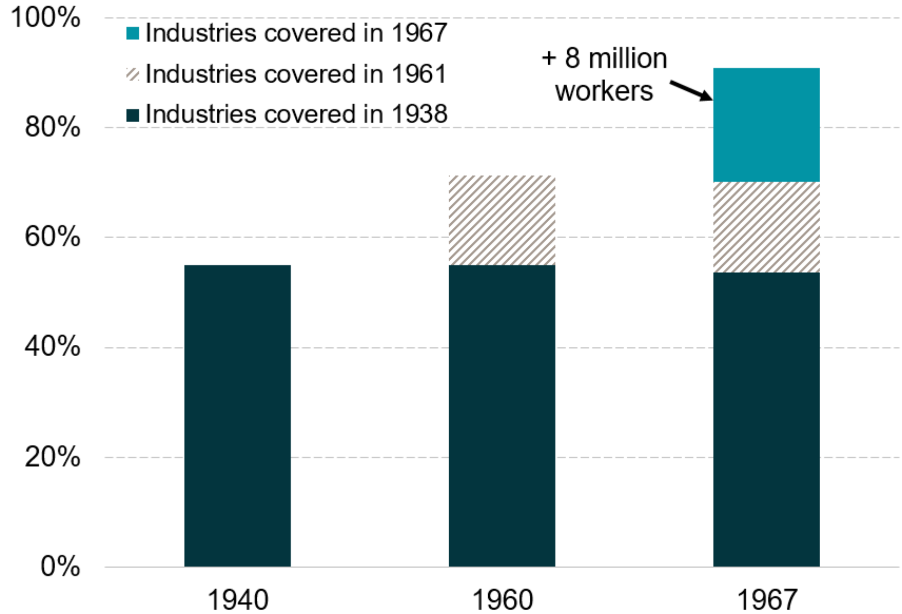


Source: For the breakdown by industry: see our analysis of the Fair Labor Standards Act in Appendix A. For the values of the minimum wage, see Department of Labor, Wage and Hour Division, History of Federal Minimum Wage Rates Under the Fair Labor Standards Act, 1938-2009, available at: `https://www.dol.gov/whd/minwage/chart.htm`.

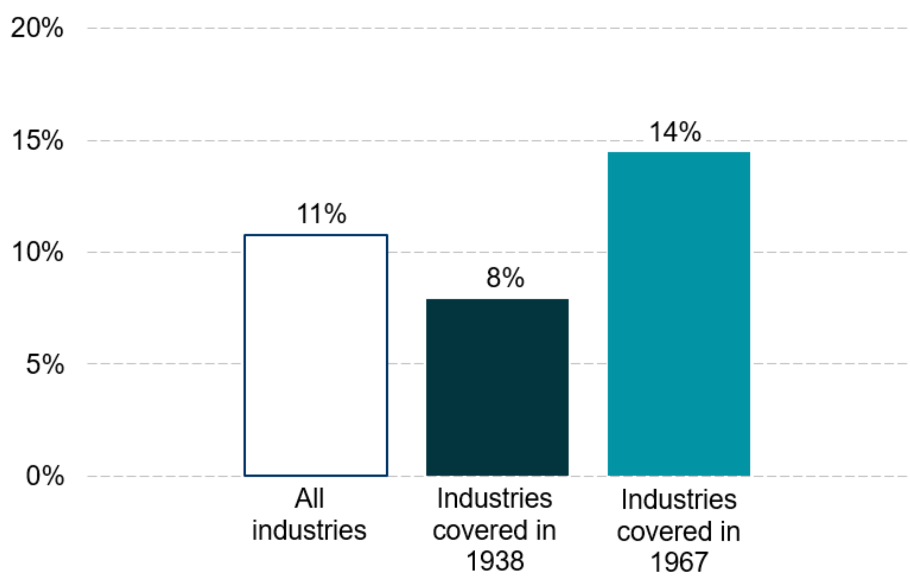
Notes: The 1938 Fair Labor Standards Act introduced the federal minimum wage in manufacturing, transportation, communication, wholesale trade, finance, insurance and real estate, mining forestry and fishing. In 1950, the federal minimum wage was expanded to the air transport industry. In 1961, the minimum 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 increases gradually over the following years. Minimum wages series are deflated using CPI-U-RS ($2017).

Figure 3: Share of workers covered by the minimum wage

(a) By industry, 1940-1967



(b) By fraction black, in 1967

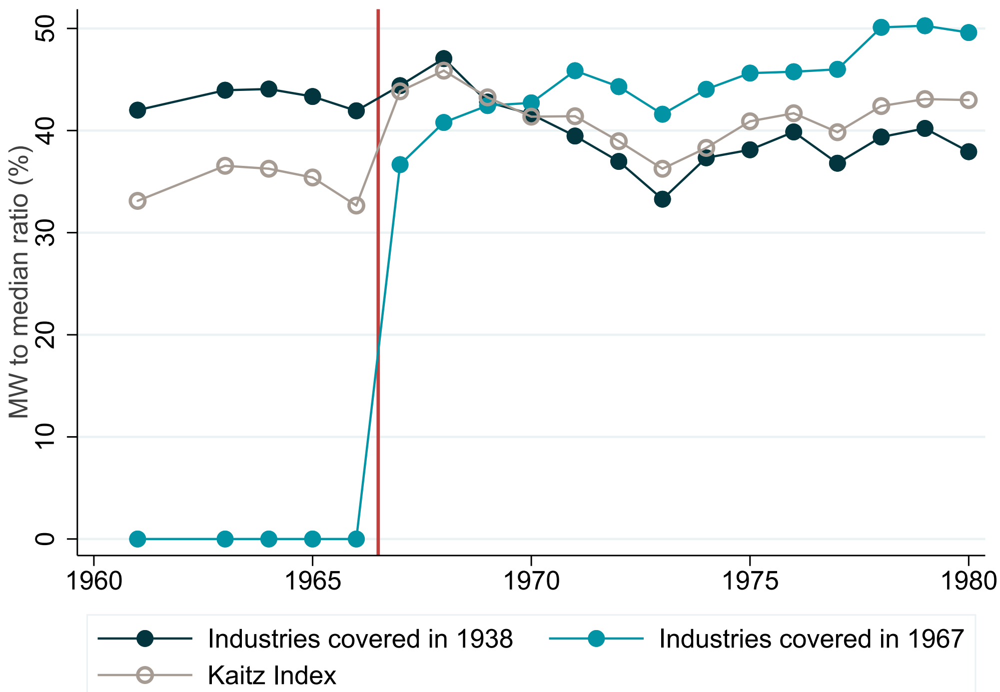


Sources: US Censuses 1940 and 1960. March CPS 1967.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: Coverage by federal minimum wage. For the values of the federal 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.

Figure 4: Minimum wage to median ratio

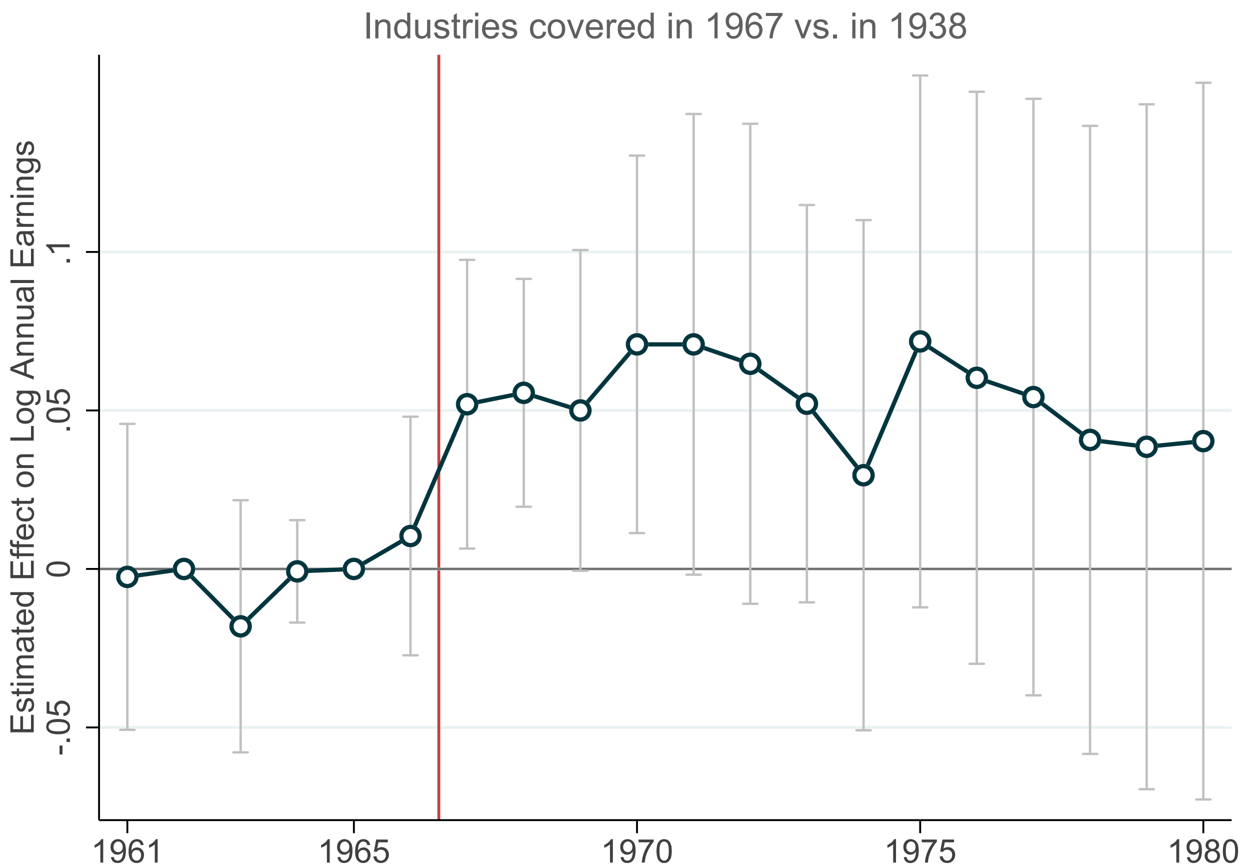


Source: March CPS 1962-1981 for median wages. For the values of the minimum wage, see Department of Labor, Wage and Hour Division, History of Federal Minimum Wage Rates Under the Fair Labor Standards Act, 1938-2009, available at: https://www.dol.gov/whd/minwage/chart.htm.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: Minimum wage legislation at the federal level. Industries covered in 1967, except agriculture. Full-time (40 hours a week), full-year (52 weeks workers per year) MW to median ratio. The medians are calculated separately for the industries covered in 1938 and the industries covered in 1967. The Kaitz Index is defined here as the weighted federal minimum wage to median ratio using industry composition of the economy. It writes: Kaitz Index$_y = \sum_j \frac{N_{yj}}{N_y} * \frac{\text{min.wage}_{yj}}{\text{median wage economy}}$, with N_{yj} as the number of workers working full-time full-year in our sample by industry type j (i.e. either industries covered in 1938 or industries covered in 1967), N_y as the number of workers working full-time full-year in all industries in each year y, min.wage$_{yj}$ as the min. wage law that applies at the federal level in industry type j, in each year y, and "median wage economy" as the economy-wide median wage for full-time full-year workers in our sample.

Figure 5: Impact of the 1967 reform on annual earnings



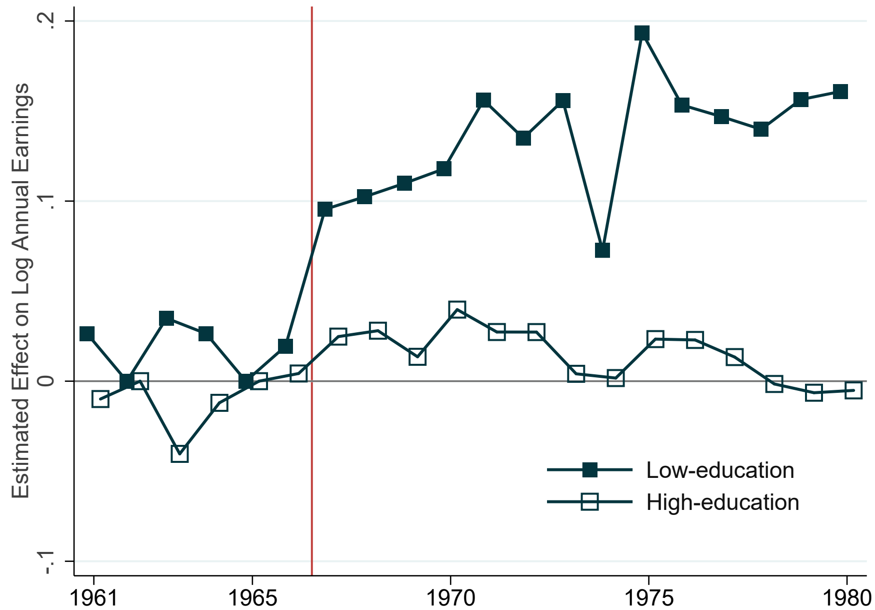
Source: March CPS 1962-1981.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

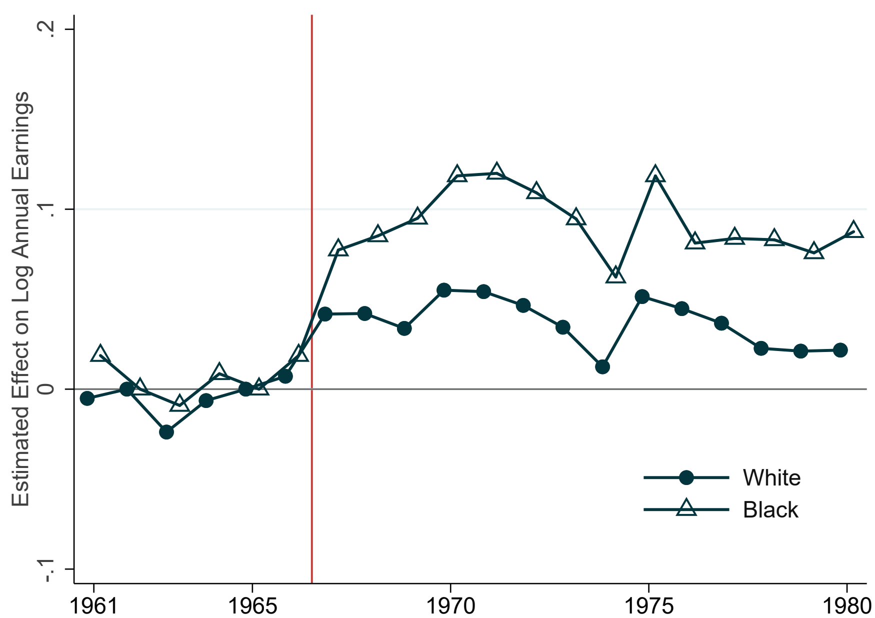
Notes: This regression uses a cross-industry design and controls for gender, race, years of schooling, a cubic in experience, full-time/part-time status, number of weeks and hours worked, occupation and marital status. Includes industry and time fixed effects. Because the CPS collects information on earnings received during the previous calendar year, we report estimates of the wage effect calculated in the 1962 CPS in the year 1961 above. The ear 1962 is excluded and set to zero. Standard errors are clustered at the industry level. Annual earnings in $2017, deflated using annual CPI-U-RS series.

Figure 6: Heterogeneity in the wage effect of the 1967 reform

(a) By level of education



(b) By race

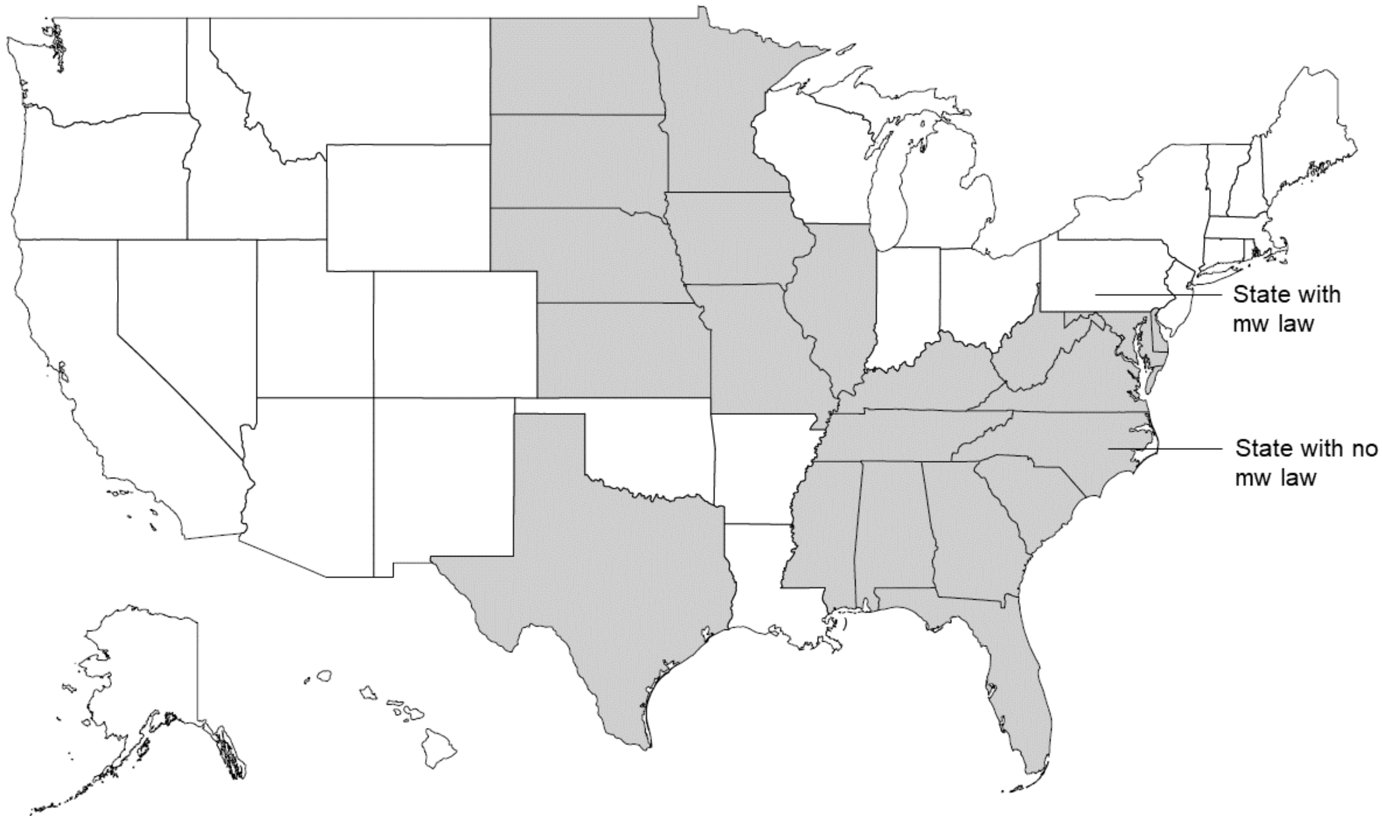


Source: March CPS 1962-1981.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: 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. Because the CPS collects information on earnings received during the previous calendar year, we report estimates of the wage effect calculated in the 1962 CPS in the year 1961 above. The year 1962 is excluded and set to zero. Standard errors are clustered at the industry level. Annual earnings in $2017, deflated using annual CPI-U-RS series.

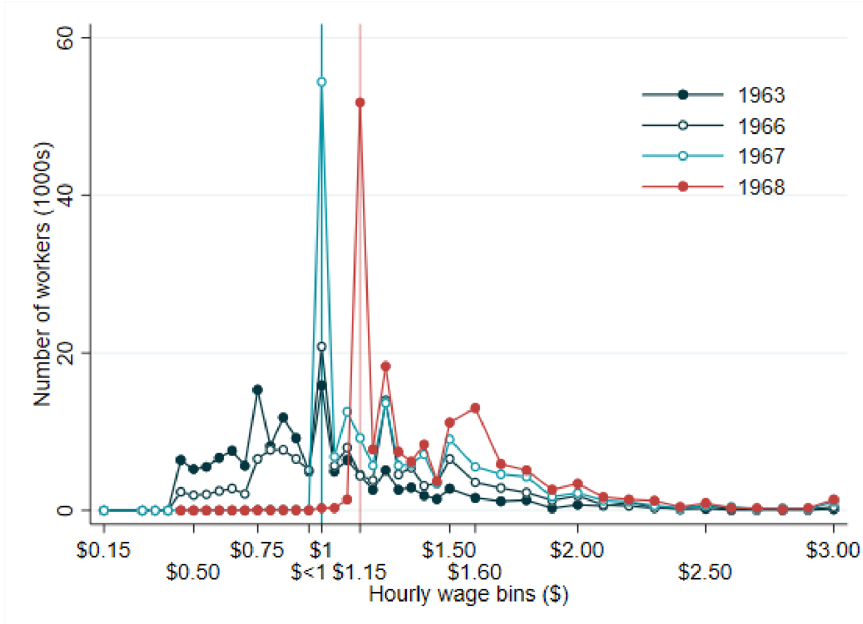
Figure 7: States with no minimum wage laws as of January 1966



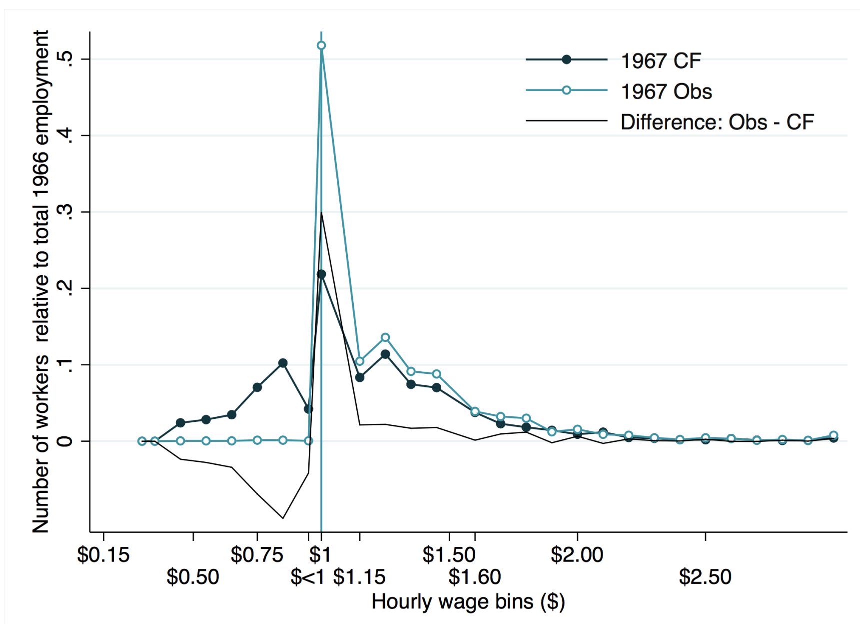
Source: Authors' minimum wage database 1950-2016. More details provided in Appendices A and E.
Note: The strongly treated state groups are the following ones: Florida, Illinois, Texas, Alabama-Mississippi, North Carolina-South Carolina-Georgia, Kentucky-Tennessee, Iowa-North Dakota-South Dakota-Nebraska-Kansas-Minnesota-Missouri,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

Figure 8: Case study: laundries earnings distributions in the South

(a) Observed distributions from 1963 to 1968



(b) Distributions used in the bunching estimation

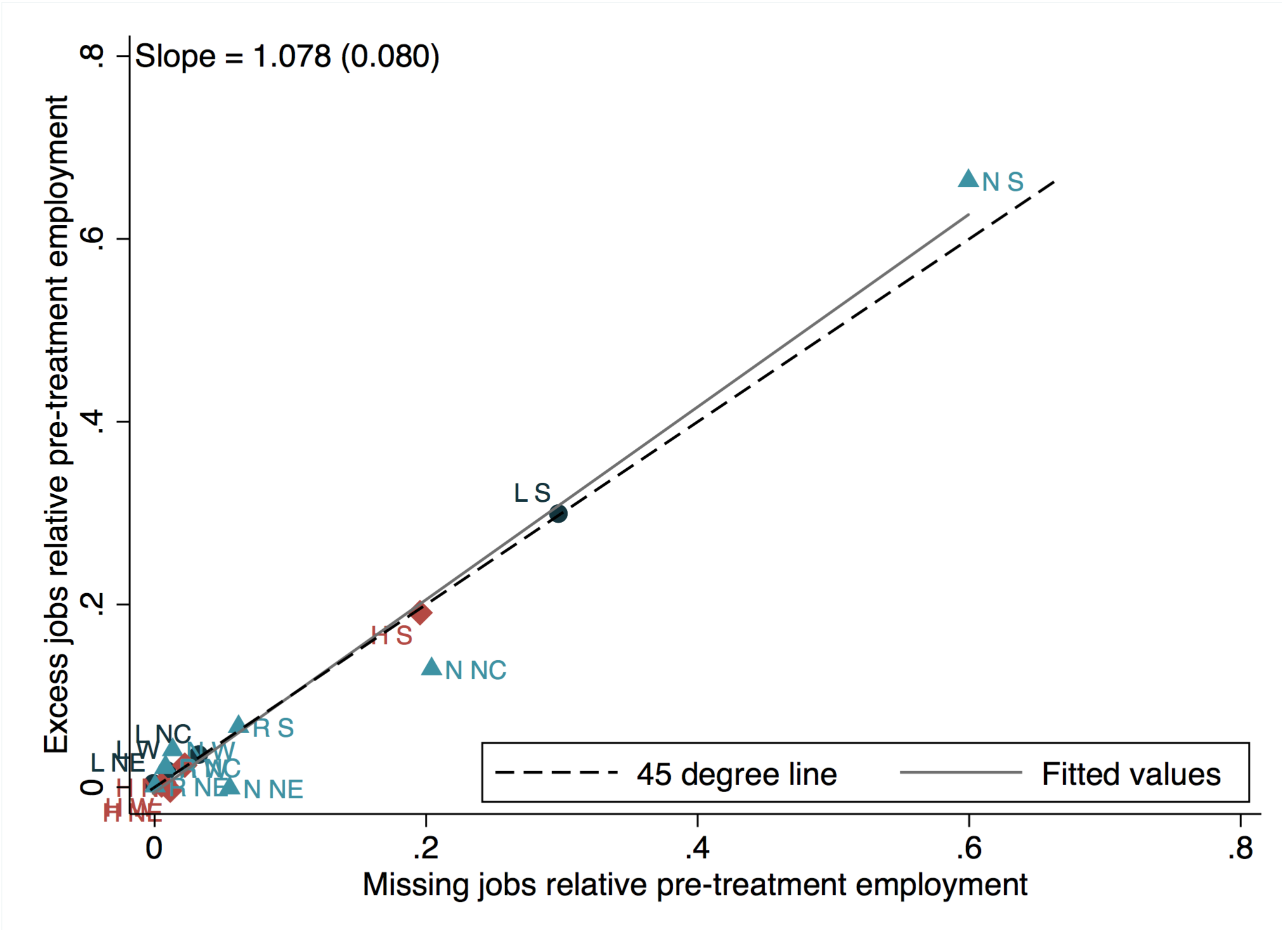


Source: BLS Industry Wage Reports.

Sample: All nonsupervisory workers.

Notes: The minimum wage was introduced at $1 in 1967. It reached $1.15 in 1968. In panel (b), the light blue line (with dots) corresponds to the observed 1967 hourly wage distribution in laundries in the South. The dark blue line (with dots) corresponds to the 1967 counterfactual hourly wage distribution. The counterfactual distribution is constructed by inflating the observed 1966 hourly wage distribution by the 1966-67 national income per capita growth rate (+ 4.4%); see section 5.1 and Appendix D for more details. The dark line corresponds to the difference in the number of workers between the 1967 observed and the 1967 counterfactual hourly wage distributions in each wage bin.

Figure 9: Missing and excess jobs in the BLS industry wage reports



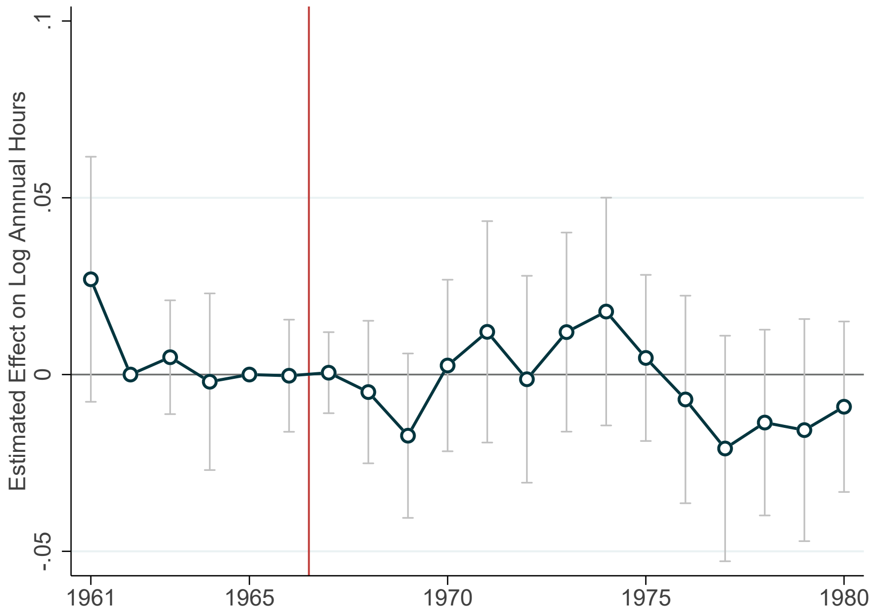
Source: BLS Industry Wage Reports.

Sample: All nonsupervisory workers, except routemen, in laundries; all non-tipped, nonsupervisory employees in year-round hotels, motels and tourist courts. The minimum wage is introduced at $1 in nominal terms in 1967.

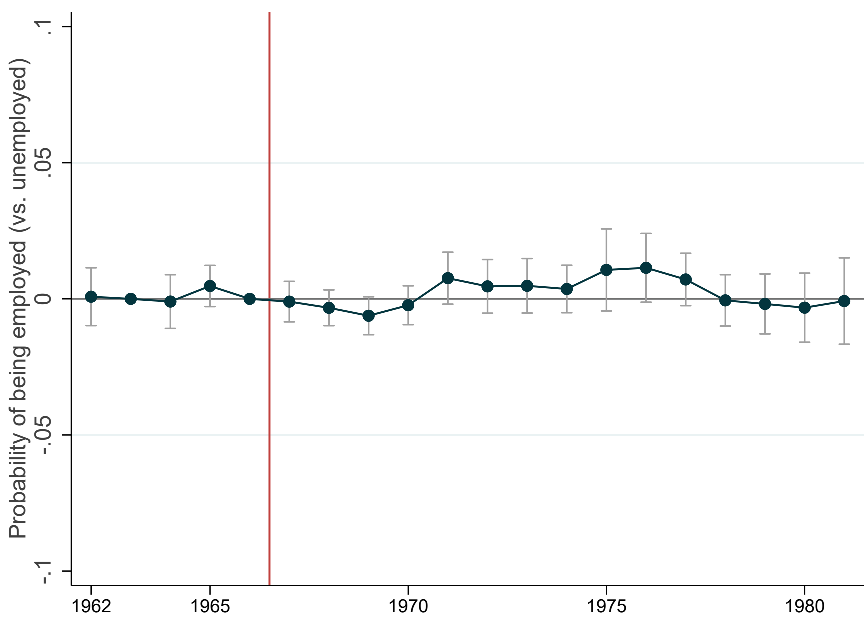
Notes: This figure shows the excess jobs (relative to pre-treatment total employment in that cell) above the new minimum wage and the magnitude of missing jobs below for different industry-region cells. The black dashed line is the 45-degree line where the number of excess jobs exactly equals the number of missing jobs, indicating a zero employment effect. Points above the line indicate positive employment effects while points below the line indicate negative employment effects. Missing and excess jobs are plotted for laundries (L), hotels (H), and restaurants (R) in the South (S), Midwest (denoted "NC" for "North Central" as in the original BLS reports), Northeast (NE), and West (W) regions.

Figure 10: Impact of the 1966 FLSA on employment

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

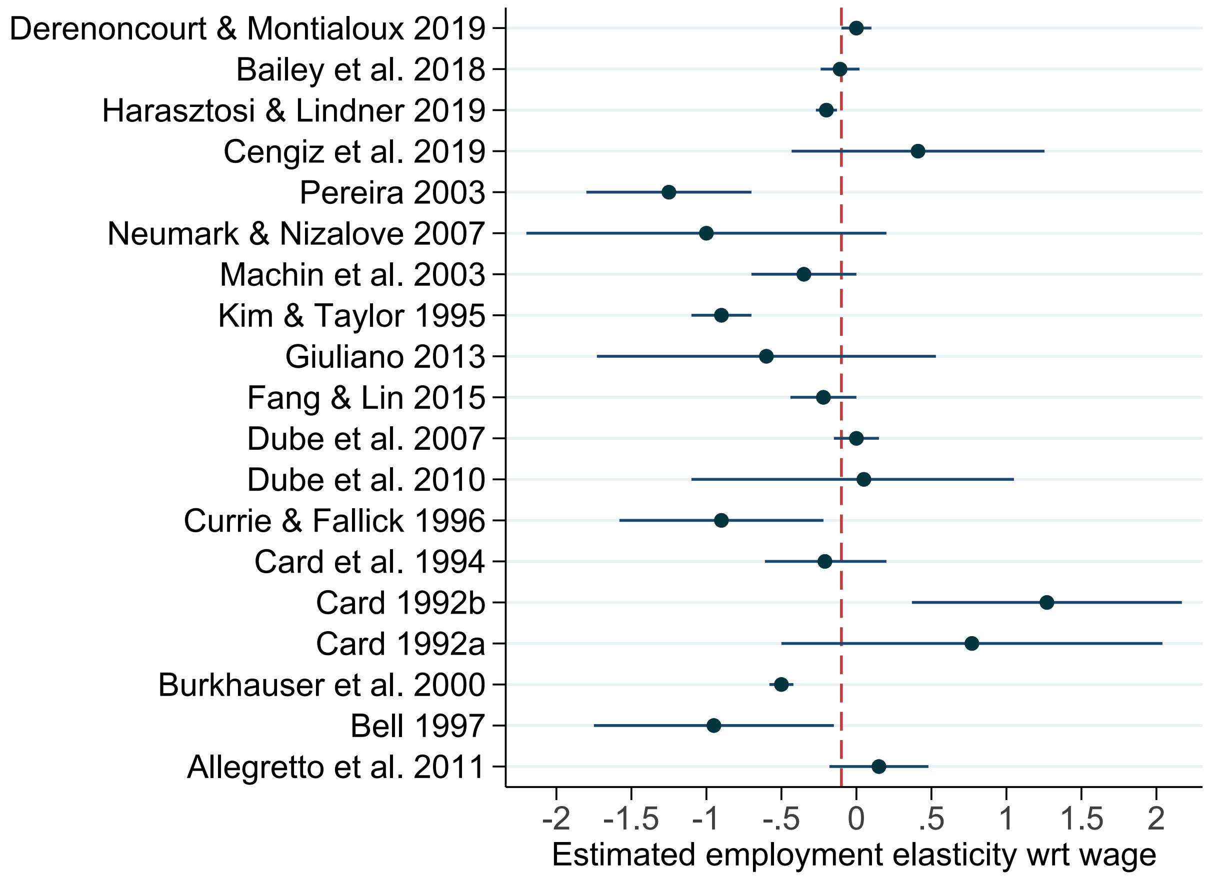


(b) Impact on probability of being employed (vs. unemployed) (extensive margin)



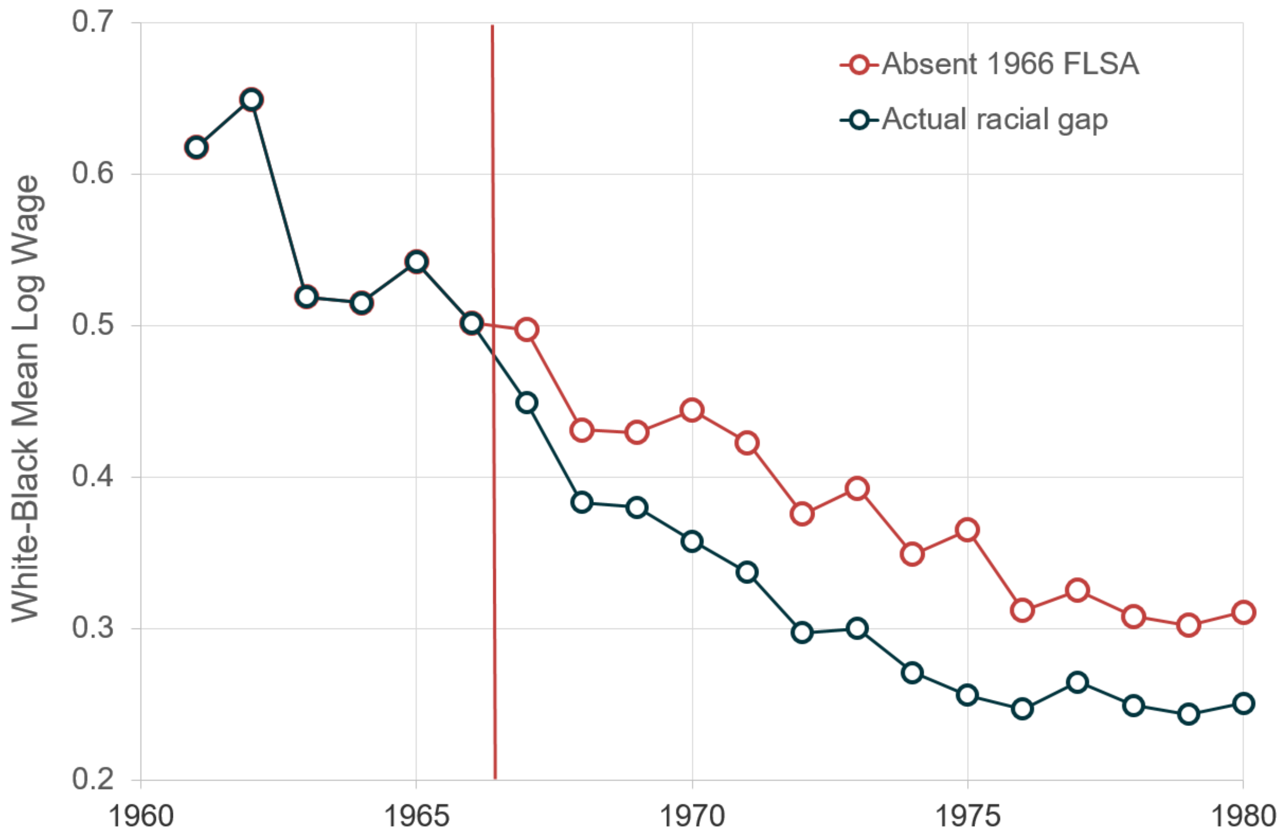
Source: March CPS 1962-1981.
Sample: Panel (a): Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. Panel (b): Adults 25-55, black or white, employed or unemployed.
Notes: 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)).

Figure 11: Employment elasticities wrt wage in the literature and in this paper



Notes: This figure summarizes the estimated employment elasticities with respect to average wage and compares it to the previous literature. The estimates in the literature were collected by Harasztosi and Lindner (2019). We add our baseline CPS employment estimate, as well as estimates in Bailey et al. (2018) (Table 3, column (3)) and Cengiz et al. (2019) (Table 1, column (1)). The dashed vertical line shows the lower bound of our benchmark estimate.

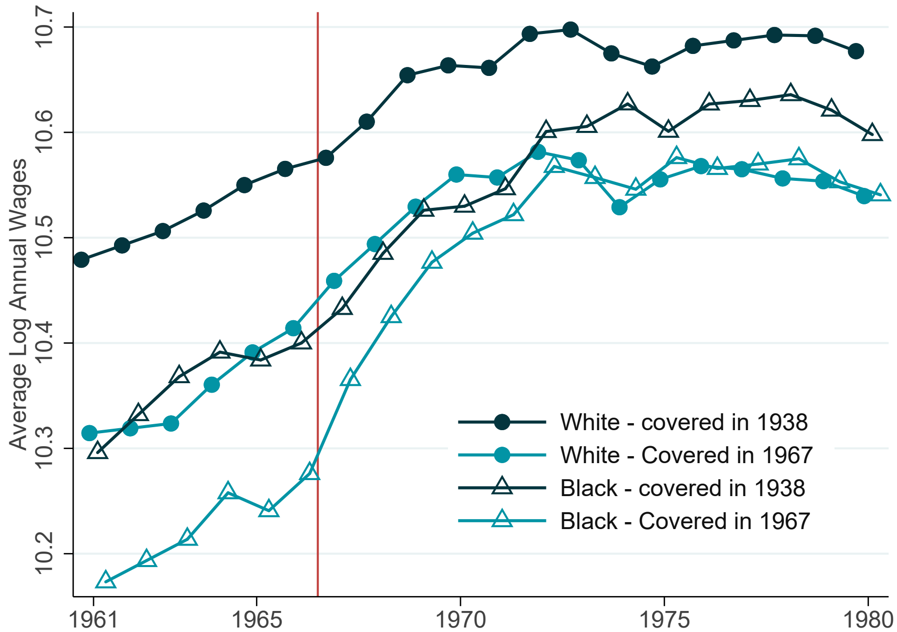
Figure 12: 1967 reform reduced economy-wide racial gap by $\sim 20\%$



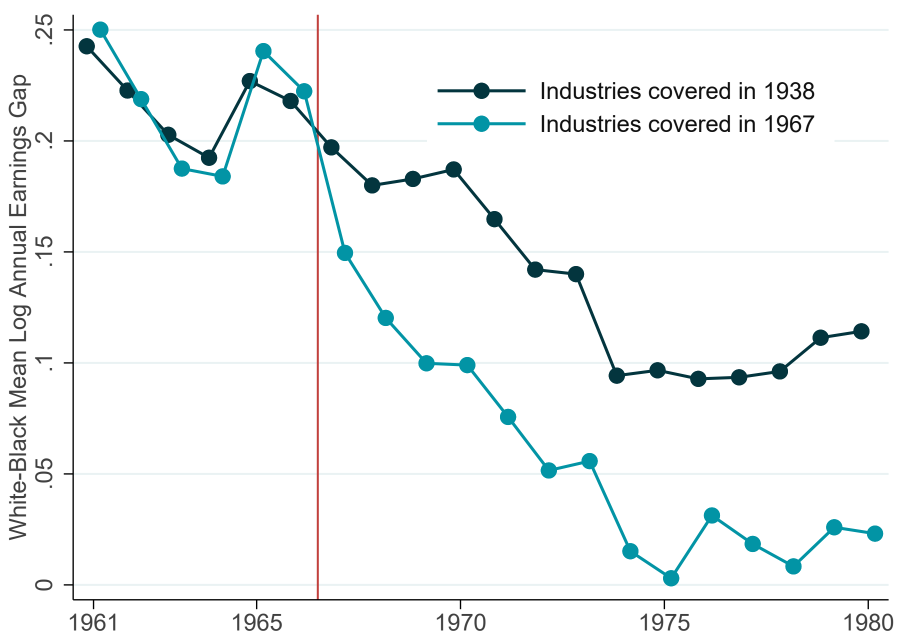
Source: March CPS 1962-1981.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: The racial gap is calculated as the difference in the average 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 e.g. in the 1962 CPS in the year 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. Annual earnings in $2017, deflated using annual CPI-U-RS series.

Figure 13: Adjusted racial wage gaps

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



(b) Adjusted racial earnings gaps, by treatment status



Source: March CPS 1962-1981.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: Racial earnings gap measures 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, 12 years of schooling, married, professional and technical occupation, working full-time full-year. In the bottom panel, the reference category is male workers working full time, 12 years of schooling, 5 years of experience, and working in Business and Repair Services. Annual earnings in $2017, deflated using annual CPI-U-RS series.

Table 1: Employment and earnings by race, 1967

	Employment		Employment shares		Earnings ($2017)	
	Number	Percent	White	Black	White	Black
All industries	38,490,848	1.00	0.89	0.11	42,575	24,522
Industries covered by 1938 FLSA	20,663,098	0.54	0.92	0.08	46,469	29,174
Manufacturing	13,134,427	0.34	0.91	0.09	45,622	30,322
Transportation	2,960,552	0.08	0.93	0.07	47,750	28,620
Finance, Insurance	1,783,952	0.05	0.96	0.04	46,021	22,923
Wholesale Trade	1,445,985	0.04	0.94	0.06	53,229	25,547
Business, Repair	921,756	0.02	0.90	0.10	44,334	23,764
Mining	377,885	0.01	0.97	0.03	47,433	35,444
Forestry, fishing	38,539	0.00	0.83	0.17	34,261	15,804
Industries covered by 1961 FLSA	6,336,330	0.16	0.92	0.08	39,854	23,701
Retail trade	3,961,711	0.10	0.93	0.07	35,438	24,463
Construction	2,374,619	0.06	0.89	0.11	47,520	22,868
Industries covered by 1966 FLSA	7,962,920	0.21	0.86	0.14	33,435	21,405
Schools	2,913,630	0.08	0.90	0.10	38,560	30,513
Nursing homes	1,419,030	0.04	0.91	0.09	37,928	23,684
Hospitals	1,260,220	0.03	0.79	0.21	27,767	20,939
Hotels, laundries	741,447	0.02	0.76	0.24	25,581	16,667
Restaurants	777,805	0.02	0.86	0.14	22,344	15,777
Agriculture	599,313	0.02	0.75	0.25	24,406	11,685
Entertainment	251,475	0.01	0.87	0.13	44,099	22,524
Public Administration	2,848,719	0.07	0.87	0.13	46,944	35,436
Domestic service	679,782	0.02	0.31	0.69	10,054	8,381

Source: 1967 March CPS.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: Employment numbers and employment shares refer to the year 1967. Because the CPS collects information on earnings received during the previous calendar year, annual earnings reported in this table were earned in 1966. Annual earnings in $2017, deflated using annual CPI-U-RS series.

Table 2: 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.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: 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 in $2017, deflated using annual CPI-U-RS series. The other demographic characteristics were collected in 1966-67.

Table 3: Wage effect: main results and robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × 1967-1972	0.065**	0.059**	0.056**	0.065**	0.063**	0.065**
	(0.025)	(0.024)	(0.022)	(0.023)	(0.023)	(0.029)
Obs	407,823	407,823	401,171	375,393	407,823	407,823
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	Y	N	N	N	N
State linear trends	N	Y	N	N	N	N
W/o agriculture	N	N	Y	N	N	N
Full-Time only	N	N	N	Y	N	N
Winsorized data	N	N	N	N	Y	N
2-way clusters	N	N	N	N	N	Y

Source: March CPS 1962-1981.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: The outcome variable is log annual earnings. Annual earnings in $2017, deflated using annual CPI-U-RS series. Individual-level controls 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. In column (5), log annual earnings as well as individual-level controls have been winsorized at the 5% level. In columns (1)-(5), standard errors are clustered at the industry level. In column (6), standard errors are clustered at the industry and state levels.

Table 4: Predicted wage effect

	(1)	(2)	(3) = (1) × (2)	(4)
	Share of workers at or below the MW (%)	Avg increase in earnings for MW workers (%)	Predicted increase in earnings (%)	Estimated increase in earnings (%)
All	16.1	33.5	5.4	5.3
By education				
Low-education	31.4	33.0	10.4	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, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: Share of minimum wage workers = workers at or below the 1967 minimum wage. Estimates in col. (3) and (4) are for 1967 only.

Table 5: 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	0.110***	0.112***	0.089***	0.081***
	(0.034)	(0.027)	(0.029)	(0.019)
1967-1969 × South			0.092***	0.136**
			(0.032)	(0.049)
Obs	167	89	167	89
Time FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Region FE	Y	Y	Y	Y

Source: BLS Industry Wage Reports. See Appendix Figure D1a for the set of tabulations digitized.
Sample: All nonsupervisory employees.
Notes: the "full" sample contains industries listed in figure D1a. The "strict" sample excludes movie theaters and schools (only available pre- or post-reform) as well as years 1961-62, 1964, and 1966 where only treatment or control industries are available. Standard errors are clustered at the industry level.

Table 6: Wage effect by race

	Baseline		Robustness check	
	Black	White	Black	White
Covered in 1967 ×				
1967-1972	0.095***	0.054**	0.074**	0.048**
	(0.022)	(0.023)	(0.030)	(0.022)
1973-1980	0.078*	0.036	0.043	0.035
	(0.037)	(0.042)	(0.043)	(0.041)
Obs	37,770	370,053	36,895	370,053
Controls	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
State FE	N	N	Y	Y
State-by-year FE	N	N	Y	Y

Source: March CPS 1962-1981.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: The outcome variable is log annual earnings. Annual earnings in $2017, deflated using annual CPI-U-RS series. 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.

Table 7: Employment elasticities by industry and region using baseline bunching methodology

	Employment counts	Workers below $1 (Percent)	Black share (Percent)	Emp. elasticity wrt average wage 1.15 ×MW	Emp. elasticity wrt average wage 1.20 ×MW
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					
U.S.	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.

Sample: All industries are composed of laundries, restaurants (non-tipped workers) and hotels (non-tipped workers), and nursing homes.

Notes: 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 3 digits codes, as opposed to 2 digits codes in March CPS 1962-1967) to separate out e.g. 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 5.1 and equation 4).

Table 8: 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									
Employment	0.000	-0.008	0.000	-0.000	-0.005	0.000	0.001	-0.006*	0.001
	(0.002)	(0.007)	(0.002)	(0.001)	(0.004)	(0.001)	(0.001)	(0.003)	(0.001)
	693,449	65,939	627,510	693,088	65,851	627,237	693,088	65,851	627,237
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)
	534,977	51,666	483,311	534,798	51,615	483,183	534,798	51,615	483,183
Emp. elasticity	0.00	-0.07	0.02	-0.02	-0.10	0.06	0.03	-0.10	0.09
se	(0.05)	(0.06)	(0.08)	(0.07)	(0.07)	(0.17)	(0.05)	(0.05)	(0.08)
lower bound	-0.10	-0.18	-0.15	-0.15	-0.24	-0.27	-0.06	-0.20	-0.07
upper bound	0.10	0.04	0.18	0.11	0.04	0.39	0.12	0.01	0.25
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.

Sample: For regression on probability of being employed vs. unemployed: Adults 25-55, black or white, employed or unemployed. For regression on log annual earnings: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: The three treatment variables used are respectively: strongly treated state vs. 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 Appendix E. The effect on employment and earnings using the two alternative designs is the effect of one standard deviation increase in the treatment variable. For the design using the 1966 Kaitz index, the mean is 0.35, 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, 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 square 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.

Online Appendix

[NOT FOR PUBLICATION]

Appendix A Minimum Wage Database (1950-2017)

Content and access. We contribute a new minimum wage database for the United States at the state, industry and gender level. We believe this database improves previously released minimum wage databases[54] in three ways: (i) it starts in 1950, allowing for greater historical depth in the study of minimum wage effects than before;[55] (ii) it includes the information on minimum wage rates not only for the industries covered by the initial 1938 Fair Labor Standards Act, but also separately for the industries covered by subsequent amendments (1961, 1966, and 1974). Therefore, the minimum wage rates are industry-specific,[56] and this is particularly relevant for the period 1950-1974 ; (iii) it includes gender-specific minimum wage rates. This variation is also particularly relevant before 1980, after which minimum wage legislation no longer varies by gender. We build the database in nominal terms at the monthly level, then collapse it to the annual level. [57] We hope this database will help foster future research on the long-run evolution of minimum wages.

Sources. *Federal level*. The minimum hourly wage rates for employees covered by the 1938 Fair Labor Standards Act, the 1961 amendments, and the 1966 and subsequent amendments at the federal level are taken from the Department of Labor website.[58]

State-level. The minimum hourly wage rates at the state level are taken from different sources, depending on the period of interest. From 1950 to 1980, we use tables published in the Report of the Minimum Wage Study Commission (1981) to get information on the minimum wage at the state, industry and gender level.[59] We digitize and analyze in particular the information contained in Volume II, "State Minimum Wage Laws, 1950-1980," written

[54] There are, to our knowledge, two main published minimum wage databases for research purposes: (i) Vaghul and Zipperer (2016) dataset (1974-2016) (available at `https://github.com/equitablegrowth/VZ_historicalminwage/releases`) and (ii) Neumark (2018) dataset (1960-2017) (available at `http://www.economics.uci.edu/~dneumark/datasets.html`)

[55] Vaghul and Zipperer (2016) starts in May 1974 and Neumark (2018) in 1960.

[56] The industry classification used in the database is the one of the March CPS. See Appendix B for more details.

[57] Both databases and Stata do-files used to create them are available at `http://clairemontialoux.com/flsa`.

[58] See Department of Labor, Wage and Hour Division, History of Federal Minimum Wage Rates Under the Fair Labor Standards Act, 1938-2009: `https://www.dol.gov/whd/minwage/chart.htm`.

[59] Volumes I & II are available at: `https://babel.hathitrust.org/cgi/pt?id=uiug.30112011667935;view=1up;seq=21` All other volumes are available from: `https://catalog.hathitrust.org/Record/001304563`.

by Aline O. Quester, Appendix Table 1A "State Minimum Wage Laws, 1950-80" (pp.32-121), Appendix Table 3A "Basic State Minimum Wage as a Fraction of Basic Federal Minimum Wage, 1950-1980" (pp.129-141) and Appendix Table 4A "New York State Minimum Wage Law" (pp.142-152). The coverage and exemption rules of the Fair Labor Standards Amendments we use are detailed in Appendix Table 2A (pp.122-128). Starting in 1980, we use the minimum wage dataset produced by Vaghul and Zipperer (2016). We update the values of the state minimum wage in 2017 using Neumark (2018).

Classification of industries by date of FLSA coverage. Which industries were covered by each subsequent amendment of the Fair Labor Standards Act? Appendix Table A1 shows the list of industries available in CPS 1962-1981 in the first column, and how we classify them in terms of coverage by the Fair Labor Standards Act and its amendments (1961, 1966, 1974 and 1986) in the second column.[60] This classification is necessarily imperfect due to the complexity of the minimum wage legislation on the one hand and the characteristics we can or cannot observe in the CPS on the other hand.[61] Our objective is to make the best possible choices given these constraints. We clarify our choices below. This classification of industries is important for our analysis as our empirical strategy relies on the comparison between previously covered industries (covered in 1938) and newly covered industries (covered in 1967). Our main results are robust to slight changes in this classification.

The 1938 Fair Labor Standards Act stipulated that the minimum wage should be applied to "employees engaged in interstate commerce or engaged in the production of goods destined for the interstate commerce." Drawing on these lines, together with the list of exemptions specified in the law,[62] we consider the following industries covered by the 1938 FLSA: mining, manufacturing (durable and non-durable), transportation, communication and other utilities,[63] wholesale trade, finance, insurance and real estate, and business and repair services.

[60] FLSA as amended available at: `https://www.dol.gov/whd/regs/statutes/FairLaborStandAct.pdf`.

[61] Minimum wage legislation varies not only by industry, but also, in the retail sector, by a sales threshold per establishment (see below paragraph on 1961 Amendments). The legislation also differs by workers' overtime status, age, etc.

[62] For a full list of exemptions, see: Appendix Table 2A p.122 in Report of the Minimum Wage Study Commission (1981), Volume II. Note that the list of exemptions to the minimum wage has evolved over time. In particular, the 1949 Amendments, effective January 1950 expanded exemptions to laundry and dry cleaning establishments and retail and service establishments.

[63] A minority of workers in transportation were, however, not covered by the 1938 FLSA. Some transportation workers, originally not covered, became covered before the period we analyze, and it is therefore appropriate to include them in the control group. This is the case of employees of air carriers who were covered in 1950. Other transportation workers were excluded from coverage even after our CPS analysis period begins, including workers transporting fruits and vegetables from farm to first processing, or those transporting other workers to and from farms for harvesting purposes. Because these workers represent a minority of transportation workers

These industries form our control group.

The 1961 Amendments to the Fair Labor Standards Act extended coverage to all employees of retail trade enterprises[64] with sales over $1m and to small retailers under certain conditions.[65] They also increased coverage to construction enterprises with sales over $350,000. Retail trade establishments and construction were therefore only partially covered in 1961 and were further affected by the 1966 and subsequent amendments.[66] Because we do not have information in the CPS on the sales amount realized by the enterprise the worker is employed in, we are not able to identify retail trade or construction workers affected by the 1961 amendments versus by later amendments. We must therefore make a choice about how to classify retail trade and construction workers as a whole. Because the 1961 amendments were the most important in terms of coverage extension for both of these types of workers, we classify retail trade and construction workers as treated in 1961. Retail trade and construction workers are therefore excluded from our main analysis that compares industries covered in 1938 to industries covered in 1967.[67]

The 1966 Amendments to the Fair Labor Standards Act extended coverage to enterprises engaged in "a common business practice" that included hospitals and institutions engaged in the care of the sick, aged, mentally ill or physically handicapped; elementary and secondary schools, whether public or private[68]; agriculture; and service enterprises with sales above $500,000. We therefore categorize the following industries as covered by the 1966 amendments: agriculture, restaurants, hotels, laundries and other personal services, entertainment and recreation services, nursing homes, and other professional services, hospitals, schools and other educational services. Below, we discuss where we had to make choices and the

and we are not able to identify them in the CPS data, we believe this approximation is not a threat to our empirical strategy.

[64]Here, retail trade excludes eating and drinking places that were specifically exempted from the minimum wage in 1961.

[65] Small retailers are covered if (i) less than 50% of their sales are within state, (ii) more than 75% of their sales are for resale, or (iii) less than 75% of their sales are retail.

[66]The 1966 amendments extended coverage to retail trade enterprises with sales over $500,000. In 1969, this threshold was reduced to $250,000. It was further increased to $350,000 in 1981, and to $500,000 in 1990. See p.25 in Neumark and Washer (2008) for a history of minimum wage laws in the retail sector. The $500,000 threshold is still in place today, see Department of Labor website: `https://www.dol.gov/whd/regs/compliance/whdfs6.pdf`.

[67] 50% of all retail trade became covered in 1961, 24% were covered by the 1966 amendments and the remaining 26% were covered later. Source: see Table 2. p. 22 in Minimum Wage and Maximum Hours Standards Under the Fair Labor Standards Act (1973), Survey conducted by the Labor Statistics for the Employment Standards Administration.

[68] The 1972 higher Education Act extended the minimum wage coverage to "preschools" (representing roughly 150,000 individuals), see p.126 of the Report of the Minimum Wage Study Commission (1981), Volume II.

strengths and limitations of these choices.

Agriculture. Agriculture was covered for the first time in 1967. However, some exemptions applied in the agricultural sector, mainly for small farms.[69] The minimum wage in agriculture was introduced at a lower rate than the federal rate and fully converges to the federal rate only ten years later (see Figure 2).

Services. There are two potential concerns about classifying restaurants, hotels, laundries and other personal services, entertainment and recreation services as industries covered in 1967: one might worry that these services were (i) already partially covered by the 1961 amendments, and (ii) that the 1966 amendments only partially covered these sectors, as service enterprises with annual sales below $500,000 were not covered. Regarding (i): Although it is true that the 1961 Amendments introduced coverage in service enterprises with sales greater than $1m, the amendments also excluded the following industries from coverage, regardless of the amount of gross sales: hotels, motels, restaurants, laundry and dry cleaning establishments, seasonal and recreational establishments. Therefore, a closer reading of the 1961 amendments allows for the interpretation that the services listed above were not covered by the 1961 amendments and were only covered beginning in 1967. Regarding (ii): What the 1966 amendments do is introduce coverage in these sectors for enterprises with sales greater than $500,000. These services were therefore partially treated in 1967, except for laundries and dry cleaning services which were fully covered – regardless of any sales amount. We estimate that the share of coverage in restaurants, hotels, and entertainment and recreation services was high. Last but not least, a tipped minimum wage was introduced in restaurants and hotels in 1967. Hourly wages of tipped employees may legally be adjusted to reflect allowance of up to 50 % of the minimum wage for tips actually received. Because we observe annual earnings in the CPS, and this measure includes all tips, we do not think the fact that the tipped minimum wage was introduced in these industries is a threat to our results.

The 1974 Amendments to the Fair Labor Standards Act extend coverage to employees of all public agencies (federal, state and local) and to private household domestic service workers. We therefore classify federal workers and domestic service workers as covered in 1974.[70] Importantly, we did not classify state and local government workers as covered in

[69] There were four notable exemptions in agriculture: (i) employees of farms employing less than 500 "mandays" of nonexempt labor in the highest quarter of the previous year; (ii) family members; (iii) local hand harvest laborers paid on a piece rate basis who worked less than $<$ 13 weeks in the preceding year; (iv) employees in range production of livestock. The agriculture exemption was further reduced in the 1974 amendments, by including within the 500 manday count the employment of local hand harvest labor.

[70] Not all federal workers and domestic workers were covered by the 1974 Amendments. Among federal workers: a few federal employees were already covered by a minor amendment in 1966, in very special

1974. Rather, we include them in the database in 1986. This is because shortly after minimum wage coverage was extended to state and local government workers starting in May 1974, the Supreme Court in the National League of Cities v. Usery ruled that the Fair Labor Standards Act could not be applied to state and local government employees engaged in activities which are traditional government functions (i.e. fire prevention, police protection, sanitation, public health and parks and recreation).[71] Coverage was extended to state and local government workers from January 1, 1986, after the U.S. Supreme Court reversal of its former decision.[72]

Uses. We are interested in knowing which minimum wage rate applies to each worker depending on his/her state, industry and gender. We merge our minimum wage database with March CPS files (1962-1980). We are also interested in knowing the average minimum wage that applies in each state. Therefore, we calculate several measures of the minimum wage that we include in the minimum wage database.

The minimum wage by year y, month m, industry j, state s, and gender g, denoted mw_{ymjsg}, is obtained by analyzing the data sources described above.

The minimum wage by year y, month m, industry j, state-group S and gender g, denoted mw_{ymjSg} is calculated by averaging the minimum wage at the state level mw_{ymjsg} across state groups, depending on the number of workers N_{sjg} working in each of the K states within a state group S:[73]

circumstances. Some others, such as federal criminal investigators were excluded from coverage, as is still the case today, see https://webapps.dol.gov/elaws/whd/flsa/screen75.asp. Among domestic workers: only domestic service workers who met Social Security qualifications were covered by the 1974 amendments. The minimum wage extension essentially applies to housekeepers, day workers, chauffeurs, full-time babysitters and cooks. Babysitters on a casual basis are still excluded from minimum wage coverage today.

[71] See Supreme Court in the National League of Cities v. Usery (6/24/76): https://supreme.justia.com/cases/federal/us/426/833/.

[72] Note that certain state and local employees started to be covered by the minimum wage by the 1966 Amendments. In September 1975, before the coverage was overturned by the U.S. Supreme Court, the Employment Standard's Administration estimated that 3.1 million state and local government workers were covered under the 1966 amendments and 3.8 million more under the 1974 amendments. In September 1976, after coverage was overturned by U.S. Supreme Court, the Employment Standard's Administration estimated that there were only 116,000 covered workers under the 1966 amendments and 221,000 under the 1974 amendments. See p.126 of the Report of the Minimum Wage Study Commission (1981), Volume II. Because of these specificities, and because we could not identify clearly the state and local government workers covered by the 1966 Amendments, we have focused our analysis on the private sector, and we exclude all public administration workers.

[73] Note that we have no direct information on the number of workers by state, industry and gender N_{sjg}, due to the limitations of the March CPS files (see Appendix B). Instead, we have information on the number of workers at the state-group, industry and gender levels in the March CPS. We approximate N_{sjg} by assuming that (1) within each state-group, the number of workers at the state level is proportional to the size of the population in that state and (2) the share of male and female workers in each state is similar to the male and female employment share at the state-group level. The data on the size of the population at the state level is given by the Census Bureau: from 1950 to 1999, we scraped the text files containing the data from https://www2.census.gov/programs-surveys/popest/tables/; from 2000 to 2009, we

Table A1: List of industries used in March CPS (1962-1987), and year of coverage by FLSA

1	Agriculture	1967
2	Forestry and Fishing	1967
3	Mining	1938
4	Construction	1961
5	Durable manufacturing	1938
6	Food manufacturing	1938
7	Other non-durable manufacturing	1938
8	Transportation, Communication, and Other Utilities	1938
9	Wholesale Trade	1938
10	Restaurants	1967
11	Retail Trade	1961
12	Finance, Insurance, and Real Estate	1938
13	Business and Repair Services	1938
14	Private households	1974
15	Hotels, laundries and other personal services	1967
16	Entertainment and Recreation Services	1967
17	Nursing homes and other professional services	1967
18	Hospitals	1967
19	Schools and other educational services	1967
20	Federal government	1974
21	State or local government	1986
22	Postal service	1938
23	Other	1938

Source: Authors' analysis of March CPS 1962-1987 and of the Fair Labor Standards Act and its amendments.

Notes: The retail trade sector excludes restaurants. Control group industries are listed in dark blue. Treated industries are listed in light blue.

$$mw_{ymjSg} = \frac{1}{\sum_{s=1}^{K} N_{jsg}} \sum_{s=1}^{K} mw_{ymjsg} \tag{8}$$

The minimum wage by year, month, industry, and state-group , denoted mw_{ymjS} is calculated by averaging the minimum wage at the state-group level mw_{ymjS} across genders, depending on the number of female and male workers N_{jSg} in each state group:

$$mw_{ymjS} = \frac{1}{\sum_{g=1}^{2} N_{jSg}} \sum_{g=1}^{2} mw_{ymjSg} \tag{9}$$

The minimum wage by year, month, industry, denoted mw_{ymj} is calculated by averaging the minimum wage at the state-group level mw_{ymjS} across industries, depending on the number of workers N_{jS} within M state-groups:

$$mw_{ymj} = \frac{1}{\sum_{S=1}^{M} N_{jS}} \sum_{S=1}^{M} mw_{ymjS} \tag{10}$$

The minimum wage by year, month, industry type T (whether control or treatment), denoted mw_{ymT} is calculated by averaging the minimum wage at the industry level mw_{ymj} across industry type (control or treatment), depending on the number of workers N_j within control (c) or treatment (t) industries:

$$mw_{ymT} = \frac{1}{\sum_{T=j_c}^{t} N_{j_T}} \sum_{T=j_c}^{j_t} mw_{ymj} \tag{11}$$

Finally, we convert nominal minimum wage rates into real minimum wage rates using the CPI-U-RS.[74]

download "st_est00int-01.csv" from https://www2.census.gov/programs-surveys/popest/tables/. From 2010-2017, we use "nst-est2017-01.xlsx" from https://www2.census.gov/programs-surveys/popest/tables/2010-2017/state/totals/. For the years 1950, 1960, 1970, 1980, 1990, 2000, and 2010, we use the census counts on April 1st. For the remaining years, we use intercensal estimates as of each July 1.

[74] The annual CPI-U-RS series are available since 1947 at: https://www2.census.gov/programs-surveys/demo/tables/p60/ (as of September 11, 2019), folder 259.

Appendix B March CPS (1962-1981)

This paper uses data from the March Current Population Survey (CPS) to analyze the effect of the 1966 Fair Labor Standards Act on annual earnings, employment, and racial inequality.[75] As noted in the IPUMS documentation,[76] the early CPS files (1962-1967) were not officially released by the U.S. Census Bureau as public use files. Because these files were used by researchers at the University of Wisconsin, they were preserved in the data archive at the Center for Demography and Ecology at the University of Wisconsin. The most recent version of these early files has been made public by IPUMS on February 23, 2009.[77] In particular, the IPUMS version of the CPS early files contains a harmonized industry variable.

B.1 Sample of Interest

Figure B1 displays how we divide the CPS sample into four categories of individuals for the purpose of our analysis: (i) Not in universe, (ii) employed, (iii) unemployed, and (iv) not in the labor force.

Not in universe. We exclude from our analysis all minors, i.e. children,[78] and teenagers below 21,[79] and older individuals (aged 66 and above). We also remove self-employed workers from our universe of interest, as the minimum wage does not apply to them. Finally, we exclude all unpaid family workers, all individuals in grouped quarters, all workers working less than 13 weeks a year[80] and more than 3 hours a week, and all individuals with a missing industry or occupation.

Employed. We include all adult workers (21-64), whether employed and at work last week or employed but not at work last week. Our analysis sample – the sample on which we conduct the bulk of our analysis of the effect of the 1967 reform on wages, employment and the racial earnings gap, is conducted on prime-age workers (25-55).

Unemployed or not in the labor force. When analyzing the employment effects of the 1967

[75] Sarah Flood, Miriam King, Steven Ruggles, and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 5.0 [March CPS]. Minneapolis, MN: University of Minnesota, 2017. https://doi.org/10.18128/D030.V5.0.

[76] See https://cps.ipums.org/cps/asec_sample_notes.shtml

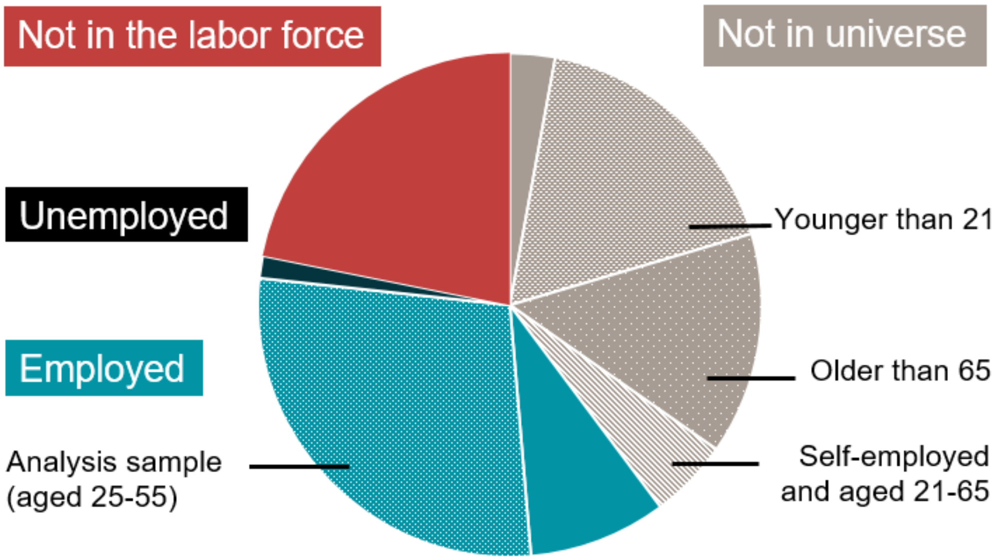
[77] See https://cps.ipums.org/cps-action/revisions

[78] From March CPS 1962 to 1979, the lowest age cut-off for employment questions is 14. It is 15 starting in 1980. For more information on the evolution of the universe of CPS employment questions, see: https://cps.ipums.org/cps-action/variables/IND#universe_section.

[79] Minimum wage legislation for minors is very different from that for adults; we exclude teenagers so that we do not introduce this layer of heterogeneity into the treatment.

[80] Starting in 1967, the minimum wage was introduced in agriculture, except for some employees, in particular, for local hand harvest laborers paid on a piece-rate basis who worked less than 13 weeks in the preceding year. See report of the minimum wage study commission (1981), volume II, p.124.

Figure B1: Analysis sample, before the reform (1966)



Source: Authors' analysis of March CPS 1967.

reform, we look at the probability of being employed vs. unemployed (or vs. unemployed or not in the labor force) and restrict the sample of analysis to adults aged 25-55.

B.2 State Crosswalks

In some years, states are identified with their Federal Information Processing Standard (FIPS) state codes, and in some others (March CPS 1962, 1968-1971, 1972, and 1973-1976) some states are grouped together. This makes it impossible to uniquely identify the state to which the interviewee belonged. For example, in March CPS 1968-1971, Minnesota and Iowa are identified as a group—we do not know whether the individuals surveyed in those years were living in Minnesota or Iowa. We only know that they were living in one of those two states. In addition, the state groupings differ across years. To overcome the state grouping limitation and the inconsistent coding of the state group variable across time, we have built a new variable that identifies homogeneous state groups for our period of interest. In total, we are able to identify 21 state groups (see Appendix Table B1). States were not grouped in the CPS at random: states grouped together are geographically close to each other, and the borders of state-groups never cross division or region lines (see Appendix Figure B2). To a certain extent, the state groups share similar economic conditions.[81]

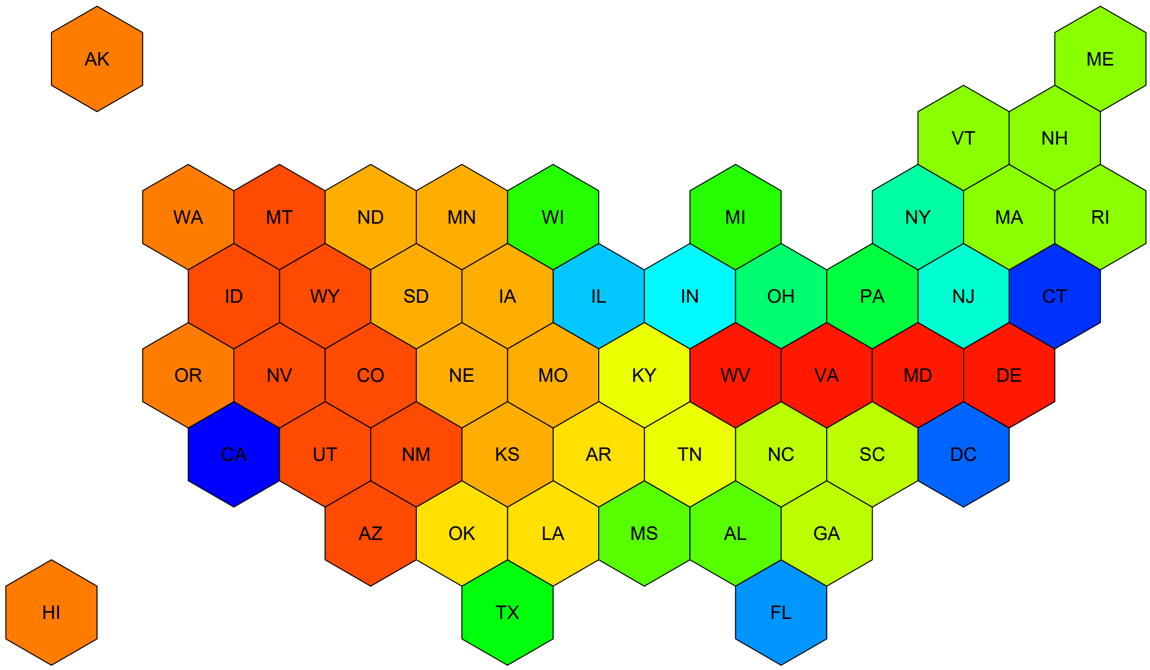
[81] A detailed crosswalk, for every year of the CPS, is available online at: http://clairemontialoux.com/flsa.

Table B1: List of state groups used in March CPS (1962-1980)

1	California	West
2	Connecticut	Northeast
3	District of Columbia	South
4	Florida	South
5	Illinois	Midwest
6	Indiana	Midwest
7	New Jersey	Northeast
8	New York	Northeast
9	Ohio	Midwest
10	Pennsylvania	Northeast
11	Texas	South
12	Michigan-Wisconsin	Midwest
13	Alabama-Mississippi	South
14	Maine-Massachusetts-New Hampshire-Rhode Island-Vermont	Northeast
15	North Carolina-South Carolina-Georgia	South
16	Kentucky-Tennessee	South
17	Arkansas-Louisiana-Oklahoma	South
18	Iowa-N Dakota-S Dakota-Nebraska-Kansas-Minnesota-Missouri	Midwest
19	Washington-Oregon-Alaska-Hawaii	West
20	Montana-Wyoming-Colorado-New Mexico-Utah-Nevada-Arizona-Idaho	West
21	Delaware-Maryland-Virginia-West Virginia	South

Source: Authors' analysis of March CPS 1962-1980.

Figure B2: State groups used in March CPS (1962-1980)



Source: Authors' analysis of March CPS 1962-1980.

States not identified. In March CPS 1963, 1964 and 1972, there are a few observations for which the state of the person interviewed was not reported and marked as "not identified." Within our sample of interest,[82] a few workers were in a state that was not identified: 25 in March CPS 1963 (0.2% of the representative sample of interest), 40 in March CPS 1964 (0.3%), and 13 in March CPS 1972 (0.04%). These observations are dropped from our analysis. Given the small number of workers involved, we do not believe this restriction introduces any bias into our results.

B.3 Industry Crosswalks

There are several industry codes available in CPS IPUMS, and their classification varies across years. We create our own industry variable, harmonized across years, and consistent with the 1950 Census Bureau industrial classification system.

To construct a harmonized industry code, we use two industry variables available in CPS IPUMS: variable IND from March CPS 1962-1967,[83] and variable IND1950 from March

[82] Our sample of interest is the sample we use to perform our analysis: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

[83] See: https://cps.ipums.org/cps-action/variables/IND#description_section.

1968-1981.[84] In both cases, the industry variable reports the industry in which the person performed his or her primary occupation. In both cases as well, the classification system used is consistent with the 1950 Census Bureau industrial classification system.[85]. However, the two industry codes differ by their precision: Codes for March CPS 1962-1967 are two digits, and the classification scheme uses 44 codes. Codes for March CPS 1968-1981 are three digits, and the classification scheme uses 148 codes. Therefore our harmonized industry code cannot be more precise than the industry code for 1962-1967. Our final industry classification uses 23 codes (see Table A1 above). Importantly, this classification allows us to disentangle industries covered by the Fair Labor Standards Act from those covered by its subsequent amendments.[86]

B.4 Topcoding

For confidentiality reasons, the income of individuals with extremely high incomes is topcoded in the CPS.

Before 1996, no replacement is provided in the CPS. We replace the topcoded values by 1.5 the value of the highest non-topcoded income. This replacement is done by industry type (covered in 1938, 1961, 1966, 1974 or 1986)[87]. Among employed individuals in March CPS 1962-1972,[88] less than 1% of the sample has topcoded incomes. This share increases progressively in the 1970s and reaches almost 5% in 1978, 8% in 1979, and peaks at 10% in 1980. Starting in 1981, this share is consistently below 5% (except for the years 1992-1994 where it is between 5% and 8%).

After 1996, topcoded values are replaced with values that vary with individual characteristics (gender, race, and full-time/part-time status).[89]

[84] See: https://cps.ipums.org/cps-action/variables/IND1950#description_section.

[85] For a confirmation that the IND variable for March 1962-1967 is consistent with the 1950 Census Bureau classification system, see the sentence "IND classifies industries according to the contemporary Census Bureau classification systems" here: https://usa.ipums.org/usa-action/variables/IND#comparability_section. The variable IND1950 is consistent with the 1950 Census Bureau industrial classification system by construction, see discussion in the Section "Integrated Occupation and Industry Codes and Occupational Standing Variables in the IPUMS" here: https://usa.ipums.org/usa/chapter4/chapter4.shtml.

[86] The detailed industry crosswalk is available online at: http://clairemontialoux.com/flsa.

[87] This is consistent with assuming that the distribution of incomes is Pareto distributed, with a Pareto coefficient of 3, that is typically used in the literature on top-income earners (Piketty et al., 2018).

[88] We refer here to employed individuals in our analysis sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

[89] For CPS samples starting in 1996, see replacement values here for the variable INCWAGE: https://cps.ipums.org/cps/topcodes_tables.shtml#1996rep.

B.5 Consistency between CPS and Census Data

We check the consistency between the CPS (and in particular the early files of the CPS) and Census data.

We start by comparing the unadjusted racial earnings gaps in the Census and in the March CPS from 1960 to today. We show the two data sources are remarkably aligned and paint a consistent picture (see Figure 1).

We then compare decennial Census of Population data from 1960 to 1980 (covering earnings data from 1959 to 1979) and the March CPS from 1962 to 1981 (covering earnings data from 1961 to 1980) to check the quality of CPS files on several dimensions. Employment counts are similar across the two data sets, see Appendix Table B2. One notable exception, however, is the first two years of the CPS, where the employment counts are much lower than in the 1960 Census and much lower than in later years of the CPS (starting in the March CPS 1964). A fraction of workers in the 1962 and 1963 CPS have been categorized – wrongly – as not in the labor force. On all other dimensions, however, the first two years of the CPS are similar to the 1960 Census. Appendix Table B2 shows that the 1960 Census and the 1962 and 1963 March CPS match well in terms of relative shares of white and black workers, male and female workers, or their annual earnings. We exclude the March CPS 1963 (i.e. corresponding to earnings earned in the year 1962) from our analysis as it also suffers from a lower number of observations and lacks demographic information (such as education) for the entire population.

Table B2: Observations, employment, and wages in the March CPS and in the Census

	Observations	Employment	Employment shares				Earnings ($2017)			
			White	Black	Men	Women	White	Black	Men	Women
March CPS										
1962	13,540	24,086,400	0.90	0.10	0.68	0.32	46,038	19,523	53,696	21,113
1963	9,638	22,277,274	0.90	0.10	0.68	0.32	37,607	18,865	42,412	21,267
1964	14,222	34,344,403	0.89	0.11	0.68	0.32	38,736	21,529	44,216	21,343
1965	14,126	34,637,727	0.89	0.11	0.68	0.32	39,708	22,997	45,420	22,158
1966	30,113	37,407,666	0.89	0.11	0.68	0.32	41,196	23,168	47,224	22,461
1967	19,191	38,490,848	0.89	0.11	0.68	0.32	42,575	24,522	49,036	23,091
1968	30,277	39,451,389	0.89	0.11	0.66	0.34	43,219	26,019	50,127	24,098
1969	30,808	40,044,846	0.89	0.11	0.66	0.34	44,579	28,242	52,076	24,935
1970	29,626	40,963,562	0.90	0.10	0.66	0.34	47,062	29,253	55,248	26,015
1971	29,130	40,594,657	0.89	0.11	0.65	0.35	47,565	30,486	55,874	26,946
1972	28,214	41,861,238	0.90	0.10	0.65	0.35	47,460	30,936	55,969	27,039
1973	28,025	42,659,268	0.89	0.11	0.64	0.36	49,744	33,601	59,060	28,255
1974	27,620	43,773,753	0.90	0.10	0.64	0.36	49,965	33,810	59,857	28,155
1975	26,474	43,108,371	0.90	0.10	0.63	0.37	48,364	34,284	58,235	27,912
1976	28,407	44,987,015	0.90	0.10	0.62	0.38	47,557	33,346	57,386	27,866
1977	33,944	46,526,101	0.90	0.10	0.61	0.39	48,197	34,215	58,382	28,390
1978	33,936	48,250,592	0.89	0.11	0.61	0.39	48,588	34,812	59,187	28,665
1979	34,468	50,109,925	0.90	0.10	0.60	0.40	48,789	36,335	59,923	29,044
1980	41,137	51,461,168	0.90	0.10	0.58	0.42	48,862	36,004	60,306	29,636
1981	41,859	53,389,185	0.90	0.10	0.58	0.42	47,624	34,640	58,541	29,490
US Census										
1960	1,662,241	33,244,820	0.90	0.10	0.69	0.31	35,857	19,429	40,231	20,684
1970	403,015	40,301,500	0.90	0.10	0.65	0.35	46,243	30,102	54,341	26,724
1980	2,613,374	52,267,480	0.89	0.11	0.58	0.42	46,870	36,367	57,205	29,905

Sources: March CPS 1962-1981. US Censuses 1960 (5% sample), 1970 (1%), and 1980 (5%).

Sample: Adults 25-65, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: Number of observations, employment numbers and shares refer to the years 1962 to 1981 in the March CPS and to the years 1960, 1970 and 1980 in the decennial Censuses. The March CPS 1962-1981 covers earnings data from 1961-1980. The decennial Censuses of 1960, 1970 and 1980 cover earnings data for 1959, 1969 and 1979. Annual earnings in $2017, deflated using annual CPI-U-RS series.

B.6 Aggregate Employment Trends in CPS

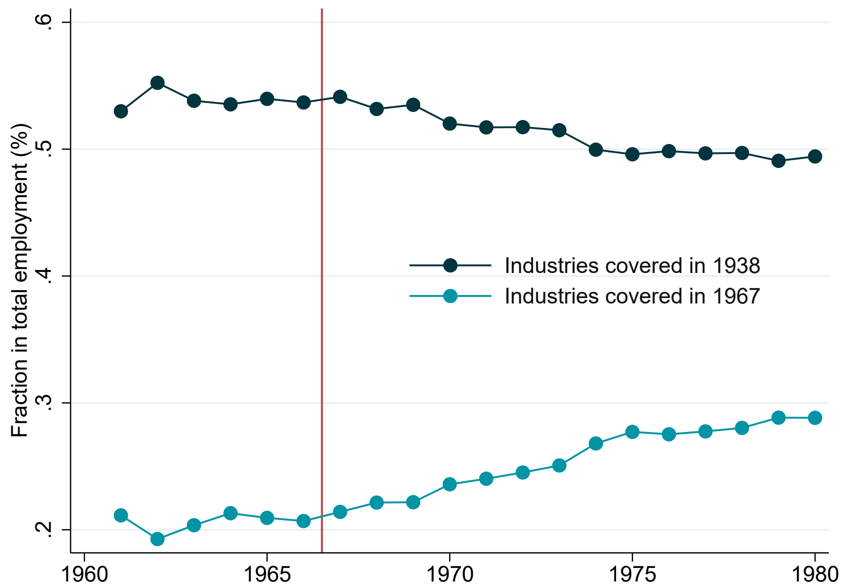
In this Section, we present aggregate evidence of stable employment trends in the CPS.

Appendix Figure B3 shows that employment shares across industry type (industries covered in 1938 vs. covered in 1967) and race are relatively stable from the early 1960s to 1980. In particular, Appendix Figure B3a shows that there is no discontinuity in the aggregate shares of workers in the treated vs. control industries around the 1967 reform. Appendix Figure B3b shows there is no discontinuity in the share of black workers (in total black and white employment) within treated or control industries around 1967.

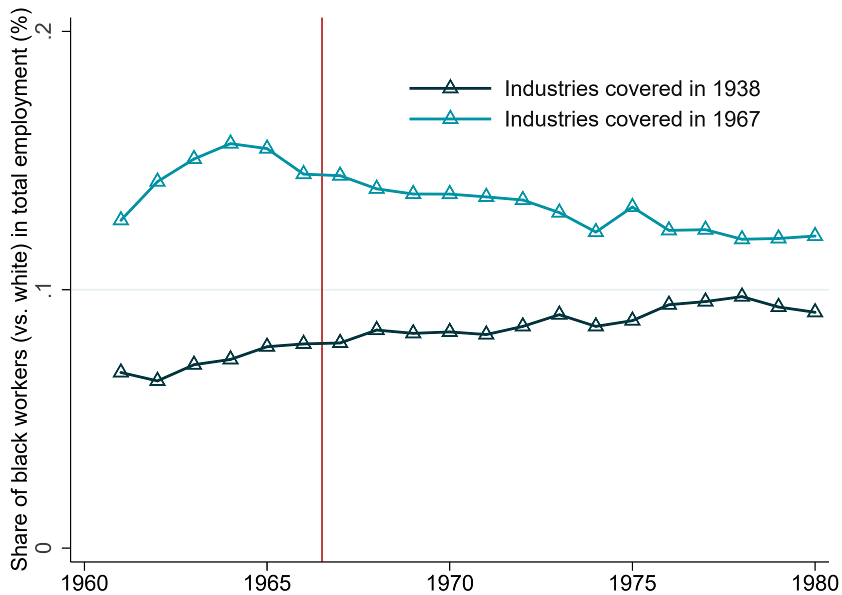
Appendix Figure B4 further decomposes these aggregate employment trends by gender. Appendix Figure B5 shows the relative stability of employment status in industries covered in 1938 and 1967 (employment, unemployment and not in the labor force) by race and gender.

Figure B3: Evolution of black and white employment in treated and control industries

(a) Employment shares in control vs. treated industries

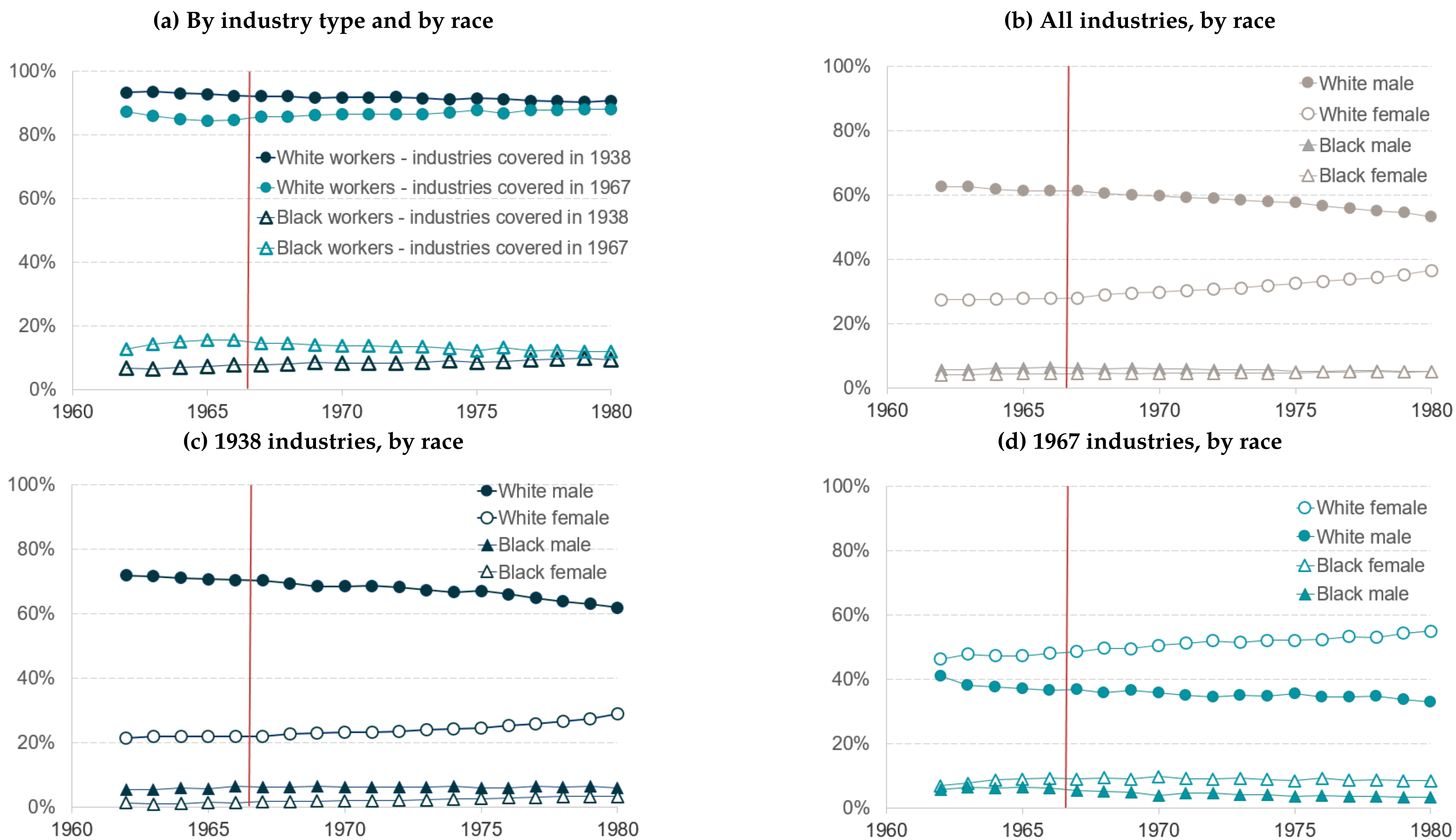


(b) Black share of employment within 1938 and 1967 industries



Source: March CPS 1962-1981.
Sample: Adults 25-65, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

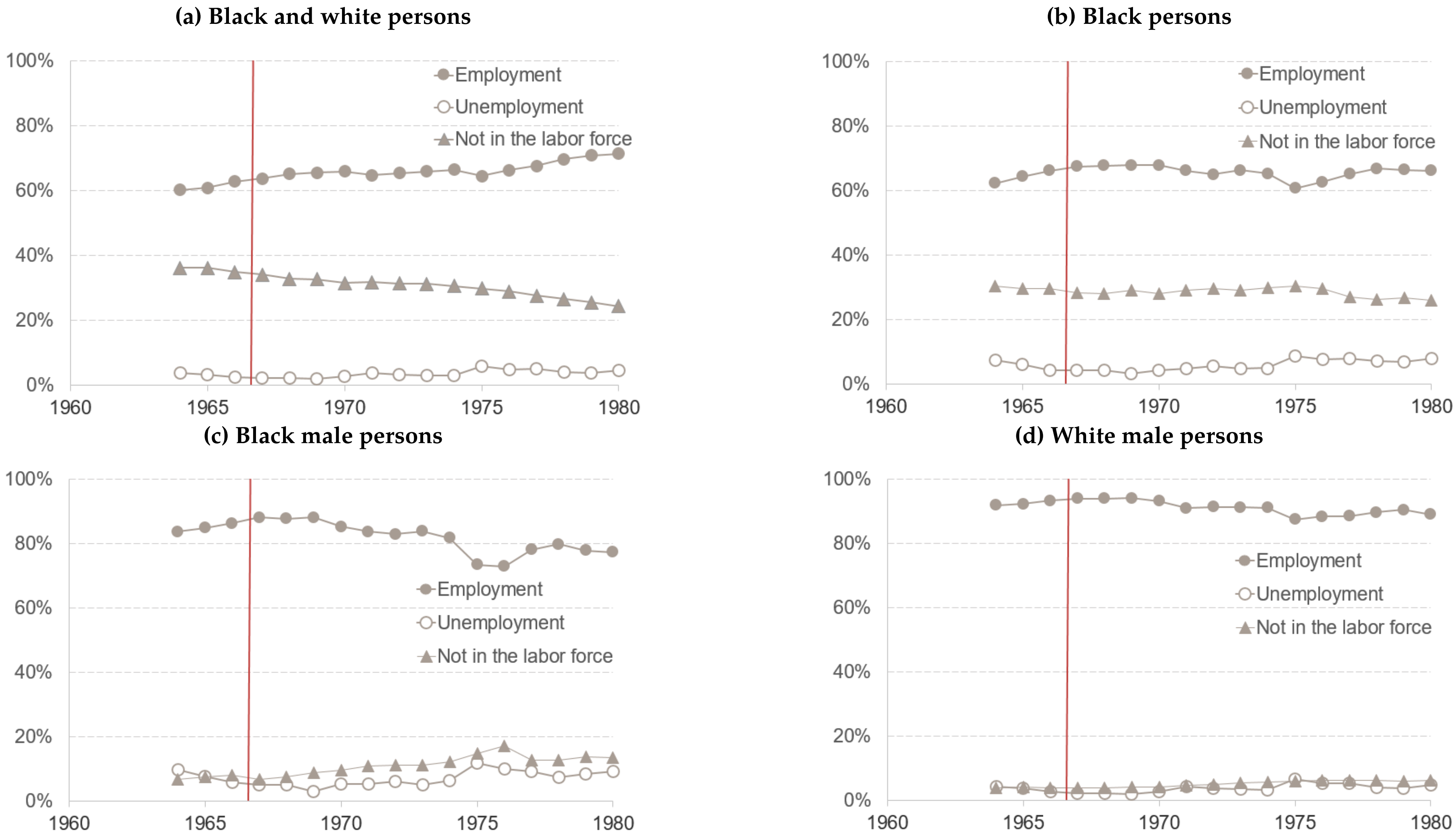
Figure B4: Aggregate employment shares



Source: March CPS 1962-1981.

Sample: Adults 25-65, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Figure B5: Employment status in industries covered in 1938 and 1967



Source: March CPS 1962-1981.

Sample: Adults 25-65, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Appendix C Additional Results on the Effect of the 1967 Reform on Wages and the Adjusted Racial Gap

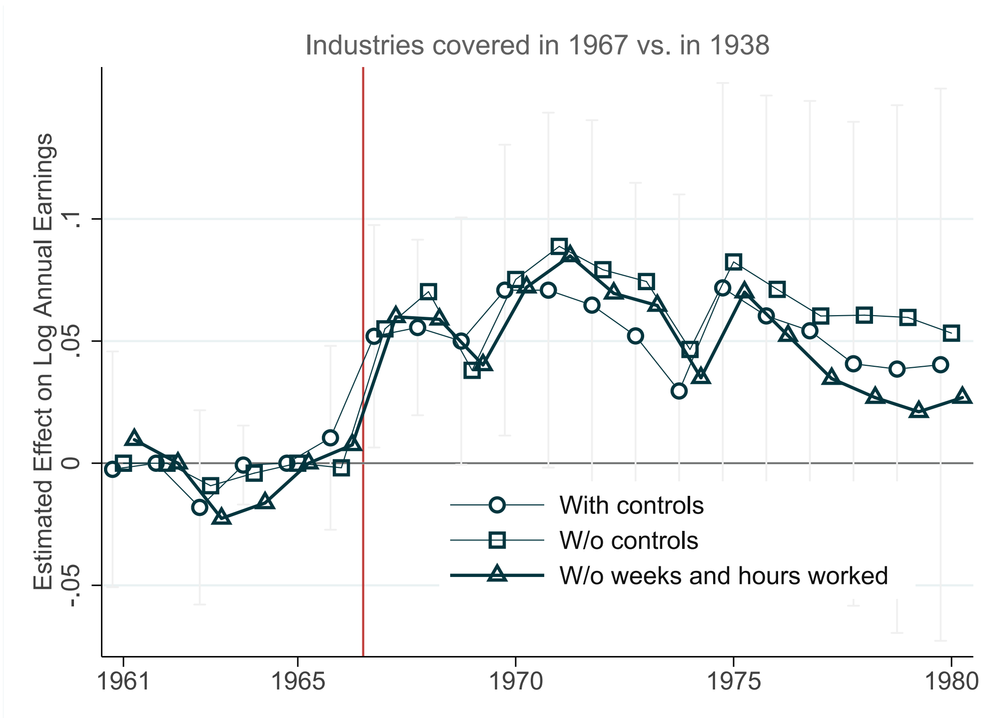
Figure C1 shows that adding or removing individual-level controls to our baseline wage regression does not affect the magnitude of our estimates, at least in the medium-run.

Figure C2 decomposes the effect of the 1967 reform on log annual earnings by race. It compares the evolution of annual earnings for black (respectively, white) workers in the industries covered in 1967 to the evolution of annual earnings for both black and white workers in the industries covered in 1938. It differs from Figure 6b which was comparing the evolution of annual earnings for black (white) workers in the industries covered in 1967 to the evolution of annual earnings for black (white) workers only in the industries covered in 1938. It shows, as expected, that the wage effect is larger in this design (as opposed to the design used in Figure 6b) because annual earnings for black workers have continuously increased during the Civil Rights Era for reasons that go beyond the 1967 reform (e.g., due to the role of anti-discrimination policies and improvements in education).

Figures C3a and C3b show that, as expected, the wage effect of the 1967 reform is concentrated among low-education workers. This is true among black and white workers separately.

Figure C4a shows that, as expected, the decline in the adjusted racial gap is concentrated among low-education workers within the treated industries and that there is no change in trend for high-education workers. By contrast, Figure C4b shows that the decline in the adjusted racial earnings gap is smooth for both high and low-education workers within the control industries.

Figure C1: Wage effect of the 1967 reform with different sets of controls

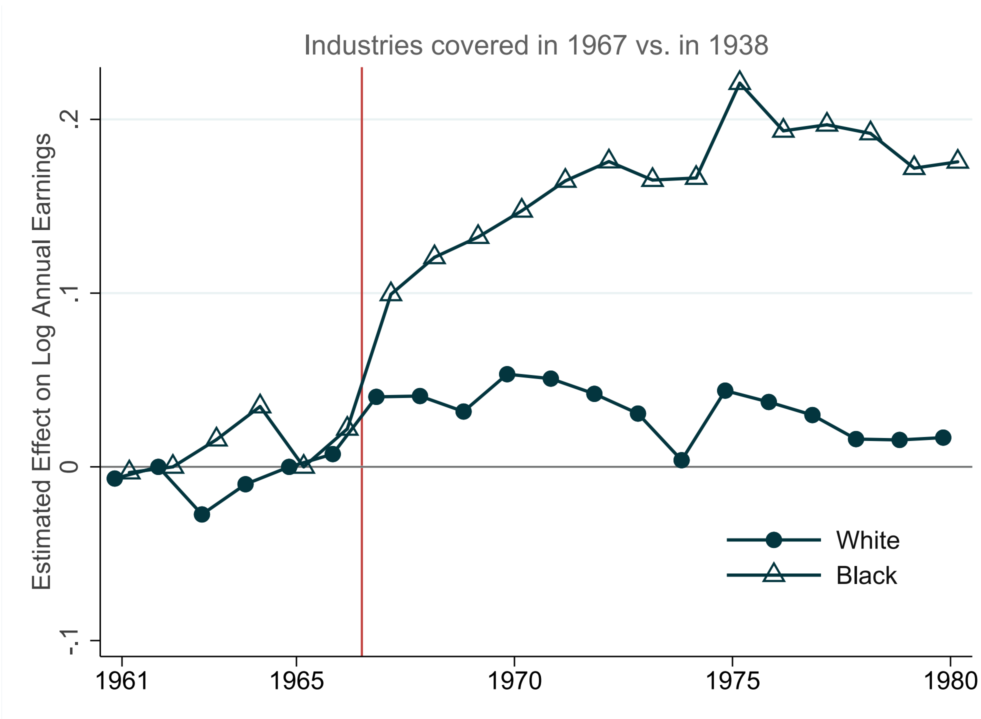


Source: March CPS 1962-1981.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: This regression uses a cross-industry design and includes industry and time fixed effects. Because the CPS collects information on earnings received during the previous calendar year, we report estimates of the wage effect calculated in the 1962 CPS in the year 1961 above. The year 1962 is excluded and set to zero. Standard errors are clustered at the industry level. Annual earnings in $2017, deflated using annual CPI-U-RS series. The regression with individual-level controls 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.

Figure C2: Impact of the 1967 reform on annual earnings by race



Source: March CPS 1962-1981.

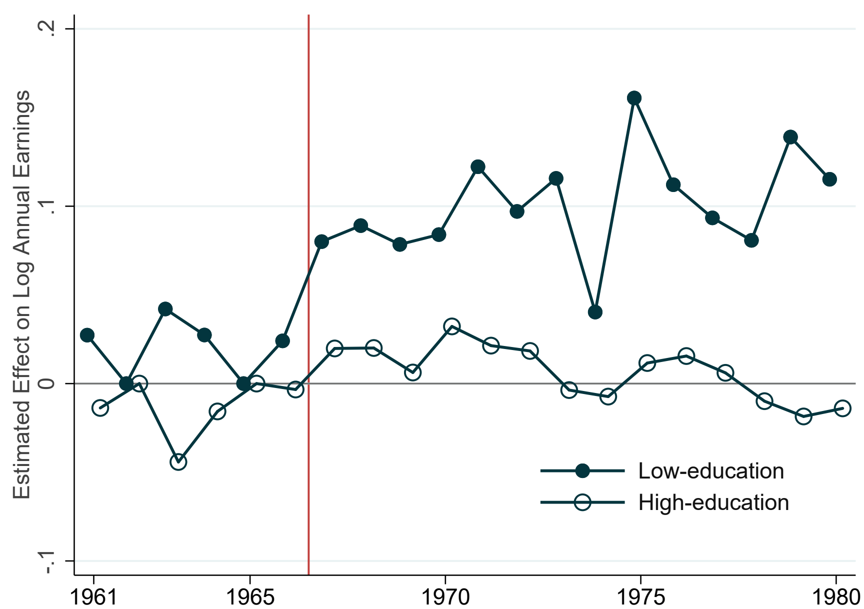
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.

Notes: This graph differs from Figure 6b: the control group for black workers is composed here by black and white workers in the industries covered in 1938, whereas in figure 6b, the control group for black workers is composed of black workers only in the industries covered in 1938. This regression uses a cross-industry design and includes industry and time fixed effects. Because the CPS collects information on earnings received during the previous calendar year, we report estimates of the wage effect calculated in the 1962 CPS in the year 1961 above. The year 1962 is excluded and set to zero. Annual earnings in $2017, deflated using annual CPI-U-RS series.

Figure C3: Heterogeneity in the wage effect by level of education

(a) Among black workers

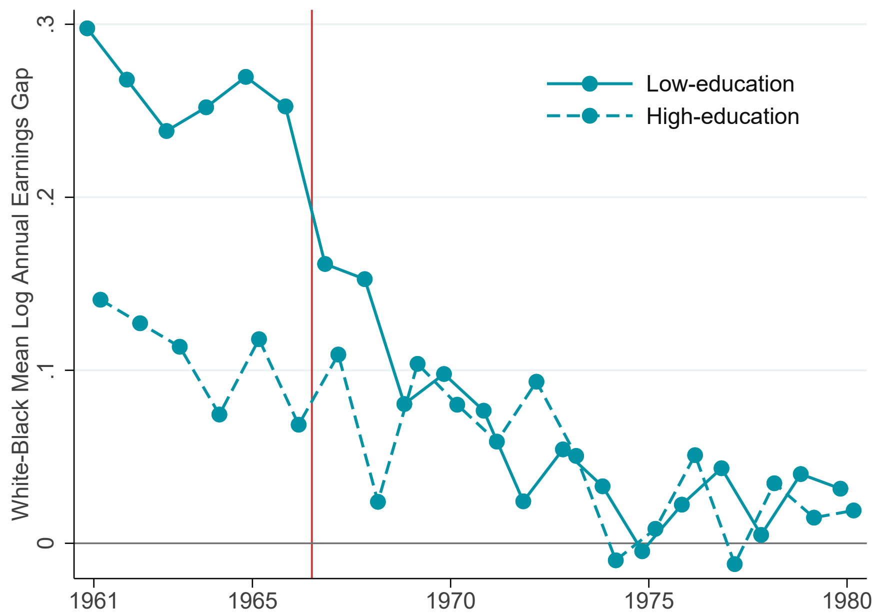
(b) Among white workers



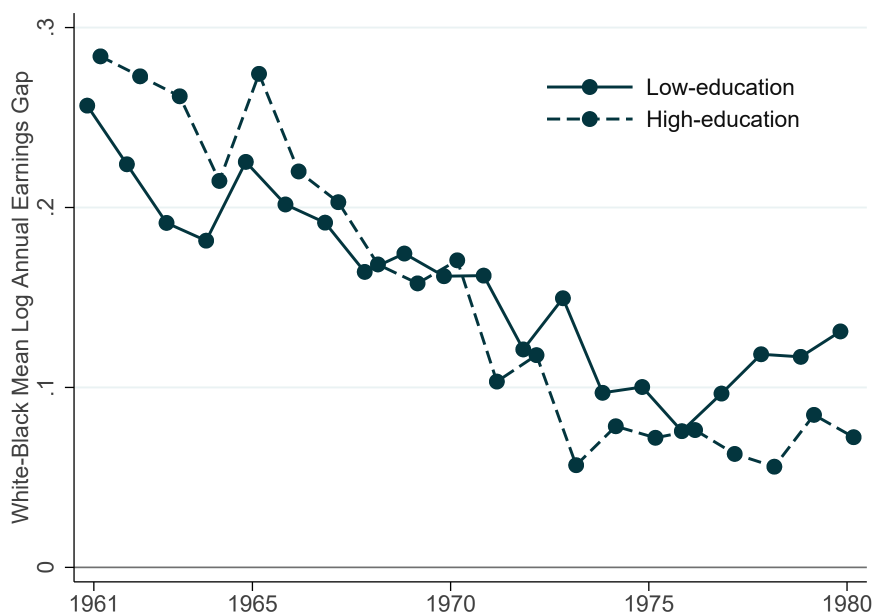
Source: March CPS 1962-1981.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: These regressions use a cross-industry design and control for gender, years of schooling, a cubic in experience, full-time/part-time status, number of weeks and hours worked, occupation and marital status. The regression includes industry and time fixed effects. Low-education is defined as 11 years of schooling or less. High-education is defined as more than 11 years of schooling. Because the CPS collects information on earnings received during the previous calendar year, we report estimates of the wage effect calculated in the 1962 CPS in the year 1961 above. 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.

Figure C4: Adjusted racial wage gaps, by level of education

(a) White-black earnings gap (adjusted) in treated industries



(b) White-black earnings gap (adjusted) in control industries



Source: March CPS 1962-1981.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: Racial earnings gap measures 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. Low-education is defined as 11 years of schooling or less. High-education is defined as 11 years of schooling or more. Annual earnings are in \$2017, deflated using annual CPI-U-RS series.

Appendix D BLS Data and Additional Evidence on Employment

D.1 BLS Data

Content and access. We contribute a new database on hourly wages for the United States in the 1960s by digitizing a large set of BLS industry wage reports. We believe this database fills a gap as, first, it provides information on hourly wages as opposed to annual earnings. To date, the primary source for wages in the 1960s has been the March CPS micro-files—which only contains direct information on annual earnings. The CPS started to collect information on hourly and weekly earnings in 1973 in the May supplement of the survey. In 1979, the earnings questions were asked each month for people in the outgoing rotation groups. Second, the BLS data provide information based on employer payroll records—as opposed to information self-declared by the worker—as is the case in the CPS and the National Longitudinal Survey data.

We built a database of hourly wage distributions for the industries covered in 1967, as well as for a set of industries covered in 1938—mainly from non-durable, low-wage manufacturing sectors. The list of 1938 and 1967 we digitized for the purpose of this project is displayed in Figure D1a).

The BLS data contain information on the distribution of hourly wages and focus on production and nonsupervisory workers. Hourly wage data exclude tips and the value of free meals, rooms, and uniforms, if provided, and premium pay for overtime and for work on weekends, holidays, and late shifts. Service charges added to customer bills and distributed by the employer to his employees are included. By contrast, annual earnings measured in the CPS correspond to total pre-tax wage and salary income—i.e. wages, salaries, commissions, cash bonuses, tips and other money income received from an employer; payments-in-kind and reimbursements for business expenses are not included.

The reports provide us with the percent of workers in each 5- or 10-cent hourly wage bin, as well as the total number of workers in the corresponding industry (see Figure D1b to see the format of the raw data for laundries in the South). We are therefore able to construct a database with information on the number of workers by detailed hourly wage bins.

In the future, this database could be improved in two ways: first, although we have only digitized the information on wages for the purpose of this project, the reports contain a wealth of information on establishment practices and supplementary wage provisions (overtime premium pay, paid holidays, paid vacations, health, insurance, and pensions plans, bonuses), shift work and supplementary benefits provisions, and the distribution of weekly hours. Second, although we have digitized most of the information on hourly wages from 1961 to 1969, these data exist in a similar form from the 1930s to the 1980s. BLS industry wage reports were first published in the 1930s when the Work Progress Administration began to monitor working conditions in low-wage industries after the 1934 general strike in the cotton textile industry. The series ended in the 1980s when the BLS began collecting some of this information through a variety of new programs (e.g., the Occupational Employment Statistics, which provide national estimates of employment and wages by occupation for more than 700 occupations; the Current Employment Statistics, a monthly survey of the payroll records of business establishments, providing national estimates of average weekly hours and average hourly and weekly earnings; or the Quarterly Census of Employment and Wages, which provide annual and quarterly average wage data by detailed industry for the US, states, counties and many metropolitan areas).

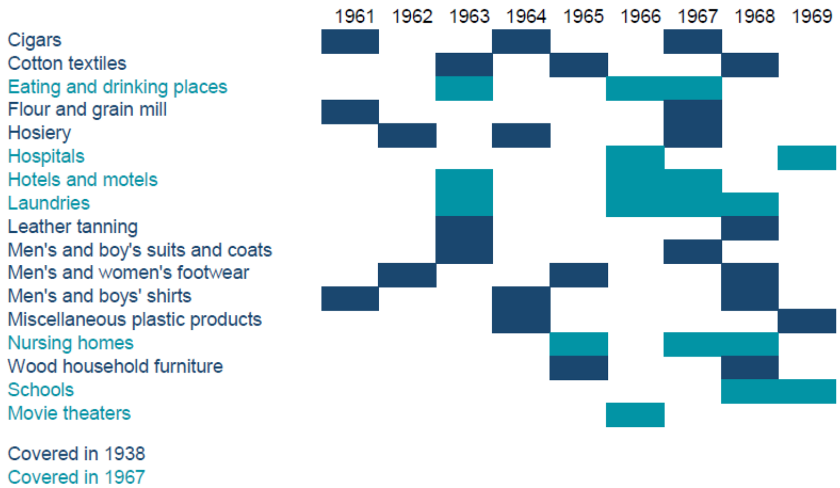
The 1940s BLS reports have been used by Goldin and Margo (1992) to make inferences about the timing and the causes of the narrowing of the wage structure (the "Great Compression") in the 1940s. A more comprehensive database could foster our knowledge of the long-run evolution of gender inequality, regional convergence, the rural-urban gap, the wage-price inflation, and the trade-off between wage vs. non-wage compensation, etc.

Sources. We collected the BLS Industry Wage reports from: `https://fraser.stlouisfed.org/series/5293#4603`. Another resource is: `https://libraryguides.missouri.edu/pricesandwages/1970-1979`. Because the reports are approximately a hundred pages long each, we developed an algorithm to extract the tabulations we were interested in. We then digitized the corresponding tables.

Uses. We have used the BLS industry wage reports to provide (i) graphical evidence of how the minimum wage affects the distribution of hourly wages. Figures D2, D3, D4, D5, D6, D7, D8, D9, D10, D11 below show how the spikes in the hourly wage distributions move with minimum wage legislation in a variety of sectors, regions and worker types and (ii) an estimation of the employment effects of the 1967 reform using a bunching estimator. Additional employment results using this design are detailed in the next section.

Figure D1: BLS industry wage reports

(a) Set of industries we digitized and years available



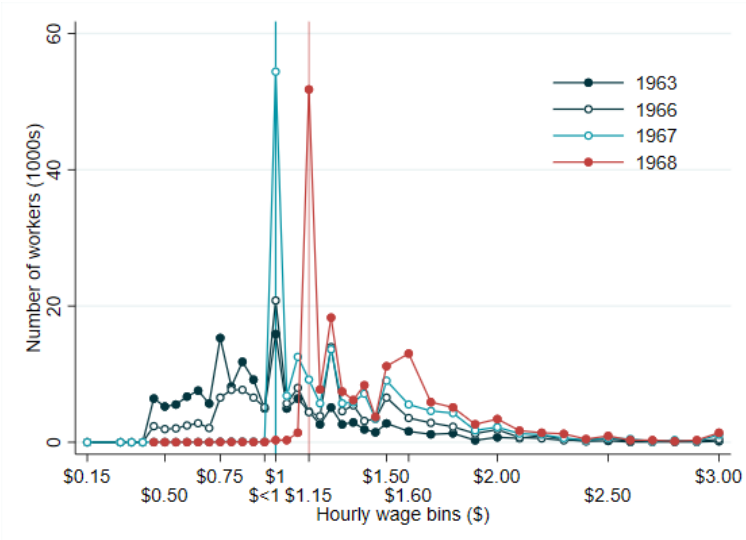
(b) Original format of the BLS data – the example of laundries

Average hourly earnings [1]	United States						Northeast	
	April 1967			April 1968			April 1967	April 1968
	Total	Men	Women	Total	Men	Women		
Under $1.00	0.3	0.4	0.2	0.1	0.3	0.1	0.2	0.2
$1.00 and under $1.05	14.9	6.0	17.5	.2	.3	.2	1.8	.1
$1.05 and under $1.10	2.0	.6	2.4	.1	([2])	.1	.1	([2])
$1.10 and under $1.15	4.1	1.6	4.8	.4	.3	.5	.8	.1
$1.15 and under $1.20	3.6	1.6	4.1	15.0	5.2	17.8	.9	2.1
$1.20 and under $1.25	2.4	1.2	2.7	2.9	1.1	3.4	.7	.7
$1.25 and under $1.30	7.2	5.1	7.8	7.2	4.3	8.0	4.2	1.9
$1.30 and under $1.35	3.1	1.4	3.6	3.1	1.2	3.6	1.5	1.0
$1.35 and under $1.40	4.2	1.9	4.8	3.2	1.5	3.6	2.6	1.1
$1.40 and under $1.45	7.1	4.0	7.9	5.6	2.9	6.4	9.2	5.4
$1.45 and under $1.50	4.2	2.3	4.8	2.6	1.5	2.9	4.7	2.5
$1.50 and under $1.55	9.2	8.7	9.4	5.8	4.6	6.2	18.3	4.5
$1.55 and under $1.60	3.6	2.8	3.8	2.4	1.3	2.8	7.1	2.6
$1.60 and under $1.65	4.2	4.3	4.2	10.5	8.5	11.1	6.2	21.7
$1.65 and under $1.70	2.6	2.6	2.5	6.4	4.3	7.0	3.1	8.0
$1.70 and under $1.75	2.3	2.9	2.1	3.4	3.3	3.4	2.8	5.1
$1.75 and under $1.80	3.2	4.8	2.8	4.3	4.0	4.4	4.0	5.2
$1.80 and under $1.85	2.0	2.6	1.8	2.2	2.6	2.1	2.6	3.0
$1.85 and under $1.90	2.0	2.9	1.7	2.4	3.0	2.2	2.5	3.2
$1.90 and under $1.95	1.4	2.5	1.0	1.6	2.4	1.4	1.7	2.3
$1.95 and under $2.00	1.1	1.7	.9	1.3	1.5	1.2	1.4	1.4
$2.00 and under $2.10	3.6	6.6	2.7	4.9	9.4	3.5	4.9	5.9
$2.10 and under $2.20	1.9	4.0	1.3	2.2	4.4	1.5	2.6	2.9
$2.20 and under $2.30	1.7	3.9	1.1	2.0	4.7	1.2	2.4	2.6
$2.30 and under $2.40	1.1	2.4	.7	1.4	2.8	1.0	1.5	1.9
$2.40 and under $2.50	.9	2.0	.5	.9	1.9	.6	1.4	1.4
$2.50 and under $2.60	1.7	4.6	.9	2.0	4.9	1.2	2.9	3.4
$2.60 and under $2.70	.8	2.1	.4	.8	1.9	.5	1.4	1.3
$2.70 and under $2.80	.7	1.9	.4	.7	2.0	.3	1.5	1.3
$2.80 and under $2.90	.5	1.7	.2	.5	1.3	.3	.9	1.0
$2.90 and under $3.00	.2	.6	.1	.5	1.3	.2	.4	.6
$3.00 and over	2.4	8.3	.8	3.5	11.1	1.0	4.0	5.5
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Number of workers	440,779	99,165	341,614	441,931	99,702	342,229	107,063	109,839
Average hourly earnings [1]	$1.55	$1.91	$1.44	$1.67	$2.04	$1.56	$1.77	$1.88

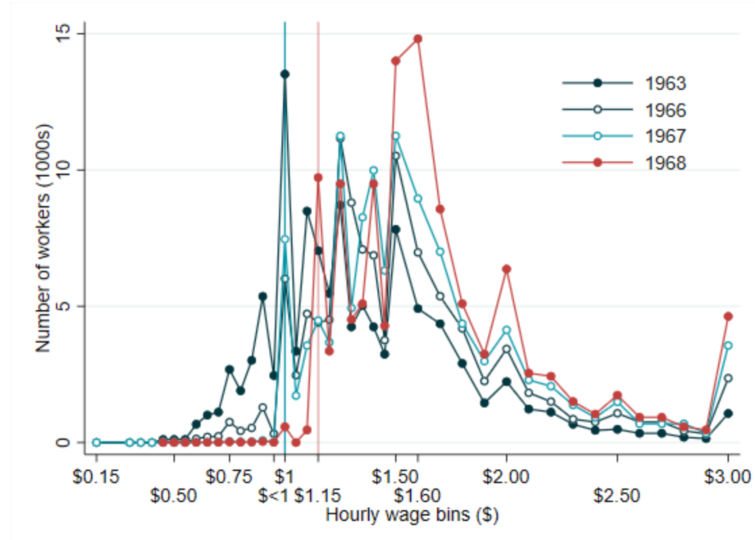
Source: Bureau of Labor Statistics Industry Wage Reports.

Notes: Panel (a) shows the set of industries we digitized: non-durable manufacturing (industries covered in 1938, in dark blue), industries covered in 1967, except agriculture (light blue). It also shows the years for which BLS industry wage reports were available. Panel (b) shows an example of hourly wage tabulations for laundries, a sector in which the minimum wage was introduced at $1 in 1967. These tabulations provide information on the hourly wage distribution by 5- or 10-cent bins. The number of workers in each bin can be easily computed using the information on the percent of workers in each bin and the total number of workers at the bottom of the table.

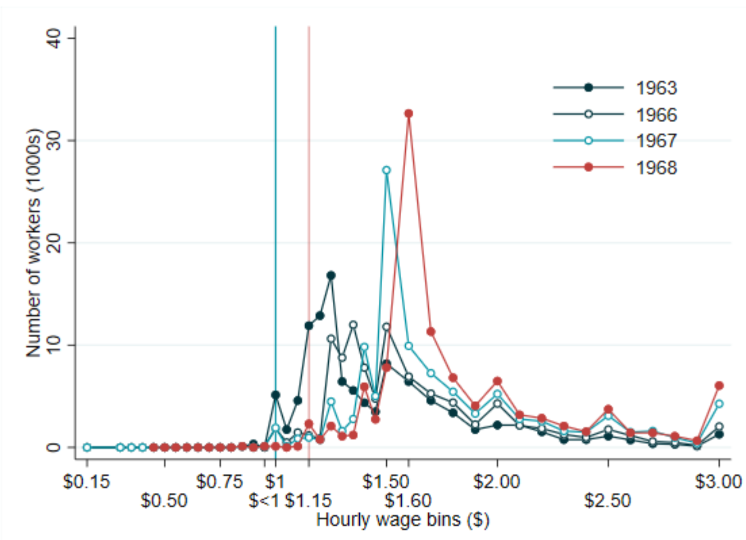
Figure D2: Earnings distributions in laundries, by region



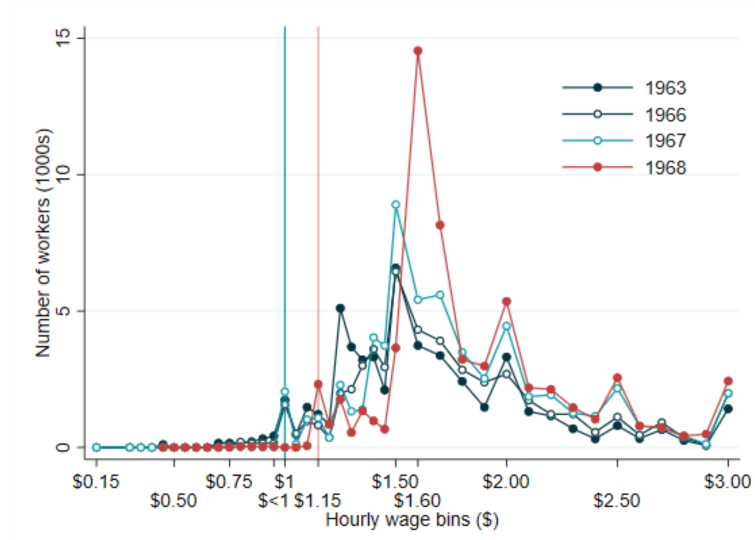
(a) South



(b) Midwest



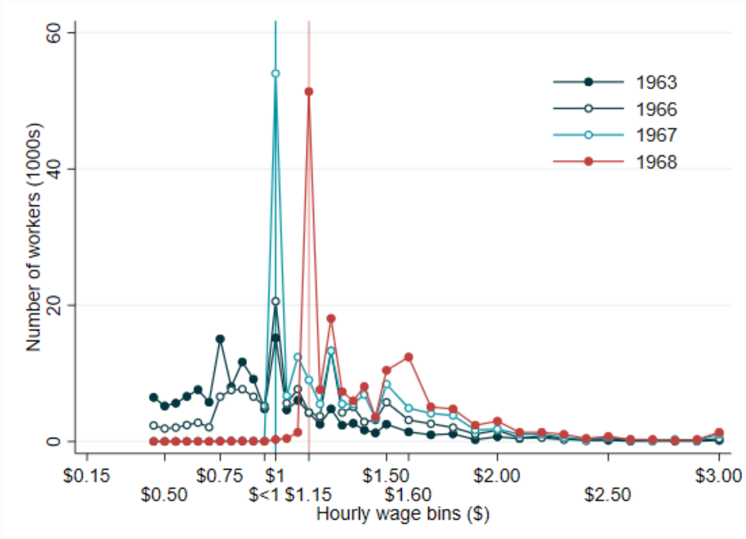
(c) Northeast



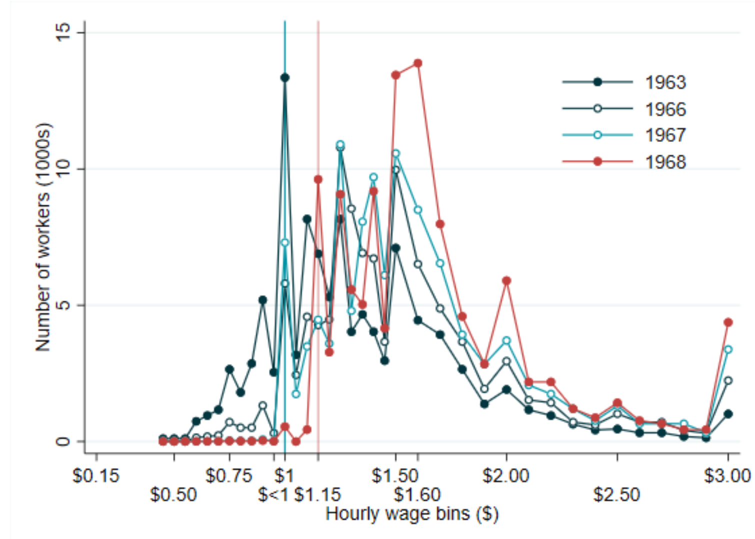
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory workers. Notes: The minimum wage was introduced at $1 in 1967 (blue solid line). It reached $1.15 in 1968 (red solid line).

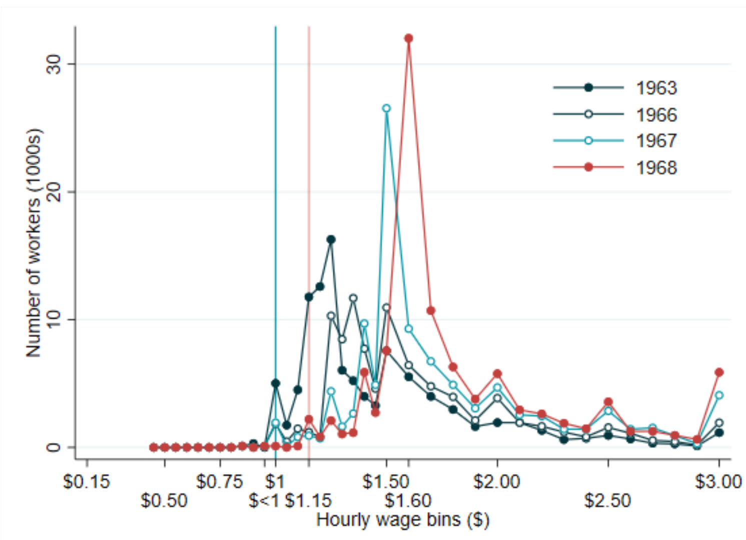
Figure D3: Earnings distributions in laundries (inside plant workers), by region



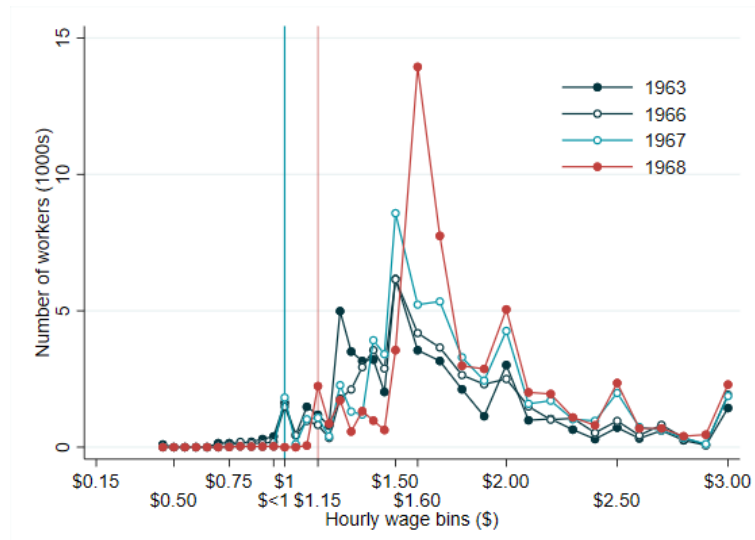
(a) South



(b) Midwest



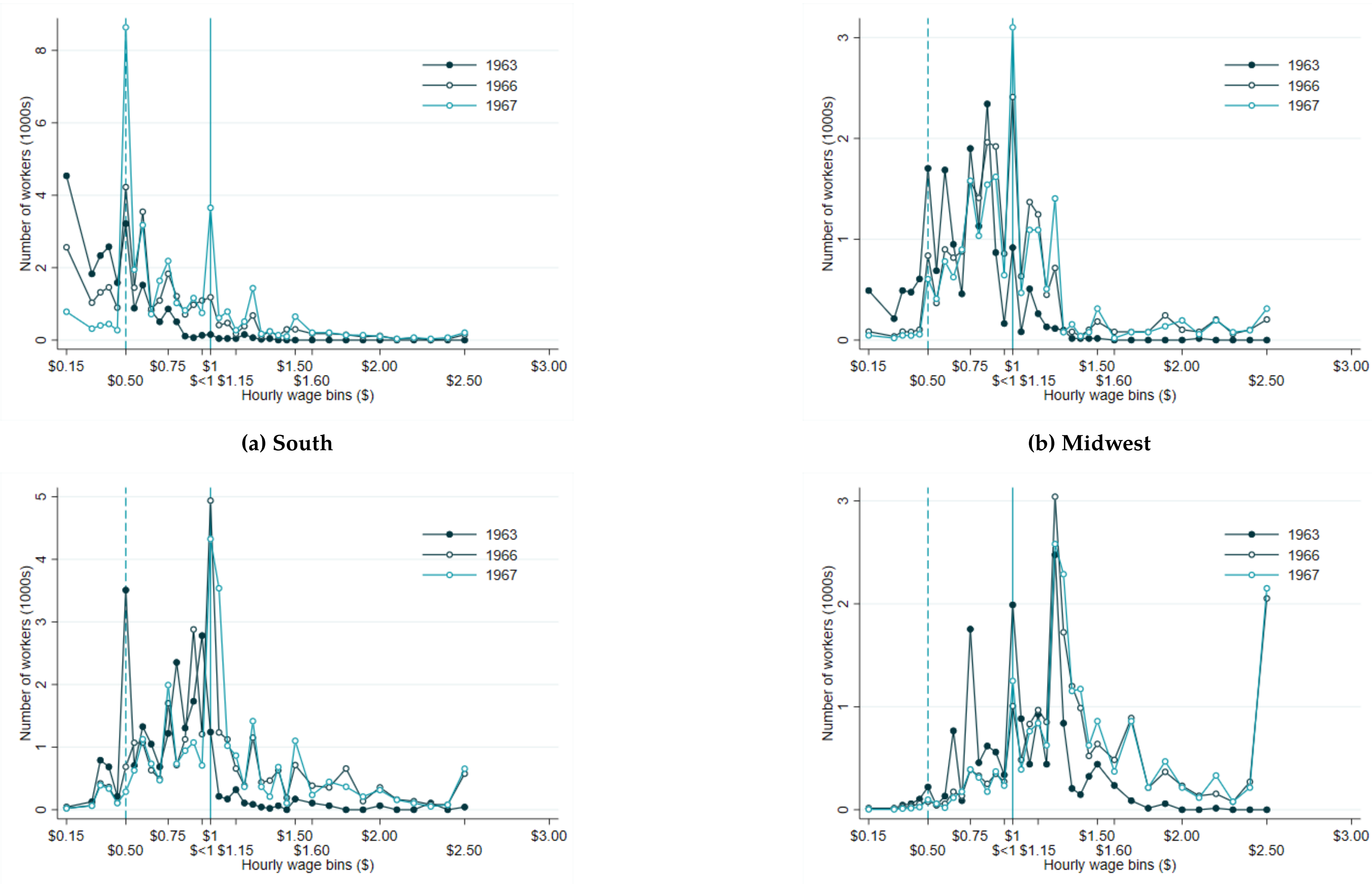
(c) Northeast



(d) West

Source: BLS Industry Wage Reports. Sample: All inside plant workers in laundries. Notes: The minimum wage was introduced at $1 in 1967 (blue solid line). It reached $1.15 in 1968 (red solid line).

Figure D4: Earnings distributions in hotels (tipped workers), by region



(a) South

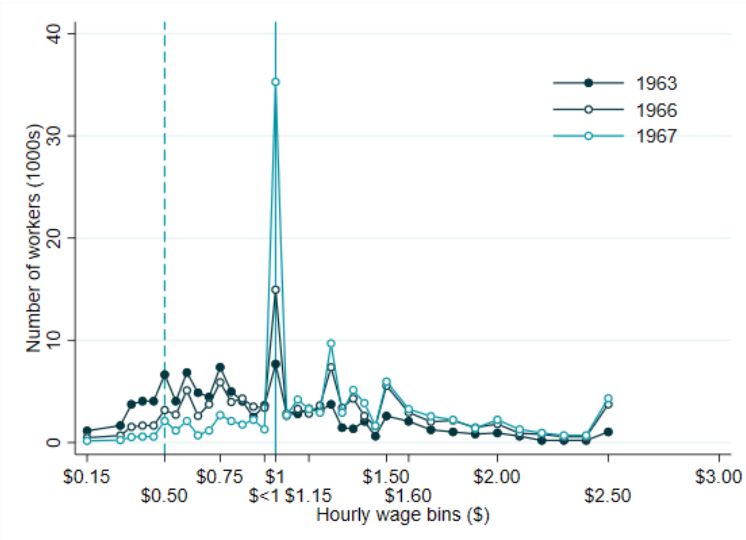
(b) Midwest

(c) Northeast

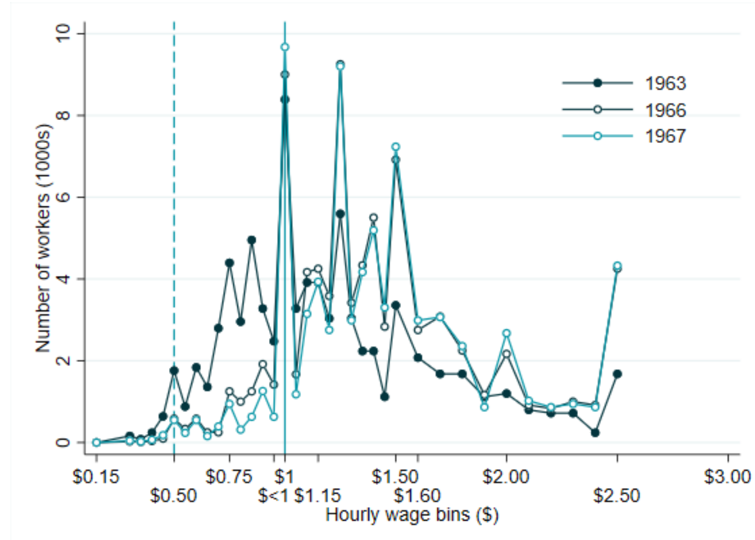
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory tipped workers in year-round hotels, motels, and tourist courts. Notes: The minimum wage was introduced at $0.50 (dashed line) for tipped workers in hotels in 1967. For non-tipped workers, the minimum wage was introduced at $1 (solid line).

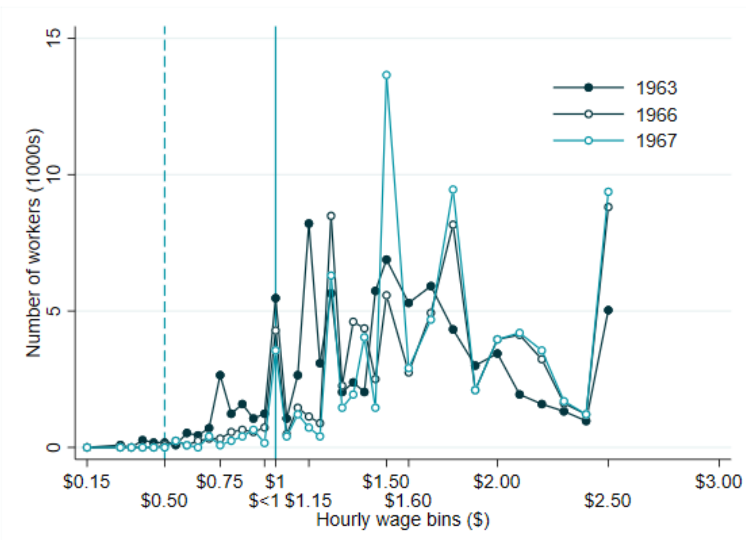
Figure D5: Earnings distributions in hotels (non-tipped workers), by region



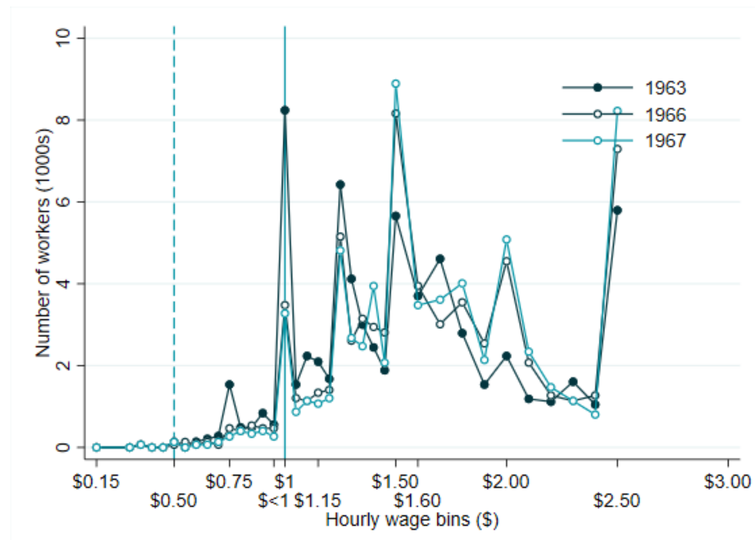
(a) South



(b) Midwest



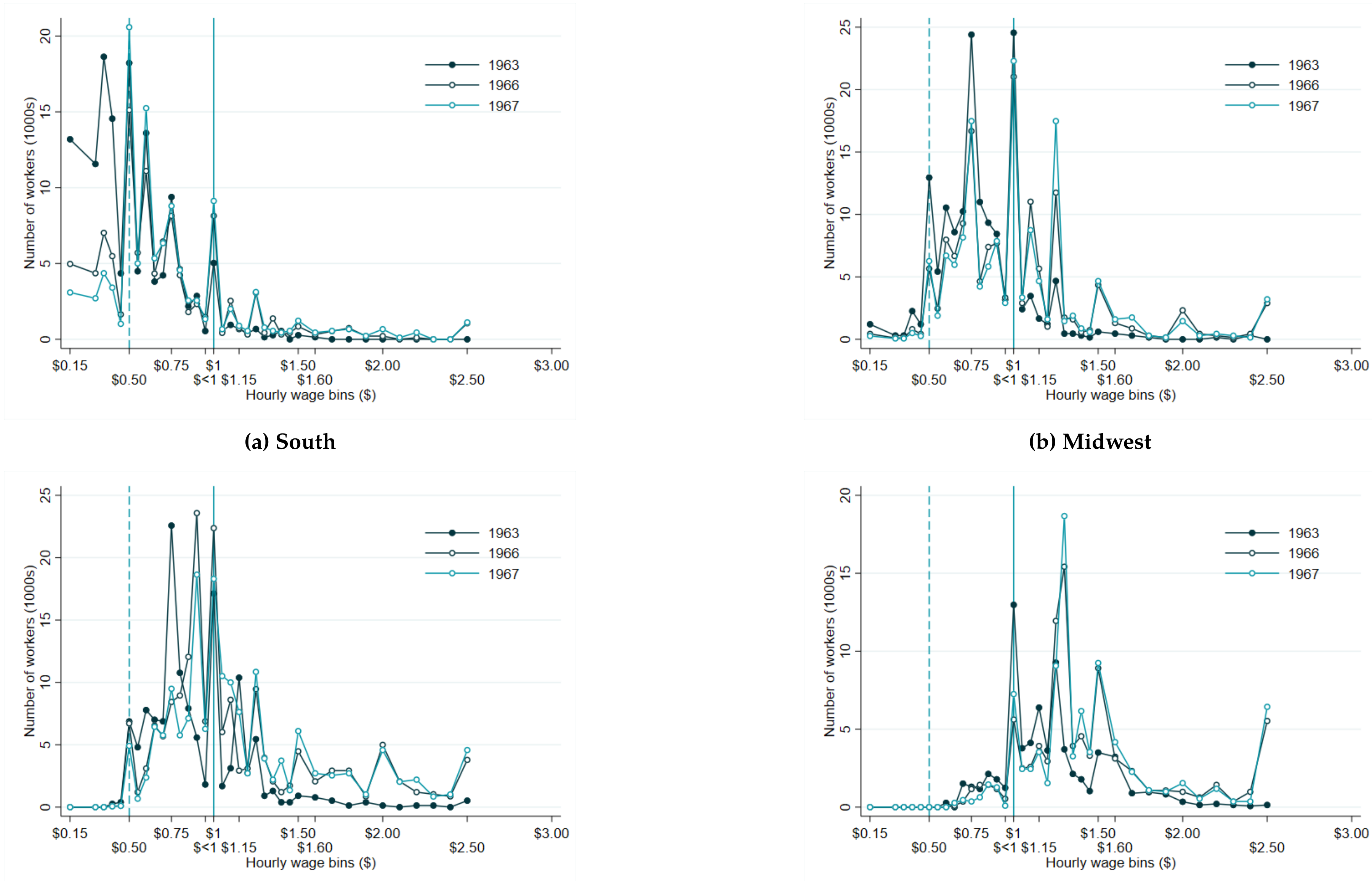
(c) Northeast



(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory non-tipped workers in year-round hotels, motels, and tourist courts. Notes: The minimum wage was introduced at $0.50 (dashed line) for tipped workers in hotels in 1967. For non-tipped workers, the minimum wage was introduced at $1 (solid line).

Figure D6: Earnings distributions in restaurants (tipped workers), by region



(a) South

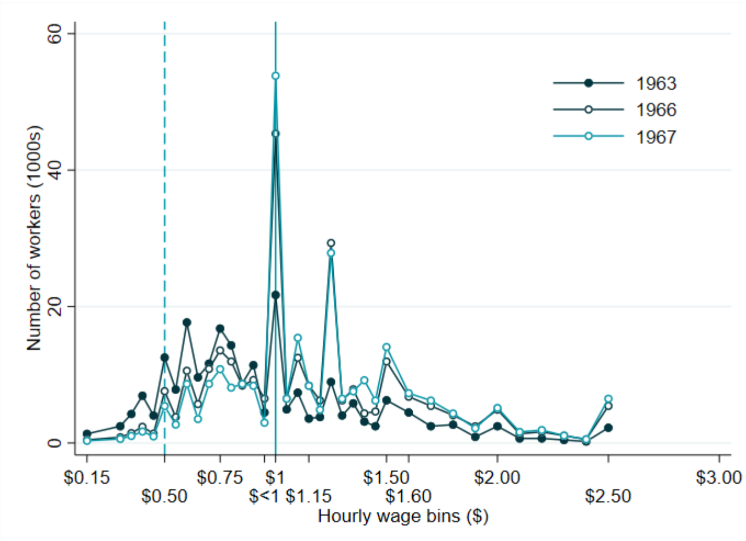
(b) Midwest

(c) Northeast

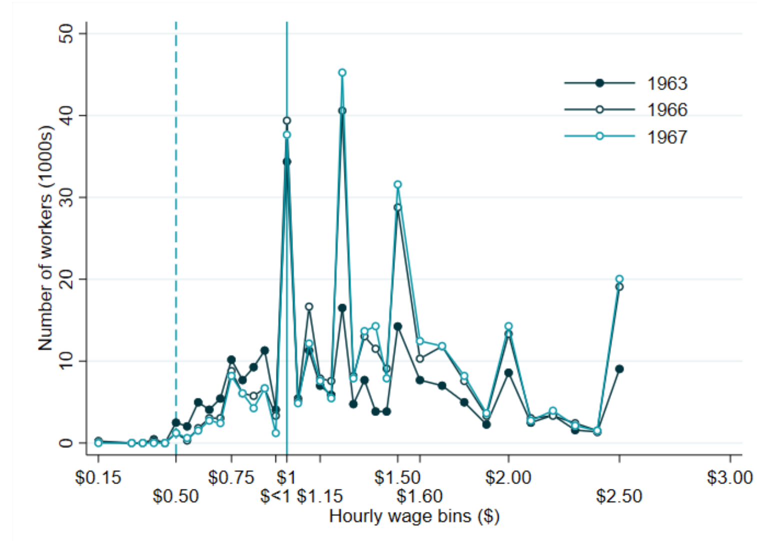
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory tipped workers in restaurants. Notes: The minimum wage was introduced at $0.50 (dashed line) for tipped workers in restaurants in 1967. For non-tipped workers, the minimum wage was introduced at $1 (solid line).

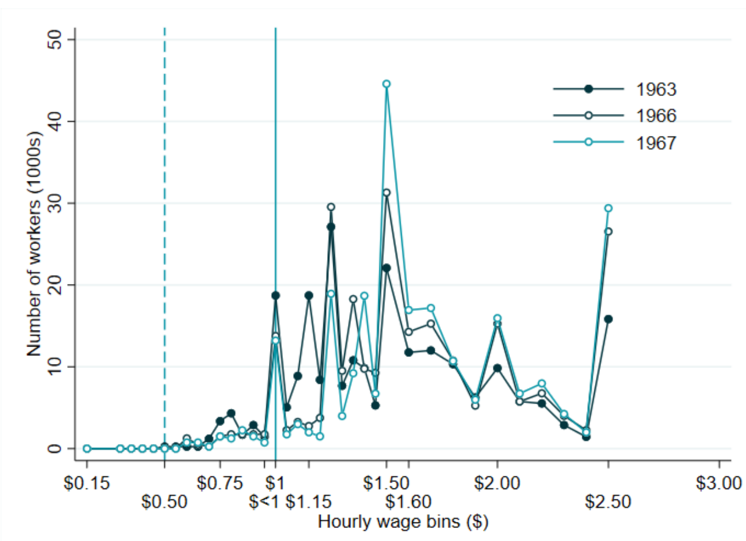
Figure D7: Earnings distributions in restaurants (non-tipped workers), by region



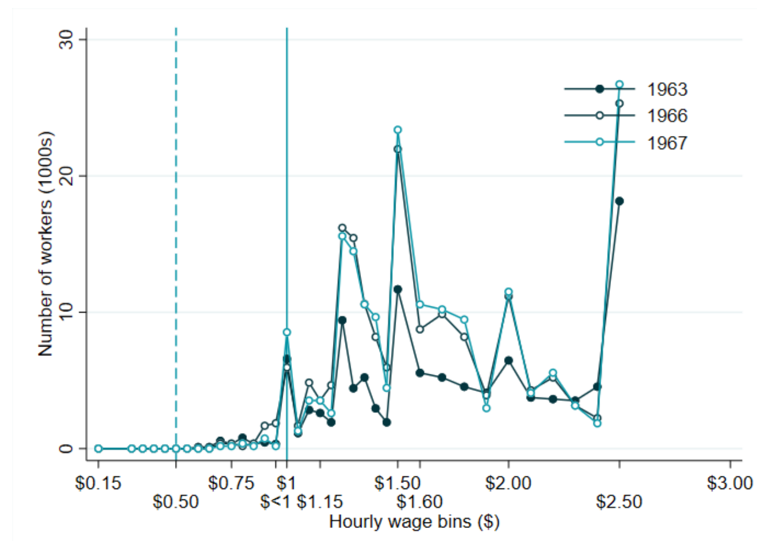
(a) South



(b) Midwest



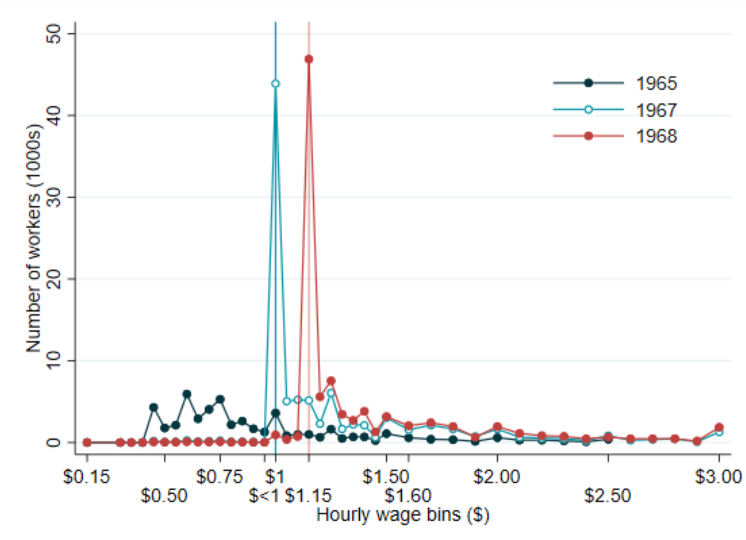
(c) Northeast



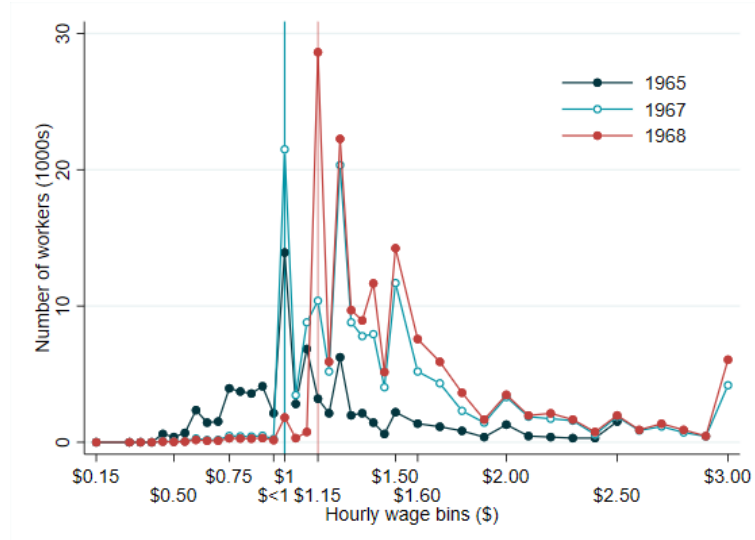
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory non-tipped workers in restaurants. Notes: The minimum wage was introduced at $0.50 (dashed line) for tipped workers in restaurants in 1967. For non-tipped workers, the minimum wage was introduced at $1 (solid line).

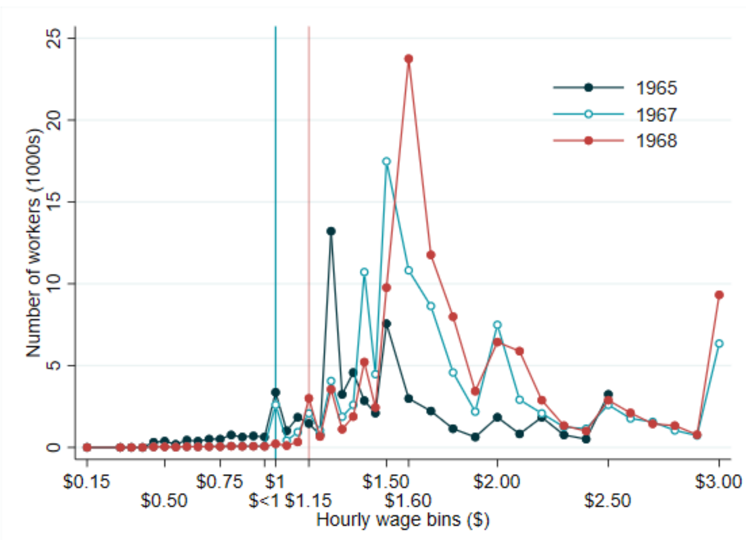
Figure D8: Earnings distributions in nursing homes, by region



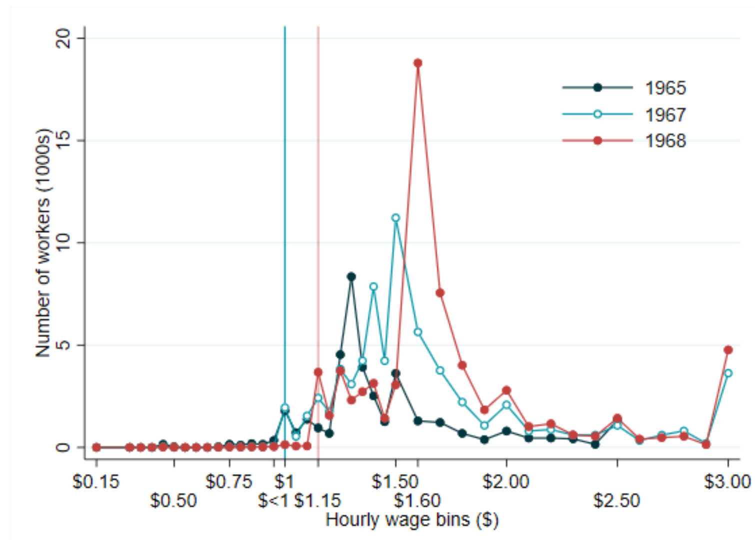
(a) South



(b) Midwest



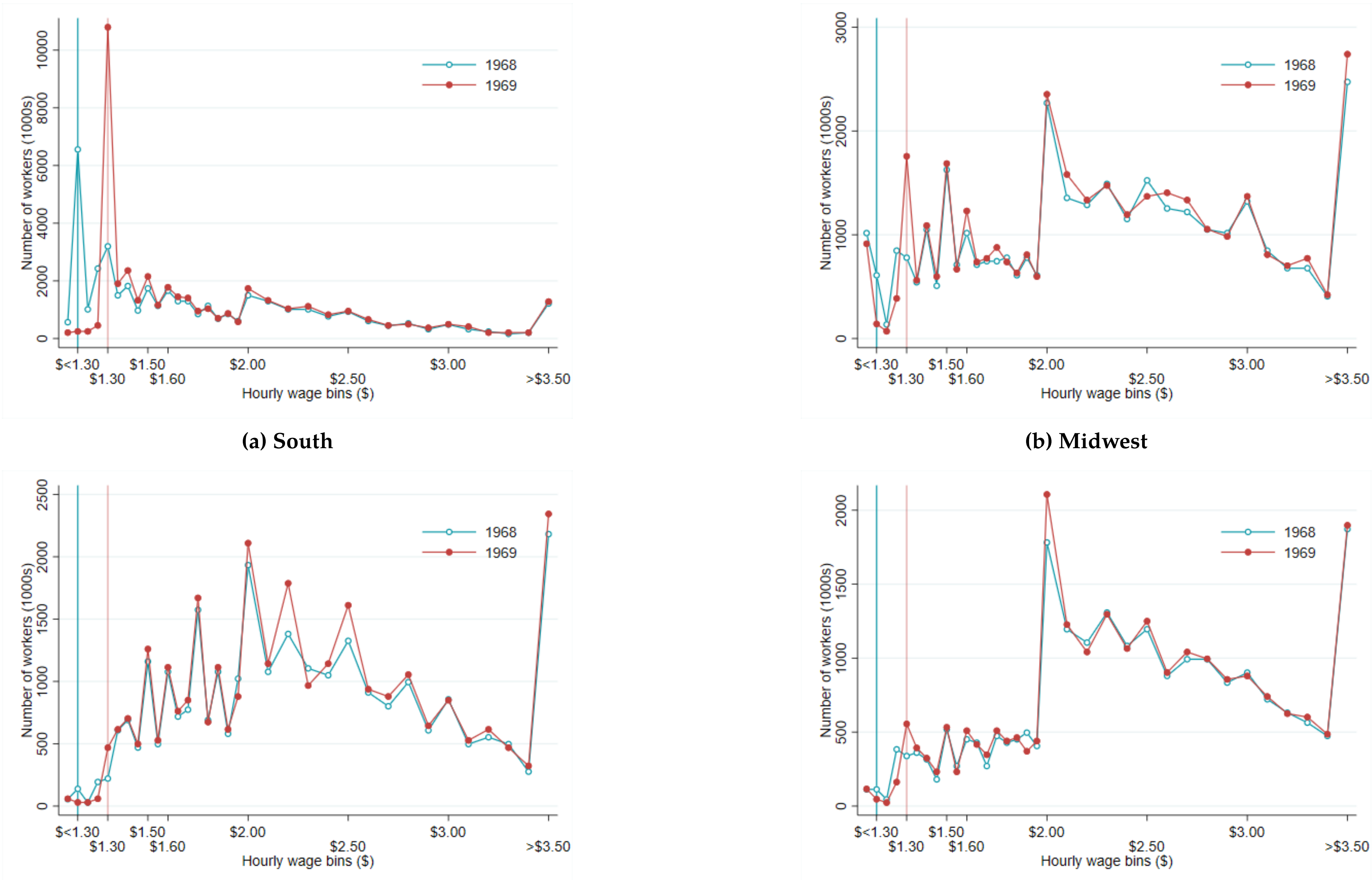
(c) Northeast



(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees in nursing homes and related facilities. Notes: The minimum wage was introduced at $1 in 1967 (blue solid line). It reached $1.15 in 1968 (red solid line).

Figure D9: Earnings distributions in schools, by region



(a) South

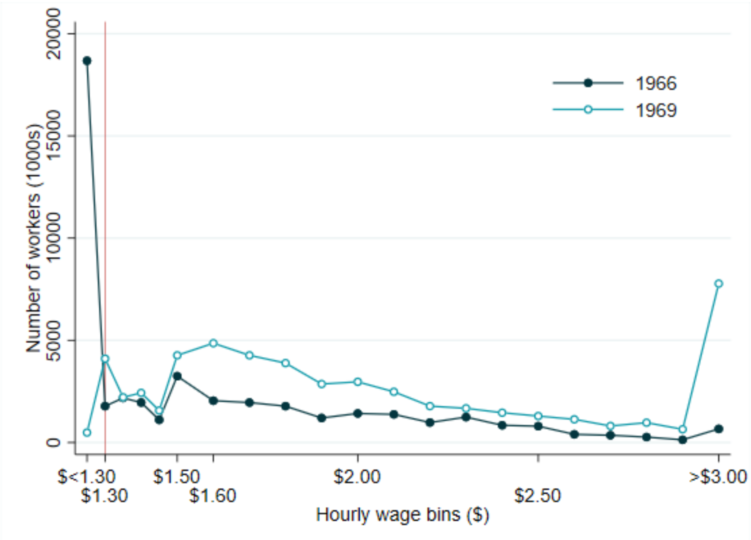
(b) Midwest

(c) Northeast

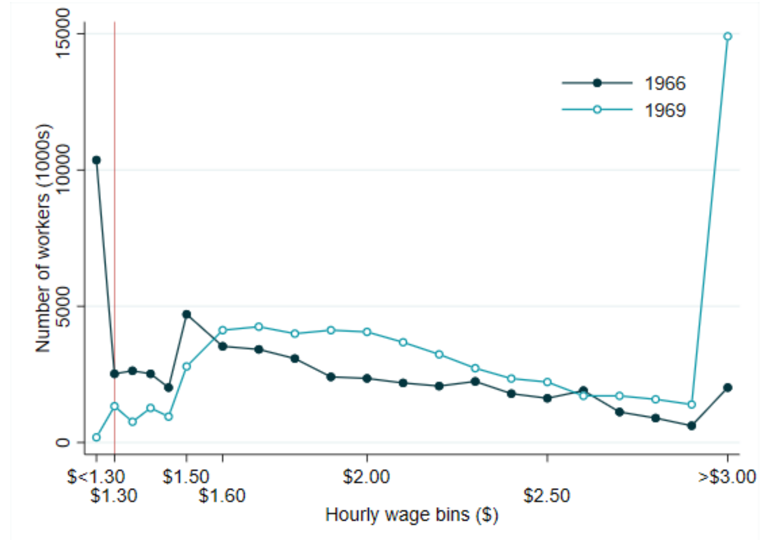
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory non-teaching employees in elementary and secondary schools (e.g., custodial employees, food service employees, office clerical employees, skilled maintenance employees, bus drivers) in schools. Notes: The minimum wage was $1.15 in 1968 (blue solid line), and $1.30 in 1969 (red solid line).

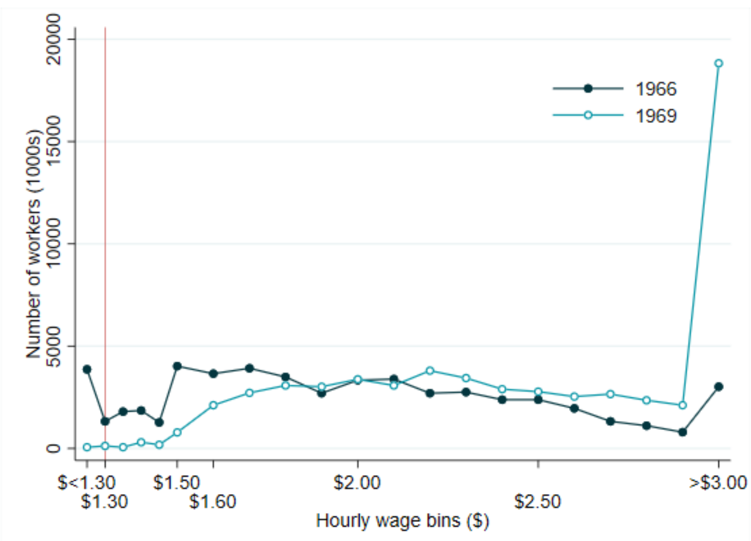
Figure D10: Earnings distributions in hospitals, by region



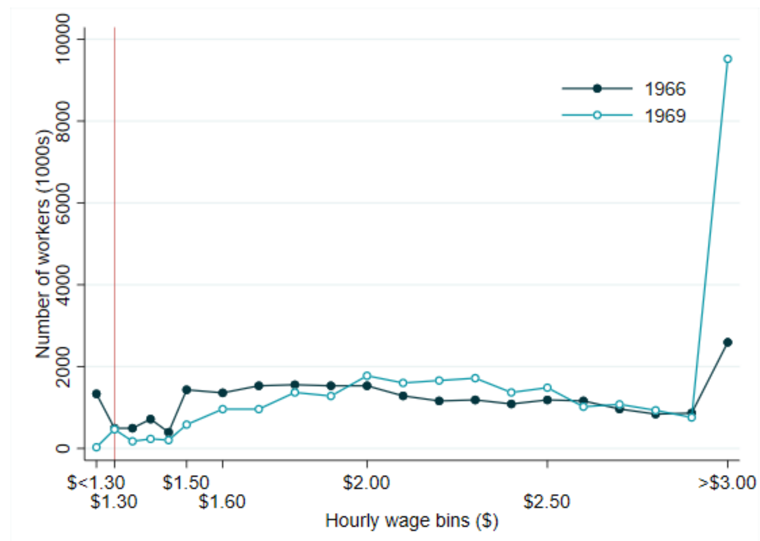
(a) South



(b) Midwest



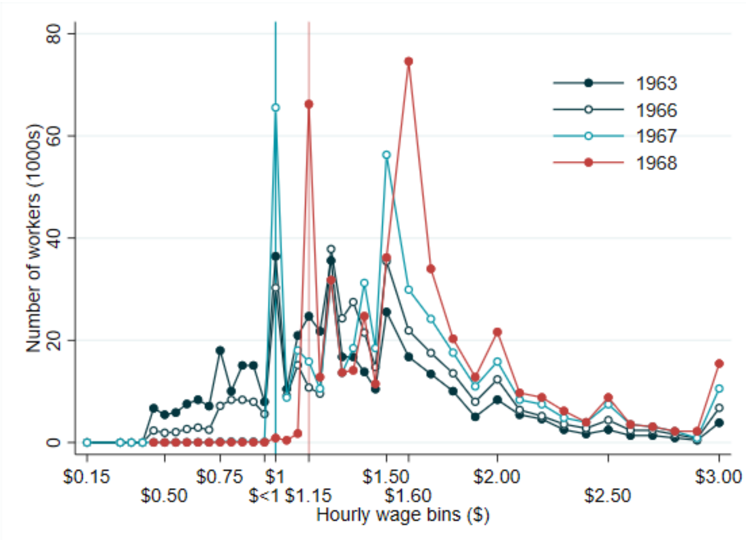
(c) Northeast



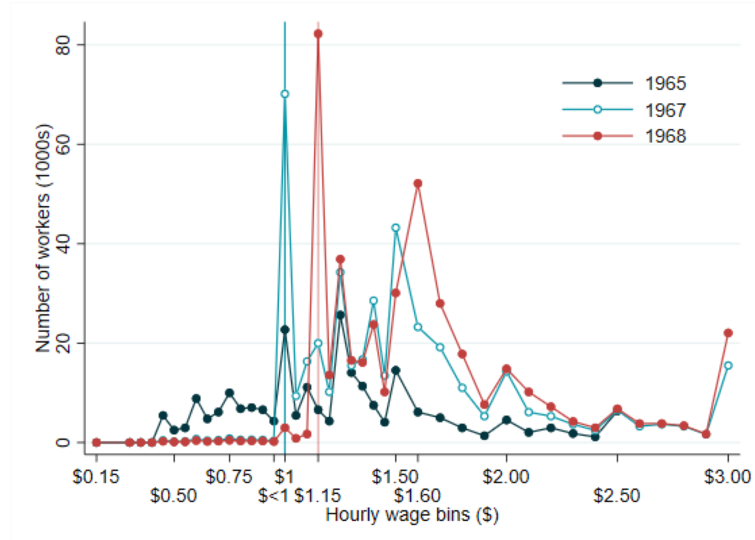
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees in all hospitals except federal hospitals, e.g., nursing aids, porters, maids, kitchen helpers, dishwashers, practical nurses, medical social workers, and dietitians, etc. Notes: The minimum wage was $1.30 in 1969 (red solid line).

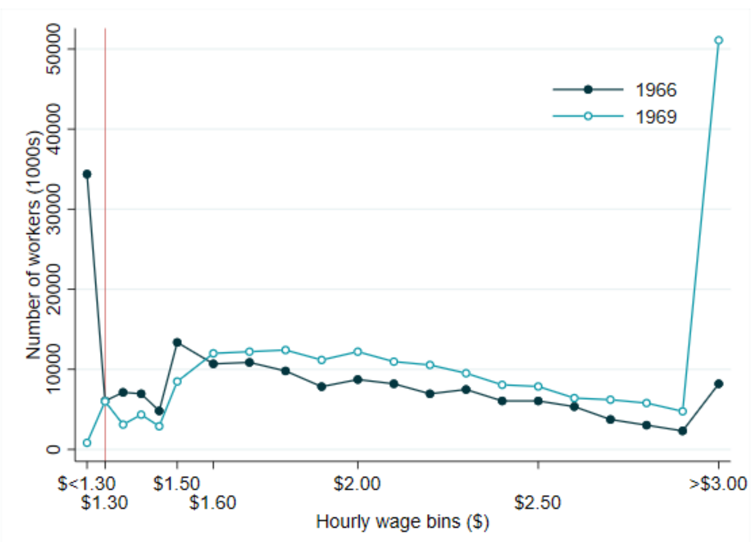
Figure D11: Hourly earnings distributions in the U.S., by industry



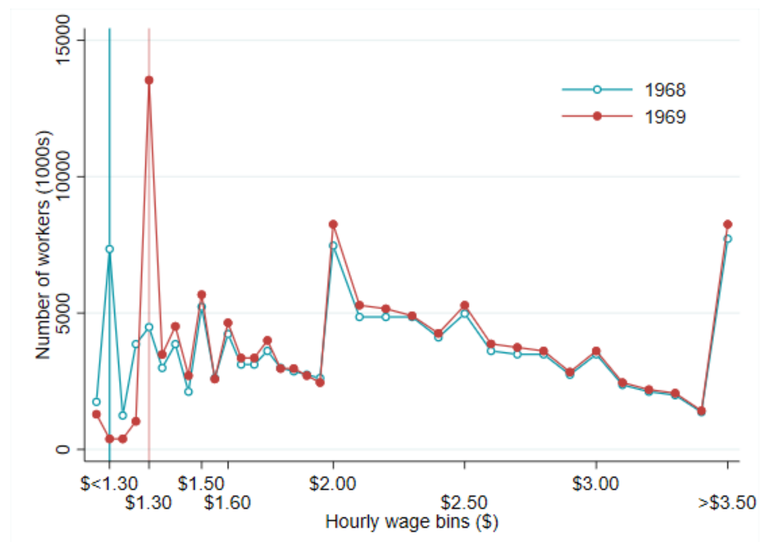
(a) Laundries



(b) Nursing homes



(c) Hospitals



(d) Schools

Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees. Notes: The minimum wage was $1 in 1967, $1.15 in 1968, and $1.30 in 1969 (solid lines).

D.2 Methodology for Nominal Wage Adjustment used in Bunching Estimator

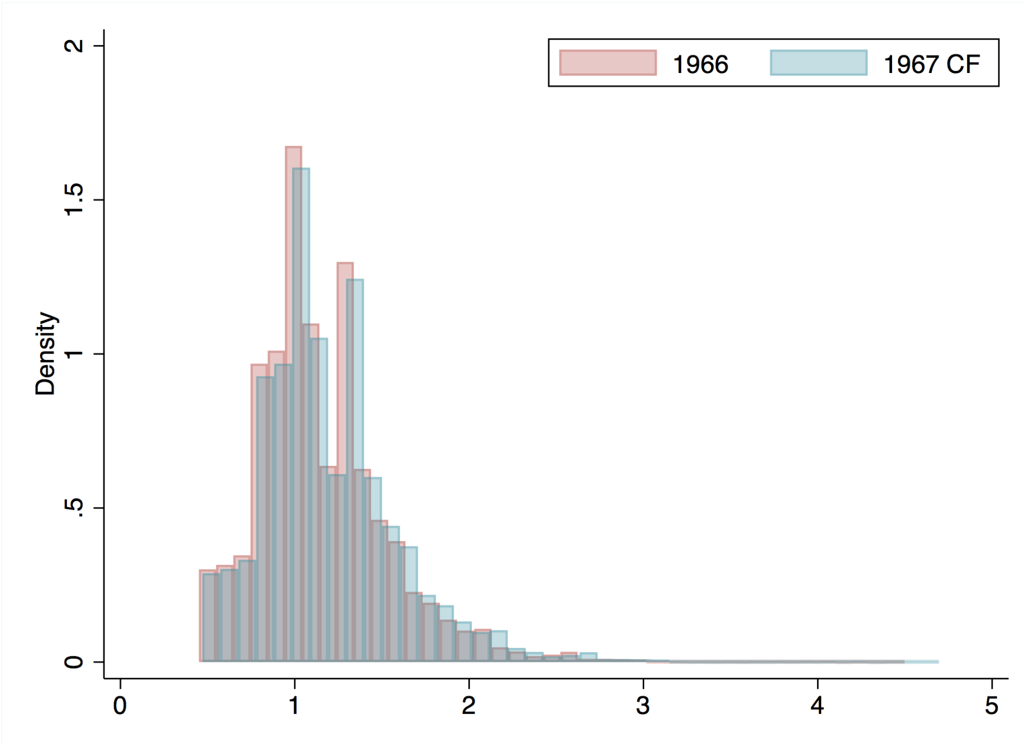
We construct a no-reform counterfactual distribution of wages for the industry-by-region groups by assuming that wages grew according to the 1966-67 national income per capita growth rate of 4.4%. In this section, we describe how we operationalize this approach. Because our data are at the wage-bin level and not the individual level, we inflate the wage distribution in three steps. First, we simulate individual-level data using the observed number of workers per bin and imposing the assumption that wages are uniformly distributed within bins. Second, we adjust wages by the per capita nominal income growth rate from 1966 to 1967. Finally, we collapse the data back into the original nominal bins. The resulting wage-bin-level data have the same nominal bin thresholds as before, but an altered number of workers per bin. Figure D12 demonstrates this shifting of the wage distribution for workers in laundries in the South.

Our assumption of a uniform distribution ignores bunching in the wage distribution at round numbers. We therefore likely over-estimate the average wage of low-wage workers in the counterfactual distribution and as a consequence, underestimate the wage effect of the reform. We do not feel, however, that this assumption systematically biases our employment effect estimates due to our methodology. The movement of jobs away from below \$1 is likely to be minor as is the change in the number of jobs at and up to $1.15 \times$ the minimum wage. This methodology does predict large swings in employment in the bin containing *exactly* \$1 because the growth rate of 4.4% pushes most of the workers in that bin to the following bin, \$1.05 to \$1.10.

D.3 Robustness Checks using an Alternative Threshold for Spillover Effects of the Reform

Figure D13 plots missing versus excess jobs assuming spillover effects of the reform up to 120% of the minimum wage. Once again the number of excess jobs is close to the number of missing jobs across industry and region groups. Using 120% as the threshold generates a slightly greater fitted slope across the 16 points, indicating a slightly more positive employment elasticity overall. The graph also indicates heterogeneity in the employment effect across industries and especially across regions. For example, nursing homes in the Midwest show a slight decline in employment with the number of excess jobs below that of missing jobs.

Figure D12: Simulation of individual observed and counterfactual wages in laundries in the South



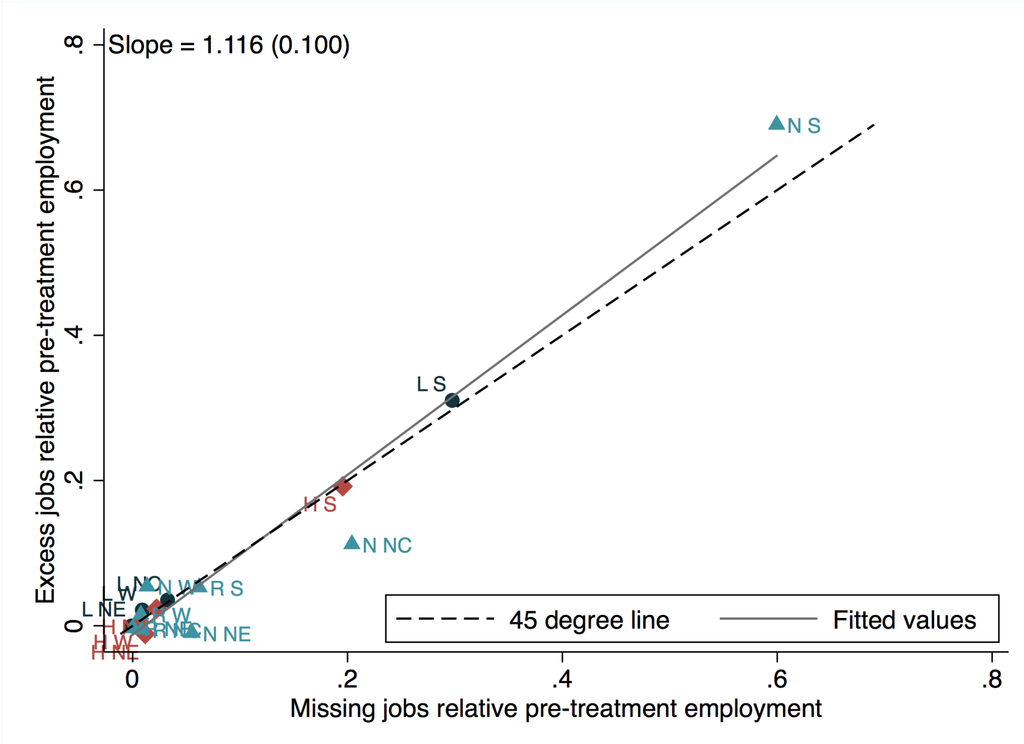
Source: BLS Industry Wage Reports.
Notes: This figure plots a histogram of wages for a simulated population of workers in laundries in the South. In blue are observed 1966 wages and in red is a counterfactual distribution of wages in 1967 where wages are assumed to grow according to the national income per capita growth rate between 1966 and 1967.

D.4 Robustness Checks using an Alternative Employment Estimator in the BLS

We develop an alternative employment estimator and show it produces results consistent with our baseline bunching estimator.

We proceed as follows. We first build counterfactual hourly wage distributions for treated industries, as described in our baseline bunching estimator, i.e. using the nominal 1966-1967 growth rate of per adult U.S. national income (+ 4.4%). We then count the number of workers at the bottom of the wage distribution in 1966 (i.e., at wage levels affected by the minimum wage, adjusted for the growth of the economy) and compare this count to the number of workers observed in 1967 at these same wage levels. We perform a similar computation at the top of the distribution (i.e., at wage levels not affected by the minimum wage). By comparing the 1966-1967 growth rate of employment at the bottom vs. at the top, we can

Figure D13: Missing and excess jobs in the BLS industry wage reports



Source: BLS Industry Wage Reports.
Notes: This figure shows the excess jobs (relative to pre-treatment total employment in that cell) above the new minimum wage and the magnitude of missing jobs below for different industry-region cells. The black dashed line is the 45-degree line where the number of excess jobs equal the number of missing jobs, indicating a zero employment effect. Points above the line indicate positive employment effects while points below the line indicate negative employment effects. Missing and excess jobs are plotted for laundries (L), hotels (H), and restaurants (R) in the South (S), Midwest (denoted "NC" for "North Central" region as in the original BLS reports), Northeast (NE), and West (W) regions. Sample: All nonsupervisory workers, except routemen, in laundries; all non-tipped, nonsupervisory employees in year-round hotels, motels and tourist courts. The minimum wage was introduced at $1 in nominal terms in 1967.

assess the effect of the minimum wage on the number of low-wage workers employed. The identification assumption is that absent the reform, the number of people employed at the bottom of the distribution would have evolved similarly to the number of people employed at the top within treated industries between 1967 and 1968.

As in our baseline bunching estimator, we assume that the part of the distribution affected by the minimum wage is the entire distribution up to 1.15 times the federal minimum wage, i.e. up to $1.15 in 1967. We also assume that the minimum wage does not have any impact in the top 30% of the distribution for treated industries overall, which roughly corresponds to wages above $1.70 in 1967.[90] We investigate how varying the first, second, or both assumptions together affects the results.

Table D1 estimates employment effects by applying the methodology described above.

The top panel presents results for laundries in the South. We find that employment below $1.15 in 1967 is 1.5% higher than 1966 employment below $1.10 (i.e., adjusted for the observed economy-wide nominal growth rate). Similarly, 1967 employment above $1.30 (roughly the top 30% of the distribution) is 3% higher than 1966 employment above $1.25. Assuming that absent the reform, employment at the bottom would have grown at the same rate as at the top (i.e., by 3.0%) we conclude that the reform had small dis-employment effects. With a wage increase for treated workers of +18.2%, the implied employment elasticity is -0.08. This result is somewhat sensitive to the assumptions made about the spillover effect of the minimum wage, however. If we assume there is no spillover, we find a zero effect of the reform on employment (+2.8% compared to +3% at the top, with an average wage increase of +27.1%, i.e., an employment elasticity of -0.01).[91] Although it is not possible to obtain a robust employment elasticity in that particular sector, the key fact is that employment in laundries in the South at and up to 1.3 times the minimum wage grew substantially between 1966 and 1967. This drove an overall expansion in that sector: total employment grew +11.5%, which can be decomposed into +16.8% below $1.30 and +3.0% above.

The bottom panel presents results for laundries, hotels and restaurants combined, for the United States as a whole.[92] Total employment grew by 2.2% in our sample of treated industries between 1966 and 1967, very close to the growth rate observed in the other sectors of the economy (2.0%). Low-wage jobs (those paying less than 1.15 times the minimum wage) also grew by 2.2% between 1966 and 1967. Employment above $1.70 (roughly the top 30% of the distribution) grew slightly more slowly, by 0.8%, implying a positive employment elasticity of 0.16; see Table D1. Our result of a small employment elasticity overall is also robust to varying assumptions on the spillover effects of the minimum wage. As reported in Table D1, considering spillover effects up to 120% of the minimum wage leads to a small negative employment elasticity (-0.28).[93]

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[90] This wage level also corresponds to 1.15 times the highest state minimum wage in force in 1967 ($1.50 minimum in New York).

[91] Allowing for spillover effects through to $1.30, however, implies large positive employment effects, as employment below $1.30 grows by 16.8% between 1966 and 1967.

[92] The estimating sample accounts for 20% of the workforce of the treated industries. For restaurants and hotels, we restrict our sample to non-tipped workers, as we are interested in capturing the effects of the minimum wage increase at $1.

[93] We have also checked that, assuming there are no spillover effects, we obtain a zero employment elasticity (-0.03). This finding suggests that labor-labor substitution (e.g., substitution of $1 workers by slightly higher

One potential concern with our approach is that there may be complementarity between low-wage workers and workers at the top of the distribution (that we use to compute counterfactual employment growth rates at the bottom). For example, the reform may have had negative employment effects of low-skill individuals and led employers to fire some of their supervisors. To address this concern, we assess whether overall employment in the treated industries increased or declined compared to overall employment in the control industries, using CPS data at the industry $\times$ year level. Figure B3a shows that prior to the reform, treated vs. control industries were on similar trends and that in 1967 and 1968 they continued to grow at the same rate. From 1969 onwards, treated industries began growing slightly faster than control industries. We obtain similar results in the BLS industry wage reports data for the sub-sample of BLS industries for which we can track total employment over time. These results suggest that our bunching design is unlikely to under-estimate the dis-employment effect of the reform.

skilled individuals) is not driving our estimates of small employment elasticities.

Table D1: Effect of 1967 reform on total number of jobs

	Threshold for Bottom	
Laundries, South	1×MW	1.15×MW
Employment		
1966-67 Change, Bottom (%)	2.8	1.5
1966-67 Change, Top [$1.30+] (%)	3.0	3.0
1966-67 Change, Total (%)	11.5	11.5
Average Wages		
Bottom in 1966 ($)	0.79	0.88
Bottom in 1967 ($)	1.01	1.04
1966-67 Change (%)	27.06	18.2
Employment Elasticity	0.48	-0.08
All industries, U.S.	1.15×MW	1.20×MW
Employment		
1966-67 Change, Bottom (%)	2.2	-1.3
1966-67 Change, Top [$1.70+] (%)	0.8	0.8
1966-67 Change, Total (%)	2.2	2.2
Average Wages		
Bottom in 1966 ($)	0.9	0.9
Bottom in 1967 ($)	0.96	0.98
1966-67 Change (%)	8.73	7.36
Employment Elasticity	0.16	-0.28

Source: BLS Industry Wage Reports. See figure D1a for the set of tabulations digitized.
Sample: All industries are composed of laundries, restaurants (non-tipped workers) and hotels (non-tipped workers).
Notes: The bottom of the distribution is the part of the distribution that is affected by the minimum wage: for example, it varies from 100% × the value of the minimum wage to 115% × the value of the minimum wage for laundries. The top of the distribution is the part of the distribution that is not affected by the minimum wage. For laundries in the South, we define the top of the distribution as the part of the distribution where hourly wages are at or above $1.30 an hour in 1967 (i.e. the top 34% of the distribution). For all industries in the U.S., we define the top of the distribution as the part of the distribution where hourly wages are at or above $1.70 an hour in 1967 (i.e. the top 28% of the distribution). The employment elasticity is calculated for the bottom of the distribution as the ratio between the employment change at the bottom and the average wage increase at the bottom.

Appendix E Cross-State Designs

E.1 Definition of Treatment

Baseline cross-state design: strongly vs. weakly treated states. A state is strongly treated if it had no minimum wage law applying to men or women as of January 1966, as reported in the Report of the Minimum Wage Study Commission (1981) and the Department of Labor Handbook on Women Workers (1965). A state-group is strongly treated if the states making up the state-group had no minimum wage law for more than 50% of the population in the state-group.

The strongly treated state groups are the following ones: 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 Figure 7).

The share of workers working at or below the 1967 federal minimum wage pre-reform (i.e. in 1966) is twice as large in the strongly treated states (11.2%) as in the weakly treated states (5.7%).

We also show that, as expected, the earnings effect measured using our main cross-industry design is higher among the newly covered industries (6.7%) than in the control industries (3%) (see Appendix Table E1). Consistent with our cross-industry design, the earnings effect is also much higher for black workers (12.3%) than for white (2.5%) and concentrated among low-education workers (14.8% vs. 2.2%).

Table E1: Wage effect in treated and control industries, by race and education level, using the baseline cross-state design

	All	Treated	Control	Black	White	Low-educ.	High-educ
Strongly treated states × 1967-1972	0.040***	0.067**	0.030***	0.123***	0.025***	0.144***	0.022**
	(0.010)	(0.024)	(0.007)	(0.025)	(0.008)	(0.033)	(0.010)
Obs	534,977	134,896	272,896	51,666	483,311	23,793	361,895
Controls	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y	Y

Source: March CPS 1962-1981.Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. Notes: Controls for 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.

Alternative cross-state design #1: Kaitz index at the state level in 1966. In order to see how the effects of the 1967 reform varied with a more precise definition of the *intensity* of the treatment, we developed an alternative cross-state design that uses the state-level Kaitz Index in 1966 as the treatment variable. The Kaitz index is a weighted minimum-to-median-wage ratio that takes state-, demographic- and industry-specific minimum wages and composition of the workforce (e.g., each worker's state, demographic group, and industry) into account. We note that the economy-wide Kaitz Index that takes into account state minimum wage laws exhibits a jump in 1967 (see figure E1).

The Kaitz Index at the state level is defined here as:

$$\text{Kaitz Index}_y = \sum_j \frac{N_{yj}}{N_y} * \frac{\text{min.wage}_{yj}}{\text{median wage economy}} \tag{12}$$

with N_{yj} as 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), N_y as the number of workers working full-time full-year in all industries in each year y, min.wage$_{yj}$ as 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 $\times$ state $\times$ gender $\times$ month), in each year y, and median wage economy as the economy-wide median wage for full-time, full-year workers in our sample. This measure of the pre-treatment level of the state-level Kaitz Index captures the variation in state minimum wage laws by gender and industry, as well as variation in the sectoral composition of the workforce in each state.

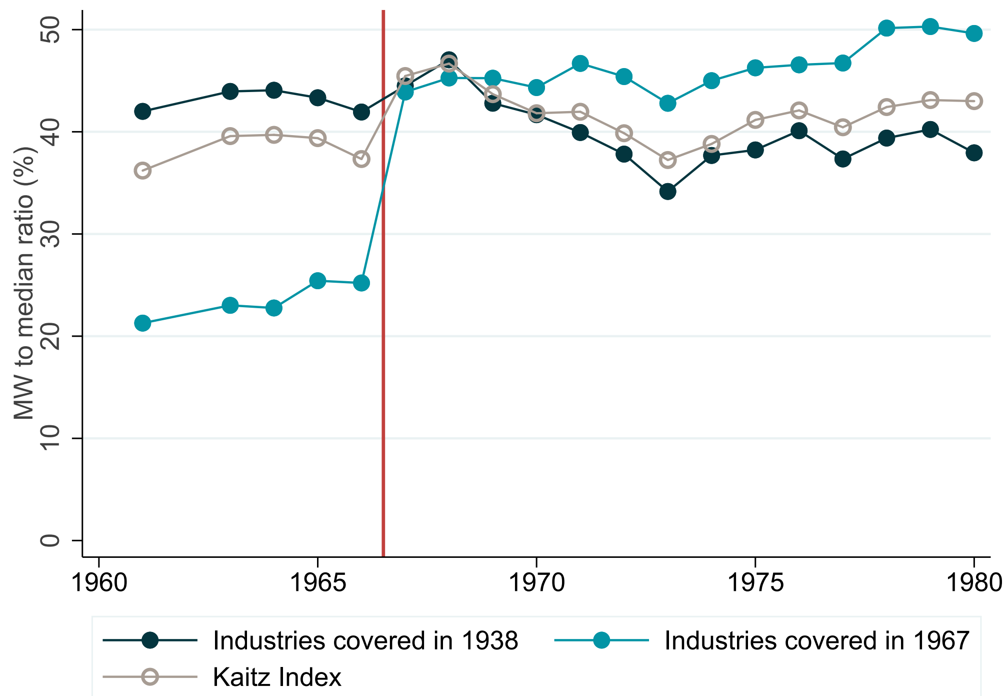
Alternative cross-state design #2: Share of workers with wages below $1.60 in 1966. Another way to capture the state-level variations in the intensity of the 1967 reform is to take the fraction of affected workers as a treatment variable. We use here the share of workers with wages below $1.60 in 1966, as in Bailey et al. (2018).[94]

E.2 Wage and Employment Effects using Cross-State Designs by Gender, Education Level and Cohort

Results on wage and employment effects by gender, education level and cohort using our main cross-state designs are reported in Appendix Table E3 and Figures E3a, E2b and E3b below. In particular, employment elasticities with respect to average wage are either slightly positive or negative, but are not distinguishable from 0 across any of the subgroups considered

[94] See their Table 1 p.26.

Figure E1: Minimum wage to median ratio using state minimum wage laws



Source: March CPS 1962-1981 for median wages.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: This figure depicts the minimum-to-median-wage ratio for full-time (40 hours a week) and full-year (52 weeks per year) workers, taking state minimum wage legislation into account. The medians are calculated separately for the industries covered in 1938 and the industries covered in 1967. The Kaitz Index is defined here as: Kaitz Index$_y = \sum_j \frac{N_{yj}}{N_y} * \frac{\text{min.wage}_{yj}}{\text{median wage economy}}$, with N_{yj} as the number of full-time, full-year workers in our sample by industry type j (i.e. either industries covered in 1938 or industries covered in 1967), N_y as the number of full-time, full-year workers in all industries in each year y, min.wage$_{yj}$ as the minimum wage law that applies at the state level in industry type j (i.e., taking into account all differences in minimum wage legislation at the industry $\times$ state $\times$ gender $\times$ month level), in each year y, and the "median wage economy" as the economy-wide median wage for full-time, full-year workers in our sample.

(except a slight positive employment elasticity for low-education workers when the outcome is measured as the probability of being employed vs. unemployed or not in the labor force, as noted in Section 5.2 in the main text).

Our results using the alternative cross-state designs, using the 1966 state-level Kaitz Index measure and the share of workers with wages below \$1.60 in 1966 are reported in Tables E4

Table E2: Values of state-level Kaitz index in 1966 (percent)

District of Columbia	15.24	South
Washington-Oregon-Alaska-Hawaii	26.17	West
Delaware-Maryland-Virginia-West Virginia	29.04	South
Montana-Wyoming-Colorado-New Mexico-Utah-Nevada-Arizona-Idaho	29.99	West
California	30.31	West
Illinois	30.98	Midwest
Ohio	31.74	Midwest
Iowa-N Dakota-S Dakota-Nebraska-Kansas-Minnesota-Missouri	33.46	Midwest
Texas	33.58	South
New Jersey	33.82	Northeast
Florida	35.64	South
Michigan-Wisconsin	35.65	Midwest
Pennsylvania	35.71	Northeast
New York	35.82	Northeast
Indiana	37.38	Midwest
Connecticut	37.42	Northeast
Arkansas-Louisiana-Oklahoma	39.19	South
Maine-Massachusetts-New Hampshire-Rhode Island-Vermont	39.29	Northeast
Kentucky-Tennessee	41.83	South
North Carolina-South Carolina-Georgia	43.42	South
Alabama-Mississippi	46.46	South

Source: Authors' analysis of March CPS 1962-1980.

Notes: See definition of the 1966 Kaitz Index in equation 12.

and E5 respectively. The pattern of the results across subgroups is consistent with our main cross-state design. The cross-state design comparing the strongly treated states vs. weakly treated states is therefore robust to alternative specifications.

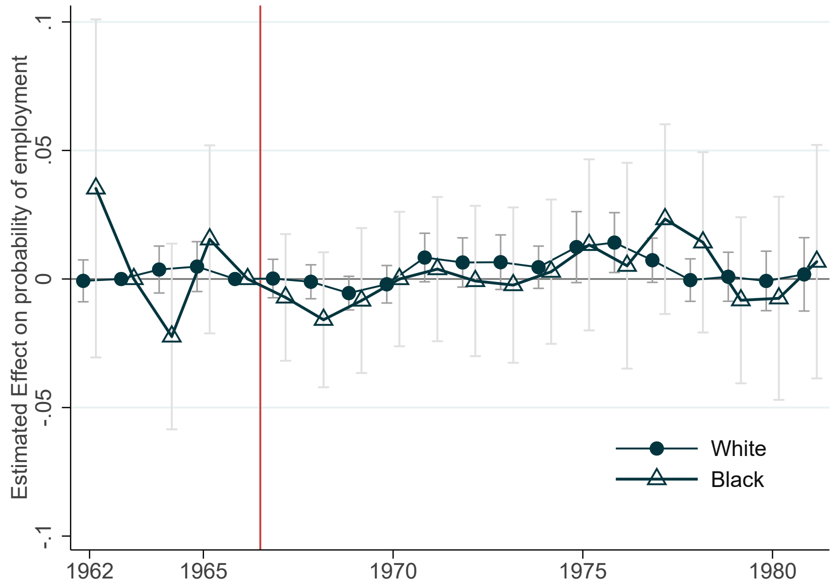
Table E3: Effect of the 1966 FLSA using strongly vs. weakly treated states

	All	Black	White	Men	Women	Low-educ.	High-educ
Strongly treated states × 1967-1972							
Earnings	0.040***	0.123***	0.025***	0.041***	0.038***	0.054***	0.024**
	(0.010)	(0.025)	(0.008)	(0.010)	(0.011)	(0.015)	(0.011)
Emp. (vs. unemp.)	0.000	-0.008	0.000	0.000	-0.000	0.000	0.002
	(0.002)	(0.007)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Emp. (vs. unemp/nilf)	0.006	0.013	0.007*	0.005*	0.007	0.016***	0.005
	(0.004)	(0.009)	(0.004)	(0.003)	(0.007)	(0.005)	(0.004)
Annual Hours	0.006	-0.000	0.006	0.003	0.014	0.003	0.000
	(0.006)	(0.013)	(0.006)	(0.005)	(0.009)	(0.009)	(0.006)
Obs	534,885	51,658	483,227	336,047	198,838	145,507	548,135
Controls	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y	Y
Emp. (vs. unemp.) elasticity	0.00	-0.07	0.02	0.01	-0.01	0.00	0.08
se	(0.05)	(0.06)	(0.08)	(0.06)	(0.07)	(0.07)	(0.09)
lower bound	-0.10	-0.18	-0.15	-0.10	-0.14	-0.13	-0.09
upper bound	0.10	0.04	0.18	0.12	0.12	0.13	0.25
Emp. (vs.unemp/nilf) elast.	0.22	0.15	0.41	0.14	0.38	0.46	0.31
se	(0.15)	(0.11)	(0.25)	(0.08)	(0.43)	(0.20)	(0.29)
lower bound	-0.07	-0.07	-0.08	-0.01	-0.47	0.08	-0.26
upper bound	0.51	0.37	0.89	0.29	1.23	0.84	0.87
Annual Hours elasticity	0.15	-0.00	0.26	0.07	0.36	0.06	0.30
se	(0.16)	(0.11)	(0.28)	(0.12)	(0.29)	(0.16)	(0.33)
lower bound	-0.16	-0.21	-0.29	-0.15	-0.22	-0.26	-0.35
upper bound	0.45	0.21	0.81	0.30	0.94	0.37	0.94

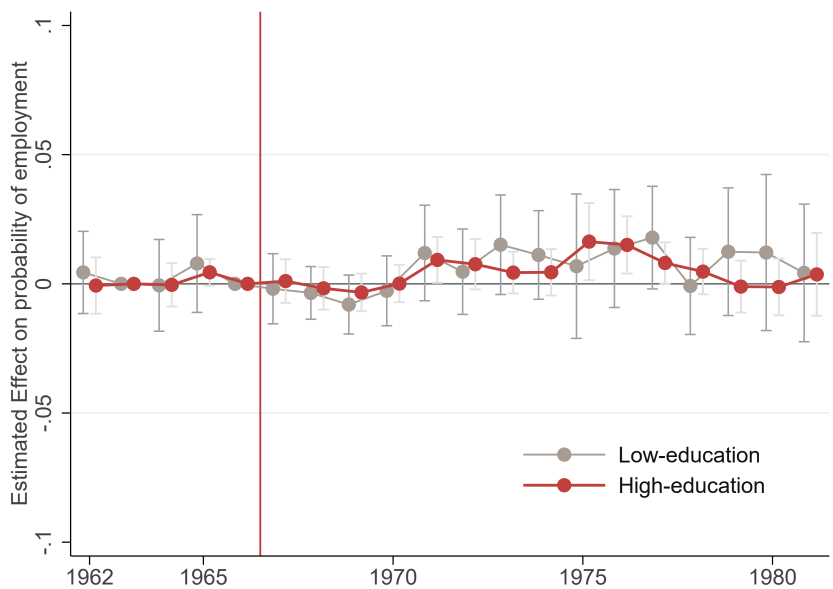
Sources: March CPS 1962-1981.
Sample: For regressions on log annual earnings and on log annual number of hours worked per year regressions: adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. For regressions on employment (measured as probability of being employed vs. unemployed or vs. unemployed or not in the labor force): adults 25-55, black or white, either employed, unemployed or not in the labor force.
Notes: This table reports the coefficient on the interaction between the period 1967-72 and strongly treated states. 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. Controls for employment regressions are gender, race, years of schooling, a quadratic in age and marital status. Controls for regressions on log annual hours are gender, race, years of schooling, a cubic in experience, occupation and marital status. Standard errors are clustered at the state level. Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling.

Figure E2: Impact of the 1966 FLSA on employment across subgroups (1/2)

(a) Black vs. white workers



(b) Low-education vs. high-education



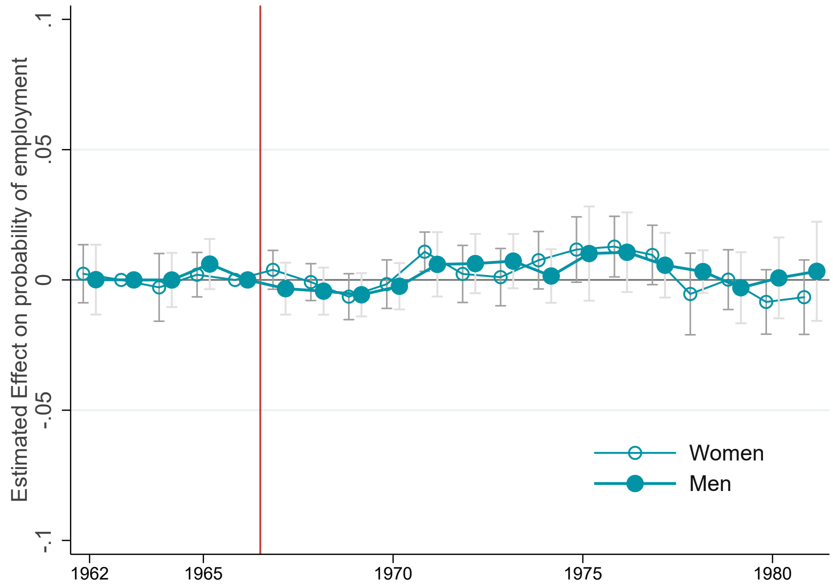
Source: CPS 1962-1981.

Sample: Adults 25-55, black or white, either employed or unemployed.

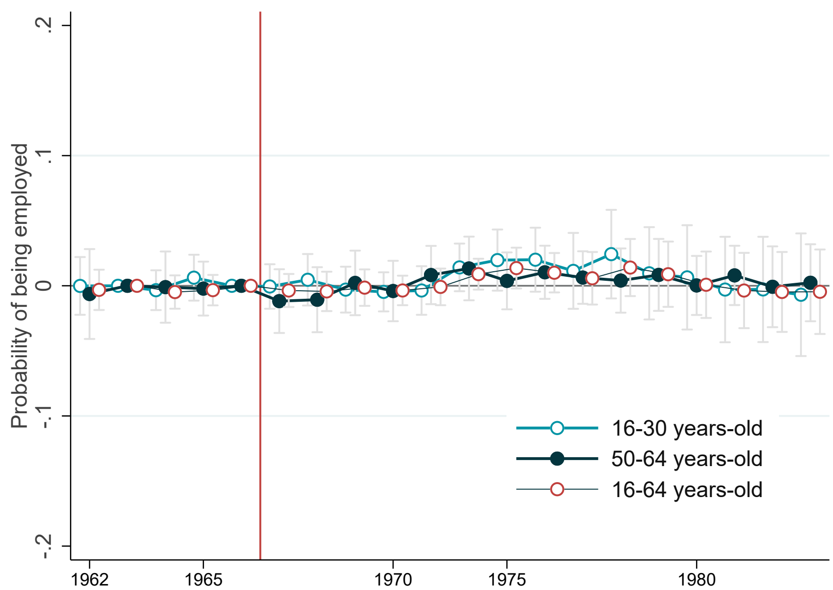
Notes: The outcome of interest is the probability of being employed vs. unemployed. Controls for gender, race (panel (b) only), years of schooling, a quadratic in age and marital status. Employment effects measured relative to the year 1966. Includes state and time fixed effects. Standard errors are clustered at the state level. Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling.

Figure E3: Impact of the 1966 FLSA on employment across subgroups (2/2)

(a) By gender



(b) By cohort



Source: CPS 1962-1981.

Sample: Adults 25-55, black or white, either employed or unemployed.

Notes: The outcome of interest is the probability of being employed vs. unemployed. Controls for gender, race (panel (b) only), years of schooling, a quadratic in age and marital status. Employment effects measured relative to the year 1966. Includes state and time fixed effects. Standard errors are clustered at the state level. Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling.

Table E4: Effect of the 1966 FLSA using the 1966 Kaitz index

	All	Black	White	Men	Women	Low-educ.	High-educ
1966 Kaitz Index × 1967-1972							
Earnings	0.014***	0.051***	0.006	0.014**	0.013***	0.031***	0.000
	(0.005)	(0.013)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)
Emp. (vs. unemp.)	-0.000	-0.005	0.000	-0.000	-0.000	-0.002	0.001*
	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Emp. (vs. unemp/nilf)	-0.000	0.000	0.000	0.000	-0.001	0.002	0.000
	(0.002)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Annual Hours	0.000	-0.003	0.001	0.000	0.002	0.004	-0.002
	(0.003)	(0.010)	(0.003)	(0.002)	(0.005)	(0.005)	(0.003)
Obs	534,885	51,658	483,227	336,047	198,838	145,507	389,378
Controls	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y	Y
Emp. (vs. unemp) elast.	-0.03	-0.11	0.06	-0.02	-0.02	-0.07	n.a.
se	(0.06)	(0.07)	(0.17)	(0.08)	(0.09)	(0.05)	n.a.
lower bound	-0.15	-0.24	-0.27	-0.17	-0.19	-0.17	n.a.
upper bound	0.10	0.03	0.38	0.13	0.16	0.02	n.a.
Emp. (vs. unemp/nilf) elast.	-0.04	0.01	0.07	0.03	-0.15	0.12	n.a.
se	(0.20)	(0.13)	(0.46)	(0.12)	(0.49)	(0.12)	n.a.
lower bound	-0.43	-0.25	-0.83	-0.20	-1.12	-0.12	n.a.
upper bound	0.36	0.27	0.96	0.26	0.82	0.35	n.a.
Annual Hours elasticity	0.02	-0.06	0.21	0.02	0.15	0.12	n.a.
se	(0.24)	(0.20)	(0.64)	(0.16)	(0.39)	(0.16)	n.a.
lower bound	-0.45	-0.44	-1.05	-0.30	-0.62	-0.20	n.a.
upper bound	0.50	0.33	1.48	0.34	0.91	0.44	n.a.

Sources: March CPS 1962-1981.
Sample: For regressions on log annual earnings and on log annual number of hours worked per year regressions: adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. For regressions on employment (measured as probability of being employed vs. unemployed or vs. unemployed or not in the labor force): adults 25-55, black or white, either employed, unemployed or not in the labor force.
Notes: Table reports the coefficient on the interaction between the period 1967-72 and the 1966 Kaitz index. Effects on earnings, employment and hours measured as the effect of one standard deviation increase in the treatment variable. The mean is 0.35, the standard deviation is 0.048. 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. Controls for employment regressions are gender, race, years of schooling, age, age square and marital status. Controls for regressions on log annual hours are gender, race, years of schooling, a cubic in experience, occupation and marital status. Standard errors are clustered at the state level. Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling.

Table E5: Effect of the 1966 FLSA using share of workers below \$1.60 in 1966

	All	Black	White	Men	Women	Low-educ.	High-educ
Share wages below \$1.60 × 1967-1972							
Earnings	0.022***	0.064***	0.012***	0.023***	0.020***	0.039***	0.008
	(0.004)	(0.012)	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)
Emp. (vs. unemp.)	0.001	-0.006*	0.001	0.001	0.000	-0.000	0.002***
	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Emp. (vs. unemp/nilf)	0.002	-0.000	0.003	0.002	0.001	0.006*	0.001
	(0.002)	(0.004)	(0.002)	(0.001)	(0.003)	(0.003)	(0.002)
Annual Hours	-0.000	-0.011	0.001	-0.001	0.002	0.001	-0.002
	(0.002)	(0.007)	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)
Obs	534,885	51,658	483,227	336,047	198,838	145,507	389,378
Controls	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y	Y
Emp. (vs. unemp) elasticity	0.03	-0.09	0.09	0.04	0.00	-0.01	0.29
se	(0.05)	(0.05)	(0.08)	(0.05)	(0.06)	(0.04)	(0.23)
lower bound	-0.06	-0.20	-0.07	-0.06	-0.11	-0.09	-0.17
upper bound	0.12	0.01	0.24	0.14	0.12	0.06	0.74
Emp. (vs. unemp/nilf) elast.	0.11	-0.01	0.37	0.08	0.15	0.25	0.19
se	(0.13)	(0.09)	(0.26)	(0.06)	(0.35)	(0.13)	(0.36)
lower bound	-0.14	-0.18	-0.14	-0.04	-0.54	0.00	-0.52
upper bound	0.35	0.16	0.89	0.20	0.83	0.50	0.90
Annual Hours elasticity	-0.01	-0.17	0.05	-0.04	0.09	0.02	-0.24
se	(0.11)	(0.13)	(0.18)	(0.08)	(0.23)	(0.10)	(0.44)
lower bound	-0.22	-0.43	-0.30	-0.19	-0.36	-0.18	-1.10
upper bound	0.20	0.10	0.41	0.11	0.53	0.22	0.62

Sources: March CPS 1962-1981.

Sample: For regressions on log annual earnings and on log annual number of hours worked per year regressions: adults 25-55, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code. For regressions on employment (measured as probability of being employed vs. unemployed or vs. unemployed or not in the labor force): adults 25-55, black or white, either employed, unemployed or not in the labor force.

Notes: Table reports the coefficient on the interaction between the period 1967-72 and the share of workers with wages below \$1.60 in 1966. Effects on earnings, employment and hours measured as the effect of one standard deviation increase in the treatment variable. The mean is 0.17, the standard deviation is 0.008. 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. Controls for employment regressions are gender, race, years of schooling, age, quadratic and cubic in experience and marital status. Controls for regressions on log annual hours are gender, race, years of schooling, a cubic in experience, occupation and marital status. Standard errors are clustered at the state level. Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling.

Appendix F Comparison of CPS Employment Effects to Bailey et al. (2018)

In a contemporaneous paper, Bailey et al. (2018) study how the high nationwide minimum wage mandated by the 1966 FLSA affected earnings and employment, using the micro-files of the CPS, and exploiting state-level differences in the bite of a national minimum wage due to differences in standard of living. To measure the extent of the bite of the minimum wage, they exploit differences across states in the share of workers below the new minimum wage of 1968 ($1.60 in nominal terms).

Consistent with our employment results using both the CPS and the BLS data, they report both small positive and negative overall demand employment elasticities with respect to average wage across four different specifications, all indistinguishable from 0 (see Table 3 p.28 in their paper). Both the point estimates and the precision of their employment estimates are consistent with ours (see Figure 11). Among their four specifications, their preferred specification exhibits a small negative overall employment elasticity – statistically indistinguishable from 0 – whereas our preferred specification exhibits a 0 employment elasticity. Using this preferred specification, they reach similar conclusions across all subgroups, except for black men, for which they report a negative employment elasticity of -0.27 [-0.11; -0.43]. They note this result is potentially different from the main employment elasticity we report on all black persons in our paper, as we report an employment elasticity of -0.07 [-0.18;0.04] (see Table 8 and Appendix Table E3). We note, however, that in both papers, the employment elasticity among black persons is more imprecisely estimated than for other sub-groups due to small sample sizes of black workers in the CPS (and particularly so in the early files of the CPS), and that our confidence intervals for this point estimate overlap. This leads us to acknowledge that we cannot exclude that the difference in the point estimates is due to noise. Nevertheless, in this appendix, we attempt to review and discuss potential explanations for why the point estimates might differ in the two papers.

We identify four main differences in our empirical analyses. First, our samples of interest differ. Whereas Bailey et al. (2018) focus exclusively on men, aged 16-64, we analyze the employment effects across all men and women aged 25-55. In particular, we do not include teens who, below 21, are subject to a different minimum wage policy, and older workers (55-64 years old). These two subgroups might be more vulnerable and face larger negative demand employment elasticities. We do not include men of draft age (between 18 and 25 years old), as the inclusion of this subgroup might lead to negative biases in the overall employment results if enrollment in the Vietnam War is contemporaneous to the implementation of the minimum wage reform, and if enrollment rates are higher in states also strongly affected by the reform. Because these strongly affected states happen to be states where black workers were overrepresented and because black workers were slightly overrepresented among soldiers, the inclusion of this subgroup might lead to more severe negative biases among black men.

Second, we measure employment and wage outcomes differently. While Bailey et al. (2018) measure employment elasticities with respect to a reconstructed measure of average hourly wages in the CPS, we measure employment elasticities w.r.t average annual earnings, which are directly reported in the CPS. This difference does not seem to explain the difference in the employment elasticity estimates however, as our earnings effect using the cross state design seems to be slightly smaller than the hourly wage effect reported in Bailey et al. (2018). That means that all else equal, our employment elasticities should be larger (in absolute terms) than in Bailey et al. (2018). Moreover, we report our results on the probability of being employed vs. unemployed in the main text and conduct our analysis at the individual level in the CPS, whereas they report results on the employment-population ratio at the state-group level. Finally, Bailey et al. (2018) report pooled estimates for 1967-1973, i.e. taking into account the first year of the 1973-1975 recession whereas we report our results for the period 1967-1972, once the minimum wage introduced in the new sectors has fully converged to the federal minimum wage.

Third, the two papers use slightly different control variables. Notably, Bailey et al. (2018) include state-by-cohort fixed effects in their preferred specification (see column 3 of their table 3 p.28) – which they show turn their main employment elasticity from a small positive into a small negative one. Although we do not include this precise control, we include a quadratic in age at the individual level in all of our employment regressions.

A fourth difference consists of a difference in the empirical approach, and most importantly, in the measure of the variation in the bite of the minimum wage reform across states. We compare employment outcomes in states that are strongly treated vs. weakly treated based on whether states had their own minimum wage law or not as of January 1966. This design attempts to capture differences in the extension of minimum wage coverage to new sectors of the economy – the reform we analyze in this paper. By contrast, Bailey et al. (2018) exploit differences across states in the share of workers below the new minimum wage of 1968 ($1.60 in nominal terms), attempting to capture the effect of both the extension in coverage across sectors and nationwide minimum wage increases for all sectors in the economy in 1967 and 1968. The two designs may be getting at different LATEs. It may be the case that for the subset of African American compliers using the share of workers with wages below \$1.60 treatment, there is a slightly more negative effect than for African American compliers in the state-pre-existing law definition of treatment.

We implement their design on a sample as close as possible to the one they describe to see if we find the same pattern of results as in their paper. We show that when using this approach, the lower bound of the employment elasticity for black men is -0.27—closer to Bailey et al. (2018)—but still indistinguishable from 0. Specifically, we run the following regression at the state level, using the fraction of workers with wages below $\$1.60$ in 1966 in each state:[95]

$\forall$ period $k \in$ [1961-1966], [1967-1972] & [1973-1980],

$$\log y_{st} = \alpha + \sum_k \beta_k \text{Share wages below \$1.60}_s \times \delta_{t+k} + \mathbb{X}'_{st}\Gamma + \nu_s + \delta_k + \varepsilon_{st} \quad (13)$$

Note that the employment effect is measured here relative to the period 1961-1966, whereas Bailey et al. (2018) measure the employment effect relative to the year 1966 only.

We report the results of this estimation for the overall sample and by subgroups in Table F1 below. There are positive effects on average earnings overall (whether we include age, share of men and share of white persons as controls (column 2) or not (column 1)) and no statistically significant effect on employment (whether on the extensive margin as measured using both the employment population ratio and $1-$ unemployment rate, or on the intensive margin using annual hours). This pattern is generally true across all subgroups. Importantly, we are able to rule out employment elasticities (based on the employment population ratio measure) greater than -0.27 among black men. This is closer to the lower bound in Bailey et al. (2018), but the estimate is still statistically indistinguishable from 0 – as is the effect on annual number of hours worked per year among black men.

Overall, the implementation of this alternative design confirms our findings. We note, however, that the effects are more imprecisely estimated using this design aggregated at the state-group level. In particular, we are not able to replicate the large employment elasticities for black men reported by Bailey et al. (2018) using their exact treatment variable on our sample. Because our individual-level design (described in Section 5.2) does not lead to large negative employment elasticities among young (16-30 years old) and older groups (50-64 years old) (see Appendix Figure E3b), or among women (see Appendix Figure E3a), and in the absence of the do-files the authors used, we conclude that the difference in the employment

[95] As reported in their Table 1 p.26

elasticity among black men in the two papers is likely due to other sample differences. We cannot exclude that it is due to noise.

Finally, we note that large disemployment effects among black workers should have engendered large compositional shifts in employment and, in particular, sharp declines in the employment share of black workers in the years following the reform. However, we do not find evidence of this in our analysis. As shown in Appendix Figure B3b, the employment share of black vs. white workers is stable in the late 1960s and early 1970s in both the industries covered in 1938 and covered in 1967. Additionally, as noted in our bunching analysis that uses BLS data (see Section 5.1), we do not find that employment elasticities are higher in the industry-by-region groups where the share of black workers is higher than average (see Table 7).

Table F1: Effect of 1966 FLSA on wages and employment, replication of Bailey et al. (2018)

	Men (1)	Men (2)	Black men	White men	Low-educ. men	High-educ. men
Share wages below \$1.60 × 1967-1972						
Earnings	0.441**	0.352**	0.554***	0.288*	0.651**	0.132
	(0.178)	(0.150)	(0.128)	(0.150)	(0.259)	(0.113)
Employment (epop)	0.049*	0.044	0.033	0.063***	0.033	0.059**
	(0.025)	(0.026)	(0.081)	(0.021)	(0.050)	(0.022)
Annual Hours	0.015	0.004	-0.110	0.024	0.026	-0.040
	(0.038)	(0.029)	(0.064)	(0.026)	(0.038)	(0.025)
State-by-year obs	399	399	393	399	399	399
Has controls	N	Y	Y	Y	Y	Y
Has Time FE	Y	Y	Y	Y	Y	Y
Has State FE	Y	Y	Y	Y	Y	Y
Emp. elasticity (epop)	0.14	0.15	0.08	0.26	0.07	0.51
se	(0.06)	(0.08)	(0.18)	(0.13)	(0.10)	(0.35)
lower bound	0.01	-0.00	-0.27	-0.00	-0.13	-0.17
upper bound	0.26	0.30	0.43	0.53	0.27	1.19
Annual Hours elasticity	0.03	0.01	-0.20	0.08	0.04	-0.30
se	(0.08)	(0.08)	(0.12)	(0.10)	(0.06)	(0.32)
lower bound	-0.12	-0.15	-0.43	-0.10	-0.07	-0.93
upper bound	0.19	0.17	0.03	0.27	0.15	0.32

Sources: March CPS 1962-1981.

Sample: For regressions on log earnings and log annual number of hours worked per year regressions: adults 16-64, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed or unpaid family worker, not in group quarters, has non-missing industry or occupation code. For regressions on employment (employment population ratio, calculated at the state level): adults 16-64, black or white, either employed, unemployed or not in the labor force.

Notes: Table reports the coefficient on the interaction between the period 1967-72 and the share of workers with wages below \$1.60 in 1966, as calculated and reported in Table 1 p.26 in Bailey et al. (2018), using the indicated outcome variables and the cross-state design described above. Controls are average age by state, share of white persons, share of married persons and share of low-education persons in each state. Annual average earnings are in \$2017, deflated using annual CPI-U-RS series. The year 1962 is excluded. Standard errors are clustered at the state level.

Appendix G Economy-Wide Racial Gap

G.1 Contribution of the 1967 Reform to the Understanding the Timing and Magnitude of the Decline in Racial Inequality

Our study contributes to a better understanding of the exact timing of the reduction in racial inequality during the Civil Rights Era, which has proved a challenging puzzle for the literature thus far. Figure G1a plots the evolution of the unadjusted racial earnings gap since the early 1960s for the 1938 and 1967 industries combined and shows that much of the decline in the economy-wide racial gap happened in just two yeas: 1967 and 1968. As noted in Section 2, anti-discrimination policies and improvements in education for the black population cannot explain the specific timing of the reduction in the racial earnings gap. Instead, the extension of the minimum wage to new sectors of the economy where black workers were overrepresented is consistent with this specific timing. As shown on Figure G1b, the unadjusted racial earnings gap fell sharply in the newly covered industries relative to the previously covered ones precisely in 1967.

G.2 Derivation of the Decomposition of the Economy-Wide Racial Gap

We define the economy-wide racial earnings gap as the mean log wage difference between white and black workers in the industries covered in 1938 and in 1967 combined. We denote this economy-wide racial earnings gap by G^{total}. It is defined as:

$$\begin{aligned} G^{\text{total}} &= \frac{1}{N_w}\sum_i log(\omega_i^w) - \frac{1}{N_b}\sum_i log(\omega_i^b) \\ &= \bar{X_w} - \bar{X_b} \end{aligned} \tag{14}$$

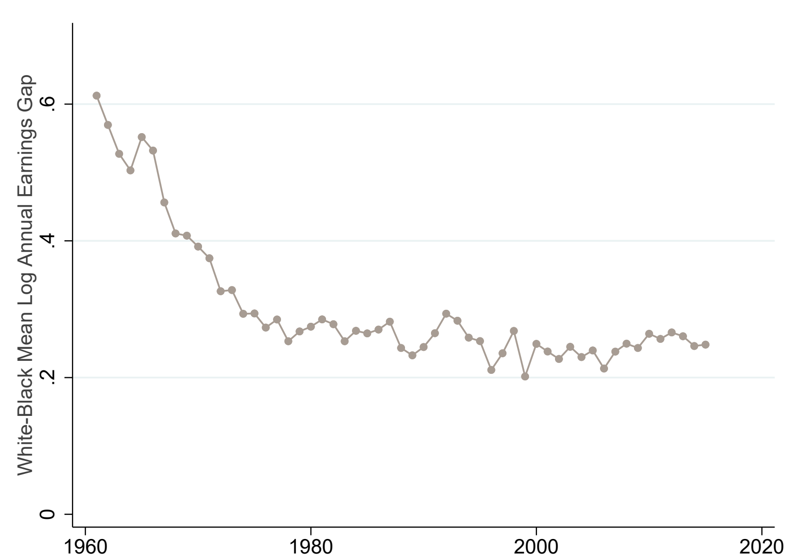
with $log(\omega_i^w)$ (respectively, $log(\omega_i^b)$) as the log of wages of white (black) workers ; N_w (N_b) as the number of white vs. black workers. We denote $\bar{X_w}$ ($\bar{X_b}$) as the average log wages of white (black) workers.

By noting that overall average log wages can be decomposed into a treatment and a control group component, we write:

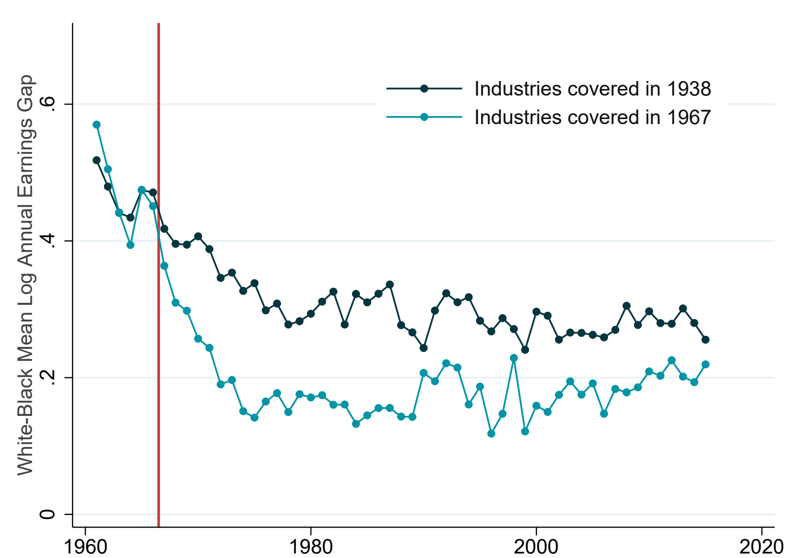
$$\begin{aligned} \bar{X_w} &= \frac{1}{N_w}\sum_i log(\omega_i^w) \\ &= \frac{N_w^c}{N_w}\cdot\frac{1}{N_w^c}\sum_{i,w} log(\omega_i^c) + \frac{N_w^t}{N_w}\cdot\frac{1}{N_w^t}\sum_{i,w} log(\omega_i^t) \\ &= s_w^c\cdot\frac{1}{N_w^c}\sum_{i,w} log(\omega_i^c) + s_w^t\cdot\frac{1}{N_w^t}\sum_{i,w} log(\omega_i^t) \end{aligned} \tag{15}$$

Figure G1: White-black unadjusted wage gap in the long-run

(a) Economy-wide



(b) By type of industry



Source: Annual Social and Economic Supplement of the Current Population Survey, 1962-2016.
Sample: Adults 25-65, black or white, worked more than 13 weeks last year and 3 hours last week, not self-employed, not in group quarters, not unpaid family worker, no missing industry or occupation code.
Notes: 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 calculated using 1962 CPS in 1961. The economy-wide racial gap is defined here as the combination between the industries covered in 1938 and the industries covered in 1967. Annual earnings in $2017, deflated using annual CPI-U-RS series.

With s^c_w (s^c_b) the share of white (black) workers working in the control group, s^t_w (s^t_b) the share of white (black) workers working in the treatment group. Note that: $s^c_w + s^t_w = 1$. Similarly, $s^c_b + s^t_b = 1$. It follows that:

$$\begin{aligned} G^{\text{total}} &= s^c_w \bar{X}^c_w + s^t_w \bar{X}^t_w - s^c_b \bar{X}^c_b - s^t_b \bar{X}^t_b \\ &= (s^c_w \bar{X}^c_w - s^c_b \bar{X}^c_b) + (s^t_w \bar{X}^t_w - s^t_b \bar{X}^t_b) \\ &= (s^c_w \bar{X}^c_w - s^c_w \bar{X}^c_b) + (s^t_w \bar{X}^t_w - s^t_w \bar{X}^t_b) + s^c_w \bar{X}^c_b - s^c_b \bar{X}^c_b + s^t_w \bar{X}^t_b - s^t_b \bar{X}^t_b \\ &= s^c_w G_c + s^t_w G_t + \underbrace{\bar{X}^c_b (s^c_w - s^c_b) + \bar{X}^t_b (s^t_w - s^t_b)}_{=\lambda} \end{aligned} \tag{16}$$

$$\begin{aligned} \lambda &= s^c_w G_c + s^t_w G_t + \bar{X}^c_b (s^c_w - s^c_b) + \bar{X}^t_b (s^t_w - s^t_b) \\ &= s^c_w \bar{X}^c_b - s^c_b \bar{X}^c_b + s^t_w \bar{X}^t_b - s^t_b \bar{X}^t_b \\ &= s^c_w \bar{X}^c_b - s^c_w \bar{X}^t_b + s^c_w \bar{X}^t_b - s^c_b \bar{X}^c_b + s^t_w \bar{X}_b t - s^t_b \bar{X}^t_b - (s^c_b \bar{X}^c_b - s^c_b \bar{X}^t_b + s^c_b \bar{X}^t_b - s^t_w \bar{X}^t_b) \\ &= s^c_w G^{ct}_b + s^c_w \bar{X}^t_b - s^t_b \bar{X}^t_b - (s^c_b G^{ct}_b + s^c_b \bar{X}^t_b - s^t_w \bar{X}^t_b) \\ &= s^c_w G^{ct}_b - s^c_b G^{ct}_b + \bar{X}^t_b \times (\underbrace{s^c_w + s^t_w}_{=1} - (\underbrace{s^c_b + s^t_b}_{=1})) \\ &= s^c_w G^{ct}_b - s^c_b G^{ct}_b \end{aligned} \tag{17}$$

Therefore:

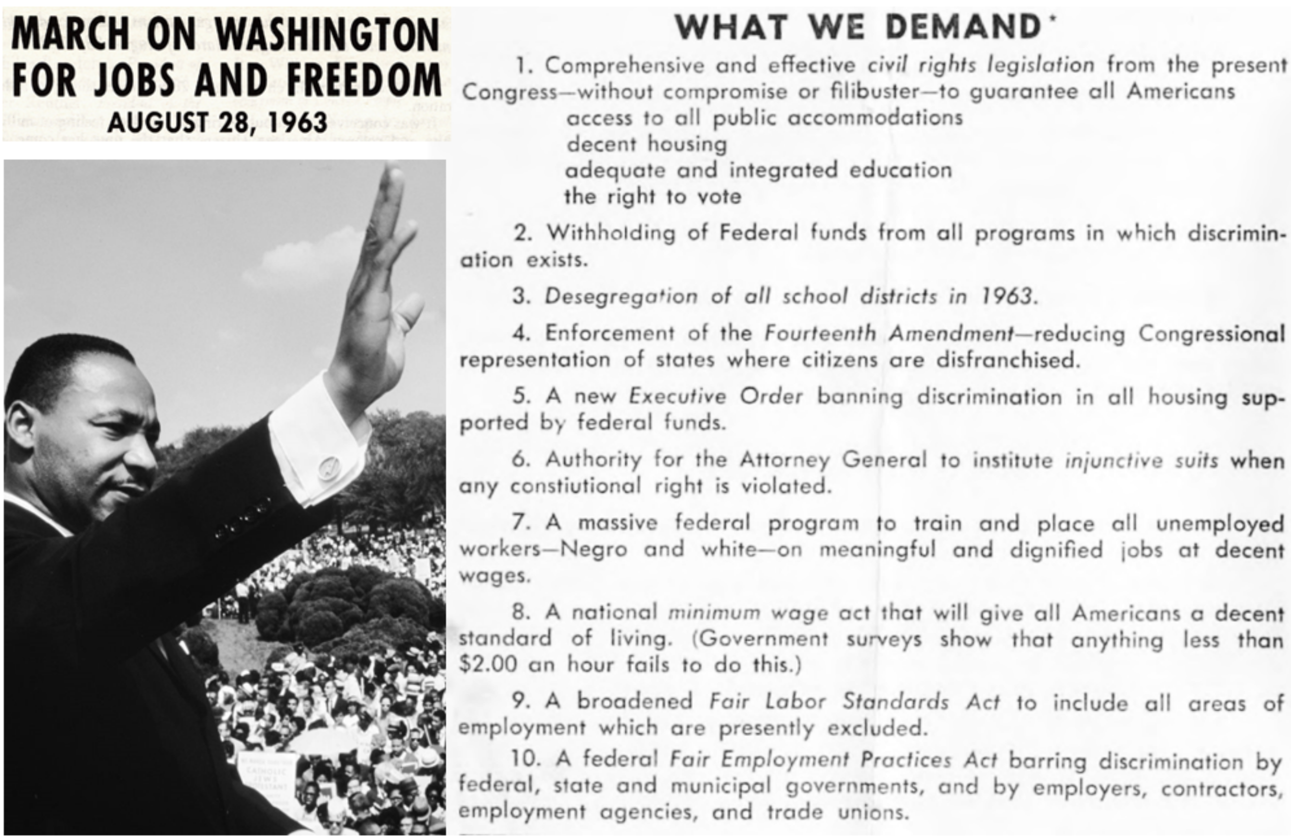
$$G^{\text{total}} = s^c_w G_c + s^t_w G_t + G^{ct}_b (s^c_w - s^c_b) \tag{18}$$

This is the formula we use in Section 6.1.

Appendix H The March on Washington for Jobs and Freedom

The 9th demand of the 1963 March on Washington for Jobs and Freedom read: "[We demand] a broadened Fair Labor Standards Act to include all areas of employment which are presently excluded," see Figure H1 and Section 3.1.

Figure H1: The 10 demands of the March on Washington for Jobs and Freedom, August 1963



MARCH ON WASHINGTON FOR JOBS AND FREEDOM
AUGUST 28, 1963

WHAT WE DEMAND*

1. Comprehensive and effective *civil rights legislation* from the present Congress—without compromise or filibuster—to guarantee all Americans
   - access to all public accommodations
   - decent housing
   - adequate and integrated education
   - the right to vote

2. Withholding of Federal funds from all programs in which discrimination exists.

3. *Desegregation of all school districts in 1963.*

4. Enforcement of the *Fourteenth Amendment*—reducing Congressional representation of states where citizens are disfranchised.

5. A new *Executive Order* banning discrimination in all housing supported by federal funds.

6. Authority for the Attorney General to institute *injunctive suits* when any constiutional right is violated.

7. A massive federal program to train and place all unemployed workers—Negro and white—on meaningful and dignified jobs at decent wages.

8. A national *minimum wage* act that will give all Americans a decent standard of living. (Government surveys show that anything less than $2.00 an hour fails to do this.)

9. A broadened *Fair Labor Standards Act* to include all areas of employment which are presently excluded.

10. A federal *Fair Employment Practices Act* barring discrimination by federal, state and municipal governments, and by employers, contractors, employment agencies, and trade unions.

Source: National Center for Civil and Human Rights in Atlanta, Georgia.

Appendix I Replication files

All the data, programs, and tex files used in this paper are available at:
clairemontialoux.com/flsa.

In what follows, we list all the figures and tables displayed in this paper and the appendix, as well as the name of the program that generated them.

Number	Title	file	do file
Main Figures and Tables			
Figure 1	Economy-wide white-black unadjusted wage gap in the long-run, in the CPS and in the decennial Censuses	unadj_rg_all_1949_2017.pdf	2a_cps_census_descriptives
Figure 2	Expansions in minimum wage coverage, and real values of the minimum wage 1938-2017 (\$2017)	reform_1986.pdf	spd_mwdescriptives.xls
Figure 3a	Share of workers covered by the minimum wage, by industry	share_workers_covered.png	2a_cps_census_descriptives
Figure 3b	Share of workers covered by the minimum wage, by fraction black, in 1967	share_workers_covered_by_race.png	2a_cps_census_descriptives
Figure 4	Minimum wage to median ratio	mw_to_median_ratio_DC_federal.pdf	2a_cps_census_descriptives
Figure 5	Impact of the 1967 reform on annual earnings	aw_industry_design.pdf	3a_cps_wage
Figure 6a	Heterogeneity in the wage effect of the 1967 reform, by level of education	aw_lshs.pdf	3a_cps_wage
Figure 6b	Heterogeneity in the wage effect of the 1967 reform, by race	aw_black_white.pdf	3a_cps_wage
Figure 7	States with no minimum wage laws as of January 1966	map_strongly_weakly_treated_states.png	2a_cps_census_descriptives
Figure 8a	Case study: laundries earnings distribution in the South, observed distributions from 1963 to 1968	laundries_s_all_mf.png	2b_bls_descriptives
Figure 8b	Case study: laundries earnings distribution in the South, distributions used in the bunching estimation	laundries_cf_actual_s_1.png	2b_bls_descriptives
Figure 9	Missing and excess jobs in the BLS Industry Wage Reports	missing_excess_mass_all_115_1.png	4a_bls_employment
Figure 10a	Impact of the 1966 FLSA on employment, intensive margin	ahours.pdf	3b_cps_employment
Figure 10b	Impact of the 1966 FLSA on employment, extensive margin	emp_all.pdf	3b_cps_employment
Figure 11	Employment elasticities wrt wage in the literature and in this paper	emp_elasticities.pdf	3b_cps_employment
Figure 12	1967 reform reduced overall racial gap by 20%	figure12a.png	3b_cps_employment
Figure 13a	Adjusted racial wage gaps, wage effects in levels by race and treatment status	aw_levels_black_white_tc.pdf	3c_cps_racial_gaps
Figure 13b	Adjusted racial wage gaps, by treatment status	adj_rg_tc_1961_1980.pdf	3c_cps_racial_gaps
Table 1	Employment and earnings by race, 1967	table_emp_earnings_by_race.tex	2a_cps_census_descriptives
Table 2	Workers characteristics, 1965-66	table_sum_stats.tex	2a_cps_census_descriptives
Table 3	Wage effect: Main results and robustness checks	table_aw_industry_design.tex	3a_cps_wage
Table 4	Predicted wage effect	table_aw_predictions_demog.tex	3a_cps_wage
Table 5	Hourly wage effect using BLS data	table_hw_bls_2models.tex	4a_bls_wage
Table 6	Wage effect by race	table_aw_black_white_wwosyfe.tex	3a_cps_wage
Table 7	Employment elasticities by industry and region using baseline bunching methodology	tab_bunching_mef.tex	4a_bls_employment
Table 8	Main effects of 1967 reform on probability of employment (extensive margin) and robustness checks	table_emp_cps_mef.tex	3b_cps_employment
Appendix tables and figures			
Figure B1	Analysis sample, before the reform (1966)	figure_sample.png	2a_cps_census_descriptives
Figure B2	State groups used in March CPS (1962-1980)	map_state_groups_cps.pdf	2a_cps_census_descriptives
Figure B3a	Evolution of black and white employment in treated and control industries, emp. Shares in control vs. treated industries	emp_share_tc.pdf	2a_cps_census_descriptives
Figure B3b	Evolution of black and white employment in treated and control industries, black vs. white emp. Shares within 1938, 1967 ind.	emp_black_share_tc.pdf	2a_cps_census_descriptives
Figure B4a	Aggregate employment shares, by industry type and by race	agg_emp_shares_by_ind_by_race.ong	2a_cps_census_descriptives
Figure B4b	Aggregate employment shares, all industries by race	agg_emp_shares_all_ind_by_race.ong	2a_cps_census_descriptives
Figure B4c	Aggregate employment shares, 1938 industries by race	agg_emp_shares_1938_ind_by_race.ong	2a_cps_census_descriptives
Figure B4d	Aggregate employment shares, 1967 industries by race	agg_emp_shares_1967_ind_by_race.ong	2a_cps_census_descriptives
Figure B5a	Employment status in industries covered in 1938 and 1967, black and white persons	emp_status_all_ind_all_race1964.png	2a_cps_census_descriptives
Figure B5b	Employment status in industries covered in 1938 and 1967, black persons	emp_status_all_ind_black_1964.png	2a_cps_census_descriptives
Figure B5c	Employment status in industries covered in 1938 and 1967, black male persons	emp_status_all_ind_black_male1964.png	2a_cps_census_descriptives
Figure B5d	Employment status in industries covered in 1938 and 1967, white male persons	emp_status_all_ind_white_male1964.png	2a_cps_census_descriptives
Figure C1	Wage effect of the 1966 FLSA with different sets of controls	aw_industry_design_with_and_without_controls_wohours.pdf	3a_cps_wage
Figure C2	Impact of the 1966 FLSA on annual earnings by race	aw_black_white_nonsep.pdf	3a_cps_wage
Figure C3a	Heterogeneity of the wage effect by level of education, among black workers	aw_lshs_black.pdf	3a_cps_wage
Figure C3b	Heterogeneity of the wage effect by level of education, among white workers	aw_lshs_white.pdf	3a_cps_wage
Figure C4a	Adjusted racial wage gaps, by level of education, White-Black earnings gap (adjusted) in treated industries	adj_rg_t_skill_highs_1961_1980.pdf	3c_cps_racial_gaps
Figure C4b	Adjusted racial wage gaps, by level of education, White-Black earnings gap (adjusted) in control industries	adj_rg_c_skill_highs_1961_1980.pdf	3c_cps_racial_gaps
Figure D1a	BLS industry wage reports, Set of industries we digitized and years available	bls_digitization.png	figures>bls_digitization.png
Figure D1b	BLS Industry Wage Reports, What they look like	laundries_data.png	figures>laundries_data.png
Figure D2a	Earnings distributions in laundries, South	laundries_s_all_mf.png	2b_bls_descriptives
Figure D2b	Earnings distributions in laundries, Midwest	laundries_nc_all_mf.png	2b_bls_descriptives
Figure D2c	Earnings distributions in laundries, Northeast	laundries_ne_all_mf.png	2b_bls_descriptives
Figure D2d	Earnings distributions in laundries, West	laundries_w_all_mf.png	2b_bls_descriptives
Figure D3a	Earnings distributions in laundries, inside plant workers, South	laundries_s_ipt_mf.png	2b_bls_descriptives
Figure D3b	Earnings distributions in laundries, inside plant workers, Midwest	laundries_nc_ipt_mf.png	2b_bls_descriptives
Figure D3c	Earnings distributions in laundries, inside plant workers, Northeast	laundries_ne_ipt_mf.png	2b_bls_descriptives
Figure D3d	Earnings distributions in laundries, inside plant workers, West	laundries_w_ipt_mf.png	2b_bls_descriptives

Figure D4a	Earnings distributions in hotels (tipped workers), South	hotels_s_td_mf.png	2b_bls_descriptives
Figure D4b	Earnings distributions in hotels (tipped workers), Midwest	hotels_nc_td_mf.png	2b_bls_descriptives
Figure D4c	Earnings distributions in hotels (tipped workers), Northeast	hotels_ne_td_mf.png	2b_bls_descriptives
Figure D4d	Earnings distributions in hotels (tipped workers), West	hotels_w_td_mf.png	2b_bls_descriptives
Figure D5a	Earnings distributions in hotels (non-tipped workers), South	hotels_s_ntd_mf.png	2b_bls_descriptives
Figure D5b	Earnings distributions in hotels (non-tipped workers), Midwest	hotels_nc_ntd_mf.png	2b_bls_descriptives
Figure D5c	Earnings distributions in hotels (non-tipped workers), Northeast	hotels_ne_ntd_mf.png	2b_bls_descriptives
Figure D5d	Earnings distributions in hotels (non-tipped workers), West	hotels_w_ntd_mf.png	2b_bls_descriptives
Figure D6a	Earnings distributions in restaurants (tipped workers), South	rest_s_td_mf.png	2b_bls_descriptives
Figure D6b	Earnings distributions in restaurants (tipped workers), Midwest	rest_nc_td_mf.png	2b_bls_descriptives
Figure D6c	Earnings distributions in restaurants (tipped workers), Northeast	rest_ne_td_mf.png	2b_bls_descriptives
Figure D6d	Earnings distributions in restaurants (tipped workers), West	rest_w_td_mf.png	2b_bls_descriptives
Figure D7a	Earnings distributions in restaurants (non-tipped workers), South	rest_s_ntd_mf.png	2b_bls_descriptives
Figure D7b	Earnings distributions in restaurants (non-tipped workers), Midwest	rest_nc_ntd_mf.png	2b_bls_descriptives
Figure D7c	Earnings distributions in restaurants (non-tipped workers), Northeast	rest_ne_ntd_mf.png	2b_bls_descriptives
Figure D7d	Earnings distributions in restaurants (non-tipped workers), West	rest_w_ntd_mf.png	2b_bls_descriptives
Figure D8a	Earnings distributions in nursing homes, South	nursing_s_all_mf.png	2b_bls_descriptives
Figure D8b	Earnings distributions in nursing homes, Midwest	nursing_nc_all_mf.png	2b_bls_descriptives
Figure D8c	Earnings distributions in nursing homes, Northeast	nursing_ne_all_mf.png	2b_bls_descriptives
Figure D8d	Earnings distributions in nursing homes, West	nursing_w_all_mf.png	2b_bls_descriptives
Figure D9a	Earnings distributions in schools, South	schools_s_all_mf.png	2b_bls_descriptives
Figure D9b	Earnings distributions in schools, Midwest	schools_nc_all_mf.png	2b_bls_descriptives
Figure D9c	Earnings distributions in schools, Northeast	schools_ne_all_mf.png	2b_bls_descriptives
Figure D9d	Earnings distributions in schools, West	schools_w_all_mf.png	2b_bls_descriptives
Figure D10a	Earnings distributions in hospitals, South	hospitals_s_all_mf.png	2b_bls_descriptives
Figure D10b	Earnings distributions in hospitals, Midwest	hospitals_nc_all_mf.png	2b_bls_descriptives
Figure D10c	Earnings distributions in hospitals, Northeast	hospitals_ne_all_mf.png	2b_bls_descriptives
Figure D10d	Earnings distributions in hospitals, West	hospitals_w_all_mf.png	2b_bls_descriptives
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