

Essays on the redistributive effects of the minimum wage

Claire Montialoux

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et Michael Reich, constitutent le troisième article de cette thèse. Cet article fait partie d'un ensemble plus vaste d'évaluations des effets économiques d'un salaire minimum à $15 au niveau de certains États (New York et Californie), villes (San Jose) et régions (Santa Clara county) que nous avons menées collectivement.

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Résumé substantiel

Cette thèse analyse les effets redistributifs du salaire minimum et se compose de trois chapitres.

Le premier chapitre étudie le rôle du salaire minimum dans la dynamique des inégalités raciales aux États-Unis. Les différences de salaire entre Noir-Américains et Blancs ont été divisées par deux aux États-Unis entre la fin des années 1960 et la fin des années 1970. Depuis le milieu des années 1970, ces inégalités sur le marché du travail ont stagné à un niveau élevé. La loi relative au salaire minimum de 1966 (Fair Labor Standards Act) a introduit le salaire minimum dans l'agriculture, l'hôtellerie, la restauration, les maisons de retraite, les écoles et les hôpitaux – des secteurs qui n'étaient pas couverts par le salaire minimum précédemment et dans lesquels plus d'un tiers des travailleurs Noir-Américains étaient employés. Nous avons numérisé plus de 1000 distributions de salaire horaire issues des rapports détaillés sur les conditions de travail dans plusieurs secteurs publiés par le Bureau des statistiques du marché du travail américain dans les années 1960 ; nous avons également utilisé les données individuelles issues de l'enquête emploi américaine pour analyser les effets de la réforme de 1966 sur les salaires, l'emploi, et les inégalités raciales. Nous utilisons une stratégie de différence de différences qui compare l'évolution des salaires dans les secteurs couverts par le salaire minimum depuis 1938 (groupe de contrôle) à l'évolution des salaires dans les secteurs nouvellement couverts en 1966 (groupe de traitement) avant et après 1967 (année de l'introduction du salaire minimum dans de nouveaux secteurs) pour montrer que les salaires ont augmenté très nettement dès 1967 dans les nouveaux secteurs couverts par le salaire minimum. L'augmentation des salaires a été deux fois plus importante pour les travailleurs Noir-Américains que pour les Blancs. Au sein même des industries nouvellement couvertes, les différences de salaires entre Blancs et Noir-Américains – ajustées par toutes les caractéristiques observables (genre, années d'expérience, niveau d'éducation, etc.) baisse de 25 points de log avant réforme à près de 0 après réforme. En utilisant un estimateur de bunching, nous ne trouvons pas d'effet de la réforme sur l'emploi. Nous pouvons écarter des élasticités de l'emploi par rapport au salaire moyen plus

fortes que -0.1. La réforme de 1966 peut expliquer plus de 20% de la réduction des inégalités de salaires et de revenu entre les Noir-Américains et les Blancs pendant le mouvement des droits civiques aux États-Unis – soit autant que la déségrégation scolaire. Nos résultats mettent en lumière le rôle des institutions du marché du travail dans le déclin des inégalités entre Blancs et Noir-Américains.

Le deuxième chapitre de cette thèse estime la transmission des augmentations de salaire minimum dans les prix des produits des supermarchés. Nous utilisons des données de caisse et exploitons un grand nombre d'augmentations de salaire minimum au niveau des États américains survenues entre 2001 et 2012. Nous trouvons qu'une augmentation de 10% du salaire minimum se traduit par une augmentation moyenne des prix de l'ordre de 0.2% dans les supermarchés. L'ampleur de cette augmentation est cohérente avec une répercussion complète des augmentations du coût du travail dans les prix à la consommation. Nous montrons que les ajustements de prix ont lieu majoritairement dans les trois mois qui suivent le vote d'une augmentation de salaire minimum. Les prix des produits consommés par les ménages à bas revenus augmentent autant que ceux consommés par les ménages à hauts revenus. Les augmentations de prix dans les supermarchés diminuent de 3 à 12% les gains de revenus nominaux liés à l'augmentation du salaire minimum, en fonction de la catégorie de revenus du ménage. Nos résultats suggèrent que ce sont principalement les consommateurs – et non pas les entreprises – qui payent le prix de l'augmentation du salaire minimum.

Le troisième chapitre de cette thèse est une calibration d'un modèle du marché du travail qui permet de simuler les effets d'une augmentation du salaire minimum au niveau fédéral à $15 d'ici 2024 aux États-Unis. Nous prenons en compte comment les travailleurs, les entreprises et les consommateurs sont affectés par les augmentations de salaire minimum et comment ils y répondent dans un modèle intégré. Il s'agit de comparer les niveaux d'emploi obtenus si la réforme est adoptée aux niveaux d'emploi obtenu si la réforme n'est pas adoptée. Nous analysons en particulier les effets d'un salaire minimum à $15 au Mississippi, un des États les plus pauvres des États-Unis. Nous trouvons qu'une augmentation

de salaire minimum à $15 au niveau fédéral d'ici 2024 génèrerait une augmentation significative des conditions de vie de 41.5 millions de travailleurs et leurs familles, alors que l'ampleur des destructions d'emplois serait limitée et que les augmentations de prix seraient absorbées par l'ensemble des consommateurs. Les effets obtenus au Mississippi seraient similaires. Une augmentation progressive du salaire minimum à $15 serait absorbée en partie par des réductions de turnover des employés, des augmentations de productivité, et majoritairement par des augmentations de prix (en particulier dans la restauration) et une augmentation des dépenses de consommation de la part des travailleurs à bas salaires.

Chapter 1

Minimum Wages and Racial Inequality

Abstract: The earnings difference between black and white 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 1966 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.

1 Introduction

One of the most striking dimensions of inequality in America is the persistence of large racial economic disparities (Bayer and Charles, 2018; Chetty et al., 2018).[1] 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 African American and white workers today.[2] 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 black-white 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). 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 the 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

[1] This chapter was written with Ellora Derenoncourt.

[2] The racial earnings gap is measured here as the mean log annual earnings difference between black and white workers (i.e., conditional on working) using the 2016 Annual Social and Economic Supplement of the Current Population Survey.

low-paid workers, and that the effects were more than twice as large for black workers compared to 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). Our paper provides the first causal 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 purpose 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 on 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 1938 industries (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 to the late 1970s. The magnitude of the wage increase is consistent with the predicted mechanical effect of the minimum wage hike estimated using the pre-reform CPS. We obtain an identical differential 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, 2017). Within treated industries, we compare the 1966-1967 evolution of the mass of workers employed at or just above the minimum wage (who were affected by the reform) to the evolution of the mass of workers employed higher up in the distribution (who were not affected). The large number of workers bunching at the newly introduced minimum wage in

1967 suggests that the minimum wage did not significantly reduce employment among low-wage workers, despite the sharp increase in wages. If anything, the reform appears to have had slight positive employment effects. Employment expanded in the newly covered industries (slightly faster than in the control industries), and employment at the bottom of the distribution expanded slightly faster than employment at the top. Our finding of small (possibly positive) employment responses is robust to considering alternative assumptions on the extent of the spillover effects of the minimum wage and the counterfactual trends in employment growth.

We confirm our core results of large wage effects and small employment effects in a different 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 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 reduces 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.

Since 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 legislations when governments are concerned about forms of inequality that cannot be addressed directly through income-based tax and transfer policies. Our goal, in the years ahead, is to extend our analysis to other countries and time periods to better understand the conditions under which the minimum wage can be effec-

tive at reducing discrimination and inequality on the labor market (such as across gender or across U.S.-born vs. immigrant workers).

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.

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.

A Literature on Racial Inequality and the Civil Rights Movement

A large body of work seeks to understand what caused the decline in the racial earnings gap during the Civil Rights Era, a period that saw major policy and economic changes. Two 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.[3] It was enforced by the Equal Employment Opportunity Commission

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

(EEOC) created in 1965.[4] 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).[5] 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, 2018) 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.

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.[6] 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.[7]

[4]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.

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

[6]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).

[7]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.

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); 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.[8] 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 northern migration of African Americans, have been found to play a minor role.[9] 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 inequality. This factor turns out to be quantitatively important, comparable in size to the impact of improvements in school quality found by Card and Krueger (1992) and in school quantity found by Smith and Welch (1986). Our

[8] 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.

[9] 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.

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. Figure 1.1a 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, a lot of the decline happened in just one year: 1967. 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).[10] Similarly, there is no sudden change in schooling quantity or quality for blacks in 1967; educational improvements occurred gradually. Income transfers also rose progressively throughout the 1960s and 1970s.[11] By contrast, the 1967 extension of the minimum wage can explain why a lot of the decline in the racial earnings gap took place in 1967. Figure 1.1b shows indeed that the unadjusted racial earnings gap fell sharply in the newly covered industries relative to the previously covered ones precisely in 1967.

B Minimum Wage Literature

A huge 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

[10]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.

[11]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/

policy can affect racial economic disparities. A large body of work discusses the efficiency costs of the 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., 2018). The literature also studies the 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). But the interplay between the minimum wage and racial inequality has not been investigated in a causal research design thus far.

Second, our paper provides evidence on the economic effects of very large minimum wage increases. The 1967 reform is a large shock to the treated industries in states that did not have a state minimum wage, because for them the wage floor moves 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 $1.4 in 1967 and $1.60 from 1968 on (the equivalent of $9.91 in 2017 dollars, i.e., its historical peak). In ongoing work, 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 standard of living. Their results show little evidence of disemployment effects for men, consistent with our results. Since 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, 2017; 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.[12] 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, 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 (in contrast to the bulk of the literature that focuses on cross-state variation).

3 The 1967 Extension of the Minimum Wage and Data

A 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 1.3) in the manufacturing, transportation and communication, wholesale trade, finance and real estate sectors (see the complete list of covered sectors in figure 1.2). 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

[12] 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).

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, 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.[13] The 1966 FLSA amendments introduced the federal minimum wage (as of February 1st, 1967) in the following sectors: agriculture, nursing homes, laundries, hotels, restaurants, public schools, and hospitals. These sectors employed about 8 million workers (see figure 1.3) 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. Conscious of this, President Johnson 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 (see figure 1.5). The minimum wage introduced in these sectors in 1967 ($1) was initially below the federal minimum wage, but converged to the level of the federal minimum

[13]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.

wage by 1971, except in agriculture where convergence was only complete in 1977. [14] 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.

B Data Used in our Analysis

We use four data sources to study the 1967 extension of the minimum wage: industry wage reports published by the Bureau of Labor Statistics that we digitized; Current Population Survey micro-files going back to 1962; U.S. decennial census data; and data on state minimum wage legislation by industry and gender.

Bureau of Labor Statistics industry wage reports. The BLS conducted regular establishment surveys in the 1960s, 1970s, and 1980s to monitor the implementation of the amendments to the Fair Labor Standards Act of 1938. The surveys were requested by the Department of Labor's wage and public contracts divisions. The BLS focused on collecting information on the distribution of employer-paid hourly earnings.[15] 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 by regions (Northeast, Midwest, South 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; bus drivers; clerical employees; food servers; custodial employees; maintenance employees in schools, etc.), gender, and type of area (metropolitan vs. non-metropolitan).

[14] 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.

[15] 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.

Figure 1.6a shows an example of the raw tabulations for the laundries sector. The BLS data allow us to transparently study the evolution of the hourly wage distributions in each sector over time and to investigate the heterogeneity of the impact of the 1967 reform across many dimensions.

For the purposes of this project, we digitized over 1,000 hourly wage earnings distributions every year from 1961 to 1969.[16]. 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;[17] see figure 1.6b.

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.[18] IPUMS released the 1962-1967 files with a harmonized industry variable in 2009. Since 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 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

[16]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

[17]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.

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

variable includes 23 different industries.[19] This is thinner than the 2-digit NAICS code but a bit coarser than the 3-digits 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.[20] 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 numbers 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 less observations than in later years, increasing the level of noise compared to more recent years. There is a slight difference in employment *counts* between the 1960 Census data and the early CPS files. However, the employment *shares* by industry and race match the information contained in the decennial census data. 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 estimates, but it increases standard errors for the years 1962-1967.

[19] 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.

[20] 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.

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. We checked that the borders of the state groups do not cross region or division lines. Importantly, we checked that the states within each group had 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.[21] 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. More details are provided in the appendix.

Minimum wage database. We use the report of the minimum wage study commission (1981) to build our minimum wage database by state, gender, and industry.[22] We supplement it with the Department of Labor Handbook on women workers (1965).[23] In 1965, 31 states and the District of Columbia had minimum wage laws. Details are provided in the appendix.

[21] 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).

[22] 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.

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

4 The Wage Effects of the 1967 Reform

A Identification Strategy, Sample, and Summary Statistics

We start by studying the effect of the 1967 extension of the minimum wage on the dynamics of wages in the CPS. Our baseline empirical approach is a cross-industry differences-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 1.7). We also show that workers do not move from one group of industries to the other around 1967. There is no discontinuity in the share of U.S. workers employed in the treated vs. control industries (figure A18a), nor in the share of black and white workers in those groups (figure A18b). As discussed below, our effects are robust to the inclusion of a wide range of controls and time-varying effects, such as state, industry, and race linear trends, 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. Before age 21, workers were subject to a different, lower minimum wage that is not the focus of our study. We also exclude the self-employed, workers in grouped 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 wages). Throughout the analysis, control industries include all industries that were covered in 1938 (that is, we exclude from the analysis the industries covered in 1961, 1974, and 1986, which together employed about 25% of the workforce). As shown by table 1.3, 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 1.2 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. In both the control and treated industries, black workers were less educated than white on average (around 40-45% have more than 11 years of schooling 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 full-time, full-year workers 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:

$$\log w_{ijst} = \alpha + \sum_{k=1}^{19} \beta_k \text{Covered 1967}_j \times \delta_{t+k} + \nu_j + \lambda_t + \mathbb{X}'_{ijst}\Gamma + \varepsilon_{ijst} \qquad (4.1)$$

where $\log w_{ijst}$ denotes the log annual earnings of worker i in industry j, state s, in year t.[24] 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 ν_j and λ_t are industry and

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

year fixed effects, respectively. The coefficient of interest, β_k, measures the effect of the 1967 reform k years after the baseline year (1961 in what follows). In all our analyses, we control for the following worker-level characteristics: gender, race, age, age squared, education, and part-time and full-time status. We also control for the number of weeks worked,[25] and the number of hours worked.[26] In section 5 below, we show that the reform did not affect the number of hours worked per year conditional on working.[27] 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).

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

Figure 1.7 shows the effect of the 1967 reform on the log annual wages of treated workers relative to control workers. Before the implementation of the reform in February 1967, the annual wages 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 wages increased substantially—by about 5%—for

[25]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.

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

[27]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;$0.25] (after converting our hourly wage measure to 2017$).

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 1.2. After 1971, the point estimates stabilizes 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) 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 1.3, 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 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 section 27 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 1.4). For these workers, the average increase involved from moving straight to the $1 nominal minimal wage introduced in 1967 is 34%; see column (2). The predicted wage effect 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.[28] The predicted wage effect is slightly larger than the observed effect, however, which could be due to several factors. There is measurement error in hourly wages, and there may be imperfect compliance with the reform, and effects of the reform on employment.

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 wage model for workers with 11 years of schooling or less vs. more than 11 years of schooling; see figure 1.8a. For workers with low levels of education, wages increase 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 (including high-skill) in the 1967 industries.

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, which were covered by the minimum wage in 1938.[29] We adapt our cross-industry

[28] Since we make predictions for 1967 alone, we compare the predicted effects to our wage coefficient obtained for 1967 alone (see figure 1.7 rather than to the pooled estimate for 1967-1972 presented in table 1.3).

[29] 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 form a good control group in this empirical setting.

design to the nature of the BLS data by estimating the following model:

$$\begin{aligned} y_{jrt} = \alpha &+ \beta_1 \text{Covered 1967}_j \times \text{Post}_t \times \text{South}_r \\ &+ \beta_2 \text{Covered 1967}_j \times \text{Post}_t + \beta_3 \text{Post}_t \times \text{South}_r \\ &+ \beta_4 \text{Covered 1967}_j \times \text{South}_r + \nu_j + \eta_r + \lambda_t + \varepsilon_{jrt} \end{aligned} \tag{4.2}$$

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 $\times$ region level. In addition, $\hat{\beta}_4$ in this specification allows us to investigate whether the wage effects are larger in the South. 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 1.6 shows that within the strict sample, wages in the newly covered industries jump by 8% relative to wages in non-durable manufacturing after the reform (1967-1969) relative to before. The magnitude of the rise is very similar to the 7% wage increase estimated using CPS data. The wage increase is higher for treated industries in the South relative to non-durable manufacturing industries in the non-South (+14%). The pattern and magnitude of the wage results are similar in the full sample of BLS industries.

C Robustness Tests and Other Estimation Strategies

The main threat to our baseline identification strategy are 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, say, in the South and if there was a sudden convergence in wages between workers in the South and in the North in 1967, then our estimates would be confounded. To address this concern, in Column 2 of table 1.3 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.

Robustness to sectoral shocks. One might be concerned about shocks happening in some treated industries, such as agriculture (e.g., mechanization). In column 3 of table 1.3 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 of 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 1.3 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. Since the intensity of the treatment varies by state, and since there might be reasons to believe that unobserved components of the annual wage for workers are

correlated within states, we implement a two-way clustering (industry and state levels). The precision of our results is unchanged.[30]. Finally, following Cameron et al. (2008) we implement a wild bootstrap approach to cluster standard errors, as in both the state and industry dimensions we have a small number of clusters (16 clusters when clustering by industry and 22 for states). Wild bootstrap improves the precision of our estimates a bit.

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 1.9 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 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]:

$$\log w_{ist} = \alpha + \sum_k \beta_k \text{Strongly treated state}_s \times \delta_{t+k} + \mathbb{X}'_{ist}\Gamma + \nu_s + \delta_k + \varepsilon_{ist} \quad (4.3)$$

where Strongly treated state$_s$ is an indicator for a state with no minimum law in January 1966. The coefficient of interest, β_k, measures the effect of the 1967 extension of the federal minimum wage k years after or before the year chosen as a baseline (1965 in this case). We control for the same workers' characteristics as

[30]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

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 3% more than in weakly treated states just after the reform and over the period 1967-1972 (see table 1.5). As in our cross-industry design, the effect is concentrated on workers with low levels of education.

D Wage Effects by Race

We now turn to our second key finding: the magnitude of the wage response to the 1967 reform is 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 Table 1.7). 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 1.8b). Similarly, we compare black workers in the treated industries to black workers in the control industries (dark line in figure 1.8b), controlling for observables as in our benchmark specification. Strikingly, black workers in the treated industries saw their wage rise 12% more than black workers in the control industries starting in 1967. Because the wages of black workers in the control industries are themselves rising faster than the wages of white workers in the control industries, the wage of black workers in the treated industries rises much faster (+20%) than average (black plus white) wages in the control industries (see Appendix Figure A5).

5 The Employment Effects of the 1967 Reform

A Bunching Estimator

Methodology. We start by studying the effect of the 1967 extension of the minimum wage on overall employment in the treated industries—and the employment of low-paid workers in particular—using the BLS industry wage reports.

We proceed as follows. Following Harasztosi and Lindner (2017), we first inflate the observed 1966 wage distributions (expressed in nominal dollars of 1966) by 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 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.

In our baseline estimate, 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. That is, we allow for spillover effects of the minimum wage up to 115% of the minimum wage, consistent with the spillover effects estimated in the recent minimum wage literature (see, e.g., Dube et al., 2018a). 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. This wage level also corresponds to 1.15 times the highest state minimum wage in force in 1967 ($1.50 minimum in New York). In the robustness tests presented below, we investigate how varying the first, second, or both assumptions together affects the results.

Case study: laundries in the South. We start by implementing this estimation strategy in laundries in the South. This case study is interesting for three reasons. First, laundries are a low-wage industry: in 1963, 85% of the workforce was paid

below $1.25 (the federal minimum wage applicable in sectors covered since 1938), including at very low wage levels (below $0.50 an hour). Second, black workers represent 40% of the workforce as opposed to 14% in the treated industries at the national level. Third, because southern states did not have any state minimum wage legislation, the 1967 reform is 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 1.10a shows the hourly wage distribution in that sector 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 distributions is the same in 1963 and 1966, except that the distribution shifts to the right as the economy grew and prices increased. Where the minimum wage was introduced at $1 in 1967, by contrast, a very large spike in the earnings distribution appears at $1. There is bunching at the minimum wage. The spike moves to the right in 1968 as the minimum wage increased to $1.15.

Table 1.8 estimates employment effects by applying the methodology described above. 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. These effects are small in the sense that the differential growth of employment (1.5% vs. 3.0%) is small relative to the wage increase for treated workers (+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 (i.e., if we compare employment below $1.05 in 1967 to employment below $1.00 in 1966), 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). 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. 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 a lot 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.

Generalized estimates. We implement the bunching approach for all the industries for which we have information both in 1966 and 1967 in the BLS industry wage reports, i.e., hotels, restaurants, and laundries (see figure 1.6b). We include all regions (not only the South). 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.[31]

In our benchmark estimate, we find a small positive employment elasticity of the reform. As shown by table 1.8, 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%). Table 1.8 shows that 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 1.8. This result is consistent with the estimate we obtain using a cross-state design in the CPS (see Section B below). Our result of a small employment elasticity overall is also robust to varying assumptions on

[31]The tipped minimum wage is introduced at $0.50 in 1967 in hotels and restaurants, i.e. 50% of the value of the minimum wage. There is clear evidence of bunching at 50 cents for tipped minimum wage workers in 1967, see appendix figures A9 and A11.

the spillover effects of the minimum wage. As reported in Table 1.8, considering spillover effects up to 120% of the minimum wage (instead of 115%) leads to a small negative employment elasticity (-0.28). Assuming there are no spillover effects, we obtain a zero effect elasticity (-0.03). In other words, it is not the case that there is a missing mass of workers at just the level of the minimum wage offset by an excess mass just above. This finding suggests that labor-labor substitution (e.g., substitution of $1 workers by slightly higher skilled individuals) is not driving our estimates of small employment elasticities. [32]

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 A18a shows that prior to the reform, treated vs. control industries were on similar trends, and that in 1967 and 1968 they continue to grow at the same rate. From 1969-on, treated industries start 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.

[32] We only have suggestive evidence that there is no important skilled-based labor-labor substitution. Ideally, if we had information on the demographic characteristics of the workers (in particular about their age and level of education) in the BLS industry wage reports, we could divide our sample by age and education levels groups. Following Cengiz et al. (2018), we could plot each groups missing mass below the new minimum wage and the excess number of jobs at the minimum wage. If these estimates were aligned on the 45 degree line, we could conclude that there is no evidence for systematic labor-labor substitution base on skills and experience.

B 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 C 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 evolve in parallel in strongly vs. weakly treated states before the reform.

Intensive margin. Starting with the the effect of the reform on the annual number of hours worked, we estimate a difference-in-differences model similar to the one of section C, except that the outcome is log annual hours.[33] Figure 1.11a 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 table 1.9. 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).[34]

Extensive margin. Next, we investigate the impact of the reform on the probability of being employed. We define non-employment as being unemployed or out of the labor force. This allows us to capture potential effects of the reform on labor force participation (in particular for women). As shown by table 1.10, the reform does not appear to affect the probability of being employed, with a point estimate for the difference-in-differences coefficient of interest of 0.001. The effect is precisely estimated. We are able to rule out a reduction in employment probability of more than 0.3 percentage points. Because average wages in the strongly

[33] Annual hours are constructed as the ratio between annual wage (directly measured in the CPS) and the (re-constructed) hourly wage.

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

treated states grew by 3% above and beyond wage growth in the weakly treated states, the lower bound employment elasticity is -0.1. As shown by Figure 1.12, this estimate is in the range of elasticities found in the minimum wage literature.

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 on Table 1.10 we can rule out a reduction in the probability of being employed for black persons of more than -1.8 percentage points. Since average wages increased 11.1% 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 (1.12). Because the 1967 reform had large positive effects of 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).

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.

A Unadjusted Racial Gap

We start by investigating how the reform affected the economy-wide unadjusted racial gap. To simplify the analysis, we only include the industries covered in 1938 and in 1967, 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 (Figure 1.1a). 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:

$$G^{\text{total}} = s_w^c G^c + s_w^t G^t + G_b^{ct}(s_w^c - s_b^c) \tag{6.1}$$

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%. [35]

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 counterfactual G^t (resp. G_b^{ct}) 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:

[35] see appendix C for a derivation of the decomposition.

$$\begin{cases} \forall k \leq 1966 : G^t_{k,\text{counterfactual}} = G^t_{k,\text{observed}} \\ \forall k > 1966 : G^t_{k,\text{counterfactual}} = G^c_{k,\text{observed}} - \frac{1}{N}\sum_{k=1961}^{1966}(G^c_{k,\text{observed}} - G^t_{k,\text{observed}}) \end{cases} \tag{6.2}$$

As shown by figure 1.13, the 1967 minimum wage extension can explain around 20% of the decline in the racial earnings gap between 1967 and 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 improvements in schooling quality found by Card and Krueger (1992).[36]

B Adjusted Racial Gaps

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

$$\log w_{ijt} = \alpha + \gamma \text{Black}_i + \sum_k \beta_k \text{Black}_i \times \delta_{t+k} + \mathbb{X}'_{ijt}\Gamma + \nu_j + \delta_k + \varepsilon_{ist} \tag{6.3}$$

Where Black_i is a dummy for being a black worker; the set of individual level

[36] There are some differences, however, between our calculations and Card and Krueger (1992)'s calculations that make a precise comparison not straightforward. 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).

controls $\mathbb{X}'_{ijt}$ is the same as in the wage regression (gender, number of years of schooling, experience, industry, full-time or part-time status, occupation and marital status).

Figure 1.14a 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 were paid 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. $\gamma + \beta_k$, k in [1961;1980]) for the control and treated industries in figure 1.14b. 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-1970 the adjusted racial gap vanished in the control industries (see light blue lines in figure 1.14a), 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 decompose the adjusted racial earnings gap for high-skill workers (12

years of schooling or more) vs. low-skill workers (11 years of schooling or less) in the treated and the control industries. Within the treated industries (figure 1.14a), the decline in the adjusted racial gap is concentrated among low-skilled workers. By contrast, there is no change in trend for high-skill workers. Within the control industries (figure 1.14a), the decline in the adjusted racial earnings gap is smooth for both high and low-skill workers. These results further suggest that the extension of the minimum wage (and not some other confounding shock) really is the driving force behind the decline in the adjusted racial earnings gap in the treated industries.

C Discussion

How can we explain the large wage and small dis-employment effects of the minimum wage we obtain? One hypothesis is that before the reform, whites 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.

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 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. 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 our paper focuses 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 wages increases on gender and racial earnings gaps today.

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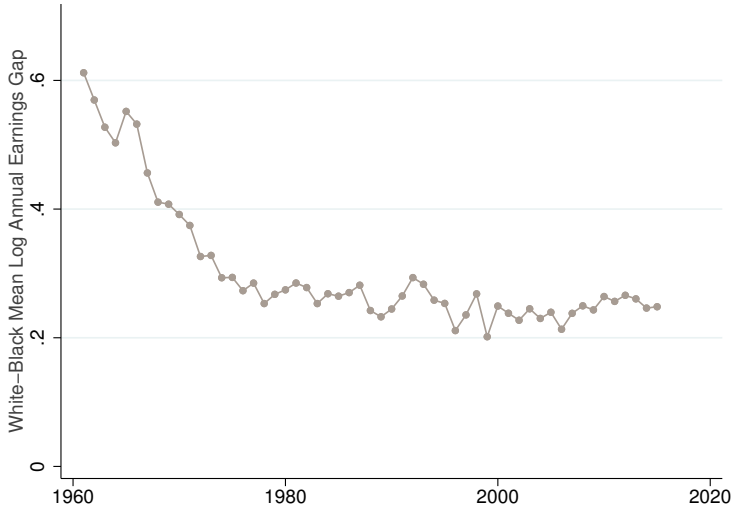
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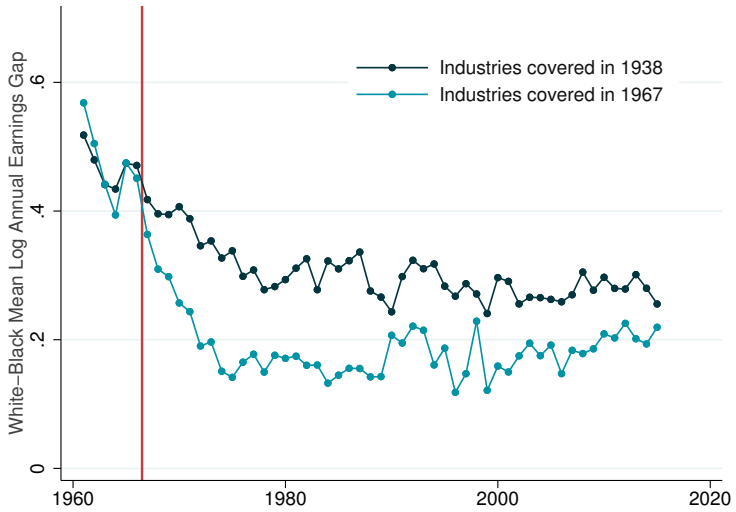
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Figure 1.1 – White-black unadjusted wage gap in the long-run

(a) Economy-wide

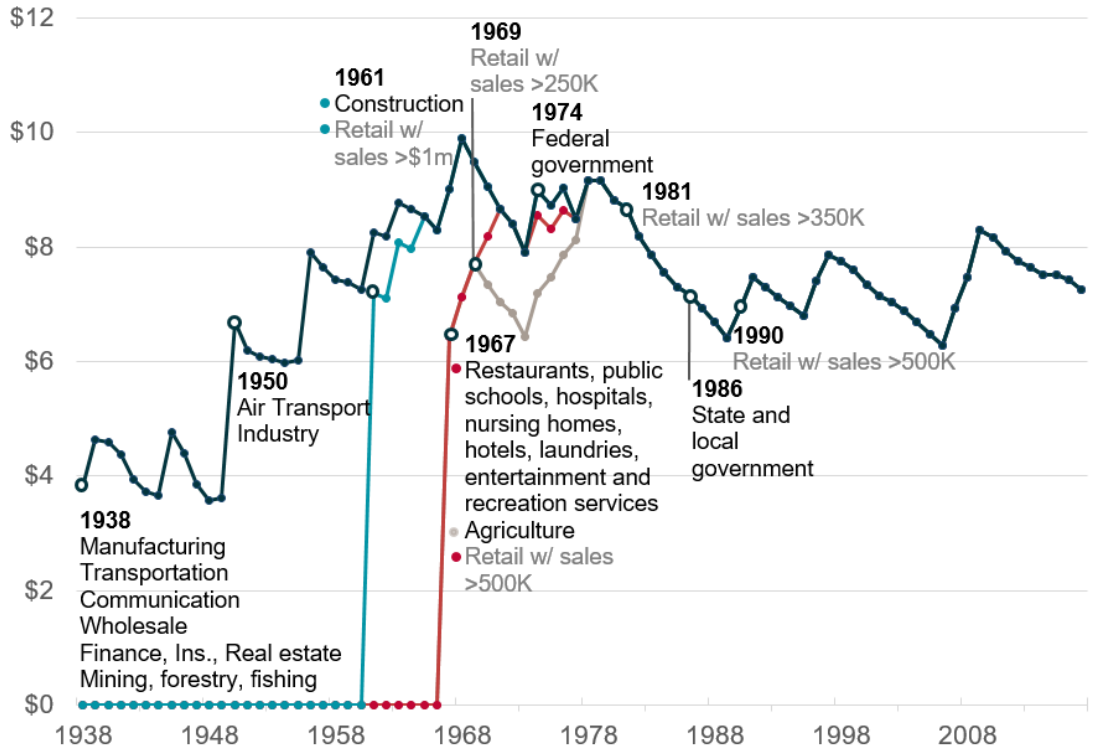


(b) By type of industry



Source: Annual Social and Economic Supplement of the Current Population Survey, 1962-2016.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: The racial gap is calculated as difference in the average log annual earnings of black workers and the 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, our estimate of the racial gap in March 1962 is reported in 1961.

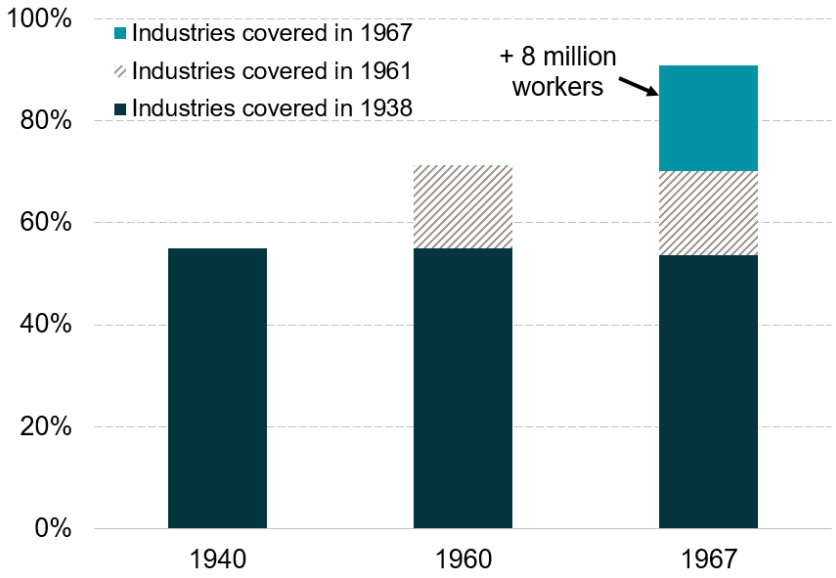
Figure 1.2 – Expansions in min. wage coverage, and real values of the minimum wage 1938-2017 ($2017)



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.

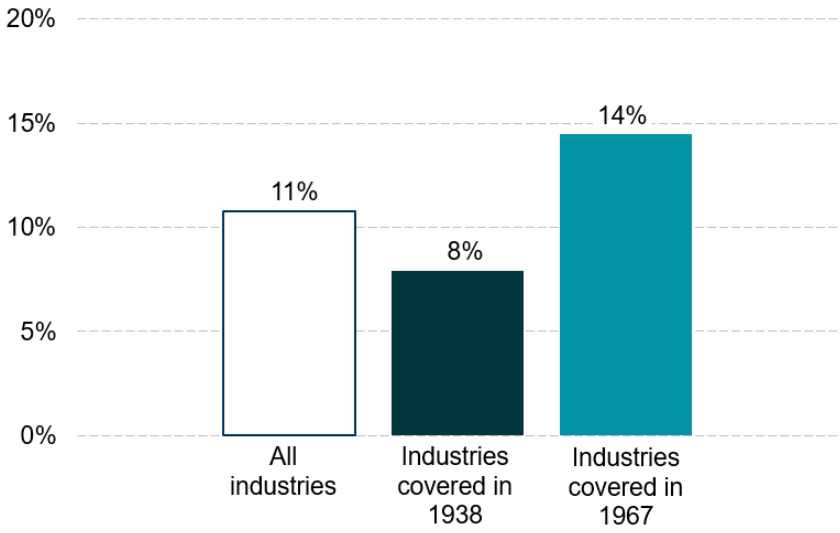
Notes: The 1938 Fair Labor Standards Act introduced the federal minimum wage in manufacturing, transportation, communication, wholesale trade, finance, insurance and real estate, mining forestry and fishing. In 1950, the federal minimum wage was expanded to the air transport industry in 1950. In 1961 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. It is introduced at $1 in nominal terms ($7.18 in $2017), which is only 87% of the federal minimum wage that year. It increases gradually over the following years. Minimum wages series deflated using CPI-U-RS ($ 2017).

Figure 1.3 – Share of workers covered by the minimum wage, 1940-1966



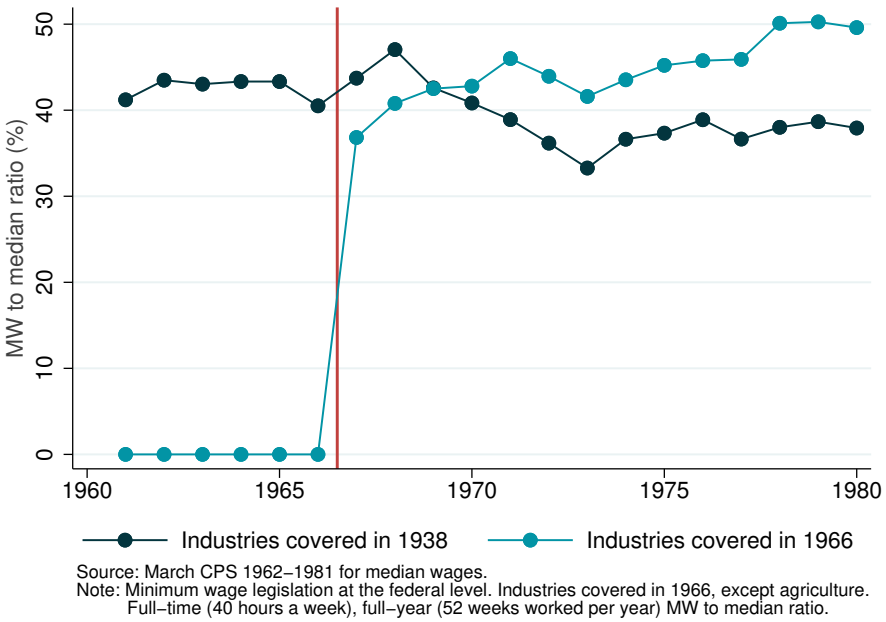
Sources: US Censuses 1940 and 1960. March CPS 1967.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.
Notes: Coverage by federal minimum wage.

Figure 1.4 – Black share of black and white workers in 1967



Source: March CPS 1967. Sample: Adults 25-55, black workers, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.
Notes: Coverage by federal minimum wage.

Figure 1.5 – Minimum wage to median ratio



Source: March CPS 1962–1981 for median wages.
Note: Minimum wage legislation at the federal level. Industries covered in 1966, except agriculture. Full–time (40 hours a week), full–year (52 weeks worked per year) MW to median ratio.

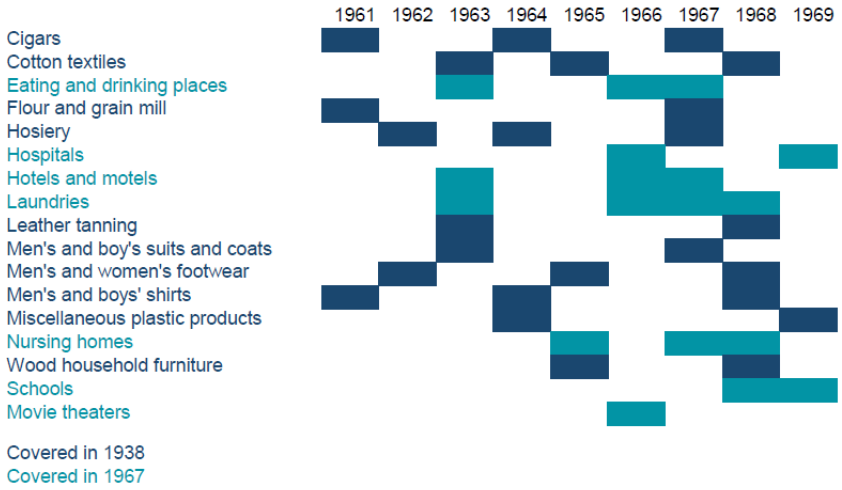
Source: March CPS 1962-1981 for median wages.
Sample: Adults 25-55, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.
Notes: Minimum wage legislation at the federal level. Industries covered in 1966, except agriculture. Full-time (40 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.

Figure 1.6 – BLS Industry Wage Reports

(a) What they look like – 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	6.0	4.4	6.0	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

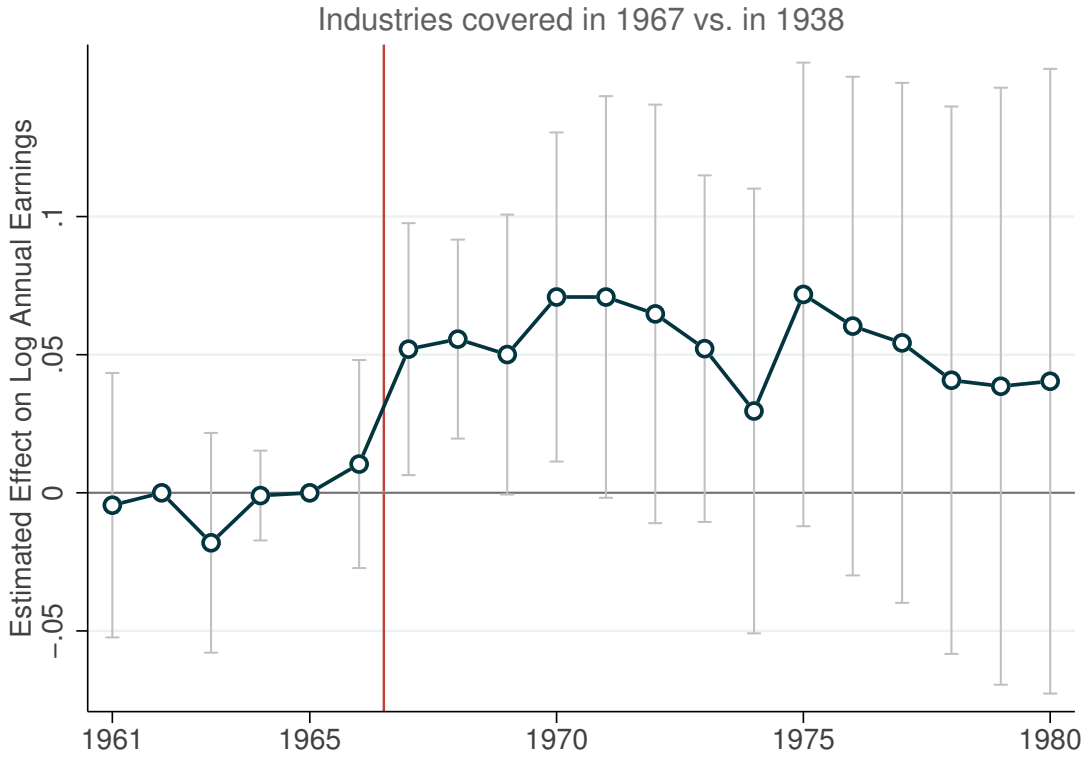
(b) Set of industries and years we digitized



Source: Bureau of Labor Statistics Industry Wage Reports.

Notes: Panel (a) shows an example of hourly wage tabulations for laundries – a sector in which the minimum wage is introduced at $1 in 1967. Those tabulations provide information on the hourly wage distribution by 5 cents or 10 cents 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. Panel (b) 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.

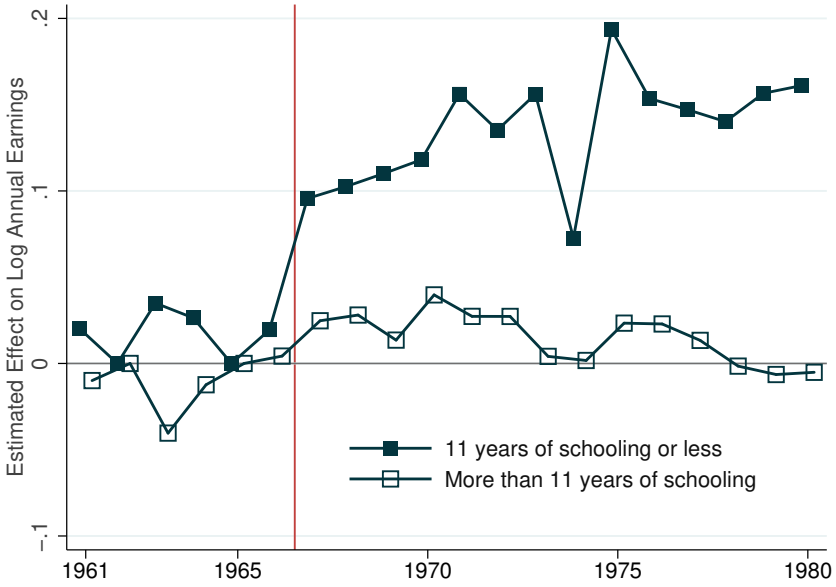
Figure 1.7 – Impact of the 1966 FLSA on annual wages



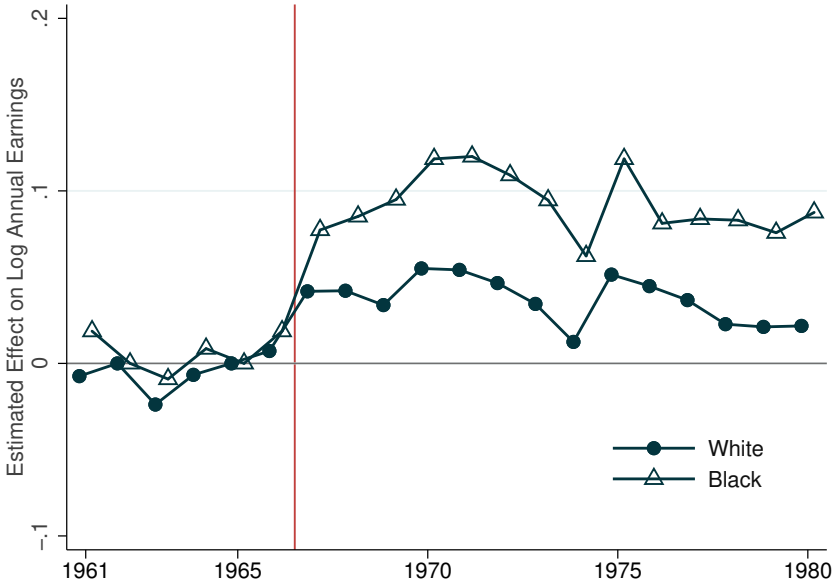
Source: March CPS 1962-1981.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Since the variable annual earnings refer to the annual earnings earned the previous year, we start our graph in 1961. Standard errors clustered at the state (group) level. Includes industry and time fixed effects. Year 1962 is excluded and set to zero.

Figure 1.8 – Heterogeneity of the wage effect of the 1966 FLSA

(a) By level of education

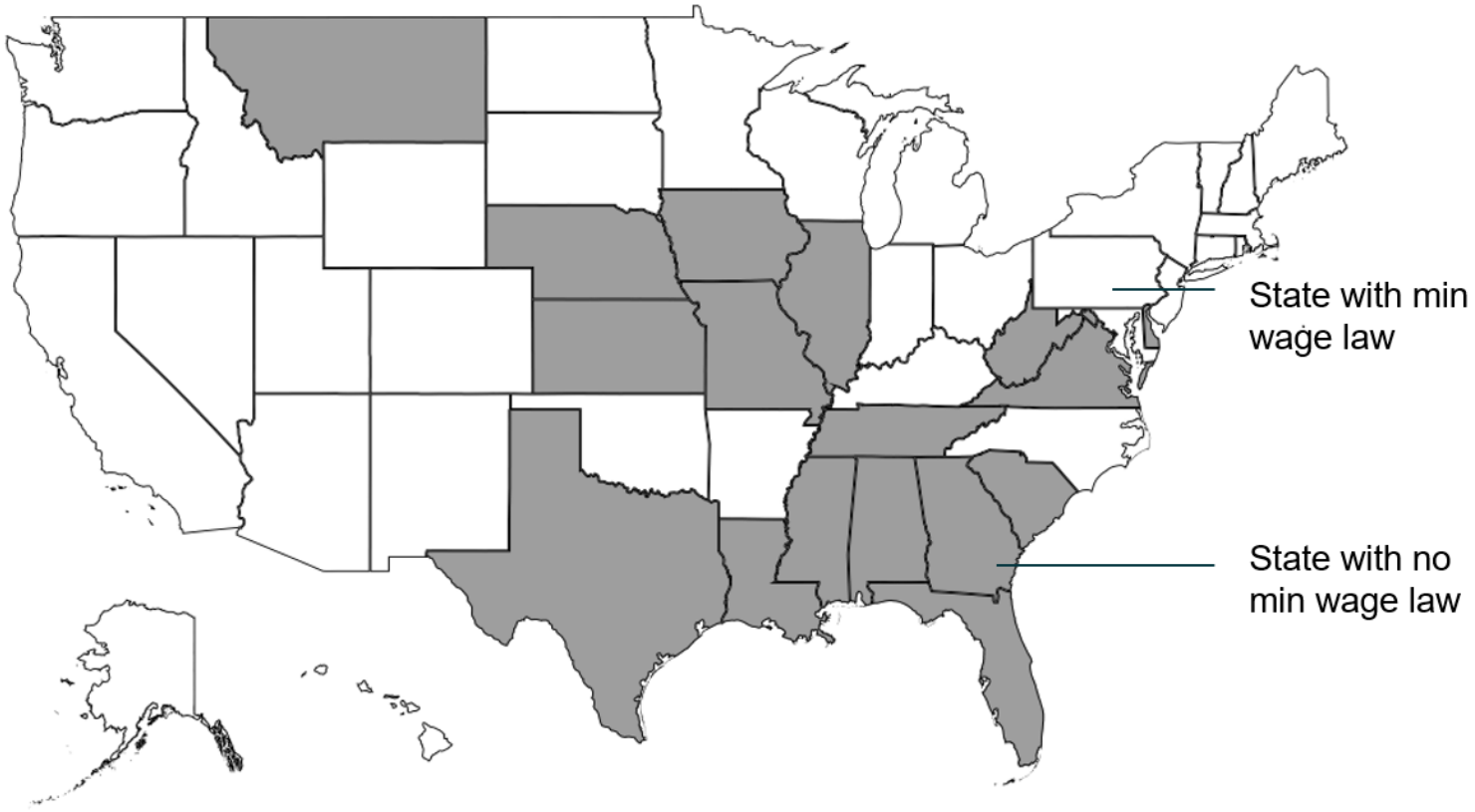


(b) By race



Source: March CPS 1962-1981. Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Low-education: 11 years of schooling or less. High-education: more than 11 years of schooling.

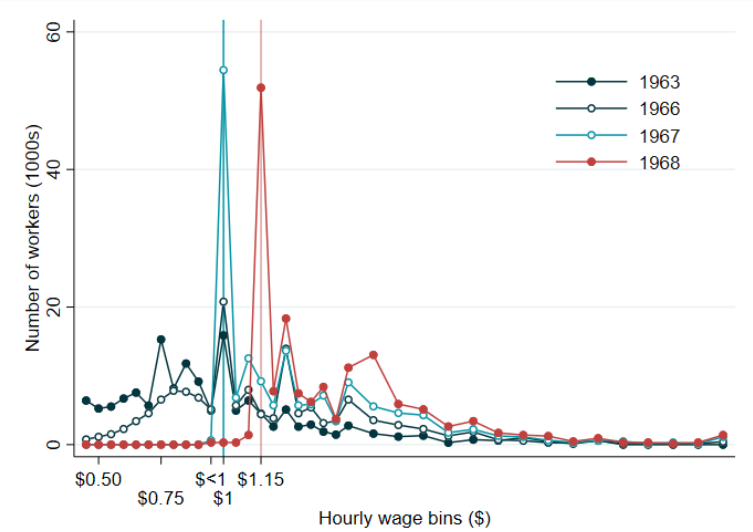
Figure 1.9 – States with no minimum wage laws as of January 1966



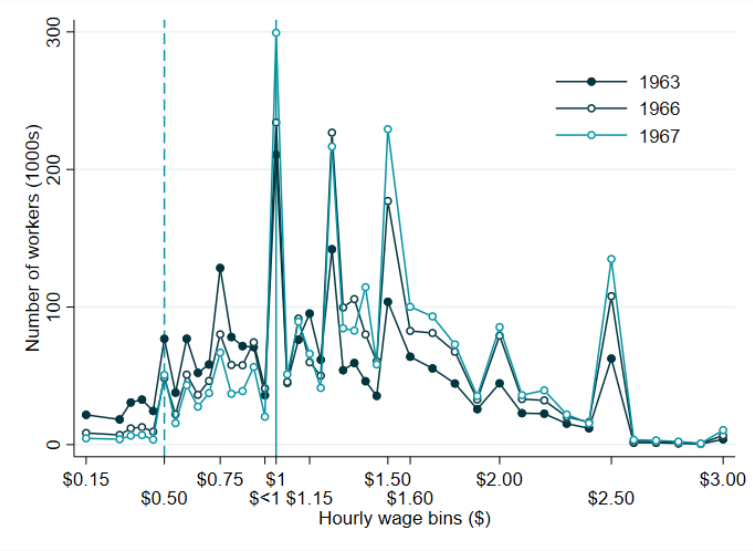
Source: Authors' minimum wage database 1950-2016. More details provided in appendix A.

Figure 1.10 – Earnings Distributions in the BLS Industry Wage Reports

(a) Laundries Earnings distribution in South



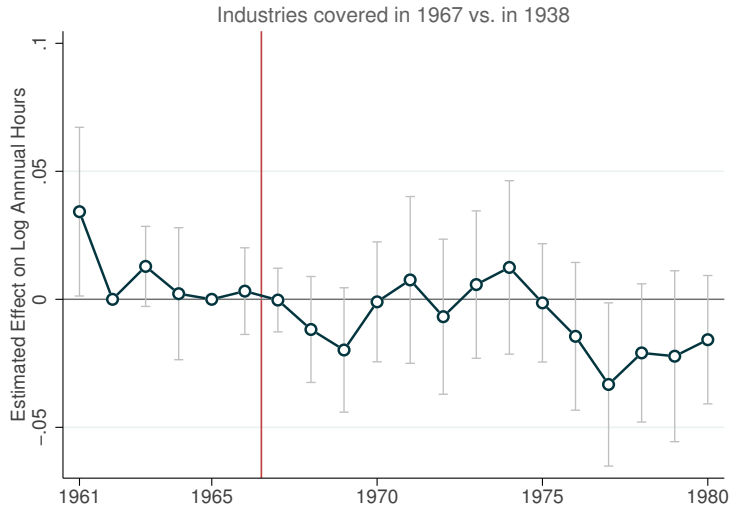
(b) Earnings distributions in hotels, restaurants and laundries – U.S.



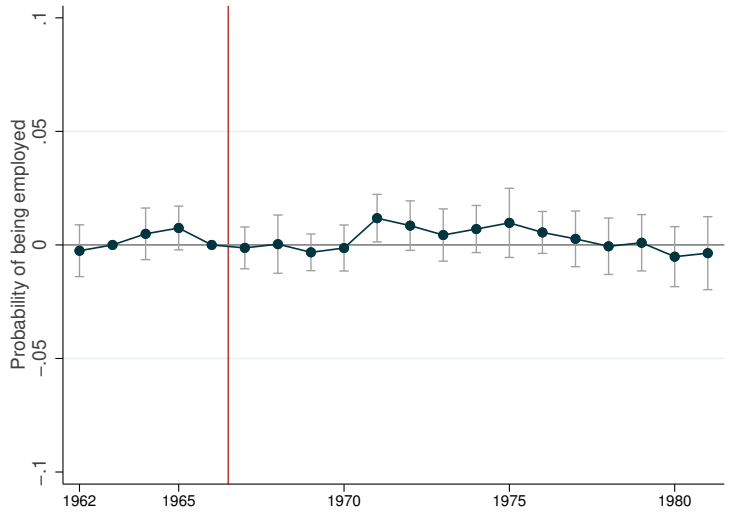
Source: BLS Industry Wage Reports.
Sample: Panel (a): All nonsupervisory workers, except routemen; Panel (b) All nonsupervisory workers in restaurants, and in laundries (except routemen); all nonsupervisory employees in year-round hotels, motels and tourist courts. Notes: Panel (a) The minimum wage is introduced at $1 in nominal terms in laundries in 1967. It is further increased to $1.15 in 1968; Panel (b) The minimum wage is introduced at $0.50 for tipped workers in hotels and restaurants in 1967. For non-tipped workers, in restaurants, hotels and laundries, the minimum wage is introduced at $1.

Figure 1.11 – Impact of the 1966 FLSA on employment

(a) Intensive margin: annual number of hours worked

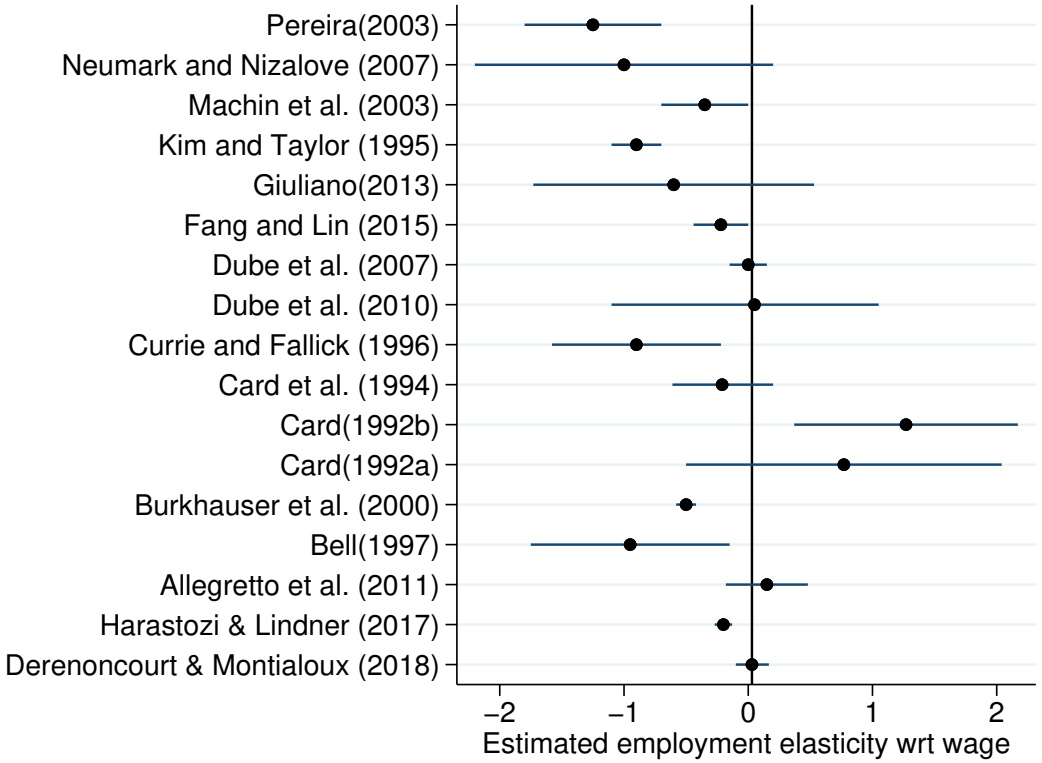


(b) Extensive margin: probability of being employed (vs. not unemployed or not in the labor force)



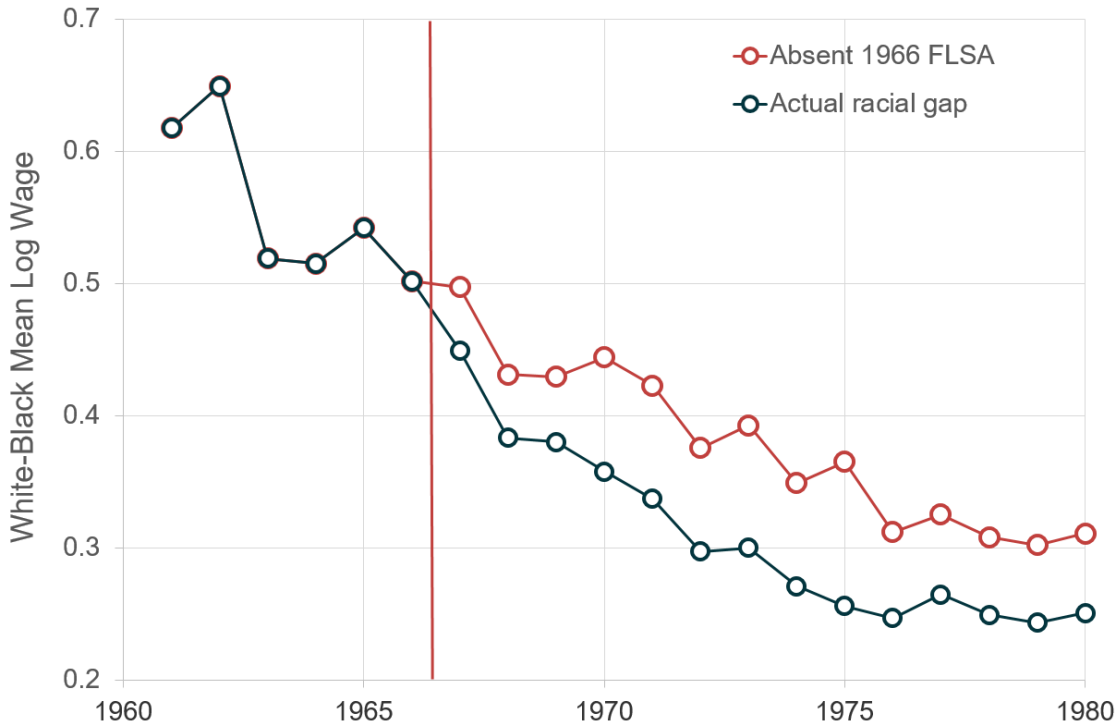
Source: March CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Panel (a) The annual number of hours is calculated as the ratio between annual earnings last year and the hourly earnings measure reconstructed using the information on the number of weeks worked and hours worked available in the CPS; Panel (b) the outcome of interest is the probability of being employed (vs. being unemployed or not in the labor force). Standard errors clustered at the industry and state (group) level. Includes state and time fixed effects.

Figure 1.12 – Employment elasticities wrt wage in the literature and in this paper



Note: This figure is taken from Harasztosi and Lindner (2017), and adds our estimate in this paper. It summarizes the estimated employment elasticities with respect to the average wage, and compares it to the previous literature.
The red vertical line shows our estimate for the employment elasticity wrt wage (0.016).

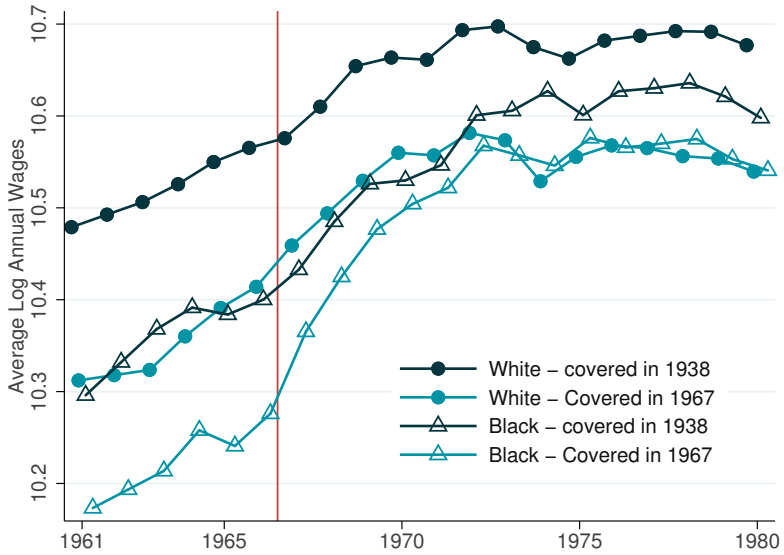
Figure 1.13 – 1967 reform reduced overall racial gap by $\sim 20\%$



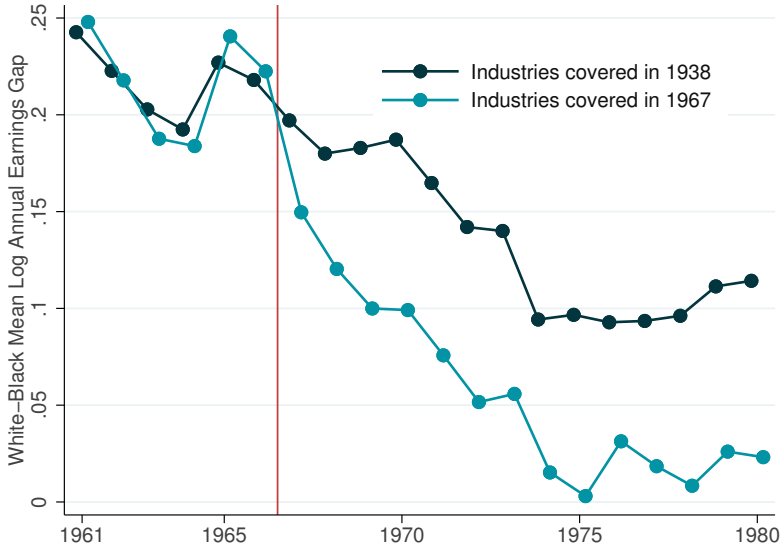
Source: March CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

Figure 1.14 – Adjusted racial wage gaps

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



(b) Adjusted racial earnings gaps, by treatment status



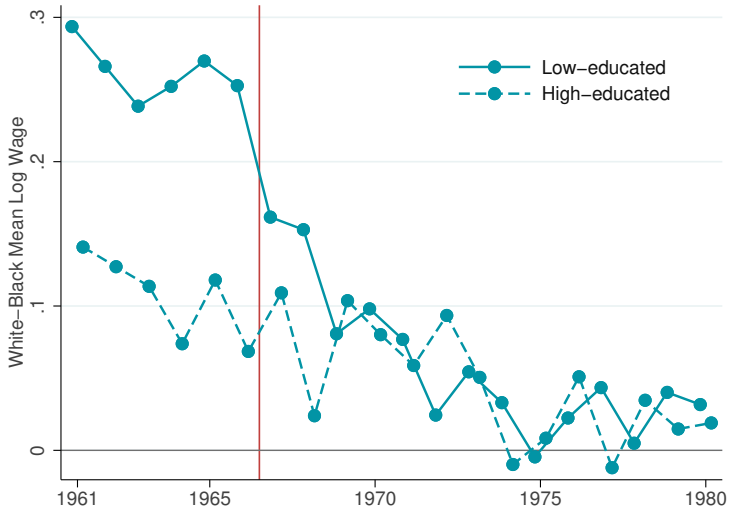
Source: March CPS 1962-1980.

Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

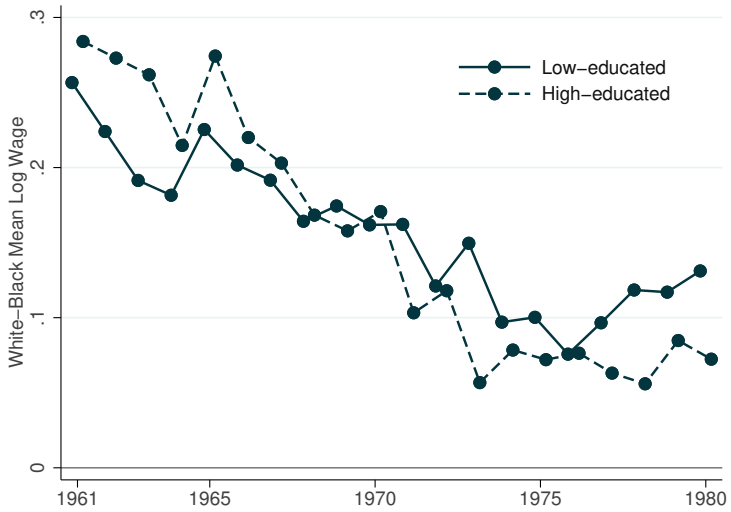
Notes: Racial earnings gap measures adjusted for gender, number of years of schooling, experience, full-time or part-time status, industry, occupation and marital status. In panel (a), the reference group is a male worker in 1965, 12 years of schooling, married, professional and technical occupation, 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.

Figure 1.15 – 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 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Racial earnings gap measures adjusted for gender, number of years of schooling, experience, full-time or part-time status, industry, occupation and marital status.

Table 1.1 – Employment, and earnings by race, 1967

	Employment		Employment shares		Earnings ($2017)	
	Number	Percent	White	Black	White	Black
All industries	38,490,848	100%	89%	11%	$42,575	$24,522
Industries covered by 1938 FLSA	20,663,098	54%	92%	8%	$46,469	$29,174
Manufacturing	13,134,427	34%	91%	9%	$45,622	$30,322
Transportation, Communication, and Other Utilities	2,960,552	8%	93%	7%	$47,750	$28,620
Finance, Insurance, and Real Estate	1,783,952	5%	96%	4%	$46,021	$22,923
Wholesale Trade	1,445,985	4%	94%	6%	$53,229	$25,547
Business and Repair Services	921,756	2%	90%	10%	$44,334	$23,764
Mining	377,885	1%	97%	3%	$47,433	$35,444
Forestry and Fishing	38,539	0%	83%	17%	$34,261	$15,804
Industries covered by 1961 FLSA	6,336,330	16%	92%	8%	$39,854	$23,701
Retail trade	3,961,711	10%	93%	7%	$35,438	$24,463
Construction	2,374,619	6%	89%	11%	$47,520	$22,868
Industries covered by 1966 FLSA	7,962,920	21%	86%	14%	$33,435	$21,405
Schools	2,913,630	8%	90%	10%	$38,560	$30,513
Nursing Homes and other professional services	1,419,030	4%	91%	9%	$37,928	$23,684
Hospitals	1,260,220	3%	79%	21%	$27,767	$20,939
Hotels and laundries	741,447	2%	76%	24%	$25,581	$16,667
Restaurants	777,805	2%	86%	14%	$22,344	$15,777
Agriculture	599,313	2%	75%	25%	$24,406	$11,685
Entertainment and Recreation Services	251,475	1%	87%	13%	$44,099	$22,524
Public administration	2,848,719	7%	87%	13%	$46,944	$35,436
Private households	679,782	2%	31%	69%	$10,054	$8,381

Source: 1967 March CPS.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.
Notes: Annual average earnings in $2017, deflated using annual CPI-U-RS series. Employment numbers refer to the year 1967. Because the CPS collects information on earnings received during the previous calendar year, annual average earnings reported in this table were earned in 1966.

Table 1.2 – Workers characteristics, 1965-66

	Control group		Treatment group	
	White	Black	White	Black
Annual wage (in $2017)	46,469	29,174	33,435	21,405
Age	39.8	38.3	39.9	39.2
Gender				
Male	0.76	0.78	0.43	0.38
Female	0.24	0.22	0.57	0.62
Education				
11 yrs of schooling or less	0.37	0.62	0.26	0.53
More than 11 yrs of schooling	0.63	0.38	0.74	0.47
Marital status				
Married	0.86	0.77	0.77	0.66
Single	0.12	0.14	0.21	0.22
Region				
North Central	0.30	0.28	0.28	0.18
North East	0.30	0.23	0.27	0.19
South	0.26	0.42	0.27	0.56
West	0.14	0.07	0.19	0.07
Occupation				
Operatives	0.32	0.51	0.03	0.12
Craftsmen	0.20	0.12	0.03	0.01
Clerical and kindred	0.16	0.08	0.14	0.06
Managers, Officials and proprietors	0.11	0.01	0.05	0.01
Professional and technical	0.10	0.03	0.43	0.21
Sales worker	0.06	0.00	0.00	0.00
Service worker	0.01	0.09	0.30	0.56
Other	0.03	0.15	0.01	0.02
Full-time/part-time status				
Full-time, full-year	0.88	0.79	0.69	0.67
Part-time	0.12	0.21	0.31	0.33

Source: March CPS 1966-67. Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.

Table 1.3 – Wage effect: Main results and robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 ×						
1967-1972	0.066**	0.060**	0.056**	0.065**	0.061**	0.066**
	(0.025)	(0.024)	(0.022)	(0.023)	(0.021)	(0.029)
1973-1980	0.050	0.045	0.037	0.056	0.043	0.050
	(0.042)	(0.041)	(0.039)	(0.040)	(0.035)	(0.046)
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	Y	Y	Y	Y	Y	Y
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 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Standard errors clustered at the industry and state (group) level. Includes state, industry and time fixed effects.

Table 1.4 – 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	16.0	34.2	5.5	5.3
Low-education	31.3	33.5	10.5	10.1
High-education	9.6	35.0	3.4	2.6
Black	29.4	36.9	10.8	8.0
White	13.8	33.2	4.6	4.6

Source: March CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
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 1.5 – Wage effect using the cross-state design

	All
1967-1972	0.041***
	(0.011)
1973-1980	0.060***
	(0.017)
Obs	407,823
Controls	Y
Time FE	Y
State FE	Y

Source: March CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Standard errors clustered at the industry and state (group) level. Includes state, industry and time fixed effects.

Table 1.6 – Hourly wage effect using BLS data

	Strict Sample	Full Sample
Covered in 1967 ×		
1967-1969	0.081***	0.089***
	(0.024)	(0.025)
1967-1969× South	0.136***	0.092***
	(0.048)	(0.033)
Obs	89	167
Time FE	Y	Y
Industry FE	Y	Y
Region FE	Y	Y

Source: BLS Industry Wage Reports. See figure 1.6b for the set of tabulations digitized.
Sample: All nonsupervisory employees.
Notes: the "full" sample contains industries listed in figure 1.6b. 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 × region level.

Table 1.7 – Wage effect by race

	Black	White
Covered in 1967 ×		
1967-1972	0.095***	0.054**
	(0.022)	(0.023)
1973-1980	0.078*	0.036
	(0.037)	(0.042)
Obs	37,770	370,053
Controls	Y	Y
Time FE	Y	Y
Industry FE	Y	Y

Source: March CPS 1962-1981.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Standard errors clustered at the industry and state (group) level. Includes state, industry and time fixed effects.

Table 1.8 – 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 1.6b 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.

Table 1.9 – Effect of 1967 reform on annual number of hours worked (intensive margin)

	All	Black	White
Covered in 1967 ×			
1967-1972	-0.014	-0.008	-0.020
	(0.012)	(0.022)	(0.012)
1973-1980	-0.021	-0.014	-0.026
	(0.016)	(0.025)	(0.015)
Obs	407,752	37,760	369,992
Controls	Y	Y	Y
Time FE	Y	Y	Y
Industry FE	Y	Y	Y

Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: The annual number of hours is calculated as the ratio between annual earnings last year and the hourly earnings measure reconstructed using the information on the number of weeks worked and hours worked available in the CPS. Standard errors clustered at the state (group) level.

Table 1.10 – Effect of 1967 reform on probability of employment (extensive margin)

	All	Black	White
State with no mw law ×			
1967-1972	0.001	-0.002	0.001
	(0.002)	(0.009)	(0.003)
1973-1980	-0.001	-0.004	-0.000
	(0.004)	(0.013)	(0.004)
Obs	435,621	41,882	393,739
Controls	Y	Y	Y
Time FE	Y	Y	Y
State FE	Y	Y	Y

Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: The outcome of interest is the probability of being employed (vs. being unemployed or not in the labor force). Standard errors clustered at the industry and state (group) level. Includes state and time fixed effects. Standard errors clustered at the state (group) level.

Chapter 2

The Pass-Through of Minimum Wages into US Retail Prices

Abstract: This chapter estimates the pass-through of minimum wage increases into prices of US grocery stores. We use high-frequency scanner data and leverage a large number of state-level increases in minimum wages between 2001 and 2012. We find that a 10% minimum wage hike translates into a 0.2% increase in grocery prices. This magnitude is consistent with a full pass-through of cost increases into consumer prices. Prices rise as much for goods consumed by low-income and for those consumed by high-income households. Depending on household income, grocery price increases offset between 3 and 12% of the nominal income gains. Our results suggest that consumers rather than firms bear the cost of minimum wage increases in the grocery sector.

1 Introduction

Minimum wage increases are very popular among voters in the US and many other countries. For example, in an opinion poll conducted in 2016 in the US, 78% of the respondents were in favor of increasing the federal minimum wage (YouGov/HuffPost, 2016).[1] Perhaps as a result, minimum wage increases at the state and local level have been ubiquitous in the last decade in the US. Several European countries have recently implemented important minimum wages hikes as well, for example Germany and the United Kingdom in 2015. A central goal in many of these cases is to raise the incomes of low-wage workers. A large body of research in labor economics shows that moderate increases in the minimum wage have no or limited disemployment effects, suggesting that it can raise nominal incomes of low-wage workers. However, we typically care about real rather than nominal incomes. One concern with minimum wage policies is that the nominal wage increases for low-wage workers are offset by price increases among goods and services consumed by the poorest households. To assess the impact of minimum wages on real incomes, it is thus central to understand the pass-through of minimum wage increases into prices.

In this paper, we study the pass-through of minimum wages increases into prices in the US. We exploit a large number of state changes in the minimum wage between 2001 and 2012 and leverage scanner-level data from weekly price observations of 2,500 distinct grocery stores. We use these data to make three main contributions. First, we provide new evidence on how minimum wages affect prices in the grocery sector, which had not been previously studied in the literature. Studying this sector is important, because groceries make up a large share of consumer expenditure, up to 15% for low-income households. Second, we take advantage of the high frequency of the data to study the dynamics of the price response over time. Since minimum wage laws are usually passed several

[1] This chapter was written with Tobias Renkin and Michael Siegenthaler.

months before implementation and typically institute a schedule of increases rather than one-off hikes, firms may increase prices in anticipation of higher future minimum wages. Using a newly collected dataset with legislation dates for every minimum wage increase in our sample period, we find strong evidence for anticipation effects. Third, we use a large consumer panel data linked to the store-level information to investigate how the price response varies across household income groups. This allows us to estimate the welfare implications of the grocery price effects of minimum wages.

Our main finding is that there is a full pass-through of minimum wage increases into grocery prices. We use two research designs to establish this result. One compares price movements across states exploiting time variation in state-level minimum wage hikes. The other exploits within-state variation in the bite of the hikes. Both designs suggest that a 10% minimum wage hike translates into a 0.2% increase in grocery prices. The price effects are larger in large and cheaper stores and in stores located in "right-to-work" states. Importantly, the average price elasticity of 0.02 is about the same size as our estimate of the minimum wage elasticity of groceries' costs, which suggests a full pass-through of cost increases. We neither find evidence that demand for grocery products increases, nor evidence that stores reduce employment. These central results suggest that consumers, rather than firm-owners or workers, bear the bulk of the burden of minimum wage increases in the grocery sector.

Another important and novel finding of this paper is that price adjustments occur mostly in the three months following the passage of minimum wage *legislation*, rather than after its actual implementation. Using Google Trends data, we show that the legislation of minimum wage increases represents a salient event at which the public get information about future minimum wage hikes. Based on a flexible event study regression tracking prices around the months in which minimum wage hikes are legislated, we find that grocery stores respond to the future cost increase by increasing selling prices, months before the minimum

wage is actually adjusted. Price inflation in grocery stores almost doubles in the three months following legislation. This forward-looking behavior of firms is qualitatively consistent with the predictions of pricing models with nominal rigidities.

We then quantify the welfare consequences of the minimum wage after accounting for full pass-through of minimum wages into prices. We first estimate that the price effects of minimum wages are similar for goods usually consumed by low-income and for goods usually consumed by high-income households. Low-income households are nevertheless disproportionately affected by the rise in grocery prices since a larger share of their expenditures is on groceries. Taken together, the price response of grocery stores undoes about one tenth of the nominal income gains for very low-income households (earning less than $10,000 a year). For households earning between $10,000 and $40,000 a year, price increases in grocery stores offset roughly 3% of the nominal income gains. Overall, the price response reduces the nominal gains for all households, but the price increases in grocery stores offset only a relatively small part of the gains of minimum wage hikes. Minimum wage policies thus remain a redistributive tool even after accounting for price effects in grocery stores.

This paper contributes to several strands of the literature. First, our paper yields new insights into the redistributive effects of minimum wages and into the price effects of minimum wages in low-wage sectors. A large literature has studied the labor market impact of minimum wages following the Card and Krueger (1994) study of employment effects in fast-food restaurants. Our paper contributes to a much smaller literature studying the product market effects of minimum wage increases. Card and Krueger (1994), Aaronson (2001), Aaronson and French (2007), Aaronson et al. (2008) and Allegretto and Reich (2018) have studied the price effects of minimum wages in restaurants. Outside of the US, Fougère et al. (2010) analyze the response of restaurant prices to an increase in the French minimum wage. Depending on time period and type of restaurant, these

papers find positive price elasticities between 0.04 and 0.1. Beyond restaurants, the product market impact of minimum wages has received little attention. Draca et al. (2011) study the effects of minimum wages on firm profits in the UK. They find that minimum wages reduce profits, but only in industries in which firms have high market power. Harasztosi and Lindner (2017) study the firm response to a large minimum wage increase in Hungary. Their results are consistent with a large degree of pass-through into prices.

We contribute to this literature by shedding light on a new and important sector, grocery stores, for which it was impossible to precisely estimate the price pass-through of minimum wages due to the lack of high-quality data. Our use of high-frequency scanner data combined with a large number of state changes in the minimum wage also enables us to overcome certain shortcomings in studies of the price effects of minimum wages in the US restaurant industry. These limitations include classical measurement error (Card and Krueger, 1994; Aaronson, 2001); the use of city-level CPI data that are only available in the largest US metro areas (Aaronson, 2001; Aaronson and French, 2007; Aaronson et al., 2008); and the fact that price and wage changes in restaurants may not be well measured due to tipping and quality changes (e.g., size of portions served). These concerns do not apply to retail scanner data, as products in grocery stores are very standardized and retail workers are not tipped.

Two closely related contemporary papers also study the relationship between minimum wages and grocery store prices. Ganapati and Weaver (2017) and Leung (2017) focus on the 2005–2015 and 2006–2015 periods, respectively, and are both based on the same alternative dataset—the Nielsen data. Nevertheless, the two studies reach conflicting conclusions. Ganapati and Weaver (2017) find no effect of minimum wages on prices. On the other hand, Leung (2017) finds price elasticities that are larger than ours (around 0.06). There are several important differences between these papers and ours beyond the different data source and sample coverage. First, our empirical approach differs from theirs. While Ganapati

and Weaver (2017) and Leung (2017) focus on panel fixed effects regressions, we estimate more flexible pass-through regressions common in the international economics literature. This transparent approach exploits the high frequency of the price data and allows us to track closely when and by how much prices respond to minimum wage hikes.[2] Moreover, it allows us to control flexibly for unobserved differences in the evolution of prices across states and stores. Second, Ganapati and Weaver (2017) and Leung (2017) study the price effect at the time of implementation of higher minimum wages. However, our results suggest that the price effects are concentrated at the time minimum wage legislation is passed.[3] We do not find robust effects at the time of implementation of higher minimum wages, and if so, they are difficult to distinguish from other factors such as changing inflation trends. By focusing on minimum wage effects at legislation, our paper also provides novel insights on the extent to which price-setting is forward-looking. Two other distinctive features of our work are that we study in detail whether our results are consistent with full pass-through of prices into costs, and that we quantify the extent to which the price increases in grocery stores affect the redistributive effects of minimum wage policies.

Our paper also contributes to the more general question of how cost shocks are passed on into retail prices. So far, the literature has studied the pass-through of changes in the costs of wholesale purchases. These papers provide mixed evidence on the extent wholesale prices are passed on to consumers.[4] The pass-through of labor cost into retail prices has not been studied in detail, and our paper provides estimates of the causal effect of increasing labor cost on price inflation.

[2]We explain the empirical differences between our work and both contemporary contributions in more detail in Appendix I.

[3]The newest version of Leung (2017) confirms that, also in his data, prices rise right after the announcement of a minimum wage hike.

[4]For instance, Eichenbaum et al. (2011) study the pass-through of wholesale cost into prices, and find that pass-through is complete but somewhat delayed. Nakamura and Zerom (2010) use variation in the market price of commodity coffee and find that the pass-through into wholesale prices is about one third, but that the increase of wholesale prices is completely passed through to consumers by retail stores.

To the best of our knowledge, our paper is also the first to use firm level microdata to document a price response to a future cost shock at the time it becomes known, and several months before it actually occurs. Forward-looking price setting is a central prediction of models with nominal frictions. Such models include the well-known Calvo (1983) model of staggered price setting, or models with adjustment cost such as Rotemberg (1982) and more modern menu cost models studied in Nakamura and Steinsson (2008) or Midrigan (2011). In the macroeconomics literature, these models have been used as a microeconomic foundation for the New Keynesian Phillips Curve (NKPC). A central feature of the NKPC is that current inflation is driven by expectations of future marginal cost. This idea has had a great influence on the way macroeconomic policy is conducted, yet the large empirical literature estimating the NKPC from aggregate data does not provide conclusive evidence on the extent to which price setting is actually forward-looking (Mavroeidis et al., 2014). Our paper uses a clearly identified future cost shock to show that, at least in the case of minimum wages in the US, firms do set prices in a forward-looking manner.

The remainder of this paper is organized as follows. The next section presents the institutional context and stylized facts relevant for minimum wages and grocery stores in the US. Section 2 discusses the data we use. Section 3 describes our empirical approach. Our main results on the minimum wage elasticity of prices are presented in section 4. In section 5, we estimate the impact of minimum wages on grocery stores' cost. Section 6 discusses the welfare cost of grocery price increases in relation to the nominal benefits of minimum wages. Section 7 summarizes our results and concludes.

2 Context and data

A Minimum wage data

The minimum wage has a long history in the United States, going back to the early 20th century. Today, federal minimum wage laws cover most workers, and higher state-level rates are common. Many municipalities have also raised their minimum wage since 2013. During the period from 2001 to 2012 covered by our price data, however, only San Francisco, CA, and Santa Fe, NM, had local minimum wage ordinances. We therefore focus on state-level minimum wage changes in our paper. For each state, we collect the legally binding rate, i.e. the maximum of federal and state rates. Overall, we exploit 166 increases in the binding state-level minimum wage.

Typically, the literature studying minimum wages focuses on the effects of minimum wages at the time they are implemented. However, most minimum wage increases are known in advance so that firms have ample time to act in anticipation. We thus combine data on the implementation of state minimum wages[5] with newly collected information on the time that each minimum wage law is passed, derived from legislative records and media sources. In some cases, passage of legislation is preceded by a series of votes and negotiations; in this case, we try to assess from media sources at which point in the process a minimum wage increase became certain. A good example is the "Fair Minimum Wages Act of 2007" that raised the federal minimum wage from $5.15 an hour to $7.25 in three steps in July 2007, 2008 and 2009. The act was passed in slightly different versions in January 2007. After a conference committee added tax-cuts for small businesses to the bill, the final version was passed and signed by President Bush in May 2007. Since the passage of the actual minimum wage part of the bill seemed certain already in January, we use January as the month of legislation in

[5]We use data from the Tax Policy Center, the US Department of Labor, and state departments of labor to construct this data set. A similar dataset is available from Zipperer and Vaghul (2016).

our baseline.[6]

An important assumption of our approach is that the legislation dates represent points in time when future minimum wage increases become more salient. We use Google Trends data to assess the plausibility of this assumption. Google trends is available from 2004 onward. We use the search volume for the term "minimum+wage+*statename*" over a month to measure interest in the local minimum wage of a given state.[7] We then estimate the following simple regression using this data:

$$\log search_{s,t} = \delta_s + \gamma_t + \sum_{r=-k}^{k} \beta_r incr_{s,t-r} + \sum_{r=-k}^{k} \alpha_r legis_{s,t-r} + \epsilon_{s,t}. \tag{2.1}$$

$incr_{s,t-r}$ and $legis_{s,t-r}$ are dummy variables indicating implementation of a higher minimum wage and passage of minimum wage legislation in state s in period $t-r$. The results of this regression are presented in Figure 2.1. Both around implementation and around the date of legislation, interest in minimum wages goes up substantially, by about 30% immediately after legislation is passed. There is no elevated interest in minimum wages in the months before legislation is passed. Three months after passage of legislation, search volume is back at the baseline value. These results show that the passage of minimum wage legislation is a salient event and that the public takes notice of pending minimum wage increases when they are written in law. The results also validate our coding choices in the collection of legislation dates.

The primary explanatory variables in our analysis are changes in the implemented minimum wage and changes in the "legislated minimum wage." Figure 2.2 shows how we measure the "legislated minimum wage". It is the highest future binding minimum wage set in current law. The legislated minimum wage

[6]We present results using only state-level legislation to show that our conclusions hold more generally and are not driven by this single important event.

[7]Note that we do not measure search requests originating from different states, but from the US as a whole for different search terms.

increases to the highest future minimum wage at the time the law is passed.

Table 2.1 and Figure 2.3 describe important features of the 166 changes in the implemented and the 62 changes in the legislated minimum wage. Table 2.1 shows that the average increase in the binding minimum wage amounts to 8.2%. Changes in the legislated minimum wage are larger on average (20%), since they usually encompass several steps. The average interval between passage of legislation and implementation of a first hike is 9 months. Hence, even the first increase in a package is often known long before it is implemented. Moreover, 36% of all increases in the implemented minimum wage and 42% of increases in the legislated minimum wage result from changes at the federal level. 24% of all increases in the implemented minimum wage result from indexation. Minimum wages in states with indexation are pegged to the national development of prices and exhibit small annual increases. We do not assign legislation dates to increases following from indexation.[8] Figure 2.3 shows the distribution of changes in the implemented and legislated minimum wage over states and time. All states in our sample experience at least 2 minimum wage hikes. The maximum is 11. Most of the events in our sample occur between 2006 and 2009.

B Minimum wages in the grocery sector

In this section, we present three stylized facts motivating our analysis of the price effects of minimum wages in the grocery sector. First, we show that groceries are an important factor in households' cost of living, particularly for poor households. Second, we show that labor costs are an important part of the overall costs of grocery stores. Third, we document that a substantial share of grocery store employees are paid wages close to the minimum wage, and as a result, minimum

[8]Indexation is practiced in 10 states at the end of our sample period. The following states in our sample have indexation: Arizona, Colorado, Florida, Missouri, Montana, Nevada, Ohio, Oregon, Vermont, and Washington. Most of these states introduced indexation starting in 2008 after ballots held in November 2006. The exceptions are Florida, Vermont (both began indexation in 2007), Oregon (beginning in 2004) and Washington (beginning in 1999).

wage increases affect the labor costs of grocery stores.

The first of these facts is shown in Table 2.2. The table presents the expenditure share of groceries using data from the Consumer Expenditure Survey (CES). We count the categories Food at Home, Household Supplies, Alcoholic Beverages and Personal Care Products and Services as groceries. Measured this way, groceries make up about 11% of household expenditures on average. For households in the poorest quintile, groceries make up 14 to 15% of expenditures. For households in the richest quintile the share amounts to 9%.

Table 2.3 presents the second fact. It shows the cost shares in total costs, variable costs and revenues of grocery stores (NAICS 4451) in 2007 and 2012. The table is based on a detailed breakdown of the costs of grocery stores available in the BLS Annual Retail Trade Survey for these years. Total costs include all operating expenses plus the Cost of Goods Sold (COGS). Variable costs comprise of labor costs, COGS, transport and packaging costs. The table illustrates that by far the most important factor in grocery store costs are the COGS. According to these data, the labor cost share in variable cost—which should matter for price setting in the short run—amounts to roughly 16%.[9]

The third fact—the importance of minimum wage workers in the grocery sector—is illustrated in Figure 2.4. Using, data on hourly wages from the NBER files of the CPS MORG, the figure plots the distribution of wages in grocery stores relative to the local minimum wage. A large share of grocery store workers are paid wages at or close to the local minimum wage during all three periods. In the period when most of the minimum wage hikes in our sample happen (2006–2009), 21% of grocery store workers earn less than 110% of the minimum wage. Recent literature suggests that even workers with wages above the minimum wage may be affected by "ripple effects" of a hike (Autor et al., 2016; Dube et al., 2015),

[9]These labor shares do not include purchased services. These services make up about 2% of total costs and include some tasks that are likely done by low-skilled workers, for example maintenance work. These costs may depend on minimum wages as well, but it is hard to determine to which extent.

and as a result a large share of grocery store workers would likely be affected by minimum wage increases. At the end of our sample period, for instance, almost half of all grocery store workers earn less than 130% of the local minimum wage. As shown by Table A8 in the appendix, the share of these workers in total hours worked in groceries amounts to approximately 40% in this period, and the share in total labor earnings to 25%.

C Data on prices in grocery stores

Our empirical analysis is based on scanner data provided by the market research firm Symphony IRI. The dataset is described in detail in Bronnenberg et al. (2008). It contains weekly prices and quantities for 31 product categories sold at grocery and drug stores between January 2001 and December 2012. The estimation sample covers 2,493 distinct stores and on average 60,600 products over this period. Products are identified by Unique Product Codes (UPC). As an example, a 12oz can and a 20oz bottle of Coca Cola Classic are treated as different products in our data. Stores are located in 530 counties, 41 states and belong to one of about 90 retail brands. The data covers 17% of US counties which are home to about 29% of the US population.[10] Most of the included product categories are packaged food products (frozen pizza, cereals, etc.) or beverages (soda, coffee, milk, etc.). The data also includes personal care products (deodorants, shampoo, etc.), housekeeping supplies (detergents, paper towels, etc.), alcoholic beverages (beer and some flavored alcoholic beverages) and tobacco.

Our empirical analysis is based on monthly store-level price indices.[11] We

[10] Figure A1 in the appendix shows the regional distribution of stores.

[11] There are several reasons why we use store-level indices instead of more disaggregated product level price data. First, wages are paid at the store and not at the product level, and we thus think that stores are the natural unit of analysis. Second, it is useful to weight products by their importance for stores and consumers, and store-level price indices are a natural way to do so. Third, entry and exit are much less of a concern at the store level than at the product level. Especially low-volume products are frequently introduced and discontinued, and may also go unsold for extended time periods due to stock-outs, seasonality or low demand. This results in frequent gaps in products' price series. By contrast, our panel at the store level is much more balanced.

detail in appendix A how we constructed them. Our approach closely follows methods used in previous articles on retail price movements (see, e.g.. Coibion et al., 2015). Since we study the price response to a permanent shock, temporary price changes (such as sales), which tend to be very large in monthly price data, are of no particular interest. We thus apply a sales filter suggested by Kehoe and Midrigan (2015) to remove temporary price fluctuations. The algorithm uses a moving window modal price to determine a "regular price" at any point in time. Importantly, the use of regular prices for our baseline index does not affect the conclusions we draw, but increases the precision of our results. We show in appendix A that the features of our price index are in line with what other researchers have documented for the IRI Symphony data and other retail price datasets.

D Data on employment and wages in grocery stores

We rely on several other data sources in our empirical analyses. Most importantly, we estimate the employment and earnings effects of minimum wages in grocery stores using data on employment and the total wage bill provided by the Bureau of Labor Statistics in the publicly available Quarterly Census of Employment and Wages (QCEW) files. The QCEW publishes a quarterly count of employment and wages reported by employers in their Unemployment Insurance (UI) contributions and covers more than 95 percent of jobs in the US. The QCEW is available at the county-industry level. We use data for grocery stores (NAICS 4511), retail (44–45), and accommodation and food services (72).

3 Main empirical specification

We estimate the price response to minimum wage increases by relating month-on-month store-level inflation rates to increases in the binding minimum wage and passage of minimum wage legislation at the state level. The identification

strategy is based on the idea that, conditional on a set of controls and fixed effects, inflation in stores in states that did not experience a minimum wage hike or new legislation is a useful counterfactual for stores in states that did. Many papers studying the effects of minimum wages in the US apply variants of this identification strategy (see Allegretto et al., 2017). The high frequency of our price data allows us to estimate detailed temporal patterns of the effects before and after an event. Our specification is similar to those commonly used in the international economics literature to study the pass-through of exchange rate variation (for example Gopinath et al., 2010):

$$\pi_{j,t} = \delta_j + \gamma_t + \sum_{r=-k}^{k} \beta_r \Delta mw_{s(j),t-r} + \sum_{r=-k}^{k} \alpha_r \Delta leg_{s(j),t-r} + \psi X_{j,t} + \epsilon_{j,t} \tag{3.1}$$

In this model, $\pi_{j,t}$ is the month-on-month inflation rate in grocery store j and calendar month t. The main exogenous variables of interest are the change in the logarithm of implemented and legislated minimum wages in the state $s(j)$ in which store j is located, which we denote $\Delta mw_{s(j),t}$ and $\Delta leg_{s(j),t}$, respectively. The coefficients β_r and α_r measure the elasticity of inflation with respect to minimum wage increases or legislation r months ago, or r months in the future in case r is negative. In our baseline estimation we control for time fixed effects γ_t and store fixed effects δ_j. Because our estimation is in first differences, the latter account for *trends* in stores' price levels. The vector of controls $X_{j,t}$ includes the county-level unemployment rate and state-level house price growth. We include these control variables to absorb variation in grocery prices that is due to business cycles or the boom and bust in house prices.[12] None of our results depend on the inclusion of controls beyond time fixed effects, but the additional controls tend to improve the precision of the estimates.

We estimate several variants of equation 3.1. First, we estimate the effects

[12]See Stroebel and Vavra (2015) for a discussion of the relationship between real estate and retail prices.

at legislation and implementation separately by omitting all terms related to either $\Delta mw_{s(j),t}$ or $\Delta leg_{s(j),t}$. However, since legislation is often passed in the 9 months preceding implementation, these separate estimates may capture the same variation in prices. To take this into account, we also jointly estimate effects at legislation and implementation of minimum wage increases by estimating equation 3.1 in full. In some specifications, we control for chain-time and census division-time fixed effects, which should capture changing trend inflation within chains or regions and effects of changes in wholesale prices, which could correlate between stores that are geographically close or within the same chain. Some specifications also control for state times month fixed effects, which account for possible state-specific seasonal patterns in prices.

Because both the price level and minimum wages are non-stationary, we prefer estimating equation 3.1 in first differences rather than levels. However, the estimates are best illustrated as the effect of minimum wages on the price level. We construct cumulative sums of β_r and α_r coefficients in the presentation of our results. We normalize the effect to zero in a baseline period two months before an event, and calculate the cumulative effect as $E_R = \sum_{r=-1}^{R} \beta_r$. We also summarize pre-event coefficients in a similar way. To be consistent with the normalization we calculate them as $P_R = -\sum_{r=2}^{-R-1} \beta_{-r}$. Our baseline measure of overall elasticities is E_4 and thus includes effects in the 6 months from one month before to 4 months after an event.[13] We report E_4 separately for implementation of minimum wages and passage of legislation, as well as the sum of both.

An important choice in our estimation is the number of estimated lag and lead coefficients k. One constraint here is that minimum wage hikes generally occur in regular intervals, often within 12 months (see Table 2.1). This implies that some observations lie, for instance, 8 months after the last and 4 months before the next minimum wage hike. In principle, we can disentangle the effects of the two

[13] In principle, we could of course report E_k and include all lag coefficients. However, coefficients beyond 4 months out are typically close to zero and insignificant. In most specifications E_k is not significantly different from E_4 but substantially less precise.

events in such cases because many states do not have minimum wage increases before 2005 and after 2009, and because some states increase minimum wages only infrequently. However, our estimation strategy will not work in practice for large k, as the leads and lags become increasingly collinear. A second constraint that we face is that our store panel is not balanced. The more leads and lags we include, the more likely it is that changes in the underlying store sample may affect our estimates. In our baseline estimation, we settle on estimating the effect with $k = 9$. This is sufficient to show the short run impact of minimum wage increases on prices. We present results for longer or shorter windows in robustness checks.

The central concern with our estimation and identification strategy is the possibility of reverse causality. States with higher inflation rates could have more frequent and higher nominal minimum wage increases to avoid reductions in the real minimum wage. In this case inflation would cause minimum wage increases, rather than the other way around.[14] Although we view it as unlikely that legislators consider changes in state-level grocery price inflation within the few months relevant for our empirical analyses, we deal with this concern in our estimation in several ways. First, our main specification includes store fixed effects, which absorb differences in trend inflation between states. Second, due to the high frequency of our price data and the flexible estimation model, we can closely examine the timing of the effect, and any remaining differences in inflation trends around a minimum wage event would be easily detected in our pre-event coefficients. Third, we present estimates that only use variation due to changes in the federal minimum wage. We view it as unlikely that federal lawmakers take into account regional inflation differences when setting the federal minimum wage policies.

[14] A special case are minimum wage increases following from indexation. All states that practice indexation peg their minimum wage to national inflation rates. Changes in national inflation are absorbed by time fixed effects in our specification.

4 The effect of minimum wage increases on prices

A Main results

In this section, we analyze the effects of minimum wage increases on grocery prices. We first present the results of separate estimations for the effects at legislation and at implementation. Panel (a) of Figure 2.5 shows our baseline estimates around the time of legislation. In this figure—as well as in the rest of the paper—standard errors are clustered at the state level. Panel (a) of Figure 2.5 shows that the pre-event coefficients capture no significant movement in prices in the months leading up to passage of legislation. Prices start to increase significantly in the month preceding legislation and continue to rise for 3 months. After that, prices are stable for the remainder of the estimation window. The results at legislation are robust to the inclusion of time fixed effects for different retail chains and census divisions (Panel b of Figure 2.5), suggesting that our results do not reflect price variation at the regional or chain level that correlates with minimum wage legislation. Columns 1–3 of Table 2.4 list the corresponding estimates of the cumulative price elasticity. Our baseline estimate for the elasticity at legislation is 0.021. The estimates including division-time and chain-time effects are slightly smaller and amount to 0.014 and 0.013.

Panels (c) and (d) of Figure 2.5 present the results at the time of implementation of minimum wage increases. Our baseline estimate for the elasticity at implementation is comparable in size to the one for legislation. It points to a gradual increase in prices in the months leading up to implementation of a minimum wage increase. We show below that these significant pre-trends are driven by minimum wage events that are known long before implementation. Hence, the significant pre-effects capture the effects at legislation for these events. Moreover, when we include chain-time or division-time fixed effects, we find no significant movement in prices before or after implementation of minimum wage increases.

The corresponding elasticities are shown in columns 4–6 of Table 2.4.

Since legislation frequently occurs in the 9 months before implementation of a first hike, the point estimates following passage of legislation and preceding implementation may reflect the same variation in prices. We thus estimate both effects jointly using the full equation 3.1. The results both qualitatively and quantitatively confirm the previous findings. The estimate of the price elasticity at legislation from joint estimation is 0.019 (column 7 of Table 2.4).[15] The estimates of the elasticity at implementation are positive but insignificant. Summing up the coefficients after legislation and implementation, as is done at the bottom of the table ($E_4^{leg} + E_4^{inc}$), suggests an elasticity of 0.036. However, the estimate becomes substantially smaller when we include chain-time or division-time fixed effects (columns 8 and 9 of Table 2.4). Given the limited robustness of the effects at implementation, our preferred estimates of the price elasticity of minimum wages of thus relies on the robust and highly statistically significant elasticity of 0.02 estimated at legislation. However, when estimating pass-through rates below, we also present results based on the sum of effects at legislation and implementation.

Overall, our preferred estimate thus suggests that the average legislated increase in the minimum wage in our sample—increasing the minimum wage by +20%—raises prices in grocery stores by about 0.4% over three months at the time when legislation is passed. In this example, inflation would almost double during these 3 months relative to the sample average rate of 0.13%. By the time the minimum wage has actually risen to the level set in the new legislation, price adjustment appears to be more or less complete. One important implication of these findings is that firms act in a forward-looking manner: they take future costs into account when they set current prices. This finding is consistent with the predictions of price setting models with nominal frictions such as adjustment costs. These frictions make firms reluctant to change their prices too often, and lead them to take into account the whole known future path of costs. Another

[15] Figure A2 presents the cumulative price effects corresponding to these regressions.

implication of these findings is that grocery stores appear to have some market power, as they are able to increase their markups in the months leading up to a minimum wage increase.

B Identification through within-state variation in wages

In the previous section, we use variation in increases in the legislated or implemented minimum wage across states to identify the effect on prices. In this section, we employ an alternative identification strategy which exploits that a statewide minimum wage hike affects stores that pay low wages more than stores that pay higher wages.[16] While we cannot observe stores' wages, we can exploit the large geographic variation in average wages of grocery stores across counties within a state.

To exploit the differences in the bite of a given state-level minimum wage hike across counties, we compute the difference between the actual average quarterly salary in grocery stores and the full-time equivalent minimum wage salary using the QCEW. We then estimate the interaction between local inflation and this relative wage level for different time periods around minimum wage legislation and implementation. The specification for the effects at legislation is presented in equation 4.1:

$$\pi_{j,q} = \delta_j + \gamma_{t,s(j)} + \sum_{r=-kq}^{kq} \alpha_r \Delta leg_{s(j),q-r} \times wage_{c(j),q-r} + \psi X_{j,t} + \epsilon_{j,t} \tag{4.1}$$

The α_r coefficients in this specification capture the extent to which prices of stores in low-wage counties react more (or less) to a given minimum wage hike than prices of stores in high-wage counties in the quarters around an increase in the minimum wage. In the case of legislation, we use the wage at the time legislation is passed as the initial wage ($wage_{q-r}$). In the case of implementation, we use

[16] Similar strategies have been used in the literature studying the employment effects of minimum wages (Card and Krueger, 1994, for example).

the wage two quarters before implementation as the initial wage ($wage_{q-r-2}$) to make sure that the initial wage is not yet affected by minimum wage increases. Because there is variation in wages within a state, we can include state-time fixed effects that absorb all statewide developments that could potentially drive both minimum wage and grocery price increases.

Table 2.5 presents the estimation results. We find that stores in higher wage counties exhibit significantly *lower* inflation than stores in the same state in low wage counties in the quarter legislation is passed. We find no significant relationship between inflation and initial wages in any other quarter around legislation, nor in the quarters around implementation of higher minimum wages. Our estimates suggest that a 10% lower initial wage increases inflation in the quarter legislation is passed by about 0.3%. The effects at legislation are robust to the inclusion of chain-time fixed effects. Overall, these results corroborate the findings presented in the previous section.

C Robustness of baseline results

Our baseline results are robust to various alternative specifications and sensitivity checks. Table 2.6 presents some of these important robustness checks for the joint estimation.[17] Columns 1–4 show that the estimated effects are similar if we weight each store by the number of products used to construct the stores' price index, if we omit the baseline controls or the store fixed effects, or if we include state-calendar month fixed effects, which control more restrictively for possible differences in the seasonality of prices increases across states. They tend to be somewhat larger if we use price indices that are not adjusted for temporary price changes (column 5). Finally, we winsorize the inflation rates below the 1st and above the 99th percentile of the distribution to show that our results are not driven by outliers (column 6).

[17]Table A3 in the appendix shows the same robustness checks for the separate estimation of effects at legislation.

In appendix section C, we present the results of conducting our analysis at the state level instead of the store level. Advantages of the state-level estimation are that the state panel is balanced and that the estimation can be extended to a longer panel without missing leads and lags due to store entry and exit. Reassuringly, the state-level estimates confirm our baseline estimates, both in terms of timing and magnitude of the effect. Moreover, we find no evidence for differential trends in state-level prices between states with and without hike in the 15 months leading up to the legislation of a minimum wage hike. The absence of differences in inflation rates prior to legislation speaks against the concern that price inflation is the cause for minimum wage hikes rather than vice versa in the short estimation window relevant for our analyses.

Figure 2.6 presents a further robustness check that speaks against reverse causality. In particular, we estimate the separate effects for federal and state-level hikes by augmenting our baseline model with separate sets of leads and lags for events following from state and following from federal legislation. The response to new minimum wage legislation is similar in both magnitude and timing for federal and state-level minimum wage changes. While changes in state-level minimum wages could potentially be a response to local price increases, it is arguably very unlikely that price developments in particular states cause adjustments in the federal minimum wage.

Finally, we also conduct a placebo test to test our inference and to see whether our results are spurious (e.g. due to misspecification). We repeatedly match all stores of a state with the minimum wage series of a random state. The match is drawn without replacement from a uniform distribution including the correct match. For each trial, we estimate the cumulative elasticity in the four months after legislation, E_4^{leg}, using equation 3.1.[18] We present the distribution of 1,000 estimated elasticities in Figure 2.7. Our baseline elasticity estimate of 0.02 lies

[18] A similar permutation test is proposed by Abadie et al. (2010) to conduct inference when applying synthetic control methods.

above *all* our placebo estimates. Furthermore, the placebo estimates are centered around zero. The permutation test suggests that our results are not driven by misspecification or structural breaks in the inflation series that correlate with temporal patterns of minimum wage increases. Moreover, the results suggest that our statistical inference is quite conservative.

D Heterogeneity of the price response

D.1 Time from legislation to implementation

One striking result from our baseline regressions is the high degree of anticipation of future cost increases in grocery stores' price setting. In order to illustrate the importance of anticipation effects further, we now look at events with different lead times between legislation and implementation of higher minimum wages.

In panel a of Figure 2.8, we split minimum wage laws into those that are followed by an increase in the minimum wage within less than a year and those with longer time between legislation and a first increase.[19] Increases resulting from indexation are excluded from this analysis. Pricing models with frictions would predict that adjustment is slower and more gradual for increases that are known long in advance, whereas adjustment should be quicker for increases that become known shortly before they are implemented. The figure indeed provides evidence that prices respond at legislation when implementation happens shortly after legislation, but not when implementation is at least a year out.

In panel b of Figure 2.8, we split increases in the minimum wage into those that follow within half a year after they were announced, and those that were announced earlier. The figure shows that grocery prices increase around implementation for those hikes that were legislated only shortly before the first increase in the minimum wage is implemented. In contrast, there are no price effects around implementation in the case of minimum wage hikes that are known

[19]There are 50 legislative events with a "short" and 12 with a "long" lead time between legislation and the first hike.

long in advance. Rather, there is some evidence for a disproportionate increase in prices in the months *before* the hike. The figure shows that the pre-trends at implementation—visible in our baseline results if we do not control for chain-time or division-time effects—are driven by minimum wage events that are known long in advance. If stores have enough time to anticipate the increase in cost, they appear to increase prices before their labor costs actually increase.

D.2 Store size, expensiveness, and chain

We now study the heterogeneity in the price response by several store characteristics, namely their size, their price level relative to other nearby stores, and what kind of retail brand they belong to. We split our sample in two groups along each of these dimensions. We reduce the length of the estimation window to 6 months before and after an event in order to reduce the number of coefficients estimated from these smaller samples. We present the results in Figure 2.9.[20] We focus on the effects at legislation, since the effects at implementation are statistically insignificantly different from zero in all these cases.

In Panel a of Figure 2.9, we first differentiate stores by size, measured by the average revenue of each store over the entire sample period.[21] We split the sample of stores into large and small stores at the median store size. We find larger and more precise point estimates for larger stores.

Next, we differentiate stores by their price level relative to other nearby stores. We use a procedure implemented by Coibion et al. (2015) to calculate expensiveness relative to other stores in a county.[22] The estimated effects are slightly larger for cheap stores and insignificant for expensive stores (Panel b of Figure

[20]Table A4, Table A5 and Table A6 in the appendix list the corresponding elasticities.

[21]The results are very similar if we use the number of products sold instead of revenue as a measure of store size (see Table A4).

[22]We first calculate the mean price during a year for each product and store. For each product, we then calculate the mean price in a county. We then calculate the deviation of each store from this price and aggregate deviations over all products sold in each store, weighted by the dollar revenue of the product. We only use products that are sold in at least 3 stores in a county and drop counties with less than 3 stores. Finally, we label stores that are on average more expensive than other stores in a county as expensive, and the remaining stores as cheap.

2.9). However, the difference in the response of the two groups of stores are not significant.[23]

Finally, Panel c of Figure 2.9 differentiates stores by the retail chain they belong to. We split chains into "national" and "regional" chains. Regional chains are those with stores in less than 5 distinct states on average, and "national" chains are those with stores in more states. We find larger and significant point estimates for regional chains. The results are consistent with the idea that in the case of regional chains the chain component of prices may be affected by local minimum wage increases.[24]

D.3 Elasticities of income-specific cost-of-living indices

In this section, we analyze differences in the price development of products that differ by their consumers' income. Heterogeneity along this dimension would be important from a distributional perspective. It could also suggest that demand shifts may play a role for the price response. The reason is that, if demand effects matter, we would expect that products consumed by poorer households would see the demand increase most, which in turn may result in an increase in prices.

We construct price indices for low-, medium- and high-income households, using a panel of household shopping data for about 5,000 households that accompanies the IRI data set. This panel allows us to calculate yearly expenditures for each UPC by household income. We pool households in three brackets of yearly income: less than $25,000, between $25,000 and $74,999 and more than $75,000. We then use expenditure shares of each UPC for a given bracket as weights to compute a cost-of-living price index for this bracket. Households in the panel are located in two metropolitan areas. We pool households in both areas and assume that their expenditure weights are representative for the US

[23] In Table A5 we show that the results are very similar for a measure of expensiveness relative to other stores in a state rather than a county.

[24] In Table A6 we also present results for chain size based on the number of stores rather than regional composition. Size is highly correlated with the national versus regional distinction, and we find that the effects are stronger in smaller chains.

overall. Furthermore, we average expenditure shares over all 10 years of data and keep weights constant in our index. Since we only observe expenditures for products bought by households in the panel, the cost-of-living indices cover a selected and smaller sample of products.[25]

The inflation rates of the resulting income-specific price indices are highly correlated. Consistent with the findings in Jaravel (2016), the average inflation rate is lower for products consumed by higher income households. In Table 2.7, we estimate our baseline specification for each index separately. All estimates are very close to our baseline estimates. The point estimates for the three indices are almost identical, and there are no significant differences between the response of price indices with expenditure weights for different income groups. This suggests that stores increase product prices across the board, and do not respond to demand shifts for specific products.

D.4 Right-to-work versus no-right-to-work states

Finally, we split our sample between states with and without so-called "right-to-work" (RTW) laws. RTW laws prohibit mandatory union membership for workers in unionized firms, and weaken the position of unions. Compared to states without RTW laws, states with such laws exhibit lower unionization rates, laxer labor market regulations in general, and wages in grocery stores tend to be lower. Addison et al. (2009) find that earnings in grocery stores are substantially more responsive to minimum wages in RTW states. Our own earnings regressions, presented in Table 2.8 and discussed in more detail below, also suggest that the minimum wage has more bite in grocery stores located in RTW states. Hence, one may expect grocery prices to be more sensitive to minimum wages in these states.

Our results, presented in Figure 2.10, are in line with this expectation. While

[25] Many products that are present in the store-level price data are sold to none or few households in our panel. There are two potential reasons for this. First, our sample is much smaller. Second, some products may not be sold in the locations of panel households.

the effect at legislation is of comparable magnitude in stores in RTW and non-RTW states, prices also increase substantially and statistically significantly at implementation in RTW states. In fact, stores in RTW states are the only subgroup analyzed in which we found evidence for a price effect at implementation, i.e. at the point in time when labor costs actually increase. Taking the effects at legislation and implementation together, the price effects of minimum wage hikes are substantially larger in RTW states.

5 The effect of minimum wage increases on cost

A A benchmark model

In this section, we estimate the impact of minimum wage increases on grocery stores' cost, and to which extent these cost increases are passed on into prices. We first clarify the assumptions required to measure marginal cost and consequently pass-through. To this end, we shortly review some basic facts on factor price elasticities of firms marginal costs and output prices.

We start out with a simple theoretical framework featuring Cobb-Douglas production, CES demand and monopolistic competition outlined in appendix D. Firms employ two types of workers—skilled and unskilled (the latter are assumed to earn the minimum wage). This model yields two basic theoretical insights: First, full pass-through means that the minimum wage elasticity of prices is equal to the minimum wage elasticity of marginal cost. Second, the minimum wage elasticity of marginal cost in the model is equal to the cost share of minimum wage workers and could consequently be measured as such.[26]

A practical complication is that it is unclear how minimum wage workers should be best defined. The empirical wage distribution is continuous, and recent research (Dube et al., 2015; Autor et al., 2016) suggests that workers earning wages

[26]See also Silberberg (1974) or Wohlgenant (2012) for further details for this case with constant returns to scale.

above the minimum wage also benefit from minimum wage hikes to some degree. For instance, table A8 in the appendix illustrates that our results on the minimum wage share of groceries' costs would depend substantially on the wage cutoff we use to define minimum wage workers.

In order to accommodate this issue, we generalize our theoretical framework. We assume that grocery stores provide retail services using a production technology $F(L, X)$, where F is homogeneous to some degree—including the possibility of non-constant returns to scale. X denotes the quantity of purchased merchandise. L is a composite input defined by a linear homogeneous aggregator over N different types of labor inputs $L_1, L_2, \ldots, L_N$ with wages $w_1, w_2, \ldots w_N$. The wages of these different types of workers may be affected by minimum wages differently. We assume monopolistic competition in product markets and competitive labor markets[27].

Under these assumptions, the minimum wage elasticity of prices and marginal cost at constant output equals (see appendix D):

$$\frac{\partial P}{\partial MW}\frac{MW}{P} = \frac{\partial MC}{\partial MW}\frac{MW}{MC} = \frac{\overline{W}L}{C} \cdot \frac{\partial \overline{W}}{\partial MW}\frac{MW}{\overline{W}} \tag{5.1}$$

Equation 5.1 suggests that we can estimate minimum wage elasticity of grocery stores' cost as the product of two factors: (i) the labor share in cost and (ii) the minimum wage elasticity of the average wage $\overline{W}$. Based on sectoral balance sheet data, we estimate in section B that the labor cost share of grocery stores is about 0.16. We estimate the minimum wage elasticity of average wages in the following section.

[27]We make this assumption because our evidence for positive price effects and no employment effects of minimum wages is generally inconsistent with monopsonistic labor markets (Aaronson et al., 2008). Monopsonistic labor markets have been brought forward as an explanation why minimum wages have limited effects on employment (Card and Krueger, 1995; Stigler, 1946). Our assumptions and our results are compatible with small or no disemployment effects if low-skilled labor is difficult to substitute with other factors—at least in the short run—and full price pass-through has small or no effects on sectoral output.

B The minimum wage elasticity of groceries' labor costs

In this section, we estimate the minimum wage elasticity of average earnings of grocery store workers using quarterly county-level data from the QCEW. We calculate average earnings as the ratio of total earnings of grocery store workers and grocery store employment. Throughout this section, we equate the elasticity of average earnings with the elasticity of the average wage. The two will be equal if there are no negative effects on employment and hours of low-wage workers. In the case of negative employment effects, the earnings elasticity will underestimate the wage elasticity. However, we do not find evidence for negative employment effects. We restrict the data to the states and time period included in our price regressions. We then estimate standard state-level two-way fixed effects regressions that are often used to estimate minimum wage effects on employment in the US (see Allegretto et al., 2017, for a critical assessment):

$$\log \overline{W}_{c,q} = \gamma_c + \delta_q + \beta \log MW_{c(s),q} + Controls_{c,q} + \epsilon_{c,q} \tag{5.2}$$

Table 2.8 shows that we find significant positive effects of minimum wages on average earnings with this specification. This is also true if we control for state-specific linear time trends—an important sensitivity check for the two-way fixed effects model in the minimum wage context (Allegretto et al., 2017). Moreover, as we show in appendix E, the elasticity of earnings in grocery stores increases with the bindingness of a minimum wage hike.

Our baseline estimate for the labor cost elasticity in grocery stores is 0.11. This is in line with what other papers have found[28], only slightly smaller than our estimate for the accommodation and food service industry, and larger than for retail trade as a whole (see columns 3–6 of Table 2.8). We present one further

[28]Our baseline labor cost elasticities are somewhat smaller than the elasticities for the US retail sector estimated in Sabia (2009) using CPS wage data. They are larger than those estimated in Addison et al. (2009) for the 1990–2005 period. Our estimates are similar to those reported in Leung (2017) and Ganapati and Weaver (2017), who also use QCEW data for a similar time period.

interesting result in appendix E. When studying the dynamics of the wage effects by including leads and lags of the minimum wage to the regression, we find that the earnings effect of the minimum wage hike is concentrated in the quarter when the hike is implemented. The response of prices at legislation thus reflects an anticipation of future wage increases, rather than premature compliance with future minimum wage laws.

In Panel B of Table 2.8, we use the two-way fixed effects model to examine whether minimum wages affect employment in grocery stores. The regressions provide no evidence for negative employment effects. In fact, when we account for linear state trends, the estimated elasticity of grocery store employment is significantly positive.[29] Panel C of Table 2.8 also tests whether the number of establishments in a county is related to the prevailing minimum wage, but we find no statistically significant effect in any of the three industries.

C Pass-through

The combined estimates of the labor cost share and the minimum wage elasticities of the average wage allow us to compute pass-through rates using equation 5.1. Our baseline point estimate for the elasticity of cost is $0.16 \cdot 0.11 = 0.018$. We compute pass-through rates by dividing the price elasticity at legislation by the estimated cost elasticity. The results are shown in Table 2.9. Our estimate for pass-through based on our baseline specification amounts to 1.1. We cannot reject the hypothesis that pass-through is equal to 1—the p-value on the test is 0.78. If we base the pass-through ratio on our estimates including division-time or chain-time fixed effects, we get values of 1.17 (p-value: 0.53) and 0.84 (p-value: 0.65).

In the lower part of the table, we also incorporate the insignificant point estimates of price effects at implementation into the computation of pass-through

[29] This evidence replicates earlier results from Addison et al. (2009) who also use county-level QCEW data but focus on the 1990–2005 period.

rates, using the results from the joint estimation in columns 7–9 of Table 2.4. In this case, the estimated pass-through is larger if we do not account for division-time or chain-time effects, but still not significantly different from 1. We thus cannot reject the hypothesis of full pass-through for any of the estimated specifications.

These first two pass-through rates do not take into account that minimum wages may also increase the cost of goods sold (COGS) in grocery stores. The producer prices of groceries may increase after a minimum wage increase, since minimum wage workers are employed in the production of grocery products, too. Due to the high share of COGS in retailers' cost—as shown in Table 2.3, COGS make up about 83% of grocery stores' variable cost—even a relatively minor increase in producer prices could affect retail prices. Moreover, retail stores have been shown to be very responsive to changes in COGS (Eichenbaum et al., 2011; Nakamura and Zerom, 2010).

The extent to which increases in COGS may be reflected in our price estimates depends, first, on whether prices of COGS increase after state-wide minimum wage hikes. Since we cannot estimate the impact of minimum wages on the wholesale cost of grocery products directly in our data, we calculate an upper bound for this effect. To this end, we use input-output tables and assume full pass-through of increases in labor costs into prices all along the production chain for each of the sectors producing groceries, similar as in MaCurdy (2015). Further assuming that all workers earning 110% (130%) of the minimum wage are affected by the minimum wage, we predict that a 10% increase in the minimum wage would increase the prices of COGS by 0.018% (0.025%) (see appendix appendix F). Full pass-through in industries producing groceries could affect the marginal costs of grocery stores to a comparable magnitude as the direct effect through increased labor costs.

Whether such a possible price effect on COGS would be captured by our price estimates depends, however, also on the extent to which the increases in the prices of COGS occur locally. If wholesale groceries are perfectly tradeable, a

minimum wage hike would increase COGS equally for stores everywhere, and any pass-through of this cost increase would be absorbed in time fixed effects in our baseline estimation. In appendix F, we thus study the origin composition of groceries sold in a state. Using grocery wholesale-to-retail flows reported in the Commodity Flow Survey, we find evidence that a substantial share of grocery products are delivered by wholesalers located in the same state or census division as the retailers they supply. As a consequence, our baseline estimates may indeed partly capture pass-through of increases in COGS if minimum wages affect prices of COGS. Hence, they likely represent a good approximation to the overall effect of minimum wages on grocery prices. However, another implication would be that the baseline pass-through rates discussed so far are biased upward.

We therefore provide pass-through rates in the lower part of Table 2.9 that incorporate our estimate of the upper bound minimum wage elasticity of the cost of COGS of 0.025. We assume that the major part of the price effect occurs in the state in which the minimum wage occurs. Obviously, incorporating the additional cost effects through COGS lowers the calculated pass-through rates. In case of our baseline specification that only includes effects at legislation (column 1), the pass-through rate falls to 0.6. The pass-through rate is 1.1 in our baseline specification that incorporates the sum of effects at implementation and announcement. The pass-through falls below one in both models if we control for chain-time fixed effects (column 3). However, we do not see this as evidence against full pass-through. The time fixed effects in the baseline specification and especially the division-time and chain-time effects in columns 2 and 3 likely absorb at least part of the price effects of COGS already, so that incorporating this additional cost effect leads to a lower bound on the pass-through rate (particularly in columns 2 and 3). For this reason, and because we use an upper bound estimate of the effect of COGS on prices to construct the rates, the estimated pass-through rates in the lower part of Table 2.9 are likely to be biased downward.

D Demand increases

So far, we have treated minimum wage increases as a cost shock to grocery stores. However, minimum wages also raise the incomes of low-wage workers, which may affect the demand for groceries. This demand may in turn also elicit a response of grocery prices. This view has been advocated in Leung (2017) and Alonso (2016), who find a positive impact of minimum wages on real grocery store revenues. In contrast, Aaronson et al. (2012) find no evidence for an impact of minimum wages on consumption of nondurables and services in households with minimum wage earners. Our results, presented in Table A16 in the appendix, also suggest that minimum wages do not affect grocery consumption. Using our baseline regression model, we find no effect of minimum wages on quantities sold at or and revenues of grocery stores, neither at legislation nor at implementation.

Even if minimum wages affected the grocery demand for households with low-wage workers, there are a priori good reasons to be skeptical that minimum wage hikes lead to a shift in market demand that would have a quantitatively important effect on prices. To see this, note that the role of demand in the price response to minimum wage increases is determined by three factors: First, minimum wages need to have a substantial effect on local *aggregate* incomes. Second, the market demand for groceries has to be responsive to changes in aggregate incomes. Third, grocery stores' prices have to be responsive to changes in demand.

We expect rather small effects of minimum wages in all three dimensions. First, Dube (2017) shows that minimum wages increase incomes of low-income families with an elasticity of up to 0.5 after two years. He finds effects on incomes up to the 15th percentile of family incomes. However, these families account for less than 2% of total incomes (in the 2011 March CPS). The elasticity of aggregate incomes would thus be at the order of $0.5 \cdot 0.02 = 0.01$. Second, the magnitude of the shift in individual demand associated with increasing income depends on the

income elasticity of grocery demand. Products sold in grocery stores are typically necessities with income elasticities below one (see, e.g., Banks et al., 1997; Lewbel and Pendakur, 2009; Okrent and Alston, 2012). Any shift in individual demand is thus likely to be smaller than the underlying increase in income. Third and most importantly, existing estimates of grocery stores' supply curve suggest that prices are unresponsive to changes in demand in the short run, even in the face of very large demand shifts (Chevalier et al., 2003; Gagnon and Lopez-Salido, 2014; Cavallo et al., 2014).

6 The impact of price increases in grocery stores on household welfare

Our results suggest that consumers rather than firms bear the cost of minimum wage hikes. We now discuss the welfare impact of raising grocery prices for households in different brackets of the income distribution. To put these magnitudes in perspective, we also compare them with the expected gains through higher wages. It is important to note that our analysis is partial and does not take into account any other potential costs and benefits of minimum wage hikes, most importantly the price response in other sectors that employ minimum wage workers.

We illustrate static welfare gains and losses based on a hypothetical increase of all binding minimum wages in the US by 20%. Our preferred estimates predict that this would trigger a price increase in groceries by 0.4%.

We compare the predicted gain in nominal incomes with the Equivalent Variation of the grocery price caused by such a hike. The Equivalent Variation is a first order approximation to the welfare cost of a price change, measured in US dollars. It assumes that households maximize utility and abstracts from second order effects reflecting the response to changes in relative prices. In particular,

the overall dollar value of the welfare gain of a household can be expressed as:

$$\Delta U_h^{USD} = \Delta Y_h - \sum_j E_{h,j} \Delta P_j \tag{6.1}$$

Here, ΔY_h denotes the mean USD increase in household incomes in income bracket h, E_{hj} denotes mean household expenditure for goods sold in sector j and ΔP_j denotes the price change in sector j. The product $E_{hj}\Delta P_j$ represents the Equivalent Variation of a price change in sector j.

A The cost of price increases

We first discuss the welfare cost of the minimum wage hike, i.e. the Equivalent Variation of price increases. We use expenditure data by income bracket provided in the Consumer Expenditure Survey (CES) and include expenditures for the CES categories Food at Home, Personal Care Products and Services, Household Supplies and Alcoholic Beverages as groceries. As shown in Section D.3 we do not find differences in the response of cost-of-living indices for different brackets, so we use our baseline elasticity of 0.02 for all brackets.

Figure 2.11a presents the costs of price increases caused by minimum wage hikes, measured in US dollars and relative to household incomes. The dollar value of costs is increasing in household incomes. Since groceries are not an inferior good, this is to be expected. For households with incomes below \$10,000, the annual costs amounts to about \$13. The costs increase up to \$43 for households with incomes above \$150,000. Expressing the costs as a percentage of annual household incomes reveals the regressive impact of the price response. The costs make up about 0.2% of annual income for households in the poorest bracket, and just one tenth of that, i.e. 0.02% for households in the richest bracket.

B The benefits of nominal wage increases

We now discuss how the costs of the price response relate to the first order effect of increasing nominal incomes for each household income bracket. We predict the mean increase in household incomes ΔY_h for each income bracket based on the March 2011 joint distribution of wages, hours worked per week, and weeks worked during the last year. Throughout this exercise, we assume that minimum wage increases have no effect on employment. The welfare effects are thus based on an upper bound on the benefit side, and would be lower if employment effects were negative.

We use the wage and weekly hours distribution during March 2011 available for the CPS monthly outgoing rotation group (MORG). We combine the MORG with the CPS Annual Socioeconomic supplement (ASEC) collected each March, which contains information on annual Household incomes and the number of weeks worked during the previous year. For every person i in the MORG, we calculate the distance to the local binding minimum wage $W_i/MW_{s(i)}$. We then construct a counterfactual labor income as follows:

$$\widehat{Y}_i^L = \begin{cases} W_i \cdot 1.2 \cdot hours_i \cdot weeks_i, & \text{if } \frac{W_i}{MW_{s(i)}} \leq 1.1 \\ W_i \cdot \left(1 + 0.2 \frac{1.3 - \frac{W_i}{MW_{s(i)}}}{1.3-1.1}\right) \cdot hours_i \cdot weeks_i, & \text{if } 1.1 < \frac{W_i}{MW_{s(i)}} < 1.3 \\ W_i \cdot hours_i \cdot weeks_i, & \text{if } \frac{W_i}{MW_{s(i)}} \geq 1.3 \end{cases} \quad (6.2)$$

This calculation assumes that wages below 1.1 times the local minimum wage are increased by 20%, and that wages between 1.1 and 1.3 times the local minimum wage increase by a linearly declining factor. This is in line with ripple effects documented in Dube et al. (2015). We calculate the predicted increase in labor income $\Delta Y_i^L = \widehat{Y}_i^L - Y_i^L$ for each individual. We then sum the increase over all household members. Finally, we calculate the average predicted increase in household income for each income bracket using the ASEC household sampling

weights.

Figure 2.11b presents the predicted increase in nominal incomes in US dollars and in percent of household income as the full length of the respective bars. The distribution of the gains expressed in US dollars may seem surprising at first.[30] In absolute terms, the poorest households gain relatively little compared to other brackets. Their annual incomes go up by about $136 and the biggest nominal benefits accrue to middle class households with incomes between $50,000 and $79,000, who gain about $565. This can be explained by low labor supply in the poorest bracket.[31] Second, households in the richest bracket still gain substantially. Minimum wage workers in this bracket differ from those in poorer households in one important aspect. As shown in Table A14 in the appendix, 71% of minimum wage workers in the richest bracket are children of the CPS household reference person, compared to around 10% in poorer brackets. Relative to household incomes, however, gains are distributed in a more progressive way: the poorest households gain 2.2% of their annual incomes, middle class households 1%, and the richest households gain 0.15%. Figure 2.11b also illustrates the part of nominal gains that is offset by the Equivalent variation of price increases, which we discuss in more detail in the next subsection.

C Comparing cost and benefits

Figure 2.12 shows the Equivalent Variation as a percentage of nominal gains to illustrate how much of the nominal gains are offset by the Equivalent Variation of price increases. For the poorest households, the price response in grocery stores offsets 9.8% of the nominal gains. This is arguably a small but non-

[30] Dube (2017) estimates the impact of minimum wage increases on family incomes at different percentiles. The range of his reported estimates is quite large and the magnitudes depend on the included controls. He also finds that the poorest families gain less than slightly less poor families. Overall, our predictions for different income brackets are within the range of his estimates.

[31] Table A13 in the Appendix illustrates that households in the lowest bracket work about 5 hours a week and 7 weeks a year on average, and as a result, labor is a relatively minor source of income.

negligible effect. The impact of price increases is very small for slightly less poor households with higher labor supply. In households with annual incomes between $10,000 and $79,000, only 3–4% of their nominal gains are offset by the price response. For the richer households the percentage rises again and goes to up to 12.8% for the richest bracket. In the right panel of Figure 2.12, we also take into account price increases in restaurants for comparison. We use a minimum wage elasticity of restaurant prices of 0.07 estimated in Aaronson (2001) and expenditures for "Food Away from Home" in the CES to calculate the Equivalent Variation. The calculations suggests that price responses in restaurants matter regarding the gains from minimum wage increases. The effects are largest for the richest households (almost 40%). In the poorest households, the Equivalent Variation now offsets 20.8% of nominal gains.

The price response mechanically reduces the nominal gains for all households. Moreover, due to differences in expenditures for groceries, the price response not only affects the level, but also the distribution of gains over different income brackets. To separately analyze the redistributive effect of minimum wage increases, we compare the distribution of gains to an inequality neutral income subsidy. In particular, we decompose gains for each income bracket as follows:

$$\frac{\widehat{Y}_h^L - Y_h^L - E_h \Delta P}{Y_h} = (1 + g + s_h) \tag{6.3}$$

In this decomposition, we choose the level of the inequality neutral subsidy g to equal the overall increase in labor incomes, $\sum_i \hat{Y}_h^L - Y_H^L = (1+g)\sum_i Y_i$. We then calculate s_h for each bracket. These bracket-specific subsidies s_h measure the extent to which a minimum wage increase is redistributive. We calculate g and s_h for three measures of gains: for the initial nominal gains, for the gains taking into account price increases in grocery stores, and for the gains taking into account price increases in grocery stores and restaurants.

Figure 2.13 presents the bracket specific subsidies. As expected, minimum

wages reduce income inequality. The impact on inequality is largest for the purely nominal gains. Taking into account the price response reduces the redistributive impact. In terms of nominal gains, households in the poorest bracket gain an additional 1.5% of household income over an inequality neutral policy. Taking into account the price response in grocery stores reduces the additional gains to 1.34%. Further taking into account restaurants reduces the gains to 1.15%. For less poor households, the price response has a smaller impact on redistribution. Households that earn above $80,000 gain less from a minimum wage increase than they would from an inequality neutral policy. Taken together, these results suggest that price responses in groceries reduce the redistributive effects of minimum wage policies, but they do not offset them.

7 Conclusion

In this paper, we study the effects of minimum wage increases on prices in grocery stores. We use scanner data to analyze the response to 166 minimum wage increases and 62 legislative events in the US from 2001 to 2012.

Our findings can be summarized by three key results. First, the minimum wage elasticity of prices is about 0.02. We estimate the minimum wage elasticity of cost as well, and find that this elasticity is roughly the same size. Our results are consistent with a full pass-through of cost increases to consumers. Second, we find that the response to minimum wage increases happens around the time of passage of legislation, rather than at the time of implementation of hikes. This result suggests that grocery stores set their prices in a forward-looking manner and confirms a key prediction of macroeconomic models with nominal frictions. Third, we show that the price response of grocery stores affect the poorest households the most, but that these price increases offset only about 10% of the average nominal gains from minimum wage increases for households in this bracket. As a consequence, minimum wage hikes remain a redistributive

policy even after accounting for price effects in grocery stores.

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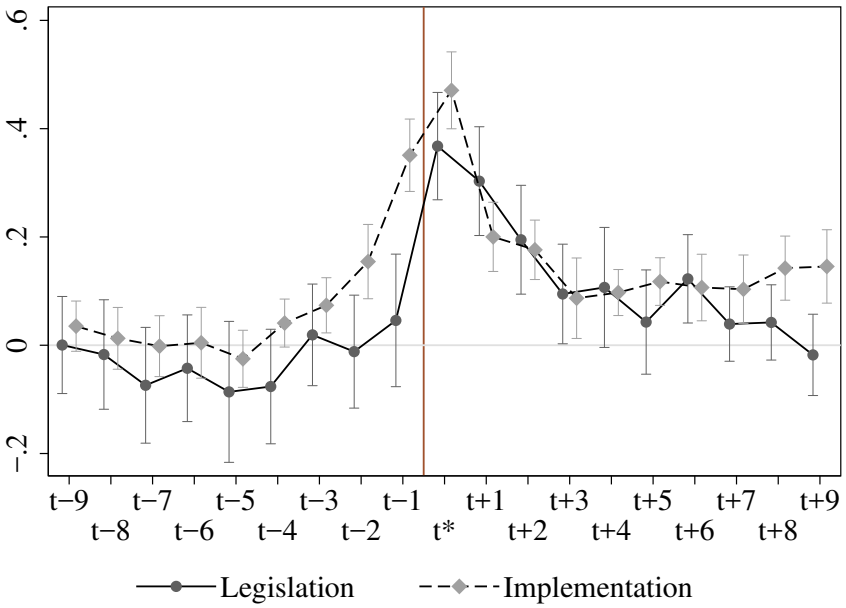
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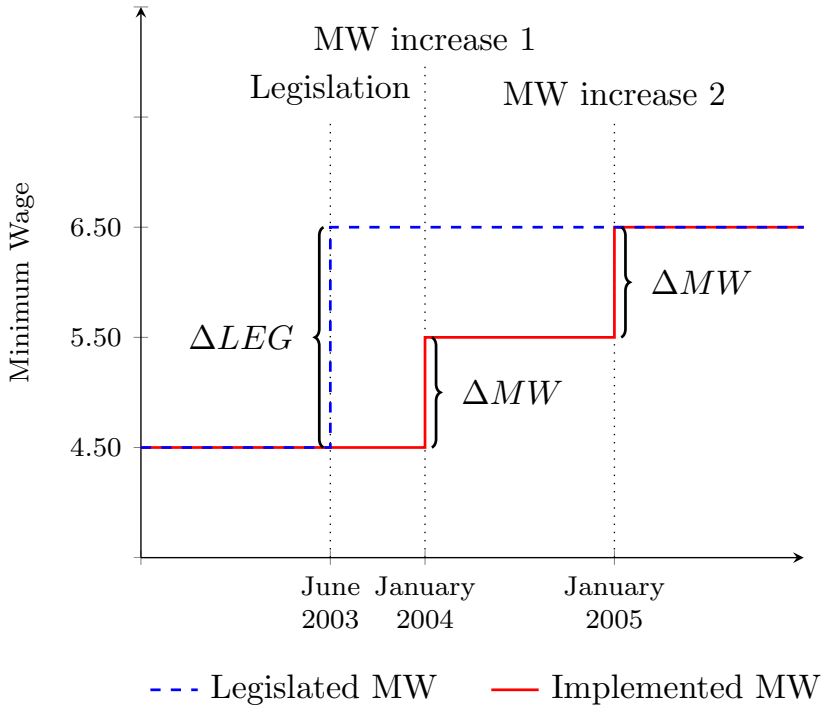
Figure 2.1 – Google search volume for "minimum wage *statename*" around legislation and implementation of minimum wage increases



Notes: The figure shows the log change in monthly Google search volume for the search term "Minimum wage+*statename*" around changes in minimum wage legislation and implementation of higher minimum wages in state *statename*. The coefficients are estimated from equation 2.1. The effects are relative to state and time fixed effects. Note that the search terms differ between states, but measured search volume is for United States as a whole.

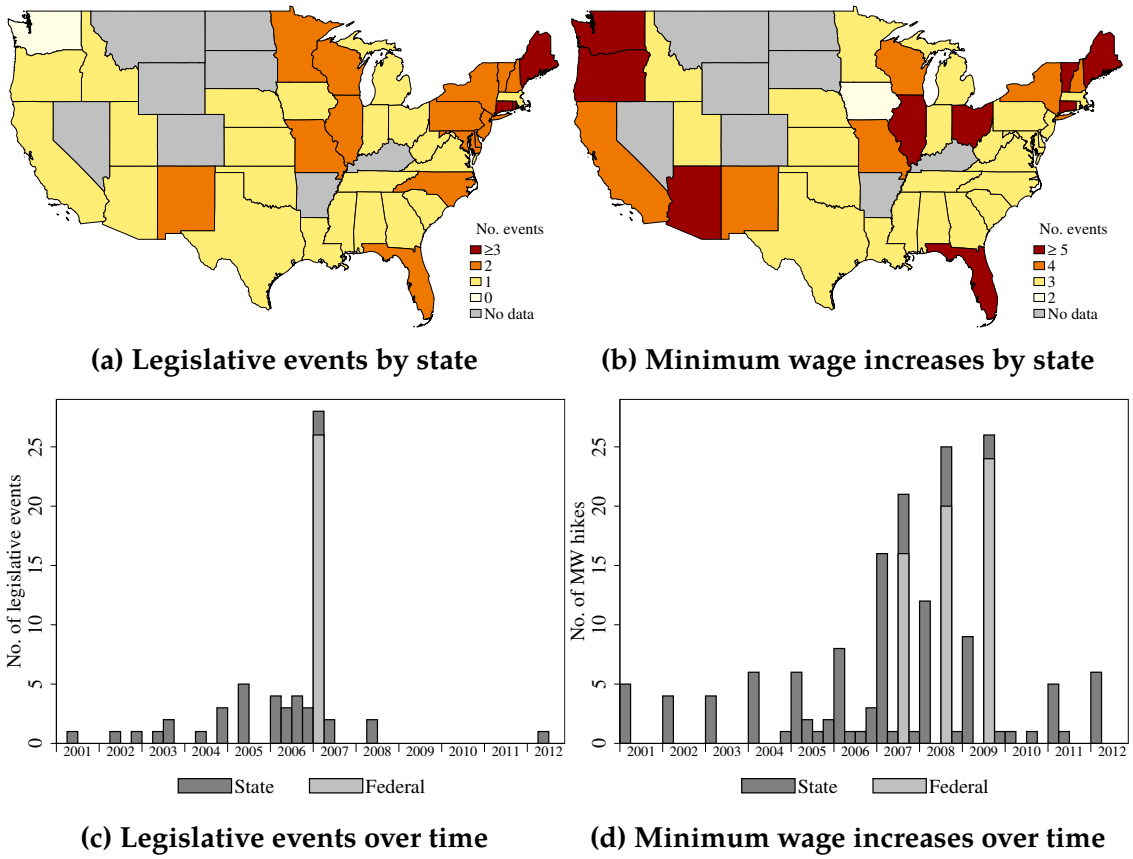
Table A2 in the appendix contains further robustness checks. The table shows that our results are robust to using only stores that we observe throughout the whole sample period (and hence are not driven by stores' entry or exit); to controlling for county level trends in the inflation rate; to changing the event window to $k = \pm 6$ or $k = \pm 12$ months; and to excluding the Great Recession. Our results also remain unchanged if we only look at the effects of the first minimum wage hike in each state in our sample period, which represents an alternative method to address the fact that all states are treated multiple times in the sample period.

Figure 2.2 – Example for the measurement of changes in the legislated minimum wages



Notes: The figure illustrates the measurement of changes in the legislated and implemented minimum wage based on an hypothetical minimum wage increase in two steps. In June 2003, legislation is passed that will increase the minimum wage in from an initial value of \$4.50 to \$6.50. The law schedules an increase to 5.50 in January 2004, and to 6.50 in January 2005. Our measure of the legislated minimum wage is equal to 4.50 before June 2003. It increases to 6.50 when the legislation is passed in June 2003. Before June 2003 and after January 2005 the legislated minimum wage is equal to the implemented minimum wage.

Figure 2.3 – Distribution of minimum wage hikes and legislative events over time and states

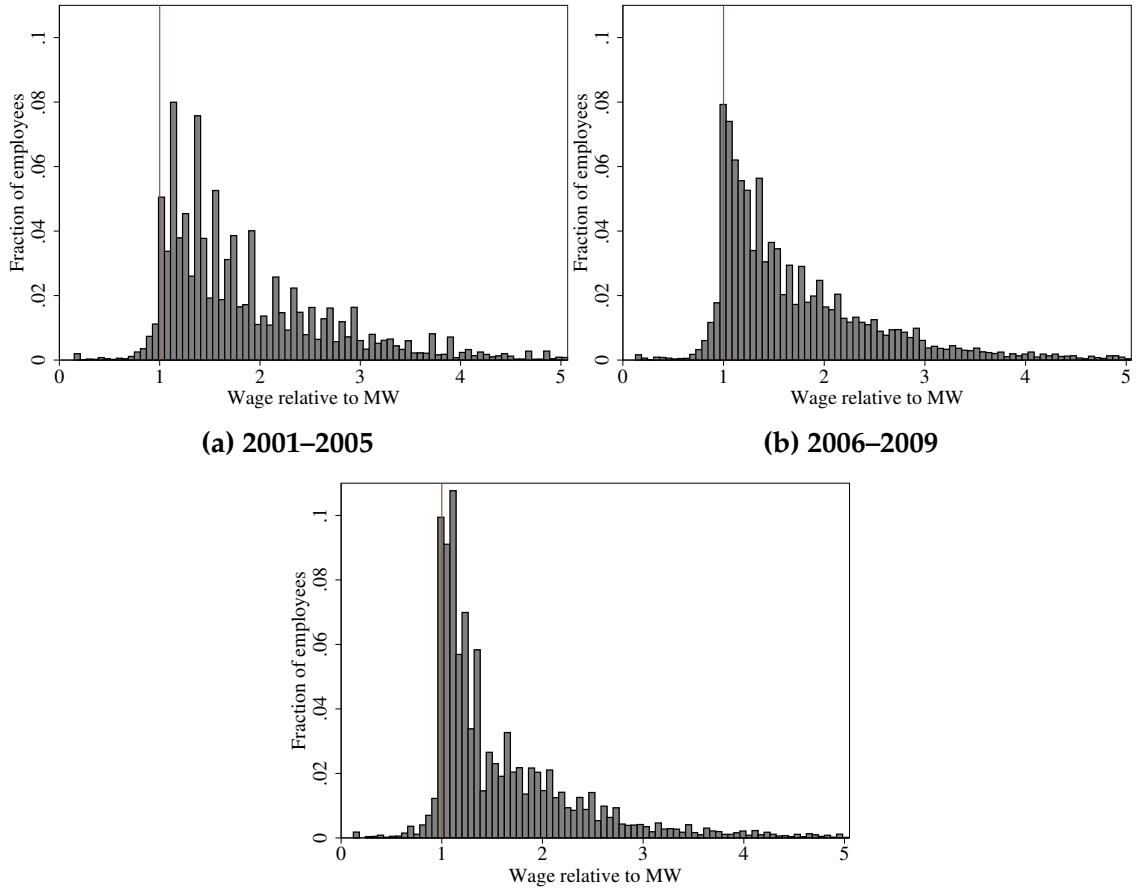


(a) Legislative events by state **(b) Minimum wage increases by state**

(c) Legislative events over time **(d) Minimum wage increases over time**

Notes: The figure illustrates the distribution of changes in the implemented minimum wage and changes in the legislated minimum wage over time and states. Overall, we observe 166 increases in the implemented minimum wage and 62 legislative events from 2001 to 2012. 60 changes in the implemented minimum wage and 26 changes in the legislated minimum wage follow from federal minimum wage policy. The remainder follows from state-level policies.

Figure 2.4 – The wage distribution in grocery stores relative to local minimum wages



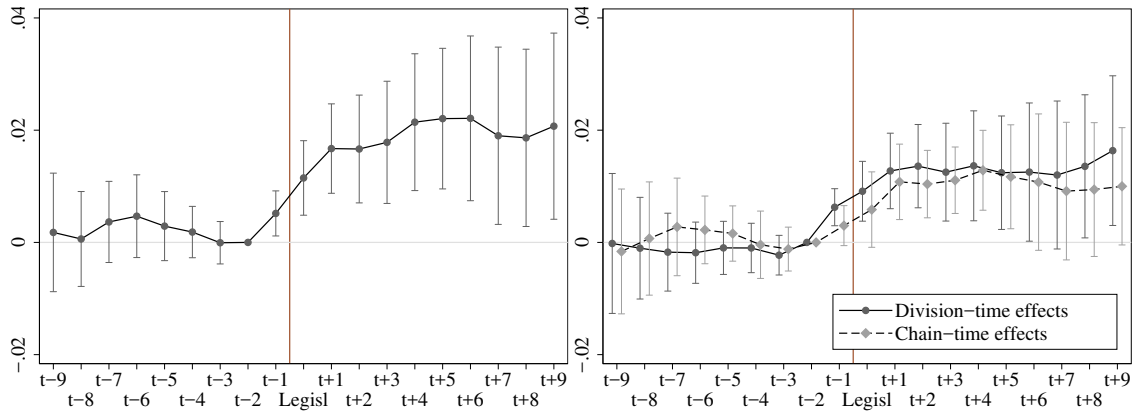
(a) 2001–2005

(b) 2006–2009

(c) 2010–2012

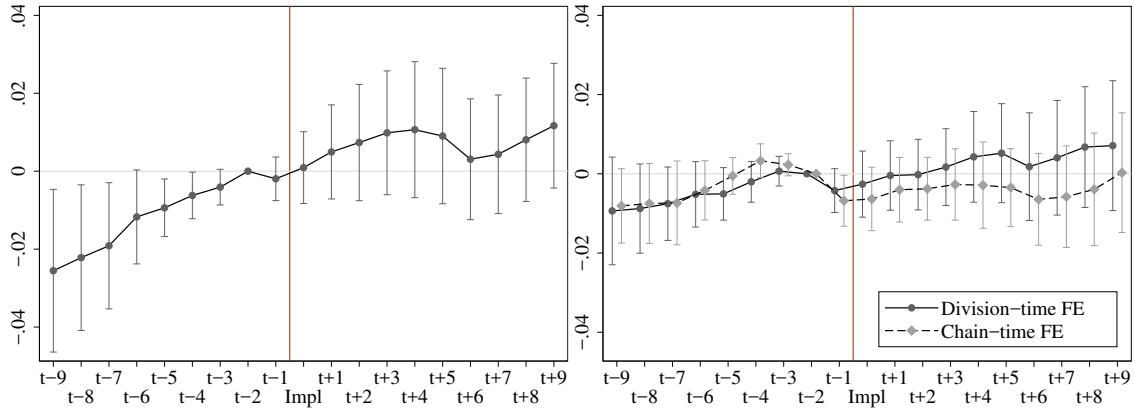
Notes: The figure illustrates the wage distribution in grocery stores relative to local minimum wages. It is based on CPS MORG data for the sector "grocery stores" (NAICS 4451). Wages are computed using reported hourly wages for workers paid by the hour, and weekly earnings divided by weekly hours for other workers. All observations are pooled for the indicated periods. Distributions are calculated using CPS earnings weights. Wages below the local minimum may correspond to workers exempted from minimum wage laws (for example full-time students, workers with disabilities) or measurement error in the CPS survey.

Figure 2.5 – Cumulative minimum wage elasticities of prices from separate estimation



(a) Legislation: Baseline specification

(b) Legislation: Controlling for chain-time or division-time effects

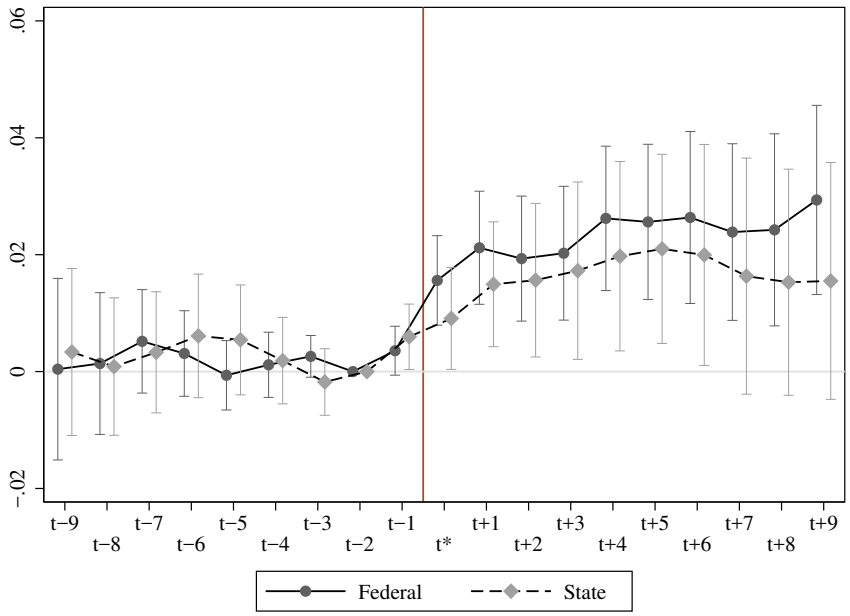


(c) Implementation: Baseline specification

(d) Implementation: Controlling for chain-time or division-time effects

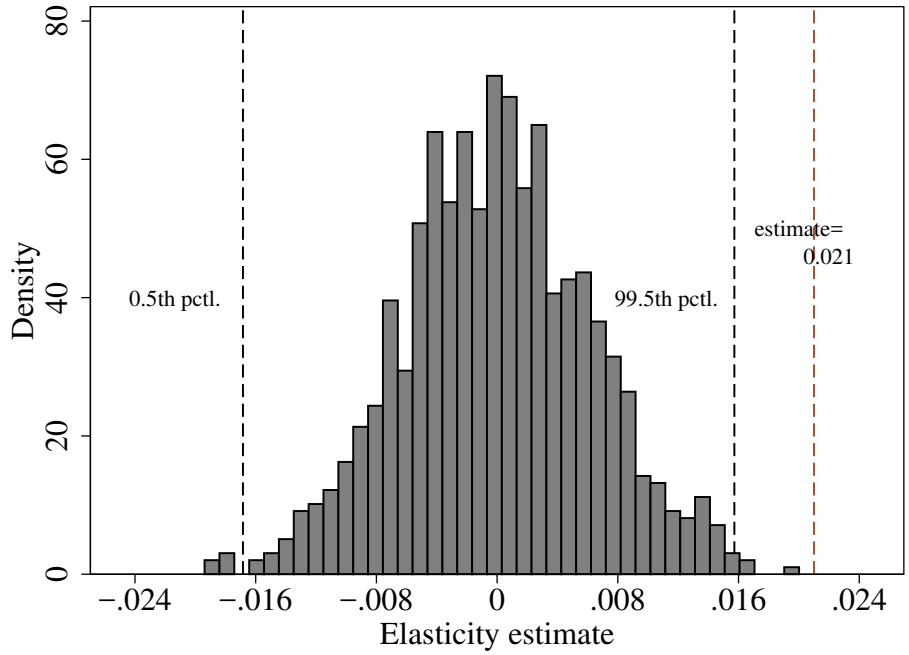
Notes: The figures present the cumulative minimum wage elasticity of prices at grocery stores. Effects at legislation and implementation are estimated separately. Panels (a) and (c) show the cumulative elasticities at legislation and implementation estimated from the separate baseline specifications. Panels (b) and (d) show elasticities estimated controlling for chain-time or division-time effects. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also present 90% confidence intervals of these sums based on SE clustered at the state level.

Figure 2.6 – Cumulative minimum wage elasticities of prices around federal- and state-level minimum wage legislation



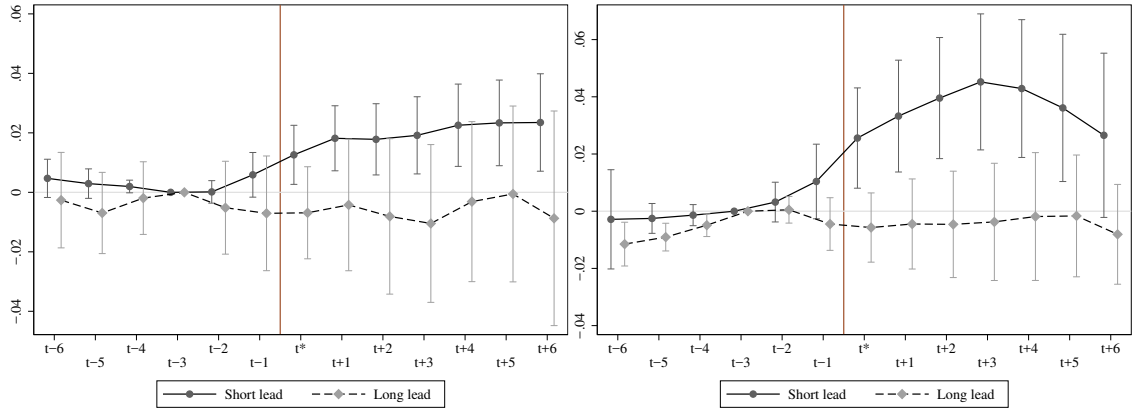
Notes: The figure presents the cumulative minimum wage elasticity of prices at grocery stores around federal and state-level minimum wage legislation. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also present 90% confidence intervals of these sums based on SE clustered at the state level.

Figure 2.7 – Placebo test



Notes: The figure presents the results of a placebo test in which we match all stores in a state with a random state's minimum wage series. Draws are without replacement and include the correct match. The histogram shows the distribution of elasticity estimates at legislation over 1000 randomly matched samples. The mean elasticity estimate is -0.00003. Our baseline estimate of the elasticity at legislation is 0.021 and clearly outside the suggested 99% confidence interval.

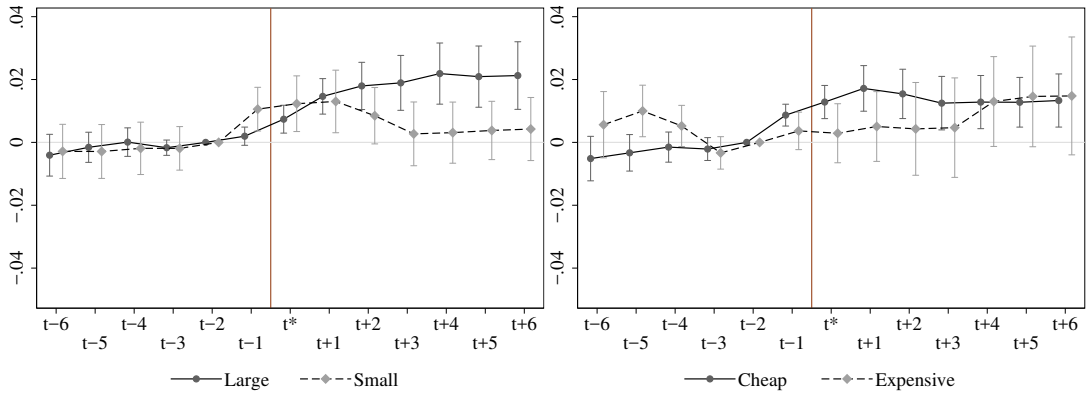
Figure 2.8 – Cumulative minimum wage elasticities of prices for events with different timing



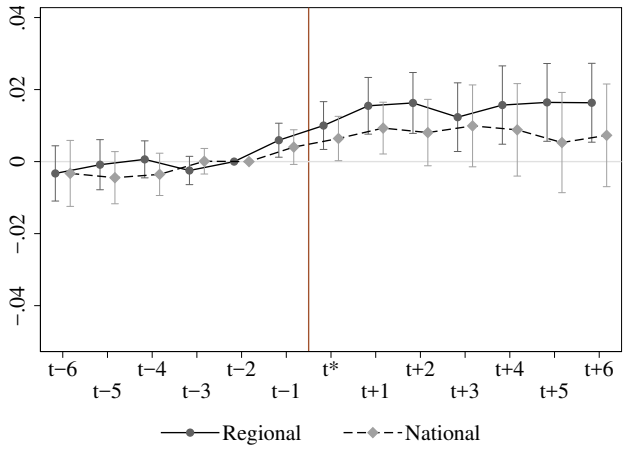
(a) Effects at legislation **(b) Effects at implementation**

Notes: The figure presents the cumulative minimum wage elasticity of prices at grocery stores. Panel (a) shows the effects at legislation for legislation that is followed by implementation of a first increase in less than a year ("short lead") and legislation that is implemented further in the future ("long lead"). Panel (b) shows the effects at implementation for increases that are preceded by legislation within less than half a year ("short lead") and those whose legislation lies further in the past ("long lead"). The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also show 90% confidence intervals of these sums based on SE clustered at the state level.

Figure 2.9 – Cumulative minimum wage elasticities of prices by store characteristics



(a) Effects at legislation by store size **(b) Effects at legislation by expensiveness**



(c) Effects at legislation by chain size

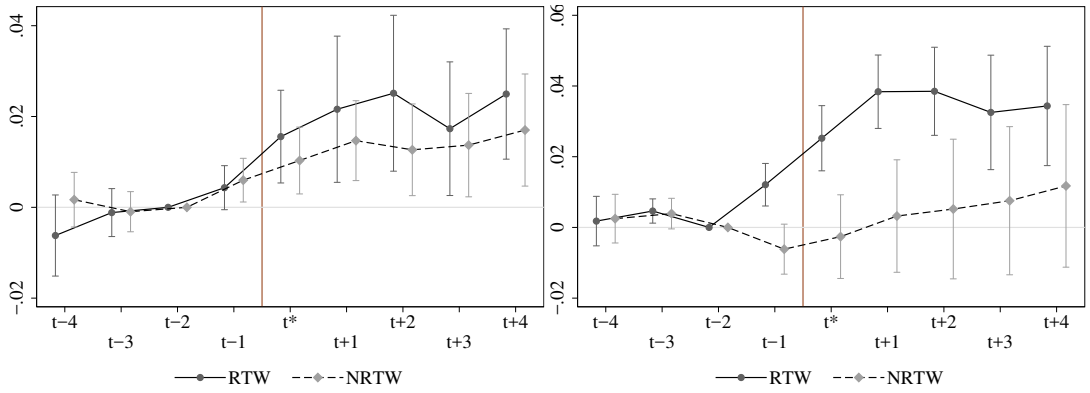
Notes: The figures present the cumulative minimum wage elasticity of prices at grocery stores by several heterogeneity dimensions. The effects shown in the panels are estimated jointly from equation 3.1, where we include the full set of leads and lags for both dimensions of heterogeneity. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also show 90% confidence intervals of these sums based on SE clustered at the state×dimension-of-heterogeneity level.

Table 2.1 – Summary statistics for minimum wage increases and minimum wage legislation

	Changes in implemented MW		Changes in legislation	
	Mean	SD	Mean	SD
Log size of increase	0.0816	(0.0560)	0.201	(0.116)
Events per state	4.049	(1.974)	1.512	(0.746)
Months to last event	13.86	(7.028)	23.32	(16.76)
Months hike to legislation / legislation to first hike	15.65	(9.823)	8.742	(8.014)
Share federal hike	0.361	(0.482)	0.419	(0.497)
Share indexed hike	0.235	(0.425)		
Share 2001–2005	0.157	(0.365)	0.242	(0.432)
Share 2006–2008	0.542	(0.500)	0.742	(0.441)
Share 2009–2012	0.301	(0.460)	0.0161	(0.127)
Share January	0.458	(0.500)	0.452	(0.502)
Share July	0.434	(0.497)	0.0484	(0.216)
Number of Events	166		62	

Notes: The table lists descriptive statistics for our two main exogenous variables: Changes in implemented and legislated minimum wages. The legislated minimum wage is the highest future minimum wage set in current law. The data on state-level binding minimum wages is a combination of data from the Tax Policy Center, the US Department of Labor, and state departments of labor. We collected data on legislative events ourselves from media sources and legislative records.

Figure 2.10 – Effects for stores in RTW and non-RTW states



(a) Elasticity at legislation **(b) Elasticity at implementation**

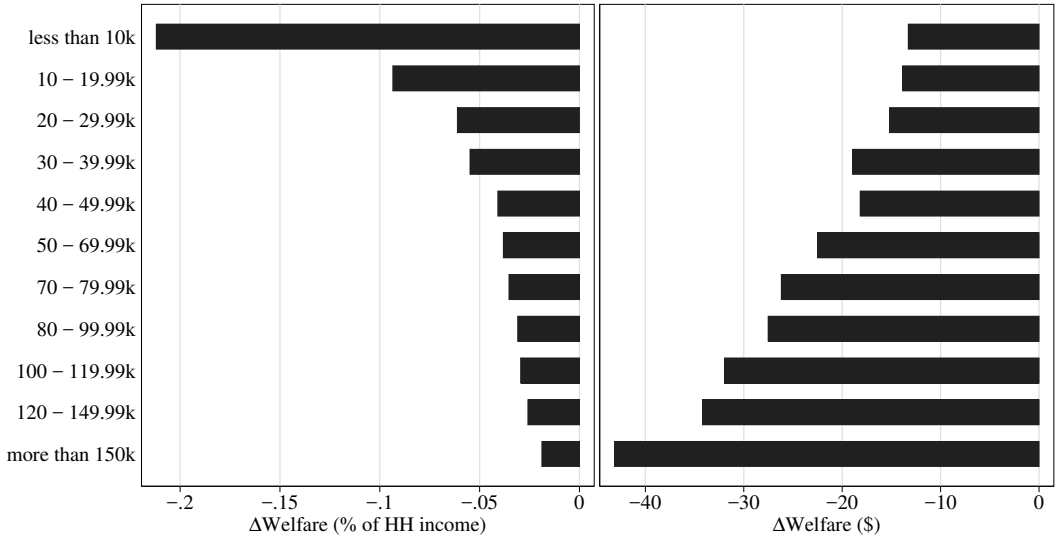
Notes: The figures present the cumulative minimum wage elasticity of prices at in states with or without Right-to-work (RTW) laws. 17 states in our sample have RTW laws. Effects at legislation and implementation are estimated jointly. We estimate the effects for a smaller estimation window and omit controls, because the lower number of states in the split samples limits the number of state clustered standard errors we can estimate. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also show 90% confidence intervals of these sums based on SE clustered at the state level.

Table 2.2 – Consumption expenditure shares on grocery stores' products by household income

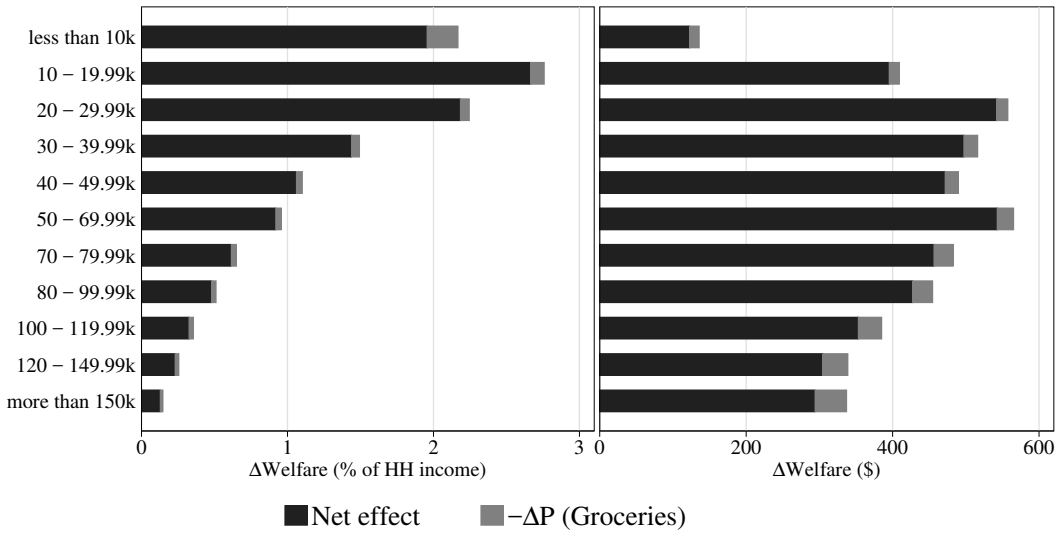
	All house-holds	1st Quintile lowest	2nd Quintile	3rd Quintile	4th Quintile	5th Quintile highest
2001 - 2005	11.1	15.3	13.6	12.1	11.1	9.1
2006 - 2009	10.7	14.3	12.7	11.4	10.7	9.0
2010 - 2012	11.0	14.4	12.8	11.6	10.8	9.4

Notes: Data are from the Consumer Expenditure Survey. Grocery products include: Food at Home, Household Supplies, Alcoholic Beverages, Personal Care Products and Services. Shares are calculated for each year and quintile of household incomes and then averaged over all years in a period.

Figure 2.11 – The welfare effects of price increases after a 20% minimum wage increase



(a) Equivalent Variation of price increase



(b) Nominal gains, Equivalent Variation and Net effect

Notes: The figures illustrate the Equivalent Variation (EV) of increasing all binding minimum wages in the US by 20%. See section 6 for a detailed description of the calculations involved. Figure 2.11a shows the EV for each income bracket in US dollars (right) and relative to mean household incomes (left). Figure 2.11b shows nominal gains (length of the bar), EV (gray) and the net effect (black) in US dollars (right) and relative to mean household incomes (left).

Table 2.3 – The cost structure of grocery stores

	Variable Cost			Fixed Cost		
	Labor Cost	COGS	Other Variable Cost	Buildings and Equipm.	Purchased Services	Other Operating Exp.
	Share in Total Cost					
2007	14.7	75.1	0.6	5.5	1.9	2.3
2012	14.1	75.4	0.6	5.4	1.8	2.7
	Share in Variable Cost					
2007	16.3	83.1	0.7			
2012	15.6	83.7	0.7			

Notes: Data are from the BLS Annual Retail Trade Survey (ARTS). All numbers are in %. A breakdown of operating expenses into categories is published every 5 years. Labor Cost includes salaries, fringe benefits and commission expenses. Cost Of Goods Sold (COGS) is calculated as nominal annual purchases minus nominal year-on-year changes in inventory. Other Variable Cost includes transport and packaging cost. Buildings and Equipment includes rents, purchases of equipment, utilities and depreciation. Purchased Services includes maintenance cost, advertisement, etc. Other Operating Expenses includes taxes and the residual operating expenses category. We illustrate shares in total cost and in Variable Cost (which includes Labor Cost, COGS and Other Variable Cost). Estimates of the shares and SE in parentheses are based on Taylor expansions using the coefficients of variation published in the ARTS.

Table 2.4 – Cumulative elasticities for our baseline estimates

Dep. variable: Store inflation	Separate estimation						Joint estimation		
	(1) Baseline	(2) Div.-time	(3) Chain-time	(4) Baseline	(5) Div.-time	(6) Chain-time	(7) Baseline	(8) Div.-time	(9) Chain-time
					Legislation				
E_0^{leg}	0.011***	0.009***	0.006				0.011***	0.013***	0.007**
	(0.004)	(0.003)	(0.004)				(0.003)	(0.002)	(0.004)
E_2^{leg}	0.017***	0.014***	0.010***				0.015***	0.019***	0.011**
	(0.006)	(0.005)	(0.004)				(0.005)	(0.004)	(0.005)
E_4^{leg}	0.021***	0.014**	0.013***				0.019***	0.021***	0.015**
	(0.007)	(0.006)	(0.004)				(0.007)	(0.005)	(0.006)
					Implementation				
E_0^{inc}				0.001	-0.003	-0.006	0.002	-0.003	-0.007
				(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)
E_2^{inc}				0.007	-0.000	-0.004	0.011	-0.000	-0.002
				(0.009)	(0.005)	(0.005)	(0.011)	(0.007)	(0.007)
E_4^{inc}				0.011	0.004	-0.003	0.016	0.006	-0.000
				(0.011)	(0.007)	(0.007)	(0.013)	(0.009)	(0.008)
					Estimation Summary				
E_4^{leg}+E_4^{inc}							0.036**	0.026**	0.015
							(0.014)	(0.011)	(0.011)
$\sum$ All	0.019	0.019	0.013	0.037**	0.015	0.010	0.046*	0.033	0.021
	(0.016)	(0.017)	(0.013)	(0.015)	(0.013)	(0.012)	(0.024)	(0.024)	(0.016)
$\sum$ Pre-event	-0.002	0.002	0.003	0.025*	0.008	0.010	0.010	-0.007	0.001
	(0.007)	(0.010)	(0.008)	(0.014)	(0.009)	(0.007)	(0.016)	(0.019)	(0.012)
N	191568	191568	190768	191568	191568	190768	191568	191568	190768
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Division time FE	NO	YES	NO	NO	YES	NO	NO	YES	NO
Chain time FE	NO	NO	YES	NO	NO	YES	NO	NO	YES

Notes: The table lists cumulative elasticities E_R, R months after legislation or implementation. The dependent variable is the store-level monthly inflation rate. Baseline controls are the unemployment rate and house price growth. Columns 1–3 show results of separate estimation of effects at legislation. Columns 4–6 show results of separate estimation of effects at implementation. Columns 7–9 show results of joint estimation of effects at implementation and legislation. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5 – Interaction between price response and initial wage in a county

Dep. variable: Store inflation	(1) Baseline	(2) Chain-time	(3) Baseline	(4) Chain-time
		Legislation		
$\text{wage}_q \times \Delta\text{leg}_{q-1}$	-0.012	0.005		
	(0.010)	(0.010)		
$\text{wage}_q \times \Delta\text{leg}_q$	-0.028**	-0.031**		
	(0.014)	(0.013)		
$\text{wage}_q \times \Delta\text{leg}_{q+1}$	0.003	0.004		
	(0.013)	(0.010)		
		Implementation		
$\text{wage}_{q-2} \times \Delta\text{mw}_{q-1}$			-0.026	-0.006
			(0.035)	(0.028)
$\text{wage}_{q-2} \times \Delta\text{mw}_q$			0.012	0.036
			(0.035)	(0.023)
$\text{wage}_{q-2} \times \Delta\text{mw}_{q+1}$			-0.016	0.010
			(0.028)	(0.025)
		Estimation Summary		
Observations	84741	84503	84748	84512
Controls	YES	YES	YES	YES
Store FE	YES	YES	YES	YES
State time FE	YES	YES	YES	YES
Chain time FE	NO	YES	NO	YES

Notes: The dependent variable is the store-level inflation rate. This specification is estimated at quarterly frequency. Baseline controls are the unemployment rate and house price growth. wage is the log county-level average weekly wage in grocery stores relative to the state minimum wage. The listed coefficients are the interaction between minimum wage increases and the local wage at legislation or 2 quarters prior to implementation. SE are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6 – Robustness checks for joint estimation

Dep. variable: Store inflation	(1) Weighted	(2) No Controls	(3) No Store FE	(4) Seasonal	(5) No Salesfilter	(6) Winsorized
			Legislation			
E_0^{leg}	0.008**	0.011***	0.011***	0.009**	0.018***	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.002)
E_2^{leg}	0.014***	0.015***	0.016***	0.015***	0.026***	0.013***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)	(0.004)
E_4^{leg}	0.019***	0.019***	0.021***	0.019***	0.031***	0.017***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.009)	(0.005)
			Implementation			
E_0^{inc}	0.008	0.002	0.001	0.002	-0.004	0.003
	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.006)
E_2^{inc}	0.016	0.012	0.011	0.011	0.013	0.012
	(0.011)	(0.011)	(0.012)	(0.011)	(0.009)	(0.011)
E_4^{inc}	0.024*	0.017	0.015	0.018	0.022*	0.015
	(0.013)	(0.013)	(0.014)	(0.013)	(0.011)	(0.012)
			Estimation Summary			
E_4^{leg}+E_4^{inc}	0.042***	0.036**	0.036**	0.037**	0.053***	0.033**
	(0.015)	(0.014)	(0.016)	(0.014)	(0.015)	(0.013)
$\sum$ All	0.058***	0.046*	0.046	0.046*	0.041	0.040*
	(0.020)	(0.024)	(0.028)	(0.025)	(0.027)	(0.021)
$\sum$ Pre-event	0.014	0.010	0.008	0.008	-0.004	0.004
	(0.013)	(0.016)	(0.018)	(0.016)	(0.018)	(0.014)
N	191568	191641	191568	191568	191568	191568
Controls	YES	NO	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	NO	YES	YES	YES
Seasonality	NO	NO	NO	YES	NO	NO
Weights	Obs	NO	NO	NO	NO	NO

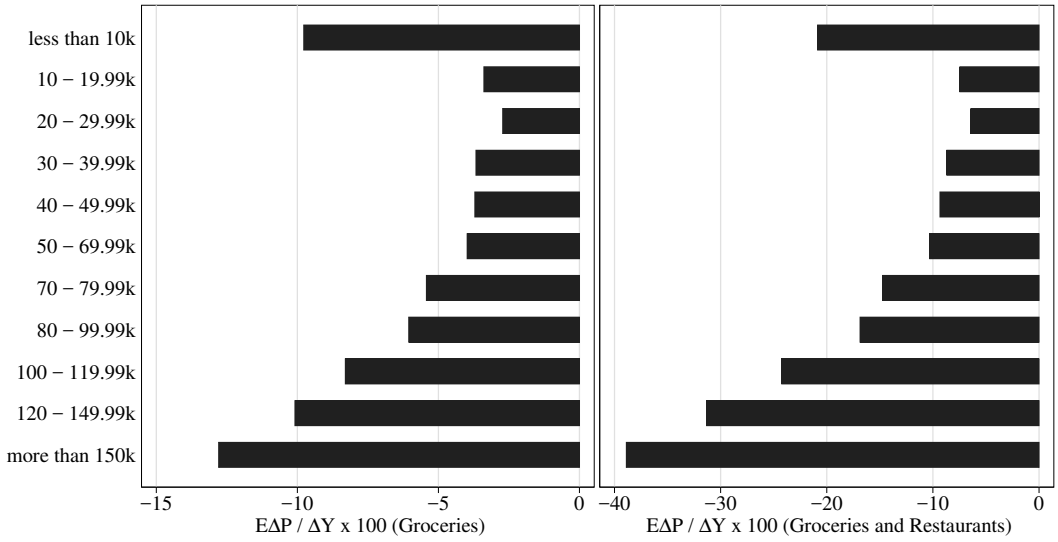
Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. (1) presents the baseline estimates for the joint estimation of the effects at implementation and legislation. (2) uses observation (UPC) weights. (3) uses observation weights and adds division-time fixed effects. (4) does not contain the control variables. (5) does not control for store fixed effects. (6) accounts for state-specific calendar month fixed effects. (7) does not correct for temporary price changes. (8) uses a winsorized outcome (98% winsorization). $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table 2.7 – Effects for income specific price indices

	Separate estimation						Joint estimation		
Dep. variable: Store inflation w. different weights	(1) Low income	(2) Medium income	(3) High income	(4) Low income	(5) Medium income	(6) High income	(7) Low income	(8) Medium income	(9) High income
					Legislation				
E_0^{leg}	0.009***	0.008***	0.010***				0.013***	0.014***	0.015***
	(0.003)	(0.003)	(0.003)				(0.004)	(0.004)	(0.004)
E_2^{leg}	0.014***	0.013***	0.014***				0.020***	0.019***	0.019***
	(0.003)	(0.003)	(0.003)				(0.004)	(0.004)	(0.004)
E_4^{leg}	0.013**	0.013**	0.014***				0.021***	0.021***	0.021***
	(0.005)	(0.006)	(0.005)				(0.005)	(0.005)	(0.006)
					Implementation				
E_0^{inc}				-0.002	-0.002	-0.002	-0.004	-0.004	-0.003
				(0.005)	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)
E_2^{inc}				-0.002	-0.001	-0.001	-0.006	-0.004	-0.002
				(0.007)	(0.006)	(0.006)	(0.009)	(0.008)	(0.008)
E_4^{inc}				0.009	0.010	0.008	0.006	0.007	0.008
				(0.009)	(0.009)	(0.009)	(0.012)	(0.012)	(0.011)
					Estimation Summary				
E_4^{leg}+E_4^{inc}	0.013**	0.013**	0.014***	0.009	0.010	0.008	0.027*	0.029*	0.029**
	(0.005)	(0.006)	(0.005)	(0.009)	(0.009)	(0.009)	(0.015)	(0.015)	(0.014)
$\sum$ All	0.013	0.010	0.010	0.013	0.017	0.021	0.021	0.023	0.028
	(0.013)	(0.012)	(0.012)	(0.016)	(0.015)	(0.014)	(0.025)	(0.024)	(0.023)
$\sum$ Pre-Event	-0.006	-0.008	-0.008	0.005	0.007	0.010	-0.018	-0.018	-0.014
	(0.008)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.017)	(0.017)	(0.017)
N	146815	146739	146739	146739	146739	146739	146739	146739	146739
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Division time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

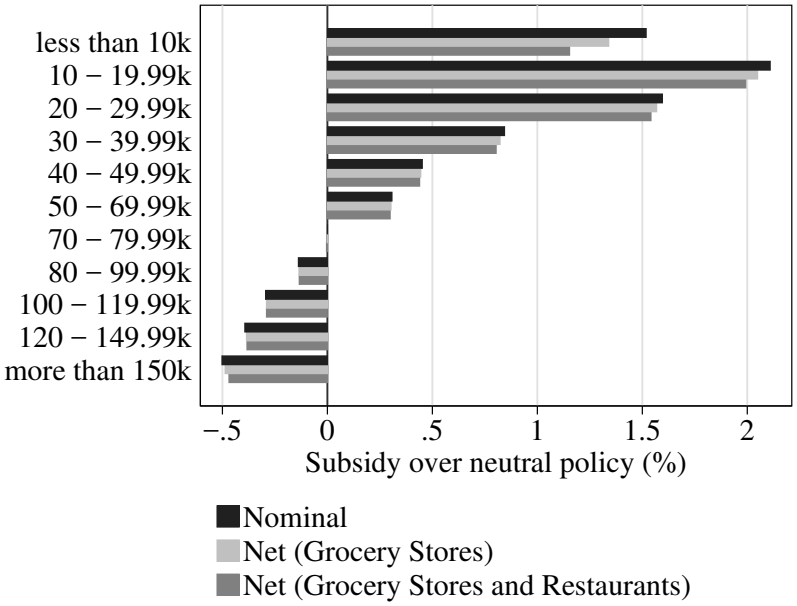
Notes: The dependent variable is the store-level inflation rate with expenditure weights for different HH income brackets. Low: $< 25k$. Medium: $25k - 75k$. High: $> 75k$. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Figure 2.12 – Equivalent Variation as percentage of nominal gains



Notes: The figure illustrates the Equivalent Variation (EV) as a percentage of nominal gains. The left panel is based on price increases in grocery stores. The right panel is based on price increases in grocery stores and restaurants.

Figure 2.13 – Bracket specific income subsidy over inequality neutral policy



Notes: The figure isolates the impact of gains from minimum wage increases on inequality from the level effect. We decompose nominal gains, gains net of price increases in grocery stores, and net of price increases in grocery stores and restaurants into an inequality neutral part and a bracket specific subsidy using equation 6.3.

Table 2.8 – Earnings and employment elasticities to the minimum wage in grocery stores, retail, and restaurants

	Grocery stores		Retail trade		Acc. and food services	
	(1) Baseline	(2) Trend	(3) Baseline	(4) Trend	(5) Baseline	(6) Trend
Panel A: Dep. variable: Labor cost per worker						
log MW	0.108**	0.083***	0.048*	0.038	0.151***	0.147***
	(0.043)	(0.027)	(0.026)	(0.024)	(0.024)	(0.025)
N	80,722	80,759	124,000	124,000	98,056	98,080
Only Right-To-Work states						
log MW	0.165***	0.159***	0.064	0.096	0.246***	0.238***
	(0.056)	(0.050)	(0.070)	(0.063)	(0.062)	(0.070)
N	40,385	40,385	71,583	71,583	56,322	56,322
Panel B: Dep. variable: Employment						
log MW	-0.010	0.089**	-0.002	-0.003	-0.042	-0.046*
	(0.048)	(0.036)	(0.027)	(0.017)	(0.033)	(0.027)
N	80,722	80,759	124,000	124,000	98,056	98,080
Panel C: Dep. variable: Number of establishments						
log MW	-4.30	-1.66	46.57	6.06	-25.51	4.29
	(3.98)	(3.96)	(36.85)	(14.22)	(24.58)	(14.37)
N	114,000	114,000	125,000	125,000	118,000	118,000
Controls	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Linear state trends	N	Y	N	Y	N	Y

Notes: The table shows elasticities to state-level minimum wages in the 2001–2012 period by industry, estimated using county-level panel data for 41 states used in our price regressions. The data are based on the QCEW. Retail trade corresponds to NAICS codes 44–45, grocery stores to NAICS code 4451, and accommodation and food services to NAICS code 72. The outcome in panel A is log average earnings by industry. The outcome in Panel B is the log employment in an industry, computed as the average employment in the three months in the respective quarter. The controls are the log of county population and the log of total employment in private industries per county. Standard errors are clustered at the state level.

Table 2.9 – Implied cost pass-through for various specifications

	(1) Baseline	(2) Division-time FE	(3) Chain-time FE
	Pass-through at legislation		
Implied cost pass-through	1.103	1.171	0.843
p-value PT = 1	0.780	0.531	0.651
	Pass-through at legislation and implementation		
Implied cost pass-through	2.026	1.491	0.836
p-value PT = 1	0.208	0.433	0.788
	Pass-through at legislation (incl. predicted effects on COGS)		
Implied cost pass-through	0.600	0.637	0.458
p-value PT = 1	0.0492	0.0163	0.00440
	Pass-through at legislation and implementation (incl. predicted effects on COGS)		
Implied cost pass-through	1.102	0.811	0.455
p-value PT = 1	0.819	0.582	0.102

Notes: The table illustrates the implied cost pass-through. Pass-through at legislation is the ratio of the elasticity of prices E_4^{leg} 5 months after legislation and the estimated elasticity of marginal cost. Pass-through at legislation and implementation reports the same ratio including the insignificant effects at implementation of minimum wage increases. p-values for a test of full pass-through are computed using standard errors for the pass-through ratio calculated using the Delta method.

Chapter 3

The Economic Effects of a $15 in the U.S.: A Simulation Approach

Abstract: We estimate a calibrated labor market model that we created specifically to analyze the effects of a $15 minimum wage. We take into account how workers, businesses, and consumers are affected and respond to such a policy and we integrate their responses in a unified manner. Our estimates compare employment numbers if policy were adopted to employment numbers if the policy had not been adopted. Other factors that may affect employment by 2024 are therefore outside the scope of our analysis. Our analysis incorporates recent laws that raised state minimum wages, such as in New York State and California. However, we ignore laws that raise minimum wages at the city level. We do so to simplify the presentation. We pay special attention to Mississippi because it is one of the lowest-wage states in the U.S. We find that a $15 nationwide minimum wage by 2024 would generate a significant increase in living standards for about 41.5 million workers and their families in the U.S. while creating a minimal effect on employment and a small price increase borne by all consumers. The effects in Mississippi would be roughly similar. A phased-in $15 minimum wage will be absorbed partly by employee turnover reductions and productivity increases, and mainly by modest price increases in restaurants and by increases in consumer spending.

1 Introduction

In April 2017, Sens. Bernie Sanders and Patty Murray, and Chuck Schumer (D-N.Y.) announced legislation that would raise the federal minimum wage to $15 an hour in the United States by 2024.[1] In this bill, the minimum wage increases are phased in over eight years, starting with $9.25 an hour on July 1, 2017 and reaching $15 an hour in 2024. The minimum wage would be indexed to the median wage growth thereafter. The goal of this chapter is to connect research with policy by providing an *ex-ante* evaluation of this bill.

The minimum wage is a recurrent policy issue and there are many analyses of the effects of minimum wage increases on income of families with members that receive a raise (Office, 2014; Cooper, 2013; 2017), and the effects on income of families with members that loose their jobs (Office, 2014). Here, we attempt to evaluate the employment effects of minimum wage policies that take into account how all economic agents – not just workers, but workers, businesses and consumers – are affected and respond to such a policy and we integrate their responses in a unified manner. In particular, we take into account the effects of minimum wage increases on aggregate demand, and how they boost consumer spending and GDP. In what follows, we estimate a calibrated labor market model that we created specifically to analyze the effects of a $15 minimum wage. We compare employment numbers if policy were adopted to employment numbers if the policy had not been adopted.

2 Policy context

A Labor market context

We review here recent labor market conditions in the U.S. and in Mississippi. We focus on how five indicators changed during the Great Recession and the

[1] This chapter has been written with Sylvia Allegretto and Michael Reich.

subsequent recovery: unemployment, employment changes, employment rates, worker pay, and pay inequality. Each indicator provides a somewhat different perspective on the nature of the current recovery. The Great Recession began at the end of 2007; the recovery began in June 2009. As figure 3.1 shows, U.S. unemployment rate continued to increase into 2010, later than the beginning of the economic recovery. Mississippi was especially affected by the recession, with the state unemployment rate reaching to about 11 percent. The Mississippi unemployment rate has been one of the highest in the nation. However, since 2010 the U.S. and Mississippi unemployment rates have fallen at about the same rate. By May 2017, the unemployment rate in Mississippi was 4.9 percent, lower than its 2007 pre-recession rate, and very close to the U.S. rate.

Figure 3.2 shows that the U.S. and Mississippi each experienced sizable job losses during the Great Recession, in about equal proportions for the nation and the state. Net job losses lasted longer in Mississippi until 2014. Mississippi employment then recovered, growing at the same pace as in the U.S., and reaching its 2007 level in 2016.

Figure 3.3 depicts trends in the employment rate – the share of the prime working age population that is employed– for both the U.S. and Mississippi. This indicator provides a companion to the unemployment rate, as it counts workers who stopped looking for work for whatever reason. After the employment rate in the U.S. fell rapidly during the Great Recession, it has been growing slowly since 2010. Nonetheless, the employment rate remains well below its pre-recession level. Economic analyses indicate that slow economic growth during the recovery is mainly responsible for the weak improvement of the prime working age employment rate. The prime-age employment rate in Mississippi has long been below the U.S. rate. This pattern is present during the Great Recession– and it has continued since. Indeed, despite the sustained decline in Mississippi's unemployment rate, the state's employment rate remains one of the lowest in the U.S.

We turn next to recent pay level and pay inequality patterns. Figure 3.4 displays recent trends in average hourly earnings, adjusted for inflation, in the U.S. and the Mississippi. Median hourly pay in Mississippi is the lowest of any state in the U.S. and considerably lower than the U.S. average. Hourly pay at first fell during the recession and then increased. Real average earnings continued to decline during the first half of the economic recovery. Although real earnings have ticked up slightly since 2013, their level remains no higher than it was before the onset of the recession.

Figure 3.5 shows that recent wage trends have been remarkably uneven. Despite six years of official economic recovery in the U.S., workers' real wages (adjusted for inflation) only increased for the bottom 10 percent of the nation's workforce in the U.S. and for those at the 80th percentile and above. In Mississippi, only workers between the median and the 70th percentile of the real wage distribution had increases. In summary, unemployment and employment trends have improved substantially in recent years. However, the earnings of typical workers have not increased, despite the economic recovery.

B Minimum wage increase schedule

Figure 3.6 displays the recent history of the federal minimum wage rate, adjusted for inflation and projected forward to 2024. Since 1980, Congress has passed bills raising the federal minimum wage on only three occasions. Since the federal minimum wage is not indexed to inflation, its value as declined over several long periods. The red dashed line represents the future evolution of the federal minimum wage in the absence of any legislated increase. The blue dashed line represents the evolution of the federal minimum wage in 2017 dollars if the Fair Minimum Wage Bill of 2017 is enacted. Table 3.1 reports the U.S. minimum wage schedule proposed in the Raise the Wage Act of 2017. The minimum wage increases would be phased in over eight years, starting with $9.25 an hour on July

1, 2017 and reaching $15 an hour in 2024.

In this paper, we use a slightly different and simplified schedule. We take into account enacted state minimum wage increases, but ignore interim increases that are implemented earlier by some cities and counties.

3 Employment analysis for the U.S.

A The model

Reich et al. (2015) developed a calibrated model to study the prospective impacts of a $15 minimum wage in Los Angeles. This model was further enhanced to study the effects of a$15 minimum wage in New York State (Reich et al., 2016a), in San Jose and Santa Clara County (Reich et al., 2016b) and in California and Fresno (Reich et al., 2017). Those reports all focus on city and state policies. In this report, we build upon our previous model by incorporating macroeconomic considerations that become important in assessing a national policy. We also adapt the model to apply to Mississippi in particular.

Our estimates draw on standard government data sources, the large body of economic research on the minimum wage, other research studies, and a standard input-output economic model (IMPLAN). These data sources and models are fully documented in the text, accompanying endnotes, and in the appendix.

Our economic impact model recognizes that higher minimum wages will affect labor supply and labor demand. Adjustments to labor supply include lower employee turnover and reduced recruitment costs for employers. Standard labor supply studies of low-wage labor markets find elasticities of about 0.3, low enough to ignore as a first approximation. Our own as yet unpublished estimates of minimum wage labor supply elasticities are somewhat higher for some groups, notably low-educated parents of children under six. Since these estimates are preliminary, we do not incorporate them here. As a result, we may

underestimate the positive effects of the policy on the earnings of these groups. Recent research reviews (Leigh, Leigh and Du 2019) indicate that minimum wages improve health outcomes and reduce absenteeism from work. We do not attempt to quantify these effects in our model, but note that they imply productivity improvements that will be larger in lower-wage states.

Adjustments to labor demand include possible substitution of capital or materials for labor and of skilled labor for unskilled labor, greater worker productivity when wages rise, reductions in employment because higher prices reduce sales, and increases in employment because workers' spending out of their higher income will increase sales and employment. The net effect depends upon the magnitudes of the individual adjustments, again taking into account interactions among them.

The labor demand model draws from standard labor economic textbook analyses. For industry labor demand, these analyses incorporate "substitution" and "scale" effects in labor, capital, and goods markets. (For a formal version of this labor demand model, see Cahuc et al. (2014)). Since our concern here is on the effects of an economy-wide minimum wage, we add an "income effect." The income effect accounts for changes in the level of economic output when wage increases lead to increased consumer demand.

B Model structure

Figure 3.7 summarizes our model qualitatively in a flow diagram. The green boxes refer to the effects on workers and the red boxes refer to the effects on businesses. The automation and productivity box is placed first to highlight how businesses will respond to a minimum wage. Automation here refers only to capital-labor substitution that is induced by the minimum wage, not to the much larger degree of automation that has taken place for decades. Productivity growth can come from automation, from workers working harder or smarter

when pay is high, and from workers having more experience, as when minimum wages reduce employee turnover.

Examine next the effects on workers, shown in the green boxes and move from left to right. The first green box refers to the higher wages received by lower-paid workers. The next green box accounts for the net effect of taxes and reduced receipt of public benefit programs on workers' income. Workers will pay more in taxes as their wages increase and eligibility for public benefits will decline. The third box refers to how workers' increased spending power out of their higher net income translates into higher consumer demand and more jobs. We will refer to this mechanism as the income effect of minimum wages. Examine now the effects on businesses and again move from left to right. The higher minimum wage will increase businesses' payroll costs, but some of these higher costs will be offset because employee turnover will fall, generating savings in recruitment and retention costs. Firms may also find that higher-paid and more experienced workers will be more productive, which could also offset payroll cost increases. In other words, one effect of a higher minimum wage is to induce more efficient management practices.

Higher payroll costs (net of turnover and productivity savings) will lead firms to increase prices, leading to reduced consumer demand. We will refer to this adjustment mechanism as the scale effect, as it identifies reductions in the scale of output that will reduce the demand for workers.

As we have already mentioned, businesses may also respond to higher minimum wages by increasing their investment in equipment. This substitution effect (think automation) also reduces their demand for workers. The income effect has a positive effect on employment, while the scale and substitution effects each have negative effects on employment. The sum of the income, scale, and substitution effects determines the net employment effect of the minimum wage, as shown in the blue box on the right side of Figure 3.7.

Figure 3.7 is useful for understanding the basic structure of our model. But it

leaves out some important details. First, the effects on businesses and workers in the red and green boxes of the model occur simultaneously, not sequentially. The effects in reality are therefore captured only by examining the net effects on the economy and employment. These net effects are symbolized by the blue box at the right of the diagram. Second, Figure 8 omits some lesser feedback loops that would make the figure unwieldy, but which are included in our calculations.

B.1 Model calibration and dynamics

The net effect of minimum wages on employment equals the sum of the income, scale, and substitution effects. The income effect will always be positive, while the scale and substitution effects will always be negative. Whether the net effect is positive, zero, or negative therefore depends upon the relative magnitudes of its three components.

These relative magnitudes in turn depend upon the quantitative responses of workers and businesses to a minimum wage increase. We refer to the model's parameters as the inputs that determine these multiple quantitative responses. Some of these parameters, such as the propensity to substitute capital for labor, may not vary with the magnitude of the minimum wage increase. Other parameters, such as turnover cost savings, are likely to vary with the size of the increase. As with any economic model, we calibrate our model using the best data and research findings available. The details are presented in Section 5 below and in the Appendix.

Note that the substitution, turnover and productivity effects operate entirely on low-wage workers, while the scale and income effects operate on the entire wage distribution of workers. It is thus possible that minimum wage policies that do not reduce employment overall can still reduce the number of low-wage jobs and increase the number of middle and high wage jobs. Since the wage distribution of jobs is thickest in its middle ranges, we would expect most of the new jobs to be located near the middle of the wage distribution.

The model's parameters and dynamics must be consistent with two conditions. First, the model must be consistent with the very small effects that researchers find for the smaller pre-2015 increases in federal and state minimum wages. Second, the model must be consistent with growing negative effects if minimum wages were to reach extremely high levels, such as at $30 or $40 per hour.

Reich et al. (2017) show that our calibrated model predicts extremely small employment effects for minimum wage increases of up to 25 percent, to a minimum wage of $10. At this minimum wage, the income, scale, and substitution effects are each very small. At minimum wages well above the mean, the (positive) income effect weakens because the increase in the proportion of workers getting pay increases slows down, and because the propensity to consume of higher-paid workers is lower than that of lower paid workers. At the same time, the (negative) scale effect strengthens because turnover cost savings diminish and the price elasticity of consumer demand becomes higher for higher-priced goods.[2]. Our model is thus consistent with growing negative employment effects at higher minimum wage levels.

The big question, of course, is: At what level do the negative effects become important? How quickly do they become still more negative?

We have tested our model's calibration by undertaking a series of robustness tests. The tests show that this net effect changes by small amounts when we vary the model's parameters. In the next sections, we discuss how we quantify the effects in each of the boxes in Figure 3.7.

B.2 Macroeconomic considerations.

Our state and local prospective studies of possible employment effects at $15 accounted for capital-labor substitution possibilities in low-wage industries, efficiency-

[2]The capital-labor substitution elasticity is not likely to be higher or lower at high minimum wage rates

wage like productivity increases, reduction in employee turnover costs, price increases that would reduce consumer demand and therefore employment, and increased consumer demand resulting from higher consumption propensities among low-wage workers–calculated from the net income increase after increased tax payments and reduced receipt of public transfer programs. When relevant, we also considered spending leakages because of in-commuters who lived outside the city or state. We consider here three issues for modeling a prospective study of a $15 federal minimum wage policy that were not salient for our modeling of prospective state and local increases: price increases and pass-throughs of intermediate goods; changes in interest rates if inflation increases; and potential output constraints that could generate inflation and constrain the income effect.

Price changes of intermediate inputs. In our state and local prospective studies, we estimated price effects by focusing solely on changes in payroll costs after taking into account minimum wage effects on capital-labor substitution, on increasing productivity growth and on reducing employee turnover costs. Here we examine adjustments in the quantities and prices of intermediate inputs that might be passed on to final demand industries. Concerning quantities of inputs, previous work (Harasztosi and Lindner, ming) shows that labor-materials substitution elasticities are close to zero. Changes in quantities of materials will not offset payroll increases.

Concerning input prices, we assumed that effects of higher state and local minimum wages on materials prices would be constrained, to the extent that these prices are determined at the national level. This constraint does not apply in the case of a federal minimum wage increase.

Materials prices will increase in proportion to the share of low-wage labor costs in their operating costs. In practice, only a few intermediate inputs embody a large enough share of low-wage work to generate detectible price effects from a $15 federal minimum wage. In practice, the most affected intermediate in-

put industries are the agricultural sectors that are low-wage and labor-intensive, much of food manufacturing and much of apparel manufacturing. Agriculture and food manufacturing prices are partly limited by prices of agricultural imports. Import competition will almost completely limit price increases in apparel manufacturing.

The materials most affected by changes in input prices are thus food and food products used or sold in restaurants and retail. (We ignore direct farm sales to consumers, such as at farmers' markets, because they constitute a small percentage of total farm sales.) Material inputs in other industries that are highly affected by minimum wage increases, such as childcare, care for the disabled and elderly, and janitorial and protective services, do not use materials in sufficient quantities to be affected substantially by changes in materials prices.

Regarding agriculture, labor accounts for 17 percent of operating costs in agriculture overall. But this average masks substantial heterogeneity by sector, as the chart below shows:

In general, the more labor-intensive sectors are those that have proven difficult to mechanize and pay lower wages to their workers. These are concentrated among fruits, vegetables and horticultural nursery products. (Wages in poultry are also low.) Within fruits, vegetables and horticultural products, we can further distinguish between crops whose planting and harvesting are nearly fully mechanized (tomatoes, almonds) from those that are hardly mechanized (strawberries, oranges). Fruit, vegetables and nursery products together account for about-one fourth of U.S. agricultural product.

Method. Our method is iterative. In the first round we estimate price increase for individual two-digit industries using only the changes in labor-related costs, just as in our previous studies. In the second round, we enter these price increases into the material costs expression of the price equation. Since only a few inputs and outputs are likely to be significantly nonzero, we expect that the second

round effects will be significant but much smaller than for the first round. We then test this intuition by computing a third round, using round 2 price increases for inputs into the price equations. If round 3 generates additional detectible effects, we continue through further rounds, until the results have significantly converged.

Effects on growth and employment through the interest rate channel. If our state-level price effects estimates are a guide to the federal effect, overall prices nationally would increase cumulatively by about 0.4 percent over the eight years to 2025. The CBO model forecasts 2 percent inflation between now and then. It also predicts that real interest rates will be above the zero lower bound that has applied in recent years. If inflation is higher because of the$15 minimum wage policy, the Federal Reserve Board might increase the federal funds rate, which in turn could have a negative effect on inflation and on real economic growth and employment. (The federal funds rate is the price the Fed charges for overnight borrowing by banks; changes in this rate are usually fully passed along to short-term rates for consumers and businesses.) Modern macroeconomic theory posits that the Federal Reserve Board will use a formula known as the Taylor Rule to set the federal funds rate:

$$r = \pi + .5 \times y + .5 \times (p - 2) + 2 \tag{3.1}$$

where r = the federal funds rate,π = the inflation rate and y = the gap between potential and actual output. The Taylor Rule implies that the real federal funds rate would adjust by 0.5 time the amount that inflation exceeds 2. A minimum wage-generated increase in the inflation rate from 2.0 to 2.4 thus implies that the federal funds rate would increase, by 2025, by 0.20 points more than it would, absent the minimum wage policy. The quantitative effects of this small increase

on the longer-term interest rates that are relevant to much consumer and business borrowing are highly uncertain, as they depend upon expectations of the future. Some economists forecast that we have entered an era of secular stagnation for interest rates and economic growth. If they are correct, a 0.20 increase by 2025 would not have any measurable effects on longer-term interest rates. If they are wrong, the effects might be detectable, but it remains unlikely that the effects on economic growth and employment would be of more than modest importance. We conclude that the effects of $15 federal minimum wages on interest rates are too small and uncertain to warrant including in our federal model.

Capacity constraints that could generate inflation rather than output growth. CBO projections assume that economic growth through 2024 will continue to close the gap between potential and actual output, while maintaining inflation at the Federal Reserve Board's target rate of 2 percent. Of course, the economy's future growth path, absent a federal minimum wage increase, might differ from these projections. Nonetheless, these projections constitute our baseline.

C Effects on workers

We begin with the effects on workers, shown in the green boxes in Figure 3.7. To quantify these effects we draw upon the estimates in Cooper (2017). Cooper applies the federal $15 minimum wage proposal to the U.S. wage distribution to estimate the number of workers who will receive a wage increase, as well as the size of those wage increases. To do so, Cooper's model simulates changes in the U.S. wage distribution in future years, under two scenarios. In the first scenario, the proposed minimum wage policy is not adopted, but wages increase in line with recent trends; in the second scenario, the policy is adopted. Cooper estimates, for each scenario and each yearly phase-in step, the number of workers who would be affected by the law and the additional wages they would receive as a result. In constructing these estimates, Cooper's model adjusts for expected

growth in employment, real wages, and inflation over time. His estimates also take into account what is often referred to as an "indirect" or "compression" effect: workers who make slightly more than the new proposed minimum wage level are also likely to receive wage increases.

C.1 Workforce impacts

Table 3.2 shows the estimated number and percentage of eligible workers who will receive pay increases by 2024. Cooper (2017) estimates that 41.5 million U.S. workers will receive a pay raise by 2024, or about 29.2 percent of the eligible workforce. Of these, 22.5 million would receive increases because their pay would otherwise be below $15 per hour when the increases would be fully implemented in 2024 (the group directly affected by the law). Another 19.0 million would receive pay increases because their pay would be only slightly more than $15 when the increases would be fully implemented (this the group indirectly affected by the law).

Table 3.2 also displays the additional earnings that affected workers would receive: the estimated cumulative increase in affected workers' hourly wages, annual earnings, and percentage increase in annual earnings, as well as the cumulative total earnings increase for all affected workers. Cooper (2017) estimate that the hourly wages of workers who will receive pay increases will rise by about $2.08 by 2024. That amounts to an estimated additional $3,470 in earnings per year. In total, workers will receive an additional $144 billion in aggregate pay by 2024.

C.2 Impact on Benefits Eligibility

Some policymakers have expressed concern that affected workers and their families could ultimately be worse off after minimum wage increases if they are no longer eligible for means-tested social assistance programs. However, research suggests that most workers will come out well ahead financially, because the

benefits from most social assistance programs phase out gradually as recipients' income rises. As the pay of affected workers increases, the benefits they receive will gradually decline?they are not eliminated all at once. The Congressional Budget Office (Office, 2012) estimates that the average marginal tax rate for low- and moderate-income workers is 34.8 percent, meaning that affected workers will keep 65.2 cents of each additional dollar they earn. So while taxes and reductions in social assistance benefits will offset some of the additional earnings for affected workers, most families will still obtain significant net gains in income from the minimum wage increases.

C.3 Downstream effects

The increases in earnings shown in Table 3.2 would be substantial and would have an immediate impact on the lives of low-wage workers and their families. But it is important to recognize that there are longer-term effects of minimum wage increases as well.

Low wages have been shown to affect workers negatively in a variety of ways, but the health impacts are most pronounced. All else being equal, low wages (and in turn poverty) result in increased rates of high blood pressure and high levels of stress, as well as shorter life expectancy (Leigh and Du, 2012). A recent study from the United Kingdom found that by reducing the financial strain on low-wage workers, an increase in the minimum wage improves mental health at a level comparable to the effect of antidepressants on depression (Reeves et al., 2016). In another study, additional income led to fewer arrests for parents and increases in parental supervision of their children (Akee et al., 2010). Similarly, increases in Earned Income Tax Credit (EITC) program payments led to improvements in the mental health of mothers (Evans and Garthwaite, 2014; Office, 2012), as well as reduction in the incidence of low birth weights (Hoynes et al., 2015).

Multiple studies also establish a causal negative effect of low incomes on outcomes for children. A recent review of peer-reviewed articles found that 29 of 34

studies established a negative effect of poverty on children's outcomes (Cooper and Stewart, 2013). Using data from a randomized control trial of the Minnesota Family Investment Program, researchers found positive, significant effects on children's social behavior and school engagement due to increases in income (Morris and Gennetian, 2003). Similarly, increases in the Earned Income Tax Credit are found to lead to large benefits on math and reading test scores in elementary and middle schools (Dahl and Lochner, 2012; Chetty et al., 2011; Maxfield, 2014). The EITC also leads to higher rates of high school completion (or GED) and higher college attendance rates (Maxfield, 2014; Manoli and Turner, 2018). This in turn translates into better employment outcomes and higher earnings bastianetal2018.

Generally, these studies show that additional income has a positive effect on the outcomes of children in households of all income levels. However, multiple studies also suggest that additional income has a larger effect in very-low-income households compared to middle-income households (Dahl and Lochner, 2012; Akee et al., 2010; Costello et al., 2003). Some evidence indicates that additional income early in life is important to cognitive outcomes, whereas additional income in later childhood may be more important in terms of behavioral outcomes (Cooper and Stewart, 2013).

D Effects on businesses

How a higher minimum wage affects a firm depends on how much the firm's operating costs change and on how the firm responds to those changes. In this section, we first identify the industries that will be highly affected by the two minimum wage increase scenarios. We then estimate the impact of the minimum wage increases on firms' operating costs across the entire economy and for highly affected industries, taking into account savings from reduced turnover.

Minimum wage increases do not affect all industries equally. Table 3.3 shows the estimated distribution of affected workers across U.S. industries by 2024. In

the first column, we show the percentage of the overall eligible workforce in each industry. The second column displays our estimate of the distribution across industries of workers getting a raise. The third column presents our estimate of the percentage of workers getting a raise within each industry.

About 40 percent of affected workers are employed in just three service sector industries: food services (15.6 percent), retail (18.2 percent), and health services (10.5 percent), which is comprised mainly of building services contractors and employment agencies. The service sector also dominates the list of industries that have high rates of low-wage work – that is, industries in which a high share of workers will get a raise (for example, 67.8 percent in food services and 47.0 percent in retail).

Table 3.3 also displays the sectoral distribution of affected workers who will get pay increases.

We determine changes in a firm's operating costs due to a minimum wage increase from the following factors: the share of workers receiving wage increases, the average size of the wage increases, and the labor share of operating costs within the firm. As we saw in Table 3.3, in most industries only a minority of workers will receive a wage increase. Furthermore, among workers that do receive an increase, not everyone will receive the full increase (because many of the affected workers already earn more than the current minimum). Specifically, we estimate that the total wages of all affected workers will increase by 17.3 percent. However, affected workers' wages represent only 13.0 percent of all workers' wages in the U.S. As a result, total wages will increase by only 1.9 percent.

Economic research suggests that some of the increased labor costs that businesses face as a result of a higher minimum wage can be offset through lower turnover. In our calculations below, we take the midpoint of those estimates and assume that 17.5 percent of increased labor costs are absorbed via turnover

savings in the first year.[3] These savings are likely to accrue at smaller rates as wage levels go higher; we therefore assume that by 2024 the marginal increase in earnings relative to 2017 no longer yields any additional turnover savings. As a result, we estimate that the total savings from turnover at a$15 minimum wage in 2024 would be 7.5 percent of increased labor costs[4].

D.1 Operating costs and prices

Table 3.4 shows our estimates of the increase in business operating costs (net of savings from reduced turnover) in all sectors. By 2024, we estimate that businesses in the restaurant industry would see their payroll costs rise by 11.3 percent and businesses in the retail industry would see their payroll costs rise by 4.2 percent; these cost estimates include payroll taxes and workers' compensation insurance expenses. [5]. Across the entire U.S. economy, we estimate that payroll costs would rise by 1.9 percent by 2024.

However, operating costs will rise by a much smaller amount, because labor costs only make up a portion of the total costs that businesses face. We estimate that labor costs excluding health benefits will account for 38.3 percent of restaurant operating costs, 12.0 percent of retail operating costs, and 29.1 percent for the overall economy by 2024. We therefore estimate that total operating costs would rise by 2024, by 4.3 percent for restaurants, 0.5 percent for retail, and 0.6 percent for the overall economy. (See Appendix A2.2 for more detail on how we estimate the labor share of operating costs by industry.)

[3]Hirsch et al. (2015) and Reich et al. (2003) found improvements in worker productivity following higher wage mandates.

[4]The turnover savings are considered constant in 2018, 2019 and 2020, at 17.5 percent of increased labor costs, a midpoint estimate in the literature (Hirsch et al., 2015; Reich et al., 2003). These savings are likely to accrue at smaller rates as wage levels go higher; we therefore assume that by 2024 the marginal increase in earnings relative to 2018 no longer yields any additional turnover savings. As a result, we estimate that the total savings from turnover at a $15 minimum wage in 2024 would be 7.4 percent of increased labor costs.

[5]We use a payroll tax rate of 7.65 percent (6.2 percent for Social Security and 1.45 percent for Medicare). Workers' compensation insurance rates: vary by industry (see table 3.6: http://www.wcirb.com/sites/default/files/documents/state_of_thewc_system_report_140815.pdf

E Effects on employment

In previous sections, we have assessed the benefits to low-wage workers as well as the impact on businesses' operating costs in particular industries. In this section we consider whether the proposed policy would generate net gains or losses to the U.S. economy.

In Section 5.1, the key issues concern how much employers will substitute equipment or skilled labor for unskilled labor and how much of their cost increases employers will pass on in the form of higher prices. In Section 5.2, we discuss who might pay the costs of the higher minimum wage. Higher prices reduce consumption demand, which translates into reductions in employment and economic activity.

Section 5.3 examines the increased spending that derives from the higher income of low-wage workers. We take into account the effects of taxes and reduction in public benefits on the affected workers' take-home pay and the rate at which their households spend income compared to others. Greater spending from consumers increases economic demand, which translates into increases in employment and economic activity. The net effects on the economy will then depend upon the sum of the effects estimated in each of these three sections. Section 5.4 estimates these net impacts on economic activity and employment.

E.1 Reductions in paid hours relative to working hours

Some commentators assert that a higher minimum wage will lead employers to cheat workers of a portion of their wages. However, such practices already exist; the question at hand is how much the minimum wage increase will increase their prevalence and intensity. Although it is difficult to measure changes in wage theft, we know that employee-reported increases in pay (to a census surveyor) after a minimum wage increase match up well to employer-reported increases in pay on administrative reports that determine payroll taxes (Dube et al., 92). These

results suggest that most employers comply about as much after the increase as before.

E.2 Employee turnover and employer recruitment and retention costs

The correlation between low wages and high employee turnover is well known (Cotton and Tuttle, 1986) [6]. Over the last decade, annual employee turnover in accommodation and food service averaged 70 percent a year, compared to 41.4 percent in other services, 30.5 percent in health care and social assistance, and 32 percent in non-durable manufacturing (Statistics 2014) [7]

Quits are higher in low-wage occupations because workers leave to find higher-wage jobs or because they are unable to stay in their jobs due to problems such as difficulties with transportation, child care, or health. Recent labor market research has gone beyond establishing a correlation between pay and turnover. We now know minimum wage increases have well-identified causal impacts that reduce worker turnover. Dube et al. (2007) found that worker tenure increased substantially in San Francisco restaurants after the 2003 minimum wage law, especially in limited service restaurants. Dube et al. (2016) found that a 10 percent increase in the minimum wage results in a 2.1 percent reduction in turnover for restaurant workers and for teens. Jacobs and Graham-Squire (2010) reviewed studies of the impact of living wage laws on employment separations and found that a 1 percent increase in wages is associated with a decline in separations of 1.45 percent.

Turnover creates financial costs for employers (Blake, 2000; Dube et al., 2010; Hinkin and Tracey, 2000). These costs include both direct costs for administrative activities associated with departure, recruitment, selection, orientation, and

[6] Since workers often increase their wages by moving from one employer to another, we cannot assume that the correlation between wages and turnover indicates that low wages are causing higher turnover. As we discuss below, however, policy experiments with living wages and minimum wages have provided the evidence needed to determine that wages do, in fact, affect turnover.

[7] These averages include the low-turnover period of the Great Recession, and can be expected to increase towards higher pre-recession levels as the labor market tightens.

training of workers, and the indirect costs associated with lost sales and lower productivity as new workers learn on the job. Hinkin and Tracey (2000) estimate the average turnover cost for hotel front desk employees at $5,864. A study of the cost of supermarket turnover by the Coca Cola Research Council estimates the replacement cost for an $8 an hour non-union worker at $4,199 (Blake, 2000). Boushey and Glynn (2012) estimate that the median replacement cost for jobs paying $30,000 or less equals 16 percent of an employee's annual salary.

Pollin and Wicks-Lim (2015) estimate that 20 percent of the increased costs from a minimum wage increase are offset by reductions in turnover. Similar estimates can be found in Fairris (2005) and Jacobs and Graham-Squire (2010). In a small case study of quick service restaurants in Georgia and Alabama (Hirsch et al., 2015), managers reported they offset 23 percent of the labor cost increases through operational efficiencies. For our calculations below, we assume that 17.5 percent of the increase in payroll costs is absorbed through lower turnover in the early years of the proposed minimum wage increase. [8] However, these turnover savings do not continue to grow at higher wage levels. Dube et al. (2016) find that most of the reduction in turnover occurs among workers with less than three months of job tenure. Zipperer (in progress) also finds that employee turnover rates begin to level off at wages that are twice the minimum wage.

These results suggest that the effect of higher wages on increasing tenure dissipates as wage levels increase. We therefore assume that the increases in wages after 2020 no longer result in turnover reductions, yielding an overall lower rate of savings from turnover of 7.5 percent in 2024.

E.3 Impact of higher wages on worker performance

Paying workers more can also affect worker performance, morale, absenteeism, the number of grievances, customer service, and work effort, among other metrics

[8] The estimate of 17.5 percent represents the midpoint between the 20 percent estimate of Pollin and Wicks-Lim (2015) and a 15 percent (unpublished) estimate that draws upon Dube et al. (2010) and Dube et al. (2016).

(Hirsch et al., 2015; Reich et al., 2014; Ton, 2012; Wolfers and Zilinsky, 2015).

Efficiency wage models of the labor market argue that wage increases elicit higher worker productivity. The possible mechanisms include: When employers pay workers more, workers are more willing to be more productive and require less supervision; workers remain with the firm longer and thereby gain valuable experience; and higher pay tends to reduce idleness on the job. This theoretical result holds whether one company raises its wage above the market-clearing level, or whether all do (Akerlof and Yellen, 1986).

Reduced employee turnover means that workers will have more tenure with the same employer, which creates incentives for both employers and workers to increase training and therefore worker productivity. A large scholarly literature makes this point, and it has been emphasized recently by firms such as Walmart, TJ Maxx, and The Gap as principal reasons underlying their announced policies to increase their minimum wages nationally to $10. However, because of the lack individual- or firm-level productivity data, the earlier efficiency wage literature does not provide a reliable quantitative assessment of the importance of the effect on worker productivity among low-wage workers.

A recent paper by Burda et al. (2016) does just that. Using microdata for 2003-2012 from the American Time Use Study, Burda et al. (2016) find that working time while on the job increases when wages are higher. Their results imply that an increase in hourly pay from $10 to $15 increases the level of productivity by 0.05 percent.

Burda et al. (2016)'s estimate may be too high, given the difficulty of disentangling cause from effect in their idleness data. On the other hand, they do not have measures of worker engagement while working, which could make the actual worker productivity improvement potentially twice as large. To capture this range of productivity effects in our model, we use the low Burda et al. (2016) estimate of 0.05 percent. [9] For a full-time worker, going from $10 to $15 per

[9] Burda et al. (2016), Table 3.6, cols 3 and 5 report that a $1 increase in weekly pay reduces the

hour raises weekly pay by $200, so the effect on productivity would be about .2 $\times$ .027 = .005, or 0.5 percent. This estimate measures just the effect of reducing idleness. Positive effects on absenteeism and worker engagement would add to the productivity engagement.

A recent study by (Abowd et al., 2012) demonstrates the substantial room for productivity and wage growth in low-wage industries in the U.S. Using longitudinally linked employer-employee data, (Abowd et al., 2012) disentangle wage differentials among industries that are attributable to individual heterogeneity (such as the demographic, educational, and work experience characteristics of workers in the industry), which they label person effects, from the characteristics of the product market and bargaining power of firms in the industry, which they label industry effects.

(Abowd et al., 2012) can observe wage changes when individual workers move from one employer to another. They find very strong industry average firm effects, particularly for industries that have high average pay and low average pay. Among restaurants, for example, they find that 70 percent of the relatively low wages in the industry are attributable to firm effects, and only 30 percent to person effects. These findings suggest that a change in an industry's environment can have large effects on worker pay.

E.4 Effect on prices

As we have seen, previous prospective studies have made different assumptions on how much costs will affect prices and therefore also profits. Card and Krueger (1995) provide an extensive discussion of this issue. As they point out, from the point of view of an individual employer in a perfectly competitive industry, profits would be unaffected only in the extreme case in which firms can costlessly replace low-wage labor with high-skill labor and/or capital, and without cutting output. Since such substitutions are in fact costly, from this perspective a minimum wage

incidence of shirking by -.27 (0.0054), on a base of .032 (from Table 3.1).

increase would have to reduce profits. Firms do not envision a price increase as a solution, as it fears losing sales to its competitors.

A different result emerges when Card and Krueger consider the point of view of an industry as a whole. This perspective is necessary since the minimum wage increase applies to all the firms in an industry. Now, when individual firms respond to the prospect of reduced profits by raising their prices, they find that other firms are doing the same. Some of the price increases will stick and the industry will recapture some of the reduced profits. However, since demand for the industry's product is not fixed, this increase in price entails some reduction in product demand, implying that industry output (and therefore employment) will fall. In other words, the price increase will permit employers to recover only a portion of their reduced profits. Card and Krueger do not, however, take into account the income effect that will increase sales when a minimum wage applies to an entire economy, not just a single industry.

The evidence on whether profits do fall is extremely scant. The most important study remains the one in Card and Krueger (1995). These authors obtained mixed results when examining the effects of minimum wage changes on shareholder returns for fast-food restaurant chains. Using British data, Draca et al. (2011) find a small negative effect on profits. However, one segment of this study uses data for firms in the British residential care industry. Firms in this industry were not permitted to increase prices, making the results not very useful for other sectors. Harasztosi and Lindner (ming) examine a large (60 percent) and persistent increase in the Hungarian minimum wage, which affected much of manufacturing. These authors find that cost increases were entirely passed through, but employment did not change and profits did not fall. Of course, the relevance of the British and Hungarian studies for the U.S. is highly uncertain.

In our model, employers pass all of the increase in operating costs stemming from a minimum wage increase onto prices, after accounting for the above-mentioned turnover savings, automation, and productivity growth. Studies of

price effects of minimum wages are consistent with this model. These studies generally examine data on restaurants. Aaronson (2001) and Aaronson et al. (2008) both find complete pass through of costs. However, their data come from a period of much higher inflation, are based on a handful of observations per metro area, and they do not correct their standard errors for clustering. In contrast, Allegretto and Reich (2017) collected a large sample of restaurant price data in and near San Jose, before and after a 25 percent minimum wage increase in 2013 (from $8.00 to $10.00). Their results indicate that most of the costs are passed through to consumers in higher prices. Using scanner data from supermarkets, Renkin et al. (2019) find a similar effect for grocery prices.

E.5 Effect on profits and rents

Some economists have argued that many firms have captured above-normal profits in recent decades. An increase in the minimum wage could therefore reduce such economic rents. We attempted to include such an effect in our model, but were stymied by limited data on the proportion of reduced profits that would be borne within the study area. Our simulations did confirm that insofar as payroll cost increases are partly absorbed by profits, then the scale effect is smaller. The reduced profits have much less effect on the income effect because propensities to spend are low among shareholders and managers, and because much of the profit decline affects capital owners outside of the study area. As a consequence, including a fall in profits in our model would have led to more positive effects on employment.

Minimum wage increases will likely affect the composition of businesses within and among industries. Aaronson et al. (2018) find that minimum wage increases raise both exit and entry rates among restaurants, suggesting that entering firms arrive with a business model that is more oriented to the higher wage minimums. These higher-wage firms could be instituting business methods that improve productivity or improve product quality, or both. It is not possible

for U.S. to quantify these secondary effects, as they require more data on such adjustment mechanisms than are available.

Franchisee-franchiser relationships and commercial rental leases could also be altered by minimum wage increases. Franchises are particularly important among restaurants. In principle, franchisees could pass their increased costs to franchisers, either through a relaxation of fees or land rent. However, data on such changes are not available, to our knowledge. Effects on commercial rents are also difficult to detect, in part because of the lack of data and in part because such leases are typically of longer duration.

F Scale effects of increased prices on reduced sales of consumer goods

Economists use the term price elasticity of consumer demand to refer to the effect of an increase in prices on reducing consumer demand. Taylor and Houthakker (2010) report price elasticities for six categories of goods and services that together cover all of consumption. We adjust their health care elasticity to -0.20, to take into account changes in the structure of health care provision since the 1990s, and then compute a weighted average elasticity across the six categories Using personal consumption expenditure shares from the U.S. Consumer Expenditure Survey (McCully, 2011). The result is a price elasticity of consumer demand of -0.72[10].

This estimate is compatible with, but somewhat larger than, price elasticities estimated from aggregate panel data. Hall (2009), for example, obtains a consumer price elasticity of -0.50. On the other hand, our estimate is very close to

[10]Taylor and Houthakker's industry elasticities are based on regressions of U.S. panel data across over 300 cities and pooled over 1996-99. As we discuss below in Section 5.5, we do not expect that a substantial component of consumer sales will move outside the state's borders. Liu and Chollet (2006)'s review essay suggests that the price elasticity of demand for out-of-pocket individual healthcare expenses is -0.2. Our health care elasticity recognizes that employers shift their cost of health care on to employees. We also recognize that for those with subsidized coverage, increases in premium costs for lower-income families–who are more price-sensitive – are borne by the federal government.

that of Blundell et al. (1993).

G Income effects

We consider here the increased spending that derives from the higher income of low-wage workers. Our model takes into account the effects of taxes and reduction in public benefits on the affected workers' take-home pay and the rate at which their households spend (as opposed to save) income compared to others. Greater spending by consumers increases economic demand, which translates into increases in employment and economic activity.

We do not expect all of the increases in household incomes to translate into increased consumption demand. A substantial portion of minimum wage earners come from households in the middle of the household wage distribution. These households will save some of their increased income. The amount of such savings will depend on their current savings rates and on the extent to which they view the increase in income as permanent, rather than a short-term windfall. Economic research has found that changes in permanent income generate much higher consumption effects than changes that are, or are perceived as, transitory. Low wage-earners who are young and have more education may regard their low-wage status as transitory. These earners may regard a minimum wage increase as transitory.

Recent research has found that an increasing proportion of minimum wage workers are stuck in minimum wage careers (Boushey, 2005; Casselman, 2015). These results suggest that the proportion of workers who regard a minimum wage increase as constituting a one-time increase will be small. Moreover, economic theory and evidence suggests strongly that the distinction between permanent and transitory income does not apply to workers who are credit-constrained and whose households' assets are very limited (Achdou et al., 2018). The majority of minimum wage workers fit this description.

The IMPLAN model does not account for savings that come from transitory income. The considerations above indicate that any such effects are likely to be small. This is nonetheless a topic for future research.

H Model calculations: net effects on employment

Table 3.5 displays the results of our model for 2024. Note that the estimates in this table represent cumulative totals. They are estimated relative to the federal minimum wage in each year, and therefore capture the full effect of increases in the suggested federal minimum wage in previous years.

Panel A: Reduction in employment due to capital-labor substitution and productivity gains. Panel A in Table 3.5 shows our estimates for the reduction in the number of jobs due to both capital-labor substitution effects and productivity gains. With an assumed capital-labor substitution elasticity of 0.2 and a productivity effect of 0.005, we find a negative employment effect from these sources of about 490,000 jobs, or about 0.28 percent.

Panel B: Scale effects due to reduced consumer spending. Panel B in Table 3.5 presents our estimates of the reductions in jobs because of reduced consumer spending resulting from the higher prices generated by higher payroll costs [11].

Panel C: Income effect– cumulative increases in wages from proposed minimum wage increase. Panel C presents the estimated increase in jobs because of the income effect: increases in consumer demand deriving from increased incomes of low-paid workers. Our estimated income effect indicates an increase of about 1.7 million jobs, or 1.18 percent of the initial workforce.

[11] IMPLAN household spending model (proportional to city consumer spending patterns by household income level), using reduced consumer spending in Row 3 and forcing IMPLAN to apply 100 percent of the reduction in the city. See the appendix for details on IMPLAN modeling.

Panel D: Net effect. As we have previously emphasized, the substitution, productivity, scale and income effects in Parts A to C occur simultaneously, not sequentially. It is thus not correct to infer that the employment changes in Parts A to C actually occur. Net employment changes occur only to the extent that we register in Part D, after we add Parts A to C together to obtain the net effects.

As Part D indicates, we estimate a net cumulative increase of about 90,000 jobs by 2024, equivalent to 0.1 percent of the workforce. To put this estimate in context, the Congressional Budget Office projects that the U.S. workforce will grow 0.45 percent each year, from 2017 to 2024. (For more details see Appendix A2.)

The models that underpin Panels A to C indicate that the effects in each panel will likely differ by job wage rates. In particular, the automation and productivity effects in Part A will occur entirely among low-wage jobs. The scale and income effects of Parts B and C, however, will affect jobs throughout the state's consumer demand industries and among a much broader wage distribution. We have not been able to quantify these differences, as they depend on the relative concentration of scale and income effects in low-wage industries.

The key finding in Table 3.5 is thus that a $15 minimum wage will have a negligible effect on net employment in the U.S.

4 Employment analysis for Mississippi

A Effects on workers

Table 3.6 shows the estimated number and percentage of eligible workers who will receive pay increases by 2024. We estimate that about 504,000 Mississippi workers will receive a pay raise by 2024, or about 44.4 percent of the eligible workforce. Of these, about 342,000 will receive increases because their pay would otherwise be less than $15 in 2024 (the group directly affected by the law). Another 162,000

will receive pay increases because their pay will be slightly more than $15 by 2024 (the group indirectly affected by the law).

Table 3.6 also displays the additional earnings that affected Mississippi workers would receive: the estimated cumulative increase in affected workers' hourly wages, annual earnings, and percentage increase in annual earnings, as well as the cumulative total earnings increase for all affected workers. We estimate that the hourly wages of workers who will receive pay increases will rise by $2.74 by 2024. That amounts to an estimated additional $4,950 in earnings per year for these workers. In total, Mississippi workers will receive an additional $2,490 million in aggregate pay by 2024.

B Pay increases by industry

Table 3.7 shows the estimated distribution of affected workers across Mississippi's industries by 2024. In the first column, we show the percentage of the overall eligible workforce in each industry. The second column displays our estimate of the distribution across industries of workers getting a raise. The third column presents our estimate of the percentage of workers getting a raise within each industry.

About half of the workers getting increases are employed in just three industries: retail trade (19.6 percent), manufacturing (14.3 percent) and food services (13.3 percent). The two industries with the highest proportions of workers getting increases are: food services (82.8 percent) and retail trade (66.0 percent).

Table 3.8 shows our estimates of the increase in business operating costs (net of savings from reduced turnover). Across the entire Mississippi economy, we estimate that payroll costs would rise by 5.2 percent by 2024. Payroll costs in the restaurant industry would rise by 21.5 percent in the restaurant industry, 12.0 percent in retail, 13.5 percent in agriculture, and 3.8 percent in manufacturing.

Operating cost increases will be much smaller: 1.5 percent of the entire Mis-

sissippi economy, 8.2 percent in restaurants, 1.4 percent in retail, 2.3 percent in agriculture and 0.7 percent in manufacturing.

C Effects on Mississippi employment

We turn next to our estimates of the effects of a $15 minimum wage by 2024 on Mississippi employment, using the same method we described above for the U.S. Table 3.9 displays the results of our model.

Panel A: Reduction in employment due to capital-labor substitution and productivity gains. Panel A in Table 3.9 shows our estimates for the reduction in the number of jobs due to both capital-labor substitution effects and productivity gains. With an assumed capital-labor substitution elasticity of 0.29 and a productivity effect of 0.005, we find a negative employment effect of about 6,000 jobs from these channels.

Panel B: Scale effects due to reduced consumer spending. Panel B in Table 3.9 presents our estimates of the reductions in jobs associated with reduced consumer spending because of price increases. Our estimate of the reduction in consumer spending from price increases departs in one detail from our estimate for the U.S. as a whole. For Mississippi, we estimate that each 1 percent increase in consumer prices results in a -0.92 percent decline in consumer spending. We use this higher demand elasticity because incomes in Mississippi are lower than in the U.S. as a whole. The result is an estimated negative effect of 9,000 jobs from this channel.

Panel C: Income effect - cumulative increases in wages from proposed minimum wage increase. Panel C of Table 9 presents the estimated job increases because of the income effect: increases in consumer demand deriving from increased incomes of low-paid workers. We estimate that the income effect will generate 17,000 jobs in Mississippi.

Panel D: Net effect. As we have previously mentioned, the substitution productivity, scale, and income effects in Parts A to C occur simultaneously, not sequentially. It is thus not correct to infer that the employment changes in Parts A to C actually occur. Net employment changes occur only to the extent that is registered after we add Parts A to C together to obtain the net effects.

Panels A to C tell U.S. that the net effects will likely differ by job wage rates. In particular, the automation and productivity effects in Part A will occur entirely among low-wage jobs. The scale and income effects of Parts B and C, however, will affect jobs throughout the state's consumer demand industries and among a much broader wage distribution. We have not been able to quantify these differences, as they depend on the relative concentration of the scale and income effects in low-wage industries.

Panel D of Table 3.9 presents our estimate of the net change in employment. We estimate a small cumulative net gain in employment, due to the policy, of 2,000 jobs by 2024, equivalent to 0.1 percent of total employment. To put this estimate in context, we project that Mississippi will grow annually by 0.27 percent from 2016 to 2024.

The key finding in Table 3.9 is that a $15 minimum wage will have a very small positive net effect on employment in Mississippi.

Like all forecasts, our estimates of the benefits and costs are subject to some uncertainty. First, economic conditions, such as employment and wage growth in the absence of the policy, may differ in future years from the standard forecasts that we rely upon in this report. For example, in recessions employment falls and wages do not grow as quickly. Our cost estimates might then be somewhat larger, but then so would our benefit estimates. Our estimates of the net effects are therefore likely to change, but not by a large amount. Second, our estimates rely on parameters that are themselves estimated with some uncertainty. The proposed policy would result in substantial benefits to low-wage workers and their families, raising wages for 41.5 million workers by 2024. Annual pay for these workers will

increase by 17.3 percent or$3,470 by 2024. These pay increases in pay will raise overall wages in for-profit businesses by only 0.6 percent in the U.S. This amount is surprisingly small because many of the workers who are now paid below$15 are already paid above the current minimum wage, and because the pay of low-wage workers makes up a small share of total payroll costs. Businesses will absorb the additional payroll costs partly through savings on employee turnover costs, higher worker productivity gains, and some automation. Most of the increase in costs will likely be passed on to consumers via increased prices. Since labor costs make up only about one-fourth of operating costs, consumer prices will increase only slightly – about 0.6 percent over the entire phase-in period. Prices will be most affected in the restaurant industry, where they will increase by 4.3 percent over the entire phase-in period.

These higher prices by themselves would somewhat reduce consumer sales and reduce the demand for labor. But simultaneous positive effects on increased consumer spending from workers receiving wage increases will offset these negative effects.

After taking into account all of these factors, we estimate that the proposed minimum wage policy would increase overall U.S. employment (as a percent of total employment) by 0.1 percent by 2024, over the baseline. This estimate is cumulative (and so will be spread over the phase-in period). In comparison, employment in the U.S. is projected to grow 0.45 percent annually in the same time period. We also find a similar employment effect for Mississippi.

In sum, a $15 minimum wage by 2024 would substantially improve living standards for nearly 30 percent of the U.S. workforce (and 44 percent of Mississippi's) without generating a significant net adverse employment effect. The minimum wage increase will be paid for primarily by induced efficiencies (more automation, productivity gains, and turnover savings) and slight price increases borne by all consumers. Based on our analysis, we conclude that the proposed minimum wage will have its intended effects in improving incomes for low-wage

workers. Any effects on employment and overall economic growth are likely to be small. The net impact of the policy will therefore be positive.

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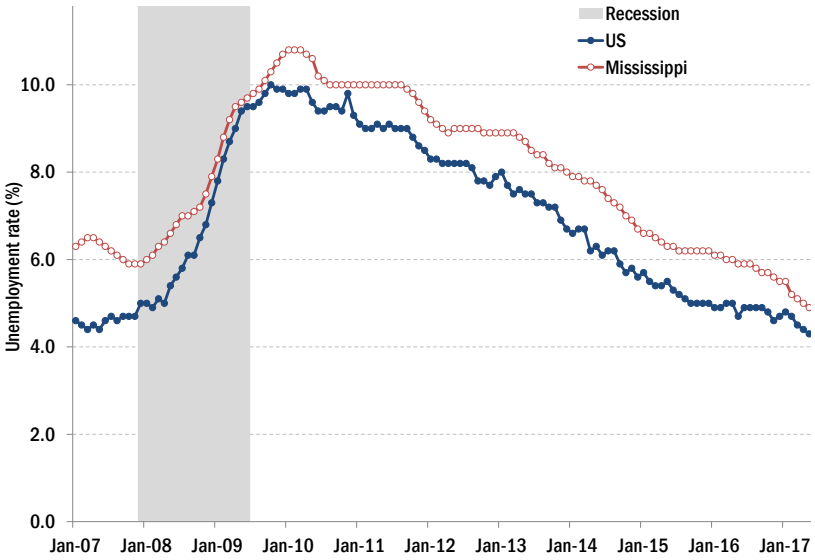
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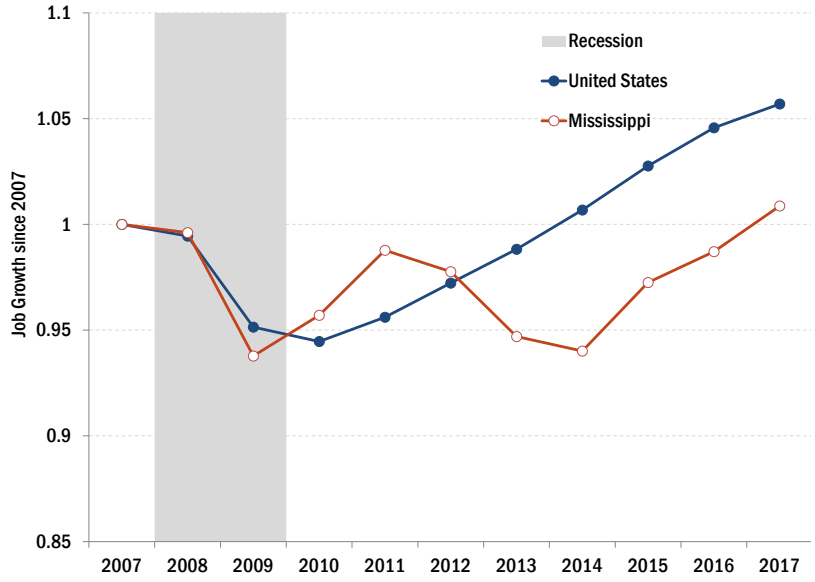
Wolfers, J. and Zilinsky, J. (2015). Higher wages for low-income workers lead to higher productivity. Technical report, Peterson Institute for International Economics. 152

Figure 3.1 – Monthly unemployment rates 2007-2017



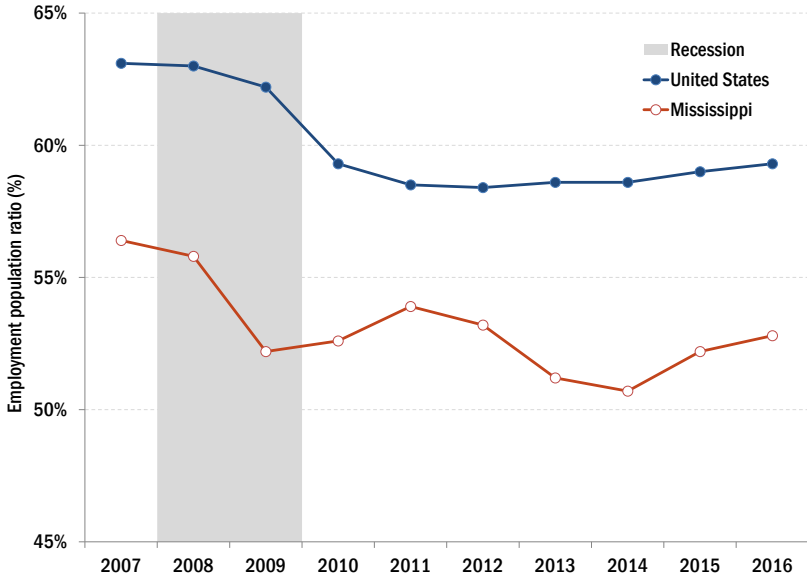
Source: Labor force statistics from the CPS for the U.S. Local Area Unemployment statistics for MS. The series are seasonally adjusted.

Figure 3.2 – Employment changes 2007-2016



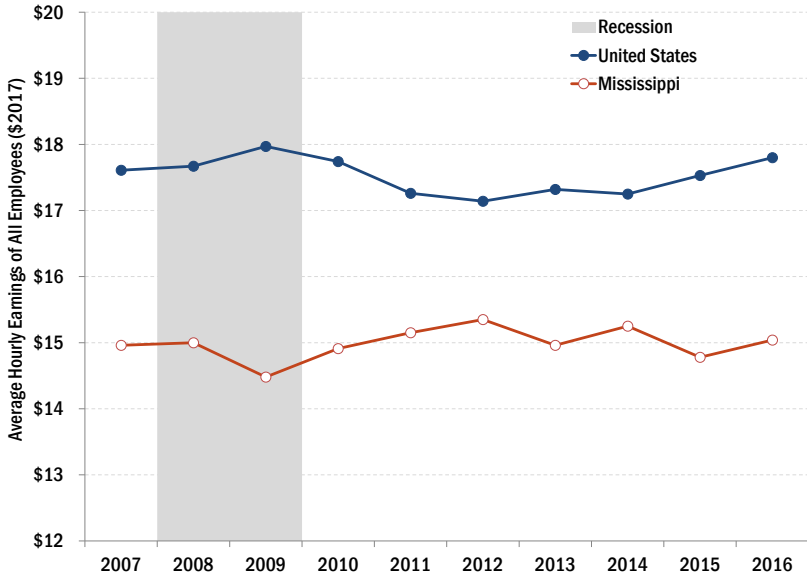
Source: Authors' calculations of growth in total nonfarm payrolls (annual averages) since 2007 from Current Employment Statistics.
Note: National and state employment levels are indexed to 1 in 2007.

Figure 3.3 – Employment population ratio, 2007-2016, 25-54 years-old



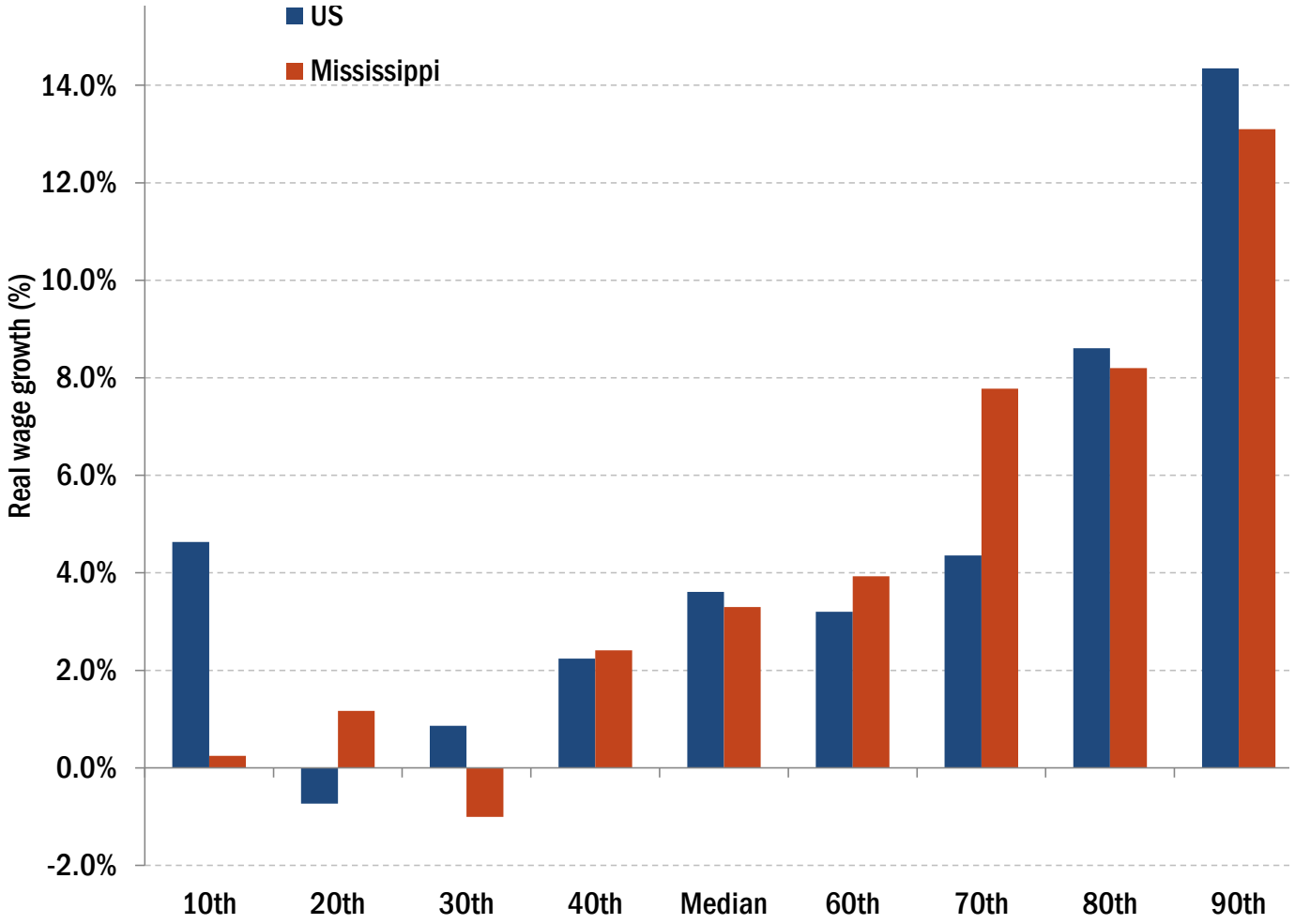
Source: Labor force statistics from the CPS for the U.S. Local Area Unemployment statistics for MS. The series are seasonally adjusted.

Figure 3.4 – Median hourly pay, adjusted for inflation ($2016)



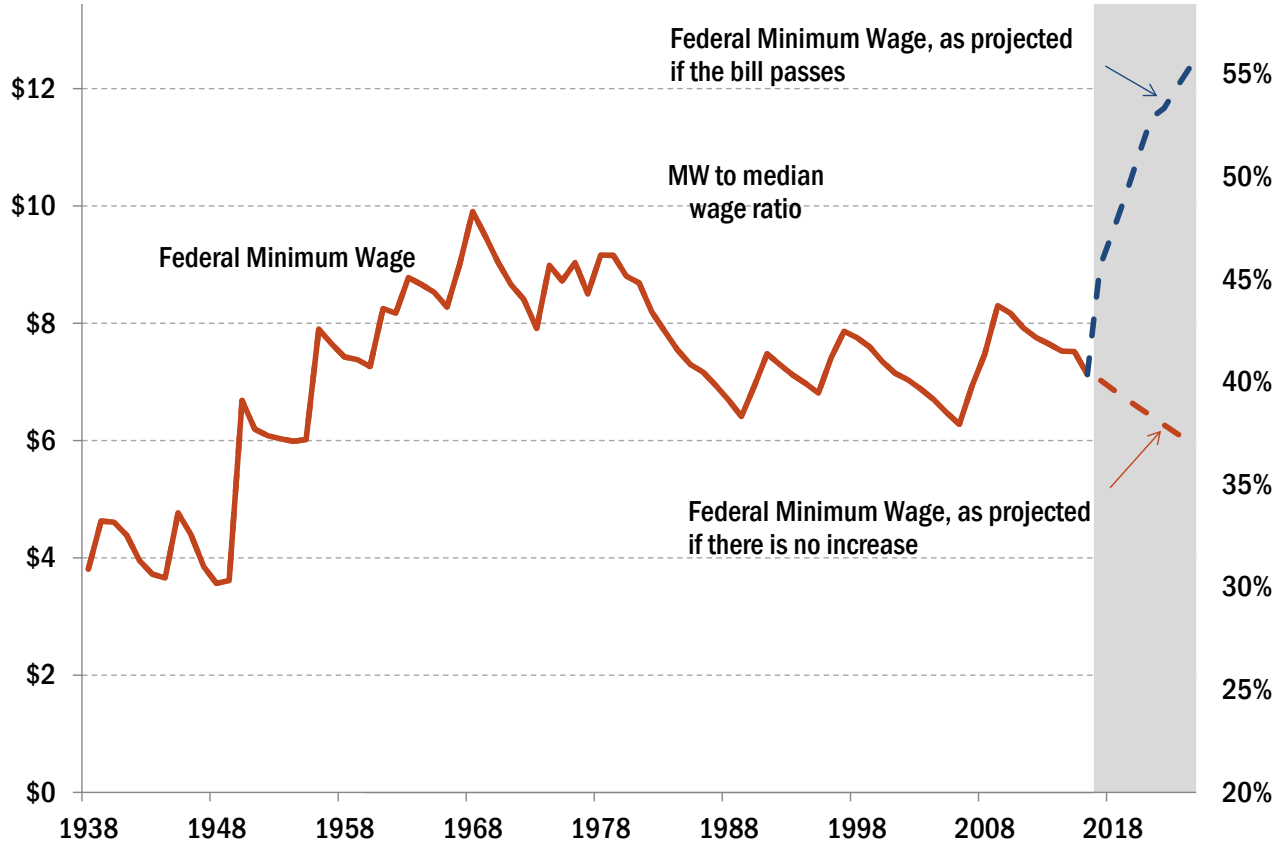
Source: Current Population Survey data provided by the Economic Policy Institute.
Note: CPI-U-RS is used to translate nominal wages into $2016.

Figure 3.5 – Real wage growth in the U.S. and Mississippi by wage percentiles, 2000-2016



Source: Authors' analysis of Current Population Survey data provided by the Economic Policy Institute.
Note: CPI-U-RS is used to translate nominal wages into $2016.

Figure 3.6 – Federal minimum wage history with projections to 2024 (in $2017)



Source: Top dashed line plots the evolution of the federal minimum wage if the Raise the Minimum Wage Bill of 2017 is enacted. Bottom dashed line represents the future evolution of the minimum wage in the absence of any legislated increase.

Note: All data are in $2017 and adjusted using the CPI-U-RS series, as published by the Bureau of Labor Statistics. Projections (dashed lines) assume an annual inflation of 2.3% in 2017-2018, 2.4% in 2019-2020, and 2.4% in 2021-2027, according to CBO projections for the consumer price index, see p.40: http://www.cbo.gov/sites/default/files/115th-congress-2017-2018/reports/52370-outlook.pdf.

Figure 3.7 – Minimum wage model

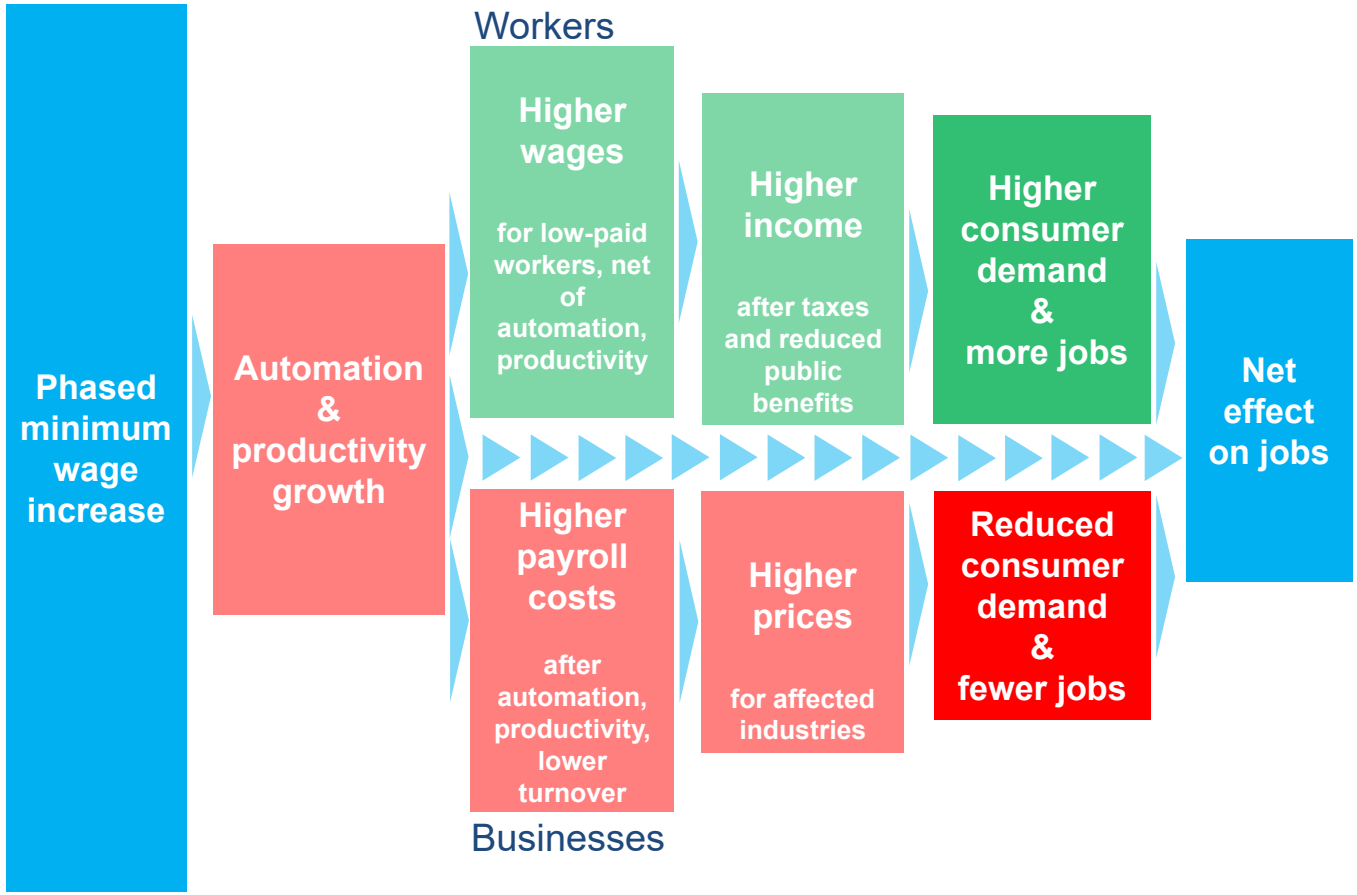
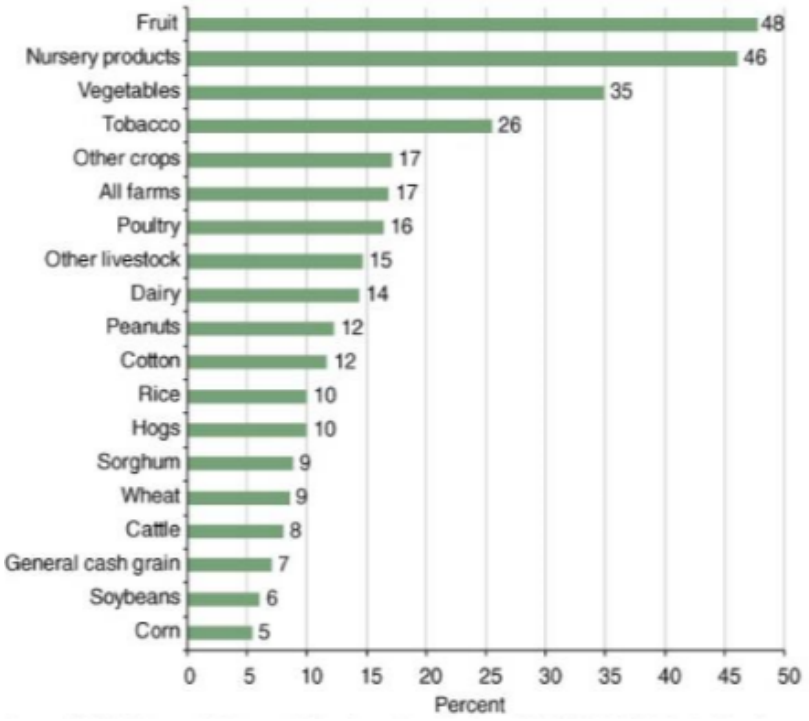


Figure 3.8 – Hired labor accounts for a large share of production costs for some crops



Source: USDA, Economic Research Service using data from USDA's 2006-2010 Agricultural Resource Management Survey.

Table 3.1 – Proposed federal minimum wage schedule

Date	New minimum wage (nominal)	New minimum wage ($2016)	New tipped minimum wage (nominal)	New tipped minimum wage ($2016)
July 1, 2017	$9.25	$9.04	$4.15	$2.76
July 1, 2018	$10.10	$9.65	$5.30	$3.45
July 1, 2019	$11.00	$10.27	$6.45	$4.10
July 1, 2020	$12.00	$10.95	$7.60	$4.72
July 1, 2021	$13.00	$11.58	$8.75	$5.31
July 1, 2022	$13.50	$11.75	$9.90	$5.86
July 1, 2023	$14.25	$12.12	$11.05	$6.39
July 1, 2024	$15.00	$12.46	$12.20	$6.90

Source: Raise the Wage Bill of 2017.

Table 3.2 – Cumulative benefits for U.S. workers by 2024

Percent of the workforce receiving increases	29.2
Total number of workers receiving increases (millions)	41.47
Number of workers affected directly (millions)	22.48
Number of workers affected indirectly (millions)	18.98
Average hourly wage increase ($2016)	$2.08
Annual earnings increase for workers receiving increases ($2016)	$3,470
Percent earnings increase for workers receiving increases ($2016)	17.3
Total aggregate increase in wage (billions, $2016)	$144

Source: Authors' calculations based on Cooper (2017) analysis of CPS data.

Note: Eligible workers are those that work in the city/county where the new minimum wage policy is implemented. Directly affected workers earned between 80 percent of the old minimum wage and 100 percent of the new minimum wage. Indirectly affected workers earned between 100 percent and 115 percent of the new minimum wage. Average annual earnings per worker, not per job.

Table 3.3 – Cumulative impacts for workers by major industries in the U.S. by 2024

	Percent of all workers getting raises	Percent of industry workers getting raises	Percent change in industry's payroll costs
Agriculture, Forestry, Fishing, Hunting	1.5	42.6	4.8
Mining	0.2	10.7	0.5
Construction	4.2	20.7	1.3
Manufacturing	8.5	22.8	1.3
Wholesale trade	1.7	20.5	1.2
Retail trade	18.2	47.0	4.2
Transportation, Warehousing, and Utilities	3.9	21.0	1.3
Information	1.1	17.1	0.7
Finance, Insurance, Real Estate, and Rental and Leasing	3.7	16.1	0.6
Professional, Scientific, and Management	2.2	9.5	0.3
Administrative and Waste Management Services	5.8	40.2	3.7
Educational Services	6.8	20.5	1.2
Health Services	10.5	25.5	1.5
Social Assistance	2.8	38.9	3.8
Arts, Entertainment, Recreation, and Accomodation	4.9	46.2	4.6
Food services	15.6	67.8	11.3
Other services	5.9	38.9	3.8
Public administration	2.5	14.3	0.7
Total	100	29.2	1.9

Source: Authors' calculations based on Cooper (2017) analysis of CPS data.

Note: The percent change in payroll costs includes payroll taxes and workers' compensation as well as employee turnover savings. The percent change in payroll costs presented here does not take into account the reduction in the total wage bill due to substitution and productivity-based job losses. We do integrate these effects into the model calculations.

Table 3.4 – Costs impacts for U.S. industries by 2024

	Percent change payroll costs	labor costs as percent of operating costs	Percent change in operating costs and prices
Agriculture, Forestry, Fishing, Hunting	4.8	16.8	0.8
Mining	0.5	16.8	0.1
Construction	1.3	33.7	0.5
Manufacturing	1.3	17.4	0.2
Wholesale trade	1.2	8.8	0.1
Retail trade	4.2	12.0	0.5
Transportation, Warehousing, and Utilities	1.3	28.3	0.4
Information	0.7	21.2	0.2
Finance, Insurance, Real Estate, and Rental and Leasing	0.6	16.1	0.1
Professional, Scientific, and Management	0.3	47.6	0.2
Administrative and Waste Management Services	3.7	44.6	1.7
Educational Services	1.2	56.2	0.7
Health Services	1.5	49.8	0.8
Social Assistance	3.8	49.8	1.9
Arts, Entertainment, Recreation, and Accomodation	4.6	34.3	1.6
Food services	11.3	38.3	4.3
Other services	3.8	44.5	1.7
Public administration	0.7	52.4	0.4
All sectors	1.9	29.1	0.6

Source: U.S. Census, Annual Wholesale Trade Report and Cooper (2017) analysis of CPS data.

Note: The percent change in payroll costs includes payroll taxes and workers' compensation as well as employee turnover savings. In this table, the percent change in payroll costs does not take into account the reduction in total wage bill due to substitution and productivity gains job losses. Those effects are, however, integrated later, in the calculations we perform in our model.

Table 3.5 – Cumulative employment changes, U.S., by 2024

A. Substitution effects: Reduction in wage bill due to automation and productivity gains	
Change in number of jobs from substitution effects and productivity gains (thousands)	-490
Percent reduction in number of jobs from substitution and productivity gains	-0.28%
B. Scale effect: reduction in consumer spending	
Change in number of jobs due to scale effect (thousands)	-940
Percent reduction in number of jobs due to scale effect	-0.7%
C. Income effect: increase in consumer good	
Change in number of jobs due to income effect (thousands)	1,520
Percent increase in number of jobs due to income effect	1.1%
D. Cumulative net change in employment	
Net change in employment (thousands)	90
Net change in employment, as a percent of total employment	0.1%

Authors' calculations using the IMPLAN economic impact model.

Table 3.6 – Cumulative benefits for Mississippi workers by 2024

Percent of the workforce receiving increases	44.4
Total number of workers receiving increases (thousands)	504
Number of workers affected directly (thousands)	342
Number of workers affected indirectly (thousands)	162
Average hourly wage increase ($2016)	$2.74
Annual earnings increase for workers receiving increases ($2016)	$4,950
Percent earnings increase for workers receiving increases ($2016)	24.6
Total aggregate increase in wage (billions, $2016)	$2.49

Source: Authors' calculations based on Cooper (2017) analysis of CPS data.
Note: Directly affected workers earned between 80 percent of the old minimum wage and 100 percent of the new minimum wage. Indirectly affected workers earned between 100 percent and 115 percent of the new minimum wage. Average annual earnings per worker, not per job.

Table 3.7 – Cumulative impacts for workers by major industries in the U.S. by 2024

	Percent of all workers getting raises	Percent of industry workers getting raises	Percent change in industry's payroll costs
Agriculture, Forestry, Fishing, Hunting	2.1	64.8	13.5
Mining	0.1	7.6	0.3
Construction	4.3	35.4	3.6
Manufacturing	14.3	39.5	3.8
Wholesale trade	1.8	39.9	3.0
Retail trade	19.6	66.0	12.0
Transportation, Warehousing, and Utilities	2.9	21.8	1.8
Information	1.0	36.3	2.1
Finance, Insurance, Real Estate, and Rental and Leasing	2.7	29.5	2.2
Professional, Scientific, and Management	1.5	26.9	1.2
Administrative and Waste Management Services	4.3	61.8	10.5
Educational Services	7.2	29.6	3.4
Health Services	10.2	36.4	3.5
Social Assistance	2.6	62.2	10.8
Arts, Entertainment, Recreation, and Accomodation	3.6	63.7	13.3
Food services	13.3	82.8	21.5
Other services	4.6	49.1	5.0
Public administration	3.9	31.6	3.0
Total	100	44.4	5.2

Source: Authors' calculations based on Cooper (2017) analysis of CPS data.

Note: The percent change in payroll costs includes payroll taxes and workers' compensation as well as employee turnover savings. The percent change in payroll costs presented here does not take into account the reduction in the total wage bill due to substitution and productivity-based job losses. We do integrate these effects into the model calculations.

Table 3.8 – Costs impacts for Mississippi industries by 2024

	Percent change payroll costs	labor costs as percent of operating costs	Percent change in operating costs and prices
Agriculture, Forestry, Fishing, Hunting	13.5	16.8	2.3
Mining	0.3	16.8	0.1
Construction	3.6	33.7	1.2
Manufacturing	3.8	17.4	0.7
Wholesale trade	3.0	8.8	0.3
Retail trade	12.0	12.0	1.4
Transportation, Warehousing, and Utilities	1.8	28.3	0.5
Information	2.1	21.2	0.4
Finance, Insurance, Real Estate, and Rental and Leasing	2.2	16.1	0.4
Professional, Scientific, and Management	1.2	47.6	0.6
Administrative and Waste Management Services	10.5	44.6	4.7
Educational Services	3.4	56.2	1.9
Health Services	3.5	49.8	1.8
Social Assistance	10.8	49.8	5.4
Arts, Entertainment, Recreation, and Accomodation	13.3	34.3	4.6
Food services	21.5	38.3	8.2
Other services	5.0	44.5	2.2
Public administration	3.0	52.4	1.6
All sectors	5.2	29.1	1.5

Source: U.S. Census, Annual Wholesale Trade Report and authors' analysis of Current Population Survey, Occupation Employment statistics and Quarterly Census for Employment and Wages data.

Note: The percent change in payroll costs includes payroll taxes and workers' compensation as well as employee turnover savings. In this table, the percent change in payroll costs does not take into account the reduction in total wage bill due to substitution and productivity gains job losses. Those effects are, however, integrated later, in the calculations we perform in our model.

Table 3.9 – Cumulative employment changes, Mississippi, by 2024

A. Substitution effects: Reduction in wage bill due to automation and productivity gains	
Change in number of jobs from substitution effects and productivity gains	-6,000
Percent reduction in number of jobs from substitution and productivity gains	-0.7%
B. Scale effect: reduction in consumer spending	
Change in number of jobs due to scale effect	-9,000
Percent reduction in number of jobs due to scale effect	-0.7%
C. Income effect: increase in consumer good	
Change in number of jobs due to income effect (thousands)	17,000
Percent increase in number of jobs due to income effect	1.3%
D. Cumulative net change in employment	
Net change in employment (thousands)	2,000
Net change in employment, as a percent of total employment	0.1%

Authors' calculations using the IMPLAN economic impact model.

Appendix A

Minimum Wages and Racial Inequality: 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[1] in three ways: (i) it starts in 1950, allowing for greater historical depth in the study of minimum wage effects than before;[2] (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[3], 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 the minimum wage legislation does not vary by gender anymore. We build the database in nominal terms at the monthly level, then collapse it at the annual level. Both databases and Stata do files used to create them are publicly available.[4] 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.[5]

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[6]. We digitize and analyze in particular the information contained in Volume II, "State Minimum Wage Laws, 1950-1980", written 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).

[1] 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)

[2] Vaghul and Zipperer (2016) starts in May 1974, and Neumark (2018) in 1960

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

[4] See http://clairemontialoux.com.

[5] 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.

[6] Volume 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.

Classification of industries by date of FLSA coverage. Which industry is covered by which amendment of the Fair Labor Standards Act? Table A1 shows the list of industries available in CPS 1962-1981 (see section B) 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.[7] This classification is necessarily imperfect as it has to deal on one hand with the complexity of the minimum wage legislation and its grey areas[8] and on the other hand by the characteristics we can observe or not in the CPS. Our objective is to make the best choices as possible given those constraints and 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) to newly covered industries (covered in 1966). We show that our main results are robust to slight changes in this classification.

The 1938 Fair Labor Standards Act stipulates 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[9], we consider that the following industries are covered by the 1938 FLSA: mining, manufacturing (durable and non-durable), transportation, communication and other utilities[10], wholesale trade, finance, insurance and real estate, and business and repair services. These industries form our control group.

The 1961 Amendments to the Fair Labor Standards Act extend coverage to all employees of retail trade enterprises[11] with sales over $1m, and to small retailers under certain conditions[12]. They also increase coverage to construction enterprises with sales over $350,000. Retail trade establishments and construction are therefore only partially covered in 1961, and are further affected by the 1966 amendments, and subsequent amendments. [13] Since in the CPS we do not

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

[8] The minimum wage legislation does not only vary by industry. It also varies e.g. in the retail sector by a sales threshold per establishment, see below paragraph on 1961 Amendments. The legislation is also different for workers working overtime, varies by age, etc.

[9] 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 in retail and service establishments.

[10] A minority of workers in transportation were however not covered by the 1938 FLSA. Some transportation workers, originally not covered, became covered before our analysis starts, and it is therefore right for us 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 starts, as e.g. workers transporting fruits and vegetables from farm to first processing, or those transporting other workers to and from farms to harvesting purposes. Since those workers represent a minority of transportation workers, and since we are not able to identify them in the CPS data, we believe this approximation is not a threat to our empirical strategy.

[11] Retail trade excludes here eating and drinking places which were specifically exempted from the minimum wage in 1961.

[12] 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

[13] The 1966 amendments extended coverage to retail trade entreprises with sales over $500,000.

have the information 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 vs. by later amendments. We therefore have to make a choice on how to classify retail trade and construction workers as a whole. Since for both types of workers, the 1961 amendments were the most important ones in terms of coverage extension, 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 1966.[14]

The 1966 Amendments to the Fair Labor Standards Act extended coverage to enterprises engaged in "a common business practice" that includes hospitals, institutions engaged in the care of the sick, aged, mentally ill or physically handicapped, as well as elementary and secondary schools, whether public or private,[15], to agriculture, and to 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. We discuss below where we had to make choices, their strengths and their limits.

Agriculture. Agriculture was covered for the first time in 1967. However, some exemptions applied in the agricultural sector, mainly for small farms[16]. 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 1.2).

Services. There are two potential concerns about classifying restaurants, hotels, laundries and other personal services, entertainment and recreation services as industries covered in 1966: one might worry that these services were (i) already partially covered by the 1961 amendments, and (ii) that the 1966 amendments were still realizing partial coverage for those sectors, since service enterprises with annual sales below $500,000 were not covered. Regarding (i): Although it is true that the 1961 Amendments introduces 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,

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, Washer (2010) for a history of minimum wage law 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.

[14] 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.

[15] 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.

[16] There were four notable exemptions in agriculture: (i) employees of farms employing less than 500 mandays of nonemxept labor in the highest quarter of the pervious year; (ii) family members; (iii) local hand harvest laborers paid on a piece rate basis who worked less than $<$ 13 weeks in 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.

restaurants, laundry and dry cleaning establishments, seasonal and recreational establishments. Therefore, a closer reading of the 1961 amendments allow us to consider that the services listed above were not covered by the 1961 amendments and started to be covered in 1966. Regarding (ii): What the 1966 amendments does is to introduce coverage for those sectors in enterprises with sales greater than $500,000. Those services were therefore partially treated in 1966, 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 1966. Hourly wages of tipped employees may legally be adjusted to reflect allowance of up to 50 % of the minimum wage for tips actually received. Since we observe annual earnings in the CPS, that includes all tips, we do not believe the fact that the tipped minimum wage was introduced in those industries be 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. [17] Importantly, we did not classify state and local government workers as covered in 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).[18] Coverage was extended to state and local government workers from January 1, 1986 after U.S. Supreme Court reversal. [19]

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 mini-

[17]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 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.

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

[19]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 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 the coverage was overturned by U.S. Supreme Court, the Employment Standard's Administration estimated that there were only 116,000 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've focused our analysis on the private sector, and we exclude all public administration workers.

Figure A1 – List of industries used in March CPS (1962-1980), and year of coverage by FLSA

1	Agriculture	1966
2	Forestry and Fishing	1966
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	1966
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	1966
16	Entertainment and Recreation Services	1966
17	Nursing homes and other professional services	1966
18	Hospitals	1966
19	Schools and other educational services	1966
20	Federal government	1974
21	State or local government	1986
22	Postal service	1938
23	Other	1938

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

mum 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 of 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:[20]

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

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:

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

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:

$$mw_{ymj} = \frac{1}{\sum_{S=1}^{M} N_{jS}} \sum_{S=1}^{M} mw_{ymjS} \tag{A.3}$$

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

[20]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 section sec: March CPS). Instead, we have information on the number of workers at the state-group, industry and gender 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 scrap the text files from https://www2.census.gov/programs-surveys/popest/tables/; from 2000 to 2009, we download "$st_e st00int - 01.csv$" 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.

the number of workers N_j within control (c) or treatment (t) industries:

$$mw_{ymT} = \frac{1}{\sum_{T=j_c}^{t} N_{j_T}} \sum_{T=j_c}^{j_t} mw_{ymj} \tag{A.4}$$

Finally, we convert nominal minimum wage rates into real minimum wage rates using the CPI-U-RS.[21]

[21]The annual CPI-U-RS series are available since 1947 at: https://www2.census.gov/programs-surveys/demo/tables/p60/ (as of March 13 2018), folder 259.

B March CPS (1962-1981)

This paper uses data from the March Current Population Survey (CPS)[22] to analyze the effect of the 1966 Fair Labor Standards Act on annual wages, employment and on racial inequality. As noted in IPUMS documentation[23], 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, those files were preserved in the data archive at the Center for Demography and Ecology at the University of Wisconsin. The most recent version of those early files has been made public by IPUMS on February 23, 2009[24]. In particular, the IPUMS version of the CPS early files has an harmonized industry variable.

A Sample of interest

Figure A2 displays how we divide the CPS sample into four categories 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 include all minors, i.e. children,[25] and teenagers below 21.[26], and older individuals (aged 66 and above). We also remove self-employed workers from our universe of interest, since 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 [27], 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 1966 reform on wages, and on the racial earnings gap (section 3), is conducted on prime age workers (25-55).

Unemployed or not in the labor force. When analyzing the employment effects of the 1966 reform (section 5), we look at the probability of being employed, vs. unemployed or not in the labor force, and restrict the sample of analysis to adults aged 25-55.

[22]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

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

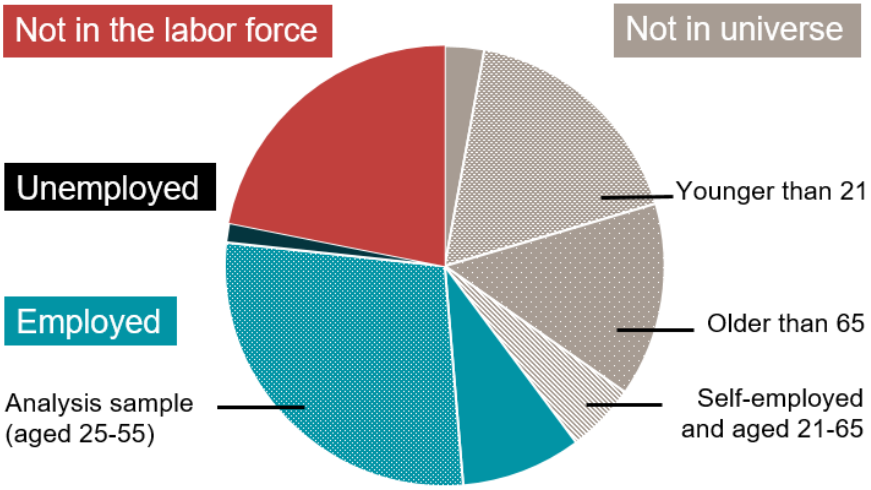
[24]See https://cps.ipums.org/cps-action/revisions

[25]From March CPS 1962 to 1979, the lowest age cut-off for employment questions us 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.

[26]The minimum wage legislation for minors is very different from the minimum wage for adults, and we've excluded teenagers so that we do not introduce this layer of heterogeneity in the treatment.

[27]Starting in 1967, the minimum wage is 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 A2 – Analysis sample, before the reform (1966)



Source: Authors' analysis of March CPS 1967.

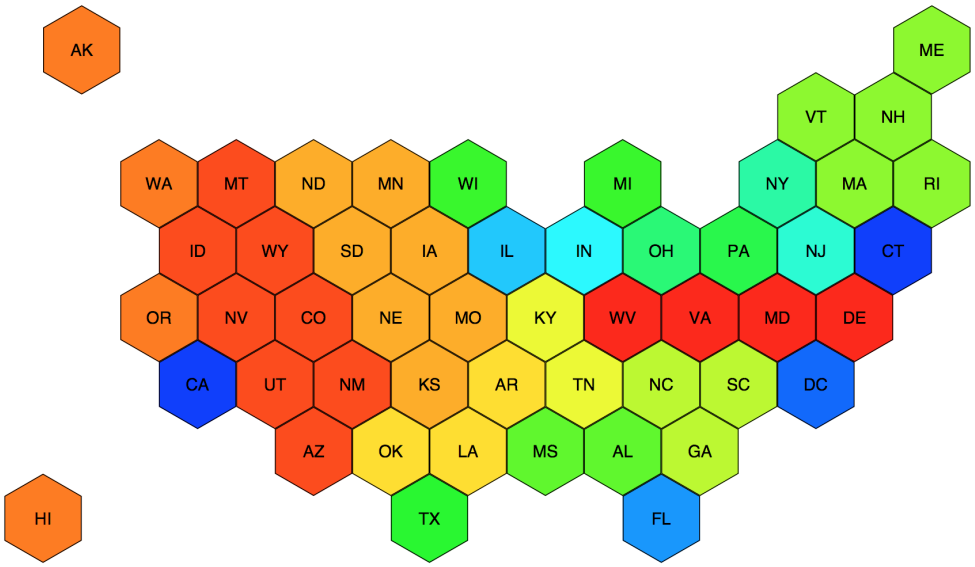
B State crosswalks

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, and it's impossible to uniquely identify the state to which the interviewee belong. For example, in March CPS 1968-1971, Minnesota and Iowa are identified as a group: we don't know whether the individuals surveyed in those years are living in Minnesota or Iowa, we just know they live in one of those two states. In addition, the state grouping is different across years. To overcome the state grouping and the inconsistency in the coding of the state variable across time, we've 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 table A1). 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 (figure A3). To a certain extent, the state groups share similar economic conditions. A detailed crosswalk, for every year of the CPS, is available online at: http://clairemontialoux.com.

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,[28] 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

[28]Our sample of interest is the sample we use to perform our analysis: Adults 25-55, employed, not self-employed or unpaid family worker, not in grouped quarters, has positive, non-missing income variable, works more than 13 weeks a year and more than 3 hours last week, has a non-missing industry or occupation code.

Figure A3 – State groups used in March CPS (1962-1980)



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

Table A1 – List of state groups used in March CPS (1962-1980)

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

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

1972 (0.04%).These observations are dropped from our analysis. Given the small number of workers involved, we believe this does not introduce any bias in our results.

C Industry crosswalks

There are several industry codes available in CPS IPUMS, and their classification varies across time. We create our own industry variable, harmonized across time, 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: IND, [29]from March CPS 1962-1967, and IND1950, [30] from March 1968-1981. 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. [31]. However, the two industry codes differ by their precision: Codes for March CPS 1962-1967 are two digits, and the the classification scheme uses 44 codes. Codes for March CPS 1968-1981 are three digits, and the 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. The detailed industry crosswalk is available online at: http://clairemontialoux.com.

D 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). [32] Among employed individuals in March CPS 1962-1972,[33] 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

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

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

[31]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.

[32]This is consistent with assuming that the distribution of incomes Pareto distributed, with a pareto coefficient of 3, that is typically used in the literature on top-income earners (**?**).

[33]We refer here to employed individuals in our analysis sample: Adults 25-55, employed, not self-employed or unpaid family worker, not in grouped quarters, has positive, non-missing income variable, works more than 13 weeks a year and more than 3 hours last week, has a non-missing industry or occupation code.

share is consistently below 5% (except for the years 1992-1994 where it is between 5% and 8%). [34]

After 1996, topcoded values are replaced with values that vary with individual characteristics (gender, race, and full-time/part-time status). [35]

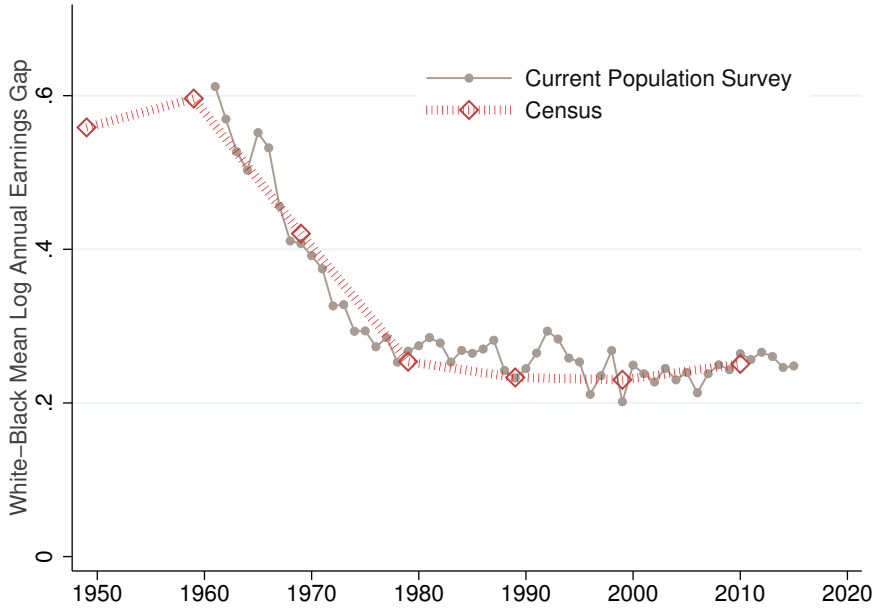
E Comparing CPS and Census data

We compare decennial Census of Population from 1960 to 1980 (covering earnings data for 1959-1979) and the March CPS from 1962 to 1981 (covering earnings for 1961-1980) data to check the quality of CPS files. Employment counts are similar across the two data sets, see table A2. On notable exception, however, are 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 CPS 1962 and 1963 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. Table A2 shows that the 1960 Census and the March CPS 1962 and 1963 match well in terms of the relative shares of white and black workers, male and female workers, or their annual earnings. We exclude the March CPS 1963 from our analysis as it also suffers from a lower number of observations, and lacks demographic information (such as education level) for the entire population. Finally, we show that the unadjusted racial earnings gaps are remarkably aligned in the Census and in the March CPS from 1960 to today (see figure A4).

[34] The stata do files that deal with topcoding are available on:http://clairemontialoux.com.

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

Figure A4 – 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, worked more than 3 hours last week, not self-employed, not in the armed forces.
Notes: The racial gap is calculated as difference in the average log annual earnings of black workers and the 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 e.g. in the 1950 Census in 1949, and in the 1962 in 1961. For the ACS, the reference period is the past 12 months, and we report estimates of the racial gap in 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.

Table A2 – Observations, employment, and wages in the March CPS and in Censuses

	Observations	Employment	Employment shares				Earnings ($2017)			
			White	Black	Men	Women	White	Black	Men	Women
March CPS										
1962	13,540	24,086,400	90%	10%	68%	32%	37,176	19,523	42,029	21,113
1963	9,638	22,277,274	90%	10%	68%	32%	37,607	18,865	42,412	21,267
1964	14,222	34,344,403	89%	11%	68%	32%	38,736	21,529	44,216	21,343
1965	14,126	34,637,727	89%	11%	68%	32%	39,677	22,997	45,379	22,158
1966	30,113	37,407,666	89%	11%	68%	32%	41,196	23,168	47,224	22,461
1967	19,191	38,490,848	89%	11%	68%	32%	42,575	24,522	49,036	23,091
1968	30,277	39,451,389	89%	11%	66%	34%	43,219	26,019	50,127	24,098
1969	30,808	40,044,846	89%	11%	66%	34%	44,575	28,242	52,070	24,935
1970	29,626	40,963,562	90%	10%	66%	34%	47,062	29,253	55,248	26,015
1971	29,130	40,594,657	89%	11%	65%	35%	47,563	30,486	55,870	26,946
1972	28,214	41,861,238	90%	10%	65%	35%	47,460	30,936	55,969	27,039
1973	28,025	42,659,268	89%	11%	64%	36%	49,744	33,601	59,060	28,255
1974	27,620	43,773,753	90%	10%	64%	36%	49,962	33,810	59,852	28,155
1975	26,474	43,108,371	90%	10%	63%	37%	48,364	34,284	58,235	27,912
1976	28,407	44,987,015	90%	10%	62%	38%	47,557	33,346	57,386	27,866
1977	33,944	46,526,101	90%	10%	61%	39%	48,197	34,215	58,382	28,390
1978	33,936	48,250,592	89%	11%	61%	39%	48,588	34,812	59,187	28,665
1979	34,468	50,109,925	90%	10%	60%	40%	48,789	36,335	59,923	29,044
1980	41,137	51,461,168	90%	10%	58%	42%	48,862	36,004	60,306	29,636
1981	41,859	53,389,185	90%	10%	58%	42%	47,624	34,640	58,541	29,490
US Census										
1960	1,662,241	33,244,820	90%	10%	69%	31%	41,044	22,238	46,053	23,674
1970	403,015	40,301,500	90%	10%	65%	35%	52,274	34,027	61,431	30,208
1980	2,613,374	52,267,480	89%	11%	58%	42%	50,268	39,001	61,357	32,072

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

Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.

Notes: Annual average earnings in $2017, deflated using annual CPI-U-RS series. Employment numbers 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 of 1959, 1969 and 1979.

C 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. Let's denote G^{total}, this economy-wide racial earnings gap. It's defined as:

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

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

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

$$\begin{aligned} \bar{X_w} &= \frac{1}{N_w}\sum_i log(\omega_i^w) \\ &= \frac{N_w^c}{N_w}\cdot\frac{1}{N_w^c}\sum_{i,w} log(\omega_i^c) + \frac{N_w^t}{N_w}\cdot\frac{1}{N_w^t}\sum_{i,w} log(\omega_i^t) \\ &= s_w^c\cdot\frac{1}{N_w^c}\sum_{i,w} log(\omega_i^c) + s_w^t\cdot\frac{1}{N_w^t}\sum_{i,w} log(\omega_i^t) \end{aligned} \tag{C.2}$$

With s_w^c (respectively s_b^c) the share of white (resp. black) workers working in the control group, s_w^t (respectively s_b^t) the share of white (resp. black) workers working in the treatment group. Note that: $s_w^c + s_w^t = 1$. Similarly, $s_b^c + s_b^t = 1$. It follows that:

$$\begin{aligned} G^{\text{total}} &= s_w^c\bar{X}_w^c + s_w^t\bar{X}_w^t - s_b^c\bar{X}_b^c - s_b^t\bar{X}_b^t \\ &= (s_w^c\bar{X}_w^c - s_b^c\bar{X}_b^c) + (s_w^t\bar{X}_w^t - s_b^t\bar{X}_b^t) \\ &= (s_w^c\bar{X}_w^c - s_w^c\bar{X}_b^c) + (s_w^t\bar{X}_w^t - s_w^t\bar{X}_b^t) + s_w^c\bar{X}_b^c - s_b^c\bar{X}_b^c + s_w^t\bar{X}_b^t - s_b^t\bar{X}_b^t \\ &= s_w^c G_c + s_w^t G_t + \underbrace{\bar{X}_b^c(s_w^c - s_b^c) + \bar{X}_b^t(s_w^t - s_b^t)}_{=\lambda} \end{aligned} \tag{C.3}$$

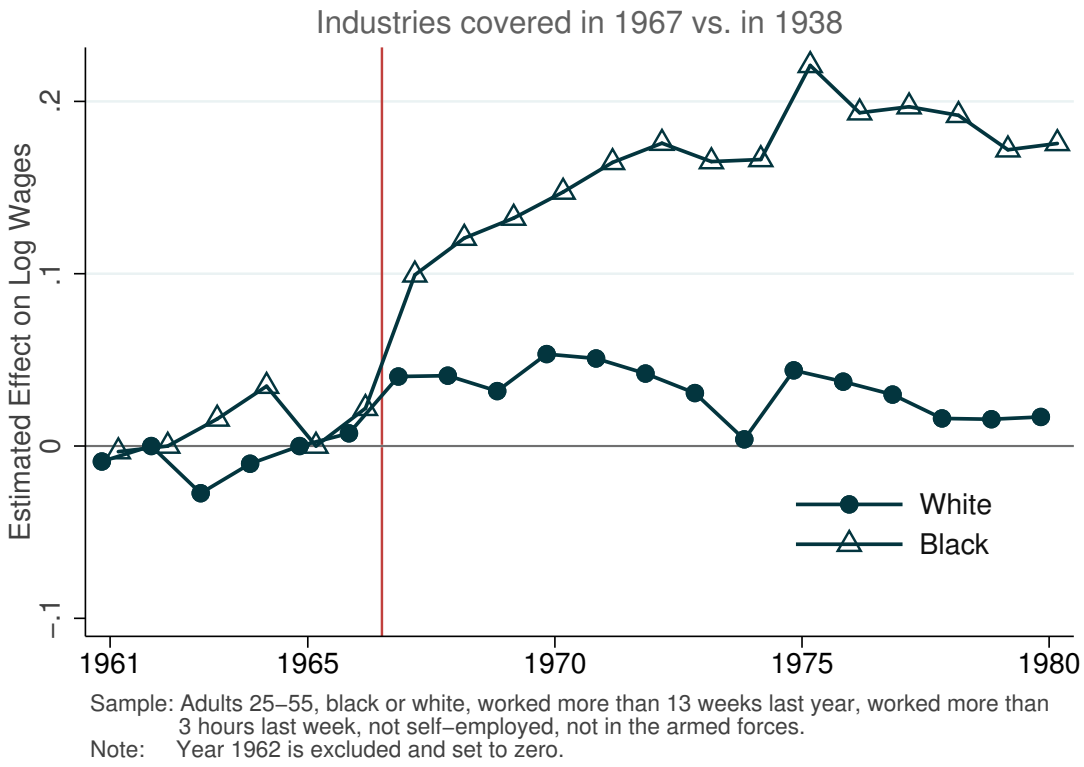
$$
\begin{aligned}
\lambda &= s_w^c G_c + s_w^t G_t + \bar{X}_b^c(s_w^c - s_b^c) + \bar{X}_b^t(s_w^t - s_b^t) \\
&= s_w^c \bar{X}_b^c - s_b^c \bar{X}_b^c + s_w^t \bar{X}_b^t - s_b^t \bar{X}_b^t \\
&= s_w^c \bar{X}_b^c - s_w^c \bar{X}_b^t + s_w^c \bar{X}_b^t - s_b^c \bar{X}_b^c + s_w^t \bar{X}_b t - s_b^t \bar{X}_b^t - (s_b^c \bar{X}_b^c - s_b^c \bar{X}_b^t + s_b^c \bar{X}_b^t - s_w^t \bar{X}_b^t) \\
&= s_w^c G_b^{ct} + s_w^c \bar{X}_b^t - s_b^t \bar{X}_b^t - (s_b^c G_b^{ct} + s_b^c \bar{X}_b^t - s_w^t \bar{X}_b^t) \\
&= s_w^c G_b^{ct} - s_b^c G_b^{ct} + \bar{X}_b^t \times (\underbrace{s_w^c + s_w^t}_{=1} - (\underbrace{s_b^c + s_b^t}_{=1})) \\
&= s_w^c G_b^{ct} - s_b^c G_b^{ct}
\end{aligned}
\tag{C.4}
$$

Therefore:

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

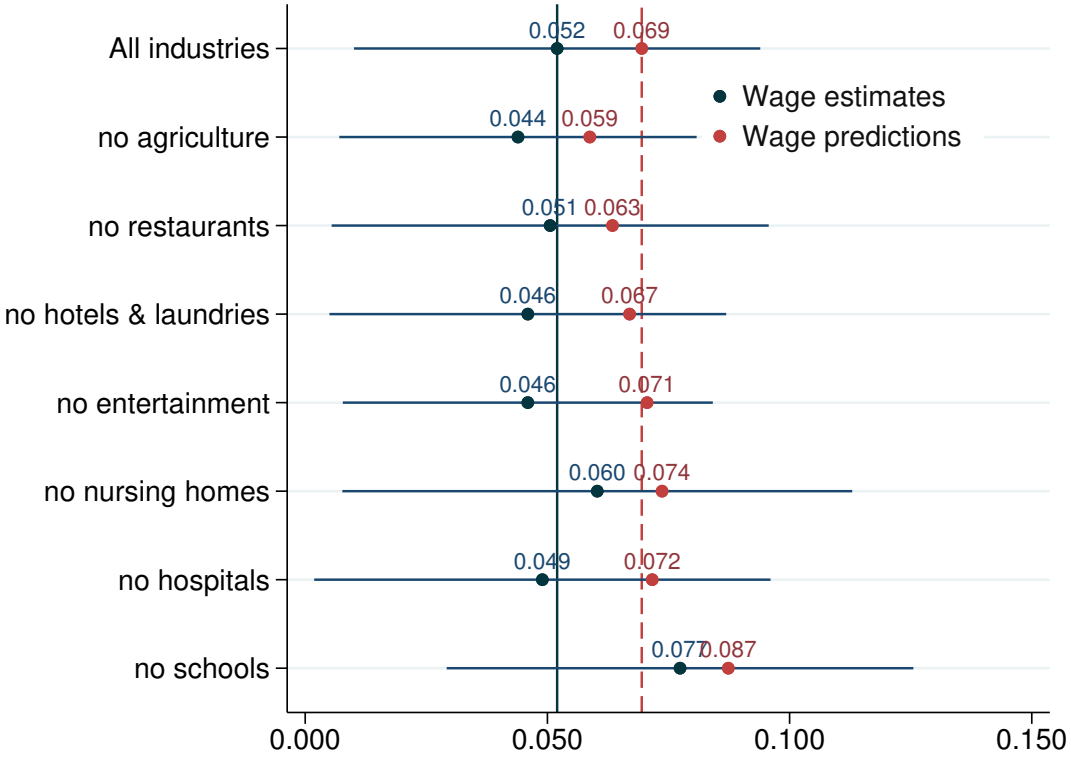
D Appendix Figures and Tables

Figure A5 – Impact of the 1966 FLSA on annual wages by race



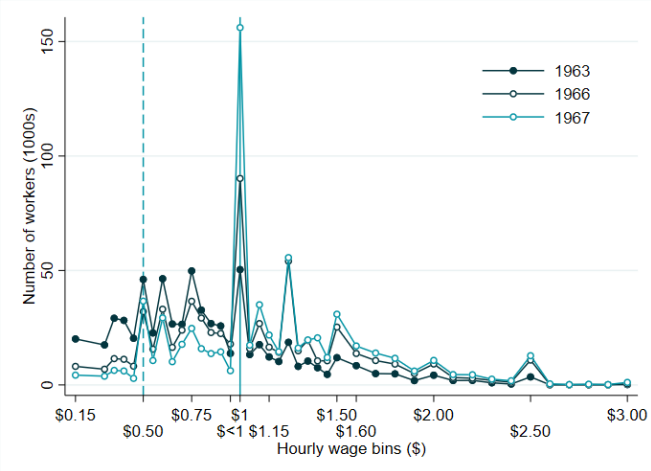
Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: This graphs differs from figure 1.8b: the control group for black workers is composed here by black and white workers in the industries covered in 1938, whereas in figure 1.8b, the control group for black workers is composed of black workers only in the industries covered in 1938.

Figure A6 – Wage estimates and wage predictions, by industry

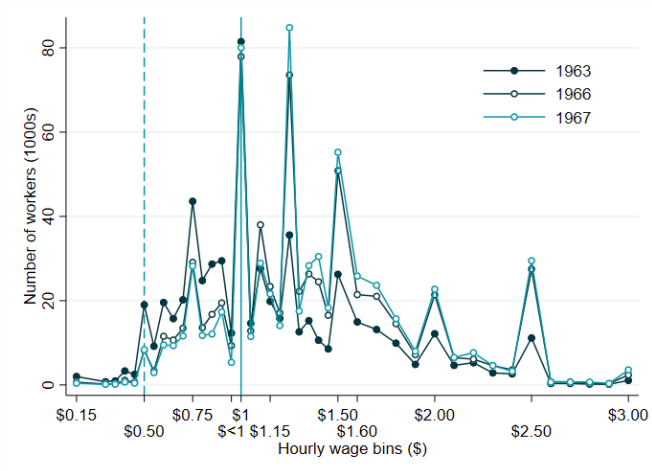


Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Wage estimates and wage predictions are for 1967.

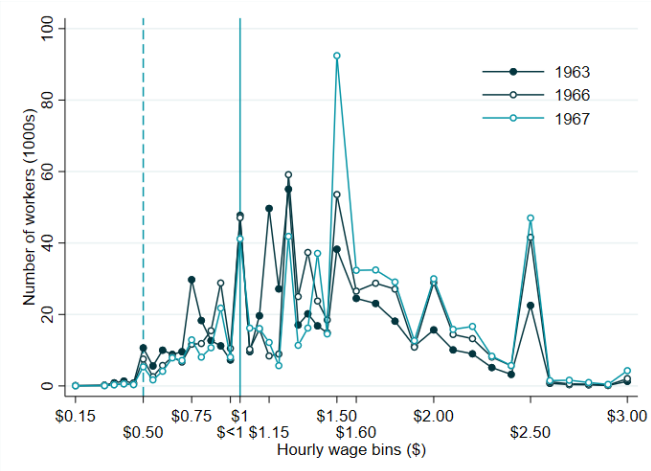
Figure A7 – Earnings distributions in hotels, restaurants and laundries, by region



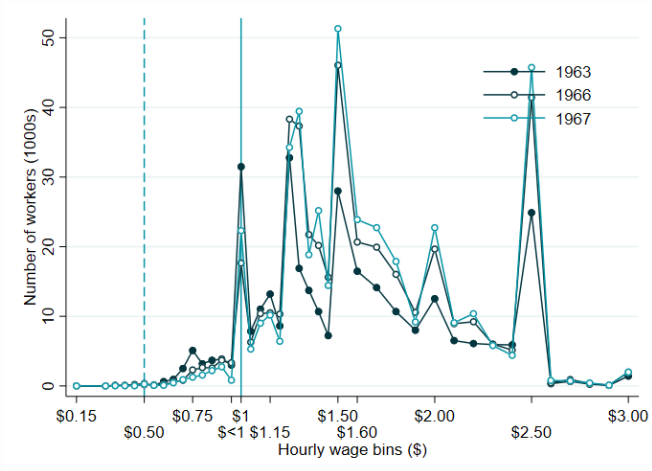
(a) South



(b) Midwest



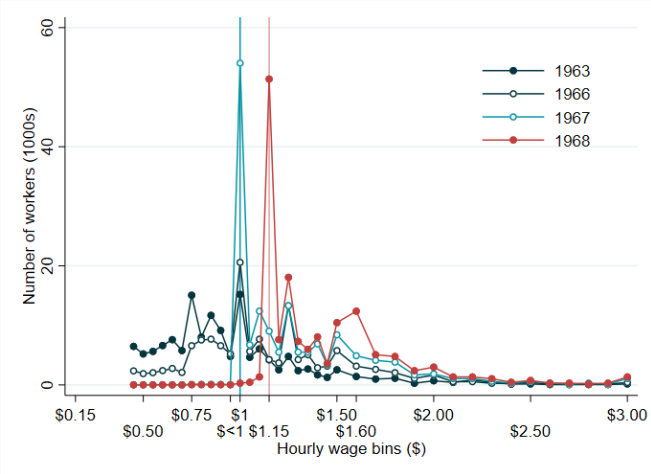
(c) Northeast



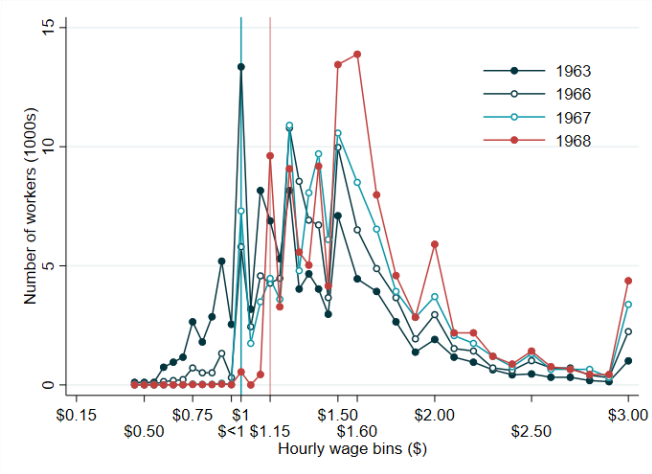
(d) West

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

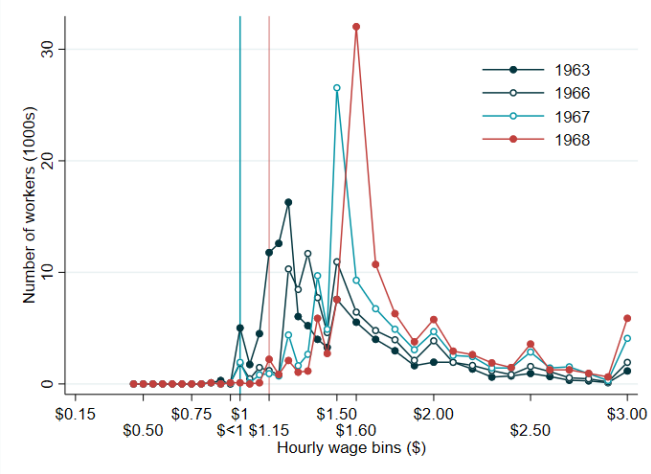
Figure A8 – Earnings distributions in laundries (inside plant workers), by region



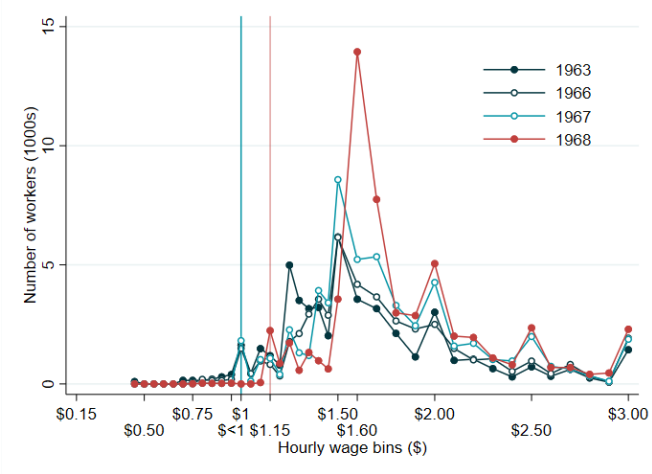
(a) South



(b) Midwest



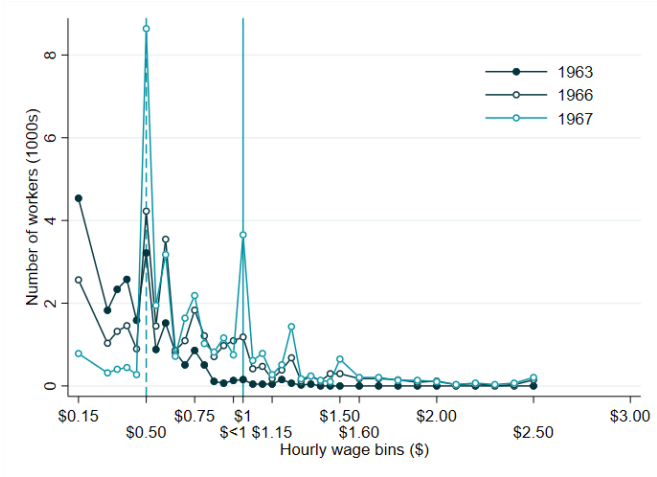
(c) Northeast



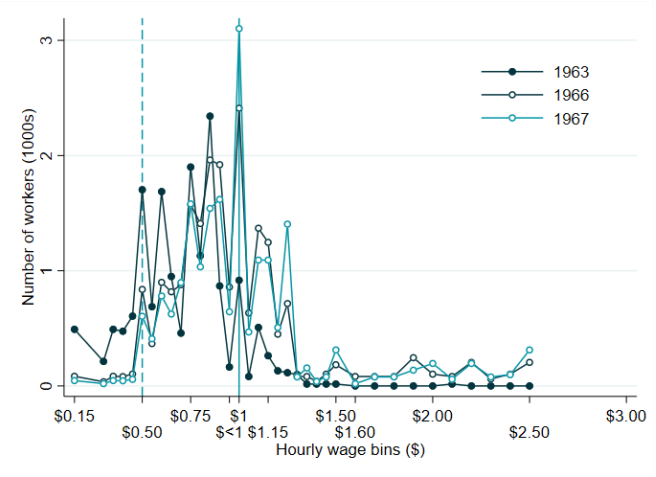
(d) West

Source: BLS Industry Wage Reports. Sample: All inside plant workers in laundries. In laundries, the minimum wage is introduced at $1 (solid line) in 1967.

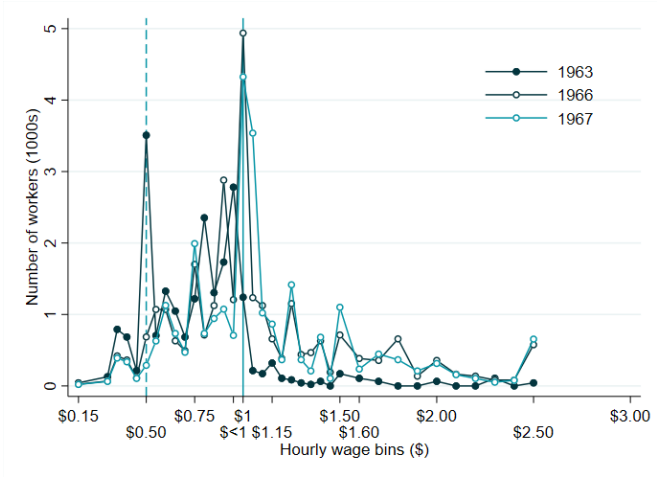
Figure A9 – Earnings distributions in hotels (tipped workers), by region



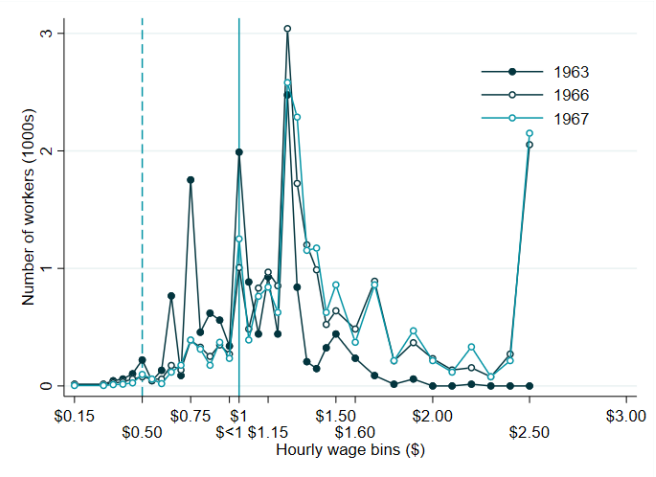
(a) South



(b) Midwest



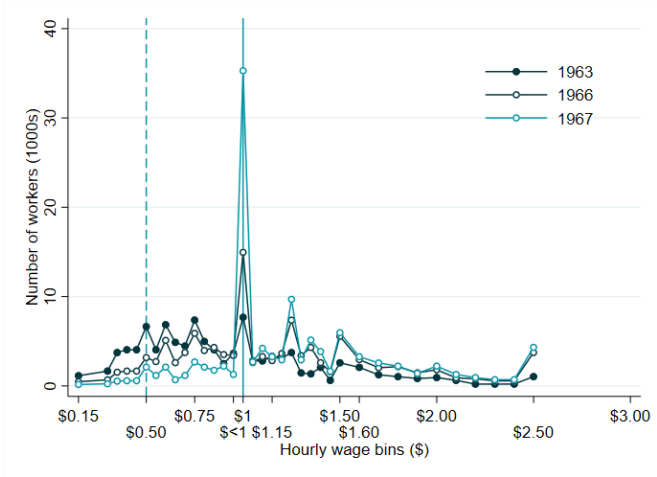
(c) Northeast



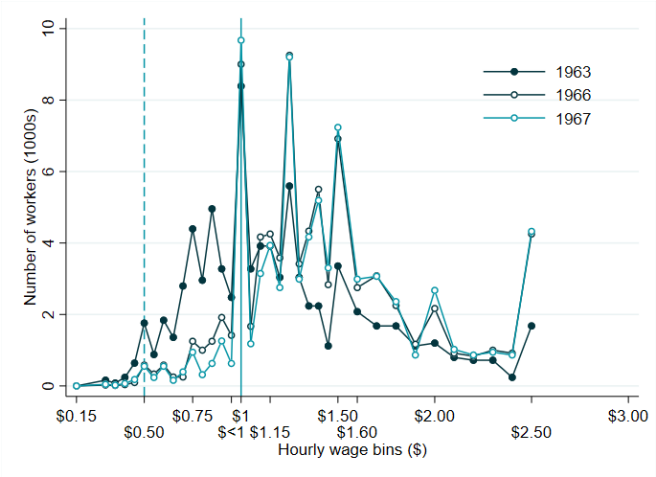
(d) West

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

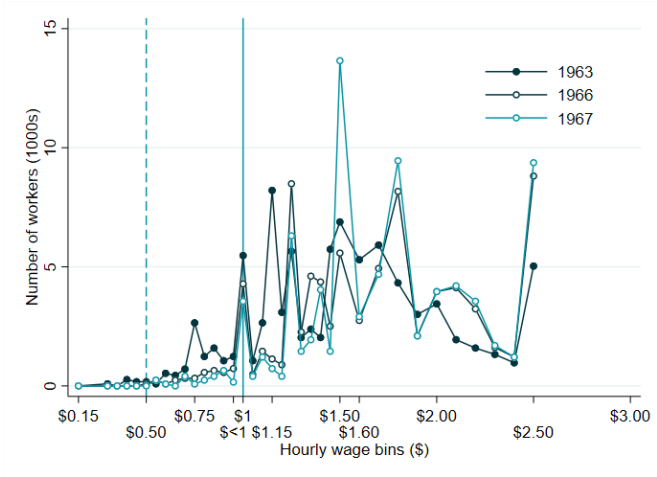
Figure A10 – Earnings distributions in hotels (non-tipped workers), by region



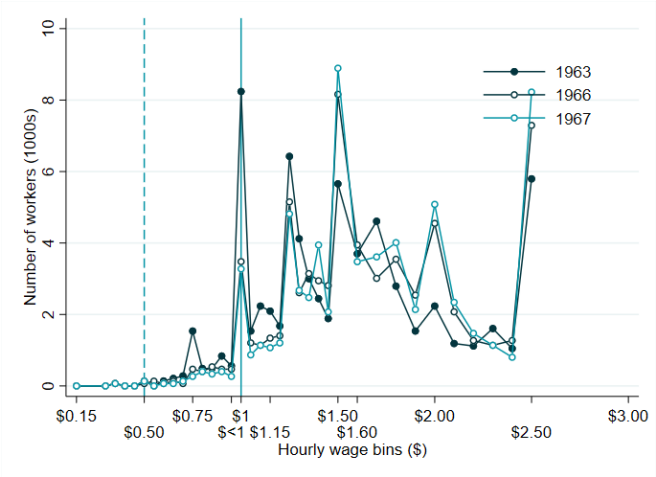
(a) South



(b) Midwest



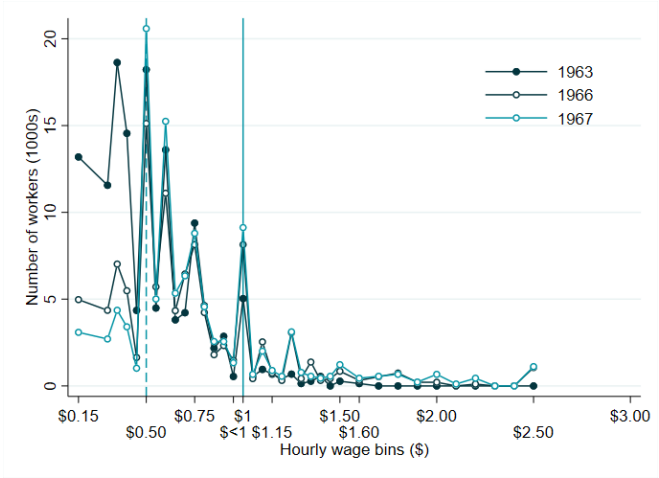
(c) Northeast



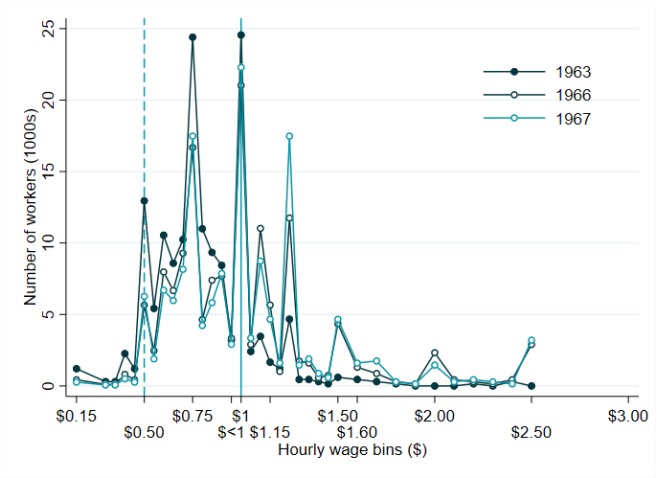
(d) West

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

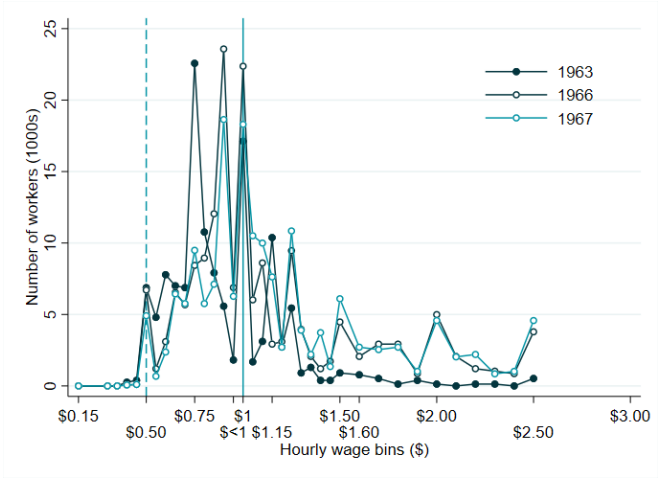
Figure A11 – Earnings distributions in restaurants (tipped workers), by region



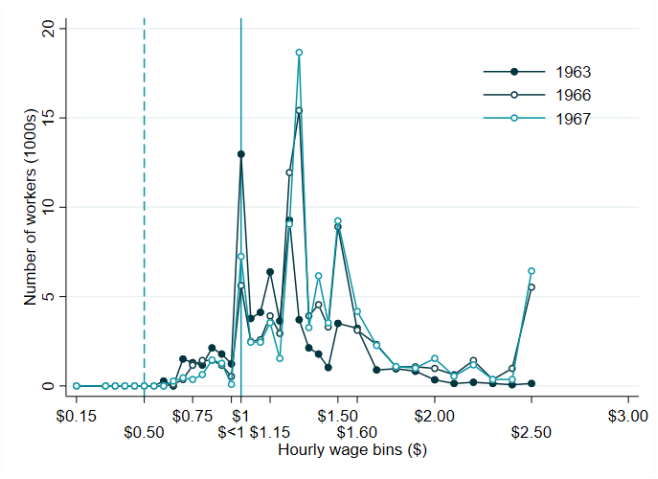
(a) South



(b) Midwest



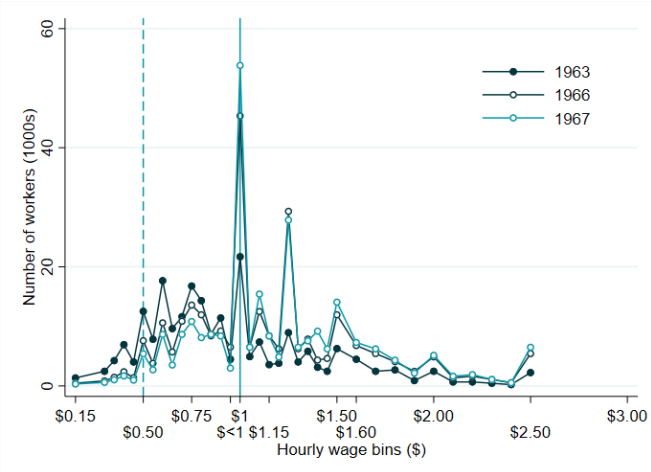
(c) Northeast



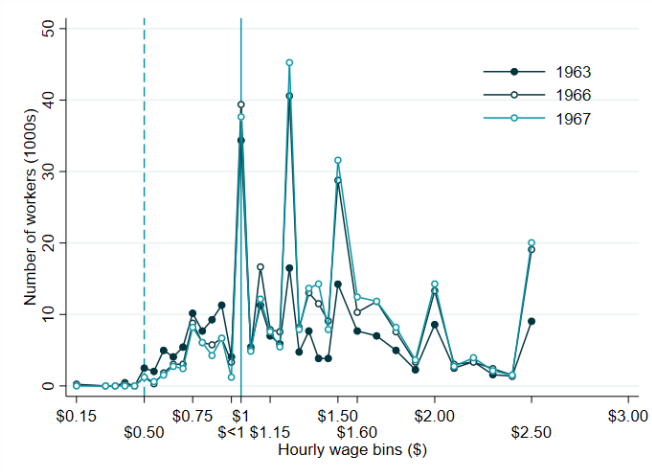
(d) West

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

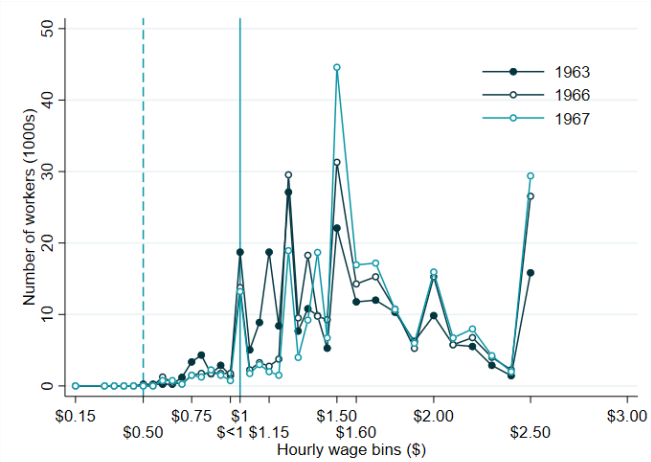
Figure A12 – Earnings distributions in restaurants (non-tipped workers), by region



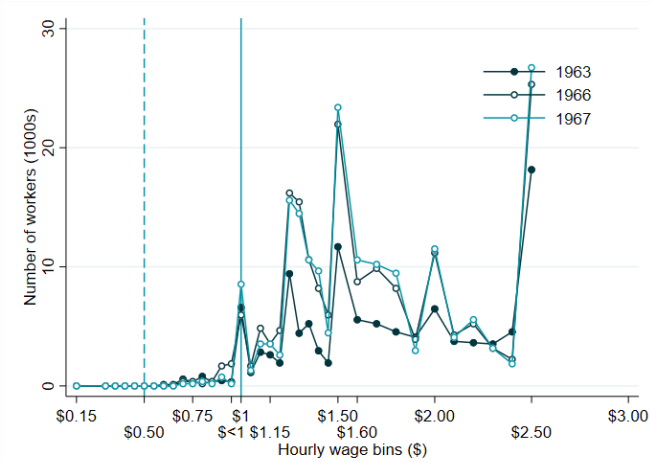
(a) South



(b) Midwest



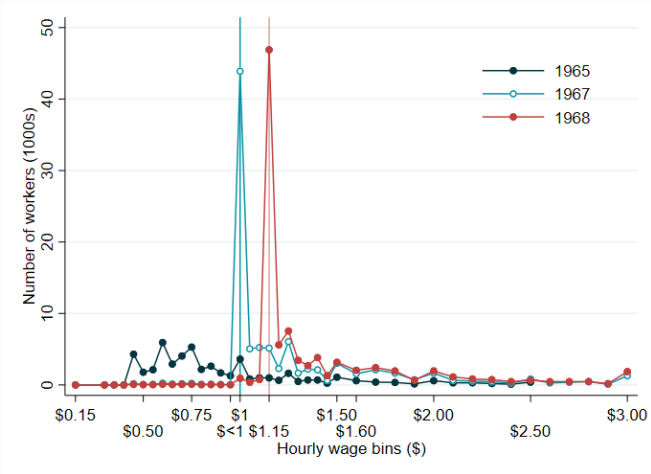
(c) Northeast



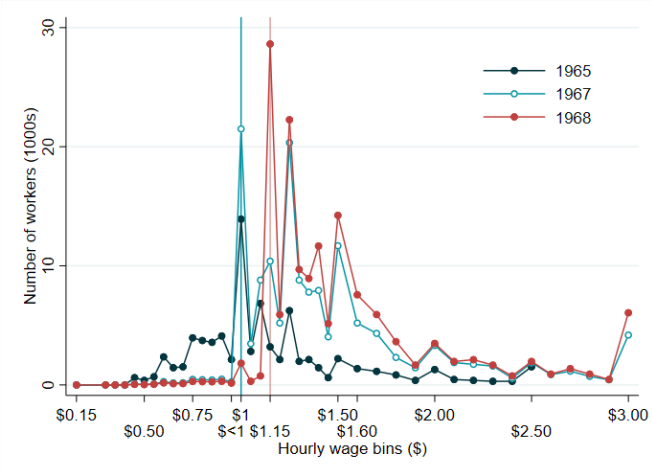
(d) West

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

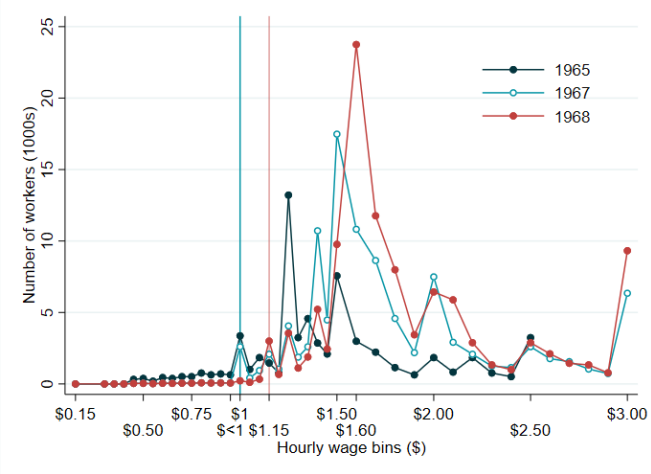
Figure A13 – Earnings distributions in nursing homes, by region



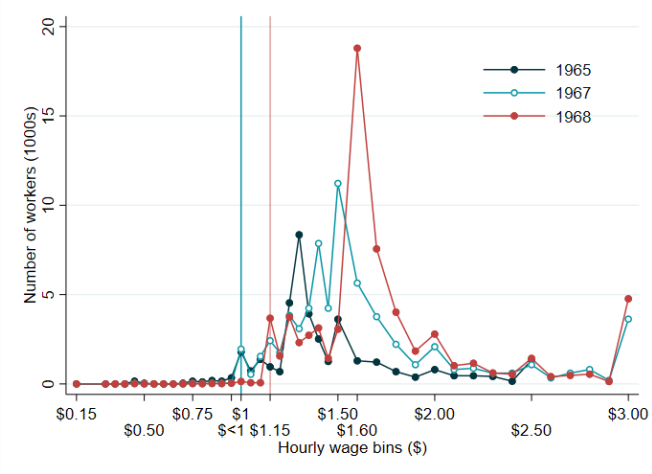
(a) South



(b) Midwest



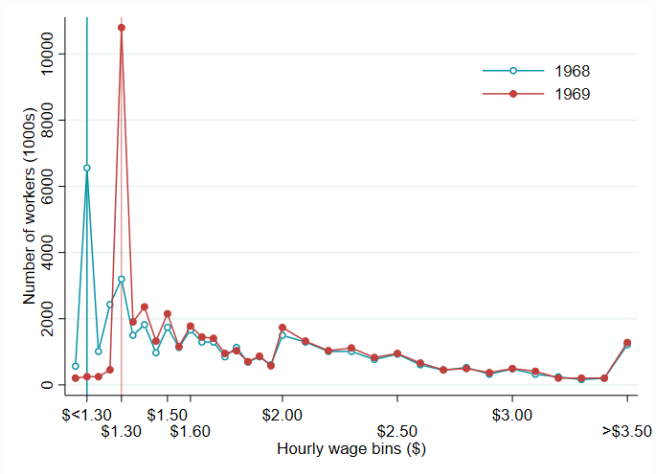
(c) Northeast



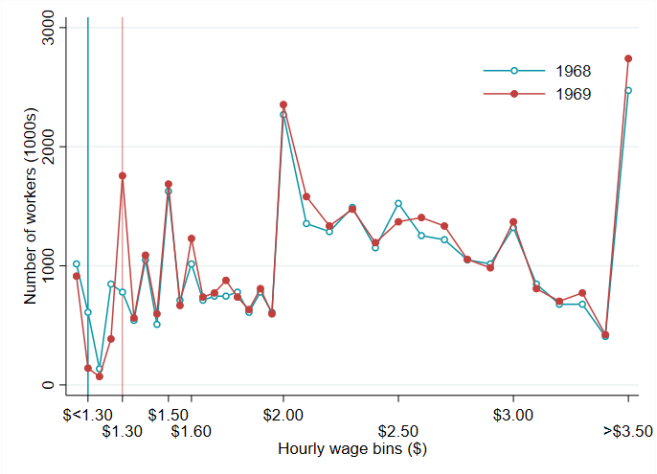
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees in nursing homes and related facilities.

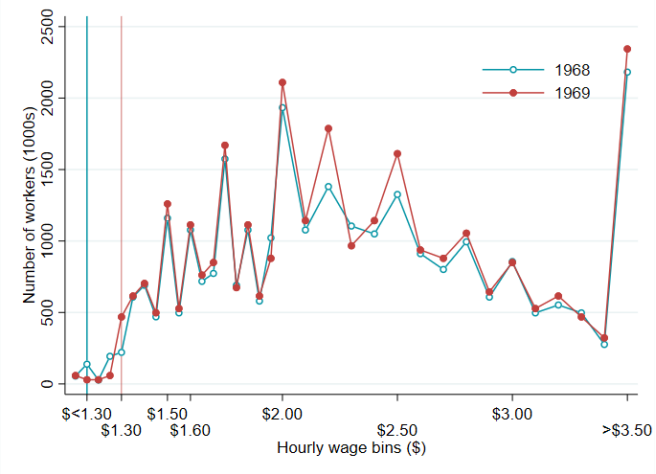
Figure A14 – Earnings distributions in schools, by region



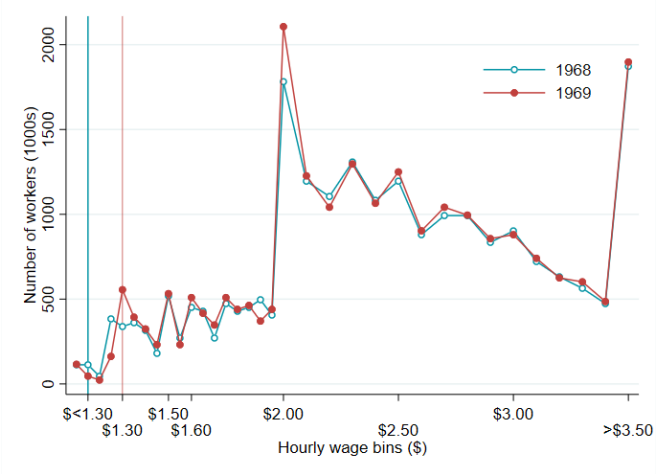
(a) South



(b) Midwest



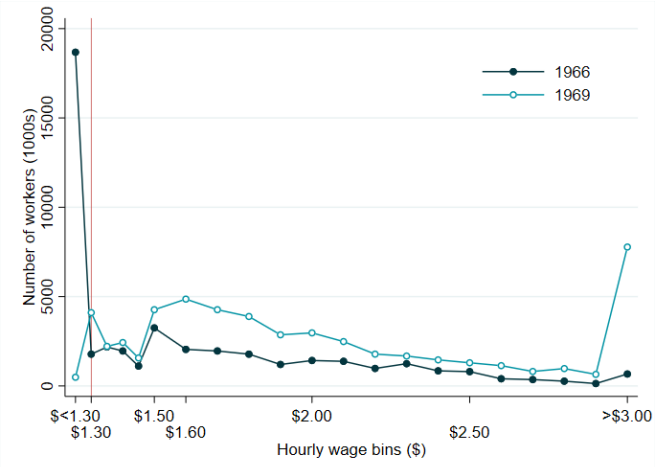
(c) Northeast



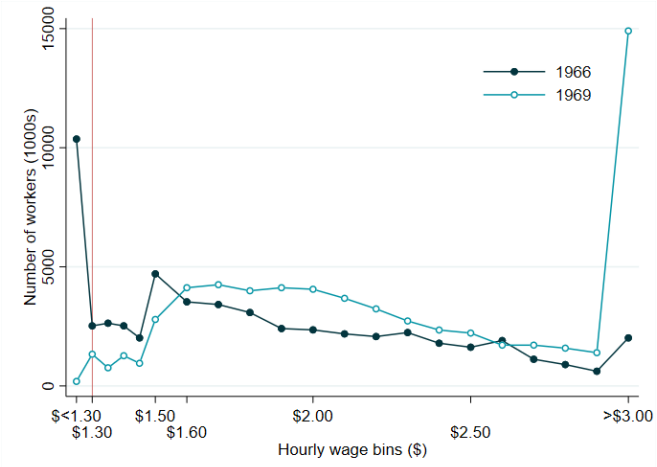
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory nonteaching employees (i.e. e.g. custodial employees, food service employees, office clerical employees, skilled maintenance employees, bus drivers) in schools.

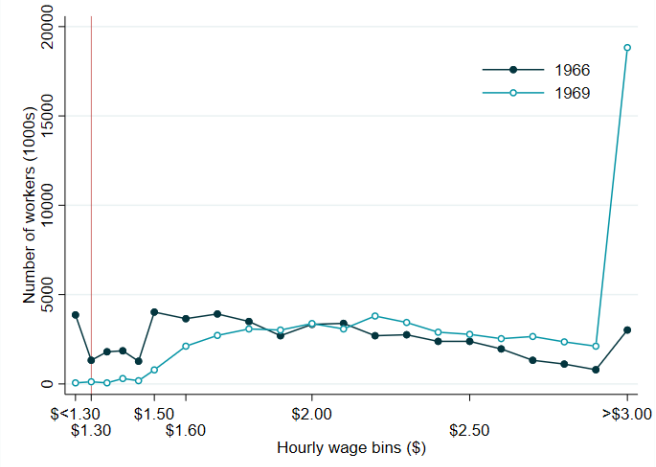
Figure A15 – Earnings distributions in hospitals, by region



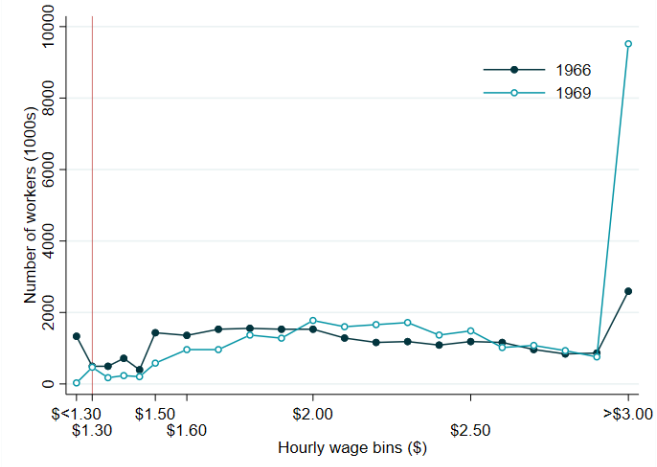
(a) South



(b) Midwest



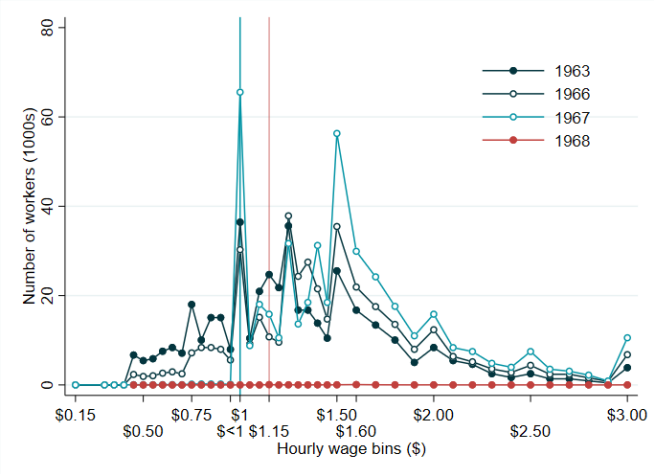
(c) Northeast



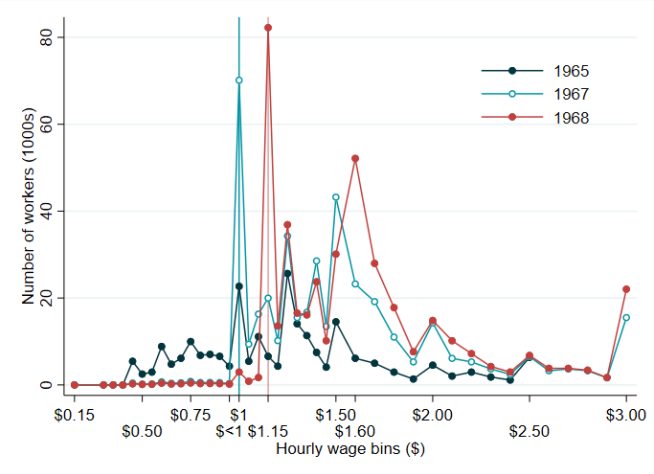
(d) West

Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees in all hospitals (except federal hospitals) (i.e. e.g. nursing aids, porters, maids, kitchen helpers, dishwashers, practical nurses, medical social workers, dietitians, etc.).

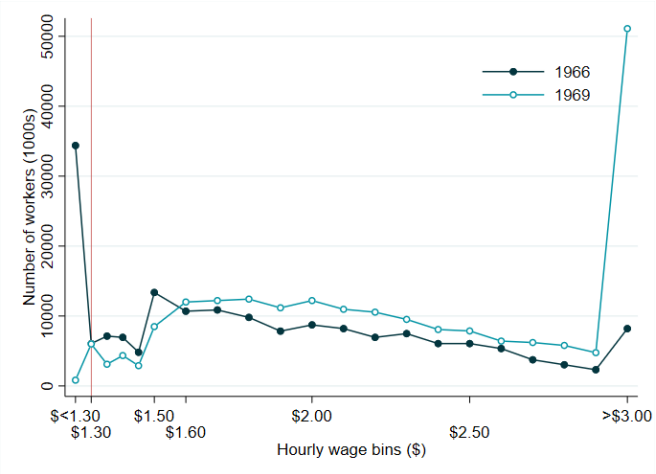
Figure A16 – Hourly earnings distributions



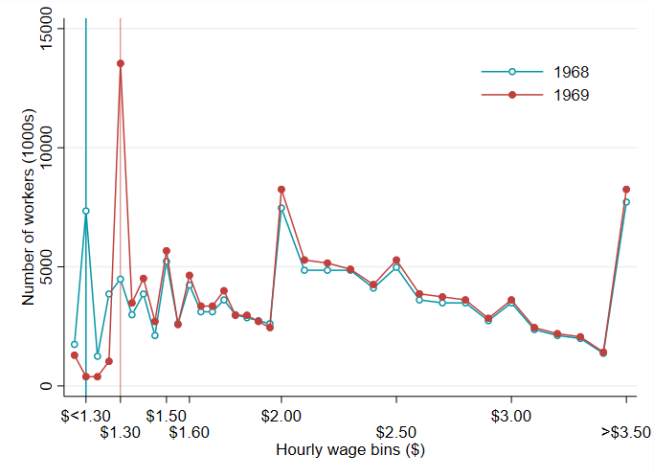
(a) Laundries



(b) Nursing homes



(c) Hospitals

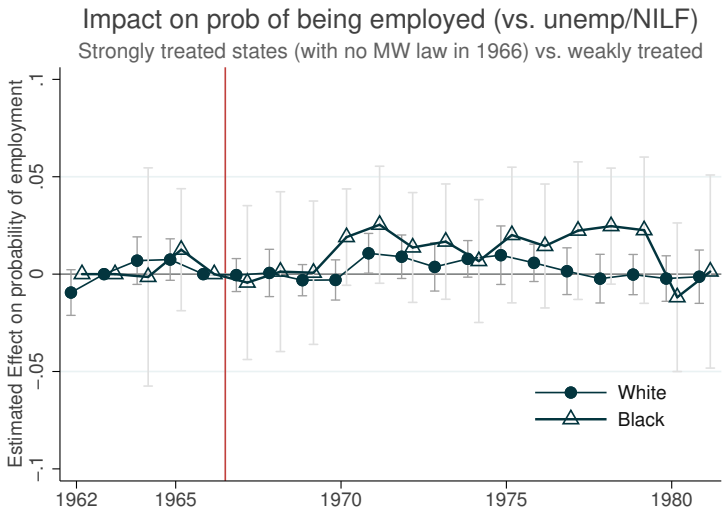


(d) Schools

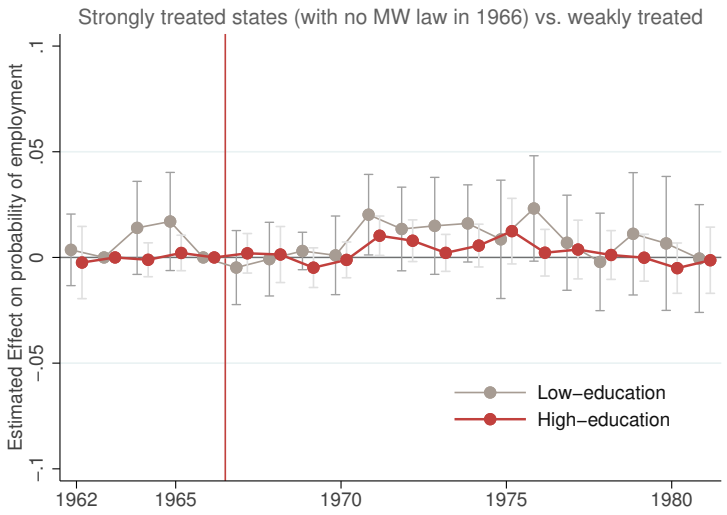
Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees.

Figure A17 – Impact of the 1966 FLSA on probability of being employed (vs. not unemployed or not in the labor force)

(a) Black vs. white workers



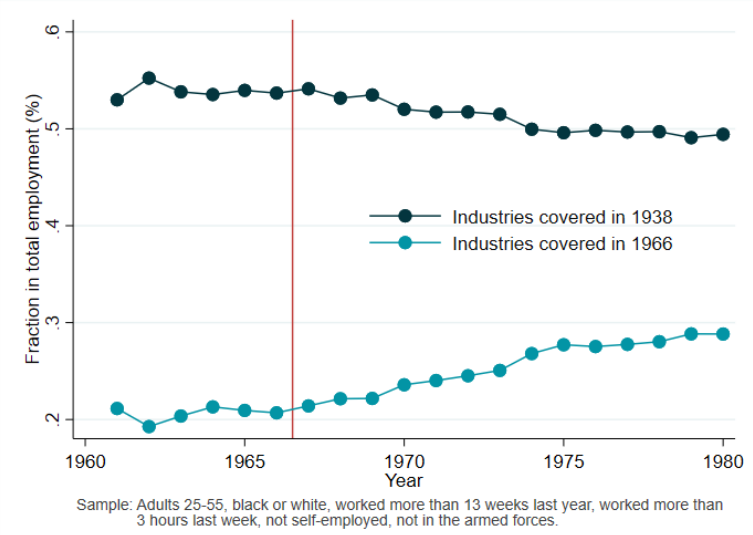
(b) Low-education vs. high-education



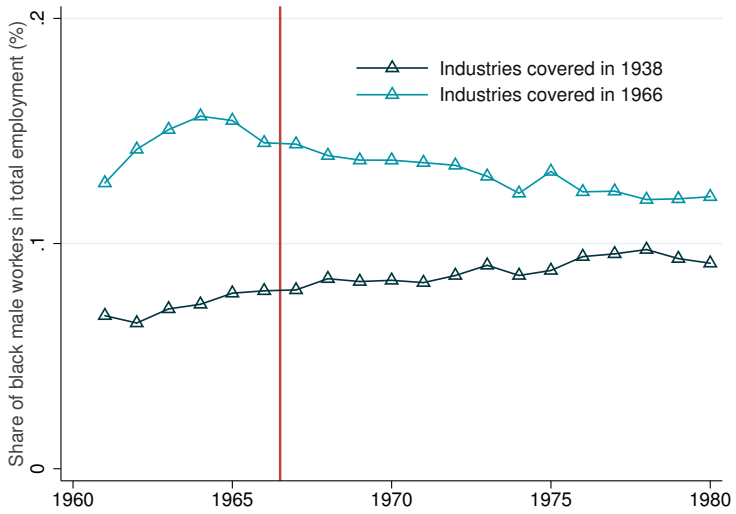
Source: CPS 1962-1980. Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Standard errors clustered at the industry and state (group) level. Includes state, industry and time fixed effects.

Figure A18 – Evolution of Black and White employment in treated and control industries

(a) Employment shares in control vs. treated industries

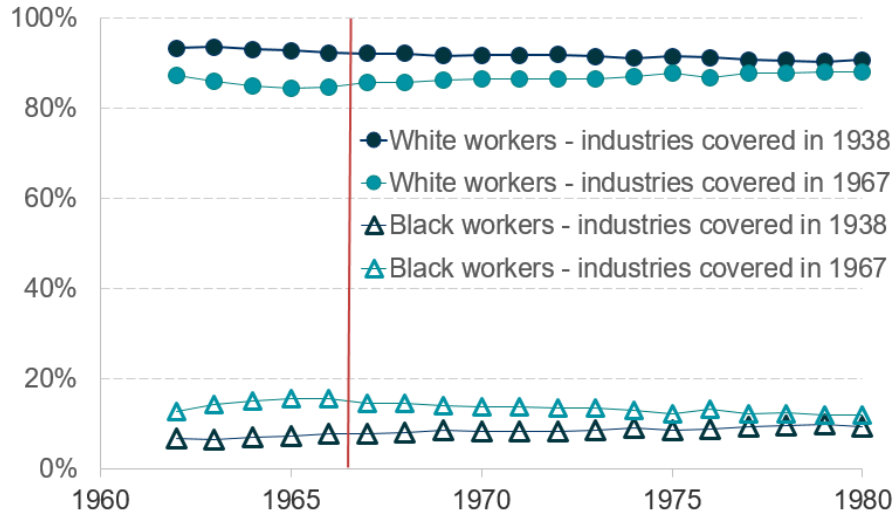


(b) Black (vs. white) employment shares within 1938 and 1967 industries



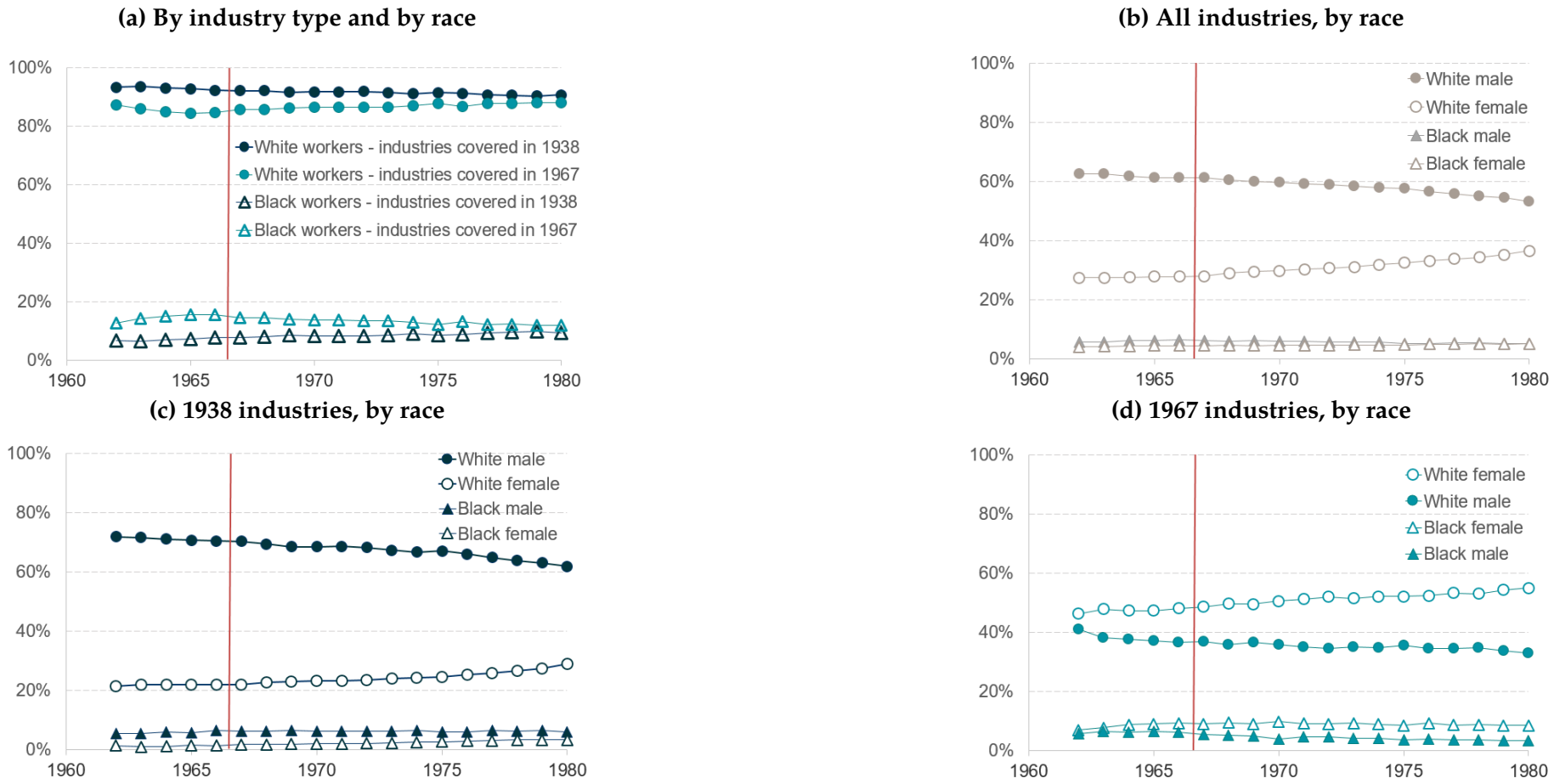
Source: CPS 1962-1980. Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

Figure A19 – Aggregate employment shares by industry type and by race



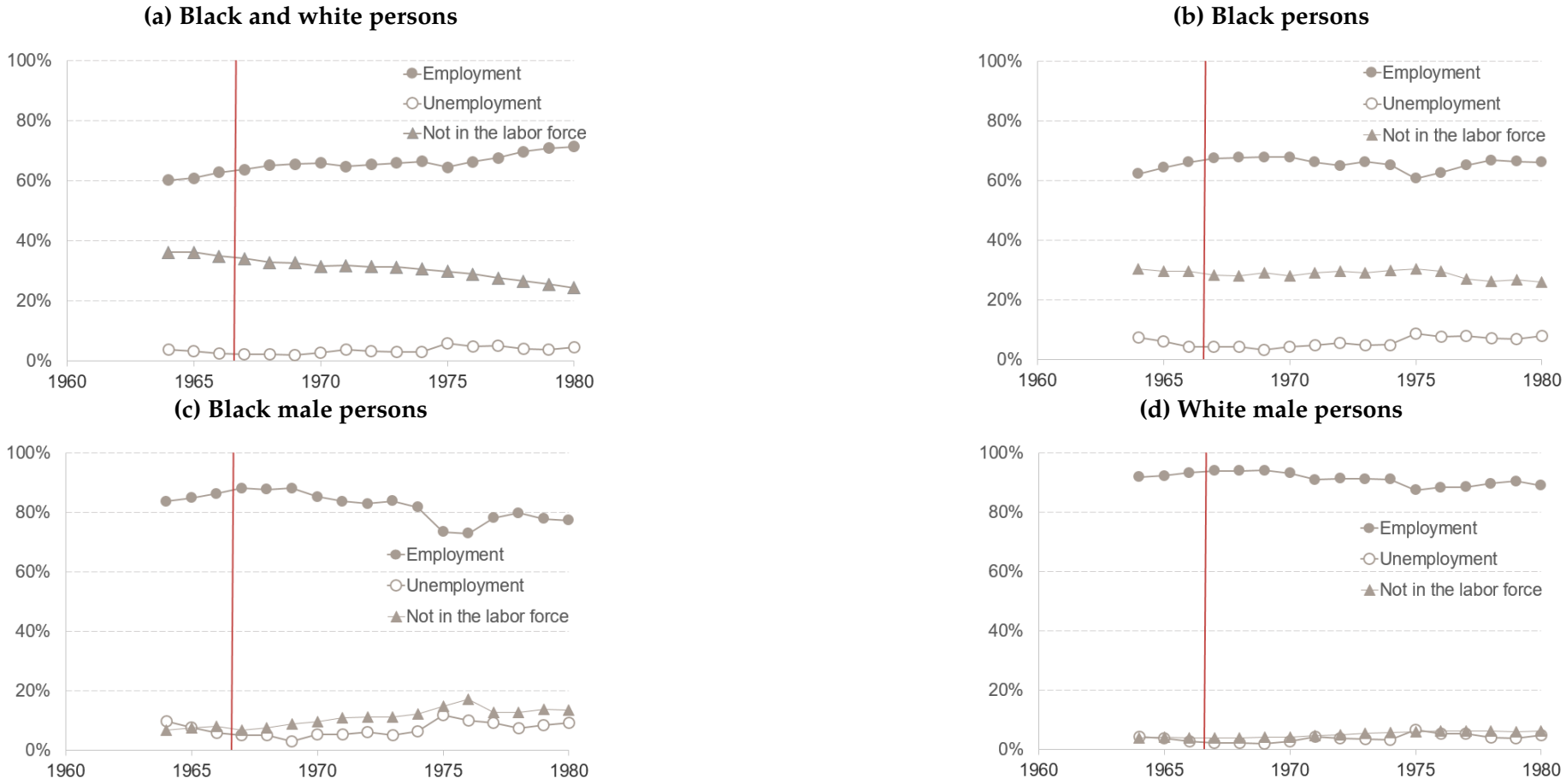
Source: CPS 1962-1980. Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

Figure A20 – Aggregate employment shares



Source: CPS 1962-1980. Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Standard errors clustered at the industry and state (group) level. Includes state, industry and time fixed effects.

Figure A21 – Employment status in 1938 and 1967 industries



Source: CPS 1962-1980. Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

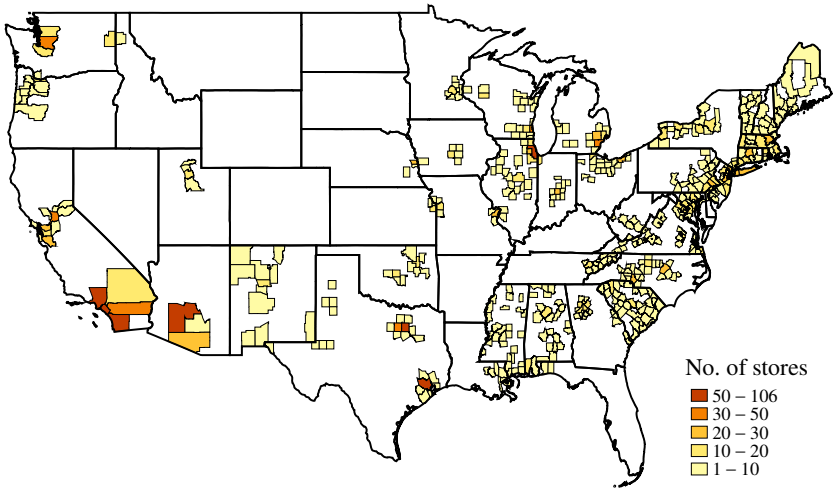
Appendix B

The Pass-Through of Minimum Wages into US Retail Prices: Appendix

A Construction of price series

Our empirical analysis is based on scanner data provided by the market research firm Symphony IRI. The dataset is described in detail in Bronnenberg et al. (2008). It contains weekly prices and quantities for 31 product categories sold at grocery and drug stores between January 2001 and December 2012. Stores report total revenue (TR) and total sold quantities (TQ) at the level of UPCs for each week. Figure A1 shows the regional distribution of the stores in our dataset.

Figure A1 – Regional distribution of stores in IRI data across the US



Notes: Geographical distribution of stores in the IRI data. The map shows stores per county. Of the 3142 counties in the US, 530 (17%) are covered with at least one store in the IRI data.

In order to construct store-level price indices, we first calculate the average price of product i in grocery store j and week w from quantities and revenues:

$$P_{ijw} = \frac{TR_{ijw}}{TQ_{ijw}}.$$

We next calculate the average monthly price for each series and construct a geometric index of month to month price changes for each product category c in each store:

$$I_{cjt} = \prod_i \left(\frac{P_{ijt}}{P_{ijt-1}} \right)^{\omega_{ijy(t)}}. \tag{A.1}$$

The weight $\omega_{ijy(t)}$ is the share of product i in total revenue of category c in store j during the calendar year of month t.[1] In a second step, we aggregate across

[1]Price indices are often constructed using lagged quantity weights. Since product turnover in grocery stores is high, using lagged weights would limit the number of products used in the construction of our index. We thus use contemporaneous weights.

different categories to create store-level price indices and inflation rates:

$$I_{jt} = \prod_c I_{cjt}^{\omega_{cjy(t)}} \text{ and } \pi_{jt} = \log I_{jt} \tag{A.2}$$

Again, the weight $\omega_{cjy(t)}$ is the share of category c in total revenue in store j during the calendar year of month t. Note that this approach does not take into account changes in the price level due to the introduction of new products, or due to reappearance of products at a new price after a stock-out, a feature shared by most price indices.

An important characteristic of high frequency retail price data is that prices often change temporarily and return to their original level afterward. These movements, usually due to temporary sales, are large and affect the volatility of inflation rates at a monthly frequency. We thus apply an algorithm developed by Kehoe and Midrigan (2015) to determine "regular prices". Regular prices in our case are "permanent prices". Stores charge this price during long time periods, but often deviate from it during temporary sales. The regular price determined by the algorithm is based on the modal price for a product during a running window. For completeness, we reproduce a slightly edited description of the algorithm given in the web appendix to Kehoe and Midrigan (2015):

1. Choose parameters: $l = 2$ (size of the window: the number of weeks before and after the current period used to compute the modal price), $c = 1/3$ (=cutoff used to determine whether a price is temporary), $a = 0.5$ (=the share of non-missing observations in the window required to compute a modal price).

2. Let p_t be the price in week t and T the length of the price series. Determine the modal price for each time period $t \in (1 + l, T - l)$:

   - If the number of weeks with available data in $(t - l, \ldots, t + l)$ is larger than or equal $2al$, then $p_t^M = mode(p_{t-1}, \ldots, p_{t+l})$ and $f_t =$ the fraction of periods with available data where $p_t = p_t^M$.
   - Else $f_t = .$ and $p_t^M = .$ (missing)

3. Determine the first-pass regular price for $t = 1, ..., T$:

   - Initial value: If $p_{1+l}^M \neq .$, then $p_{1+l}^R = p_{1+l}^M$. Else, set $p_{1+l}^R = p_{1+l}$.
   - For all other $t = l + 1, \ldots, T$: If $p_t^M \neq .$ and $f_t > c$ and $p_t = p_t^M$, then $p_t^R = p_t^M$. Else: $p_t^R = p_{t-1}^R$.

4. Make sure regular prices are updated at the right times. Repeat the following procedure l times (this adjusts the timing of regular price changes to the first occurrence of a new modal price).

   (a) Let $R = \{t : p_t^R \neq p_{t-1}^R \,\&\, p_{t-1}^R \neq 0 \,\&\, p_t^R \neq 0\}$ be the set of weeks with regular price changes

   (b) Let $C = \{t : p_t^R = p_t \,\&\, p_t^R \neq 0 \,\&\, p_t \neq 0\}$ be the set of weeks in which a store charges the regular price

(c) Let $P = \{t : p^R_{t-1} = p_{t-1} \,\&\, p^R_{t-1} \neq 0 \,\&\, p_{t-1} \neq 0\}$ be the set of weeks in which a store's last week's price was the regular price

(d) Set $p^R_{\{R \cap C\}-1} = p_{\{R \cap C\}}$. Set $p^R_{\{R \cap P\}} = p_{\{R \cap P\}-1}$.

Table A1 reports features of price adjustments for the regular prices that our index is based on. Prices change with a median monthly frequency of 10.3% from 2001 to 2006 and 12.2% from 2007 to 2012. This implies a median duration of a price spell of 9.2 and 7.7 months, respectively. The median size of a price change is about 11.4% during the first half period of the sample, and 10.5% during the second half. The share of price increases in price changes is about 57% during the first half of the sample and 60% during the latter half. Price increases are smaller than price decreases. Finally, monthly inflation rates are lower during the first half of the sample compared with the second half. The monthly rates correspond to annualized inflation rates of 1% in the first and 1.8% in the second half of the sample. Overall, those numbers are in line with what other researchers have documented for our and other retail price datasets.[2]

B Additional Regression Results

C Results using state-level price series

In this section, we conduct an analysis of the response of prices at the state level instead of at the store level. Our construction of state-level price indices largely follows Stroebel and Vavra (2015). One advantage of the state-level compared to our baseline store level estimation is that the state panel is balanced and that we can extend the estimation to a longer panel without missing leads and lags due to store entry and exit.

Table A7 presents the estimation results for the baseline specifications using the state panel data set. The results confirm our baseline estimates, both in terms of timing and magnitude of the effect. The estimated elasticity at legislation amounts to about 0.02 and there are no significant estimates around implementation of hikes. Figure A3 shows the estimated effect on price inflation (panel a) and on the price level (panel b) if we allow the event window to span more than one year before and after the event, focusing on the effects at legislation. The figures provide no evidence for differential trends in the 15 months leading up to the legislation of a minimum wage hike.

D The minimum wage elasticity of marginal cost

In this section, we present a simple theoretical model that helps to illustrate the relationship between the minimum wage elasticity of prices and the minimum wage elasticity of marginal cost at constant output.

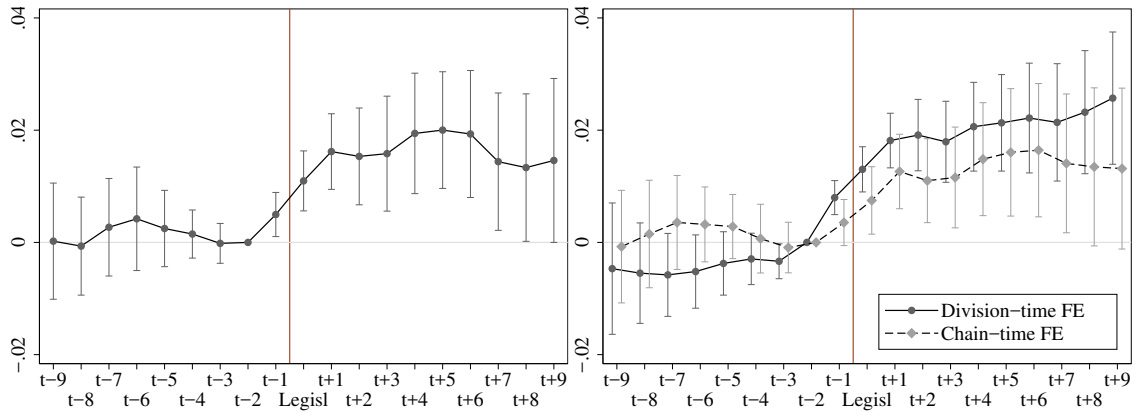
[2]See Nakamura and Steinsson (2008) for CPI data from 1998 to 2005 or Midrigan (2011) for an alternative scanner data set from 1989 to 1997. Stroebel and Vavra (2015) construct state-level indices based on the same data used in our paper and find that inflation rates are lower than CPI inflation from the beginning of the data until 2007.

Table A1 – Features of regular prices

	2001-2006		2007-2012	
	Mean	Median	Mean	Median
Frequency of price change	0.117	0.103	0.132	0.122
Implied median duration	8.037	9.200	7.064	7.686
Frequency of price increase	0.067	0.060	0.078	0.074
Frequency of price decrease	0.050	0.040	0.054	0.043
Share of price increases in changes	0.605	0.576	0.623	0.602
Absolute size of price change	0.154	0.114	0.144	0.105
Absolute size of price increase	0.147	0.105	0.140	0.100
Absolute size of price decrease	0.184	0.146	0.166	0.132
SD log price	0.152	0.154	0.150	0.151
Monthly inflation	0.0007	0.0008	0.0016	0.0015

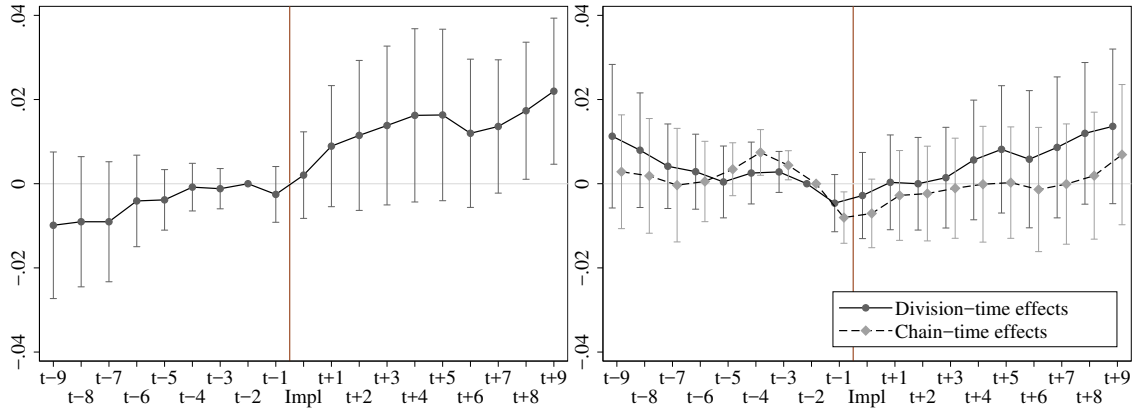
Notes: To construct these measures, we first calculate the frequency and size of price changes for each product in each store separately. For frequencies, we count changes and divide them by the number of observations for which we also observe a lagged price. We also calculate the standard deviation of the logarithm of prices within each state for each unique product. We then construct expenditure weighted means and medians for each category for the periods 2001 to 2006 and 2007 to 2012. Finally, we take expenditure weighted means over all 31 broad product categories. To summarize inflation rates, we take the weighted mean or median of our store-level inflation rates for the same periods.

Figure A2 – Cumulative minimum wage elasticities of prices from joint estimation



(a) Legislation: Baseline specification

(b) Legislation: Controlling for chain-time or division-time effects



(c) Implementation: Baseline specification

(d) Implementation: Controlling for chain-time or division-time effects

Notes: The figures present the cumulative minimum wage elasticity of prices at grocery stores. For each specification, the effects at legislation and implementation are estimated jointly from equation 3.1. Panels (a) and (c) show the cumulative elasticities at legislation and implementation estimated from the baseline specification. Panels (b) and (d) show elasticities estimated controlling for chain-time or division-time effects. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also present 90% confidence intervals of these sums based on SE clustered at the state level.

Table A2 – Further robustness checks for joint estimation

Dep. variable: Store inflation	(1) Balanced panel	(2) County trends	(3) Short window	(4) Long window	(5) Pre-2007	(6) Only first hike
			Legislation			
E_0^{leg}	0.008***	0.011***	0.011***	0.012**	0.012***	0.011**
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
E_2^{leg}	0.010**	0.015***	0.015***	0.017**	0.018***	0.020***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.006)
E_4^{leg}	0.014**	0.019***	0.018***	0.022**	0.024***	0.025***
	(0.006)	(0.006)	(0.006)	(0.009)	(0.006)	(0.008)
			Implementation			
E_0^{inc}	0.002	0.002	-0.000	0.003	0.000	0.002
	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)
E_2^{inc}	0.017	0.011	0.005	0.012	0.010	0.010
	(0.011)	(0.011)	(0.010)	(0.010)	(0.009)	(0.011)
E_4^{inc}	0.024*	0.015	0.008	0.015	0.017	0.017
	(0.013)	(0.012)	(0.012)	(0.011)	(0.011)	(0.013)
			Estimation Summary			
E_4^{leg}+E_4^{inc}	0.038***	0.035**	0.027*	0.037**	0.040***	0.042***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)
$\sum$ All	0.024	0.045*	0.026	0.040	0.044**	0.031
	(0.024)	(0.022)	(0.021)	(0.040)	(0.021)	(0.021)
$\sum$ Pre-event	-0.003	0.010	0.005	-0.005	0.000	-0.011
	(0.016)	(0.016)	(0.012)	(0.022)	(0.014)	(0.014)
N	73646	191568	206477	176822	108217	186151
Controls	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES
County trends	NO	YES	NO	NO	NO	NO

Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. (1) presents the baseline estimates for the joint estimation of the effects at implementation and legislation. (2) focuses on stores that are present in all 142 periods of our sample. (3) adds county-specific time trends in the inflation rate. (4) uses an event window of length $k \pm 6$. (5) uses an event window of length $k \pm 12$. (6) restricts on the 2002–2007 periods. (7) computes the price effects by only exploiting the first minimum wage hike in each state in the sample period. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A3 – Robustness checks for separate estimation of effect at legislation

Dep. variable: Store inflation	(1) Weighted	(2) No Controls	(3) No store FE	(4) Seasonal	(5) No salesfilter	(6) Winsorized
			Legislation			
E_0^{leg}	0.009**	0.011***	0.012***	0.009**	0.018***	0.009***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)
E_2^{leg}	0.016***	0.017***	0.017***	0.016***	0.022***	0.014***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.004)
E_4^{leg}	0.022***	0.021***	0.022***	0.020***	0.024***	0.019***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.006)
			Estimation Summary			
E_4^{leg}+E_4^{inc}	0.022***	0.021***	0.022***	0.020***	0.024***	0.019***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.006)
$\sum$ All	0.022*	0.019	0.021	0.018	0.036***	0.014
	(0.013)	(0.016)	(0.018)	(0.016)	(0.013)	(0.013)
$\sum$ Pre-event	-0.002	-0.002	-0.001	-0.001	0.014	-0.004
	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)	(0.006)
N	191568	191641	191568	191568	191568	191568
Controls	YES	NO	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	NO	YES	YES	YES
Seasonality	NO	NO	NO	YES	NO	NO
Weights	Obs	NO	NO	NO	NO	NO

Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. (1) presents the baseline estimates for the effects at legislation. (2) uses observation (UPC) weights. (3) uses observation weights and adds division-time fixed effects. (4) does not contain the control variables. (5) does not contain store fixed effects. (6) accounts for state-specific calendar month fixed effects. (7) does not correct for temporary price changes. (8) uses a winsorized outcome (98% winsorization). $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A4 – Effects for small and large stores

Dep. variable: Store inflation	(1) Small (revenue)	(2) Small (prod. range)	(3) Large (revenue)	(4) Large (prod. range)
		Legislation		
E_0^{leg}	0.015***	0.012**	0.007**	0.007***
	(0.005)	(0.005)	(0.003)	(0.003)
E_2^{leg}	0.013**	0.008	0.016***	0.018***
	(0.005)	(0.005)	(0.005)	(0.005)
E_4^{leg}	0.006	0.003	0.020***	0.022***
	(0.006)	(0.006)	(0.006)	(0.006)
		Implementation		
E_0^{inc}	-0.010	-0.008	0.004	0.003
	(0.008)	(0.008)	(0.005)	(0.005)
E_2^{inc}	-0.007	-0.000	0.006	0.003
	(0.007)	(0.007)	(0.007)	(0.007)
E_4^{inc}	-0.007	-0.002	0.015	0.013
	(0.008)	(0.008)	(0.010)	(0.009)
		Estimation Summary		
E_4^{leg}+E_4^{inc}	-0.000	0.001	0.034**	0.035***
	(0.011)	(0.012)	(0.013)	(0.013)
$\sum$ All	0.001	0.000	0.042*	0.047**
	(0.015)	(0.018)	(0.022)	(0.021)
$\sum$ Pre-event	0.011*	0.006	0.004	0.010
	(0.006)	(0.008)	(0.010)	(0.010)
N	95077	103473	111400	103004
Controls	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Store FE	YES	YES	YES	YES
Division time FE	YES	YES	YES	YES

Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A5 – Effects for cheap and expensive stores

Dep. variable: Store inflation	(1) Cheap (state)	(2) Cheap (county)	(3) Expensive (state)	(4) Expensive (county)
		Legislation		
E_0^{leg}	0.012***	0.013***	0.003	0.003
	(0.003)	(0.003)	(0.005)	(0.006)
E_2^{leg}	0.015***	0.015***	0.008	0.004
	(0.004)	(0.005)	(0.007)	(0.009)
E_4^{leg}	0.014***	0.013**	0.008	0.013
	(0.005)	(0.005)	(0.009)	(0.009)
		Implementation		
E_0^{inc}	-0.002	-0.001	-0.001	-0.004
	(0.005)	(0.006)	(0.006)	(0.007)
E_2^{inc}	0.003	0.003	-0.000	-0.009
	(0.006)	(0.006)	(0.008)	(0.011)
E_4^{inc}	0.007	0.010	0.012	0.005
	(0.008)	(0.008)	(0.010)	(0.014)
		Estimation Summary		
E_4^{leg}+E_4^{inc}	0.021**	0.023**	0.020	0.018
	(0.010)	(0.010)	(0.013)	(0.017)
$\sum$ All	0.024	0.034*	0.046**	0.037
	(0.016)	(0.017)	(0.022)	(0.032)
$\sum$ Pre-event	0.005	0.015*	0.030**	0.018
	(0.007)	(0.008)	(0.013)	(0.017)
N	155518	126557	50959	32658
Controls	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Store FE	YES	YES	YES	YES
Division time FE	YES	YES	YES	YES

Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A6 – Effects for large and small, regional and national brands

Dep. variable: Store inflation	(1) Small Chain	(2) Regional Chain	(3) Large Chain	(4) National Chain
		Legislation		
E_0^{leg}	0.011***	0.010**	0.007***	0.006*
	(0.004)	(0.004)	(0.002)	(0.004)
E_2^{leg}	0.018***	0.016***	0.007*	0.008
	(0.005)	(0.005)	(0.004)	(0.006)
E_4^{leg}	0.018***	0.016**	0.005	0.009
	(0.006)	(0.007)	(0.005)	(0.008)
		Implementation		
E_0^{inc}	0.003	0.004	-0.009	-0.012
	(0.005)	(0.005)	(0.008)	(0.009)
E_2^{inc}	0.006	0.005	-0.005	-0.006
	(0.007)	(0.007)	(0.007)	(0.009)
E_4^{inc}	0.013	0.011	-0.005	-0.006
	(0.008)	(0.009)	(0.010)	(0.012)
		Estimation Summary		
E_4^{leg}+E_4^{inc}	0.031**	0.027**	0.001	0.003
	(0.011)	(0.013)	(0.012)	(0.016)
$\sum$ All	0.037**	0.037*	0.003	-0.004
	(0.018)	(0.020)	(0.017)	(0.019)
$\sum$ Pre-event	0.007	0.012	0.003	-0.004
	(0.007)	(0.009)	(0.010)	(0.011)
N	108336	131959	98141	74518
controls	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Store FE	YES	YES	YES	YES
Division time FE	YES	YES	YES	YES

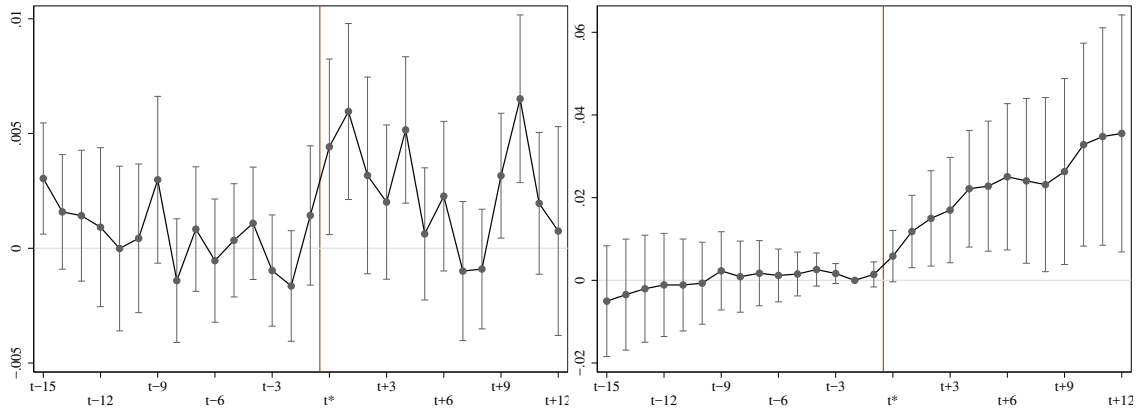
Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A7 – State-level estimations

	Separate estimation				Joint estimation	
Dep. variable: State inflation	(1) Baseline	(2) Weights	(3) Baseline	(4) Weights	(5) Baseline	(6) Weights
			Legislation			
E_0^{leg}	0.005	0.006*			0.004	0.005
	(0.004)	(0.003)			(0.004)	(0.003)
E_2^{leg}	0.013*	0.014**			0.010	0.012*
	(0.007)	(0.005)			(0.007)	(0.006)
E_4^{leg}	0.019**	0.020***			0.016*	0.016**
	(0.008)	(0.007)			(0.009)	(0.007)
			Legislation			
E_0^{inc}			0.008	0.008	0.007	0.008
			(0.007)	(0.006)	(0.007)	(0.006)
E_2^{inc}			0.009	0.011	0.009	0.011
			(0.010)	(0.009)	(0.010)	(0.010)
E_4^{inc}			0.013	0.015	0.013	0.016
			(0.012)	(0.011)	(0.012)	(0.012)
			Estimation Summary			
E_4^{leg}+E_4^{inc}	0.019**	0.020***	0.013	0.015	0.029*	0.032*
	(0.008)	(0.007)	(0.012)	(0.011)	(0.017)	(0.016)
$\sum$ All	0.018	0.021	0.044**	0.048**	0.051*	0.057**
	(0.015)	(0.013)	(0.022)	(0.019)	(0.026)	(0.023)
$\sum$ Pre-event	-0.003	-0.002	0.024**	0.026**	0.014	0.016
	(0.006)	(0.006)	(0.010)	(0.010)	(0.010)	(0.011)
N	5330	5330	5330	5330	5330	5330
Controls	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES
Weights	NO	Var	NO	Var	NO	Var

Notes: The dependent variable is the state-level inflation rate. Baseline controls are the state unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. Estimations with "Var" weights use the inverse of the variance of the state-level price series as weight to account for the fact that inflation series in states with few stores are more noisy. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A3 – State level estimates of the price effects of the minimum wage around the time of legislation, using an extended event window



(a) Effect on the inflation rate **(b) Effect on the price level**

Notes: The figure presents estimates using state level price indices and an extended event window of $k = -15$ to $k = 12$, focusing on the minimum wage effects at legislation. The dependent variable is the state-level month-on-month inflation rate. The panel on the right presents the estimates of α_r and the left panel their cumulative sum over the 24 month panel. Each panel also shows corresponding 90% confidence intervals based on SE clustered on the state level. The controls included are time and state FE, local unemp. rate and house price growth.

Two labor types and Cobb-Douglas production

Let us start with the simple case with just two labor types. We assume that grocery stores produce retail services using minimum wage labor L_M, skilled labor L_H and merchandise X with a constant returns to scale Cobb-Douglas technology, $Q = L_M^{\alpha} L_H^{\beta} X^{1-\alpha-\beta}$. Furthermore, we assume that factor markets are competitive. MW denotes the minimum wage, W the market wage of non-minimum wage labor, and P_X the price of merchandise. The cost function of a cost-minimizing grocery store in this setting equals:

$$C(Q, MW, W, P_X) = QMW^{\alpha} W^{\beta} P_X^{1-\alpha-\beta} \Omega \tag{D.1}$$

Ω is a constant that depends on α and β. With CES demand, firms charge a constant markup over marginal cost. As a result, the minimum wage elasticity of prices is equal to the minimum wage elasticity of marginal cost:

$$\frac{\partial P}{\partial MW}\frac{MW}{P} = \frac{\partial C_Q}{\partial MW}\frac{MW}{C_Q} = \alpha \tag{D.2}$$

In the Cobb-Douglas case, α also corresponds to the minimum wage share in cost:

$$\frac{MWL_M}{C} = \frac{MW\frac{\partial C}{\partial MW}}{C} = \alpha$$

A more general case

Let us now generalize this derivation to the case with more than two types of workers and to any homogeneous production function. We assume that the production technology is $Q = F(L, X)$, $L = G(L_1, L_2, \ldots, L_N)$, with factor prices $P_x, W_1, W_2, \ldots W_N$. F is assumed to be homogeneous of degree h and G is assumed to be linearly homogeneous. We assume competitive labor markets. We derive the elasticity of marginal cost to minimum wages keeping output constant.

Deriving the correct labor cost index

First, we are interested in the correct factor price index $\overline{W}$ that represents the marginal cost of increasing L. The firm minimizes labor cost LC:

$$LC(L, W_1, W_2, \ldots, W_N) = \min_{L_1, L_2, \ldots, L_N} W_1L_1 + W_2L_2 + \cdots + W_NL_N$$
$$\text{s.t. } L = G(L_1, L_2, \ldots, L_N)$$

The FOC for any L_i is that $\lambda G_i' = W_i$. λ_i is the Lagrange multiplier and equal to marginal labor cost LC_L. Because G is homogeneous of degree one, it follows that:

$$LC(L, w_1, w_2, \ldots, w_N) = \lambda \sum_{i=1}^{N} G_i' L_i = \lambda L$$

Since λ is equal to marginal cost of increasing labor inputs, we can plug in $\lambda = LC_L$ and solve the resulting differential equation $LC = LC_L L$ to get that $LC = \overline{W}L$ for some $\overline{W}$ that is constant in L. As a result, marginal cost equals average cost, both are independent of the overall level of L, and $\overline{W} = LC/L$:

$$\overline{W}(W_1, W_2, \ldots, W_N) = \sum_{i=1}^{N} \frac{W_i L_i^*}{L}$$

Deriving an expression for the elasticity

We can express the overall cost function as $C(\overline{W}, P_x, Q)$ and the overall marginal cost function as $C_Q(\overline{W}, P_x, Q)$. The derivative of marginal cost w.r.t. minimum wages can be written as:

$$\frac{\partial C_Q}{\partial MW} = \frac{\partial \frac{\partial C}{\partial Q}}{\partial \overline{W}} \frac{\partial \overline{W}}{\partial MW} = \frac{\partial L}{\partial Q} \frac{\partial \overline{W}}{\partial MW}$$

The last step uses Shepard's Lemma. Converting the derivative to an elasticity:

$$\frac{\partial C_Q}{\partial MW} \frac{MW}{C_Q} = \underbrace{\frac{\overline{W}L}{C}}_{(1)} \underbrace{\frac{\partial \overline{W}}{\partial MW} \frac{MW}{\overline{W}}}_{(2)} \underbrace{\frac{AC}{MC}}_{(3)} \underbrace{\frac{\partial L}{\partial Q} \frac{Q}{L}}_{(4)}$$

The minimum wage elasticity of marginal cost is given by the product of: (1) The cost share of labor cost in total variable cost; (2) the minimum wage elasticity of the average wage; (3) the ratio of average to marginal cost; (4) the output elasticity of labor demand.

Final step

We now show that $\frac{AC}{MC} \frac{\partial L}{\partial Q} \frac{Q}{L} = 1$ when F is homogeneous of degree h. If $h = 1$, both $\frac{AC}{MC} = 1$ and $\frac{\partial L}{\partial Q} \frac{Q}{L} = 1$. More generally, if F is homogeneous of degree h, we can write the cost function as $C = Q^{\frac{1}{h}} \omega$, where ω is constant in Q and typically depends on factor prices. As a result:

$$\frac{AC}{MC} = \frac{Q^{\frac{1}{h}-1}\omega}{\frac{1}{h} Q^{\frac{1}{h}-1}\omega} = h$$

and applying Shepard's Lemma:

$$\frac{\partial L}{\partial Q} \frac{Q}{L} = \frac{\partial \frac{\partial C}{\partial \overline{w}}}{\partial Q} \frac{Q}{\frac{\partial C}{\partial \overline{w}}} = \frac{\partial (Q^{\frac{1}{h}} \frac{\partial \omega}{\partial \overline{w}})}{\partial Q} \frac{Q}{Q^{\frac{1}{h}} \frac{\partial \omega}{\partial \overline{w}}} = \frac{1}{h} Q^{\frac{1}{h}-1} Q^{1-\frac{1}{h}} = \frac{1}{h}$$

As a result $\frac{AC}{MC} \frac{\partial L}{\partial Q} \frac{Q}{L} = 1$, and

$$\frac{\partial C_Q}{\partial MW} \frac{MW}{C_Q} = \frac{\overline{W}L}{C} \frac{\partial \overline{W}}{\partial MW} \frac{MW}{\overline{W}}$$

The minimum wage elasticity of marginal cost is equal to the minimum wage elasticity of the average wage, times the labor share in cost.

It is instructive to discuss the three assumptions that are central for this derivation. First, we need to assume that different labor inputs can be aggregated in a linear homogeneous way. This implies that the shares of different types of workers do not depend on the size of a store. Second, we need to assume that grocery stores' overall production technology is homogeneous to some degree. This assumption is much less restrictive and is fulfilled by all commonly used production functions we are aware of. Finally, we derive these predictions assuming constant output. Output does not matter for marginal cost in the case of constant returns to scale. In the case of non-constant returns, any change in output affects marginal cost in a way we do not account for here. We look into the effects on minimum wages on grocery store output in Table A16 in the appendix and do not find any evidence for a change in grocery stores' output.

E Further evidence on the earnings elasticity in grocery stores

We first present some additional statistics on minimum wage employment in the grocery sector. Table A8 presents the share of workers below 110% and 130% of the local minimum wage in employment, hours and earnings of grocery stores. We also compare the share to other relevant industries. These statistics complement the full wage distribution in grocery store employment presented in Figure 2.4. The shares in hours are lower than in employment—as minimum wage workers are more likely to work part-time—and in earnings, as minimum wage workers have the lowest hourly wages.

In the main part of the paper (Section 5) we report regressions that show that earnings in grocery stores are strongly affected by minimum wage hikes. This section discusses several extensions to this result. In Table A9 we first look into the dynamics of the wage effects by including leads and lags of the minimum wage to the regression. We find that the earnings effect of the minimum wage hike are concentrated in the period when the hike is implemented. The leads and lags are generally not statistically significant. Second, the table also reports the results of specifications that account for Census-division period fixed effects (columns 2, 5, 8) and that weight the regressions with county average total employment (columns 3, 6, 9). The results are similar as in our baseline table reported in the main part of the paper.

In Table A10, we study how the estimated earnings elasticity varies with the bindingness of the minimum wage in a county. We expect larger earnings effects in counties where the difference between the new minimum wage and the initial prevailing wage is larger. For each county, industry and each minimum wage hike, we thus compute the difference between the new minimum wage after the hike and the prevailing average wage in the respective industry four quarters before the hike.[3] For each county and industry, we then average these differences

[3]The difference is estimated by computing a rough measure for the quarterly earnings of a

Table A8 – Statistics on minimum wage employment in grocery stores and other relevant sectors

	Employment		Hours		Earnings	
	$\leq 110\%$	$\leq 130\%$	$\leq 110\%$	$\leq 130\%$	$\leq 110\%$	$\leq 130\%$
			2001 - 2005			
Grocery Stores	12.1	29.6	9.0	23.0	4.5	13.1
Other Retail Trade	7.6	18.5	5.6	14.1	2.2	6.5
Restaurants	31.7	50.2	26.1	42.0	13.1	25.0
Other sectors	4.0	8.5	3.1	6.8	0.9	2.3
			2006 - 2009			
Grocery Stores	20.7	38.8	16.1	31.4	8.9	19.0
Other Retail Trade	11.6	25.0	8.5	19.3	3.6	9.4
Restaurants	39.5	58.3	32.9	50.1	18.3	31.9
Other sectors	5.2	11.1	4.1	9.0	1.2	3.2
			2010 - 2012			
Grocery Stores	25.1	48.8	19.2	40.3	11.1	25.4
Other Retail Trade	15.9	34.8	11.8	27.4	5.3	13.9
Restaurants	45.2	66.5	37.9	58.1	22.5	39.4
Other sectors	6.5	14.7	5.1	12.0	1.6	4.4

Notes: Based on CPS ORG data. Retail trade corresponds to NAICS 44–45, grocery stores to NAICS 4451, and restaurants to NAICS 722. Wages are computed using reported hourly wages for workers paid by the hour, and weekly earnings divided by weekly hours for other workers. Shares are calculated first for each state and year and subsequently averaged over all states and years in a period. All statistics are calculated using the CPS earnings weight.

over all hikes in a county. We use this average difference to assign counties into four groups in terms of the bindingness of the minimum wage, based on the county's position in the distribution of differences between prevailing wage and new minimum wage. If it belongs to the first quartile of this distribution, the county is considered a county where the minimum wage has low bindingness in the respective sector. If it belongs to the top quartile of the distribution, the minimum wage is considered to be strongly binding. Table A10 reports separate earnings elasticities for the four categories of counties. In the case of grocery stores, the earnings elasticity is larger than our baseline elasticity in counties in which the minimum wage is strongly binding. We find no differences within the remaining three groups of counties. In each of them, the elasticity is slightly lower than our baseline estimate.

full-time minimum wage worker. We do this by multiplying the hourly minimum wage by eight hours and $22 * 3$ days per quarter.

F Minimum wages and prices of COGS

In this section, we discuss the possibility that minimum wages increase the cost of goods sold (COGS). As shown in Table 2.3, COGS make up about 83% of grocery stores' variable cost. Moreover, retail stores have been shown to be very responsive to changes in COGS (see e.g. Eichenbaum et al. (2011) or Nakamura and Zerom (2010)). If minimum wage workers are employed in the production of grocery products, producers may also increase their prices. Due to the high cost share of COGS in retailers' cost, even a relatively minor increase in producer prices could affect retail prices.

Our price data does not include any measure of wholesale cost, and we cannot estimate the impact of minimum wages on the wholesale cost of grocery products directly. We thus follow MaCurdy (2015) and use input-output tables to calculate a prediction of the elasticity of prices for sectors producing groceries under the assumption of full pass-through all along the production chain.

The input-output tables of the national accounts cover sectoral labor shares[4] s_j^L, which we use as the labor cost elasticity of prices for this sector. We use minimum wage shares in sectoral earnings s_j^{mw} computed from the CPS as the elasticity of labor cost to minimum wages. Finally, we compute the value of output of industry j used to product one dollar of output in industry i from the input-output tables.[5] We denote this coefficient $\alpha_{i,j}$. We can then predict the minimum wage elasticity of producer prices in each sector as:

$$\frac{\partial P_i}{\partial MW}\frac{MW}{P_i} = \sum_j \alpha_{i,j} \cdot s_j^L \cdot s_j^{mw} \tag{F.1}$$

We present the predicted elasticity of producer prices based on equation F.1 in Table A11. We use the domestic requirements table for 389 disaggregated industries provided by the BEA. We predict the elasticity for 26 manufacturing industries that are relevant for grocery stores. All calculations are based on data from 2007. Columns 2 and 3 present the direct cost elasticity, which captures the impact of minimum wage workers employed in the sector itself. These elasticities are quite small. Columns 4 and 5 present the final elasticities, which also capture predicted price increases of inputs. These elasticities are substantially larger. The difference is driven by low wages in the sectors that deliver primary inputs to food manufacturing sectors. We present both measures for minimum wage shares based on workers earning below 110% and 130% of the local minimum wage.

Overall, the elasticities reported in Table A11 are of similar magnitude as the direct impact of increases in labor cost on retail marginal cost. The output-weighted average predicted elasticity of producer prices in grocery manufacturing industries amounts to 0.018 when we use 110% of the minimum wage to define minimum wage workers and 0.026 when we use 130%. Full pass-through in manufacturing industries could thus affect the marginal cost of grocery stores

[4] These labor shares are in revenues, not cost.

[5] This corresponds to the i, j entry of the domestic requirements matrix in the input-output tables.

to a comparable extent as the direct effect of increasing labor costs.

The extent to which increases in COGS affect our estimates depends on whether they are passed through, but also on whether they occur locally. If wholesale groceries are perfectly tradeable, a minimum wage hike would increase COGS equally for stores everywhere, and any pass-through of this cost increase would be absorbed in time fixed effects in our baseline estimation.

Our data does not contain information about where a particular product is produced. However, we can study the origin composition of groceries sold in a state using grocery wholesale-to-retail flows reported in the Commodity Flow Survey.[6] This dataset covers sales of manufacturing companies, but also intermediaries such as merchant wholesalers or warehouses. As a result, we cannot identify the location of production with certainty.

We analyze the origin composition of products sold in retail using data on intrastate trade flows from wholesalers for groceries, farm products, alcoholic beverages and drugs (subsequently summed up under the term "groceries") provided in the 2007 Commodity Flow Survey. The Commodity Flow Survey data are subject to some important limitations. First, it counts flows originating from manufacturers, but also from distribution centers and similar establishments. The latter may not be produced locally. Second, the flows capture all flows originating from merchant wholesalers, irrespective of the destination industry. Merchant wholesalers are defined by selling to retail establishments, but the flows in the CFS capture not just flows to grocery stores but also other retail establishments. The numbers we calculate here should be interpreted as very suggestive evidence.

Let Y_{ij} be the flow of groceries from state i to state j. We calculate "production" of state s valued at wholesale prices as the sum of all flows originating in state s, $\sum_j Y_{sj}$. We can calculate "consumption" of state s as all flows with destination in state s, $\sum_i Y_{is}$. The share of locally produced products in grocery consumption of state s is then given by $Y_{ss}/\sum_i Y_{is}$. The exposure of state s to cost increases in another state S can be calculated as $Y_{Ss}/\sum_i Y_{is}$.

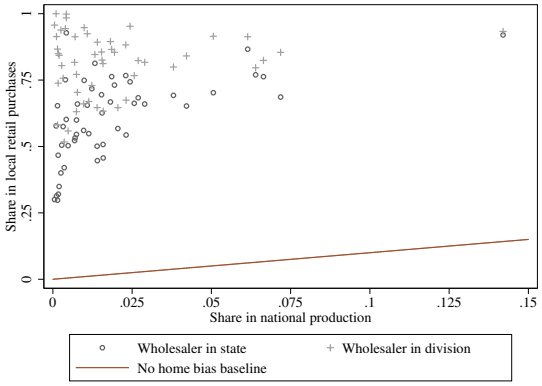
Our results suggest that the share of local products in grocery consumption is higher than the state's share in national grocery production. For example, California has a 14% share in the national production of groceries and 91% of groceries consumed in California are produced locally. Vermont accounts for a mere 0.1% of US grocery production, yet 30% of groceries consumed in Vermont are produced locally. This suggests a substantial home bias in US grocery consumption, a fact that has been documented for interstate trade as a whole in Wolf (2000). We document this relationship in Figure A4.

Table A12 documents trade flows for all states. The share of local grocery products in consumption (Destination) is systematically higher than the share of states' products in national production (Origin). Flows from other states are small on average. Even in small states like Delaware or Rhode Island, the average flow from other states amounts to less than 1.5% of consumption.

Overall, these results suggest that a disproportionate share of grocery products are delivered by wholesalers located in the same state or census division as the retailers they supply. We interpret this as evidence for some home bias

[6]The Commodity Flow Survey has been used to document home bias in intra-national trade in the US by Wolf (2000). We refer the reader to his paper for a detailed description of the data.

Figure A4 – Home bias in grocery wholesale-to-retail flows



in grocery consumption. Consequently, it is possible that some effects of local wholesale price changes would be captured in our estimation, especially if we do not account for chain-time fixed effects.

G Details on welfare calculations

Our calculation of the nominal gains of a hypothetical 20% increase in all binding minimum wages is based on the joint distribution of hours worked and wages (from the CPS Monthly Outgoing Rotation Group) and household incomes and weeks worked during a year (from the Annual Socioeconomic Supplement). We use the ASEC and MORG files provided by the NBER. We first calculate wages for the March 2011 MORG. For workers paid by the hour, we use reported hourly wages. For workers not paid by the hour, we use weekly earnings divided by usual weekly hours to calculate the hourly wage. We then merge the March 2011 ASEC to the March 2011 MORG. For every person i in the MORG, we calculate the distance of the hourly wage to the local binding minimum wage $W_i/MW_{s(i)}$. We then construct a counterfactual labor income as described in equation 6.2. We set hours and wages to zero for all workers that are not coded as "non-self-employed workers for pay". When the $weeks_i$ variable is missing, but weekly earnings and annual labor income is observed, we impute weeks based on this information and cap it at 52. If we cannot calculate labor income for one household member, we exclude the entire household from the analysis.

In Table A13, we report some additional statistics relevant to our calculations. We compare annualized labor earnings based on the March 2011 MORG and reported weeks worked to actual reported labor income in the ASEC. Our calculation fits reported earnings quite well for households earning between \$20,000 and \$70,0000 a year. The annualized measure is larger than reported labor earnings for poorer and smaller for richer households. Two factors could explain this discrepancy. First, labor market conditions were improving in March 2011 after the through of the recession in 2010. Hours and wages of poor households could thus be higher in March 2011 than during 2010. Furthermore, the discrepancy for rich households could be due to differences in top-coding between the MORG

and the ASEC. Furthermore, we present summary statistics on wages, hours, weeks worked and the size of households in different brackets. In Table A14 we present summary statistics of minimum wage workers in the different brackets. There are some important differences between minimum wage workers in poor and rich households. Most importantly, minimum wage workers in richer households tend to be the children of the CPS reference person. In poorer households, minimum wage workers are more likely to be female.

Finally, we report the numbers corresponding to Figures 2.11a, 2.11b, and 2.12 in Tables A15 and A15b. The Tables do not contain any information not depicted in the Figures, but provide a more readable summary of the results.

H Effects of minimum wages on output

Table A16 presents the results of equation 3.1 estimated with quantities and revenues as dependent variables. Quantity indices are constructed the same way as the price index. Log revenues are total store revenues. We find no significant impact. Both outcome variables have a substantially higher variance than price indices. Note that the gap between quantity indices and revenues is insignificant but largely consistent with the price response we estimate.

I Comparison to Ganapati and Weaver (2017) and Leung (2017)

Two closely related contemporaneous papers also study the effects of minimum wages on prices in grocery stores. Ganapati and Weaver (2017) and Leung (2017) both use scanner data provided by Nielsen that covers a somewhat different time period. Despite using the same data, the two papers reach different conclusions: Leung (2017) finds much larger elasticities than we do, while Ganapati and Weaver (2017) find no effects of the minimum wage on grocery prices.

Our empirical analyses differ from those of Leung (2017) in two and from those of Ganapati and Weaver (2017) in three main respects. Most importantly, both contemporaneous papers study the effects of minimum wage increases at the time of implementation, while our main effect occurs at the time legislation is passed (a finding that was subsequently confirmed in an later version of Leung (2017)). Second, our econometric approach is different to both Leung (2017) and Ganapati and Weaver (2017) . We estimate pass-through regressions in first differences, which relate inflation to changes in the minimum wage and include fixed effects that control for differential inflation trends. Both other papers estimate level regressions and therefore control for a different set of fixed effects. Moreover, the leads and lags in the pass-through regressions allow us to study the timing of the effect in detail.

Leung (2017) finds large minimum wage elasticities of prices of around 0.06. He focuses on what we call the implemented minimum wage. His elasticity estimates are about 3 times our estimate at the time of legislation. There are various explanations for this difference. First, Leung's data cover a different time

period, from 2006–2015. As we show in 2.4, minimum wages are considerably more binding toward the end of our sample period, and as Leung shows, his elasticity estimates are especially higher than ours during the later years of the Nielsen data that our dataset does not cover. Second, his estimation does not control for differential trend inflation rates in different stores. In Table A17 we replicate a specification similar to his baseline and also find larger elasticities. However, as shown in the table, including store-level linear time trends—that are captured by store fixed effects in our baseline first difference regressions— reduces these estimates to similar values as in our baseline first difference specification. Third, when including the legislated minimum wage in these regressions, it becomes clear that this variable, rather than the implemented minimum wage is driving the results. Finally, we want to highlight that the table suggests that our first-difference specification is more efficient. The level coefficients including linear trends are of a similar magnitude, but less precisely estimated. This is to be expected. Both prices and minimum wages are not stationary in levels. Furthermore, the price level is highly autocorrelated due the stickiness of prices

Ganapati and Weaver (2017) have a different empirical approach than Leung (2017) and our paper and their results are thus more difficult to compare to ours. Instead of constructing store-level price indices, they draw a 1% sample of 5000 unique products from their data, collapse prices to the county-product level, and estimate the effects with county-product combinations as their unit of observation. All of their specifications include product-time fixed effects and thus absorb potential wholesale price changes (see our discussion in section C). Many grocery products are chain- or region- specific and their baseline specification thus absorbs some variation that we absorb through chain-time or division-time fixed effects. Overall, our results are compatible with their findings: there is no robust effect of minimum wage increases on prices at the time that they are implemented. However, since we find effects at the time of legislation, our conclusions are different from the ones they draw.

There are several reasons why chose to estimate our models on the store rather than the product level.[7] First, we view it as ex ante desirable to weight products by their importance to both consumers and grocery stores. Moreover, entry and exit rates at the product-store level are very high in retail, since low-volume products are frequently introduced and discontinued, and may also go unsold in for extended time periods due to stock-outs, seasonality or low demand. Indeed, when Ganapati and Weaver (2017) only use a subset of products to form a balanced panel, their sample size shrinks by 75%. Using revenue weights in index construction has the attractive side effect of assigning low weights to products that are likely to exit or to have frequent gaps in their price series. Entry and exit is much less pronounced at the store level.

[7]Both, Ganapati and Weaver (2017) and Leung (2017) also discuss the advantages and disadvantages of using indices versus product-county level prices in the appendices to their papers.

Table A9 – Robustness: Earnings and employment elasticities to the minimum wage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Retail trade			Grocery stores			Acc. and food services		
Panel A: Earnings									
t-4	0.011			0.004			-0.019		
	(0.020)			(0.035)			(0.027)		
t-3	0.022			0.043			0.037*		
	(0.015)			(0.039)			(0.019)		
t-2	-0.021*			-0.024			-0.042		
	(0.012)			(0.037)			(0.026)		
t-1	-0.003			-0.001			0.057*		
	(0.010)			(0.030)			(0.030)		
t	0.039**	0.075***	0.048*	0.056*	0.062*	0.108**	0.046*	0.171***	0.151***
	(0.015)	(0.020)	(0.026)	(0.029)	(0.036)	(0.043)	(0.027)	(0.027)	(0.024)
t+1	0.011			0.011			0.073***		
	(0.018)			(0.037)			(0.022)		
t+2	0.009			0.021			-0.011		
	(0.013)			(0.034)			(0.030)		
t+3	-0.003			-0.008			0.057*		
	(0.010)			(0.027)			(0.029)		
t+4	-0.013			0.036			-0.029		
	(0.015)			(0.034)			(0.024)		
Obs	124,000	124,000	124,000	80,722	80,722	80,722	98,056	98,056	98,056
Panel B: Employment									
t-4	0.026			-0.076			-0.035		
	(0.021)			(0.050)			(0.034)		
t-3	-0.018**			0.010			0.054**		
	(0.007)			(0.016)			(0.021)		
t-2	0.021*			0.005			0.031		
	(0.011)			(0.014)			(0.022)		
t-1	0.007			0.002			-0.001		
	(0.010)			(0.010)			(0.024)		
t	-0.017*	0.029*	-0.002	-0.018	0.072**	-0.010	-0.055**	-0.008	-0.042
	(0.009)	(0.016)	(0.027)	(0.018)	(0.036)	(0.048)	(0.024)	(0.026)	(0.033)
t+1	-0.012			0.018			0.022		
	(0.007)			(0.014)			(0.021)		
t+2	0.017			0.005			0.013		
	(0.011)			(0.014)			(0.023)		
t+3	0.014			0.003			-0.019		
	(0.011)			(0.014)			(0.028)		
t+4	-0.033*			0.034			-0.069**		
	(0.016)			(0.041)			(0.026)		
Obs	124,000	124,000	124,000	80,722	80,722	80,722	98,056	98,056	98,056
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Div.-time FE	N	Y	N	N	Y	N	N	Y	N
Weights	N	N	Y	N	N	Y	N	N	Y

Notes: The table shows elasticities to state-level minimum wages in the 2001–2012 period by industry, estimated using county-level panel data from the QCEW for the 41 states used in our price regression. Retail trade corresponds to NAICS 44–45, grocery stores to NAICS 4451, and accommodation and food services to NAICS 72. The outcome in panel A is log the average earnings by industry. The outcome in Panel B is the log of the number of workers by industry, computed as the average employment of the three months in the respective quarter. Controls are log of county population and the log of total employment in private industries per county. SE are clustered at the state level.

Table A10 – Earnings elasticities by bindingness of the minimum wage

	(1) Strongly binding	(2) Moderately binding	(3) Weakly binding	(4) Very weakly binding
		Grocery stores		
log MW	0.155***	0.081**	0.083*	0.079
	(0.045)	(0.033)	(0.042)	(0.067)
N	16,567	19,200	21,406	19,851
		Retail trade		
log MW	0.081***	0.026	0.010	0.007
	(0.024)	(0.025)	(0.026)	(0.033)
N	28,606	30,840	32,216	32,139
		Accomodation and food services		
log MW	0.168***	0.183***	0.184***	0.079***
	(0.026)	(0.022)	(0.033)	(0.028)
N	21,242	23,724	25,076	25,880

Notes: The table shows elasticities to state-level minimum wages in the 2001–2012 period by industry, estimated using county-level panel data from the QCEW for the 41 states used in our price regression. Retail trade corresponds to NAICS 44–45, grocery stores to NAICS 4451, and accommodation and food services to NAICS 72. The outcome is log the average earnings by industry. Controls are log of county population and the log of total employment in private industries per county. SE are clustered at the state level. The minimum wage bindingness is the average county-level difference between the industry-specific wage (4 quarters before a subsequent hike) and the new minimum wage, averaged across all hikes in a county. The four categories correspond to quartiles of the distribution of this gap.

Table A11 – Predicted MW elasticities of producer prices in grocery manufacturing

Manufacturing Sector	Direct cost elasticity		Final cost elasticity	
MW worker definition:	$< 110\%$	$< 130\%$	$< 110\%$	$< 130\%$
Breakfast cereal	0.003	0.005	0.019	0.029
Sugar and confectionery	0.006	0.009	0.020	0.030
Frozen food	0.006	0.009	0.019	0.029
Fruit and vegetable canning, pickling, and drying	0.005	0.008	0.018	0.026
Fluid milk and butter	0.003	0.005	0.018	0.026
Cheese	0.002	0.003	0.017	0.024
Dry, condensed, and evaporated dairy	0.002	0.003	0.015	0.022
Ice cream and frozen dessert	0.004	0.005	0.018	0.026
Animal slaughtering, rendering, and processing	0.005	0.008	0.022	0.033
Poultry processing	0.007	0.012	0.020	0.030
Seafood product preparation and packaging	0.009	0.013	0.020	0.031
Bread and bakery	0.012	0.018	0.017	0.026
Cookie, cracker, pasta, and tortilla	0.008	0.012	0.019	0.029
Snack food	0.008	0.012	0.029	0.042
Coffee and tea	0.008	0.012	0.017	0.025
Flavoring syrup and concentrate	0.013	0.019	0.024	0.035
Seasoning and dressing	0.011	0.016	0.026	0.039
All other food	0.011	0.016	0.011	0.018
Soft drink and ice	0.002	0.003	0.008	0.012
Breweries	0.001	0.002	0.013	0.021
Wineries	0.002	0.004	0.004	0.007
Distilleries	0.001	0.002	0.004	0.006
Tobacco	0.001	0.001	0.022	0.033
Sanitary paper	0.004	0.005	0.011	0.016
Soap and cleaning compound	0.002	0.003	0.006	0.010
Toilet preparation	0.002	0.003	0.006	0.010
Output weighted average	0.005	0.008	0.016	0.024
Average	0.005	0.008	0.018	0.026

Table A12 – Summary of wholesale-to-retail flows between states

	Share in National Total		Share in Consumption		Flows from other states		
	Consumption	Production	Production in State	Production in Division	Mean Consumption Share	Max Consumption Share	Max Origin
California	0.134	0.142	0.92	0.933	0.002	0.013	New Jersey
Florida	0.06	0.061	0.866	0.913	0.003	0.044	Georgia
Texas	0.067	0.064	0.77	0.796	0.005	0.039	Tennessee
Washington	0.021	0.023	0.767	0.883	0.005	0.063	Oregon
Minnesota	0.019	0.019	0.763	0.865	0.005	0.061	Illinois
Illinois	0.052	0.066	0.763	0.824	0.005	0.093	Missouri
Nebraska	0.008	0.01	0.749	0.948	0.005	0.094	Kansas
Michigan	0.029	0.024	0.743	0.953	0.005	0.083	Ohio
North Carolina	0.02	0.019	0.731	0.855	0.005	0.047	Georgia
Arizona	0.014	0.012	0.717	0.73	0.006	0.2	California
New Jersey	0.039	0.051	0.702	0.915	0.006	0.145	New York
Iowa	0.016	0.015	0.695	0.856	0.006	0.065	Illinois
Ohio	0.039	0.038	0.692	0.799	0.006	0.092	Pennsylvania
New York	0.074	0.072	0.686	0.854	0.006	0.135	New Jersey
Massachusetts	0.026	0.027	0.683	0.823	0.006	0.1	New York
Wisconsin	0.021	0.018	0.668	0.895	0.007	0.215	Illinois
Tennessee	0.017	0.026	0.663	0.767	0.007	0.094	Kentucky
Missouri	0.023	0.029	0.66	0.817	0.007	0.137	Kansas
Utah	0.007	0.008	0.66	0.704	0.007	0.112	Arkansas
Oregon	0.011	0.011	0.655	0.925	0.007	0.179	Washington
Vermont	0.001	0.001	0.653	0.867	0.007	0.14	New Hampshire
Pennsylvania	0.041	0.042	0.652	0.841	0.007	0.104	New Jersey
Kansas	0.015	0.016	0.626	0.825	0.007	0.15	Missouri
Oklahoma	0.009	0.007	0.6	0.771	0.008	0.152	Texas
New Mexico	0.004	0.003	0.575	0.757	0.009	0.17	Texas
Louisiana	0.033	0.02	0.568	0.646	0.009	0.107	Illinois
Alabama	0.012	0.01	0.56	0.661	0.009	0.131	Georgia
Georgia	0.025	0.023	0.543	0.674	0.009	0.147	Tennessee
South Carolina	0.01	0.007	0.532	0.913	0.009	0.189	Georgia
Mississippi	0.007	0.007	0.522	0.817	0.01	0.147	Tennessee
Virginia	0.021	0.016	0.508	0.812	0.01	0.205	Maryland
Idaho	0.004	0.003	0.505	0.805	0.01	0.262	Utah
Connecticut	0.012	0.014	0.501	0.646	0.01	0.188	New York
Maryland	0.016	0.016	0.457	0.633	0.011	0.139	Pennsylvania
Indiana	0.016	0.014	0.447	0.893	0.011	0.266	Illinois
West Virginia	0.004	0.004	0.42	0.517	0.012	0.222	Pennsylvania
Maine	0.005	0.003	0.4	0.938	0.012	0.442	Massachusetts
New Hampshire	0.003	0.002	0.349	0.843	0.013	0.285	Massachusetts
Rhode Island	0.003	0.002	0.32	0.849	0.014	0.364	Massachusetts
DC	0.002	0.001	0.313	0.914	0.014	0.384	Maryland
Delaware	0.002	0.001	0.298	0.581	0.014	0.284	Maryland
Mean	0.023	0.023	0.607	0.811	0.008	0.158	

Table A13 – Summary statistics for different income brackets

	MORG labor income	ASEC labor income	Labor income share	Hourly Wage	Hours worked	Weeks worked	HH size	MW share	Number of HH
less than 10k	2.1	1.5	26.7	12.7	4.7	6.9	1.7	5.8	947.0
10 - 19.99k	6.6	5.5	37.1	11.2	9.1	14.1	1.8	8.4	1764.0
20 - 29.99k	13.3	12.9	52.0	12.4	13.0	20.8	2.1	8.7	1737.0
30 - 39.99k	22.5	22.2	64.4	14.5	16.9	26.2	2.2	7.8	1486.0
40 - 49.99k	29.5	29.7	66.8	15.6	18.9	28.6	2.3	6.4	1287.0
50 - 69.99k	40.9	44.9	76.0	17.0	21.6	32.7	2.6	6.9	2199.0
70 - 79.99k	55.4	59.5	80.2	20.4	24.5	36.3	2.5	6.5	953.0
80 - 99.99k	63.8	72.7	81.7	21.2	25.3	37.1	2.7	6.5	1386.0
100 - 119.99k	79.2	90.4	83.3	24.4	26.2	37.5	2.8	6.1	1053.0
120 - 149.99k	91.2	109.0	82.4	26.8	26.4	39.0	2.8	4.0	892.0
more than 150k	124.5	186.2	82.5	37.3	25.1	38.2	2.9	4.3	1258.0

Notes: MORG labor income is equal to $hours \times wage \times weeks$. Wage and hours are from the MORG, weeks from the ASEC. ASEC labor income is annual labor income reported in the ASEC. The Labor income share is the share of labor in total household income (both from ASEC). Wages, Hours and Weeks worked are unweighted averages over household members, then averaged over households using HH weights.

Table A14 – Characteristics of minimum wage workers in different income brackets

	Relation to Reference Person (RP)					Other characteristics			
	Female RP	Male RP	Child of RP	Other family	Not family	Hours worked	Weeks worked	Age	Female
less than 10k	37.7	13.3	9.1	1.8	38.1	24.7	28.6	31.0	0.7
10 - 19.99k	34.9	21.1	8.6	5.2	30.2	31.4	41.2	34.5	0.6
20 - 29.99k	34.3	15.0	13.6	7.2	29.9	30.6	42.9	37.4	0.6
30 - 39.99k	33.2	15.3	17.3	5.3	28.9	29.6	40.7	37.4	0.6
40 - 49.99k	31.9	9.5	21.5	10.6	26.5	30.3	37.8	31.8	0.6
50 - 69.99k	27.3	18.6	34.5	5.3	14.3	29.2	42.7	32.8	0.6
70 - 79.99k	27.9	15.1	37.8	3.9	15.2	29.6	40.3	31.5	0.6
80 - 99.99k	21.3	18.2	39.2	7.8	13.6	29.0	40.8	32.6	0.4
100 - 119.99k	21.0	9.7	56.4	8.0	4.9	27.0	37.7	27.7	0.5
120 - 149.99k	12.9	8.4	62.6	3.8	12.2	23.6	36.8	27.0	0.5
more than 150k	15.1	6.8	71.4	3.5	3.2	26.8	40.0	26.0	0.4

Notes: The table breaks down minimum wage workers by relationship to the reference person in their household. Minimum wage workers are all workers earning less than 110% of the local minimum wage. Data from MORG (wages) and ASEC (for income brackets).

Table A15 – Nominal gains and Equivalent Variation of grocery price increases after a 20% increase in the minimum wage

(a) Taking into account price effects in grocery stores

	in USD			in % of HH income			$\frac{100 \cdot E_h \Delta P}{\Delta Y_h^L}$
	ΔY_h^L	$E_h \Delta P$	Net	ΔY_h^L	$E_h \Delta P$	Net	
less than 10k	135.95	−13.29	122.66	2.17	−0.21	1.96	−9.77
10 - 19.99k	409.16	−13.86	395.3	2.76	−0.09	2.67	−3.39
20 - 29.99k	557.47	−15.17	542.3	2.25	−0.06	2.18	−2.72
30 - 39.99k	516.27	−18.94	497.33	1.49	−0.05	1.44	−3.67
40 - 49.99k	489.88	−18.17	471.71	1.1	−0.04	1.06	−3.71
50 - 69.99k	565.3	−22.5	542.8	0.96	−0.04	0.92	−3.98
70 - 79.99k	482.66	−26.18	456.48	0.65	−0.04	0.62	−5.42
80 - 99.99k	454.74	−27.52	427.22	0.51	−0.03	0.48	−6.05
100 - 119.99k	385.13	−31.96	353.17	0.35	−0.03	0.33	−8.3
120 - 149.99k	338.96	−34.19	304.77	0.26	−0.03	0.23	−10.09
more than 150k	337.25	−43.14	294.12	0.15	−0.02	0.13	−12.79

(b) Taking into account price effects in grocery stores and restaurants

	in USD			in % of HH income			$\frac{100 \cdot E_h \Delta P}{\Delta Y_h^L}$
	ΔY_h^L	$E_h \Delta P$	Net	ΔY_h^L	$E_h \Delta P$	Net	
less than 10k	135.95	−28.37	107.59	2.17	−0.45	1.72	−20.86
10 - 19.99k	409.16	−30.49	378.67	2.76	−0.21	2.55	−7.45
20 - 29.99k	557.47	−35.79	521.68	2.25	−0.14	2.1	−6.42
30 - 39.99k	516.27	−44.84	471.43	1.49	−0.13	1.36	−8.69
40 - 49.99k	489.88	−45.73	444.14	1.1	−0.1	1.0	−9.34
50 - 69.99k	565.3	−58.21	507.08	0.96	−0.1	0.86	−10.3
70 - 79.99k	482.66	−71.05	411.61	0.65	−0.1	0.55	−14.72
80 - 99.99k	454.74	−76.69	378.06	0.51	−0.09	0.42	−16.86
100 - 119.99k	385.13	−93.35	291.78	0.35	−0.09	0.27	−24.24
120 - 149.99k	338.96	−106.16	232.8	0.26	−0.08	0.18	−31.32
more than 150k	337.25	−131.14	206.11	0.15	−0.06	0.09	−38.88

Notes: The tables shows the nominal gains and Equivalent Variation (EV) of price increases in response to increasing all binding minimum wages in the US by 20%. Table A15 uses Equivalent Variation of price increases in grocery stores. Table A15b uses Equivalent Variation of price increases in grocery stores and restaurants. See section 6 for a more detailed description of the calculations involved. We show the mean nominal gains and EV for each income bracket in US dollars and in % of HH income. ΔY_h^L is the increase in nominal household incomes. $E_h \Delta P$ is the EV of the predicted increase in prices at grocery stores. Net is the remaining welfare effect. $100 \cdot E_h \Delta P / \Delta Y_h^L$ illustrates the % of nominal income gains that is offset by price increases.

Table A16 – Effects of minimum wages on output and revenues

	(1) Quantity index	(2) Log revenues	(3) Quantity index	(4) Log revenues	(5) Quantity index	(6) Log revenues
			Legislation			
E_0^{leg}	-0.031	0.014			-0.026	0.012
	(0.026)	(0.025)			(0.035)	(0.029)
E_2^{leg}	-0.036	0.019			-0.039	0.000
	(0.029)	(0.029)			(0.038)	(0.037)
E_4^{leg}	-0.039	0.001			-0.033	-0.014
	(0.045)	(0.041)			(0.045)	(0.047)
			Implementation			
E_0^{inc}			0.022	0.024	0.006	0.013
			(0.042)	(0.046)	(0.043)	(0.052)
E_2^{inc}			-0.036	-0.013	-0.028	-0.001
			(0.041)	(0.056)	(0.051)	(0.073)
E_4^{inc}			-0.056	-0.044	-0.041	-0.020
			(0.055)	(0.056)	(0.064)	(0.067)
			Estimation Summary			
E_4^{leg}+E_4^{inc}	-0.039	0.001	-0.056	-0.044	-0.073	-0.034
	(0.045)	(0.041)	(0.055)	(0.056)	(0.086)	(0.088)
$\sum$ All	-0.116*	-0.125	-0.090	-0.028	-0.093	-0.085
	(0.068)	(0.077)	(0.074)	(0.086)	(0.103)	(0.093)
$\sum$ Pre-event	-0.012	-0.054	-0.084	-0.018	-0.022	-0.024
	(0.035)	(0.036)	(0.059)	(0.066)	(0.068)	(0.074)
N	201973	201578	201973	201578	201973	201578
Controls	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES

Table A17 – Baseline results using the price level

Dep. variable: log store price level	(1) Baseline	(2) Trend	(3) Baseline	(4) Trend	(5) Baseline	(6) Trend	(7) Chain time FE	(8) Division time FE
log MW	0.040	0.018			0.006	0.010	0.007	-0.003
	(0.039)	(0.019)			(0.031)	(0.017)	(0.016)	(0.011)
log Legisl. MW			0.059*	0.024	0.056**	0.022	0.016*	0.011**
			(0.029)	(0.016)	(0.021)	(0.014)	(0.009)	(0.004)
Observations	222166	222166	222166	222166	222166	222166	222166	221318
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES	YES	YES
Store trend	NO	YES	NO	YES	NO	YES	YES	YES
Chain time FE	NO	NO	NO	NO	NO	NO	NO	YES
Division time FE	NO	NO	NO	NO	NO	NO	YES	NO

Notes: The dependent variable is the store-level price level. MW is the binding minimum wage. The Legislated MW is the highest future minimum wage set in current law. The regressions control for the county unemployment rate, state-level house prices, and county-level population. SE are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C

The Economic Effects of a $15 in the U.S.: A Simulation Approach: Appendix

A Structure of the minimum wage model

Table A1 presents the 27 steps that we use to calibrate our employment estimates. These steps are aggregated into five main parts, labeled Parts A-E in Table A1. Part A describes the number of workers in the state who will receive pay increases by 2024. Part B describes the effects of automation and worker productivity gains. Part C describes how much consumer prices will increase and how much those increases will reduce consumer demand and employment. Part D describes how we calculate the income effect: how much pay increases will increase consumer spending and employment. Part E describes how we add up the estimates in Parts B, C and D to calculate the net effect on employment. We document here the data and methods that we use in each part of Table A1. In section A2, we document the source of the key parameters we use to calibrate our model.

A Part A: Workers affected and wage increase

Lines [1] to [3] in Table A1 use our estimates (described in detail in the first section of the appendix) on how the labor force will grow and how the proposed minimum wage increase would affect the wage distribution of workers in the U.S. The wage estimates include the number of workers directly and indirectly affected in the two scenarios, and their nominal wages with and without the policy. We also use here our estimates of the total wage bill by 2024: $7.55 trillion in the U.S. in $2016 with the minimum wage increases and $7.41 trillion without the policy. We provide the details for these estimates in Section A2.

B Part B: Impact of capital-labor substitution and productivity gains

Part B displays our estimated impact of capital-labor substitution and productivity gains on employment and on the total wage bill. Both calculations estimate the reduction in employment when output is held constant, everything else equal. We estimate the reduction in the number of jobs from substitution effects (line [5] in Table A1) by multiplying four components: (i) the capital-labor substitution elasticity; (ii) the average wage increase of workers getting increases, which we estimate to be 25 percent, (iii) the profit share of revenues, and (iv) the total number of workers getting pay increases. We calculate the reduction in the number of jobs from productivity gains ([6]) by multiplying two components: (i) the productivity gains and (ii) the total number of affected workers (estimated as 5.26 million in our wage simulation model). We calculate the reduction in the wage bill due to substitution effects and productivity gains ([7]) by multiplying the reduction in number of jobs due to capital-labor substitution and productivity gains ([8]) by the nominal average annual earnings of workers who would otherwise remain employed ([9]).

Table A1 – Detailed structure of the minimum wage model

A. Workers affected and wage increases	
Total employment	[1]
Total number of affected (directly and indirectly) workers in 2024	[2]
Working age population growth from 2014 to 2024	[3]
B. Impact of K-L substitution and productivity gains on number of jobs and wage bill	
Reduction in employment from substitution effects and productivity gains	[4] = [5] + [6]
Reduction in employment from substitution effects in 2024	[5]
Reduction in employment from productivity gains in 2024	[6]
Reduction in wage bill due to substitution effects and productivity gains-based job loss	[7] = [8] × [9]/1e6
Reduction in employment from substitution effects and productivity gains	[8] = [4]
Nominal average annual earnings of directly and indirectly affected workers without the policy	[9]
C. Scale effects: increase in consumer prices and reduction in consumer demand	
Percentage increase in consumer prices	[10] = [11]
Percentage increase in operating costs	[11] = [12] × [13]
Payroll share of operating costs	[12]
Net percentage payroll increase, accounting for savings from reduced turnover and productivity gains	[13]
Annual reduction in consumer demand from price increase	[14] = [15] × [16]
Percentage reduction in demand from price increase	[15]
Annual aggregate consumer spending	[16]
Reduction in employment from consumer spending reduction	[17]
Reduction in employment, as a percentage of total employment	[18]
D. Income effects: effects of pay increases on consumer spending and employment	
Net change in compensation for workers (millions)	[19] = [20]−[21]
Total wage increase for workers from proposed minimum wage increase (millions)	[20]
SNAP and ACA benefit reduction	[21]
Increase in employment from wage increase	[22]
Increase in employment, as a percentage of total employment	[23]
E. Net effects	
Cumulative net change in employment	[24]
Cumulative net change in employment, as a percent of total employment	[19] = [25]=[24]/[1]
Annual net change in employment	[26]=[24]/5
Annual net change in employment, as a percent of total employment)	[27] = [25]/5

C Part C: Scale effects: increase in consumer prices and reduction in consumer demand

Part C of Table A1 estimates the percentage increase in consumer prices due to an increase in operating costs for firms and the annual reduction in consumer demand from these price increases. We then use the 2015 IMPLAN model to calculate the impact of this reduction in consumer spending on employment. Our estimates are calculated as follows:

- We assume that the percentage increase in consumer prices [10] is equal to the percentage increase in operating costs [11], following the widely-used Dixit-Stiglitz model of monopolistic competition (Dixit and Stiglitz 1977).
- We obtain the percentage increase in operating costs [11] by multiplying the net percentage payroll increase [13] by the labor share of operating costs [12].
- The net percentage payroll increase [13] includes savings from reduced turnover and the reduction in wage bill due to substitution effects and productivity gains. We estimate the total wage bill increase to be $144bn in the U.S. by 2024. We subtract the reduction in the total wage bill due to substitution effects and productivity gains [1]. We also account for the increase in payroll costs that corresponds to Medicare, Social Security, and Workers' Compensation costs. This share equals 10.36 percent in 2024.To compute the net percentage increase in payroll costs, we apply a partial offset for turnover cost savings.
- We obtain the reduction in consumer demand from the price increase [14] by multiplying the percentage reduction in consumer demand from price increase [15] by annual aggregate consumer spending [16]. The estimated reduction in consumer demand due to higher prices equals $67bn in the U.S. The key components of this calculation are:
  - The percentage reduction in consumer demand from price increase [14] depends on two parameters: (i) the percentage increase in consumer prices as calculated in line [10], and (ii) the price elasticity of consumer demand.
  - We obtain annual aggregate consumer spending [16])by multiplying projected annual GDP for the U.S. by our overall estimated share of household consumer spending in GDP. We estimate the U.S. GDP so that it is consistent with the underlying value of the GDP in IMPLAN in 2024. We estimate that annual aggregate household consumer spending is $13.79 trillion in 2024.
- We translate the annual reduction in consumer spending resulting from price increases into employment effects using the multipliers in the 2015 IMPLAN model. Day (2013) provides documentation of the IMPLAN model. We adjust those estimates by projected working age population growth from 2017 to 2024.

D Part D: Income effects

Part D of Table A1 estimates the income effects resulting from pay increases for low-wage workers, the resultant increase in consumer demand, and its impact on employment. Our estimates are calculated as follows:

We compute the net change in compensation for affected workers [19] as the total wage bill increase for affected workers [20] minus the wage bill reduction from a reduction in the Supplemental Nutrition Assistance Program (SNAP) and in premium tax credits under the Affordable Care Act benefit reduction [21].

- We stimate the offset from SNAP and premium tax credits [21] under the ACA to be 14.35 percent of the total wage increase (see Appendix A2). We apply this amount to the total wage bill increase for all households, as there is no easy way to disaggregate this effect by income brackets.
- We estimate the annual increase in jobs resulting from higher consumer demand using the 2015 IMPLAN model. We adjust those estimates by estimated working age population growth from 2014 to 2024 – 4. 2 percent for the overall period in the U.S. (see section A2.2 for the source). Our estimated effects on increased consumption are supported by two related empirical studies (Alonso 2016) and Cooper, Luengo-Prada and Parker (2017).

E Part E: Net effects

Part E of Table A1 presents our estimated cumulative net effect on employment [24]. We subtract the reductions in employment due to substitution effects, productivity gains [4] and scale effects [17] from the employment gains due to income effects [22]. We compute the annual estimates by dividing the cumulative effects on employment by seven, to account for the number of years over which the policy will phase in fully.

B Key model parameters

We summarize in Table A2 the values of our model's key parameters, for both the U.S. and Mississippi. We explain and document below the values of our parameters and the sources we used to obtain them.

Capital-labor substitution. It is often argued that a higher minimum wage will lead firms to reduce their use of workers. This reduction in labor demand can occur through two different channels: one involves substituting capital for labor, i.e., automation or mechanization of jobs while keeping sales at the same level; the other involves lower demand for workers when prices increase and sales fall. We discuss here the automation channel and consider the effect on sales in the following section.

Automation: economic theory and measurement. Mechanization does not necessarily lead to a net loss of jobs. As David Autor (2014a; 2014b) points out,

machines (including smart robots) do not just substitute for labor; they are also complements to existing jobs and they can lead to the creation of new jobs and industries. Indeed, previous rounds of automation and computerization have created more jobs than they destroyed. Moreover, automation does not involve only the replacement of labor by machines. It also involves the replacement of old machines (think manual cash registers) with newer ones (think electronic cash registers and electronic screens like iPads).

In general, the effect of automation on employment depends upon the elasticity of substitution of capital for labor – the change in the relative prices of capital and labor – and the share of profits in revenue. The lower this elasticity is, the more difficult it is to substitute capital for labor. Robert Chirinko, the leading economist specializing in estimates of capital-labor substitution, finds an economy-wide elasticity of about 0.4 (Chirinko and Mallick 2016). While the estimates in this study are identified across all economic sectors, most of the variation occurs among manufacturing industries. Lawrence (Lawrence 2015) also finds that the economy-wide sigma is less than 1 and that it is lower still in low-wage manufacturing industries than in high-wage manufacturing industries.

Alvarez-Cuadrado, Van Long and Poschke (2015) estimate substitution elasticities separately for manufacturing and services using data on 16 countries. They find that service sector elasticities are considerably lower than in manufacturing. However, their study does not examine low-wage services separately. The results in these papers nonetheless suggest, as Autor et al. conjectured, that automation possibilities are lower in low-service jobs. Aaronson and Phelan (2015) have carefully studied the short-run impact of minimum wages on the automation of different kinds of low-wage jobs. Their study is the first to examine automation within low-wage industry contexts. Aaronson and Phelan find that minimum wage increases do reduce routinized low-wage jobs (such as cashiers) and increase the number of less-routinized low-wage jobs (such as food preparation). As it turns out, the changes offset each other almost equally, resulting in no net change in employment. Thus, Aaronson and Phelan find that the capital-labor substitution elasticity is essentially zero in low-wage occupations.

We use an elasticity of 0.2 in our calculations, half-way between Chirinko and Mallick and Aaronson and Phelan. This conservative assumption may therefore result in an over-estimate of the magnitude of the automation effect. Aaronson and Phelan's findings also suggest very little substitution of highly skilled workers for lower skilled workers. Dube, Lester and Reich (2016) and Cengiz, Dube, Lindner and Zipperer (2019) obtain similar results. Consequently, we do not include any substitution of skilled labor for unskilled labor in our model.

Automation in practice. Machines that process automated transactions' at airports and in airplanes, banks, self-checkout stations in retail stores, parking garages, and gasoline stations' have become particularly widespread over the past 30 years. During this period, the price of computer-related machines has rapidly declined. Labor-saving automation will occur even when wages do not rise, insofar as the technological change continues to push down the price of equipment, making investments in new equipment and software profitable.

The effects of a rising minimum wage on actual automation depend in part

upon whether new labor-saving technology that has not yet been adopted continues to become available. We suggest that much of existing labor-saving technological change has already been embodied in low-wage industries, in equipment and software such as smart electronic cash registers, remote reservations, and ordering systems. An increase in the minimum wage is likely only to generate small increases in the adoption of more automated systems.

Equally important, the rate of adoption of technical change depends on changes in the relative prices of capital and labor, not just on the price of low-wage labor. Although the prices of computer-related equipment and software have fallen dramatically, by approximately a factor of ten in the past several decades, the decline in the past five years is much smaller. Meanwhile, median wages have stagnated and real minimum wages remain lower than they were in the early 1970s.

The declining cost of capital is also reflected in declines in long-term interest rates in recent decades. Five-year and ten-year inflation-protected interest rates have also fallen dramatically. These changes in relative prices have been the main impetus to increased automation. Even a doubling of the minimum wage policy, which would imply (according to (Allegretto et al. 2015) an average wage increase of about 22 percent, would have very little impact in comparison. However, interest rates are unlikely to fall further. It is therefore likely that actual automation in low-wage industries is slowing. To summarize, empirical estimates of the elasticity of substitution of capital for labor that include low-wage industries in their sample range between 0 and 0.4. We use 0.2, the midpoint of this range. Since Aaronson and Phelan find a much smaller elasticity, our use of 0.2 is conservative.

Profit share of revenues. We estimate that the profit share of revenues is 14 percent. To calculate this number, we use Annual Trade reports for wholesale trade and retail trade and BEA tables for all other sectors.

Productivity gains. For a discussion of productivity gains and the sources we used, see section 5.1 in the main report.

Labor share of operating costs. Net payroll cost increases for businesses are a function of three factors: (1) the total wage bill increase, after reduction due to substitution effects and productivity gains; (2) Medicare, Social Security, and Workers' Compensation increases, and (3) turnover costs savings. The payroll costs increase as total compensation increases and decrease with turnover costs savings.

- The total wage bill increase from 2016 to 2024 is estimated using Cooper (2017) wage simulation model based on micro data. For each year, Cooper (2017) calculate the reduction in wage bill due to job losses from substitution effects and productivity gains, assuming that capital-labor substitution and productivity gains are constant over the years. We assume in our calculations that capital-labor substitution is equal to 20 percent every year, and that productivity gains are equal to 5 percent every year.

- Employers' costs for Medicare, Social Security, and Workers' Compensation will equal 10.36 percent of wages from 2017 to 2024. We estimate the three components: Medicare (1.45 percent), Social Security (6.2 percent), and Workers' Compensation costs separately. Since we are estimating only the effects of a minimum wage increase, we assume the Medicare and Social Security rates will not change between 2017 and 2024. For Workers' Compensation costs, we draw from a report of the National Academy of Social Insurance (2013). Table 14 (p. 37) of this report indicates that Workers' Compensation employer costs in 2013 amounted to $1.50 per $100 of eligible wages. These costs increased $0.11 cent increase a year over 2011–2013, slightly more than the 2009–2011 change. To account for these cost increases, we adjust the 2013 cost by $0.34. Consequently, we estimate that Workers' Compensation costs will equal 1.84 percent of wages in the U.S. and in MS from 2017 to 2024.

- Turnover costs savings are based on the estimates of Pollin and Wicks-Lim (2015), Fairris (2005), Dube, Freeman and Reich (2010), Dube, Lester and Reich (2016), Boushey and Glynn (2012), and Jacobs and Graham-Squire (2010). See section 5.1 in the main report.

Labor share of operating costs by industry. For retail trade and wholesale trade, we estimate labor costs as the sum of the annual wage costs, payroll taxes and employer paid insurance premiums (except health insurance), and other benefits (other than contributions to pension plans). The labor share is estimated using 2012 Census Bureau surveys – the most recent year available. We document here our sources and methods for these two individual industries:

- Retail trade (including grocery stores): The 2012 U.S. Census Annual Retail Trade Reports provides data on retail sales, payroll costs, merchandise purchased for resale, and detailed operating expenses. We add operating expenses and purchases together to determine total operating costs. We add the costs of payroll taxes, employer paid insurance premiums, and employer benefits (excluding health insurance and retirement benefits) to annual payroll to estimate total labor costs. Health and retirement benefits are excluded since, unlike payroll taxes and Workers' Compensation insurance, the costs of the benefits will not change if wages are increased. Dividing labor costs by operating costs gives U.S. the labor share in retail trade.

- Wholesale trade: Data are from the U.S. Census. Annual Wholesale Trade Report. We follow the same methods as with retail trade.

For all other sectors, we use the 2015 BEA accounts to compute the labor share of operating costs. The numerator is composed of total compensation. The denominator is composed of the gross output (sum of value added and intermediate inputs) net of taxes on production and imports less subsidies. The overall labor share of the economy is computed as a weighted average of the labor shares obtained for each sectors. The share of each sector in gross output is used

as the weight variable. We estimate that overall, the labor share of operating costs for the U.S. is 29.1 percent in 2015. We use the same assumption for Mississippi. By 2024, we estimate that the overall labor share will increase to 30.3 percent in scenario 1 for the U.S. (i.e. with a capital-labor substitution elasticity of 0.2 and productivity gains at 0.05 percent) and 32.7 percent in scenario 1 for Mississippi.

Share of payroll costs for Medicare, Social Security and Workers' compensation. We estimate the Medicare, Social Security, and Workers' Compensation costs separately. Employers are liable for 6.2 percent Social Security taxes and 1.45 percent Medicare taxes. We estimate that the Workers' Compensation employer cost is 2.71 percent of wages in the U.S. as a whole and in Mississippi. The share of Medicare, Social Security, and Workers' Compensation is 10.36 percent from 2017. We assume that this same share applies throughout the phase-in period.

Turnover reduction. For a discussion of savings generated by turnover reduction and the sources we used, see section 5.1 in the main report.

Price elasticity of demand. The price elasticity of demand measures the effect of a price increase on reducing consumer demand. We use a price elasticity of 0.72 for the U.S. and 0.92 for MS. The 0.72 estimate is based on Taylor and Houthakker (2010), who report price elasticities for six categories of goods and services. We adjust their estimates to account for changes in the elasticity of health care spending attributable to the Affordable Care Act and other changes in the health care system.

GDP in 2024 for the U.S. and in Mississippi. We forecast 2024 GDP using the following method:

- We start with the 2015 GDP reported in IMPLAN, i.e. $23,454 billion in the U.S. and $140 billion in Mississippi.
- We then forecast the GDP for the U.S. by applying the employment growth forecast of 4.2 percent from 2015 to 2024 (2.51 percent for Mississippi), projected wage growth using CBO projections of the Employment Cost index of 3.0 percent, and the GDP deflator in IMPLAN for 2024 (1.058 for both the U.S. and Mississippi).

Share of consumer spending in GDP. Our estimate of the share of consumer spending in GDP includes only consumer spending that flows through households. We therefore reduce the BEA's estimate of the consumption share by 14.1 percent.

Offsets from benefit reductions and payroll tax increases. We estimate that the total offset from reduced EITC payments to be 0.2 percent, the offset from reduced SNAP benefits to be 4.20 percent, the offset from lower premium tax credits under the ACA to be 2.3 percent, and the offset from reduced payroll taxes to be 7.65 percent (the remaining personal income taxes are automatically

subtracted by IMPLAN). We used Congressional Budget Office (2012) to calculate these offsets. These offsets refer to 2012; we assume they will remain constant until 2024.

Table A2 – Key parameters of the model for the U.S. and Mississippi

	U.S. 2017	U.S. 2021	U.S. 2024	Mississippi
A. Workers affected and wage increases				
Working age population growth from 2014 to 2024	0.01	0.02	0.04	0.03
Impact of K-L substitution and productivity gains on number of jobs and wage bill				
Capital-labor substitution elasticity	0.20	0.25	0.29	0.29
Capital share in gross output (excluding depreciation)	0.14	0.14	0.14	0.14
Productivity gains - in levels	0.005	0.005	0.005	0.005
C. Scale effects: increase in consumer prices and reduction in consumer demand				
Labor share of gross output	0.29	0.30	0.30	0.33
Materials share of gross output in the restaurant industry	0.51	0.51	0.51	0.51
Materials share of gross output in retail trade	0.78	0.78	0.78	0.78
Percent of wage costs for Medicare, Social Security, and worker compensation (employee side)	0.10	0.10	0.10	0.10
Turnover reduction (as share of payroll increase)	0.18	0.13	0.007	0.007
Price elasticity of demand	-0.72	-0.72	-0.72	-0.72
Projected annual GDP in 2024 (in billions)	$19,286	$22,248	$24,918	$149
Share of consumer spending in GDP	0.588	0.588	0.588	0.588
D. Income effects: effects of pay increases on consumer spending and employment				
Percentage offset from reduced SNAP benefits and lower premium tax credits	0.14	0.14	0.14	0.14
Offset from reduced EITC	0.20	0.20	0.20	0.20
Offset from reduced SNAP benefits	4.20	4.20	4.20	4.20
Offset from lower premium tax credits under the ACA	2.30	2.30	2.30	2.30
Offset from reduced payroll taxes	7.65	7.65	7.65	7.65
E. Net effects				
No key parameters used in this section.				

C Demographic and job characteristics of affected workers

Table A3 – Demographic and job characteristics of affected workers in the U.S.

	Percent of all workers	Percent of all workers getting a raise	Percent of group getting a raise
Gender			
Male	52.0	44.4	24.9
Female	48.0	55.6	33.8
Age			
Over 20	96.4	90.2	27.3
Under 20	3.6	9.8	79.4
16 to 24	13.8	29.9	63.2
25 to 39	34.5	32.3	27.4
40 to 54	31.3	21.8	20.3
55 to 64	20.5	16.1	22.9
Race/Ethnicity			
White (Non-Latino)	58.7	53.5	26.5
Black (Non-Latino)	12.2	16.7	40.1
Latino-a	19.7	22.7	33.5
Asian (Non-Latino)	7.1	4.2	17.2
Other	2.3	3.0	38.3
Education			
Less than High-School	9.2	17.7	56.2
High-School or G.E.D	26.4	35.8	39.5
Some College	18.5	23.9	37.7
Associate's degree	10.5	9.9	27.6
Bachelor's degree or higher	35.4	12.7	10.4
Family structure			
Married parents with kids	25.9	17.2	19.4
Single parents with kids	7.7	10.8	40.8
Married parents with no kids	27.0	19.7	21.3
Single parents with no kids	39.3	52.3	38.8
Full-time/part-time worker			
Part-time (fewer than 20 hours)	5.3	11.0	60.1
Part-time (20-34 hours)	13.6	26.0	55.9
Full-time (35 hours per week or more)	81.1	63.0	22.6

Source: Authors' analysis based on Cooper (2017) analysis of CPS data.

Table A4 – Demographic and job characteristics of affected workers in Mississippi

	Percent of all workers	Percent of all workers getting a raise	Percent of group getting a raise
Gender			
Male	49.5	43.0	38.5
Female	50.5	57.0	50.1
Age			
Over 20	97.2	94.2	43.0
Under 20	2.8	5.8	91.2
16 to 24	13.1	24.0	80.9
25 to 39	34.1	36.4	47.3
40 to 54	33.9	25.9	33.9
55 to 64	18.8	13.7	32.4
Race/Ethnicity			
White (Non-Latino)	54.4	42.9	35.0
Black (Non-Latino)	39.4	49.1	55.3
Latino-a	3.6	4.6	57.8
Asian (Non-Latino)	1.5	2.0	58.0
Other	1.1	1.3	53.5
Education			
Less than High-School	10.2	17.0	73.7
High-School or G.E.D	30.5	38.1	55.3
Some College	19.9	23.4	52.3
Associate's degree	14.0	11.0	34.9
Bachelor's degree or higher	25.4	10.5	18.4
Family structure			
Married parents with kids	26.3	19.4	32.7
Single parents with kids	10.7	14.6	60.7
Married parents with no kids	26.1	17.9	30.5
Single parents with no kids	36.9	48.1	57.8
Full-time/part-time worker			
Part-time (fewer than 20 hours)	3.7	6.1	72.6
Part-time (20-34 hours)	11.9	21.0	78.2
Full-time (35 hours per week or more)	84.4	73.0	38.4

Source: Authors' analysis of American Community Survey data.

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Titre : Essais sur les effets redistributifs du salaire minimum

Mots clés : Salaire minimum, inégalités entre Blancs et Noir-Américains, répercussion des coûts du travail dans les prix à la consommation

Résumé : Cette thèse analyse les effets redistributifs du salaire minimum. Le premier chapitre montre que l'introduction du salaire minimum en 1967 dans un certain nombre de secteurs de l'économie qui en étaient exclus jusqu'alors peut expliquer plus de 20% de la réduction des inégalités entre Blancs et Noir-Américains dans les années 1960 et le début des années 1970 aux États-Unis – la seule période (depuis la seconde guerre mondiale) au cours de laquelle les inégalités raciales sur le marché du travail ont diminué. Cette réforme a eu un rôle aussi déterminant dans l'évolution des inégalités raciales que l'augmentation du nombre d'années d'études pour les Noir-Américains ou les lois contre la discrimination. Le deuxième chapitre de cette thèse est consacré à l'estimation de la transmission des augmentations de salaire minimum dans les prix des produits vendus dans les supermarchés américains. Une augmentation moyenne de 10% du salaire minimum se traduit par une augmentation de 0.2% dans les prix des supermarchés entre 2001 et 2012. Cette elasticité-prix est cohérente avec une tranmission de l'intégralité de l'augmentation des coûts du travail dans les prix de vente aux consommateurs. L'augmentation des prix des supermarchés réduit les gains de revenu nominaux liés à l'augmentation du salaire minimum entre de 3 à 12%, selon le niveau de revenu du ménage. Le troisième chapitre calibre un modèle du marché du travail qui permet de simuler les effets d'une augmentation du salaire minimum au niveau fédéral à $15 d'ici 2024 aux États-Unis. Il s'agit de comparer les niveaux d'emploi obtenus si la réforme est adoptée aux niveaux d'emploi obtenu si la réforme n'est pas adoptée, et ce, selon les valeurs d'une série d'élasticités bien identifiées.

Title : Essays on the redistributive effects of the minimum wage

Keywords : minimum wage, racial inequality, price pass-through

Abstract : This dissertation studies the redistributive effects of minimum wage policies. The first chapter provides the first causal evidence of how the minimum wage has affected the historical evolution of racial inequality in the United States. It shows that the extension of the federal minimum wage to new sectors of the economy in 1967 can explain more than 20% of the decline in the racial earnings gap observed during the Civil Rights Era -- the only period of time (post World-War II) during which racial inequality fell in the United States. This effect is as large as previously studied policies and economic factors, such as the improvement in schooling for African-Americans or federal anti-discrimination policies. The second chapter estimates the pass-through of minimum wage increases into prices of US grocery stores, using high-frequency scanner level data. A 10% minimum wage hike translates into a 0.2% increase in grocery prices between 2001 and 2012. This magnitude is consistent with a full pass-through of cost increases into consumer prices. Depending on household income, grocery price increases offset between 3 and 12% of the nominal income gains. The third chapter estimates a calibrated labor market model to analyze the likely effects of a $15 federal minimum wage by 2024. It compares employment numbers if the policy were adopted to employment numbers if the policy had not been adopted using a wide range of well-identified elasticities.

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