The Economic Impact of a High National Minimum Wage: Evidence from the 1966 Fair Labor Standards Act

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This paper examines the short- and longer-term economic effects of the 1966 Fair Labor Standards Act (FLSA), which increased the national minimum wage to its highest level of the twentieth century and extended coverage to an additional 9.1 million workers. Exploiting differences in the "bite" of the minimum wage owing to regional variation in the standard of living and industry composition, this paper finds that the 1966 FLSA increased wages dramatically but reduced aggregate employment only modestly. However, some evidence shows that disemployment effects were significantly larger among African American men, 40% of whom earned below the new minimum wage.

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I. Introduction

The 1966 Amendments to the Fair Labor Standards Act (1966 FLSA) capped almost 15 years of real minimum wage increases in the United States, leading to the highest national minimum wage of the twentieth century. In addition to raising the nominal hourly minimum by 28% to $11.83 (in 2019 dollars) for covered workers, the 1966 FLSA expanded coverage to 9.1 million workers in the economy's lowest-earning industries (Martin 1967).[1] Changes in coverage increased the share of private sector workers under the FLSA by 14 percentage points to 77% and the share of government employees under the FLSA from 0% to 40% (Brown 1999).

This moment in history presents a unique opportunity to study the short and lagged economic effects of a very high national minimum wage with effects that persisted for newly covered sectors. Under both competitive and monopsonistic labor market models, the sustained increase in wages could generate larger employment responses than more recent minimum wage changes, which were rapidly eroded by inflation (Boal and Ransom 1997; Brown 1999).[2] Understanding the employment responses to the 1966 FLSA is important for evaluating the economic theory of labor markets and as a point of reference for contemporary proposals to raise federal, state, and local minimum wages to similar levels (Cooper, Schmitt, and Mishel 2015).

This paper quantifies the wage and employment responses to the 1966 FLSA by comparing states that were more affected to those that were less affected. Adapting Card (1992), our research design relies on the idea that the 1966 FLSA had a larger "bite" in states where wages and coverage were lower in 1966, thus allowing a dose-response analysis. To capture the impact of the 1966 FLSA on previously covered workers as well as on newly covered workers, we exploit differences across states in the share of workers below the new minimum wage of $1.60.[3] Although nationally representative surveys of workers in our period do not ask about hourly wages, the 1960 US Census of the Population (Ruggles et al. 2015a) and 1962–74 March Current Population Surveys (CPS; Ruggles et al. 2015b) show that the share of workers with implied hourly wages below the new minimum wage is highly and robustly correlated with state-level wage increases after 1966. This relationship allows us to examine locations where more workers were affected

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[1] This calculation uses the minimum wage of $1.60 in February 1968 and adjusts to February 2019 dollars per https://data.bls.gov/cgi-bin/cpicalc.pl.

[2] Alan Krueger (2015) makes this point in a recent op-ed cautioning policy makers about proposed increases in the minimum wage to $15 per hour.

[3] To calculate the implied hourly wage, we divide annual wage earnings by weeks worked in the previous year and hours worked in the reference week.

by the 1966 FLSA where we expect the legislation's effects on wages and employment to be largest. Similar to Cengiz et al. (2019), a key benefit of our approach is that we can examine the effect of a minimum wage increase on all workers.

Our analysis begins with a quantification of the 1966 FLSA on wages. A dynamic, event-study framework estimates the wage and employment effects in the years before the amendments took effect (leads provide a placebo test) as well as in the first 7 years after implementation (lags characterize the postlegislation responses). The internal validity of the research design is bolstered by the fact that wages in states with greater shares of workers earning wages below \$1.60 follow trends similar to those in less affected states from 1959 to 1966. However, the March CPS shows that men's hourly wages increased significantly more in more affected states after 1966. Our estimates imply that states such as Texas, where 26% of workers earned less than \$1.60 per hour in 1966, experienced a 6% larger increase in average wages relative to New York, where 11% of workers earned less than \$1.60 per hour. These results are robust to the inclusion of individual covariates for age, race, marital status, and metropolitan residence to account for changing composition of birth cohorts. This relationship holds within states as well. Hourly wages in lower-earning industries (that would have been disproportionately affected by the 1966 amendments) increased by significantly more after 1966, even after including state-by-year fixed effects to account for differential, exogenous changes at the state level in the demand for or supply of workers. Across the United States, our estimates suggest that average wages increased by 6.5% because of the 1966 FLSA, with around one-fifth of the increase due to a higher minimum wage for previously covered workers and the remainder due to coverage increases and spillovers to higher-earning workers.

In terms of hiring and hours, the March CPS shows that employment during the year fell by a modest 0.7% more in lower-earning states and annual hours worked by 0.4% more, as the 1966 FLSA increased wages significantly more in these areas. The implied demand elasticities are −0.14 for employment (a one-sided test rejects zero at the 5% level; 95% confidence interval [CI]: −0.29 to 0.02) and −0.07 for annual hours worked, which cannot be distinguished from zero. Interestingly, employment in the reference week fell little in response to the 1966 FLSA, suggesting that the legislation's impact on employment was concentrated among workers with less attachment to the labor force (i.e., workers less likely to be employed for the full year).

An important alternative explanation for these findings is that areas more affected by the 1966 FLSA experienced exogenously slower growth in the demand for labor after 1966, which would lead our research strategy to overstate the negative employment response. To account for this possibility, we include time-varying, state-level controls for gross state product. Contrary to this hypothesis, areas with lower wages (which were more affected by the 1966 FLSA) were growing more quickly. Accounting for this faster growth results in slightly larger demand elasticities: −0.18 for employment during the year (95% CI: −0.36 to −0.05), −0.28 for annual hours worked (a one-sided test rejects zero at the 5% level; 95% CI: −0.59 to 0.03), and a larger but statistically insignificant −0.10 for employment in the reference week.

A final analysis disaggregates these estimates by subgroups to examine the incidence of the legislation. For teens, we estimate larger but imprecise elasticities of employment with respect to wages. Among the 46% of men with less than a high school education, the long-run employment elasticity is −0.14 (95% CI: −0.34 to 0.06). The evidence is more decisive for African American men. Their employment during the year decreased by 3.4% and annual hours worked fell by 5% after the 1966 FLSA was implemented when moving across the interquartile range. The estimated disemployment effects for black men vary somewhat across outcomes, as employment in the reference week decreased by 1% for the same comparison. Changes in employment for white men were considerably smaller and statistically insignificant. The resulting demand elasticities for black men are statistically significant and range from −0.14 for employment at any point during the year (95% CI: −0.35 to –0.14) and −0.42 for annual hours worked (95% CI: −0.72 to −0.12). In summary, even if aggregate employment responded little to the 1966 FLSA, the legislation engendered compositional changes in employment and impacted some of the more disadvantaged workers in the economy.

II. The History and Expected Effects of the 1966 Amendments to the FLSA

At the time of their enactment, the 1966 amendments (P.L. 89-601) were regarded as the most wide-ranging changes to the FLSA since 1938 (Levin-Waldman 2001, 112). The purpose of the legislation closely related to President Lyndon Johnson's war on poverty agenda. Proponents of this legislation stressed how increases in the coverage and level of the minimum wage would alleviate poverty and help struggling low-wage workers. The president of the American Federation of Labor and Congress of Industrial Organizations noted in June 1965 that "the minimum wage law amendments now pending before Congress are 'anti-poverty' legislation, designed to improve the lot of the 'working poor.'" Opponents of the legislation, such as the National Association of Manufacturing, countered that the proposed "minimum [would] . . . be increased to a point where it would cause difficulty to those employing unskilled and inexperienced" (Levin-Waldman 2001, 113). Ultimately, the proponents won the day. The 1966 amendments were passed on September 23, with their first provisions effective in February 1967.[4] This national minimum wage was binding, with its level exceeding the state minimum in all but a handful of cases (Quester 1981; Sutch 2010).[5]

The impact of the 1966 amendments was expected to be large enough that they were challenged as unconstitutional. In *Maryland v. Wirtz*, 392 U.S. 183 (1968), the state of Maryland (later joined by 27 other states and a school district) argued that the Supreme Court should enjoin the act on the basis that its provisions exceeded Congress's authority to regulate under the commerce clause; in particular, the states objected to requirements that they meet federal pay and overtime standards in their schools and hospitals. The 1966 amendments survived this challenge. On June 10, 1968, the Warren Court affirmed the 1966 amendments and instructed states to enforce them.

A. Increases in the Statutory Minimum Wage for Previously Covered Workers

The 1966 amendments raised the real minimum wage for covered workers to its highest level in the twentieth century, as shown in figure 1*A*. To minimize the burden on firms, they were phased in over 2 years (Martin 1967). On February 1, 1967, the statutory minimum wage for covered workers increased from \$1.25 to \$1.40 (\$9.60 and \$10.76 in 2019 dollars). In its report to Congress, the Department of Labor estimated that 3.72 million covered workers would benefit from this increase (Martin 1967). The second minimum wage hike occurred the following year on February 1, 1968, and increased the statutory minimum wage for covered workers to \$1.60 (\$11.83 in 2019 dollars). This amounted to a 28% nominal increase over 2 years, or a 23% increase in real terms. The effective wage increase for many of the lowest-earning, previously uncovered workers was significantly larger (Kocin 1967), as we discuss below.[6]

[4] When signing the amendments, President Johnson said, "The new minimum wage—\$64 per week—will not support a very big family but it will bring workers and their families a little bit above the poverty line." He followed up by stressing his commitment to the war on poverty's other human capital programs: "My ambition is that no man should have to work for a minimum wage, but that every man should have the skills he can sell for more."

[5] Quester (1981) shows that in 1966 only Alaska, California, New York, and Massachusetts had a higher minimum wage for some purposes and groups than the federal minimum. In all states but Alaska, the state minimum was only \$0.05 above the national minimum in 1966. Moreover, men were not covered by the state minimum wage in California. Although Sutch (2010) disagrees with Quester (1981) in a handful of cases, both scholars agree that state minimum wage legislation was less binding than the federal minimum.

[6] As with earlier amendments, there were a number of exceptions. See Anderson (1967) for details.

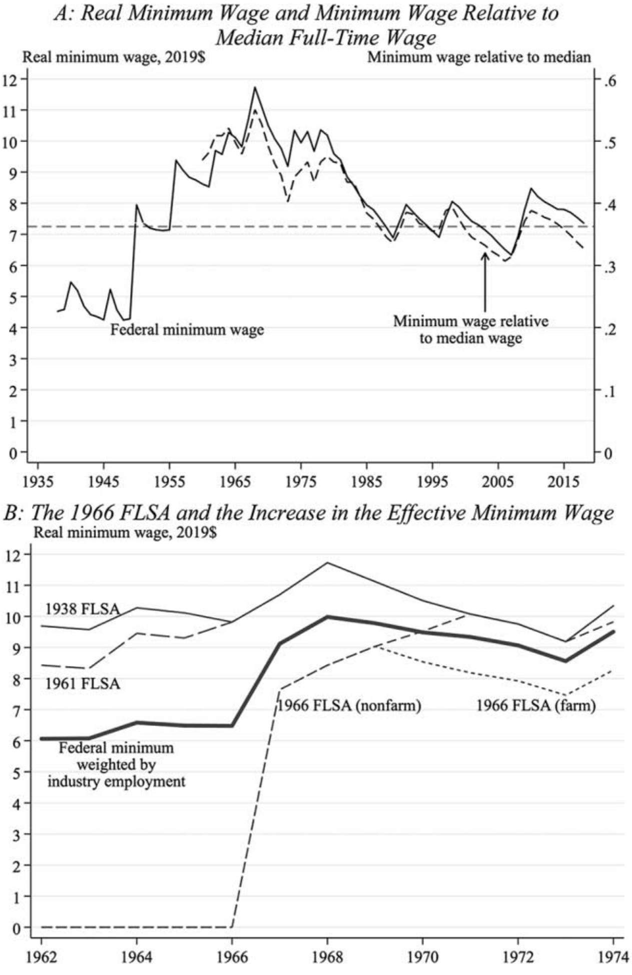


FIG. 1.—Real minimum wages in the United States, 1938–2018. Nominal minimum wages are inflated to 2019 dollars using the consumer price index for all urban consumers (US city average for all items; CUUR0000SA0, https://data.bls.gov/timeseries/CUUR0000SA0). In *A*, the solid line displays the statutory federal minimum wage in effect for the majority of the year, and the nonhorizontal dashed line shows the minimum wage relative to the median full-time wage from the OECD (in 2019). The boldface line in *B* is constructed as the weighted average of the real minimum wage levels and the share of workers employed in industries first covered by each Fair Labor Standards Act (FLSA) amendment (see Derenoncourt and Montialoux 2021; table A1). A color version of this figure is available online.

B. Increases in Coverage and Statutory Minimum Wages for Previously Uncovered Workers

A major feature of the 1966 FLSA was its dramatic expansion of coverage. Figure 1*B* shows the federal statutory minimum wage in 2019 dollars for workers covered under the 1938 FLSA and workers added in the 1961 FLSA. In April of 1967, the *Monthly Labor Review* estimated that the 1966 amendments had expanded the FLSA's coverage to an additional 9.1 million workers, up from 32.3 million workers covered under previous legislation (Martin 1967). This happened through the 1966 FLSA's narrowing of exemptions as well as its expansion of industries covered under the "enterprise volume test." Figure 1*B* shows the changes in the statutory minimum wage for workers newly covered under the 1966 FLSA. (Note that the pre-1967 wages for newly covered workers were not zero—we use zero to represent the absence of the federal statutory minimum wage.)

The increase in coverage occurred through a direct expansion of the legislation to include employees on large farms, federal service contracts, federal wage board employees, and certain Armed Forces employees (e.g., post exchanges). It also narrowed or repealed exemptions for employees of hotels, restaurants, laundries and dry cleaners, hospitals, nursing homes, schools, automobile and farm implement dealers, small loggers, local transit and taxi companies, agricultural processing, and food services. The 1966 FLSA also included an indirect expansion of coverage through its reduction in the enterprise volume test from $1 million (in the 1961 amendments) to $250,000 within 3 years.[7] This meant that employees of smaller firms engaged in "interstate commerce" gained coverage by February 1, 1969.[8] As a consequence of both these changes, 95% of newly covered workers were employed in five industries (Martin 1967). Just over three million (3.1) of the newly covered workers were in services,[9] 2.4 million were in government,[10] 2.2 million were in retail trade, 0.6 million were in construction, and 0.5 million were in agriculture.

[7] In agriculture, the law used man-days of labor instead of sales volume in determining coverage. The 1966 FLSA extended coverage to employees of farms using more than 500 man-days of labor in any quarter.

[8] The reduction in the enterprise volume test extended the provisions of the 1961 amendments, which expanded the coverage of the FLSA to all employees within an enterprise engaged in interstate commerce so long as the enterprise had $1 million in gross annual volume. The earlier 1961 amendments had thus extended coverage to employees in retail or service, local transit, construction, and gasoline service stations.

[9] Employees of laundries, schools, hospitals, nursing homes, and large hotels represented more than half of all coverage in the services category (Martin 1967, 21).

[10] Approximately one million workers were employed in public schools, 610,000 were in state and local government hospitals, and 70,000 were in local government transit systems. The remainder of public workers consisted of 606,000 federal wage board workers and 110,000 employees of post exchanges and other nonappropriated fund establishments (Martin 1967, 21).

The 1966 FLSA specified different wage increases for newly covered workers. Newly covered nonfarm workers began at a minimum wage of $1.00 per hour in 1967, with increases of $0.15 per year to reach $1.60 by 1971.[11] Newly covered farm workers began at a minimum wage of $1.00 in 1967 and increased by $0.15 per year to reach $1.30 in 1969, which is why the series in figure 1*B* diverges for farm and nonfarm workers after 1969.

The 1966 FLSA also applied overtime provisions to newly covered workers. As of February 1, 1967, newly covered workers working more than 44 hours per week were paid time and a half. In 1968, this maximum fell to 42 hours per week, and in 1969 it fell to 40 hours per week.[12]

Documenting the impact of the 1966 amendments on the wages of previously uncovered workers is difficult because (as we show in the appendix) measurement error in the March CPS hourly wage is particularly acute near the minimum wage. To place our subsequent estimates in context, we entered Bureau of Labor Statistics (BLS) tabulations of industry surveys both before and after the 1966 amendments took effect. Because these studies did not rely on nationally representative samples (they are localized to certain cities, regions, and industries) and because noncompliance may be underreported to the federal government, extrapolating from these findings to the state and national impact of the 1966 FLSA is difficult. Nevertheless, these reports cover changes in the wages of approximately two-thirds of the newly covered workers, including about half of the service industry (employees of laundries, schools, nursing homes, and hospitals), about two-thirds of the newly covered government workers (employees in public schools and government hospitals), all workers in retail trade, and all workers in agriculture.

For laundries in 1966, 72.5% of all US employees and 89.3% of employees in the South earned less than $1.60 per hour. By 1968, those figures had fallen to 48.7% and 73.6%, respectively. Between 1966 and 1968, the average industry wage increased by 16% in the United States and by 23% in the South. Similarly, average weekly hours fell from 38.7 to 36 as compliance with new overtime provisions increased.

Because nursing homes, hospitals, and public schools received public funding, such as Medicaid, Medicare, and Title I funds from the Elementary and Secondary Education Act, we expect even greater compliance in these industries (Almond, Chay, and Greenstone 2003; Cascio et al. 2010). Data on hospitals closely accord with this hypothesis. For instance, 43.4% of nonsupervisory employees in nongovernmental hospitals earned less than $1.60 per hour in July 1966, and average hourly earnings were $1.83. By March 1969, the share of workers earning below $1.60 per hour had fallen to

[11] The Department of Labor estimated that the initial increase to $1.00 would apply to around 953,000 farm workers.

[12] See estimates in the appendix (available online) suggesting that these changes had at most short-lived effects on overtime.

11.2% and average hourly earnings increased by 35% to $2.47. Average weekly hours fell from 36 to 34.7, as the share of employees working over 40 hours fell from 15.7% to 10.9%.

C. Expected Effects of the 1966 FLSA on Wages and Employment

The literature on the minimum wage is so vast that "we are almost at the point where there are meta-studies of meta-studies" (Manning 2016).[13] One area of consensus is that increases in the wage floor should raise wages. However, quantifying the magnitude of the effects of the 1966 amendments on wages in the US economy is difficult, owing to a lack of information on the number of directly affected, previously covered individuals as well as the number of newly covered individuals. Our analysis uses the 1960 US Census of the Population and the March CPS—nationally representative data sets of workers for our period of interest—to estimate the national impact of the 1966 amendments on wages as well as the lag structure of these adjustments.

The magnitude and speed of the wage responses also inform expectations about the 1966 amendments' effects on employment, which are theoretically ambiguous. In the classic (perfectly competitive) labor market case, the aggregate labor demand and labor supply curves pin down wages and employment at the competitive equilibrium. In the monopsonistic case, the marginal cost of hiring additional workers lies above the aggregate labor supply curve. The intersection of the marginal cost curve and demand curve pin down the labor market equilibrium, where both employment and wages lie below the perfectly competitive equilibrium. A key result in standard monopsonistic models is that the imposition of a wage floor up to the perfectly competitive level could raise employment to the perfectly competitive level. In both models, however, raising wages above the wage set in a perfectly competitive labor market would lower employment. In standard two-sector models of the labor market, increasing the coverage rate (or the probability of finding a covered sector job) should exacerbate the effects of raising the minimum wage (Brown 1999). Finally, monopsonistic firms may also engage in wage discrimination. Assuming that they have some information about the labor supply elasticities of different groups, firms could pay workers with lower labor supply elasticities (potentially because of fewer outside options or lower incomes) lower wages (Boal and Ransom 1997).[14]

It is doubtful that the labor market is a pure form of perfect competition or monopsony, so these predictions benchmark extremes with the actual labor market lying somewhere in-between. The important theoretical prediction is that both competitive and monopsonistic labor market models suggest that

[13] Many recent papers have been summarized in multiple reviews (Neumark and Wascher 2007; Schmitt 2013; Belman and Wolfson 2014) and meta-studies, updating Brown, Gilroy, and Kohen (1982) and Brown (1999).

[14] This result assumes that all workers are equally productive.

a high enough minimum wage should reduce employment. There is less agreement, however, on the point at which this high level of wages would be reached. The 1966 FLSA presents a unique opportunity to study the short and lagged economic effects of the highest national minimum wage of the twentieth century—a level similar to recent policy proposals. In addition, the 1966 FLSA represents a permanent increase in the minimum wage for a large number of newly covered workers. Our analysis considers both the magnitude of disemployment effects and whether these effects varied by group of worker.

III. Evaluating the Economic Effects of a National Minimum Wage

Our research design follows the spirit of Card (1992), who makes use of the long-standing criticism of the national minimum wage—namely, that geographic variation in the cost of living makes the impact of a national minimum wage larger in some areas (Stigler 1946). For instance, the same nominal minimum wage in New York would be effectively much higher in Texas after accounting for the cost of living. This geographic variation in cost of living means that imposing a high, uniform, and national minimum wage should have differential real impacts on local economies, allowing a dose-response-style analysis.

Card (1992) exploits this fact in a simple two-period model to study the 1990 national minimum wage increase. Focusing on teens, a group largely earning the minimum wage, Card uses variation in the fraction of workers affected by the change in the national minimum wage, F_s^*, as an instrumental variable in the following two-equation model:

$$\Delta \log W_s = \gamma_1 + \gamma_2 F_s^* + \boldsymbol{X}_s' \boldsymbol{\gamma}_3 + \varepsilon_s, \tag{1}$$

$$\Delta E_s = \beta_1 + \beta_2 \Delta W_s + \boldsymbol{X}_s' \boldsymbol{\beta}_3 + \omega_s. \tag{2}$$

The dependent variables, $\Delta \log W_s$ and ΔE_s, capture the change in mean log wages or employment rates (employment-to-population ratio) among teens in state s during a period before and after the minimum wage increase. In some specifications, $\boldsymbol{X}_s$ represents the employment-to-population ratio among all workers or the overall unemployment rate. The variable F_s^* represents the number of workers earning above the old minimum wage and below the new minimum wage, divided by the number of workers in the state. Thus, F_s^* captures the bite of the minimum wage as the fraction of workers in a state who would be affected by the 1990 national minimum wage increase. Card finds evidence that an increase in the federal minimum wage generates greater wage gains in states with a greater fraction of workers affected, showing that $\gamma_2 = 0.15$. Card then tests whether employment falls more in places where the fraction of workers affected by the minimum wage was higher, or $\beta_2 < 0$. As he notes, β_2 is proportional to the labor demand elasticity in this simple model.

Our analysis uses a nationally representative sample of prime-aged (16–64) male workers from the 1960 US Census of the Population and annual 1962–74 March CPS. This broad age range is important for capturing the national effects of the legislation, as employers may have substituted hiring across age or skill groups in response to the 1966 FLSA. We exclude women because they were impacted by the 1963 Equal Pay Act, which also amended the FLSA (Bailey, Helgerman, and Stuart 2021).[15] We also exclude self-employed workers, who are not covered under the FLSA.[16] To increase consistency between the CPS and the census, we also restrict the census sample to individuals not living in institutional group quarters. Finally, we convert income and wages into 2019 dollars using the consumer price index for all urban consumers and index wages and employment to the relevant year (annual earnings and weeks worked refer to the year before the survey, while employment in the reference week does not). See the appendix for more details.

The CPS allows us to extend Card's (1992) methodology in several ways. First, we estimate a dynamic version of his 2-period model to examine how wages and employment changed from 1959 to 1973 in response to the 1966 FLSA.[17] Second, we use the share of workers earning less than $1.60 in 1966 (rather than the share of workers earning between the old and the new minimum) as a measure of the state's labor market that is potentially affected. This measure ameliorates concerns regarding measurement error in implied hourly wages in the March CPS. In periods where we observe both reports of hourly wage earnings (monthly outgoing rotation group [ORG]) and annual wage earnings (March CPS) for the same year, the distribution of implied hourly wages is very similar for workers earning just above the minimum wage (see the appendix), whereas the March CPS measure of implied hourly wages severely misstates the share of workers earning between the old and the new minimum wage. In addition, our cumulative measure captures the 1966 FLSA's increase in coverage that impacted wage earners below the

[15] For the interested reader, the appendix contains estimates that include women. When including women in the sample, the estimated increase in wages is similar and there is no evidence of a reduction in employment, indicating that our conclusions about the overall impact of the 1966 FLSA are not driven by the focus on men.

[16] When examining wages, employment during the year, and annual hours worked, we follow Lemieux (2006) and focus on likely covered workers by restricting the sample to civilians for whom the ratio of self-employment plus farm income to labor income does not exceed 10% in absolute value. When examining employment in the reference week, we exclude individuals who report being self-employed that week. Our results are robust to including self-employed workers, as we report in the appendix.

[17] We examine labor market outcomes up to 1973 because the federal minimum wage increased again on May 1, 1974.

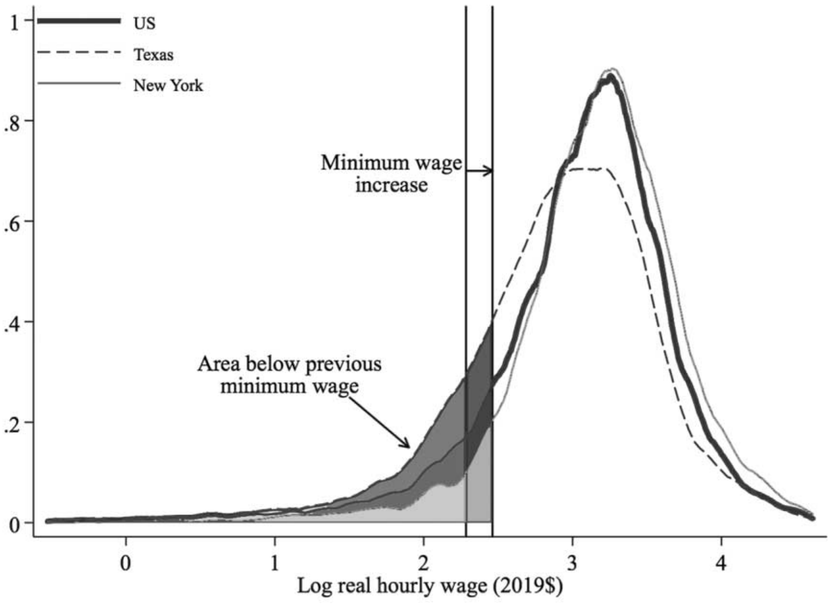


FIG. 2.—1966 average hourly wage distribution. This figure displays log real March wage densities (in 2019 dollars) for men aged 16–64. Densities are estimated only among wages between the 0.5 and 99.5 percentiles of the aggregate wage distribution. Densities are weighted by the product of the Current Population Survey (CPS) weight and the annual number of hours. Texas and New York are at the 25th and 75th percentiles, respectively, of the share of wages below \$1.60 in 1966. Vertical lines correspond to the federal minimum wage before and after the 1966 Fair Labor Standards Act. Source: 1967 March CPS. A color version of this figure is available online.

old minimum wage of \$1.25 per hour.[18] While the available data do not allow us to precisely measure workers exposed to the coverage expansion, the share of wages below \$1.60 captures this better than using the share of wages between \$1.25 and \$1.60.[19]

Figure 2 illustrates the spirit of this approach, plotting kernel density estimates of the implied hourly wage in different states in 1966. We construct

[18] Measuring workers potentially affected by the minimum wage change is key to Card's construction of F_s^* in the CPS monthly ORG data. Because these data begin in 1979, they are not available for study of the 1966 FLSA. Moreover, continuous measures of hourly wages in the May CPS are not available until 1973.

[19] The share of wages below \$1.60 in a state is very strongly related to lower percentiles of the state wage distribution: a bivariate regression of the share of wages below \$1.60 in 1966 on the 10th percentile yields a point estimate of −0.344 (SE: 0.016) and an R^2 of 0.96. Not surprisingly, our results are very similar if we use the 10th percentile instead. In addition, we have considered using the fraction below \$1.92 per hour (1.2 × \$1.60). Our results are nearly identical.

hourly wages in the March CPS by dividing annual wage earnings in the previous year by the mean of weeks worked within each reported category in the previous year and hours worked in the week before the survey.[20] For a given change in the nominal minimum wage, the share of workers affected (approximated as the share with wages between \$1.25 and \$1.60) is larger in lower-earning states (such as Texas) than in higher-earning states (such as New York). Notably, however, Card's (1992) fraction affected does not capture changes in the FLSA's coverage that also extended to workers earning less than \$1.25 per hour—a crucial feature of the 1966 legislation that motivates our use of the cumulative share of workers earning less than \$1.60 per hour. This measure is correlated with the share of workers between the old and the new minimum wage but also accounts for concentration of low wages outside the covered range. Because the impact of the 1966 FLSA should be larger in lower-earning states, economic theory predicts that the law's effects on wages and employment should also be larger.

Table 1 displays the variation in fraction affected—the share of workers earning below the 1966 FLSA new minimum wage in the year before it took effect—and figure 3*A* presents this information in map form, where darker shades capture a higher share of wages below \$1.60 in 1966. As noted in Department of Labor wage studies, the share of wages below \$1.60 in 1966 was much higher in the South and interior states. However, there is substantial variation within the South and interior states in the bite of the statute, which our study leverages.

Our analysis presents the reduced-form estimates using the following event-study (eq. [3]) and difference-in-differences (eq. [4]) specifications:

$$Y_{s,b,t} = \sum_k \theta_k 1(t = k) F_{s,1966} + X'_{s,t}\boldsymbol{\beta} + \gamma_{s,b} + \delta_t + \varepsilon_{s,b,t}, \tag{3}$$

$$Y_{s,b,t} = \tilde{\theta} 1(t > 1966) F_{s,1966} + X'_{s,t}\boldsymbol{\beta} + \gamma_{s,b} + \delta_t + \varepsilon_{s,b,t}. \tag{4}$$

[20] This approach to constructing Card's "fraction affected" is very noisy, because the implied hourly wage suffers from (1) misreports by respondents about wage earnings, weeks, or hours, (2) the aggregation of weeks and hours into categories, or (3) failure of hours worked in the week before the survey to represent the hours worked in the average week during the previous year. This source of measurement error is so severe that—in contrast to the 1992 ORG—there is no spike in wages near the statutory minimum wage in the March CPS (fig. A1; figs. A1–A12 are available online). Similar to the smoothness of the March CPS in the 1990s, both the national and the state wage distributions from the March CPS show that a large fraction of workers appear to have earned below the statutory minimum in 1966 and fail to exhibit any heaping just above it. To demonstrate that the cumulative share of wages below the new minimum wage is correlated with fraction affected, table A1 (tables A1–A9 are available online) shows that, although we are unable to obtain Card's (1992) results using a direct calculation of fraction affected in the March CPS (rather than Card's use of the ORG), an approach using the cumulative share yields comparable results.

Table 1
Share of Workers with Hourly Wages below the 1966 Minimum Wage of $1.60, by State Group

State Group	Fraction Affected
New Jersey	.083
Alaska/Hawaii/Oregon/Washington	.090
California	.091
Illinois	.094
Ohio	.098
New York	.107
Pennsylvania	.109
Michigan/Wisconsin	.111
Connecticut	.117
Indiana	.130
Maine/Massachusetts/New Hampshire/Rhode Island/Vermont	.152
Delaware/Maryland/Virginia/West Virginia	.166
Arizona/Colorado/Idaho/Montana/Nevada/New Mexico/Utah/Wyoming	.176
Iowa/Kansas/Minnesota/Missouri/Nebraska/North Dakota/South Dakota	.193
Washington, DC	.223
Texas	.257
Georgia/North Carolina/South Carolina	.259
Kentucky/Tennessee	.279
Florida	.291
Arkansas/Louisiana/Oklahoma	.319
Alabama/Mississippi	.392
United States	.161

SOURCE.—Authors' calculations using the 1967 March Current Population Survey (CPS).
NOTE.—This table reports the share of men aged 16–64 with average hourly earnings below $1.60 in 1966. Sample includes men not residing in group quarters or in the military and for whom self-employment income accounts for no more than 10% of total income. Rows indicate the 21 state groups consistently identified in the CPS for our sample period.

Outcomes in the March CPS are average log hourly wages and employment during the year, reference week, or average annual hours worked in state group s, birth cohort b, where b ranges from 1895 (age 64 in 1959) to 1958 (age 16 in 1974), and year t, where t ranges from 1959 to 1973 for employment during the year or hourly wages and 1960–73 for employment in the reference week.[21] In equation (3), we normalize $\theta_{1966} = 0$, the year before the FLSA took effect and the year we measure the share of wages below \$1.60, $F_{s,1966}$. State fixed effects, γ_s, account for time-invariant differences across states, such as unchanging differences in legislation, geography, resource endowments, and cost of living. Year fixed effects, δ_t, account for national

[21] Note that reference week refers to the survey year, whereas weeks worked and hourly wages refer to the year preceding the survey. Therefore, our definition of t depends on the dependent variable. One limitation of the publicly available CPS data is that only 21 state groups are identified throughout our period of interest. The small number of groups limits our ability to account for autocorrelation.

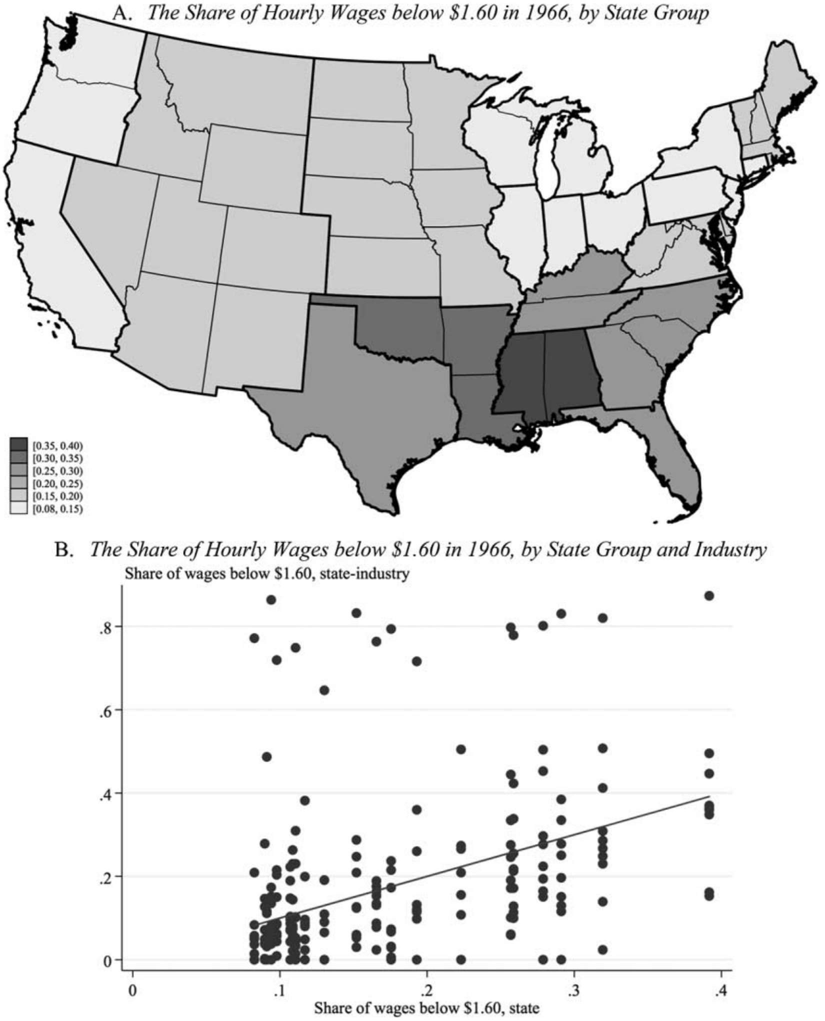


FIG. 3.—Share of workers in 1966 earning below the 1966 Fair Labor Standards Act (FLSA) minimum wage of $1.60. *A*, Share of hourly wages below $1.60 in each state group (see also table 1). *B*, Share of hourly wages below $1.60 in each state group/industry cell (*Y*-axis) by state group fraction below $1.60 (*X*-axis). Variation in the vertical dimension shows variation in the bite of the 1966 FLSA within state groups. We use 10 one-digit industries. Source: 1967 March Current Population Survey. A color version of this figure is available online.

changes across years that may also affect wages: large tax cuts (1964), the Civil Rights Act (1964) and Voting Rights Act (1965), Medicare (1966), and other Great Society legislation (Bailey and Danziger 2013; Bailey and Duquette 2014).

In some specifications, we also include state-by-birth-cohort fixed effects, $\gamma_{s,b}$, to account for time-varying characteristics of each state's labor force. For instance, these fixed effects would account for the differential evolution of school quality (Card and Krueger 1992b) and racial discrimination (Donohue and Heckman 1991; Wright 2013) across birth cohorts within states. Finally, we include gross state product to account for potentially different exogenous rates of economic growth across states unaccounted for by changes across birth cohorts.[22] This final covariate intends to reduce omitted-variable bias due to differential changes in the demand for workers in states differentially affected by the 1966 FLSA. However, because of concerns about endogeneity to the effects of the 1966 FLSA, which could affect economic growth directly, we omit this variable from our preferred specification. For computational reasons, we partial out covariates to adjust for potentially confounding changes in individual characteristics in some specifications using the Frisch-Waugh-Lovell theorem (Frisch and Waugh 1933; Lovell 1963).[23]

The point estimates of interest, θ, capture the regression-adjusted, reduced-form comovements of the outcome variable with the bite of the 1966 FLSA. Because the 1966 FLSA should affect outcomes only after the amendments took effect, one test of the validity of the research design is whether $\theta_t = 0$ jointly for all $t < 1966$ in equation (3). Of course, the 1961 FLSA may have differentially impacted wages in states with a greater share of wages below \$1.60 in 1966, which may lead to a slight pretrend. Similarly, because the 1966 FLSA should increase wages after 1966, we should observe $\theta_t > 0$ only for $t > 1966$. Standard errors are corrected for an arbitrary within-state covariance structure (Arellano 1987).

In addition to presenting the estimates for the reduced form, we estimate the labor demand elasticity by estimating equation (4) using two-stage least squares, with log wages as the outcome in the first stage and the employment rate (in levels) as the outcome in the second stage. We calculate the elasticity by dividing the resulting second-stage point estimate of β by the mean employment rate in 1966.

[22] These data come from the BEA regional economic accounts (https://apps.bea.gov/regional/downloadzip.cfm). Observations for 1959–62 are unavailable, so we extrapolate linearly from the 1963–66 period for the period when they are missing.

[23] We partial out these covariates by estimating regressions on individual-level data. The dependent variables in these regressions are the outcomes of interest and the interactions between fraction affected and year, and the explanatory variables are the indicated covariates. The 1960 US Census of the Population has 2.4 million individual observations, while the CPS surveys contain 13,000–40,000 individuals per year. We therefore weight the individual-level regressions by the inverse of the number of people in each survey year in our employment sample (positive weeks worked) to ensure that each survey year contributes more equally to the estimates. We also weight estimates of eqq. (3) and (4) by the number of individuals in each state-year cell, so that each survey year is weighted equally.

IV. Results: The Effects of the 1966 FLSA on Wages and Employment

Documenting the aggregate effect of the 1966 FLSA is key to understanding the effect of a high national minimum wage on the economy. Although the BLS conducted a number of surveys to address this question, these studies were specific to certain industries and are not representative of all US workers. Our analysis therefore uses the March CPS to quantify (1) the wage effects of the 1966 FLSA for a nationally representative sample and (2) any resulting changes in employment.

A. Wages

Figure 4*A* plots the event-study results for all men aged 16–64 using the baseline specification of equation (3) (all covariates except for gross state product). Dashed lines represent the 95%, point-wise confidence intervals. In addition, we report the comparable reduced-form difference-in-differences estimate from equation (4), summarizing the effect averaged over all years after 1966 (table 2, col. 3). Consistent with these estimates reflecting the 1966 FLSA itself (rather than potentially confounding policy changes), hourly wages in lower- and higher-wage states followed similar trends before the 1966 FLSA, and these increases appear after the 1966 FLSA was implemented. Our baseline estimates imply that lower-earning states such as Texas (the lower quartile of the interquartile range of share of workers with wages below \$1.60 in 1966) experienced a 6.0% larger increase (0.397×0.15) in average wages relative to states such as New York (upper quartile), where wages were higher and the 1966 FLSA was less binding. The increase in wages persists through the end of our sample in 1973.

One potential threat to the internal validity of our research design is that other state or federal changes after 1966—not accounted for in gross state product—could confound our estimates. Because there is a great deal of within-state, across-industry variation in the share of wages below \$1.60 (fig. 3*B*), we test this hypothesis by refining our estimating equation to examine changes within a state-industry cell using the following event-study specification:

$$Y_{j,s,t} = \sum_{k=1959}^{1973} \pi_k 1(t = k) F_{j,s,1966} + \delta_{j,s} + \delta_{s,t} + \varepsilon_{j,s,t}. \tag{5}$$

One-digit industries are indexed by j, and other notation remains as previously described. The advantage of this specification is that it permits fixed effects by state-year, $\delta_{s,t}$, as well as by industry-state, $\delta_{j,s}$. State-year fixed effects flexibly control for any exogenous state-level changes in the demand for or supply of workers (which are not captured in gross state product in eqq. [3] and [4]). The point estimates of interest, π, capture changes after 1966 in lower-wage state-industry combinations (which would have been more affected by the 1966 FLSA) relative to higher-wage state-industries.

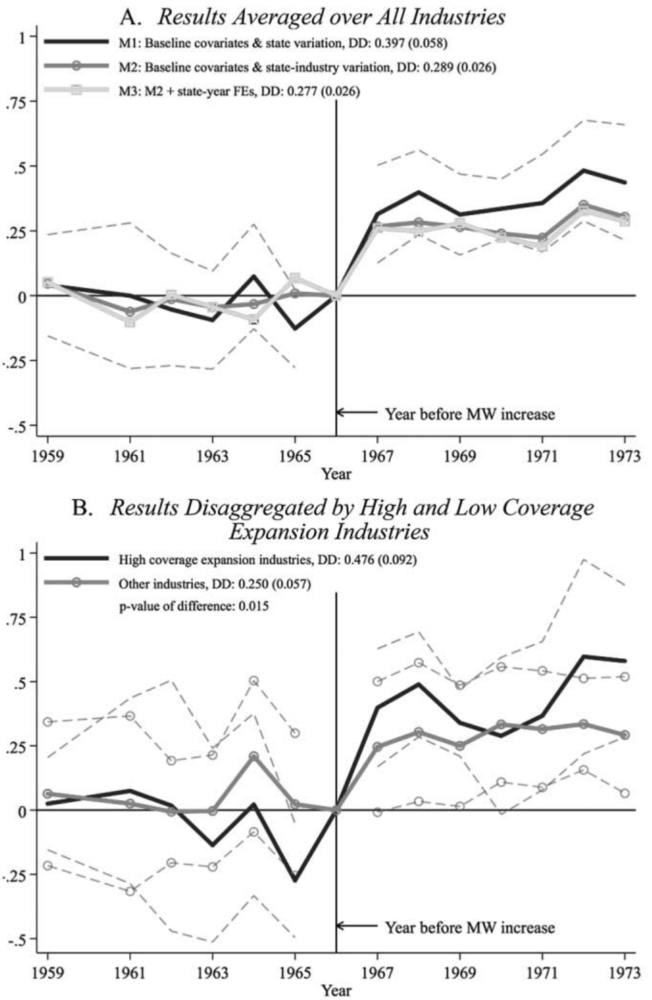


FIG. 4.—Effects of the 1966 Amendments to the Fair Labor Standards Act (FLSA) on log hourly wages. *A*, Point estimates and 95% confidence intervals for equations (3) and (5) using the log hourly wage as the dependent variable. All regressions include indicators for state by birth cohort, year, age, nonwhite, marital status, and metropolitan residence status. Model 1 (M1) plots estimates of equation (3) and includes state and year fixed effects. Models 2 (M2) and 3 (M3) plot estimates of equation (5). Both models include state-by-industry and year fixed effects, and M3 additionally includes state-by-year fixed effects. *B*, Estimates of equation (3), separately for industries with a large coverage expansion in the 1966 FLSA and for other industries (see main text for definition). Sample includes men aged 16–64 not residing in group quarters or in the military for whom self-employment income accounts for no more than 10% of total income. Standard errors are clustered at the state group level. Sources: 1960 US Census of the Population and 1962–74 March Current Population Survey. MW = minimum wage. A color version of this figure is available online.

Table 2
Reduced-Form Effects of the 1966 Amendments to the Fair Labor Standards Act on Wages and Employment

	(1)	(2)	(3)	(4)
A. Log hourly wage (mean real wage in 1966: $22.92):				
Post-1966 × fraction affected	.589	.491	.397	.366
	(.064)	(.062)	(.058)	(.066)
F statistic	85.654	61.821	47.427	30.661
Effect of moving across IQR (.15)	.088	.074	.060	.055
Effect of 1 SD increase (.09)	.053	.044	.036	.033
B. Employed during year (mean in 1966: .917):				
Post-1966 × fraction affected	.061	.041	−.049	−.060
	(.032)	(.034)	(.027)	(.027)
Effect of moving across IQR	.009	.006	−.007	−.009
Effect of 1 SD increase	.005	.004	−.004	−.005
C. Employed in reference week (mean in 1966: .819):				
Post-1966 × fraction affected	.108	.074	−.002	−.025
	(.030)	(.030)	(.035)	(.037)
Effect of moving across IQR	.016	.011	.000	−.004
Effect of 1 SD increase	.010	.007	.000	−.002
D. Annual hours worked (mean in 1966: 1,631):				
Post-1966 × fraction affected	294.459	148.391	−42.854	−166.064
	(72.196)	(67.249)	(63.525)	(84.225)
Effect of moving across IQR	44.169	22.259	−6.428	−24.910
Effect of 1 SD increase	26.501	13.355	−3.857	−14.946
State and year fixed effects	×	×	×	×
Demographic covariates		×	×	×
State-by-cohort fixed effects			×	×
Log gross state product				×
State-year observations	294	294	294	294

Sources.—1960 US Census of the Population, 1962–74 March Current Population Survey, Bureau of Economic Analysis regional economic accounts.

Note.—Panel titles refer to the dependent variable used for eq. (4). Estimates are the coefficient on the interaction between the share of workers with wages in each state below $1.60 in 1966 and an indicator variable for the year being 1967–73 (inclusive). Sample includes men aged 16–64 not residing in group quarters or in the military. In panels A, B, and D, we exclude individuals for whom self-employment income accounts for no more than 10% of total income. In panel C, we exclude individuals who report being self-employed in the reference week. Standard errors are clustered at the state group level. All dollar amounts are adjusted to 2019 dollars using the consumer price index for all urban consumers. For the share of wages below $1.60 in 1966, the cross-state standard deviation is .090 and the interquartile range is .150. Panel C has 21 fewer observations because we focus only on outcomes through 1973. Number of observations is 1,878,830 (panel A), 2,407,230 (panels B and D), and 2,447,550 (panel C). IQR = interquartile range.

Figure 4*A* plots the results as model 2 (M2), which changes the key independent variable to a state-by-industry variable and adds state-by-industry fixed effects, and model 3 (M3), which adds state-by-year fixed effects to M2. The similarity of these estimates to those from our baseline specification (M1) and to one another (M2 vs. M3) implies that state-year changes in worker demand or supply are not driving (or offsetting) the legislation's effects—a finding that should ameliorate concerns about the interpretation of employment analyses where we cannot use industry variation and include state-year fixed effects.[24]

Table 2 presents additional robustness checks as reduced-form difference-in-differences estimates. Similar to the robustness in figure 4*A*, panel A of table 2 shows how the combined post-1966 effects are affected by the inclusion of individual covariates (age, race, marital status, and metropolitan area; col. 2), state-by-birth-cohort fixed effects to account for unobserved changes within states across birth cohorts (such as improvements in school quality or the cohort-evolving antidiscrimination efforts in the South; col. 3), and time-varying, state-level controls for the natural log of gross state product (col. 4). The inclusion of state-by-birth-cohort effects only modestly reduces the estimated wage effects across the interquartile range by 0.014 when moving from column 2 to column 3 and by 0.005 when moving from column 3 to column 4. Noteworthy is that the estimates change little across specifications and our baseline specification (col. 3) is not statistically different when controlling for gross state product in column 4 ($p = .38$). For the interested reader, the appendix presents the event-study estimates for these specifications.

These wage increases likely reflect both the 1966 FLSA's increase in the real minimum wage for previously covered workers and its coverage expansion for previously uncovered workers. To separate these effects, we estimate equation (3) separately for "high-coverage-expansion industries"—which Martin (1967) indicates to be agriculture, forestry and fisheries, construction, retail trade (eating and drinking establishments and other retail establishments), services (personal, entertainment and recreation, medical, hospitals, and educational), and government (postal service, federal, state, and local)—and other industries. The resulting estimates quantify the wage effects of the 1966 FLSA in industries where coverage expanded the most and those where the effects are predominantly driven by increases in the minimum wage (not coverage).

As expected, figure 4*B* shows that the wage increase in high-expansion industries, which employed 40.6% of all workers in 1966, is substantially larger than in industries where most workers were previously covered under the 1966 FLSA: the difference-in-differences estimates are 0.48 (SE: 0.09) and 0.25 (SE: 0.06), respectively. These estimates are also statistically distinguishable at the 5% level. This makes sense, because wages in industries uncovered before the 1966 FLSA increased by much more than wages in industries that were covered. Interestingly, our difference-in-differences estimate of 25 log points for industries not experiencing a large coverage expansion (labeled "other") is a bit larger than the 23% increase in the real minimum

[24] Industry is reported for most individuals who are at work or looking for a job. It is not reported for unemployed workers without prior work experience or the long-term unemployed. Therefore, we cannot correctly compute the share of an industry-state cell that is employed, because the denominator is not measured.

wage (sec. II.A), which likely reflects cross-industry spillovers as well as the difficulty in mapping aggregated industries to finer FLSA regulations about coverage. This sizable increase in industries where most workers were previously covered suggests an important role for the statutory minimum wage increase and, potentially, general-equilibrium wage adjustments across industries. While the real minimum wage declined over time because of inflation (fig. 1*A*), the estimated effect on real log wages is persistent, which reflects the large increase in coverage under the 1966 FLSA (fig. 1*B*).

A final, partial-equilibrium exercise seeks to gauge the plausibility of these effect sizes. As a benchmark, our difference-in-differences estimate of 0.40, scaled by an estimated 16.2% of workers having wages below $1.60 in 1966 in the March CPS, suggests that, nationally, average wages rose by 6.5% because of the 1966 FLSA. This estimate is also consistent with the following decomposition of the wage effects among employees covered before the 1966 FLSA, b; employees newly covered under the 1966 FLSA, n; and employees uncovered by the 1966 FLSA, u,

$$\Delta \log W = \phi_b^{66} \Delta \log W_b + \phi_n^{66} \Delta \log W_n + \phi_u^{66} \Delta \log W_u . \quad (6)$$

The weights, ϕ, represent the share of US employees in each of these groups in 1966 prior to the legislation (which implicitly assumes no disemployment effects due to the 1966 amendments). According to the Department of Labor, 44% of workers were covered by the FLSA before 1966, and 12% of these workers would have been directly affected by the minimum wage increase because their wages were between $1.25 and $1.60. Assuming that these workers received an average raise of 25% (in real terms, see "Other industries" in fig. 4*B*) implies a 1.3% average wage increase in the economy. More difficult to quantify is the effect among newly covered workers (roughly 13% of all US workers in 1966), whose nominal wages grew in some cases from less than $1.00 in 1967 to $1.60 in 1971. If half of this group experienced a 48% real wage gain (see "High coverage expansion industries" in fig. 4*B*), then average wages would have increased by another 3.1%.[25] Finally, 43% of workers remained uncovered after the 1966 amendments, and we expect their wages to rise in equilibrium, assuming no disemployment effects and no spillovers above the minimum wage. This group would need to have experienced another 2% increase in wages (e.g., 20% of these workers experienced a 24% real wage gain, half of that experienced by covered workers). In short, while

[25] Based on a series of industry studies conducted by the Department of Labor, Karlin (1967) estimates that this group should have contributed 0.8% to the yearly payroll using only the increase to $1.00 in 1967. Noteworthy is that Karlin's calculation is limited in its applicability to industries outside the subset considered in his study. For instance, it neglects most of the public sector employees affected, comprising 27% of newly covered workers.

there is considerable uncertainty about some of the inputs into this back-of-the-envelope calculation, the magnitudes of our estimates are plausible.

B. Employment and Annual Hours Worked

These robust increases in wages to a high (real) level could lead to disemployment in both perfectly competitive and monopsonistic models of the labor market. To investigate this, panels *A* and *B* of figure 5 present the reduced-form estimates for our baseline specification of equation (3) for several different measures of employment: employment at any point during the year (positive weeks worked), employment in the reference week, and annual hours worked (including individuals working no hours).[26] The first two measures capture different employment adjustments to the 1966 FLSA. The former captures longer-term, persistent employment responses by measuring disemployment only if the individual is not employed at any point during the year. The latter captures employment responses only during the March reference week and so is more sensitive to short-term, transitory fluctuations in employment. Annual hours worked describe changes in the combination of the extensive and intensive margins of work.

Figure 5*A* presents our baseline event-study specification as well as specifications that control for log gross state product. The rationale for including this additional time-varying covariate is that we cannot use the state-industry variation in the share of potentially affected workers or, by extension, include state-by-year fixed effects to account for differential, exogenous changes in the demand for or supply of workers. Panels B–D in table 2 additionally summarize these results using the difference-in-differences specification (eq. [4]) for the specifications previously discussed.

This analysis shows that the 1966 FLSA's wage and coverage increases had only modest disemployment effects that, interestingly, appear mainly for longer-term employment. The March CPS shows that the share of men employed during the year fell by 0.7% in areas such as Texas relative to New York, when these areas experienced larger wage changes after the 1966 FLSA (panels B and C in table 2, col. 3). In contrast, men's employment during the reference week fell by only 0.03%—or not at all, given the event-study estimates in figure 5*B*. A natural explanation that reconciles these two findings is that many of the men who no longer work during the year after the 1966 FLSA were less attached to the labor market and therefore less likely to be working in the March reference week, even in the absence of the 1966 FLSA. Consequently, employment in the reference week shows less of a decline after the 1966 FLSA's implementation. These findings

[26] Annual hours worked are constructed by multiplying the mean of weeks worked (within each reported category) by the hours worked last week. We use the year for weeks worked as the index of t in our regressions.

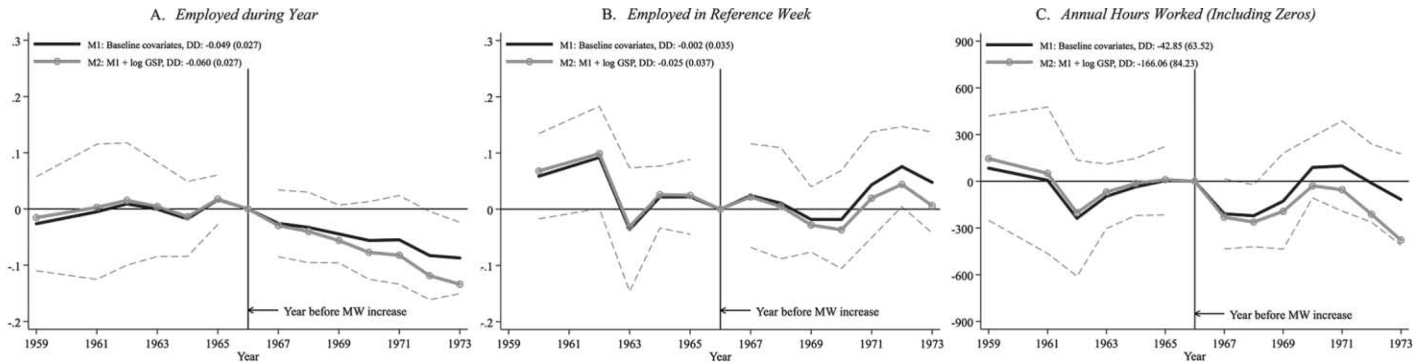


FIG. 5.—Effects of the 1966 Amendments to the Fair Labor Standards Act on employment. This figure plots point estimates and 95% confidence intervals for equation (3) using the dependent variable indicated in the panel title. Regressions include state and year fixed effects and indicators for state by birth cohort, age, nonwhite, marital status, and metropolitan residence status (corresponding to col. 3 of table 2). Sample includes men aged 16–64 not residing in group quarters or in the military who are not self-employed (for details, see table 2's note). Standard errors are clustered at the state group level. Sources: 1960 US Census of the Population and 1962–74 March Current Population Survey. MW = minimum wage. A color version of this figure is available online.

may also be related to the fact that agricultural employment—where workers are more seasonal—was particularly impacted by the 1966 FLSA's coverage expansion. Annual hours worked were 6 hours lower per year over a pre-1966 FLSA average of 1,631, implying a 0.4% decrease. The weaker effects on annual hours worked (which includes zeros) suggests that some of the extensive margin disemployment is offset by increased hours among men remaining employed. The pattern of rising disemployment in figure 5*A* is consistent with the results in Meer and West (2016). However, we do not see evidence of this pattern for other employment measures.

Table 2 shows that the employment effects are sensitive to the inclusion of state-by-cohort effects. The motivation for these covariates is that school quality—measured by teacher-to-pupil ratios, term length, and teacher wages—was differentially improving in Southern states during our period of interest (Card and Krueger 1992b). The appendix maps Card and Krueger's data into high and low fraction affected states and shows that school quality improved faster for cohorts in the former category. In addition, for both white and black men, the average years of schooling in more affected states converged to that of less affected states. These trends suggest that—even in the absence of the 1966 FLSA—productivity and employment would have risen differentially in more affected states, at the same time that the black-white earnings gap narrowed (Card and Krueger 1992a). Higher productivity growth in more affected states tends to reduce disemployment effects when omitting these controls.

Consistent with changes in unobserved cohort attributes improving employment differentially in states where the minimum wage's impact was larger, including the state-by-cohort effects reverses the sign of the relationship of the 1966 FLSA to employment (cf. cols. 2 and 3). Similarly, controlling for gross state product—which controls for differential productivity growth in the South in the absence of the legislation—leads to slightly more negative employment effects in column 4 of table 2. Just as figure 4*A* suggests that including state-year effects tends to dampen the estimated wage effects, controlling for gross state product tends to make the employment estimates more negative. The estimates for employment during the year are not different from one another ($p = .57$). However, estimates for employment in the reference week are marginally statistically different from one another ($p = .107$), and annual hours worked are statistically different from one another ($p = .071$).

The sensitivity of our results to controlling for gross state product is consistent with two explanations. First, states where the 1966 FLSA had a larger impact tended to experience higher economic growth for exogenous reasons, so accounting for differential employment trends tends to make the observed effects of the 1966 FLSA more negative. In addition, states where the 1966 FLSA had a larger impact tended to experience faster employment growth because of the 1966 FLSA, so controlling for one result of the

Table 3
Elasticities of Employment and Annual Hours Worked with Respect to Wages

	(1)	(2)	(3)	(4)
A. Employed during year:				
Wage elasticity	.112	.091	−.135	−.177
	(.056)	(.069)	(.077)	(.086)
B. Employed in reference week:				
Wage elasticity	.263	.217	−.009	−.099
	(.069)	(.080)	(.128)	(.154)
C. Annual hours worked:				
Wage elasticity	.307	.185	−.066	−.277
	(.070)	(.076)	(.098)	(.158)
State and year fixed effects	×	×	×	×
Demographic covariates		×	×	×
State-by-cohort fixed effects			×	×
Log gross state product				×
State-year observations	294	294	294	294

NOTE.—This table reports own-wage elasticities from two-stage least squares estimation of eq. (4). Panel titles refer to the dependent variable used in the employment regressions. First-stage estimates and dependent variable means are shown in table 2. See table 2's note for information on the sample and sources.

legislation's effect tends to make the observed effects smaller. The data do not distinguish between these explanations, so both should be considered in interpreting the results.

C. Aggregate Demand Elasticities

Table 3 presents the demand elasticities implied by these estimates. For employment during the year, the demand elasticity with respect to the wage is −0.135 (col. 3, one-sided test rejects zero) to −0.177 (col. 4, also statistically significant). The increase in the estimates between columns 3 and 4 implies that accounting for faster employment growth in areas more affected by the 1966 FLSA generates larger demand elasticities. For employment in the reference week, the demand elasticities are −0.009 to −0.099 in the specification without and with controls for gross state product, respectively, and statistically indistinguishable from zero in both cases. For annual hours worked, the respective elasticities are −0.066 to −0.277 (one-sided test for the latter rejects zero). Noteworthy is that the extensive margin elasticities are smaller in magnitude than the employment elasticities for teens presented in Brown (1999; elasticities ranging from −0.5 to −2.7) and at the lower end of the range found by Card (1992; elasticities ranging from −0.12 to 0.39).[27] Figure 6 places our demand elasticity estimates within the context of recent

[27] In reviewing the earlier time-series literature, Brown (1999) finds a consensus range of elasticities of teenage employment with respect to the minimum wage of −0.1 to −0.3. As he notes, these elasticities need to be multiplied by five to nine to obtain traditional labor demand elasticities (2114–15).

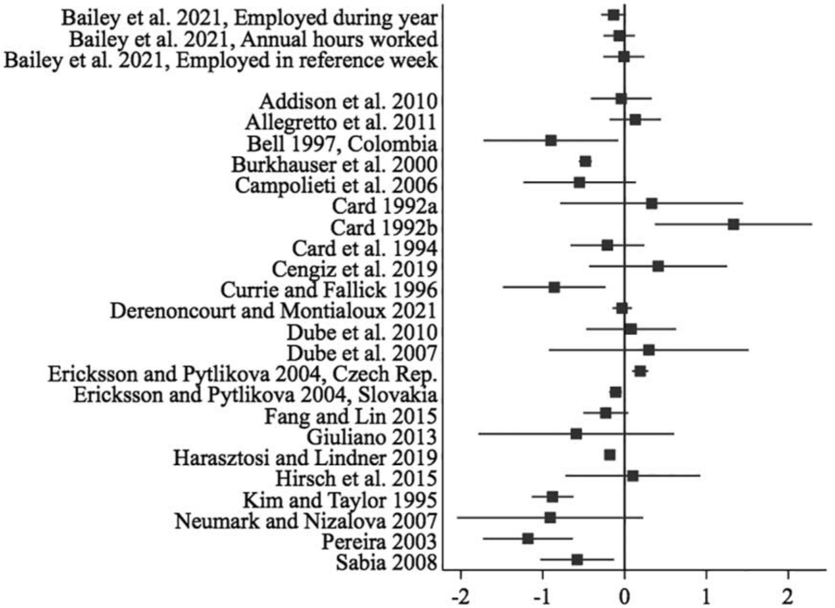


FIG. 6.—Comparison of own-wage employment elasticities to previous research. This figure displays point estimates and 95% confidence intervals of own-wage employment elasticities from other papers studying minimum wage increases. Expanding figure A6 of Harasztosi and Lindner (2019), this figure contains estimates from column 3 of table 3, as well as estimates from Cengiz et al. (2019) and Derenoncourt and Montialoux (2021). The vertical line is at zero. A color version of this figure is available online.

papers. Our estimated demand elasticities fall roughly in the middle of elasticities in this literature, and the results for employment during the year are identical to Harasztosi and Lindner's (2019) estimated −0.18 elasticity (with respect to the workers' wages in Hungary in 2001), where the real minimum wage increased by around 60%.

D. Heterogeneity in Effects by Race, Age, and Education

A final analysis examines heterogeneity in the effects of the 1966 FLSA by demographic and skill groups. Several reasons suggest that the effects could vary across demographic groups. First, different workers have different skills, and less skilled workers (often proxied by less education, younger age, or minority racial group) were more likely to be directly affected by the 1966 FLSA. Second, different workers (even with the same skills) may be more concentrated in different industries because of regional differences in industry concentration or historical or institutional reasons (e.g., both agriculture and the share of African American workers is higher in the South). Because the FLSA affected coverage according to industry of employment, younger and less skilled workers may be more affected. Third, the 1966 FLSA also could lead to different effects for workers with different supply elasticities, which is related to race- or age-based discrimination. Although the CPS is not rich enough to separate the mechanisms for differential effects, this final section documents the differential incidence of the FLSA by different race, age, and education subgroups by estimating equations (3) and (4) separately for each subgroup.[28]

Table 4 presents reduced-form, difference-in-differences estimates of wages for subgroups of workers defined by race, education, and age. Figures 7 and 8 present the complementary event-study specifications, which—owing to smaller sample sizes—group years to increase precision as follows: 1959–61, 1962–63, 1964–65, 1967–68, 1969–70, and 1971–73 (estimates plotted against the year-group midpoint on the X-axis). Panel A in table 4 and figure 7 show that all groups experienced wage increases and, as expected, groups with lower average skill experienced larger increases in wages. In 1966, for instance, 38% of black men and 15% of white men earned below the 1966 FLSA minimum wage (see the appendix). And while both groups experienced large wage increases after 1966, the estimate for African American men was almost three times as large as that for white men, owing to the fact that black men lived in lower-earning regions (e.g., the South) and tended to work in lower-earning industries previously uncovered by the FLSA. Similarly, the wages of men with less than a 12th-grade education (approximately the median in 1966) increased by 33% more than men with at least a 12th-grade education. Teenagers experienced a larger wage increase than men aged 20–35, who in turn experienced a larger wage increase than those aged 36–64. However, large wage growth among teenagers comes with the caveat that the event-study estimates in figure 7*C* show that their wages were trending upward in more affected states before the 1966 FLSA took effect, which limits the strength of conclusions about causal effects of the legislation. The broad conclusion, however, is that the 1966 Amendments to the FLSA substantially increased wages for a large group of workers across the country.

The remaining panels of table 4 present the reduced-form effects of the 1966 FLSA on employment, and figure 8 shows the corresponding event-study estimates.[29] Owing to smaller sample sizes, most employment outcomes are imprecisely estimated. Similar to the estimates in table 2, the absolute

[28] When examining heterogeneity across age groups, we pool men aged 16–64 in the same regression and allow for interactions between age group indicators and all explanatory variables except for state-by-cohort fixed effects. We do this because state-by-cohort fixed effects absorb most of the state-by-year variation with small age ranges, such as men aged 16–19.

[29] Tables A4 and A5 report results when we control for log gross state product.

Table 4
Reduced-Form Effects of the 1966 Amendments to the Fair Labor Standards Act on Log Wages, Employment, and Annual Hours Worked, by Subgroup

	White Men (1)	African American Men (2)	Less than 12 Years Education (3)	At Least 12 Years Education (4)	Aged 16–19 (5)	Aged 20–35 (6)	Aged 36–64 (7)
A. Dependent variable: log hourly wage:							
Mean[a]	23.62	16.64	19.79	25.23	13.39	21.89	24.69
Post-1966 × fraction affected	.290	.792	.426	.318	.625	.406	.328
	(.062)	(.105)	(.074)	(.075)	(.217)	(.112)	(.048)
F statistic	21.73	57.25	32.99	17.94	8.29	13.26	45.88
Effect of moving across IQR (.15)	.044	.119	.064	.048	.094	.061	.049
Effect of 1 SD increase (.09)	.026	.071	.038	.029	.056	.037	.030
B. Dependent variable: employed during year:							
Mean	.923	.873	.866	.960	.731	.964	.936
Post-1966 × fraction affected	−.026	−.200	−.053	.011	−.075	−.024	−.057
	(.027)	(.067)	(.037)	(.016)	(.083)	(.035)	(.031)
Effect of moving across IQR	−.004	−.030	−.008	.002	−.011	−.004	−.009
Effect of 1 SD increase	−.002	−.018	−.005	.001	−.007	−.002	−.005

C. Dependent variable: employed in reference week:							
Mean	.827	.760	.750	.881	.405	.882	.890
Post-1966 × fraction affected	−.004	−.055	.002	.027	−.061	.058	−.034
	(.036)	(.063)	(.041)	(.040)	(.126)	(.036)	(.036)
Effect of moving across IQR	−.001	−.008	.000	.004	−.009	.009	−.005
Effect of 1 SD increase	.000	−.005	.000	.002	−.005	.005	−.003
D. Dependent variable: annual hours worked:							
Mean	1,666	1,356	1,392	1,837	335	1,796	1,871
Post-1966 × fraction affected	19.3	−452.7	39.9	−61.2	−174.6	−11.6	−102.8
	(73.5)	(140.3)	(80.1)	(62.5)	(101.0)	(87.9)	(104.1)
Effect of moving across IQR	2.89	−67.90	5.99	−9.18	−26.19	−1.73	−15.42
Effect of 1 SD increase	1.73	−40.74	3.59	−5.51	−15.71	−1.04	−9.25
State-year-(age group)[b] cells	294	266[b]	273[b]	273[b]	882[b]	882[b]	882[b]

NOTE.—Panel titles refer to the dependent variable used in eq. (4) (specification for table 2, col. 3). Column headings identify subsamples. Columns 5–7 pool three age groups for 294 state-year cells for a total of 882 cells, allowing for interactions between age group dummies and all covariates except for state-by-cohort fixed effects. Standard errors are clustered at the state group level. Number of observations in panel A is 1,693,170 (col. 1), 168,354 (col. 2), 959,314 (col. 3), 910,701 (col. 4), 101,764 (col. 5), 721,594 (col. 6), and 1,055,467 (col. 7). In panels B and D, the number of observations is 2,140,780 (col. 1), 241,222 (col. 2), 1,330,950 (col. 3), 1,064,540 (col. 4), 286,634 (col. 5), 855,638 (col. 6), and 1,264,958 (col. 7). In panel C, the number of observations is 2,179,050 (col. 1), 242,890 (col. 2), 1,351,140 (col. 3), 1,083,870 (col. 4), 286,514 (col. 5), 867,745 (col. 6), and 1,293,288 (col. 7). See table 2's note for additional information on the sample and sources. IQR = interquartile range.

[a] Mean is the 1966 average of hourly wages for wages greater than zero and less than the 95th percentile.

[b] To ensure that state groups are balanced across all years for a subgroup, we drop Connecticut and Alaska/Hawaii/Oregon/Washington in col. 2. Columns 3–4 exclude 1963 because the 1963 Current Population Survey does not contain education.

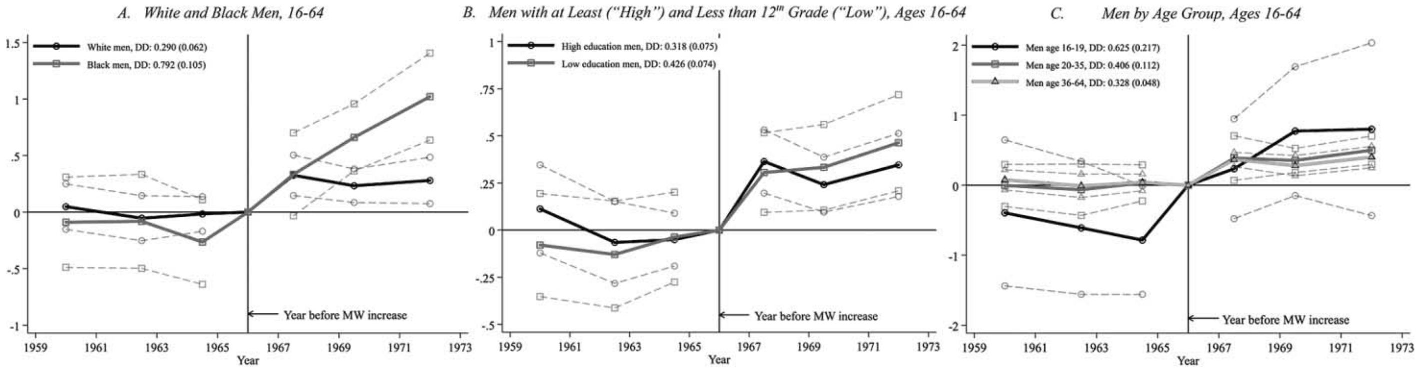


FIG. 7.—Effects of the 1966 Amendments to the Fair Labor Standards Act on log wages, by subgroup. Panel titles refer to subgroups examined. This figure plots estimates of the interaction between the share of wages in each state below \$1.60 in 1966 and indicators for years per equation (3). Because subgroup samples are smaller, we group years 1959–61, 1962–63, 1964–65, 1967–68, 1969–70, and 1971–73 and plot estimates at the midpoint of each interval. Difference-in-differences regressions using the same covariates are given in table 4 and summarized in the legend. See also figure 5's note. MW = minimum wage. A color version of this figure is available online.

magnitudes of the disemployment estimates are generally larger for employment during the year relative to employment in the reference week and annual hours worked.

One noticeable exception is for African American men, who experienced a sharp and statistically significant decline in employment during the year and annual hours worked. Moving across the interquartile range implies that employment during the year was 3.4% lower (3 percentage points over a baseline of 87%) and their annual hours worked fell by 5% (68 hours over a baseline of 1,356) after the 1966 FLSA became effective. Notably, employment in the reference week decreased by less, at 1.1% (or 0.8 percentage points over a baseline of 76%). This is consistent with Clemens, Kahn, and Meer (2020), who also find evidence of labor substitution that disadvantages the lowest-wage workers. By comparison, changes in employment for white men when moving across the interquartile range were much smaller and statistically insignificant. In addition, the black-white differences in table 4 for log wages, employment during the year, and annual hours worked are statistically significant (p-values of .001, .052, and .012, respectively), but the difference in employment in the reference week is not (p-value of .44). The event-study estimates show the persistence of the employment responses for black men during the year as well as in annual hours worked, although these effects fade modestly after 1969 as inflation reduced the magnitude of the wage effects for previously covered workers. The persistence is likely driven by reductions in employment in newly covered areas of the economy with permanently higher wages.

The results for employment in the reference week provide a direct comparison with Derenoncourt and Montialoux (2021), who use employment in the reference week conditional on labor force participation as their primary outcome. In addition, their preferred specification does not control for state by birth cohort or gross state product, which table 2 demonstrates are important in accounting for differential skill and economic growth in more affected states. Similar to our findings, Derenoncourt and Montialoux (2021) report that the 1966 FLSA had no detectable effect for employment in the reference week.[30]

Figure 9 summarizes the relevant demand elasticities across groups, and table 5 reports them for each group. The demand elasticities for black men are −0.289 (SE: 0.089) for employment during the year, −0.107 (SE: 0.124) for employment in the reference week, and −0.421 (SE: 0.153) for annual hours worked. The elasticity for annual hours worked is especially large and stems from a reduction in weeks worked and the usual number of hours worked per week (see the appendix). Notably, the black-white difference in the own-wage elasticity is statistically different for annual hours worked

[30] The appendix provides additional discussion of the relationship between our work and Derenoncourt and Montialoux (2021).

A. White and Black Men, Ages 16-64

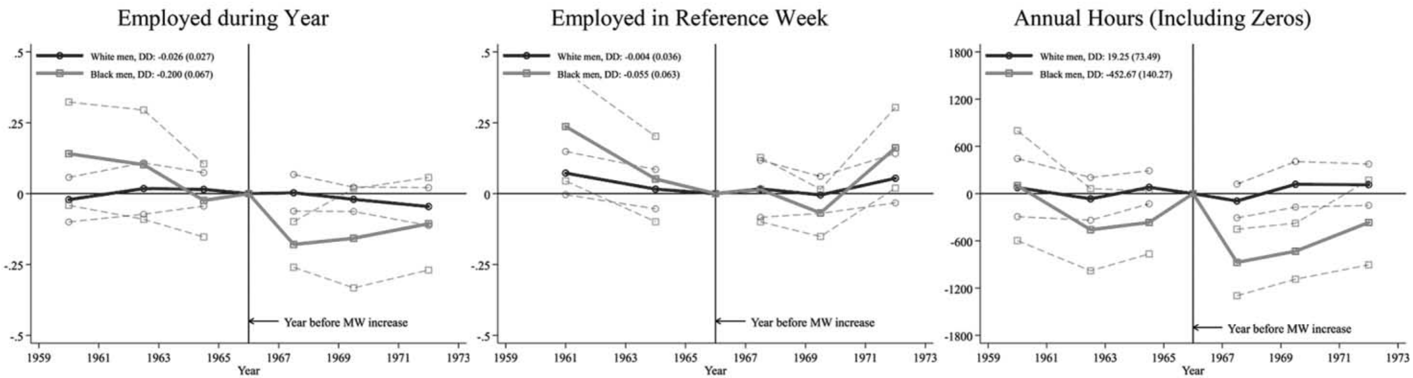
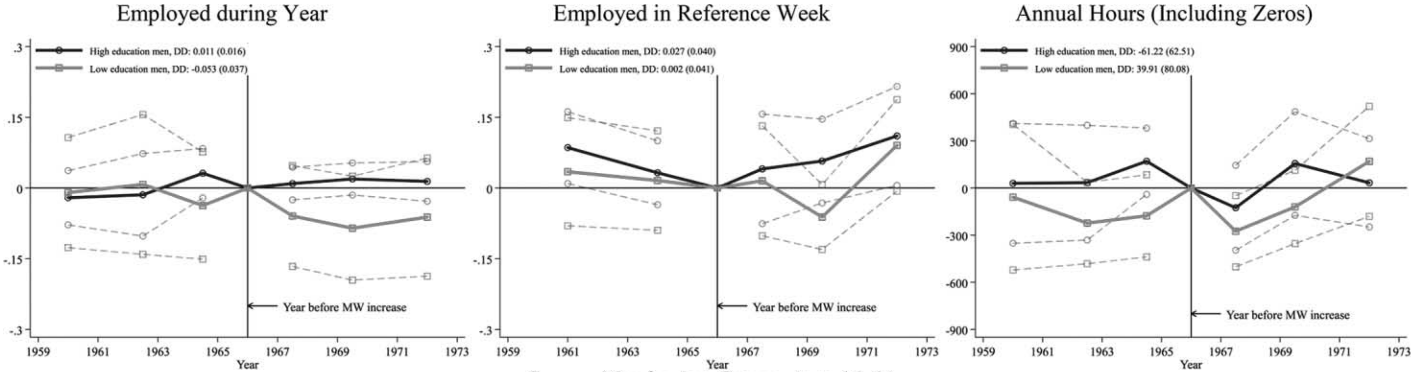


FIG. 8.—Effects of the 1966 Amendments to the Fair Labor Standards Act on employment, by subgroup. Panel titles refer to subgroups examined. This figure plots estimates of the interaction between the share of wages in each state below \$1.60 in 1966 and indicators for years per equation (3). Because subgroup samples are smaller, we group years 1959–61, 1962–63, 1964–65, 1967–68, 1969–70, and 1971–73 and plot estimates at the midpoint of each interval. Difference-in-differences regressions using the same covariates are given in table 4. See also figure 5's note. MW = minimum wage. A color version of this figure is available online.

B. Men with at Least ("High") and Less than 12th Grade ("Low"), Ages 16-64



C. Men by Age Group, Ages 16-64

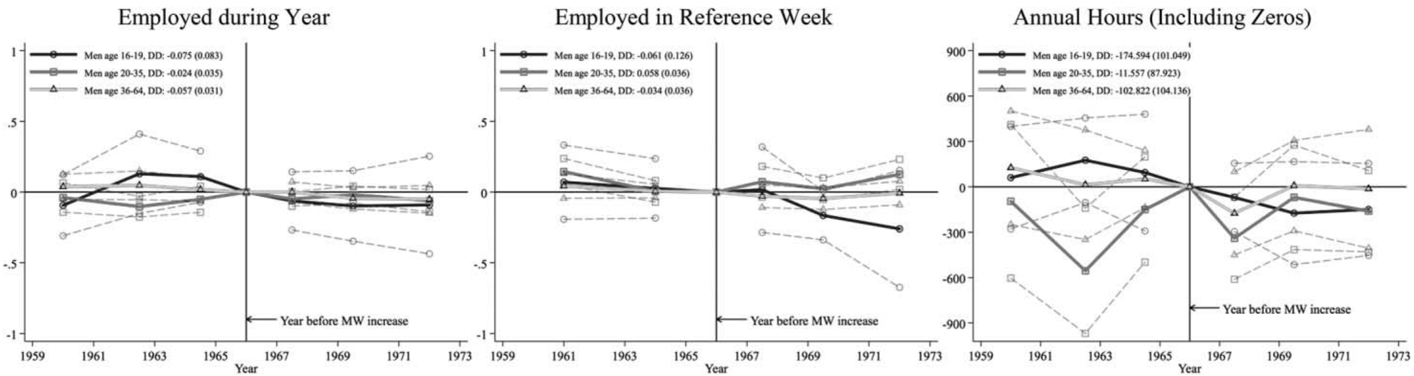


FIG. 8.—(*Continued*)

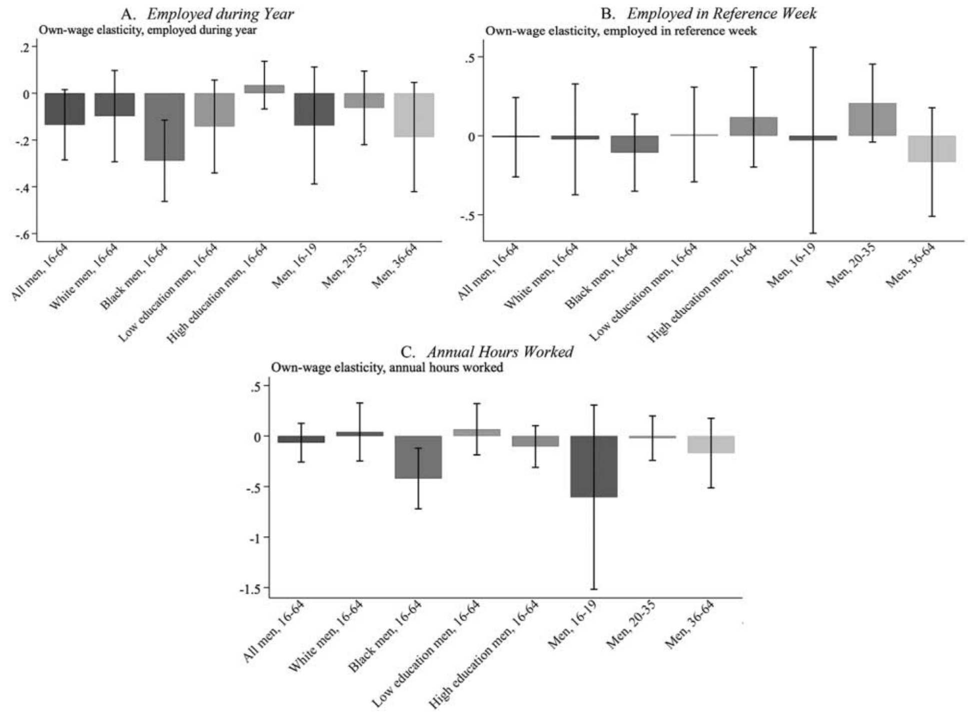


FIG. 9.—Own-wage employment elasticity in the late 1960s, by subgroup. Panel titles refer to measure of employment used in equation (4). This figure displays point estimates and 95% confidence intervals from regressions in column 3 of table 3 and columns 1–7 of table 4. A color version of this figure is available online.

Table 5
Elasticities of Employment and Annual Hours Worked with Respect to Wages, by Subgroup

	White Men (1)	African American Men (2)	Less than 12 Years Education (3)	At Least 12 Years Education (4)	Aged 16–19 (5)	Aged 20–35 (6)	Aged 36–64 (7)
A. Dependent variable: employed during year:							
Wage elasticity	−.098	−.289	−.142	.035	−.138	−.063	−.187
	(.100)	(.089)	(.101)	(.052)	(.128)	(.080)	(.119)
B. Dependent variable: employed in reference week:							
Wage elasticity	−.023	−.107	.008	.118	−.029	.207	−.166
	(.179)	(.124)	(.153)	(.161)	(.300)	(.126)	(.175)
C. Dependent variable: annual hours worked:							
Wage elasticity	.040	−.421	.067	−.104	−.606	−.021	−.169
	(.147)	(.153)	(.129)	(.105)	(.465)	(.112)	(.176)
State-year-(age group) cells	294	266	273	273	882	882	882

NOTE.—This table reports own-wage elasticities from two-stage least squares estimation of eq. (4). Panel titles refer to the dependent variable used in the employment regressions. First-stage estimates and dependent variable means are shown in table 4. See table 2 and table 4's notes for additional information on the samples and sources.

(p-value of .074), but the elasticities for employment during the year and employment in the reference week are not (p-values of .27 and .73, respectively).

Employment elasticities for teens are also larger but imprecisely estimated owing to smaller sample sizes, at −0.138 (SE: 0.128) for employment during the year, −0.029 (SE: 0.300) for employment in the reference week, and −0.606 (SE: 0.465) for annual hours worked. An important caveat to the findings for teenagers is that their wages began to rise and employment began to fall before the 1966 FLSA took effect. The largest own-wage employment elasticities for black men or young men are at the bottom of the range of the early time-series literature but are also consistent with more recent work, as shown in figure 6.

In summary, we find that the employment of African American men and perhaps younger men fell with implementation of the 1966 FLSA. Broadly speaking, the magnitude of the demand elasticities and disemployment effects suggests that—although the aggregate effects are not large—the 1966 FLSA may have had adverse consequences for some workers.

V. Conclusions

This paper examines the effects of the 1966 Amendments to the FLSA, which raised the federal minimum wage to its highest level of the twentieth century while significantly expanding the act's coverage. Using variation across states in the bite of the 1966 FLSA, we find that the amendments led to large increases in wages. Our estimates imply that wages rose 6% faster after 1966 in states such as Texas, where many more workers had wages below the new federal minimum, than in states such as New York, where fewer workers did. Extrapolating from our results suggests that, nationwide, wages increased by 6.5% on average because of the FLSA.

Notably, we estimate relatively small aggregate employment responses to this legislation. The average employment rates and annual hours worked decreased by 0.7% and 0.4% more, respectively, in lower-earning states, both statistically indistinguishable from zero. Our estimates also imply labor demand elasticities of −0.14 for employment (measured as positive weeks worked during the year) and −0.07 for annual hours worked with respect to wages. These elasticities are smaller than those implied by the early time-series literature covering the United States in the same period as our analysis (Brown 1999) but are similar to more recent estimates of very large wage increases in other countries (Harasztosi and Lindner 2019). These aggregate effects mask the incidence of the 1966 FLSA on different subgroups. For instance, substantial decreases in employment and annual hours for African American men following the 1966 FLSA suggest that large changes in the minimum wage could shift the composition of employment and harm certain groups of workers. Interestingly, the disemployment effects are concentrated on measures of employment during the year and, likely, workers with less attachment to the labor force as we consistently find little effect on employment in the March CPS reference week.

This evaluation of the 1966 FLSA offers a unique opportunity to evaluate the economic effects of a large increase in the real minimum wage to a high level for a sizable share of the economy that persisted in the short term in the face of inflation because of the large increase in coverage. Although we find disemployment effects for some groups in the economy, the magnitude of these effects appears fairly modest in magnitude. Also noteworthy is the persistence of wage effects over time, alongside relatively stable impacts on employment. Although putty-clay models in Sorkin (2015) and Aaronson et al. (2018) imply that disemployment effects would increase over time as capital-intensive firms enter, our estimates show little evidence of this. During the 1960s, the increase in the minimum wage and its coverage may have instead led to capital deepening that enhanced the output of less skilled workers as well as a reallocation of employment to more productive or less discriminatory establishments (Dustmann et al. 2020). The available data do not allow us to study the mechanisms underlying these wage and employment dynamics, which is left for future research.

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