Long-Run Determinants of US Racial Inequality: Evidence From the Great Migration and the FLSA

Long-run determinants of US racial inequality: Evidence from the Great Migration and the FLSA

A dissertation presented

by

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to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Economics

Harvard University

Cambridge, Massachusetts

May 2019

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Abstract

Racial economic divides seem a fixed feature of American society. Yet the past 75 years have witnessed important shifts in this dimension of inequality. This dissertation studies two key episodes in American economic history that have shaped current patterns of racial disparities. The first chapter examines the role of the Great Migration in the changing geography of upward mobility for black families. I show that northern cities responded endogenously to black population increases during the Great Migration, lowering the gains from growing up in destination cities and widening the racial gap in upward mobility in the region. The second chapter explores mechanisms of the Migration's effect on upward mobility. Starting in the 1960s, destination commuting zones exhibited higher white private school enrollment rates, greater investment in police services, higher urban murder rates, and increased incarceration, suggesting rising segregation and urban decline as plausible channels. The third chapter of the dissertation uncovers the role of federal minimum wage policy in the sharp decline of racial earnings gaps during the Civil Rights Era. The 1966 Fair Labor Standards Act extended federal minimum wage coverage to retail, services, agriculture, and other sectors where black workers were overrepresented. The reform increased wages for workers in newly covered industries, with twice as large an effect on black workers as on white, and with no detectable effects on employment. The 1966 extension can explain 20% of the reduction in the racial earnings gap in the late 1960s and early 1970s.

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Acknowledgments

"Economics is a toolkit. You can answer any question you want." Thank you to my mentor, Suresh Naidu, who, by uttering these words, kickstarted an intellectual journey from which I've never looked back. Leah Boustan, Nathan Nunn, and Max Kasy have provided critical support along the way.

I thank my advisors Lawrence Katz, Claudia Goldin, Nathan Hendren, and Ed Glaeser, otherwise known as the dream team. I turned to each of them for their expertise, encouragement, and advice, and this exhilarating journey could not have been completed without them. I should not omit my fifth committee member, Pika Goldin, who has diligently attended nearly every advising meeting.

I thank my mother and best friend, Ajanta Naidu, for holding me up; my father, Frantz Derenoncourt, for cheering me on; my sage brother Ravi Derenoncourt, for reminding me what's important; and my nieces Kylie and Kayla for bringing me joy.

To the family, friends, and communities that have sustained me over the last six years, thank you: Shruti Parekh, my aunts Pushpa and Madhu, Tatiana Chaterji, Niharika Singh, Heather Sarsons, Clémentine Van Effenterre, Claire Montialoux, Elizabeth Mishkin, Chenzi Xu, Sydnee Caldwell, Nathaniel Lane, Leander Heldring, James Feigenbaum, Joshua Abel, Krishna Dasaratha, Felix Owusu, Ariel Gomez, Karen Narefsky, Remeike Forbes, Sana Sheikh, and the Harvard Graduate Students Union - UAW.

I thank my partner, Spencer Strub, for unwavering love and support over the last 8 years, despite my becoming a positivist.

To Shakuntala Naidu,
whose spirit flew where she could not and in doing so, guides us

Introduction

> The story of the Negro in America is the story of America—or, more precisely, it is the story of Americans. It is not a very pretty story: the story of a people is never very pretty. The Negro in America, gloomily referred to as that shadow which lies athwart our national life, is far more than that.
>
> James Baldwin

Writing nearly three decades ago on changes in black economic status in the 20th century, John Donohue II and James Heckman concluded that "the story of black economic progress is not one of uniform secular advance, but rather of episodic change."[1] Indeed, though the economic divide between black and white America seems so entrenched as to transcend history, important shifts in the nature of this inequality have occurred over the past 75 years.

This dissertation examines two episodes in American economic history that have shaped current patterns of racial inequality. The Great Migration of African Americans from the South to the North, particularly between 1940 and 1970, and the 1966 amendments to the Fair Labor Standards Act, which extended minimum wage coverage to sectors with large shares of black workers.

A major shift in the geography of upward mobility for black families motivates the first chapter. In it, I provide evidence that responses to the Great Migration in destination cities eroded the gains to growing up in the North for subsequent generations of black children. The northern United States long served as a land of opportunity for black families, but today the region's racial gap in intergenerational mobility rivals that of the South. I show

[1] See Donohue and Heckman (1991).

that racial composition changes during the peak of the Great Migration (1940-1970) reduced upward mobility in northern cities in the long-run, with the largest effects on black men.

I identify urban black population increases during the Migration at the commuting zone level using a shift-share instrument, interacting pre-1940 black southern migrant location choices with predicted out-migration from southern counties. The Migration's negative effects on children's adult outcomes appear driven by neighborhood factors, not changes in the characteristics of the average child. I estimate that the overall change in childhood environment induced by the Great Migration explains 28% of the upward mobility gap between black and white families in the region today.

In the second chapter, I analyze the mechanisms by which destination cities declined in the wake of the Great Migration. I assembled a new database on schools, local government expenditures, and crime rates at the commuting zone level spanning nearly 100 years, from 1920-2015. Analysis of the Great Migration's impacts on these measures over time reveals that the 1960s were a turning point for destination cities. The Migration led to greater white enrollment in private schools, increased spending on policing, and higher crime and incarceration rates. These changes have persisted well into the 2000s, and the latter changes in the urban environment provide plausible candidates for disproportionate effects on black men as a subgroup.

The third chapter examines the overlooked role that the federal minimum wage played in the evolution of the racial wage gap. Racial earnings gaps fell dramatically in the late 1960s, around the same time that the 1966 Fair Labor Standards Act introduced high minimum wages in sectors where black workers were overrepresented, including agriculture, retail, and services. We show that this reform increased wages for workers in newly covered industries and that the impact was twice as large for black workers as for white. At the same time, we find no evidence of an effect of the reform on employment. The 1966 extension of the minimum wage can explain 20% of the reduction in the racial earnings gap in the United States as a whole in the late 1960s and early 1970s, thus shedding new light on the forces compressing racial inequality during the Civil Rights Era.

Chapter 1

Can you move to opportunity? Evidence from the Great Migration

1.1 Introduction

The northern United States historically offered black families a pathway to economic mobility. In 1940, black children from similar economic backgrounds fared substantially better in the North than in the South (Card *et al.*, 2018). Today, however, no such apparent advantage exists for black children growing up outside the South (Davis and Mazumder, 2018). Racial gaps in upward mobility—defined as children's adult outcomes conditional on parent economic status—are similar across the country (Chetty *et al.*, 2018).

The regional shift in black upward mobility coincided with a decisive moment in black geographic mobility. Between 1940 and 1970, four million African Americans left the South and settled in urban areas in the north and west of the country. The Great Migration, as it is known today, radically transformed the racial demographics of destination cities, prompting white flight from urban neighborhoods and potentially altering the policies of local governments (Boustan, 2010).[1] The link between these two phenomena—the migration

[1]Tabellini (2018) finds that the first wave of the Great Migration lowered city government expenditures on education.

North and declines in upward mobility—is important for assessing the stability of childhood location effects, which have been shown to be substantial in a variety of experimental and quasi-experimental contexts (Chetty *et al.*, 2016; Chetty and Hendren, 2018a,b).

This paper tests whether the Great Migration (termed "Migration") reduced northern cities' ability to facilitate black intergenerational progress. I do so by comparing commuting zones in the North that exogenously experienced larger or smaller increases in their urban black population. I estimate the impact of these increases on average outcomes for individuals born in the 1980s. I find that the Migration lowered upward mobility in the long run. Black men were the most affected subgroup, implying a widening of the racial gap in former Great Migration destinations. Using an alternative measure of upward mobility–the childhood exposure effects of commuting zones–I show that the Migration's impact was mediated by childhood environment, not unobserved family characteristics. Northern cities responded endogenously to racial composition changes in ways that reduced the gains from growing up in the North for future generations of black children.

I draw on the complete count US censuses from 1920-1940 and commuting zone level upward mobility estimates from Chetty and Hendren (2018b) and Chetty *et al.* (2018). The core sample is 130 non-southern commuting zones covering roughly 85% of the non-southern US population in 2000 and 58% of the US population overall. These locations contained 97% of the non-southern black population in 2000 and 50% of the black population overall.

The empirical strategy makes use of the fact that black migrants during the Great Migration settled where previous migrants from their communities had moved, giving rise to highly specific linkages between southern locations and northern destinations (Boustan, 2010; Black *et al.*, 2015; Stuart *et al.*, 2018). To address omitted factors that may codetermine increases in the urban black population during the Great Migration and declines in upward mobility, I use a "shift-share" approach. I combine information on pre-1940 black southern migrants' location choices with supply-side variation in county outmigration from 1940-

1970,[2] predicted from southern economic variables. As the set of these variables is large, I use a machine learning technique, Least Absolute Shrinkage and Selection Operator ("LASSO"), to optimize the set of predictors of net-migration rates from the South. Assigning inflows to cities according to historical settlement patterns yields the predicted increase in the black population from southern variation alone, which I normalize by the initial 1940 urban population.[3] Black in-migration is a right skewed distribution, so I define the Great Migration shock to a commuting zone to be the percentile of predicted black population increase.[4]

Using this strategy, I show that the Great Migration led to a reduction in upward mobility in destination commuting zones in the North today. A 30-percentile greater increase in the black population, approximately 1 standard deviation of the shock, lowered adult income rank of children from low income families by 3 percentiles, approximately a 9% drop in adult income. As a benchmark, a 1 standard deviation increase in residential racial segregation lowers adult income by about 5.2%.[5]

Two potential mechanisms underlie this effect: selection, or changes in the characteristics of the average child; and location, or changes in local public goods or neighborhood quality. To disentangle these two channels, I use data on the childhood exposure effects of commuting zones from Chetty and Hendren (2018b). These data contain estimates of each commuting zone's causal effect on children's adult outcomes today. I examine whether the causal effect of a commuting zone varies with exogenous historical increases in the black population. The interpretation is as follows: if a random child were to spend one additional year in a Great Migration city versus one less affected by the Migration, how does this affect

[2]One example is variation in the share of agricultural land planted in cotton. Cotton mechanization accelerated after World War II, contributing to black outmigration from the South (Whatley, 1985); variation in cotton acreage thus provides plausible variation in southern county migration rates.

[3]Normalizing by the initial urban population accounts for potentially different growth paths in the urban population across CZs.

[4]See Sequeira *et al.* (2019) for a similar scaling of estimated effects. The authors report the impact of percentile increases in historical European immigration on long-run economic development in US counties.

[5]See Chetty and Hendren (2018b).

his or her income as an adult? I estimate a robust negative effect of the Migration on this measure of upward mobility. My estimates suggest that the cumulative effect of spending one's entire childhood in a Great Migration city accounts for all of the negative impact of the Migration on average upward mobility. In other words, I find no evidence that negative selection of families contributes to the association between historical racial composition shocks and declines in upward mobility.

Next I explore which groups of children were affected by the Migration. I use observational estimates of upward mobility for different racial groups and show that the largest negative effects manifest for black men. I find no impact of the Migration on the household income of black women, but the impact on their individual earnings is weakly positive. The evidence is consistent with an income effect: black women who formed or would have formed households with black men increased their labor supply to make up for men's reduced income. Nonetheless, the higher individual earnings of black women do not offset overall reductions in black household income in cities that experienced greater inflows during the Migration.

A key competing explanation for these long-run declines in Great Migration cities is deindustrialization. Black southerners moved to manufacturing centers that subsequently underwent greater job loss than more economically diversified locations. In all specifications, I control for the share of the labor force in manufacturing in 1940, which largely accounts for variation in manufacturing shares in subsequent decades. Further, I find no effect of the Migration on the outcomes of white men from low income families, a group likely to have been affected if the findings were driven by deindustrialization alone. White southerners also migrated to northern cities over the 20th century. In a placebo exercise, I show that instrumenting for white southern inflows has no effect on black upward mobility or on commuting zone childhood exposure effects. Finally, the impact of black population increases is robust to flexibly controlling for lagged black population shares prior to 1940, suggesting that changes in the racial composition, not simply the levels of the black population, help explain the findings.

This paper provides evidence that responses to the Great Migration in destination cities eroded some of the gains of migrating for future generations of black children. The effects were particularly deleterious for black boys. A counterfactual exercise suggests that without the causal impact of the Great Migration, the racial gap in upward mobility among men in the North would be roughly 43% smaller.[6] An important component of the relationship between the Great Migration and intergenerational mobility that this paper does not speak to, however, is the causal effect of the Migration on the descendants of migrants themselves. The best estimates suggest that moving North nearly doubled the wages of migrants compared to those who stayed behind in the South (Boustan, 2016). Thus the children and grandchildren of migrants living in the North likely benefited from their parents and grandparents moving up in the national income distribution. Losses incurred through northern cities' responses to the Migration must be placed in context with overall improvements in black economic status from moving North.

This paper relates to several literatures. First, a large literature seeks to identify neighborhood effects and the impact of residential segregation and urban poverty on children's outcomes.[7] More recently, both experimental and quasi-experimental studies have shown childhood location to be an important determinant of adult outcomes and that substantial variation in these effects exists across the US (Chetty *et al.*, 2016, 2014; Chetty and Hendren, 2018a,b). Although the correlates of location effects and differences in upward mobility have been well documented, the stability of these effects in response to shocks is much less understood. I provide novel evidence that mid-century racial composition shocks altered the effects locations had on children, turning high opportunity locations into opportunity deserts, particularly for black families.

Second, I contribute to the literature on the effects of the Great Migration on destination cities. Boustan (2010, 2009) showed that black in-migration spurred post-war white flight

[6]Defined as the commuting zone level gap in mean income rank between black and white men with median income parents.

[7]For literature on this topic, see Ananat (2011); Andrews *et al.* (2017); Cutler and Glaeser (1997); Kasy (2015); Massey and Denton (1993); Graham (2016); Sampson *et al.* (2002); Wilson (2012).

into suburban neighborhoods and increased labor market competition among black workers in the North. Papers focusing on the earlier period of the Migration (1910-1930) have shown that the Migration increased residential racial segregation (Shertzer and Walsh, 2016), lowered city government expenditures (Tabellini, 2018), and aided the assimilation of European immigrants (Foukas *et al.*, 2018). In this paper, I provide evidence of long-run effects of the Great Migration on upward mobility and shed light on a new intermediate impact on cities: higher crime and incarceration rates and greater relative investment of public expenditures in policing.[8]

1.2 Historical background

> My mother was my inspiration... she was one of those 6,000,000 black people who left the South so that her children wouldn't have to grow up and put up with what she had to grow up and put up with.
>
> Helen Singleton, Civil Rights activist from Los Angeles

Starting in the 1910s, black Americans migrated in large numbers from southern states to northern states, a phenomenon known as the Great Migration.[9] By the middle of the 20th century, the Migration was so great that the share of the black population in the South fell to just over 50% by 1970, from 90% in 1910.

Under Jim Crow laws in the South, black Americans faced significant limitations on their political, social, and economic freedoms. Declining labor demand in southern agriculture gradually loosened the largely rural black population's ties to the land. Further, job opportunities for black workers opened up in many northern cities. As a result of these

[8]Two studies examine the effects of the first wave of the Great Migration on incarceration.Muller (2012) finds that the Migration increased racial disparities in incarceration in the North, and Eriksson (2018) shows that migrating North increased black men's likelihood of incarceration.

[9]For a comprehensive study of the Great Migration and its contemporaneous economic impacts on destination cities, see Boustan (2016).

changes, black migrants increasingly undertook the journey North.[10] In doing so, they sought better lives for themselves and their children, and for many decades, the North appeared to deliver on this promise.

Helen Singleton, daughter of a migrant and later an activist in the Civil Rights Movement, recalled her surprise hearing about *Brown v. Board of Education*, the US Supreme Court ruling that rendered segregated schooling unconstitutional. Having attended high school in Los Angeles, California, the concept of a segregated school was foreign to her. By contrast, for many black children in the South, even those from educated families, the paucity of public black high schools made secondary schooling very costly (Margo, 1990, 1991). Singleton's experience was reflected more broadly in educational patterns for black children across the US in 1940.

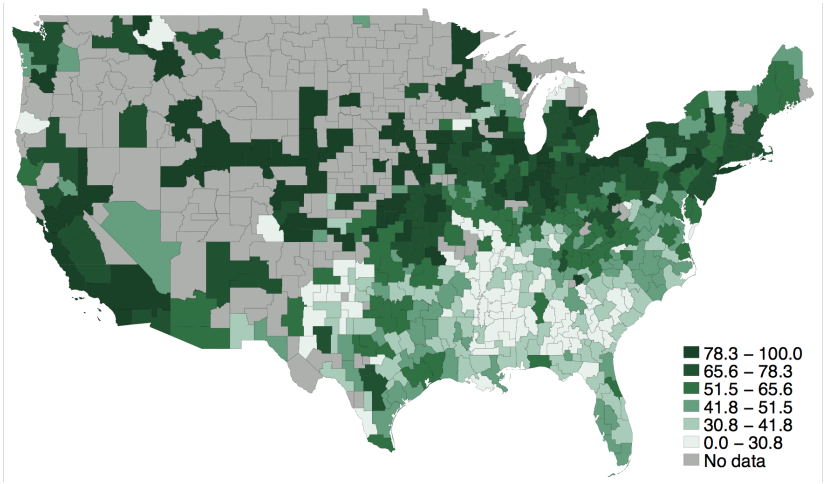
Figure 1.1 illustrates geographic patterns in upward mobility for black families historically and today. Panel A shows the fraction of black teenagers from median-educated households who obtained 9 or more years of schooling. The map illustrates stark differences in upward mobility for black children in the North compared to the South. Furthermore, racial gaps in teen school attendance were much lower in the North. In Appendix A1, I document that this regional difference remained fairly constant over the period 1880-1940. A major shift in the geography of upward mobility for black Americans appears to have taken place in the decades after 1940.

Panel B illustrates the current geographic distribution of black upward mobility in the US. Illustrated in the map is average income rank for black men and women who grew up in low income families in each commuting zone in the 2000s. Several northern locations that exhibited high outcomes for black children in 1940 exhibit some of the worst outcomes for black children today. The fact that the peak of the Great Migration took place in between motivates an empirical investigation of the Migration's role in declining black upward

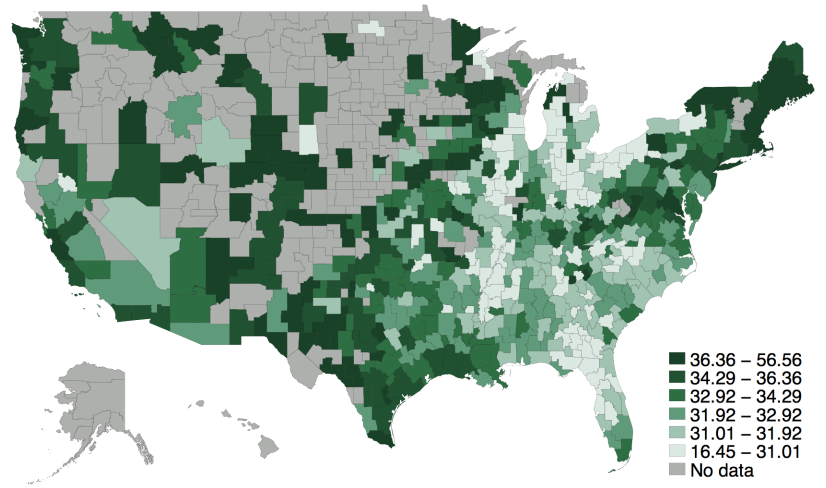
[10]See Whatley (1985); Collins (1997); Hornbeck and Naidu (2014) for further discussion of the economic and political determinants of the Great Migration. For example, Collins (1997) shows how northern industrialists' hiring and recruiting black workers hinged on reduced presence of and access to European immigrant labor during World War I and immigration controls put in place in the 1920s.

mobility in the North.

FIGURE 1.1: *Black Upward Mobility in 1940 and 2015*



(a) Fraction of black teens from median educated families with 9-plus years of schooling, 1940



(b) Household income rank of black men and women from below median income families, 2015

Notes: This figure depicts the geographic patterns in black upward mobility in 1940 and 2015. Panel (a) depicts black educational upward mobility in 1940 defined as fraction of 14-17 year-old boys and girls who have at least 9 years of schooling, from households where the household head has between 5 and 8 years of schooling. Panel (b) shows expected mean household income rank in 2015 by childhood commuting zone for 1978-1983 birth cohorts of black men and women from families at the 25th percentile of the parent income distribution. Darker shades indicate commuting zones with higher levels of upward mobility. Data sources: IPUMS 1940 complete count census for panel (a) and Chetty, Hendren, Jones, and Porter (2018) for panel (b).

1.3 Data

In this section I describe the data used to measure upward mobility, the construction of the analysis sample of commuting zones, and my measure of urban black population change during the Great Migration. I conclude the section by discussing key correlates of the Migration and upward mobility for the commuting zones in my sample. Throughout, I define upward mobility in a location as the average outcomes of children conditional on parent income or educational status.

1.3.1 Upward mobility

Educational upward mobility in 1940

To measure upward mobility in commuting zones prior to the 1940-1970 wave of the Great Migration, I use the complete count 1940 census.[11] Following Card *et al.* (2018), I define educational upward mobility as the fraction of 14 to 17 year-olds in each commuting zone with 9 or more years of schooling from households where the household head has between 5 and 8 years of schooling, approximately the median for adults in the US at the time.[12] In addition, I use complete count censuses from 1920 and 1930 to develop pre-1940 measures of educational upward mobility, specifically, the school attendance rates of teenagers with low occupation score fathers.

Teenagers typically reside in the same households as their parents, obviating the need to match them across censuses to observe parent economic status. At the same time, teenagers are old enough that their educational attainment is likely predictive of their adult educational attainment and future labor market outcomes. Observing outcomes for the near universe of enumerated teenagers reduces the scope for sampling bias in constructing upward mobility measures at fine geographies. Finally, teenager upward mobility can be

[11] The Integrated Public Use Microdata Series ("IPUMS") version.

[12] I use the household head's years of education as the measure for parent educational attainment while Card *et al.* (2018) use the maximum of father and mother educational attainment.

constructed separately by race without differential selection bias across groups arising from lower match rates for African Americans.[13]

Income upward mobility for 1980s birth cohorts

For contemporary measures of upward mobility in commuting zones, I use data made available by Chetty and Hendren (2018b) and Chetty *et al.* (2018). Based on the universe of federal income tax records from 1996-2012, the data contain measures of income upward mobility by childhood commuting zone for individuals born between 1980 and 1986. Parent and children were linked via dependent claiming. The key measure of upward mobility is estimated mean individual or household income rank, conditional on parent household income rank.[14]

Income for individuals in the sample is income at age 26, during the years 2006-2012, and income rank is rank in the national income distribution for individuals from the same birth cohort. Parent income is measured using returns filed when individuals were between the ages of 14 and 20, and parent income rank is rank in the national parent income distribution by child birth cohort. Separate upward mobility estimates are available for individuals from the 25th and 75th percentile of the parent income distribution. Estimates are also available separately by gender.

How comparable are educational upward mobility in 1940 and income upward mobility in the 2000s? The two measures are strongly correlated across US CZs, where both are available, with a correlation coefficient is 0.43. Additionally, income upward mobility is strongly correlated with high school graduation rates in low income families today, with a correlation coefficient of 0.53.

[13]Matching methods, which typically rely on first and last name to link individuals across historical censuses, are not well suited to linking African Americans who have fewer unique surnames as a result of slavery.

[14]Household income measures for parents and children are drawn from Adjusted Gross Income on 1040 tax returns, and individual income rank is measured using income reported on W-2 forms, UI and SSDI benefits, and half of household self-employment income where relevant.

Childhood exposure effects of commuting zones

I use an alternative measure of upward mobility in the 2000s from Chetty and Hendren (2018b): the childhood exposure effects of commuting zones. Starting from the universe of tax filers described above, the authors restricted the sample to individuals whose parents moved once across commuting zones during their childhood. They then compare the outcomes of children exposed for more or less time to a given commuting zone based on children's ages at the time their families moved. Precisely, the data contain estimates of the causal effect of one additional year of childhood in a given commuting zone relative to an average commuting zone, for an arbitrary child. The outcome of interest is adult income rank at age 26. The estimates and assumptions behind them are discussed in greater detail in Section 1.5.2.

Race-specific measures of upward mobility

Race-specific measures of upward mobility come from Chetty *et al.* (2018). These data are based on the same universe of federal income tax records as the measure described above; however, they cover a slightly different set of birth cohorts: 1978-1983. Individual federal income tax records were linked to the US Census in order to retrieve information on race as well as additional outcomes measured by the Census. The data contain the estimated mean individual or household income rank, conditional on parent household income rank, of black and white men and women at the 25th and the 75th percentile of the parent income distribution by childhood commuting zone. In this dataset, outcomes are measured in 2015 when individuals are between the ages of 32 and 37.

1.3.2 City demographic data, 1940-1970

I draw on two main sources of data to construct historical black population measures for cities in northern commuting zones in 1940 and 1970: the complete count 1940 census and

the City and County Data Books 1944-1977 series ("CCDB")[15], which contains information on cities with a population of 25,000 or more. I measure urban black populations in 1940 using the complete count census, as the CCDB only report information on the number of whites and non-whites in cities that year. I use the CCDB to collect information on the black population in cities in 1970.[16] I restrict the sample to cities that are not missing population data in 1940 in the CCDB. I further restrict the dataset to those cities that had at least one recent black southern migrant, defined as an individual who listed a southern county of residence in 1935, but resided in a northern city in 1940. The total number of cities that meet these criteria is 294.[17] My final sample of commuting zones is the 130 commuting zones containing these cities.

I define black population change in a commuting zone during the Great Migration as the 1940 to 1970 increases in the urban black population as a share of the initial 1940 urban population:

$$\Delta\text{Black pop}_{CZ}^{1940-1970} = \frac{b_{\text{urban},CZ}^{1970} - b_{\text{urban},CZ}^{1940}}{\text{pop}_{\text{urban},CZ}^{1940}} \tag{1.1}$$

where $b_{\text{urban},CZ}^{t}$ is the total black population in all sample cities in commuting zone CZ in year t.

Functional form Because the distribution of black population increases is highly right skewed, I define the quantile function GM_{CZ}, or the percentile of the increase, to be the key independent variable in the empirical analysis.

Figure 1.2 depicts GM_{CZ} across northern commuting zones during the Great Migration.

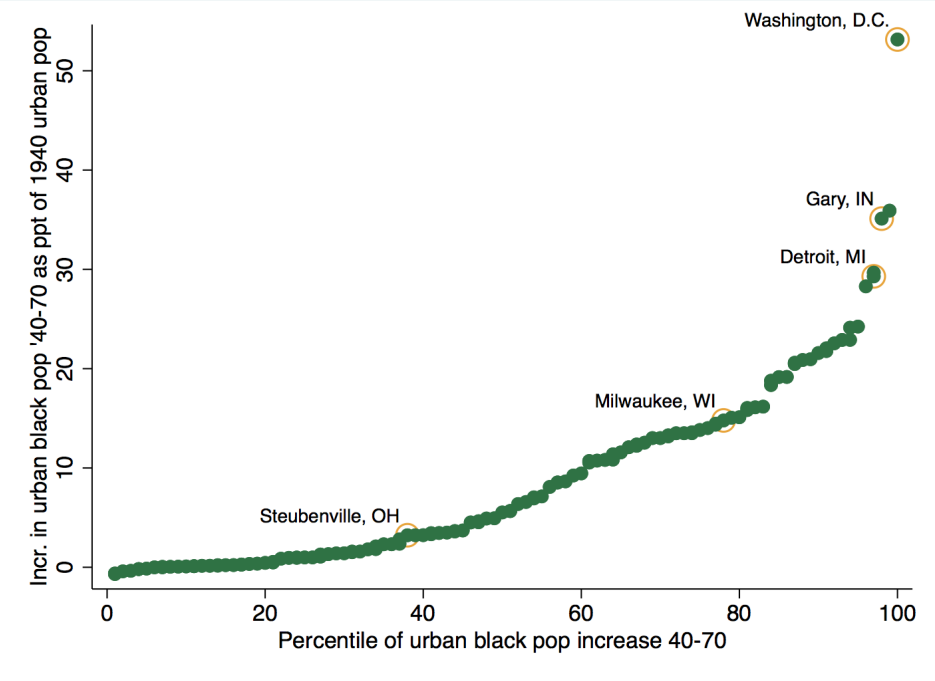
[15] Available from the Inter-university Consortium for Political and Social Research ("ICPSR").

[16] I'm unable to locate the following cities from the CCDB in the 1940 census: Boise City, ID; East Providence, RI; Huntington Park, CA; West Haven, CT; and Warwick, RI. I drop these cities from the analysis due to missing data.

[17] I manually record black population data for two cities in the published 1940 US census: Butte, MT and Amsterdam, NY. Both cities received black southern migrants between 1935 and 1940, but data on their black population in 1970 was not available in the CCDB. Including these two cities brings the total number of commuting zones in the sample to 130 from 128. Finally, the city of New Albany, IN is in the Louisville, KY commuting zone, which is included in the sample. Results are robust to excluding this commuting zone.

Plotted on the y-axis is the measure in equation 1.1, multiplied by 100 so that the units are percentage points. The x-axis measures GM_{CZ}, the quantile function or the percentile of urban black population increase.

FIGURE 1.2: *Quantiles of urban black share increases, 1940-70*



Notes: This figure plots the quantile function of 1940-1970 increases in the urban black population in commuting zones as a share of the total initial 1940 urban population and multiplied by 100, so that the units are in percentage points. The CZs in sample are those containing the 294 non-southern mainland cities with information on the black population in both 1940 and 1970 from the City and County Data Books, 1944-1977. ''Non-southern" mainland excludes cities in the following states: Alabama, Alaska, Arkansas, Florida, Georgia, Hawaii, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. Note, Washington, D.C. and cities in Delaware and Maryland were net-receivers of black migrants during the Great Migration and are included in the sample. The city of New Albany, IN is in the Louisville, KY commuting zone, which is included in the sample. Results are robust to excluding this commuting zone. Data sources: CCDB.

The median increase across commuting zones in the sample was 5.5 percentage points. As the figure demonstrates, however, historical black share increases were very unevenly distributed across the North, even among commuting zones in the same region. Take for example, two commuting zones in the midwest—Steubenville, OH and Milwaukee, WI. Both were major manufacturing centers in the 1940s. Steubenville's urban black population

share increased by 3.2 percentage points (38th percentile) while Milwaukee's increased by 14.8 percentage points (78th percentile). At the tail end, commuting zones like Detroit, MI, Gary, IN, and Washington, DC, saw very large increases, ranging between 30-50 percentage points.

The descriptive relationship between black population change during the Great Migration and average income upward mobility today can be seen in Figure **??**. The relationship is strikingly negative and linear.[18] A 1-percentile greater black population increase between 1940 and 1970 is associated with a .15 percentiles reduction in adult income rank for individuals with lower income parents. However, this relationship cannot be interpreted as causal as correlates of black population change may drive this relationship.

1.3.3 Descriptive characteristics of Great Migration CZs

Why did urban black populations in the North increase so dramatically between 1940 and 1970? After a period of reduced mobility during the Great Depression, black outmigration from the South resumed at an accelerated pace after 1940. War-time jobs in the defense industry and in naval shipyards led to substantial black migration to California and other Pacific states for the first time since the Migration began. Migration continued apace to midwestern cities in the 1950s and 1960s, as the booming automobile industry attracted millions more black southerners to the North, particularly to cities like Detroit or Cleveland. Of the six million black migrants who left the South during the Great Migration, four million of them migrated between 1940 and 1970 alone.[19]

As is clear from the discussion above, mid-century economic conditions in northern cities influenced where migrants moved and are thus likely correlated with increases in the

[18]The linearity of the relationship suggests that very large increases in the black population share at the tail end of the distribution in Figure 1.2 had similar effects as smaller increases at the bottom and middle of the distribution. This may in part be due to the positive relationship between levels of the black population share and the changes in the black population between 1940 and 1970. Smaller increases take place in a context of lower levels of the black population share and therefore may also have a large impact. As I discuss in Section 1.5, my results are robust to including controls for the level of the black population share in 1940.

[19]After 1970, black migration reversed course, with individuals on net relocating to the South, though in much smaller numbers than the migration North.

black population during this period. These underlying characteristics may also determine the dynamics of upward mobility in destination cities. Figure **??** shows the correlation between the percentile of black population increase during the Great Migration and several baseline 1940 characteristics: educational upward mobility, the share of the labor force in manufacturing, and the share of the population made up of recent black southern migrants.[20]

Black urban populations increased more in places with higher levels of educational upward mobility, a greater share of the labor force in manufacturing, and in locations that already had a substantial number black southern migrants. Given that these destination-level factors may influence both black population increases and future levels of upward mobility, I construct an instrument for the former that is plausibly exogenous with respect to such pre-1940 destination characteristics.

1.4 Empirical Strategy

The intuition behind the empirical strategy is well captured by the migration histories of Detroit and Baltimore and depicted in Figure 1.3. Both were major destinations for black migrants during the Great Migration, and both were major industrial centers in 1940. However, black migrants arriving in these locations in 1940 came from parts of the South that experienced very different patterns of outmigration between 1940 and 1970. Panel B depicts variation in black migration for these two cities. Detroit drew the plurality of its migrants from Alabama while Baltimore drew the plurality from Virginia. Migrants from Alabama tended to come from counties specialized in cotton production, and negative shocks to cotton spurred outmigration from these areas. Virginia, by contrast, was a major recipient of war production spending during World War II. War production jobs attracted

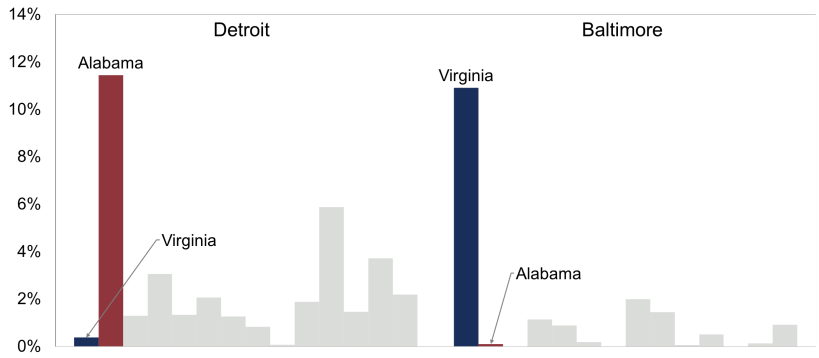
[20]Data on recent black southern migrants come from the 1940 complete count census. I define a recent southern black migrant as those who reported a southern county of residence in 1935 and lived in an northern city as of 1940. Here, southern is defined as being from the following states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

black workers and consequently lowered outmigration rates.

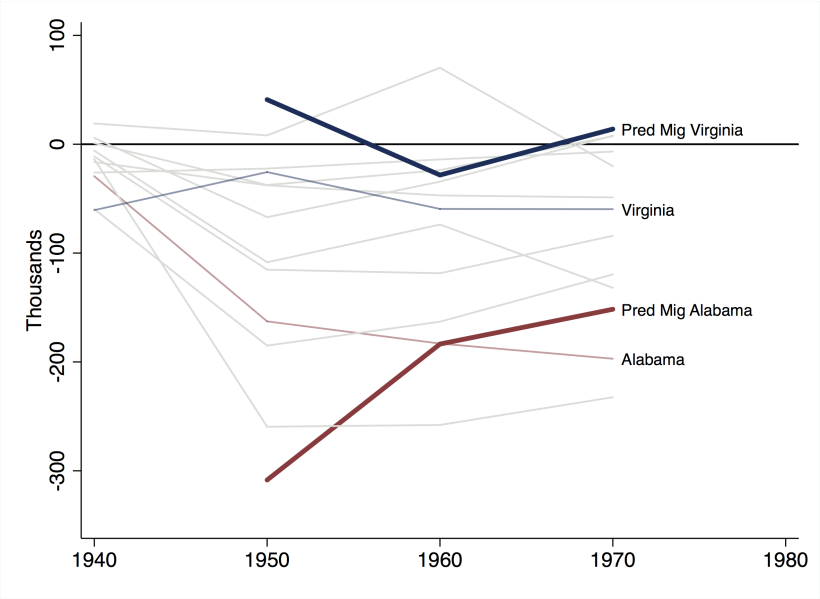
The empirical strategy generalizes from the example above by building on a standard shift-share approach for the local labor market impacts of migration (Altonji and Card, 1991). The technique was first adapted to the Great Migration context by Boustan (2010). Black southern migrants tended to move where previous migrants from their communities had settled, thus generating chain migration patterns similar to those observed in the international migration context. The resulting variation in migrant composition is plausibly orthogonal to characteristics of destinations that influence the location choices of future migrants as well as the evolution of upward mobility in destination locations. I evaluate potential violations of this assumption in detail in Section 1.5.4.

Variation in pre-1940 migrant composition can then be interacted with variation in outmigration from origin locations driven by origin factors alone ("push factors"). Push factors include war spending and shocks to cotton as well as other economic sectors in the South, for example, tobacco and mining.

FIGURE 1.3: *Shift-share Instrument for Great Migration*



(a) Composition of recent 1935-1940 black southern migrants in Detroit vs. Baltimore



(b) Southern state net-migration, 1910-1970

Notes: This figure depicts the variation underlying the shift-share instrument for urban black population change in northern commuting zones in the case of Detroit and Baltimore. Panel (a) shows the share of recent black southern migrants (those who migrated between 1935 and 1940) living in Detroit and Baltimore in 1940 by 1935 county, for the largest sending county in each southern state. For Alabama and Virginia, these are Jefferson County (Birmingham) and Richmond City County, respectively. Detroit receives the plurality of its migrants from Alabama while Baltimore receives the plurality from Virginia. Panel (b) shows net-migration and predicted net-migration for southern states each decade from 1910-1970, with net-migration figures for Alabama and Virginia highlighted. Negative numbers indicate outmigration. In darker lines are net-migration figures predicted using one-decade lagged southern county agricultural and World War II spending measures. Appendix A3 describes the construction of the instrument based on this variation. I use LASSO to select predictors each decade, interacting predicted migration with share of recent black southern migrants from each county, and summing up over all southern counties to construct counterfactual increases in the urban black population from 1940-1970 using variation in black southern migration alone. Data sources: IPUMS complete count US census for 1940; Boustan (2016) and Foukas et al. (2018).

Building off of the approach described above, I define the following instrument to analyze the long-run impact of the Migration on upward mobility: the percentile of black population change if the black population were to increase solely through the interaction of the two sources of variation described above. More precisely, I replace the numerator in Equation 1.1 with the predicted, as opposed to actual, increase in the black population:

$$\text{Predicted black pop}_{CZ}^{1940-1970} = \frac{\hat{\Delta b}_{\text{urban},CZ}^{1940-1970}}{\text{pop}_{\text{urban},CZ}^{1940}} \tag{1.2}$$

where $\hat{\Delta b}_{\text{urban},CZ}^{1940-1970}$ denotes the predicted increase, which I define as follows:

$$\hat{\Delta b}_{\text{urban},CZ}^{1940-1970} = \sum_{j \in S} \sum_{c \in CZ} \omega_{jc}^{1935-1940} \times \hat{m}_j^{1940-1970}. \tag{1.3}$$

The term $\hat{m}_j$ is predicted black migration from southern county j over the decades 1940 to 1970; ω_{jc} is the share of recently migrated pre-1940 black southern migrants from county j living in city c in 1940.[21] The term $\hat{m}_j^{1940-1970}$ consists of the sum of fitted values of decadal predictions of southern county net migration rates (from 1940-1950, 1950-1960, and 1960-1970) using lagged southern economic predictors of migration:

$$\hat{m}_j^{1940-1970} = \sum_{t=1950}^{1970} \hat{m}_{jt}$$

where fitted values, $\hat{m}_{jt} = m_{jt} - \varepsilon_{jt}$, come from the following prediction of net-migration:

$$m_{jt} = \beta_0 + \mathbb{Z}'_{jt-10}\beta_1 + \varepsilon_{jt}.$$

Appendix A3 describes the construction of ω_{jc} and $\hat{m}_j^{1940-1970}$, and the procedure for choosing predictors $\mathbb{Z}'_{jt-10}$ in detail.

Functional form My instrument for the percentile of black population increase during the Great Migration, GM_{CZ}, is $G\hat{M}_{CZ}$, the percentile of the predicted black population

[21] Recently migrated black southern migrants in 1940 are black individuals who reported a southern county of residence in 1935 and were enumerated in a different county of residence in 1940. The 1940 census was the first census to record internal migration in this way. Enumerators asked individuals about their prior residence (city, county, and state) in 1935.

increase defined above. I use the percentile of the predicted increase as the key independent variable as the distribution of predicted black population increases mirrors that of the actual increases—both are heavily right skewed. In reporting the effects of percentile changes in the black population, I follow Sequeira *et al.* (2019) who report the impact of a zero to 50th percentile increase in European immigration during the Age of Mass Migration on the long-run economic development of US counties.

My empirical strategy builds off the identification strategy developed by Boustan (2010) to estimate impacts of the Great Migration on destination cities. There are two differences worth noting between my approach and the one developed by Boustan (2010). Boustan (2010) uses southern state of residence in 1935 to assign future waves of migrants from southern states to northern cities. I use the complete count 1940 census, which contains microdata on the universe of recent black southern migrants into northern cities, including their county of residence in 1935.[22] Using county of residence in 1935 and city of residence in 1940, I construct a matrix of southern-county-to-northern-city linkages containing the share of each southern county's outmigrants who settled in each northern city.

Because there are over 1200 southern counties from which migrants moved North, this approach yields highly specific linkages between southern and northern locations. Using the microdata to construct the migration matrix also gives me the flexibility to explore alternative specifications of the instrument using age, gender, and demographic characteristics of the individual migrants. As I discuss further in Section 1.5.4, I construct several different instruments for black population change and conduct overidentification tests, which fail to reject the null that the estimates of the impact of the Great Migration are statistically indistinguishable from one another.[23] As I discuss in the same section, the results are also robust to defining migration shares using southern state of birth in 1940, 1930, or 1920.

The second difference is that I use machine learning to improve the prediction of net

[22]Boustan (2010) uses tabulated census reports that report 1935 state of residence to construct southern-state-to-northern-city weights to assign migrants. The complete count 1940 census was declassified in 2012.

[23]Hornbeck and Moretti (2018) performs overidentification tests using several instruments for productivity changes across cities.

migration from southern counties. The motivation for this approach is that the set of potential predictors from southern county variables is large. Given that the first stage prediction of an endogenous variable by an instrument can be viewed as a pure prediction problem (Belloni *et al.*, 2011), I select among the predictors for migration used by Boustan (2010) using a Post-LASSO estimation procedure. In this procedure, for each decade of migration between 1940 and 1970, I use LASSO to select predictors among county characteristics in the previous decade with a penalty on the absolute number of predictors, where the tuning parameter has been chosen by 5-fold cross-validation. I then use the variables chosen by this procedure to estimate the relationship between these variables and county net migration rates using OLS.[24]

Although the instrument alleviates some concerns regarding the endogeneity of black population increases in northern urban areas, the shares themselves reflect migration patterns during the first wave of the Great Migration and may themselves be endogenous to characteristics of destinations that affect the course of upward mobility in subsequent decades. I therefore include controls for the baseline 1940 characteristics discussed in Section 1.3, including the share of the labor force in manufacturing and the share of the 1940 city population made up of recent black southern migrants.

I also include educational upward mobility in 1940 as a control in all baseline regressions. As I describe in greater detail in Section 1.4, these regressions can be interpreted as estimating the effect of historical change in the black population on the change in upward mobility in the sample commuting zones, where I allow for dynamics in upward mobility. If upward mobility changed in the treated commuting zones for reasons other than the Great Migration, forcing the coefficient on historical upward mobility to be 1 may be a mis-specification of the true relationship between the Migration and upward mobility. My main results are robust to alternative specifications where I estimate the impact of the Great Migration directly on the change in upward mobility for black Americans between 1940 and 2015.

My preferred specification includes baseline 1940 characteristics for robustness; however,

[24]Southern county-net migration rates are taken from Boustan (2016).

their inclusion does not significantly alter the point estimates. I report key results with and without this baseline set of controls. Finally, all specifications include census division fixed effects to control for systematic regional differences in migration patterns and upward mobility.[25]

Estimating equation

I estimate the relationship between the Great Migration and upward mobility using the following empirical framework:

$$\bar{y}_{p,CZ} = \alpha + \beta GM_{CZ} + \mathbb{X}'_{CZ}\Gamma + \varepsilon_{CZ} \tag{1.4}$$

$$\text{First Stage: } GM_{CZ} = \gamma + \delta \hat{GM}_{CZ} + \mathbb{X}'_{CZ}\mu + \epsilon_{CZ} \tag{1.5}$$

In equation 1.4, the coefficient β represents the OLS estimate of the effect of GM_{CZ}, the percentile of a commuting zone's 1940-1970 black population increase, on $\bar{y}_{p,CZ}$, the average adult income rank of children with parents at income rank p, conditional on baseline characteristics and census division fixed effects represented by the control vector X_{CZ}. Equation 1.5 estimates the first stage relationship between the percentile of predicted black population change $\hat{GM}_{CZ}$ and the percentile of actual black population change, GM_{CZ}. The reduced form effect of my instrument for the Great Migration on upward mobility can be written as follows:

$$\bar{y}_{p,CZ} = \tilde{\alpha} + \tilde{\beta} \hat{GM}_{CZ} + \mathbb{X}'_{CZ}\tilde{\Gamma} + \tilde{\varepsilon}_{CZ} \tag{1.6}$$

[25]Including census division fixed effects leads to more precise and larger IV estimates of the impact of the Great Migration on upward mobility. However, the point estimate without controls is not statistically different from the point estimate with census division fixed effects or with the full set of baseline controls. See columns 1, 2, and 3 of Table 1.10. A potential reason for the difference in the point estimates between columns 1 and 2 is that the instrument for black population increases leverages linkages between southern origin locations and northern destinations made between 1935 and 1940. Relatively few black southern migrants had settled in the West by 1940, thus, relative to the endogenous variable, the instrument reallocates migrants towards the Midwest as opposed to the West. It would be ideal to use the 1950 census to establish the migrant network for the West as many African Americans moved west for the first time during World War II. The required micro data from the 1950 census will be available in 2022. Given these data constraints, inclusion of census division fixed effects reduces the noise introduced by the pre-1940 migrant networks.

where $\tilde{\beta}$ represents the reduced form impact of the percentile of predicted black population change on upward mobility. For all main results, I report the estimated OLS (β), reduced form ($\tilde{\beta}$), and two-stage least squares ($\frac{\tilde{\beta}}{\delta}$) coefficients.

Identifying assumption

In order for the approach above to identify the causal impact of the Great Migration, then conditional on the specified baseline 1940 characteristics, my instrument for black population increases must be orthogonal to omitted characteristics that are correlated with changes in upward mobility after 1940.

$$\mathbb{E}[G\hat{M}_{CZ} \cdot \tilde{\varepsilon}_{CZ} | \mathbb{X}_{CZ}] = 0 \tag{1.7}$$

Although this identifying assumption cannot be directly tested, I show that using this empirical strategy, the Great Migration has no effect on pre-1940 measures of educational upward mobility, defined as the school attendance rate of teens with low occupation score fathers, or on median adult educational attainment in 1940.[26]

Table 1.1 reports the results from these placebo checks. The results show that conditional on baseline controls, the instrument for the Great Migration is uncorrelated with educational upward mobility prior to 1940. The coefficients on $G\hat{M}$ are very small in magnitude, statistically insignificant, and similar across the decades 1920 to 1940. The Migration also does not predict any differences in adult median educational attainment in 1940.

[26]Defined as the population-weighted average median educational attainment of adults by county.

TABLE 1.1: *Placebo test of identification strategy using pre-1940 upward mobility and educational attainment*

	School Attendance			Median education
	1920	1930	1940	1940
GM Shock	-0.006	-0.007	0.005	-0.011
	(0.023)	(0.024)	(0.011)	(0.009)
Baseline mean	65.477	74.912	80.676	27.355
Std Dev	7.425	8.674	5.710	2.863
Observations	130	130	130	130
Baseline Controls	Y	Y	Y	Y

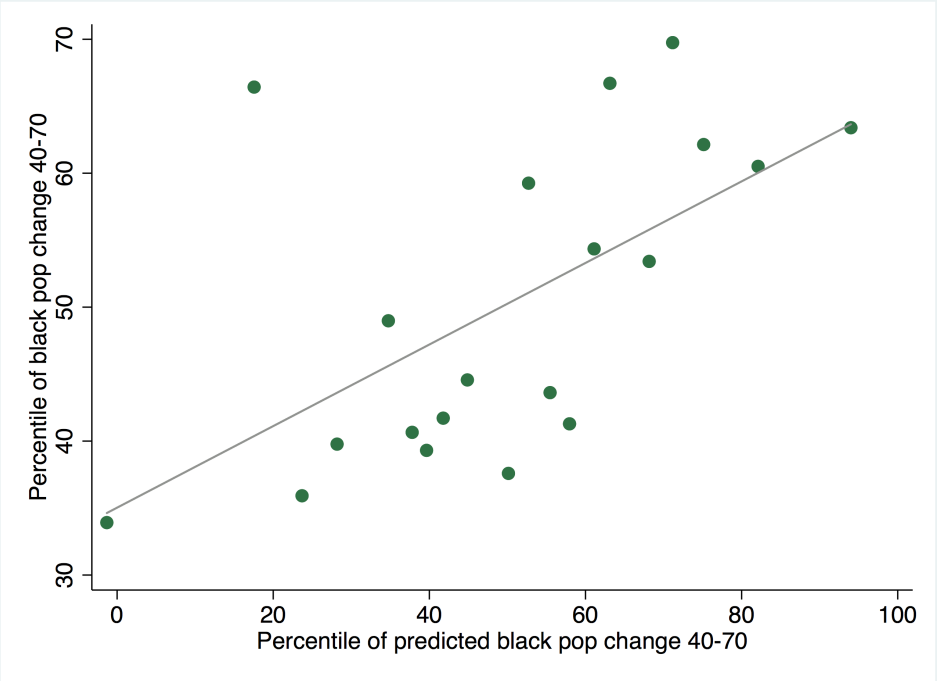
Notes: This table reports the effect of the Great Migration on pre-1940 educational upward mobility and attainment. In columns 1 through 3, the dependent variable is the school attendance rate of 14-17 year-old boys and girls with below-median occupation score fathers in 1920, 1930, and 1940, respectively. In column 4 the dependent variable is median education attainment of adults aged 25 and older in 1940. Independent variable is the percentile of black population increase during the Great Migration. The instrument for black population increase is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: IPUMS complete count US census for 1940; Boustan (2016).

First-stage results

Figure 1.4 shows a binned scatterplot of the relationship between GM, the percentile of actual black population increase, and $\hat{GM}$, the percentile of predicted black population increase, where both measures have been residualized on census division fixed effects and the set of 1940 baseline controls: educational upward mobility, the share of the labor force in manufacturing, and the share of the 1940 urban population made up of recent southern black migrants from any southern county. The y-axis plots mean percentile of black population change within each 5-percentile bin of predicted black population change. The slope of the regression line is equivalent to the coefficient $\hat{\delta}$ from equation 1.5. A one-percentile larger

predicted black population increase is associated with a 0.3 percentile greater actual black population increase over the time period. The F-statistic on the first stage is 23.[27]

FIGURE 1.4: *First Stage on Black Population Change*



Notes: This binned scatterplot depicts the relationship between the percentile of actual black population increase during the Great Migration (1940 to 1970) for northern commuting zones and the instrument for black population increase over the same period. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. The unit of observation is a commuting zone. The right hand side variable is grouped into 20 bins (5 percentiles each). Both the left hand and right hand side variables have been residualized on the set of baseline 1940 controls, including share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: IPUMS complete count US census for 1940; Boustan (2016).

[27] In Appendix Figure A9, I also show the first stage with respect to the level of black population increase, normalized by initial 1940 CZ urban population. Panel B shows this increase for a windsorized sample. The levels of predicted black population increases are shifted down relative to the actual. This is due to using a relatively low period of migration (1935-1940) to form the migration matrix generating predicted inflows into cities in the sample.

1.5 Results on upward mobility

The Great Migration represented a large-scale movement to opportunity for black Americans. In the North, jobs were far better paying, black children could attend high school, and racial equality was taken for granted in many facets of northern life.[28] From the vantage point of 1940, there was every reason to believe future generations of black children would continue to reap the benefits of their parents and grandparents having migrated. The results from the empirical analysis in this paper suggests otherwise.

Many of the locations where black migrants moved in large numbers are now among the worst places to grow up, in stark contrast with geographic patterns in upward mobility in the northern US in 1940. I show that this transformation appears causally related to the Migration. Using exogenous variation in where the black population increased the most during the period of the Migration, I find that mid-century shocks to the racial composition of northern cities lowered the average outcomes of children growing up in the 1990s and 2000s. The driver of this effect appears to be changes in location characteristics, not shifts in the composition of families living in Great Migration cities, which could mechanically give rise to lower average upward mobility. Analysis of which groups of children were affected by these changes suggest that black men were the most negatively affected sub-group. This section describes the results on upward mobility in detail, before investigating plausible local mechanisms in Section 2.1.

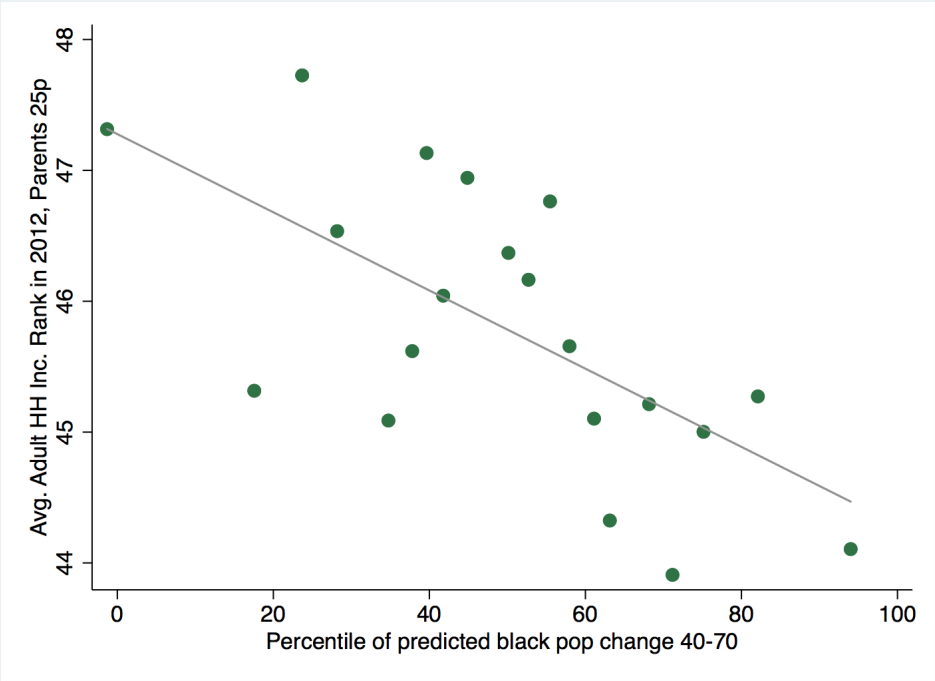
To focus on the more plausibly causal estimates of the impact of the Great Migration, I primarily discuss reduced form and IV results in what follows. For all main results, however, I report first stage, OLS, reduced form and two-stage least squares ("2SLS") results and briefly discuss differences between OLS and 2SLS estimates.

[28] See Wilkerson (2011) for accounts and experiences of individual migrants arriving in and navigating new lives in the North.

1.5.1 Impact on average upward mobility

I first estimate the impact of predicted black population increases during the Great Migration, or $G\hat{M}$, on average upward mobility at the commuting zone level (the model in equation 1.6). The outcome variable is mean expected household income rank of individuals from the 1980-1986 birth cohorts with parents at income rank p by their childhood commuting zone, where individuals' income is measured at age 26. Figure 1.5 shows a binned scatterplot of the relationship between $G\hat{M}$ and upward mobility for individuals with low income parents (at the 25th percentile of the parent income distribution). Both the outcome and $G\hat{M}$ have been residualized on the baseline set of controls discussed in Section 1.4. Each dot represents average outcomes across commuting zones within 5-percentile bins of the shock. The figure shows a striking negative relationship between historical black migrant inflows and average outcomes for individuals from low income families in the destination CZs today.

FIGURE 1.5: *Great Migration reduced upward mobility for low income families in northern commuting zones*



Notes: This binned scatterplot depicts the relationship between average upward mobility in the 2000s for men and women with low income parents and the instrument for black population increases during the Great Migration. The unit of observation is a commuting zone. The right hand side variable is grouped into 20 bins (5 percentiles each). Upward mobility is defined as expected mean household income rank for men and women with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Both the left hand and right hand side variables have been residualized on the set of baseline 1940 controls, including share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: IPUMS complete count US census for 1940; Boustan (2016); Chetty and Hendren (2018b).

Table 1.2 reports 2SLS estimates of the relationship. A 1-percentile increase in the historical black population lowered household income rank by -0.0981 percentile points (s.e. = 0.0301). OLS estimates are reported in Table 1.2 as well. The 2SLS coefficients are larger in magnitude than the OLS. One potential explanation for this is that omitted characteristics that are correlated with both black population change and upward mobility are positively correlated with both. For example, the black population grew more in places with higher levels of median educational attainment in 1940. To the extent that higher education levels

reflect better school quality, which may persist over time, OLS estimates of the relationship between the Great Migration and upward mobility today would be biased towards zero.

TABLE 1.2: *Great Migration impact on average upward mobility for low income families in the 2000s*

	Ordinary Least Squares					
	Pooled HH Lower Inc	Women HH Lower Inc	Men HH Lower Inc	Pooled Ind Lower Inc	Women Ind Lower Inc	Men Ind Lower Inc
GM	-0.0557***	-0.0455***	-0.0660***	-0.0203*	0.0128	-0.0526***
	(0.0100)	(0.0104)	(0.0103)	(0.0111)	(0.0143)	(0.0108)
R-squared	0.563	0.503	0.595	0.307	0.184	0.493
	Reduced Form					
	Pooled HH Lower Inc	Women HH Lower Inc	Men HH Lower Inc	Pooled Ind Lower Inc	Women Ind Lower Inc	Men Ind Lower Inc
$\hat{GM}$	-0.0299***	-0.0223**	-0.0374***	-0.0184*	0.0000493	-0.0371***
	(0.00952)	(0.00964)	(0.00996)	(0.00978)	(0.0127)	(0.00984)
R-squared	0.493	0.449	0.515	0.308	0.178	0.457
	Two-stage least squares					
	Pooled HH Lower Inc	Women HH Lower Inc	Men HH Lower Inc	Pooled Ind Lower Inc	Women Ind Lower Inc	Men Ind Lower Inc
GM	-0.0981***	-0.0733**	-0.123***	-0.0606*	0.000162	-0.122***
	(0.0301)	(0.0300)	(0.0324)	(0.0328)	(0.0402)	(0.0350)
R-squared	0.498	0.474	0.494	0.232	0.178	0.321
N	130	130	130	130	130	130
Outcome Mean	45.79	47.04	44.55	45.54	42.74	48.29
Outcome SD	3.379	3.283	3.617	2.972	3.527	3.375

Notes: This table reports the estimated impact of the Great Migration on average upward mobility in the 2000s for men and women with low income parents. The unit of observation is a commuting zone. Dependent variable is expected mean individual or household income rank for individuals with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. Pooled income refers to household income, pooling across men and women. Independent variable is the percentile of black population increase during the Great Migration, or the instrument for the same. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: CCDB, IPUMS complete count US census for 1940; Boustan (2016); Chetty and Hendren (2018b).

Table 1.3 reports the results for individuals with high income parents. I do not find strong effects of $\hat{GM}$ on the outcomes of individuals with high income parents (at the 75th

percentile of the parent income distribution). I find evidence of weak negative effects on the individual income rank of men with high income parents.

TABLE 1.3: *Great Migration impact on average upward mobility for high income families in the 2000s*

	Ordinary Least Squares					
	Pooled HH Higher Inc	Women HH Higher Inc	Men HH Higher Inc	Pooled Ind Higher Inc	Women Ind Higher Inc	Men Ind Higher Inc
GM	-0.0340***	-0.0293***	-0.0387***	-0.00658	0.0116	-0.0239***
	(0.00792)	(0.00793)	(0.00822)	(0.00816)	(0.0106)	(0.00810)
R-squared	0.528	0.513	0.533	0.474	0.425	0.465
	Reduced Form					
	Pooled HH Higher Inc	Women HH Higher Inc	Men HH Higher Inc	Pooled Ind Higher Inc	Women Ind Higher Inc	Men Ind Higher Inc
$\widehat{GM}$	-0.0109	-0.00807	-0.0138*	-0.000766	0.00961	-0.0109
	(0.00743)	(0.00734)	(0.00778)	(0.00722)	(0.00936)	(0.00733)
R-squared	0.466	0.463	0.463	0.471	0.424	0.437
	Two-stage least squares					
	Pooled HH Higher Inc	Women HH Higher Inc	Men HH Higher Inc	Pooled Ind Higher Inc	Women Ind Higher Inc	Men Ind Higher Inc
GM	-0.0359	-0.0265	-0.0452*	-0.00252	0.0316	-0.0359
	(0.0222)	(0.0223)	(0.0231)	(0.0229)	(0.0302)	(0.0229)
R-squared	0.528	0.512	0.531	0.473	0.408	0.455
N	130	130	130	130	130	130
Outcome Mean	58.82	60.40	57.28	57.95	55.39	60.44
Outcome SD	2.570	2.533	2.684	2.510	3.118	2.470

Notes: This table reports the estimated impact of the Great Migration on average upward mobility in the 2000s for men and women with high income parents. The unit of observation is a commuting zone. Dependent variable is expected mean individual or household income rank for individuals with parents at the 75th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. Pooled income refers to household income, pooling across men and women. Independent variable is the percentile of black population increase during the Great Migration, or the instrument for the same. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

How should one interpret the negative effect of the Migration on average upward mobility? In a simple framework where the adult outcomes of children conditional on parent economic status are a function of childhood location and an unobservable family

component, the Migration may influence mean outcomes either by changing aspects of the location or changing the characteristics of the average child. More formally, let the outcome for a child i with parent household income rank p living in CZ be the sum of a pure location component and an idiosyncratic family component:

$$y_{ip,CZ} = \mu_{p,CZ} + \theta_{ip,CZ} \tag{1.8}$$

Recall, I observe mean outcomes in a location at a given parent income rank p:

$$\bar{y}_{p,CZ} = \mu_{p,CZ} + \bar{\theta}_{p,CZ} \tag{1.9}$$

Because any migration event changes the composition of families in a destination location, there is a potential mechanical effect of the Great Migration on $\bar{\theta}_{p,CZ}$.[29] Alternatively, the Migration may affect behavior of incumbents within a commuting zone, for example, altering the equilibrium bundle of public goods voted on by local residents or their residential choices within a commuting zone, giving rise to various forms of segregation. These choices may in turn affect the outcomes of children growing up in these locations in the future, independent of their familes' characteristics, $\theta_{ip,CZ}$. In that case, the Migration would affect average outcomes through the channel of $\mu_{p,CZ}$.

One example of θ_i includes the race of the child, which if unobserved, could explain a substantial portion of the Migration's estimated impact on $\bar{y}_{p,CZ}$. Several studies have found persistent differences in intergenerational mobility by race (Davis and Mazumder, 2018; Mazumder, 2014; Bhattacharya and Mazumder, 2011). Chetty *et al.* (2018) find that black men have lower income rank than white at every parent income rank, and these gaps persist even among those observed to be growing up in the same census tract. Areas with a higher black share of the population likely have lower average upward mobility.

Another example of θ_i would be a family's propensity to invest in the human capital of their children. Even after conditioning on parent income, if families tend to value or invest

[29] Further, incumbent families may leave an with high levels of in-migration, further potentially affecting $\bar{\theta}_{p,CZ}$.

in human capital differently, this may lead to divergent adult outcomes for children from these families, even after conditioning on parent income rank.

1.5.2 Impact on childhood exposure effects

To address sources of selection θ_i that may be driving the relationship between the Migration and average upward mobility in Figure 1.5, I turn to an alternative metric of upward mobility in locations that attempts to isolate the causal effect of childhood location.

I take these estimates from Chetty and Hendren (2018b). The authors estimate the causal effect of growing up in every commuting zone in the United States using federal income tax data on families that moved across commuting zones and exploiting variation in children's ages at the time their families moved.[30] Under the assumption that the age of a child at the time a family moved is orthogonal to unobserved family characteristics θ_i, estimating the effect of one additional year of childhood exposure to a location and multiplying this effect by number of years of childhood provides a direct estimate of $\mu_{p,CZ}$ in the model in equation 1.8.[31]

The advantage to using these measures is that they provide metrics of upward mobility that isolate the effect of childhood location. Thus, any impact of the Great Migration on this alternative measure of upward mobility can be interpreted as follows: a child randomly assigned to spend an additional year in CZ A that experienced a large shock versus CZ B that experienced a small shock has greater or lower adult income rank. One downside to these measures is that they are not available separately by race. This means I identify impacts of the Migration on childhood exposure upto an average effect across black and white children. Data limitations prevent me from exploring potentially heterogeneous impacts of the Migration on $\mu_{pr,CZ}$, location effects for black versus white children. In

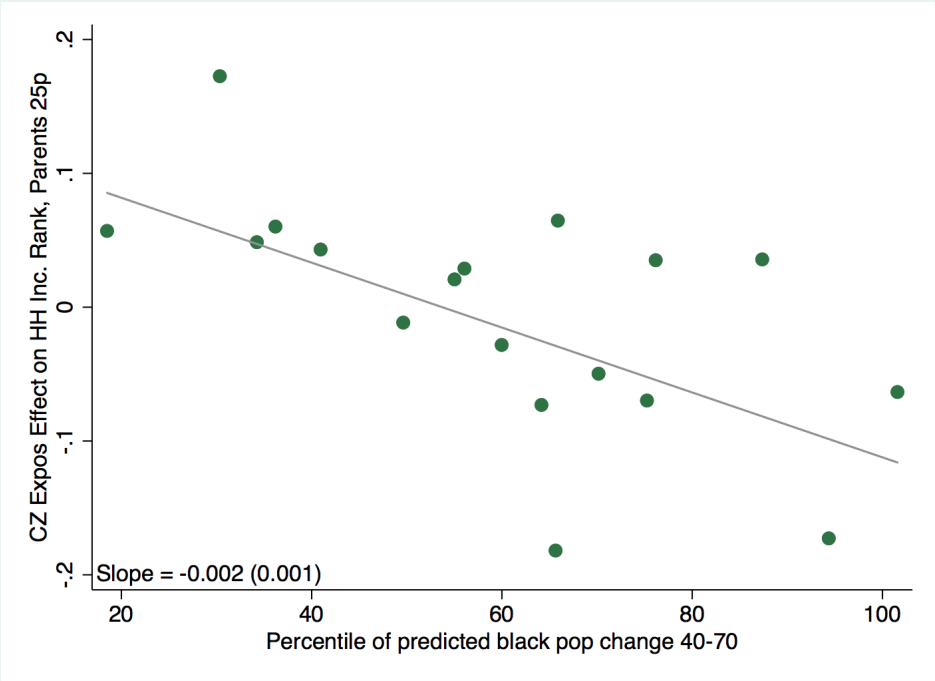
[30] Parents and children are assigned commuting zones based on the ZIP Code information available on their tax returns.

[31] These estimates are valid estimates of $\mu_{p,CZ}$ if the identifying assumption that the age of the child at the time of the family's move is orthogonal to omitted determinants of outcomes for children. See Chetty and Hendren (2018a) for several checks of these identifying assumptions including instrumenting for moves with displacement shocks to families and the inclusion of family fixed effects.

Section 1.5.3, I explore this heterogeneity using race-specific measures of average upward mobility in commuting zones and discuss the plausibility of the findings being driven by race-specific selection stories ($\bar{\theta}_{pr,CZ}$).

Figure 1.6 shows a binned scatterplot of the impact of the Great Migration on CZ childhood exposure effects for individuals with parents from the 25th percentile of the parent income distribution. Both the outcome and $G\hat{M}$ have been residualized on the baseline set of controls discussed in Section 1.4. Each dot represents average outcomes across commuting zones within 5-percentile bins of the shock. The figure shows a strong negative relationship between historical black migrant inflows and the effects of childhood exposure to destination CZs.

FIGURE 1.6: *Childhood exposure to Great Migration CZs reduced adult income of children from low income families*



Notes: This binned scatterplot depicts the relationship between commuting zone childhood exposure effects in the 2000s for men and women with low income parents and the instrument for black population increases during the Great Migration. The unit of observation is a commuting zone. The right hand side variable is grouped into 20 bins (5 percentiles each). Childhood exposure effects are the estimated causal impact of one additional year of childhood in the commuting zone on adult household income rank for men and women with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Both the left hand and right hand side variables have been residualized on the set of baseline 1940 controls, including share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: IPUMS complete count US census for 1940; Boustan (2016); Chetty and Hendren (2018b).

Table 1.4 reports OLS and 2SLS estimates of the relationship. The 2SLS estimates an be interpreted as follows: a 1-percentile larger increase in the historical black population lowers household income rank by -0.0083 percentile points (s.e.=.0025). The first stage is around 0.30, so the 2SLS coefficients are three times larger in magnitude than the OLS, indicating again that there may be omitted characteristics positively correlated with both childhood exposure effects and black population change that then bias the OLS estimates towards zero.

TABLE 1.4: *Great Migration impact on childhood exposure effects in the 2000s for low income families*

	First Stage on GM					
GM Shock	0.292***	0.289***	0.294***	0.290***	0.288***	0.294***
	(0.0652)	(0.0652)	(0.0658)	(0.0651)	(0.0655)	(0.0656)
F-Stat	27.46	26.82	26.97	27.72	27.19	27.22
	Household Income Rank			Individual Income Rank		
	Pooled	Women	Men	Pooled	Women	Men
	Ordinary Least Squares					
GM	-0.00282***	-0.00203**	-0.00404***	-0.00167***	0.000591	-0.00336***
	(0.000582)	(0.000861)	(0.000867)	(0.000590)	(0.000857)	(0.000920)
R-squared	0.206	0.106	0.227	0.176	0.0190	0.198
	Reduced Form					
GM Shock	-0.00242***	-0.00218**	-0.00341***	-0.00203***	-0.000927	-0.00315***
	(0.000628)	(0.000928)	(0.000963)	(0.000641)	(0.000939)	(0.00102)
R-squared	0.249	0.137	0.229	0.213	0.0381	0.196
	Two-stage least squares					
GM	-0.00830***	-0.00756**	-0.0116***	-0.00701***	-0.00322	-0.0107***
	(0.00250)	(0.00343)	(0.00357)	(0.00245)	(0.00330)	(0.00363)
R-squared	-0.0774	-0.0486	0.0250	-0.0259	-0.0490	0.0709
N	130	130	130	130	130	130
Precision Wt	Y	Y	Y	Y	Y	Y
Mean Expos FX	-0.0160	-0.0151	-0.0303	0.0223	0.0236	-0.0000692
SD Expos FX	0.172	0.235	0.259	0.172	0.226	0.271
SD GM	24.82	24.42	24.84	24.99	24.76	24.95

Notes: This table reports the estimated impact of the Great Migration on commuting zone childhood exposure effects. The unit of observation is a commuting zone. Dependent variable is the estimated causal impact of one additional year of childhood in the commuting zone on adult household income rank for men and women with parents at the 25th percentile of the parent income distribution. Pooled income refers to household income, pooling across men and women. Independent variable is percentile of black population increase during the Great Migration, or the instrument for the same. See text for baseline controls. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I find strong negative effects of predicted black population increases on exposure effects for individuals with high income parents (see Table 1.5), in contrast with the impact of the Migration on average upward mobility for high income families. The impact on exposure effects on men's individual income rank is particularly strong, compared to effects on

household income. Overall, the impact of predicted black population increases on childhood exposure effects is similar for individuals with low and high income parents.

TABLE 1.5: *Great Migration impact on childhood exposure effects in the 2000s for high income families*

	First Stage on GM					
GM Shock	0.302***	0.300***	0.302***	0.301***	0.301***	0.302***
	(0.0632)	(0.0633)	(0.0633)	(0.0631)	(0.0635)	(0.0632)
F-Stat	27.69	27.32	27.41	28.00	27.74	27.69
	Household Income Rank			Individual Income Rank		
	Pooled	Women	Men	Pooled	Women	Men
	Ordinary Least Squares					
GM	-0.00132**	-0.00110	-0.00188**	-0.000128	0.000958	-0.00115
	(0.000587)	(0.000808)	(0.000770)	(0.000550)	(0.000830)	(0.000784)
R-squared	0.252	0.196	0.133	0.466	0.378	0.175
	Reduced Form					
GM Shock	-0.00127**	-0.000742	-0.00144*	-0.00192***	-0.00131	-0.00226***
	(0.000599)	(0.000829)	(0.000823)	(0.000564)	(0.000890)	(0.000822)
R-squared	0.324	0.260	0.141	0.515	0.385	0.214
	Two-stage least squares					
GM	-0.00421**	-0.00247	-0.00476*	-0.00638***	-0.00434	-0.00747***
	(0.00205)	(0.00274)	(0.00266)	(0.00229)	(0.00317)	(0.00289)
R-squared	0.232	0.220	0.125	0.228	0.246	0.0582
N	130	130	130	130	130	130
Precision Wt	Y	Y	Y	Y	Y	Y
Mean Expos FX	-0.00323	-0.0253	-0.0162	0.0305	0.0182	-0.00525
SD Expos FX	0.175	0.228	0.212	0.195	0.270	0.222
SD GM	24.40	24.08	24.29	24.52	24.33	24.38

Notes: This table reports the estimated impact of the Great Migration on commuting zone childhood exposure effects. The unit of observation is a commuting zone. Dependent variable is the estimated causal impact of one additional year of childhood in the commuting zone on adult household income rank for men and women with parents at the 75th percentile. Independent variable is percentile of black population increase during the Great Migration, or the instrument for the same. See text for baseline controls. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Interpretation of results on childhood exposure effects

The results thus far support the hypothesis that one way the Great Migration lowered upward mobility was by negatively impacting childhood environment. These estimated impacts on childhood environment can be combined with the first set of results on upward mobility to quantify the impact of the Migration through $\mu_{p,CZ}$ versus $\bar{\theta}_{p,CZ}$. I do this by scaling the 2SLS estimated effect on one year of childhood exposure to represent full childhood exposure to a Great Migration destination and comparing the resulting scaled estimate with the 2SLS estimated impact on observed upward mobility.

Scaling the estimated impact on childhood exposure effects requires making assumptions about the relationship between the average effect of a year of childhood exposure to a location and the age at which the child is exposed to the location. In other words, if the effect of childhood location remains constant over years of childhood, then multiplying the impact of one year by total number of years exposed yields the effect of full childhood exposure.

Chetty and Hendren (2018a) and Chetty and Hendren (2018b) assume constant location effects over each year of childhood and multiply exposure effects by 20 to approximate full childhood exposure. In more recent work, however, Chetty *et al.* (2018) using data on earlier cohorts of individuals find that the relationship between age at move and predicted income rank in a destination exhibits a kink around age 13, with pre-teen years of childhood exposure having a smaller effect on adult outcomes than teen and post-teen years (see Appendix Figure A11).

The table below decomposes the impact of the Great Migration on upward mobility through the channel of location versus selection using each assumption in turn. Estimates have been scaled to represent the effects of a 30-percentile increase, or approximately 1 s.d., in the historical black population. The first row reports results from assuming constant effects over 20 years of childhood exposure leading to a multiplier of 20, and the second row assumes muted effects in the pre-teen years, leading to a multiplier of 15.52. Appendix Section A4.1 provides the exact numbers used to calibrate this scaling exercise.

Column 1 reports the impact of the Great Migration on location effects, scaled to

Table 1.6: *Contribution of location versus selection in Great Migration effects*

	CZ Childhood Exposure Effects	Average Upward Mobility
20 years	-5	-3
15.52 years	-3.9	-3

represent full childhood exposure to those locations. Column 2 reports the impact of the Great Migration on average upward mobility. The latter estimate combines the Migrations effect through the selection channel as well as the location channel. The ratio of Column 1 estimates to Column 2 estimates gives a sense of what share of the impact of the Migration is driven by location versus selection effects.

The first row suggests that the channel of childhood location explains 167% ($\frac{5}{3} \times 100$) of the impact of the Migration on upward mobility, or that selection effects are in fact positive. The second row makes this comparison using the assumption of more muted impacts of early years of childhood exposure. In this case, I find that the location channels explain 130% of the Migration's effect on upward mobility.

These results suggest that changes in childhood environment are the main mechanism for the impact of the Great Migration on upward mobility. If the empirical strategy is valid, the estimates reported above reflect the causal effect of black population changes during the Great Migration on childhood environment.

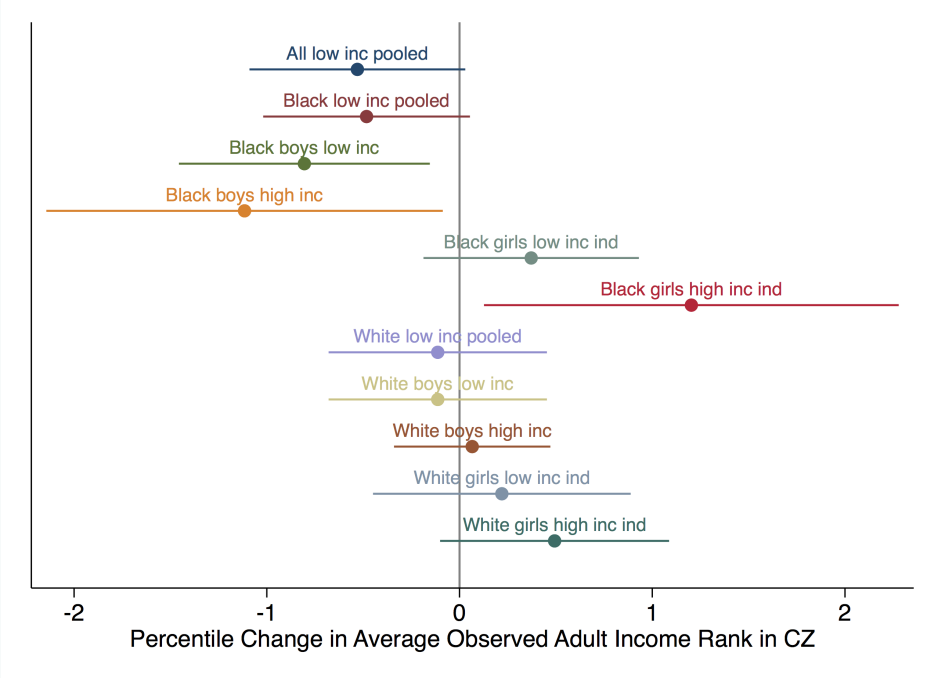
1.5.3 Heterogeneity by race and gender

In this section, I explore whether different groups of children were affected more or less by the Migration. I do so by estimating the impact of $\hat{GM}$ on race-specific average upward mobility in CZs for black and white men and women from the 1978-1983 birth cohorts. The outcome variable is mean conditional income rank in 2015 by childhood commuting zone. OLS, reduced form, and 2SLS results are reported in Table 1.7 for black men and women and Table 1.8 for white men and women.

Figure 1.7 summarizes these regressions in a plot of the coefficients on percentile of predicted black population change in regressions of each sub-group's average upward

mobility on $G\hat{M}$. Here the shock has been scaled to be in 30-percentile units, approximately 1 standard deviation. The negative effects of the Migration load on black men. A 30-percentile increase in the intensity of a CZ's Great Migration shock lowers the individual income rank of black men by around 1 percentile point, with slightly larger effects on men with higher income parents. By contrast, I find no effects of the Migration on the individual earnings of white men from any parent income group.[32]

FIGURE 1.7: *Race and gender heterogeneity in impact of Great Migration on upward mobility*



Notes: This figure plots coefficients from regressions of average upward mobility in the 2000s for men and women from low and high income parents on the instrument for black population increases during the Great Migration, in approximately one standard deviation units. The unit of observation is a commuting zone. Upward mobility is defined as expected mean individual or household income rank where income is measured from IRS tax returns for cohorts born between 1978 and 1983. Pooled income refers to mean household income rank, pooling across men and women. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. A one standard deviation increase is approximately 30 percentiles. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: IPUMS complete count US census for 1940; Boustan (2016); Chetty et al. (2018).

The point estimates for the Great Migration's impact on black women's individual

[32]The term "white" refers to non-Hispanic white population (Chetty *et al.*, 2018).

earnings are positive and insignificant for black women from low income families and positive and significant at the 10% level for black women from higher income families. These positive effects may represent an income effect. Interracial marriage rates are very low, so black women who marry men likely form households with black men. Given that black men's income is lower in Great Migration destinations, women may increase their labor supply to compensate for missing men's income. To test this hypothesis, I estimate the effect of the Migration on black women's household income rank as opposed to their individual income rank. The Migration has a negative and insignificant effect on black women's household income rank, consistent with black women increasing their labor supply in locations with a low marriage rate or missing income of black men in shared households. I report these results in Table A4. Results for white households are reported in Table A5.

TABLE 1.7: *Great Migration impact on average upward mobility for black families in the 2000s*

	First Stage				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
$\widehat{GM}$	0.304***	0.304***	0.304***	0.304***	0.304***
	(0.0750)	(0.0750)	(0.0750)	(0.0750)	(0.0750)
F-Stat	23.32	23.32	23.32	23.32	23.32
	Ordinary Least Squares				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	-0.0532***	-0.0386***	-0.0723***	-0.0437**	-0.101***
	(0.00959)	(0.0113)	(0.0113)	(0.0215)	(0.0174)
R-squared	0.416	0.274	0.445	0.203	0.357
	Reduced Form				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
$\widehat{GM}$	-0.0165*	-0.0114	-0.0247**	-0.00476	-0.0393**
	(0.00921)	(0.0102)	(0.0112)	(0.0190)	(0.0167)
R-squared	0.286	0.212	0.288	0.176	0.214
	Two-stage least squares				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	-0.0516**	-0.0358	-0.0773***	-0.0149	-0.123***
	(0.0253)	(0.0298)	(0.0299)	(0.0572)	(0.0463)
R-squared	0.416	0.273	0.444	0.191	0.349
N	129	129	129	129	129
Outcome Mean	33.19	35.22	31.21	46.67	44.21
Outcome SD	2.747	2.900	3.335	5.281	4.757

Notes: This table reports the estimated impact of the Great Migration on average upward mobility in the 2000s for black men and women with high income parents. The unit of observation is a commuting zone. Dependent variable is expected mean household income rank for individuals with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1978 and 1983. Independent variable is the percentile of black population increase during the Great Migration. Independent variable is percentile of black population increase during the Great Migration, or the instrument for the same. See text for baseline controls. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 1.8: *Great Migration impact on average upward mobility for white families in the 2000s*

	First Stage				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
$\widehat{GM}$	0.304***	0.304***	0.304***	0.304***	0.304***
	(0.0750)	(0.0750)	(0.0750)	(0.0750)	(0.0750)
F-Stat	23.32	23.32	23.32	23.32	23.32
	Ordinary Least Squares				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	0.000760	0.00678	-0.00356	-0.00678	-0.0137
	(0.0127)	(0.0140)	(0.0117)	(0.00888)	(0.00835)
R-squared	0.197	0.186	0.209	0.241	0.312
	Reduced Form				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
$\widehat{GM}$	0.00483	0.00748	0.00305	0.00881	0.00341
	(0.0112)	(0.0123)	(0.0103)	(0.00781)	(0.00744)
R-squared	0.198	0.187	0.209	0.245	0.298
	Two-stage least squares				
	Pooled Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	0.0159	0.0246	0.0100	0.0289	0.0112
	(0.0359)	(0.0395)	(0.0329)	(0.0265)	(0.0243)
R-squared	0.187	0.175	0.200	0.141	0.262
N	130	130	130	130	130
Outcome Mean	45.23	46.27	44.27	61.78	59.58
Outcome SD	3.163	3.459	2.926	2.273	2.246

Notes: This table reports the estimated impact of the Great Migration on average upward mobility in the 2000s for white men and women with high income parents. The unit of observation is a commuting zone. Dependent variable is expected mean household income rank for individuals with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1978 and 1983. Independent variable is percentile of black population increase during the Great Migration, or the instrument for the same. See text for baseline controls. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results above do not rule out within-race selection (θ_{ir}) of families into Great Migration locations today as a potential mechanism for the effect of the Migration on average upward mobility. Certain family characteristics, especially family structure or

presence of both parents in a household, have been shown to have much stronger effects on boys versus girls (Bertrand and Pan, 2013). Boys' outcomes are also more elastic than girls' to other inputs as well, for example, school quality (Figlio *et al.*, 2016). If black families that invest less in their children's human capital are more likely to live in Great Migration destinations today, then boys from these families may be more affected as adults than girls.

Implications for the racial gap

The fact that black men have reduced conditional income as a result of the Migration but white men are unaffected has implications for the racial gap in income upward mobility in the US. Chetty *et al.* (2018) find that conditional on parent income, black and white women have identical income rank as adults. This implies that the substantial racial gap in upward mobility is driven primarily by differences in the outcomes of black and white men with similar parent income during childhood. In this section I conduct a counterfactual exercise to quantify the contribution of the Great Migration to the gap in upward mobility between black and white men with low income, high income, and median income parents.

The counterfactual seeks to address the following question: what would the racial gap in men's upward mobility in North be without the changes induced by Great Migration? I first calculate the racial gap in each commuting zone at each of the listed parent income rank by taking the difference in mean white and black men's conditional income rank. Figure A10 illustrates the positive relationship between $\hat{GM}$ and the racial gap. I then predict what the racial gap would be if each location instead received the lowest percentile of shock.

A comparison of the actual average racial gap across northern urban commuting zones and the counterfactual gap in absence of the effect of the Great Migration is reported in the table below.

The first row reports the average gap in mean black and white men's upward mobility across commuting zones in the sample: 10.46 income rank percentiles for men with parents at the 25th percentile and 11.03 income rank percentiles for men with parents at the 75th percentile. The second row reports the counterfactual average gap if each location received

Table 1.9: *Contribution of the Migration to the racial upward mobility gap among men*

	Low Parent Income	High Parent Income
Average gap	10.46	11.03
Counterfactual gap (s.e.)	6.9 (.16)	5.0 (.24)
Pct Change	-34%	-55%

the lowest percentile of Great Migration shock. Under this counterfactual, the average racial gap across northern commuting zones would be 6.9 percentiles (s.e. = .16 percentiles) for men with low income parents, and 5 percentiles (s.e. = .24 percentiles) for men with high income parents.

These estimates suggests the Migration increased the racial gap by 36% for low income families and 55% for high income families. Finally, looking at men with median income parents, I estimate the Migration increased the gap between black and white men with median income parents by 43%. These substantial effects on upward mobility and the racial gap warrant an exploration of the local mechanisms through which the Migration affected outcomes. Before assessing these potential mechanisms in Section 2.1, I first discuss several alternative explanations for the findings, namely, the role of potential omitted variables.

1.5.4 Alternative explanations

Deindustrialization A key competing explanation for reductions in children's outcomes in Great Migration destinations is deindustrialization: black southerners moved to booming industrial centers, and these areas subsequently underwent greater job loss than locations less specialized in manufacturing. In all specifications, I control for the share of the labor force in manufacturing in 1940, which largely accounts for variation in manufacturing shares in subsequent decades.[33] I find no effect of the Migration on the share of the labor force in manufacturing from 1950 to 1970. Further, I find no effect of the Great Migration on the adult outcomes of white men with low income parents, a demographic group that would

[33]The correlation between 1950 share of the labor force in manufacturing and the baseline period share is 0.96. By 1970, this drops only slightly, to 0.84.

likely be affected if the findings were driven by deindustrialization rather than changes in racial composition.

European immigrant labor Prior to their reliance on southern black labor, major industrial centers in the North employed European immigrants. Sequeira *et al.* (2019) demonstrate that counties that received larger influxes of European immigrants subsequently had higher growth and less poverty. It's possible that the effect of the Great Migration confounds the loss of this labor supply during World War I and after the Immigrant Exclusion Act of 1924, which induced these areas to begin hiring black workers from the South. I do not find evidence consistent with historical European immigrant shares driving my findings: controlling for lagged European immigrant shares prior to 1940 does not alter the precision or magnitude of the impact of the Great Migration on upward mobility.

White southern migration A further consideration is the effect of changing the southern born share of the population. Southerners may have different policy preferences than northern incumbents. The increase in the southern born share of the population is therefore a confounding factor in the Great Migration's estimated impact on upward mobility. I explore this alternative explanation by leveraging the fact that white southerners also migrated to northern cities during this period. In a placebo exercise, I show that instrumenting for the change in the white southern population during this period has no effect on black men's upward mobility. White southern in-migration also has no impact on childhood exposure effects.[34] Appendix Figure A14, Panel B, shows the relationship between white southern in-migration and black men's outcomes in binned scatterplots. The relationship is insignificant and the coefficient has the opposite sign as the effect of black population increases.

[34]White southern migration appears associated with lower outcomes for white men and women from lower income parents. The lack of an effect on childhood exposure effects suggests that the channel is the composition of the average white child as opposed to changes in local public goods or neighborhood quality in response to historical in-migration of white southerners.

Black population shares Finally, I examine the extent to which the findings are driven by variation in historical black population levels as opposed to increases. If locations with high black population shares are fundamentally different from those with lower black population levels, this fixed characteristic could be a confounding factor for my findings. The impact of exogenous black population increases ($\hat{GM}$) is robust to controlling for lagged black population shares prior to 1940, suggesting that changes in the racial composition, not simply the levels of the black population, contributed to the changes I document. I include controls for the 1920, 1930, and 1940 black population shares.

Results are reported in tables 1.10 and 1.11. In the case of childhood exposure effects, the point estimates are similar in magnitude and precision across these specifications. The coefficient attenuates slightly for the impacts on black men's upward mobility. However the upward mobility estimates for black men are less precise in places with very small black populations, which may lead to attenuation in the estimated impact of the Migration due to down-weighting locations with well measured outcomes for black men. In the case of childhood exposure effects, which rely on a different source of variation (children's ages at the time their families relocated across commuting zones) results are highly robust to including lagged black population shares and flexible controls for the black population share in 1940 (e.g., separate controls for quartiles of the black population share—see Appendix Figure A15).

Bartik: potential endogeneity of shares and shifters Recent analyses of Bartik or shift-share instruments commonly used to estimate the causal impact of immigration or of demand shocks to labor markets have generated new intuitions on the exogeneity of shift share instruments. Two views have emerged on the source of exogeneity in the instrument. Goldsmith-Pinkham *et al.* (2018) argue for exogeneity in terms of shares: the identification condition in a shift-share or Bartik style instrument is satisfied if, for example in the context of manufacturing labor demand shocks, industry shares are exogenous to location characteristics. The authors recommend investigation of whether specific shares drive the

TABLE 1.10: *Robustness of results on childhood exposure effects*

$\widehat{GM}$	-0.00144***	-0.00266***	-0.00242***	-0.00204***	-0.00233***	-0.00220***	-0.00261***	-0.00246***
	(0.000468)	(0.000506)	(0.000628)	(0.000637)	(0.000673)	(0.000632)	(0.000645)	(0.000619)
R-squared	0.0684	0.227	0.249	0.283	0.320	0.291	0.258	0.278
N	130	130	130	130	130	130	130	130
Precision Wt	Y	Y	Y	Y	Y	Y	Y	Y
Census Div FE	N	N	Y	Y	Y	Y	Y	Y
Baseline Controls	N	Y	Y	Y	Y	Y	Y	Y
Pop 1940	N	N	N	Y	N	N	N	N
Black Shares 1920-1940	N	N	N	N	Y	N	N	N
1940 Black Share Quartile FEs	N	N	N	N	N	Y	N	N
Recent WS Mig Share 1940	N	N	N	N	N	N	Y	N
FB White Share 1940	N	N	N	N	N	N	N	Y

Notes: This table reports robustness of the estimated impact of the Great Migration on commuting zone childhood exposure effects to several alternative specifications. The unit of observation is a commuting zone. Dependent variable is commuting zone childhood exposure effects in the 2000s for men and women with low income parents. Childhood exposure effects are the estimated causal impact of one additional year of childhood in the commuting zone on adult household income rank for men and women with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. Pooled income refers to household income, pooling across men and women. Independent variable is the percentile of black population increase during the Great Migration. The instrument for black population increase is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: IPUMS complete count US census for 1940; Boustan (2016); Chetty and Hendren (2018b).

TABLE 1.11: *Robustness of results on black men's upward mobility*

GM Shock	-0.000380***	-0.000366***	-0.000248**	-0.000292**	-0.000266**	-0.000160	-0.000290**	-0.000263**
	(0.0000884)	(0.0000927)	(0.000113)	(0.000114)	(0.000122)	(0.000111)	(0.000118)	(0.000107)
R-squared	0.127	0.138	0.185	0.208	0.187	0.258	0.196	0.275
N	129	129	129	129	129	129	129	129
Precision Wt	Y	Y	Y	Y	Y	Y	Y	Y
Baseline Controls	N	N	Y	Y	Y	Y	Y	Y
Census Div FE	N	Y	Y	Y	Y	Y	Y	Y
Pop 1940	N	N	N	Y	N	N	N	N
Black Shares 1920-1940	N	N	N	N	Y	N	N	N
Black Share Rank 1940	N	N	N	N	N	Y	N	N
Recent WS Mig Share 1940	N	N	N	N	N	N	Y	N
FB White Share 1940	N	N	N	N	N	N	N	Y

Notes: This table reports robustness of the estimated impact of the Great Migration on black men's upward mobility to several alternative specifications. The unit of observation is a commuting zone. Dependent variable is expected mean individual income rank for individuals with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1978 and 1983. Independent variable is the percentile of black population increase during the Great Migration. The instrument for black population increase is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: IPUMS complete count US census for 1940; Boustan (2016); Chetty *et al.* (2018).

findings as endogeneity of these shares are a particular threat to the identification strategy.

In my setting, over 1200 shares link northern destinations to southern county migration. The large number of shares that can provide identification is reassuring: both Adão *et al.* (2018) and Goldsmith-Pinkham *et al.* (2018) find that identifying assumptions are more likely to be met the greater the number of shares. To test whether there are particular southern counties driving the results on upward mobility, I directly regress outcomes on city-population weighted average shares at the commuting zone level.[35] Because the number of shares far exceeds the number of destinations, I use LASSO to select shares that best predict outcomes, focusing on the childhood exposure effects outcome and including baseline controls. Four shares are ultimately selected from the following southern counties: Tucker County, WV; Armstrong County, TX; Mitchell County, NC; and Hamilton County, FL. Results are robust to controlling for these shares in the baseline regression.

Borusyak *et al.* (2018) show that in the case where shares are endogenous, shifters can provide exogeneity provided that shocks to industry, or in my setting, southern counties are not correlated with shocks northern destinations. Relying on predicted rather than actual outmigration from southern counties alleviates some of these concerns. To address the possibility that shocks to particular states are correlated with shocks to, for example, Detroit or Baltimore, I show that my results are robust to first residualizing county net-migration on state fixed effects and then predicting migration on a new set of optimal county-level predictors chosen through my Post-LASSO estimation procedure. In Appendix Figure **??**, I report these results.

Finally, I conduct an additional check on the validity of my identification strategy using over-identification tests using three slightly different sources of variation to construct the instrument. In addition to the baseline instrument, a second instrument is constructed using the southern county outmigration rates that are first residualized on state fixed effects. A third version of the instrument uses variation in state of birth across the southern born

[35] If southern counties A and B sent migrants to city C and D, then average A and B shares at the commuting zone level are the sum of the shares of each county in C and D weighted by the population in C and D, divided by the total urban population $C + D$ in the commuting zone.

black population in northern cities in 1940 to address the fact that 1935-1940 migration rates were low as a result of the Great Depression. Results using each of these versions of the instrument are extremely similar. Further, an over-identification test fails to reject the null that the estimated effects on upward mobility are statistically indistinguishable from each other.[36]

Together, the evidence presented thus far supports the interpretation that racial composition shocks during the Great Migration lowered upward mobility in destination commuting zones through a deterioration of the northern urban childhood environment. Further, the change in childhood environment appears to have been particularly detrimental for black men.

1.6 Conclusion

The divergent trajectories of black and white boys with observably identical parental income is one of the most striking examples of inequality in the US. One of the policies aimed at reducing disparities in children's outcomes is to incentivize families to "move to opportunity" (MTO), or to relocate to neighborhoods with better opportunities, as measured by poverty rates, average outcomes, or, more recently, childhood exposure effects. Yet the persistent gap in upward mobility among black and white boys growing up in the same neighborhood raises the question of whether these types of policies can be effective at reducing racial gaps in children's outcomes, and if not, why not.

Over the 20th century, black Americans engaged in perhaps the largest natural experiment in MTO in US history. The Great Migration of African Americans out of the South into Detroit, Chicago, New York, Los Angeles, and hundreds of other cities across the north and west secured concrete gains for migrants that they reasonably believed would be enjoyed by future generations. Using a shift share instrument to isolate exogenous increases in the black population in northern urban commuting zones during the Great Migration, I show

[36]Hansen J statistic p-value of .15.

that racial composition changes during this period reduced the ability of northern cities to promote upward mobility in the long run and harmed black upward mobility in the North specifically.

My findings have implications for policies that incentivize families to move to areas with better opportunities and, in particular, the general equilibrium effects of scaling such a policy. In response to millions of black migrants moving North to improve economic outcomes, receiving northern cities changed in ways that eventually shuttered this pathway to black economic progress. In addition to better understanding the specific policies in locations that contribute to intergenerational mobility, more concerted efforts aimed at reducing disparities within locations, rather than relocating the disadvantaged, may be warranted.

Chapter 2

Mechanisms of urban decline in the wake of the Great Migration

2.1 Introduction & Data

Why did upward mobility decline in Great Migration destination commuting zones? This chapter explores mechanisms behind the transformation of the urban North during and after the Migration, with particular focus on characteristics of the urban environment that may affect children's long-run outcomes.

To identify potential mechanisms, I assembled a new database on commuting zones spanning the years 1920-2015. The database covers statistics on schooling, demographics, local government expenditures, incarceration, and crime, among other characteristics of commuting zones. I harmonized data from a variety of sources, including historical reports that I digitized from the US Department of Education's Biennial Statistics of Education reports, the FBI's Uniform Crime Reporting series, and the US Census Bureau's Financial Statistics of States and Local Governments.

Additionally I used the complete count censuses between 1920-1940 to construct additional measures of local government investments and incarceration rates and digitized special reports and tables from the 1940 and 1960 censuses on local county jail populations.

I supplemented these data sources with the City and County Data Books series and several county-level tabulated measures from 1970-2010 US Censuses. Finally, I used a pre-release of rich new data on incarceration from the Vera Institute of Justice, locating for each federal and state prisoner the county jail that committed them to federal and state prison. I provide details on the construction of this database, including data sources, and detailed definitions of key measures in Appendix B2.

2.2 Empirical strategy

The database allows me to evaluate what aspects of urban neighborhoods causally responded to the Migration by estimating the impact of predicted black population increases on outcomes before, during, and after the period of the Migration. I estimate the following:

$$M_{CZ}^{t} = \eta + \mu G\hat{M}_{CZ} + \mathbb{X}_{CZ}'\phi + \nu_{CZ} \tag{2.1}$$

where t refers to the period the mechanism is measured, and M refers to the mechanism of interest. I standardize all mechanism variables and scale the Migration shock $G\hat{M}_{CZ}$ so that the units are approximately one standard deviation (30 percentiles of predicted black population increase).

The results from this analysis reveal shifts in three areas dating back to the 1960s and 1970s that persist over the next several decades. First, I find increased white enrollment in private schools and declines in the number of whites living in urban neighborhoods in commuting zones that experienced greater in-migration. Second, I find that public spending in Great Migration destinations was reallocated towards policing. Third, some of the increase in police spending may be related to elevated crime rates in Great Migration cities, particularly in the late 1960s. During this period, a national crime wave and race riots struck cities throughout the US. I find that race riots were of greater intensity in Great Migration cities, lasting longer and involving more deaths, injuries, and arrests.

Section 2.3 briefly reports the specific findings for each mechanism. In section 2.3.1, I situate the results in the broader literature on the impact of education, segregation, crime

and criminal justice policies on children.

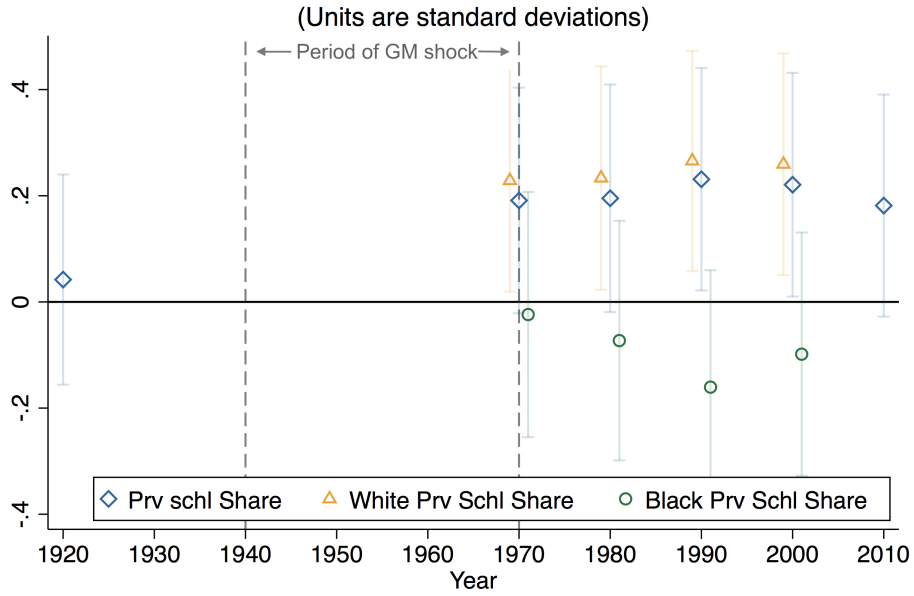
2.3 Results on mechanisms

Impact on private schooling and residential segregation

I begin by reporting results on private schooling and residential segregation. Figure 2.1 plots the coefficients on predicted black population increases on standardized measures of private school enrollment rates separately for each year that data are available. The outcome variables is the share of elementary and high school students enrolled in private school. Beginning in 1970, these measures are available separately by race. I find no impact of the 1940-1970 Migration shock on private school enrollment rates in 1920. In 1970, the next year that data are available,[1] a 30-percentile increase in the Great Migration shock is associated with a 0.2 standard deviation increase in the private school enrollment rate. This association appears driven entirely by white private school enrollment rates. The point estimate for black children's private school enrollment rates is negative and statistically insignificant.

[1]Starting in 1960, the Census began asking about the type of school households enrolled their children in; however aggregate statistics for children attending high school as well as breakdowns by race are only available through the *National Historical Geographic Information System* ("NHGIS") data consortium until 1970. See Appendix B2 for more details.

FIGURE 2.1: *Great Migration impact on private schooling*



Notes: This figure plots the coefficient on the instrument for black population increases during the Great Migration, in approximately one standard deviation units, in separate regressions for each year where the dependent variable is private school enrollment rates. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. A one standard deviation increase is approximately 30 percentiles. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Biennial Statistics of Education, 1920-1922; NHGIS county-level aggregates of elementary and high school enrollment by school type (public or private), 1970-2010. Instrument data sources: IPUMS complete count US census for 1940; Boustan (2016).

Consistent with Boustan (2010) and Tabellini (2018), I find that black population increases also predicts large declines in the urban white share at the commuting zone level. These results are shown in Appendix Figure B1. In Appendix Table B2, I show additional results on residential sorting: the long-run impact of the Migration shock on racial and income residential segregation across commuting zones. The results above are consistent with white flight from public schools and urban neighborhoods. I discuss the implications these findings may have for upward mobility of black and white children from low income families in Section 2.3.1.

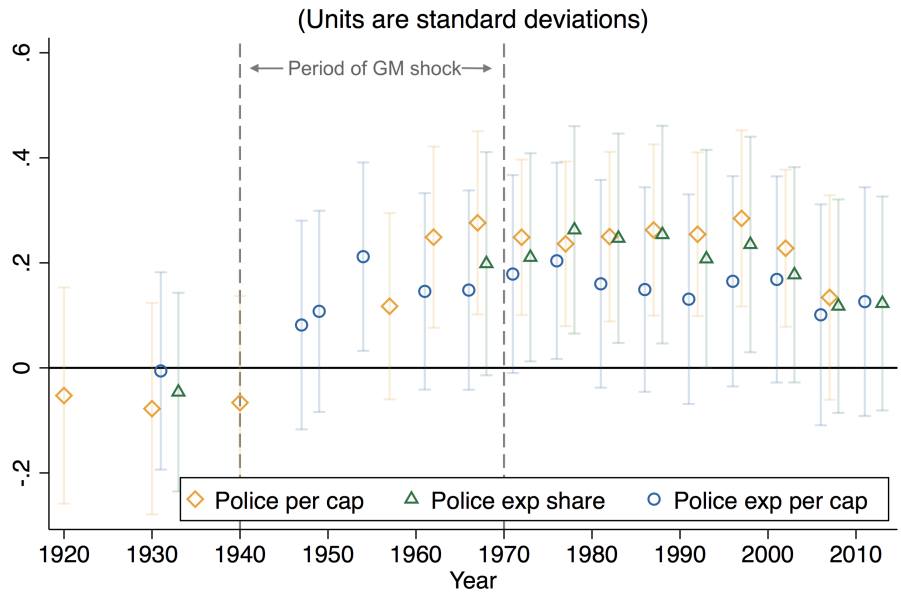
Impact on local government expenditures

Next I examine the impact of the Migration on the public spending patterns of local governments. I focus on categories of public expenditures over which sub-state governments have a large degree of discretion. Appendix Table F1 shows the contribution of different levels of government to each of several main categories of public expenditures. I focus on two categories in particular, police and school expenditures. Spending on police indicates levels of neighborhood safety and crime, but also may have direct effects on the outcomes of black male youth in particular, which I discuss further in Section 2.3.1. School spending has natural implications for the average outcomes of children in a given location.[2]

Figure 2.2 plots the coefficients on predicted black population increases on standardized measures of police investments separately for each year that the data are available. The outcome variables are police expenditures per capita, the share of local government expenditures on police, and police officers per capita. As can be seen in the Figure, the Migration had no statistically significant or large effects on pre-period police investments from 1920-1940. Starting after 1940, the association between the Migration and police spending increases, peaking in the late 1970s and persisting for several decades after. At the peak of the association between the Migration and police investments, a 1 standard deviation increase in the Migration shock increased the police expenditure share and police expenditure per capita by just over 0.2 standard deviations.

[2]See Section 2.3.1.

FIGURE 2.2: *Great Migration impact on policing investments*



Notes: This figure plots the coefficient on the instrument for black population increases during the Great Migration, in approximately one standard deviation units, in separate regressions for each year where the dependent variable is either the share of local government expenditures on policing, police expenditures per capita, or city police employees per 100k urban population. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. A one standard deviation increase is approximately 30 percentiles. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Data on police expenditure shares and per capita spending come from Financial statistics of states and local governments, 1932; US Census Bureau Annual Survey of Local Governments (1967-2012); police employees from City Government Employment and IPUMS complete count US censuses (1920-1940). Instrument data sources: IPUMS complete count US census for 1940; Boustan (2016).

I then look at the impact of the Great Migration on educational investments in affected commuting zones. These investments include direct educational expenditures by school districts, both as a share of all local government expenditures in commuting zones and per pupil. Appendix Figures B5 and B6 report these results. I estimate a noisy negative association between the Migration on pre-period (1932) aggregate educational expenditure shares. In B6, I control for 1932 educational expenditure shares and estimate the Migration's impact on post-1970 educational investments. I find no impact of the Migration on aggregate education expenditures at the commuting zone level in the post-Migration period. I discuss

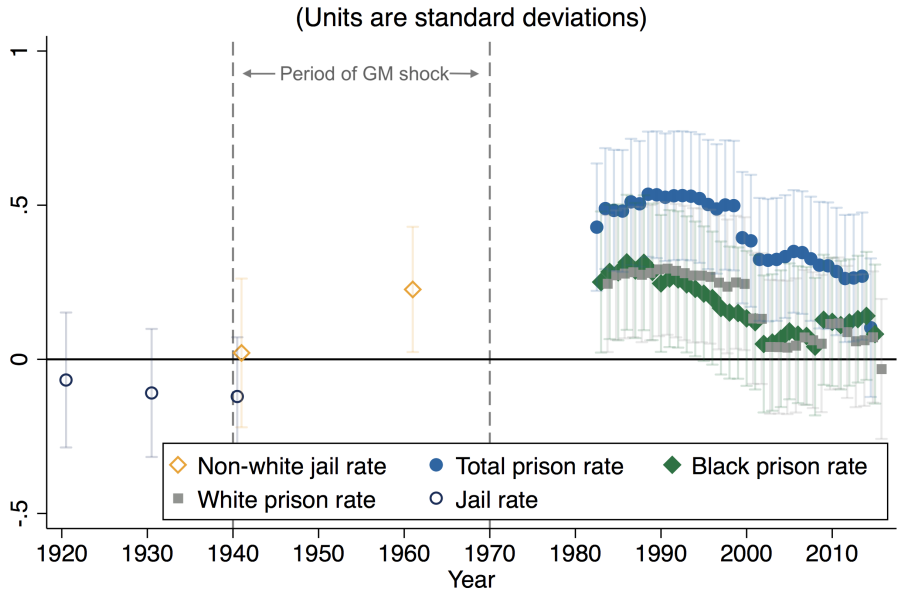
the implications of these findings in detail in Section 2.3.1.

To check whether the effect of the Migration on police expenditures is simply driven increases in municipal spending in Great Migration destinations, I estimate the impact of the shock on fire fighting expenditures. Appendix Figure B4 reports these results. I find no impact of the Migration on fire-fighting expenditures. Higher police expenditures may be associated with higher crime and incarceration rates. I investigate these below.

Impact on incarceration rates

Figure 2.3 plots the coefficients on predicted black population increases on standardized measures of incarceration separately for each year. The outcome variables are the local correctional institution population per 100,000, the non-white local correctional institution population per 100,000 of the non-white population, and the state and federal imprisoned population by commuting-zone-of-commitment per 100,000, for all individuals aged 15-64 and then separately for this group by race. As can be seen in the Figure, the Migration had no statistically significant effects on pre-period incarceration. The Migration is most strongly associated with incarceration in the 1980s and 1990s, during the rise of incarceration rates nationally.

FIGURE 2.3: *Great Migration impact on incarceration rates*



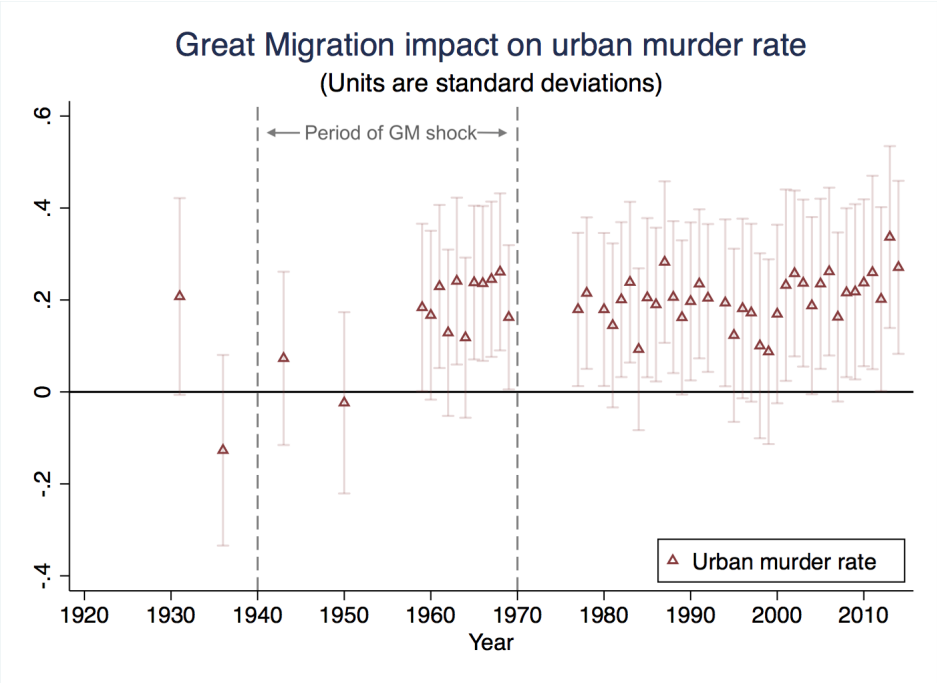
Notes: This figure plots the coefficient on the instrument for black population increases during the Great Migration, in approximately one standard deviation units, in separate regressions for each year where the dependent variable is county jail population per 100,000 (1940 and 1960) or federal and state prison population by 100,000 by county-of-commitment from 1983-2015. Each jail or prison population group is normalized by the population for that group. Federal and state prison rates are for black and white men aged 15-64. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. A one standard deviation increase is approximately 30 percentiles. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: 1940 county jail rates come from US IPUMS complete count US census; 1960 county jail rates come from 1960 Census report on county correctional institution population; data on 1983-2015 federal and state prison population by county-of-commitment come from Vera Institute of Justice In Our Backyards Database. Instrument data sources: IPUMS complete count US census for 1940; Boustan (2016).

In Appendix Figure B2, I report the impact of the Migration on the incarceration rate in levels. At the peak of the association between the Great Migration and black incarceration rates, in 1992, a 30-percentile increase in predicted black population increases was associated with 300 more black people per 100,000 being committed to federal and state prison. The impact for whites was an increase of approximately 30 per 100,000.

Impact on murder rates

Figure 2.4 shows the impact of the migration on standardized measures of murder rates between 1931 and 2015. A 30-percentile increase in the Great Migration shock is associated with 0.3 standard deviations higher murder rates in 1931, before the period of black population change predicted by the shock, but is not associated with higher murder rates in 1936 or 1943. Murder rates are not significantly associated with the Migration again until the late 1960s. In the post-1970 period, a 30-percentile increase in the migration shock is associated with a .5 standard deviation increase in murder rates. Controlling for the 1931 murder rate attenuates some of the impact of the Migration on post-1970 murder rates, but the effect on late 1960s murder rates remains positive and statistically significant.

FIGURE 2.4: *Great Migration impact on murder rates*



Notes: This figure plots the coefficient on the instrument for black population increases during the Great Migration, in approximately one standard deviation units, in separate regressions for each year where the dependent variable is urban murder rates per 100,000 in commuting zones. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. The instrument is the percentile of predicted black population increase, defined as the interaction between pre-1940 black southern migration patterns and post-1940 outflows of migrants as predicted by southern economic factors alone. A one standard deviation increase is approximately 30 percentiles. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Uniform Crime Reports. Instrument data sources: IPUMS complete count US census for 1940; Boustan (2016).

The late 1960s coincided with increases in the murder rate in cities across the US. At the same time, race riots erupted in urban areas as well. I explore whether the Migration affected the intensity of these riots. Table 2.1 reports these results. I find that Great Migration destination cities experienced longer riots and that riots in these areas involved more deaths, injuries, and arrests than places with fewer black migrant inflows. The magnitude of the effect of the Migration on arrests is large: a 30-percentile increase in the Migration shock is associated with over 30 more arrests per 100,000 during the 1960s riots. Both of these events may have contributed to rising police investments during this period. Both the impact on police expenditures and incarceration rates appear to have persisted for several

decades afterwards. I discuss the implications of this and the other findings reported above in the sections below, first beginning with the results on private schooling and residential segregation.

TABLE 2.1: *Great Migration impact on 1960s riots*

	Ordinary Least Squares					
	Killed	Arson	Arrests	Days of Riots	Injured	Riots
	Per 100k	Per 100k	Per 100k	Per 100k	Per 100k	Per 100k
GM	0.00266***	0.188***	1.026***	0.0255***	0.175***	0.0103***
	(0.000426)	(0.0243)	(0.119)	(0.00381)	(0.0254)	(0.00151)
R-squared	0.296	0.397	0.583	0.290	0.423	0.292
	Reduced Form					
GM Shock	0.00117**	0.0661**	0.446***	0.0155***	0.0991***	0.00654***
	(0.000531)	(0.0282)	(0.138)	(0.00523)	(0.0311)	(0.00207)
R-squared	0.307	0.484	0.646	0.150	0.454	0.162
	Two-stage least squares					
GM	0.00300**	0.169**	1.138***	0.0395***	0.253***	0.0167***
	(0.00131)	(0.0702)	(0.351)	(0.0116)	(0.0784)	(0.00461)
R-squared	0.310	0.477	0.627	0.317	0.430	0.317
N	130	130	130	130	130	130
Mean Dep Var	0.0589	4.697	24.91	0.950	4.474	0.413
SD Dep Var	0.162	9.952	58.87	1.437	10.65	0.572
SD GM	28.98	28.98	28.98	28.98	28.98	28.98

Notes: This table reports the estimated impact of the Great Migration on 1960s race riots and riot severity. Dependent variables in columns 1-5 are individual measures of the severity of riots, including number of individuals killed, number of arson incidents, number of arrests, the duration of the riot in days, number of injuries; the final column is total number of riots. All outcomes are normalized by the total CZ population in 1960 and multiplied by 100,000, so they are in per 100,000 of the population units. Independent variable is black population increase between 1940 and 1970. The instrument for black population increase is the predicted black population increase through variation in black southern migration alone. OLS, Reduced Form, and 2SLS estimates are reported. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: Collins and Margo (2007). Great Migration data sources: CCDB, IPUMS complete count US census for 1940; Boustan (2016).

2.3.1 Discussion

Education spending, school quality, and residential segregation

Two prominent explanations for local differences in children's outcomes and racial inequality in outcomes are school quality and segregation. Recent work by Jackson *et al.* (2015) has shown that school spending can improve adult earnings, and Card *et al.* (2018) show that in the early 20th century, improved school quality improved educational attainment of children and educational upward mobility.

Aggregate differences in school spending across commuting zones do not appear to be a mechanism for the impact of the Great Migration on upward mobility in destination cities. I find no impact of the Migration on educational expenditures per capita or on the share of total spending by local governments devoted to education. Further, if there were aggregate reductions in school spending, one would expect to see worse outcomes for white men and women and black women from low income families growing up in these locations. However, I find no negative impacts of the Migration on these subgroups. The results are consistent with Rothstein (2018), who finds that differences in school quality can explain very little of the variation in average upward mobility across commuting zones (roughly eleven percent).

Still, aggregate measures of school spending may mask differences across individual school districts within commuting zones. I find that a higher fraction of white children are enrolled in private schools in Great Migration destination cities. Private school enrollment rates tend to be higher in urban areas, so these results are suggestive of lower school quality in urban school districts. If school spending decreased in urban school districts, which serve more minority student populations, and simultaneously increased in suburban school districts, these two effects could cancel each other out at the commuting zone level. Further analysis utilizing individual school district data is needed to test whether this reallocation within commuting zones explains the null results on education.

Residential racial segregation has been shown to negatively impact black children's outcomes, as well as black-white differences (Ananat, 2011; Cutler and Glaeser, 1997). Further, segregation is a major correlate of average upward mobility in counties and

commuting zones (Chetty *et al.*, 2014; Chetty and Hendren, 2018b; Andrews *et al.*, 2017). I find evidence that the Migration increased increased segregation and that destination commuting zones are more segregated today. A large literature in sociology examines the confluence of several factors negatively correlated with black children's outcomes in racially segregated neighborhoods, including the prevalence of single parent families and high crime rates (Wilson, 2012; Massey and Denton, 1993).

My findings are strongly consistent with the existing evidence linking residential racial segregation with worse outcomes for black children. Lower marriage rates and higher single parent families in segregated areas also help explain why the Migration is positively associated with the individual earnings of black women. In areas with lower marriage rates, more black women may be in the labor force and working more. Still, the particularly negative effects on black men's upward mobility warrant a look into facets of neighborhoods and local policy that may disproportionately affect black men.

Policing, crime, and incarceration

How might higher policing, crime, and incarceration affect upward mobility for black men? Several studies have shown that exposure to crime increases individual criminal behavior, which has consequences for one's probability of incarceration and traditional employment (Case and Katz, 1991; Damm and Dustmann, 2014; Heller *et al.*, 2017; Sviatschi, 2018). Crime and residential racial segregation are highly correlated across urban areas, which suggests that black children are disproportionately exposed to crime and violence compared to white children growing up in the same commuting zones. Childhood exposure to higher crime rates may thus directly reduce black men's income upward mobility relative to white in Great Migration cities.

A growing literature, however, suggests that polices used to curb crime may also have direct exacerbating effects on racial inequality. A recent paper by Liu (2018) examines the impact of incarceration of black men on black women's marriage outcomes and family structure for black children using variation in federal and state sentencing policy from

1986 to 2009. Incarceration lowers the marriage rate for black women and increases black children's likelihood of being born out of wedlock and living in single parent households. The author further finds that incarceration increases the gap in upward mobility between black and white men. The results are consistent with incarceration being a mechanism for the Great Migration's impact on black men's upward mobility through incarceration of the father generation. Further Liu (2018) finds that higher incarceration rates increase black women's probability of employment. These results are consistent with shocks to black men's incomes having an income effect on black women's labor supply.

Legewie and Fagan (2018) find that a policy increasing police activity in New York City had negative effects on test scores and school attendance of black male teenagers. The mechanism appears to be increased police stops and arrests of black male teens, which disrupted their education. Reductions in crime were small in magnitude, suggesting that police activity may have had a net negative impact on black boys' outcomes in this case. Ang (2018) finds that police shootings of civilians lowered educational and behavioral outcomes of students in Los Angeles, leading to reduced high school graduation and college attendance rates. Dobbie *et al.* (2018) find that parental incarceration increases teen crime and pregnancy and lowers subsequent employment for youths from disadvantaged families.

Another way that reallocation of public spending to policing may affect black men's upward mobility is through diverting resources from other uses. Jackson *et al.* (2015) find that school spending has positive effects on the adult income of children, and low income children in particular. Despite a likely increase in the share of children who would benefit from additional educational expenditures, local governments did not increase educational expenditures in response to the Great Migration. Rising crime rates or the perception of rising crime rates in the late 1960s may have led governments to specialize instead in police protection, and this allocation of public spending persisted for several decades since.

2.4 Conclusion

This chapter explores the mechanisms through which the urban North declined in the wake of the Great Migration. To do so, I collected and harmonized data on schooling patterns, local government expenditures, and crime and incarceration rates for destination commuting zones from 1920 to 2015.

In response to mid-century changes in the racial composition of northern cities, white families withdrew from shared public schools and urban segregation increased. Starting in the 1960s, the quality of the urban environment sharply deteriorated. Local governments in Great Migration destinations increased public spending on police in both absolute and relative terms, a reallocation possibly driven by increases in crime or in response to race riots in the late 1960s. These locations remained differentially invested in policing over the next several decades, potentially crowding out investments in, for example, education for an increasingly disadvantaged urban population.

At the time that prison populations began to rise dramatically in the US in the 1980s and early 1990s, places with larger increases in their black population during the Great Migration sent substantially more of the black population to federal and state prison. The timing of the effect of the Migration on incarceration rates suggests that parents of children born in the 1980s would be most affected. Many studies suggest that incarceration reduces black employment prospects and increases the prevalence of single-parent families, effects that may propagate to future generations.

Further research will have to separately assess the long-run impact that increased crime, the race riots of the 1960s, and city policy responses to each have had on black men's outcomes. A key question is whether alternative policies can reduce racial inequality in upward mobility given the sizable gaps under the existing set of policies.

Chapter 3

Minimum wages and racial inequality[1]

3.1 Introduction

A sizable 25% gap exists between the average annual earnings of white and black workers in the US today.[2] Over the last 70 years, the racial earnings gap fell only once, during the late 1960s and early 1970s, shrinking by a factor of two from 50% to 25%. What made the white-black earnings gap fall? Understanding the factors behind this historical improvement may provide insights for reducing the large racial disparities that continue to exist today (Bayer and Charles, 2018; Chetty *et al.*, 2018).

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 3.2 below).

[1] Co-authored with Claire Montialoux.

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

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 the reform had large positive effects on wages for low-wage workers, and that the effects were more than twice as large for black workers as for white. Our estimates suggest that the 1967 extension of the minimum wage can explain more than 20% of the decline in the racial earnings gap during the late 1960s and early 1970s. Moreover, we find that this reform did not have detectable adverse employment effects on either black or white workers. The extension of the minimum wage thus not only reduced the racial earnings gap (the difference in earnings for employed individuals) but also the racial income gap (the difference in income between black and white individuals, whether working or not). 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 BLS 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 industries covered in 1938 (our control group) evolve in parallel to the annual earnings of workers in the industries covered in 1967 (our treated group) before the reform. In 1967, they jump by 6% relative to the control industries and the effect is permanent through the late 1970s.

The magnitude of the wage increase is consistent with the predicted 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 employ-

ment. Using our BLS data, we implement a "bunching estimator" (following Harasztosi and Lindner, Forthcoming). 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 using a second research design. Just as today, some states had their own minimum wage laws (on top of the federal minimum wage) in the 1960s while others did not. This variation made the 1967 reform more or less binding across states. We build a minimum wage database by state, industry, and gender spanning the 1950-2016 period. We compare states without a state minimum wage law as of January 1966 (strongly treated) to other states (weakly treated). Because the federal minimum wage was high in the late 1960s (much higher than today relative to the median wage), the 1967 reform is a particularly large shock in the strongly treated states. In this research design, the 1967 reform has a precise zero effect on employment. We are able to rule out employment elasticities 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 legislation when governments are concerned about forms of inequality that cannot be addressed directly through income-based tax and transfer policies.

The remainder of the paper is organized as follows. We start by relating our work

to the literature in Section 3.2. Section 3.3 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 3.4 and its effects on employment in Section 3.5. Section 3.6 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 3.7 concludes.

3.2 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.

3.2.1 Literature on Racial Inequality and the Civil Rights Movement

A large body of work seeks to understand what caused the decline in the racial earnings gap during the Civil Rights Era, a period that saw major policy and economic changes. Two types of explanations have been put forward: changes in the demand side of the labor market vs. changes in the supply side.

Demand side of the labor market. A cornerstone of the Civil Rights movement was the introduction of federal anti-discrimination policies. Title VII of the 1964 Civil Rights Act prohibited both employment and wage discrimination based on race.[3] It was enforced by the Equal Employment Opportunity Commission (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,

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

[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.

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]

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

[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.

(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 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 3.1a 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 two yeas: 1967 and 1968. Neither the demand nor supply factors described above can easily explain the specific timing of the reduction in the racial earnings gap. Anti-discrimination

[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.

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 3.1b shows indeed that the unadjusted racial earnings gap fell sharply in the newly covered industries relative to the previously covered ones precisely in 1967.

3.2.2 Minimum Wage Literature

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

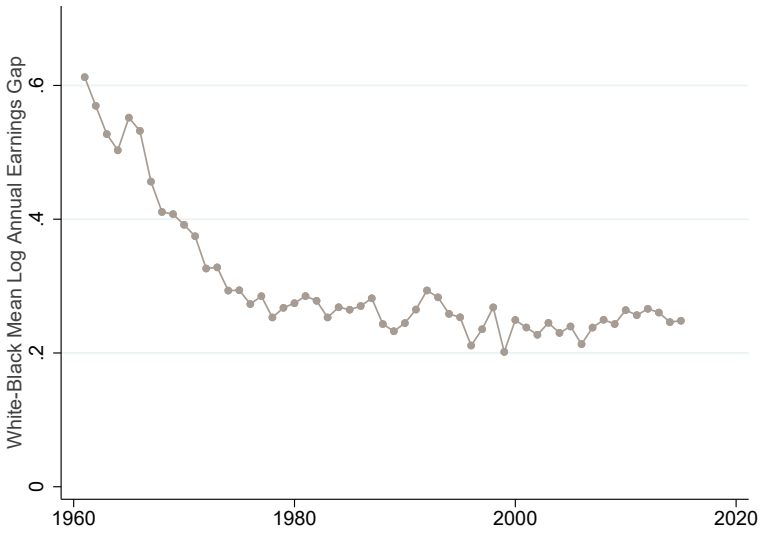
First, our study is the first to provide causal evidence on how minimum wage policy can affect racial economic disparities. A large body of work discusses the efficiency costs of the minimum wage and focuses on its employment effects (see, e.g., Card, 1992; Card *et al.*, 1993; Neumark and Washer, 1992; 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 to date the interplay between the minimum wage and racial inequality has not been investigated in a

[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.

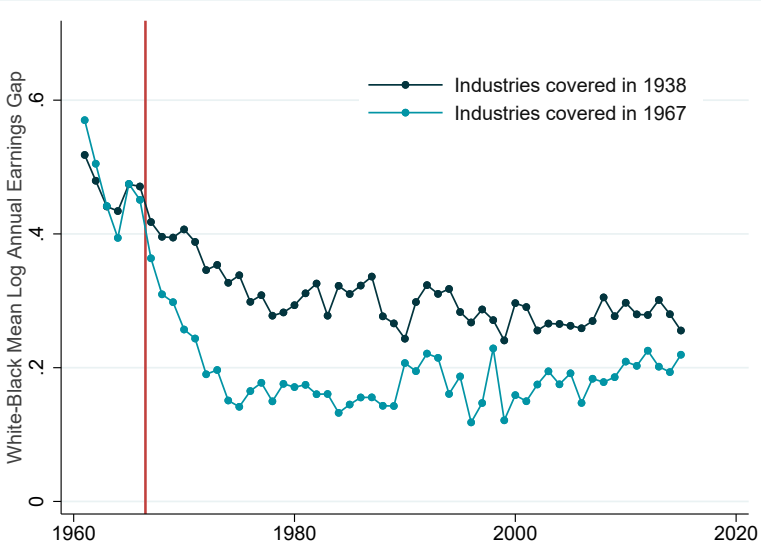
[11]Medicare and Medicaid were introduced in 1966, but were initially small quantitatively (1.7% of all government transfers in 1966) before gradually increasing to 4.8% of all transfers in 1970, 6.4% in 1975, and 8.2% in 1980. See table II-C3b in Piketty *et al.* (2018). Food stamps was introduced in 1964, the rolled out across counties. It's only in 1975 that all counties were mandated to offer a food stamp program (Hoynes and Schanzenbach, 2009). Aid to Families with Dependent Children (AFDC) expanded cash benefits in early 1970s (of Health & Human Services, 2001). Taken together, all transfers accounted fo 24% of the national income per adult in 1961, 24% in 1966, and 28% in 1970, 32% in 1975. See table II-C3b in Piketty *et al.* (2018).

Figure 3.1: *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.

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). 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.

As 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) from Bailey *et al.* (2018), 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, Forthcoming; 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

[12]By contrast, the bulk of the literature has focused on teen employment or workers in specific industries,

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).

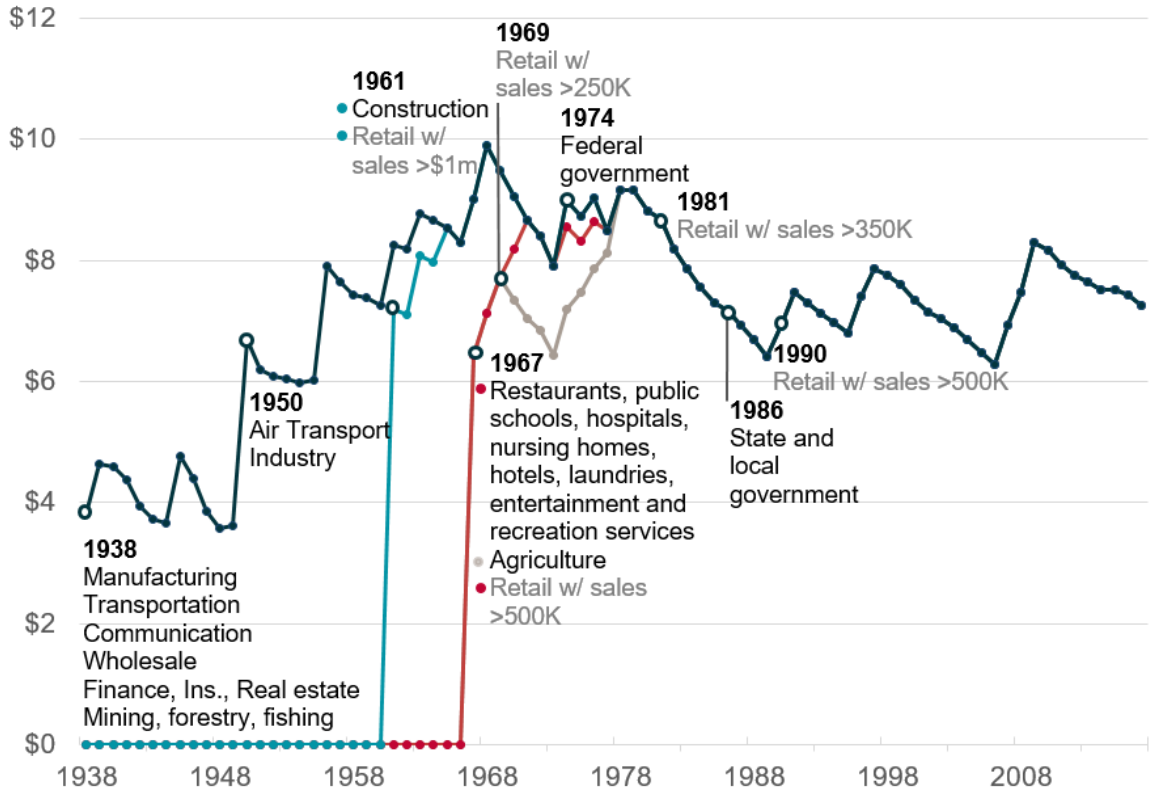
3.3 The 1967 Extension of the Minimum Wage and Data

3.3.1 The 1966 Fair Labor Standards Act

Political economy of the reform. The Fair Labor Standards Act (FLSA) of 1938 introduced the federal minimum wage in the United States. Millions of workers became subject to a wage floor. The coverage of the Act, however, was incomplete: a number of sectors were excluded. The 1938 FLSA covered about 53% of the U.S. workforce (see figure 3.3) in the manufacturing, transportation and communication, wholesale trade, finance and real estate sectors (see the complete list of covered sectors in figure 3.2). President Roosevelt intended to cover the economy as a whole but faced resistance in Congress, particularly from Southern Democrats (Phelps, 1939). The law enacted in 1938 stipulates that only employees engaged in interstate commerce or the production of goods for interstate commerce be covered (Daugherty, 1939). In practice, this meant that a number of sectors where black workers were overrepresented, such as agriculture, were excluded. The 1938 Fair Labor Standards Act, as a number of other programs passed in the 1930s and 1940s, had a discriminatory dimension (Katznelson, 2006; Mettler, 1994; Rothstein, 2017).

typically restaurants (Abowd *et al.*, 2000; Allegretto *et al.*, 2017; Neumark *et al.*, 2014).

Figure 3.2: *Expansions in minimum 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 C1. 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, DOL.gov.
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).

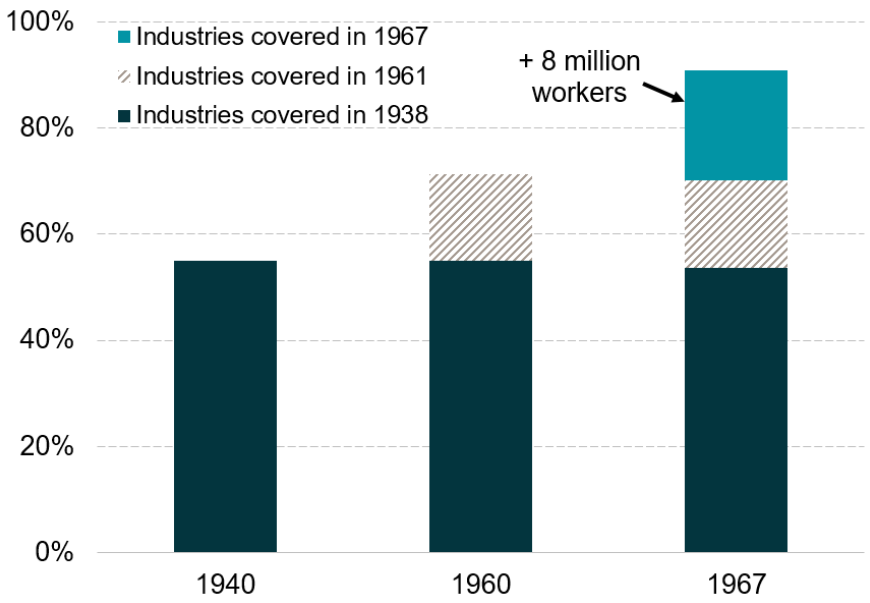
Over time, a series of amendments to the 1938 FLSA extended the minimum wage to the rest of the economy. In this paper, we focus on the 1966 FLSA amendments, the largest expansion of the federal minimum wage.[13] The 1966 FLSA amendments introduced the federal minimum wage (as of February 1st, 1967) in the following sectors: agriculture, nursing homes, laundries, hotels, restaurants, public schools, and hospitals. These sectors employed about 8 million workers (see figure 3.3) 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 3.5). 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 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.

[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.

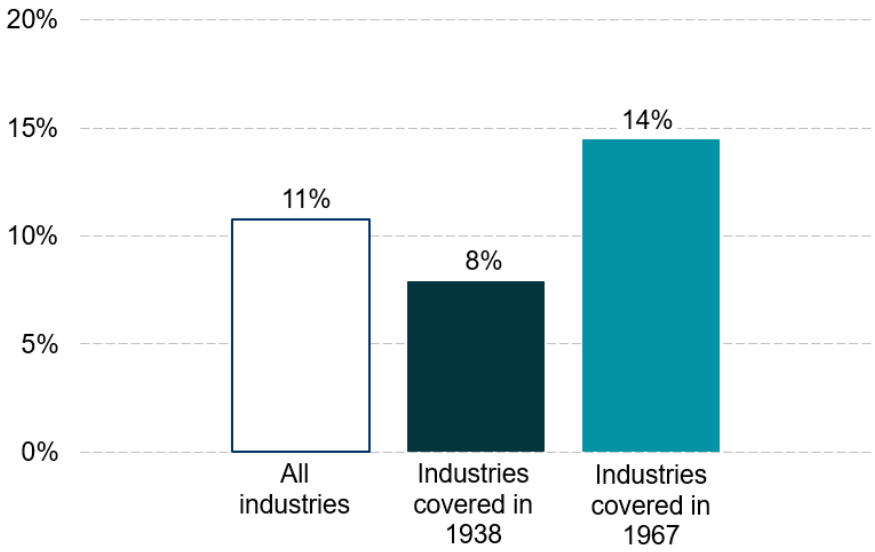
[14]In all sectors except agriculture, the minimum wage was introduced at $1 an hour in February 1967. Then the minimum wage was raised annually in 15 cent-an-hour increments, effective each February 1 through 1971, to $1.60 an hour.

Figure 3.3: *Share of workers covered by the minimum wage, 1940-1966*



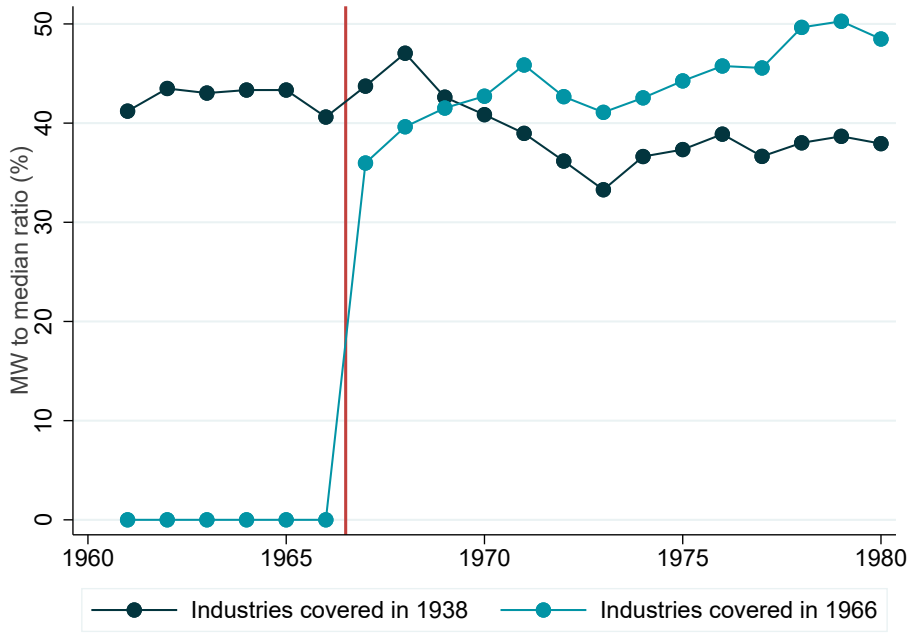
Sources: US Censuses 1940 and 1960. March CPS 1967.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.
Notes: Coverage by federal minimum wage.

Figure 3.4: *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 3.5: *Minimum wage 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.

3.3.2 Data Used in our Analysis

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

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). Figure 3.6a 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. 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;[16] see figure 3.6b.

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. 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

[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.

[16] 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.

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 variable includes 23 different industries.[17] This is more detailed than the 2-digit NAICS code but a bit coarser than the 3-digit NAICS code. For instance, we are able to separate restaurants from the rest of the retail sector, but we cannot separate hotels and lodging places from laundries and other professional services due to data limitations in the 1962-1967 CPS. The BLS industry wage reports have hourly wage information for more detailed sectors.

There are three main limitations involved in using March CPS data to analyze the 1967 reform:

First, we only directly observe annual earnings in the CPS files of the 1960s and early 1970s, not hourly wages.[18] 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,[19] which increases the level of noise compared to more recent years. There is a difference in em-

[17] 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 IPUMS.org.

[18] 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.

[19] About 15,000 observations in our sample in March CPS 1962-1965, then around 30,000 through the mid-1970s, see table C3.

ployment *counts* between the 1960 decennial Census data and the early CPS files. However, conditioning on being employed, annual earnings in March CPS and Census are perfectly in line (see figure C3)[20] 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.

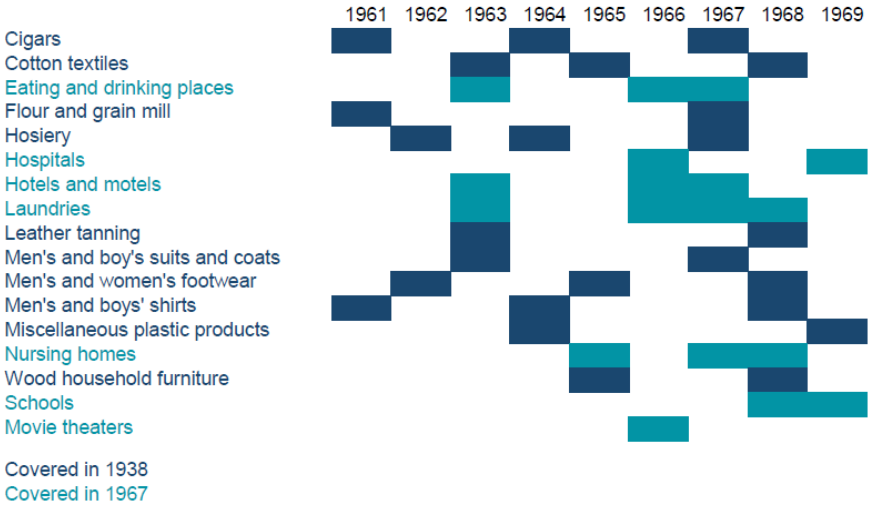
[20]Table C3 shows that we estimate the number of employed persons in March CPS 1962 and 1963 in our sample is lower (average of 23,181,837 over the those two years) than the estimate we get in 1960 in Census data (33,244,820). Starting in March 1964, the *numbers* of people employed is in line with Census data. The black-white and the men-women employment shares however are similar in March CPS 1962 and 1963 and Census 1960.

Figure 3.6: *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	4.0	4.4	4.0	5.2
$1.80 and under $1.85	2.0	2.6	1.8	2.2	2.6	2.1	2.6	3.0
$1.85 and under $1.90	2.0	2.9	1.7	2.4	3.0	2.2	2.5	3.2
$1.90 and under $1.95	1.4	2.5	1.0	1.6	2.4	1.4	1.7	2.3
$1.95 and under $2.00	1.1	1.7	.9	1.3	1.5	1.2	1.4	1.4
$2.00 and under $2.10	3.6	6.6	2.7	4.9	9.4	3.5	4.9	5.9
$2.10 and under $2.20	1.9	4.0	1.3	2.2	4.4	1.5	2.6	2.9
$2.20 and under $2.30	1.7	3.9	1.1	2.0	4.7	1.2	2.4	2.6
$2.30 and under $2.40	1.1	2.4	.7	1.4	2.8	1.0	1.5	1.9
$2.40 and under $2.50	.9	2.0	.5	.9	1.9	.6	1.4	1.4
$2.50 and under $2.60	1.7	4.6	.9	2.0	4.9	1.2	2.9	3.4
$2.60 and under $2.70	.8	2.1	.4	.8	1.9	.5	1.4	1.3
$2.70 and under $2.80	.7	1.9	.4	.7	2.0	.3	1.5	1.3
$2.80 and under $2.90	.5	1.7	.2	.5	1.3	.3	.9	1.0
$2.90 and under $3.00	.2	.6	.1	.5	1.3	.2	.4	.6
$3.00 and over	2.4	8.3	.8	3.5	11.1	1.0	4.0	5.5
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Number of workers	440,779	99,165	341,614	441,931	99,702	342,229	107,063	109,859
Average hourly earnings[1]	$1.55	$1.91	$1.44	$1.67	$2.04	$1.56	$1.77	$1.88

(b) *Set of industries we digitized and years available*



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. These tabulations provide information on the hourly wage distribution by 5- or 10-cent bins. 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.

Table 3.1: *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	0.90	0.10	0.68	0.32	37,176	19,523	42,029	21,113
1963	9,638	22,277,274	0.90	0.10	0.68	0.32	37,607	18,865	42,412	21,267
1964	14,222	34,344,403	0.89	0.11	0.68	0.32	38,736	21,529	44,216	21,343
1965	14,126	34,637,727	0.89	0.11	0.68	0.32	39,677	22,997	45,379	22,158
1966	30,113	37,407,666	0.89	0.11	0.68	0.32	41,196	23,168	47,224	22,461
1967	19,191	38,490,848	0.89	0.11	0.68	0.32	42,575	24,522	49,036	23,091
1968	30,277	39,451,389	0.89	0.11	0.66	0.34	43,219	26,019	50,127	24,098
1969	30,808	40,044,846	0.89	0.11	0.66	0.34	44,575	28,242	52,070	24,935
1970	29,626	40,963,562	0.90	0.10	0.66	0.34	47,062	29,253	55,248	26,015
1971	29,130	40,594,657	0.89	0.11	0.65	0.35	47,563	30,486	55,870	26,946
1972	28,214	41,861,238	0.90	0.10	0.65	0.35	47,460	30,936	55,969	27,039
1973	28,025	42,659,268	0.89	0.11	0.64	0.36	49,744	33,601	59,060	28,255
1974	27,620	43,773,753	0.90	0.10	0.64	0.36	49,962	33,810	59,852	28,155
1975	26,474	43,108,371	0.90	0.10	0.63	0.37	48,364	34,284	58,235	27,912
1976	28,407	44,987,015	0.90	0.10	0.62	0.38	47,557	33,346	57,386	27,866
1977	33,944	46,526,101	0.90	0.10	0.61	0.39	48,197	34,215	58,382	28,390
1978	33,936	48,250,592	0.89	0.11	0.61	0.39	48,588	34,812	59,187	28,665
1979	34,468	50,109,925	0.90	0.10	0.60	0.40	48,789	36,335	59,923	29,044
1980	41,137	51,461,168	0.90	0.10	0.58	0.42	48,862	36,004	60,306	29,636
1981	41,859	53,389,185	0.90	0.10	0.58	0.42	47,624	34,640	58,541	29,490
US Census										
1960	1,662,241	33,244,820	0.90	0.10	0.69	0.31	35,857	19,429	40,231	20,684
1970	403,015	40,301,500	0.90	0.10	0.65	0.35	46,243	30,102	54,341	26,724
1980	2,613,374	52,267,480	0.89	0.11	0.58	0.42	46,870	36,367	57,205	29,905

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

Sample: Adults 25-55, 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.

Third, from 1968 to 1976, the IPUMS data report information by state groups as opposed to states. We have information for 21 state groups across all years. The states that were grouped together were small (e.g., large states such as California and New York are always one single state) and geographically close to each other (see figure C2). We checked that the borders of the state groups do not cross region or division lines. Importantly, the states within each group have similar state minimum wage policies. Thus this data limitation is unlikely to be a threat to our cross-state empirical strategy. In our analysis using CPS data, for simplicity we use the term "states" to refer to "state groups."

U.S. Census data. We use the 1-100 national random sample of the population from the 1940, 1950, 1960, 1970, and 1980 decennial censuses to compute the share of workers covered by the Fair Labor Standards Act of 1938 and its subsequent amendments.[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. We cross-check the information in the Minimum Wage Study Commission (1981) with the information contained in the Department of Labor Handbook on women workers (1965). In 1965, 31 states and the District of Columbia had minimum wage laws. Details are provided in the appendix.

3.4 The Wage Effects of the 1967 Reform

3.4.1 Identification Strategy, Sample, and Summary Statistics

We start by studying the effect of the 1967 extension of the minimum wage on the dynamics of wages in the CPS. Our baseline empirical approach is a cross-industry difference-in-

[21] Census data were accessed from the IPUMS website, with variables—in particular the industry variable—harmonized with the CPS files, see Ruggles *et al.* (2018).

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 3.7). 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, nor in the share of black and white workers in those groups; see figure **??**. 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. Workers younger than 21 were subject to a different, lower minimum wage that is not the focus of our study. We also exclude the self-employed, workers in group quarters, unpaid family workers, and individuals working less than 13 weeks a year and less than 3 hours a week (to remove noise generated by very low annual earnings). Throughout the analysis, control industries include all industries that were covered in 1938 (that is, we exclude from the analysis the industries added in 1961, 1974, and 1986, which together employed about 25% of the workforce, see C1). As shown by table 3.2, 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 3.3 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

Table 3.2: *Wage effect: Main results and robustness checks*

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

Source: March CPS 1962-1980.
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.

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 workers that are full-time full-year was higher in the treated industries (88% for white and 79% for black workers) than in the control industries (69% for white and 67% for black workers).

We estimate the following difference-in-differences model:

$$\log w_{ijst} = \alpha + \sum_{k=1961}^{1980} \beta_k \text{Covered 1967}_j \times \mathbb{1}[t = k] + \delta_j + \delta_t + \mathbb{X}'_{ijst}\Gamma + \varepsilon_{ijst} \tag{3.1}$$

where $\log w_{ijst}$ denotes the log annual earnings of worker i in industry j, state s, in year t.[22] The dummy variable Covered 1967_j equals 1 if worker i works in an industry covered in 1967, 0 if they work in an industry covered in 1938. t is the year when the reform was implemented (1967), and δ_j and δ_t are industry and year fixed effects, respectively. The coefficient of interest, β_k, measures the effect of the 1967 reform k years after the baseline year (1965 in what follows). In all our analyses, we control for the following worker-level characteristics contained in the vector $\mathbb{X}_{ijst}$: gender, race, experience, experience squared, education, occupation, marital status and part-time or full-time status. We also control for the number of weeks worked,[23] and the number of hours worked.[24] In section 3.5 below, we show that the reform did not affect the number of hours worked per year conditional on working (see figure 3.11a and table 3.7).[25]

More generally, adding individual-level controls doesn't affect our results suggesting that there is no sorting on observables (see figure C4). It increases however the precision of our estimates.[26] We report standard errors clustered at the industry level to allow for

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

[23]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.

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

[25]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\$).

[26]Adding or not individual-level controls doesn't affect at all our medium-run point estimates as shown on figure C4. Starting in 1971, the point estimates with all the individual-level controls are slightly higher than the point estimates in our baseline specification. One possibility is that the extension of the minimum wage has a positive effect on the number of years of schooling in the medium and long-run.

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).

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

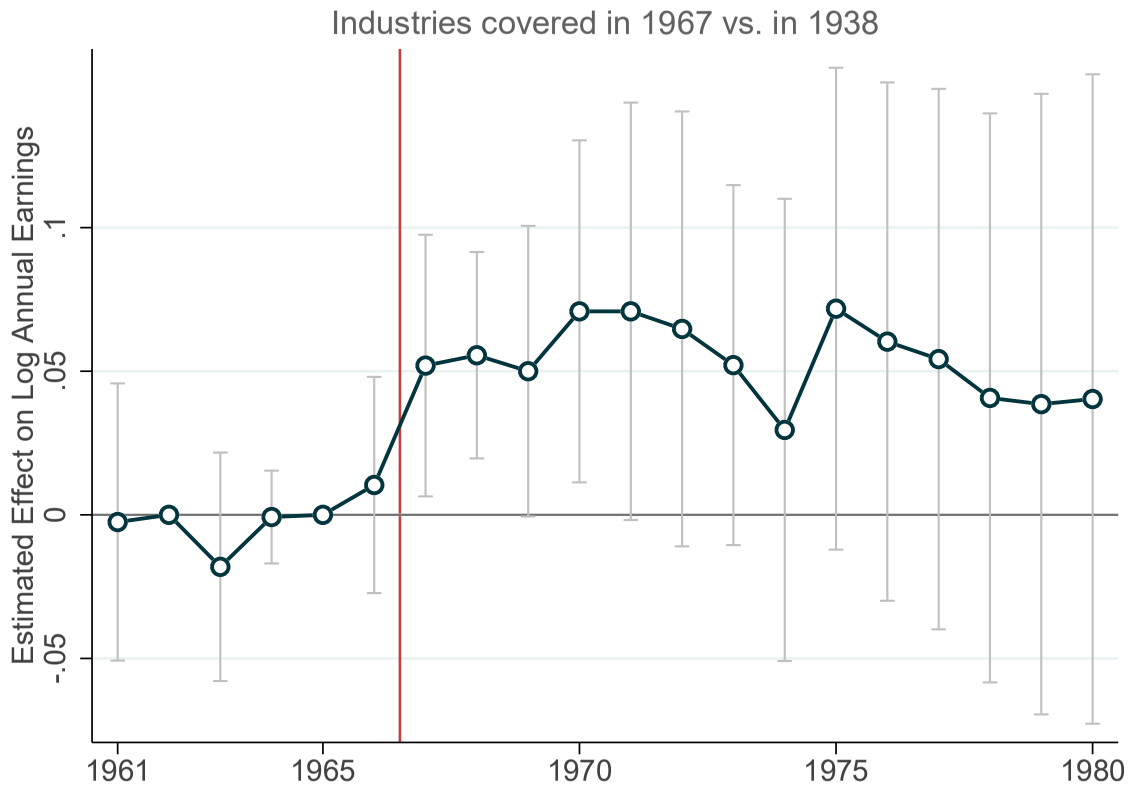
Figure 3.7 shows the effect of the 1967 reform on the log annual earnings of treated workers relative to control workers. Before the implementation of the reform in February 1967, the annual earnings of workers in the treated vs. control industries evolved in parallel: the point estimates for the years 1961-1966 are centered around 0 and are not statistically different from 0.

Table 3.3: *Workers' characteristics, 1965-66*

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

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

Figure 3.7: *Impact of the 1966 FLSA on annual earnings*



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.

Starting in 1967, annual earnings increased substantially—by about 5%—for workers in the newly covered industries relative to workers in the control industries. Relative wages continued to increase after 1967 through to 1971 when the treatment effect peaks (+7%). This pattern of increase is consistent with the fact that in the newly covered industries, the minimum wage was first introduced in 1967 at a level ($1 in nominal terms) below the prevailing federal minimum wage ($1.25), before gradually converging to the level of the federal minimum wage over the 1967-1971 period (except in agriculture); see figure 3.2. 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 3.2, 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 footnote 25 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 3.4). For these workers, the average increase resulting from moving straight to the $1 nominal minimal wage introduced in 1967 is 34%; see column (2). The predicted wage effect 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.[27] The predicted wage

[27]Since we make predictions for 1967 alone, we compare the predicted effects to our wage coefficient obtained for 1967 alone (see figure 3.7 rather than to the pooled estimate for 1967-1972 presented in table 3.2).

effect is slightly larger than the observed effect (5.5% vs. 5.3%), however, which could be due to several factors. There is measurement error in hourly wages, there may be imperfect compliance with the reform, and there may be effects of the reform on employment. We explore the latter ones in section 3.5.

Effects by education. The wage effect shows up primarily where one would expect to see it, i.e., for workers with low levels of education. We separately estimate the above model for workers with 11 years of schooling or less vs. more than 11 years of schooling; see figure 3.8a. 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. These estimated effects are well in line with our predictions, as shown in table 3.4.

Wage effects using hourly wage BLS data. We confirm our wage results using the BLS industry wage reports instead of the CPS data. We implement the same cross-industry difference-in-differences research design: we compare the dynamics of wages in the newly vs. previously covered industries, before and after 1967. Control industries here include non-durable manufacturing industries (see figure 3.6b for the list of industries we digitized and years available), which were covered by the minimum wage in 1938.[28] We adapt our cross-industry design to the nature of the BLS data, and estimate two models: (i) a similar difference-in-differences model as described in equation 3.1; and (ii) a triple

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

Table 3.4: *Predicted wage effect*

	(1)	(2)	(3) = (1) × (2)	(4)
	Share of workers at or below the MW (%)	Avg increase in earnings for MW workers (%)	Predicted increase in earnings (%)	Estimated increase in earnings (%)
All	16.1	33.5	5.4	5.3
By education				
Low-education	31.4	33.0	10.4	10.0
High-education	9.6	34.2	3.3	2.5
By race				
Black	28.8	38.2	11.0	8.0
White	13.9	32.0	4.5	4.3

Source: March CPS 1962-1980.
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.

difference-in-differences model defined as follows:

$$\begin{aligned} y_{jrt} = \alpha &+ \beta_1 \text{Covered 1967}_j \times \text{Post}_t \times \text{South}_r \\ &+ \beta_2 \text{Covered 1967}_j \times \text{Post}_t + \beta_3 \text{Post}_t \times \text{South}_r \\ &+ \beta_4 \text{Covered 1967}_j \times \text{South}_r + \nu_j + \eta_r + \lambda_t + \varepsilon_{jrt} \end{aligned} \tag{3.2}$$

where y_{jrt} denotes log hourly wages in industry j, region r, and year t; Covered 1967_j indicates whether an industry was covered in 1967; ν_j, η_r, and λ_t are industry, region, and year fixed effects. Our standard errors are clustered at the industry level. In addition, $\hat{\beta_4}$ in this specification allows us to investigate whether the wage effects are larger in the South. 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 3.5 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.

Table 3.5: *Hourly wage effect using BLS data*

	Model 1		Model 2	
	Full sample	Strict sample	Full sample	Strict sample
1967-1969	0.110***	0.112***	0.089***	0.081***
	(0.034)	(0.027)	(0.029)	(0.019)
1967-1969 × South			0.092***	0.136**
			(0.032)	(0.049)
Obs	167	89	167	89
Time FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Region FE	Y	Y	Y	Y

Source: BLS Industry Wage Reports. See figure 3.6b for the set of tabulations digitized.
Sample: All nonsupervisory employees.
Notes: the "full" sample contains industries listed in figure 3.6b. 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 level.

3.4.3 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 3.2 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 3.2 we exclude agriculture from our sample to see whether the results still hold. We find that the magnitude of the wage effect (6%) is only a bit lower than when agriculture is included (7%). One interpretation is that there is some heterogeneity in the wage response across industries. This interpretation would be consistent with the fact that the bite of the minimum wage is higher in agriculture than in the other newly covered sectors.

Additional robustness tests. We report the following additional robustness tests. First, we vary the sample selection criteria. In Column 4 of table 3.2 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.[29]. Finally, following Cameron *et al.* (2008)

[29]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

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 slightly the precision of our estimates.

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 3.9 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]:

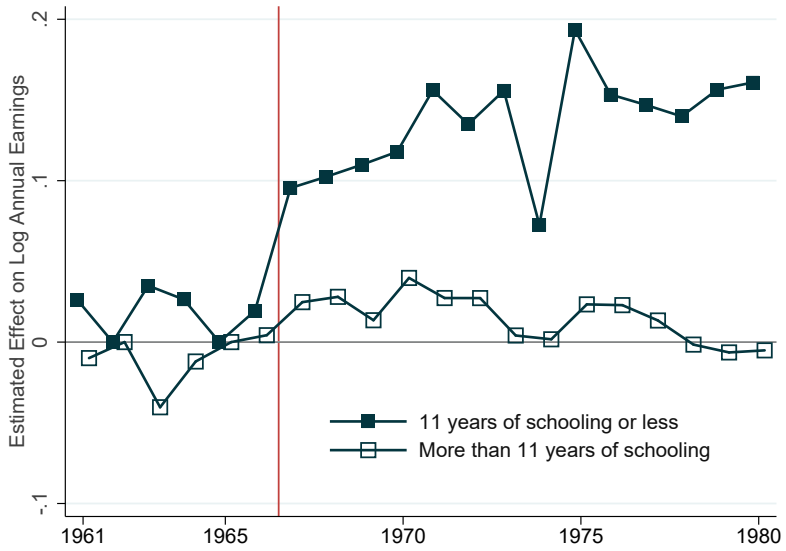
$$\log w_{ist} = \alpha + \sum_k \beta_k \text{Strongly treated state}_s \times \delta_{t+k} + \mathbb{X}'_{ist}\Gamma + \nu_s + \delta_k + \varepsilon_{ist} \tag{3.3}$$

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 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.[30] As in our cross-industry design, the effect is concentrated on workers with low levels of education.

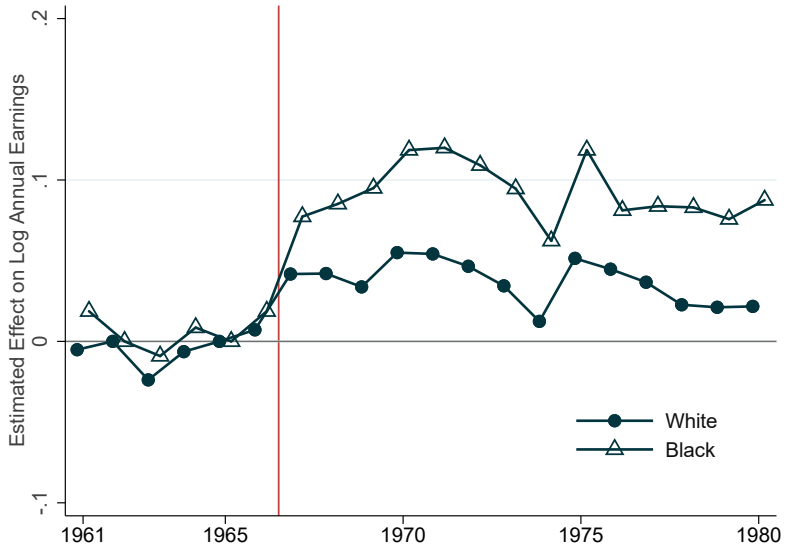
[30]Table available from authors upon request.

Figure 3.8: *Heterogeneity of the wage effect of the 1966 FLSA*

(a) *By level of education*

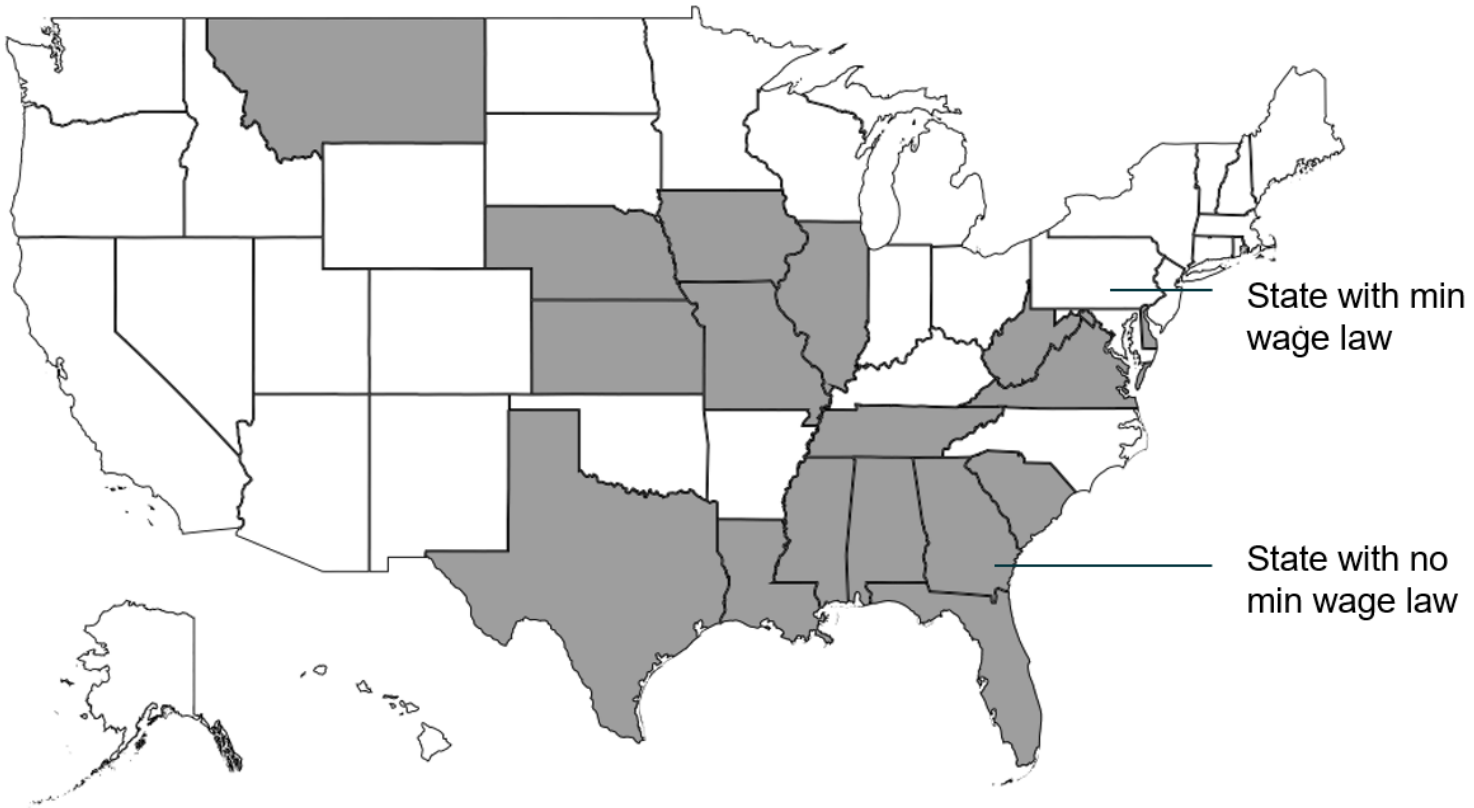


(b) *By race*



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 3.9: *States with no minimum wage laws as of January 1966*



Source: Authors' minimum wage database 1950-2016. More details provided in appendix C1.

3.4.4 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 **??**). 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 3.8b). Similarly, we compare black workers in the treated industries to black workers in the control industries (dark line in figure 3.8b), 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 C5).

3.5 The Employment Effects of the 1967 Reform

3.5.1 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 (Forthcoming), 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 3.10a 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 3.6 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 3.6b). 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 3.6, 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 3.6 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 3.6. This result is consistent with the estimate we obtain using a cross-state design in the CPS (see Section 3.5.2 below).

Our result of a small employment elasticity overall is also robust to varying assumptions on the spillover effects of the minimum wage. As reported in Table 3.6, 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 (whom we use to compute counterfactual employment growth rates at the bottom). For example, the reform may have

[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 C9 and C11.

[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 based on skills and experience.

Table 3.6: *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 3.6b 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.

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 × year level. Figure C18a 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 onwards, 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 underestimate the dis-employment effects of the reform.

3.5.2 Employment Effects in the CPS

We supplement the bunching analysis with an investigation of the employment effects of the reform in the CPS. We use the same cross-state design as implemented for wages in section 3.4.3 above: we compare employment outcomes in states that had no minimum wage law as of January 1967 (strongly treated) vs. states that did (weakly treated). We provide graphical evidence that employment outcomes 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 3.4.3, except that the outcome is log annual hours.[33] Figure 3.11a 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 3.7. 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

[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.

Table 3.7: *Effect of 1967 reform on annual number of hours worked (intensive margin)*

	All	Black	White
Covered in 1967 ×			
1967-1972	-0.014	-0.006	-0.020
	(0.012)	(0.021)	(0.012)
1973-1980	-0.021	-0.010	-0.027
	(0.015)	(0.022)	(0.015)
Obs	407,757	37,763	369,994
Controls	Y	Y	Y
Time FE	Y	Y	Y
Industry FE	Y	Y	Y

Source: CPS 1962-1980.

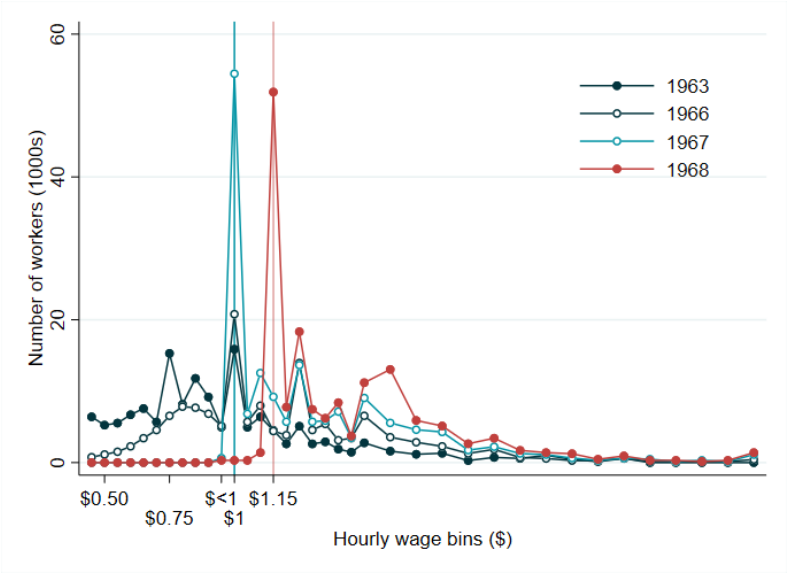
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

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.

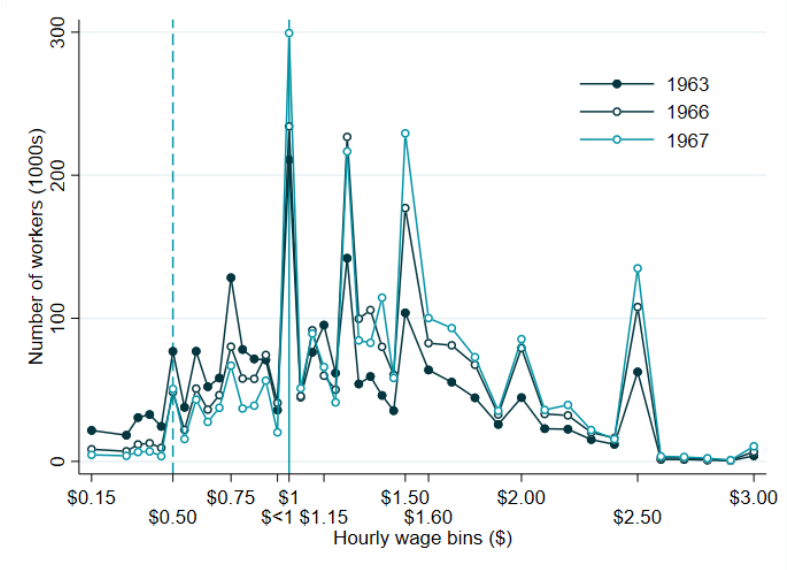
allows us to capture potential effects of the reform on labor force participation (in particular for women). As shown by table 3.8, the reform does not appear to affect the probability of being employed, with a point estimate for the difference-in-differences coefficient of interest of 0.001. The effect 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 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 3.12, this estimate is in the range of elasticities found in the minimum wage literature.

Figure 3.10: *Earnings Distributions in the BLS Industry Wage Reports*

(a) *Laundries Earnings distribution in South*



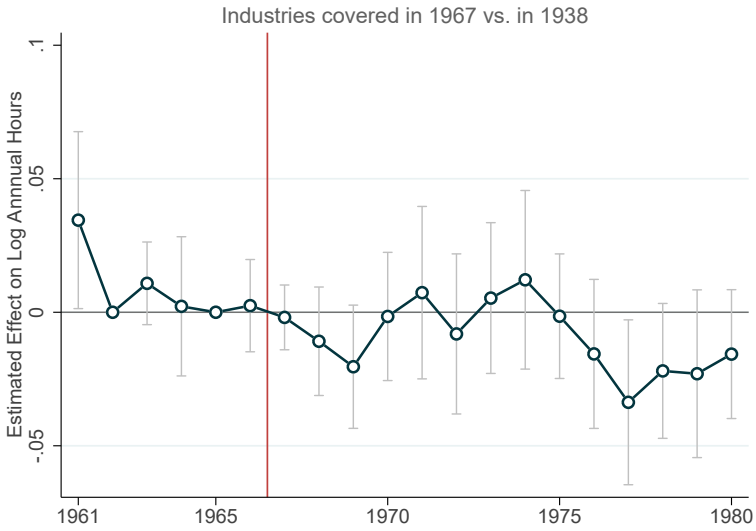
(b) *Earnings distributions in hotels, restaurants and laundries – U.S.*



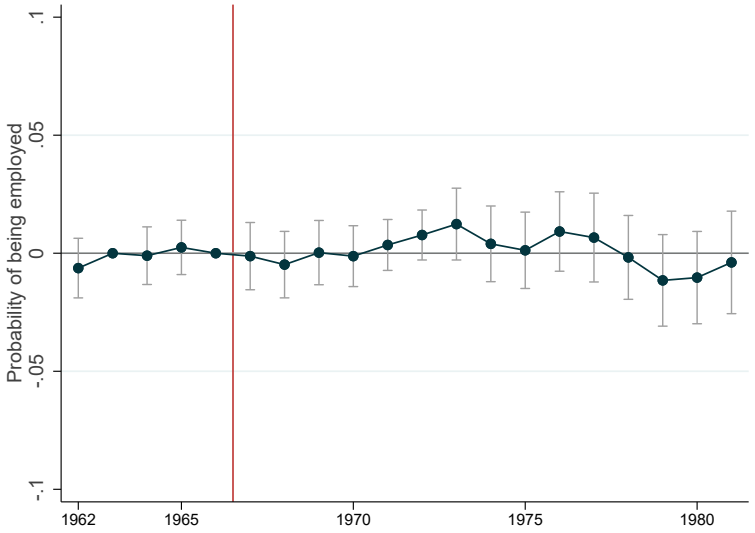
Source: BLS Industry Wage Reports.
Sample: Panel (a): All nonsupervisory workers, except routemen; Panel (b) All nonsupervisory workers in restaurants, and in laundries (except routemen); all nonsupervisory employees in year-round hotels, motels and tourist courts. Notes: Panel (a) The minimum wage is 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 3.11: *Impact of the 1966 FLSA on employment*

(a) *Intensive margin: annual number of hours worked*

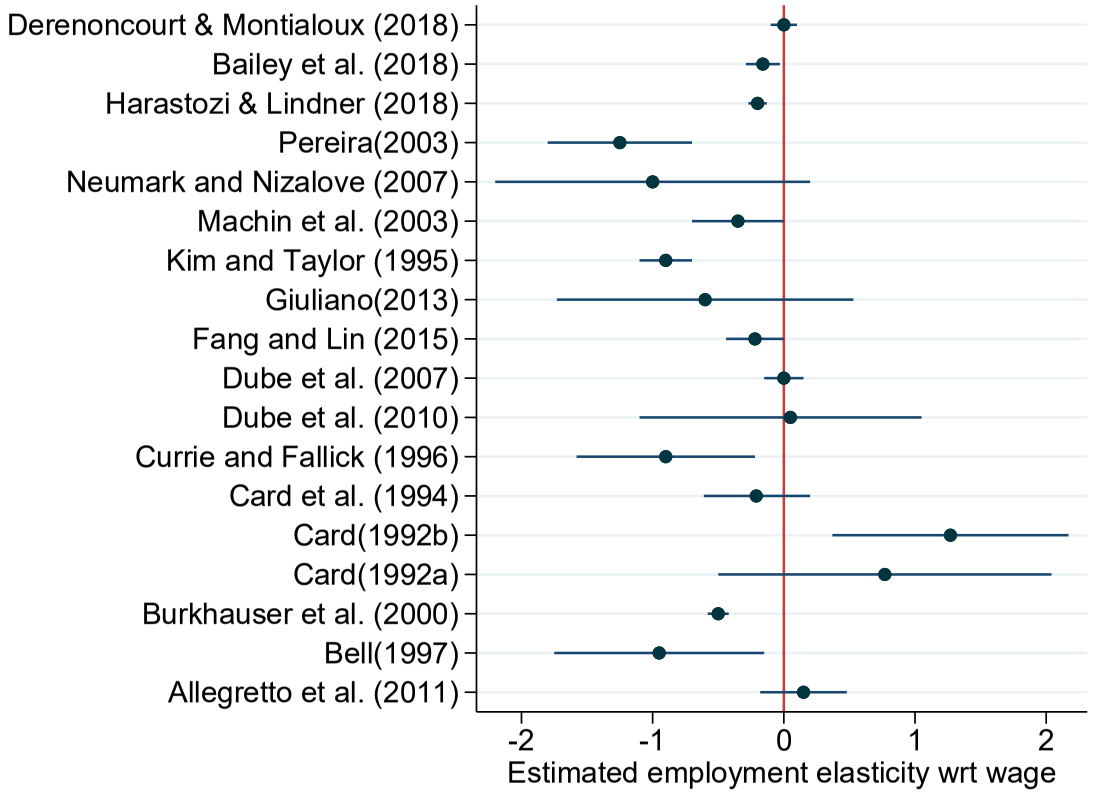


(b) *Extensive margin: probability of being employed (vs. not unemployed or not in the labor force)*



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 3.12: *Employment elasticities wrt wage in the literature and in this paper*



Note: This figure is taken from Harasztosi and Lindner (Forthcoming), 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).

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 3.8 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 (3.12). 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).

Table 3.8: *Effect of 1967 reform on probability of employment (extensive margin) and earnings*

	All	Black	White
State with no mw law × 1967-1972			
Employment	0.004	0.016	0.005
	(0.004)	(0.010)	(0.005)
Earnings	0.036***	0.115***	0.021**
	(0.010)	(0.024)	(0.008)
Obs	535,029	51,671	483,358
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.

3.6 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.

3.6.1 Unadjusted Racial Gap

We start by investigating how the reform affected the economy-wide unadjusted racial gap. To simplify the analysis, we only include the industries covered in 1938 and in 1967, i.e., we disregard the industries covered in 1961, 1974, and 1986. The two sets of industries we consider include about 75 % of all workers in 1966. Recall that the unadjusted racial earnings gap (in the 1938 and 1967 industries combined) fell by 25 log points between 1965 and 1980 (Figure 3.1a). 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:

$$G^{\text{total}} = s_w^c G^c + s_w^t G^t + G_b^{ct}(s_w^c - s_b^c) \tag{3.4}$$

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

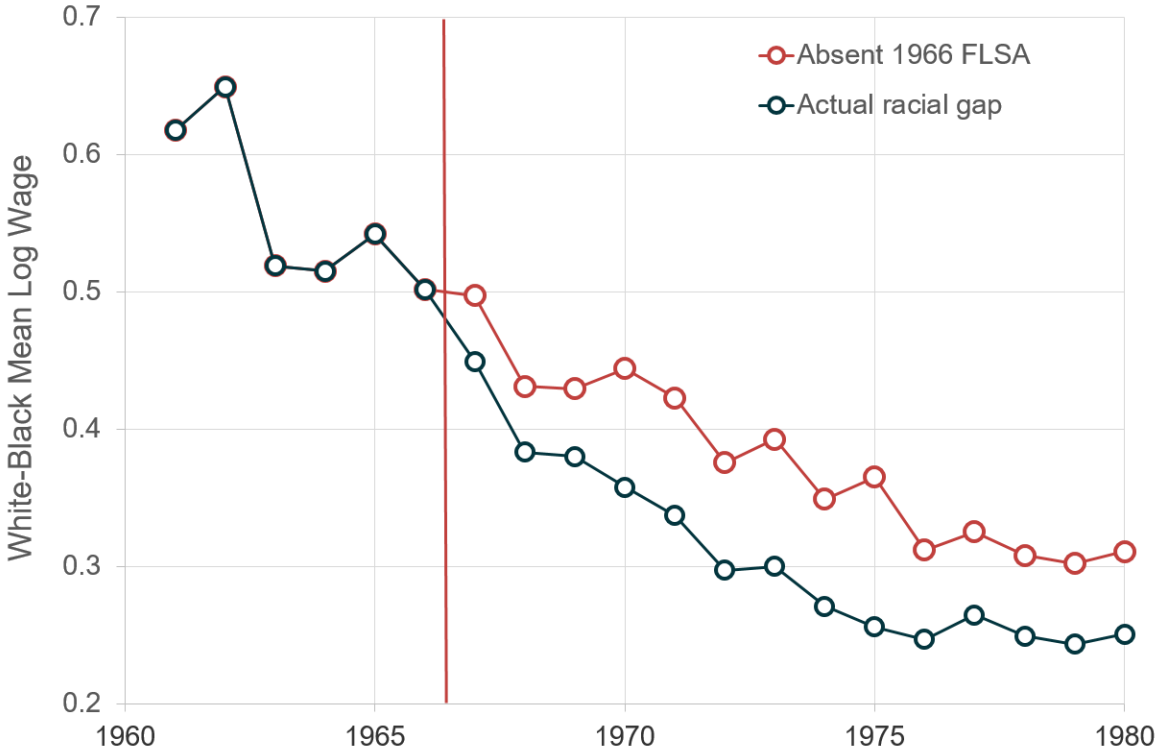
[35] see appendix C3 for a derivation of the decomposition.

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:

$$\begin{cases} \forall k \leq 1966 : G^t_{k,\text{counterfactual}} = G^t_{k,\text{observed}} \\ \forall k > 1966 : G^t_{k,\text{counterfactual}} = G^c_{k,\text{observed}} - \frac{1}{N}\sum_{k=1961}^{1966}(G^c_{k,\text{observed}} - G^t_{k,\text{observed}}) \end{cases} \tag{3.5}$$

As shown by figure 3.13, the 1967 minimum wage extension can explain around 20% of the decline in the racial earnings gap by 1980. The unadjusted racial earnings gap would have been 31 log points instead of 25 log points by 1980. 82% of this 6 log points difference owes itself to a reduction in the racial earnings gap within the treated industries (i.e., within-industry convergence). The remaining 18% owes itself to a reduction in the control-treatment earnings gap for black workers (i.e., between-industry convergence).

Figure 3.13: *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.

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]

3.6.2 Adjusted Racial Gaps

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

$$\log w_{ijt} = \alpha + \gamma \text{Black}_i + \sum_k \beta_k \text{Black}_i \times \delta_{t+k} + \mathbb{X}'_{ijt}\Gamma + \nu_j + \delta_k + \varepsilon_{ist} \tag{3.6}$$

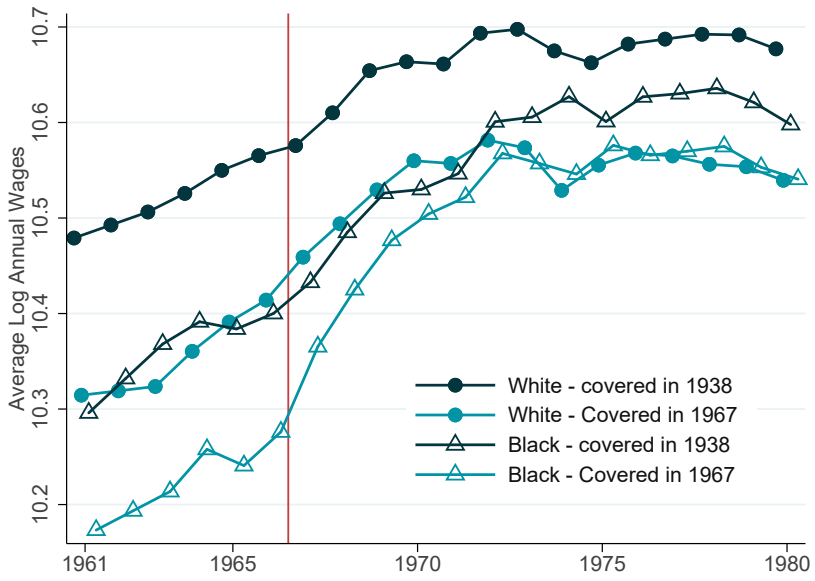
Where Black_i is a dummy for being a black worker; the set of individual level controls $\mathbb{X}'_{ijt}$ is the same as in the wage regression (gender, number of years of schooling, experience, industry, full-time or part-time status, occupation and marital status).

Figure 3.14a 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.

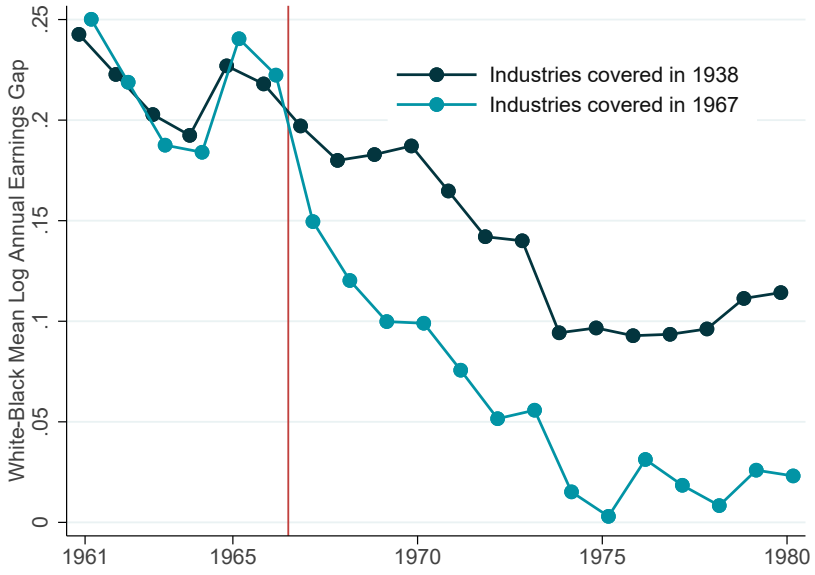
[36] There are some differences, however, between our calculations and Card and Krueger (1992)'s calculations that make a precise comparison not straightforward. In particular, Card and Krueger (1992) calculate the contribution of relative improvements in schooling quality to the decline of the unadjusted 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).

Figure 3.14: *Adjusted racial wage gaps*

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



(b) *Adjusted racial earnings gaps, by treatment status*

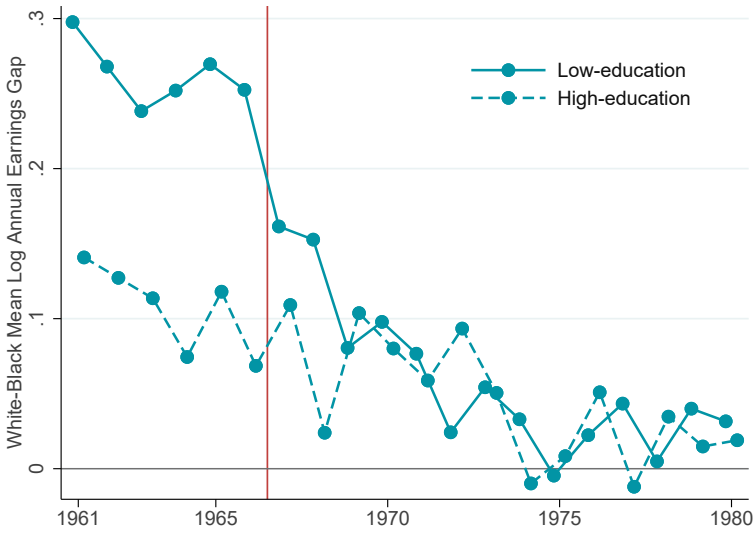


Source: March CPS 1962-1980. Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction. Notes: Racial earnings gap measures adjusted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

We plot the corresponding adjusted racial gaps (i.e. $\gamma + \beta_k$, k in [1961,1980]) for the control and treated industries in figure 3.14b. 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.

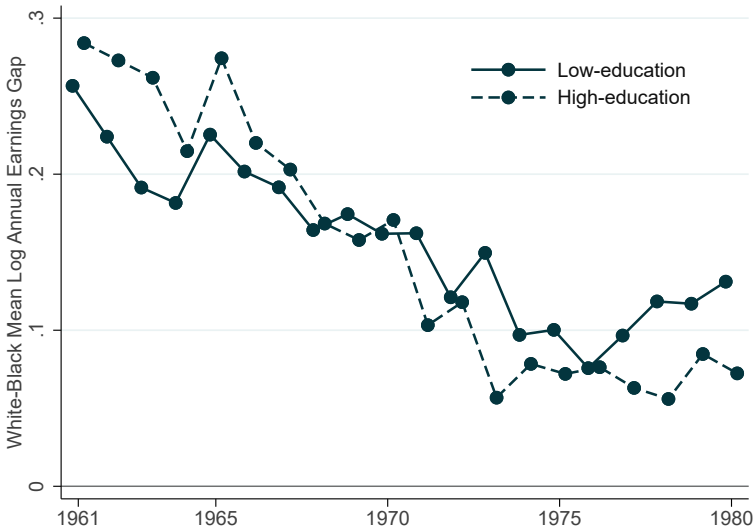
Figure 3.15: *Adjusted racial wage gaps, by level of education*

(a) *White-Black Earnings Gap (adjusted) in treated industries*



(b) *White-Black Earnings Gap (adjusted) in control industries*

x



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.

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 3.14a), 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 3.14a), 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 3.14a), 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) is the true driving force behind the decline in the adjusted racial earnings gap in the treated industries.

3.6.3 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.

3.7 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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Appendix A

Appendix to Chapter 1

A1 The changing geography of black upward mobility

In this appendix, I present descriptive evidence on the shift in regional patterns of black upward mobility in the US over the 20th century. As late as 1940, upward mobility rates were much lower in the South while the North offered a clear pathway to intergenerational progress. Today, the geography of upward mobility for black children looks very different. Some of the best locations for upward mobility in 1940 are among the worst today. I argue that a portion of this change is due to black upward mobility rates declining in the North.[1] In this section, I document these changes using historical census data and contemporary measures of upward mobility by race. The descriptive results in this section motivate the main analysis of the causal impact the Great Migration had on upward mobility.

There are two parts to this descriptive analysis. First, I construct a series of children's outcomes by race and region that dates from 1880-2010 in order to show the evolution of racial gaps in the North versus the South over a long time span. This series reveals the following: greater racial equality in the North than in the South between 1880 and 1940; improvements in relative black outcomes in both regions over this same period; and the *re-emergence* of racial gaps in the North after 1940. In the second part of this section, I

[1] Black children's outcomes in the South improve dramatically over this time period, also contributing to the changing regional pattern in upward mobility.

compare commuting zone level measures of upward mobility by race and gender in 1940 and 2015. I find complete convergence in regional differences in upward mobility for black Americans since 1940. Historical rates of black upward mobility are uncorrelated with today's. By contrast, these two measures are positively correlated for white children.

A1.1 Racial gaps in children's educational attainment, 1880-2010

One of the key limitations in constructing a long time series of upward mobility in the US is the availability of data that captures both the outcomes of children and the economic status of their parents. Prior to 1940, measures of income and educational attainment are not available in the US Census, though literacy rates are.[2] Beginning in 1940 and after, income becomes available, and more detailed information on educational attainment supplants indicators of literacy, reflecting real progress in population education levels.[3]

One measure of educational attainment that can be constructed from 1880 onwards, allows for conditioning on parent economic status, and remains economically significant today is teen school attendance.[4] Figure A1 shows black and white teen school attendance, separately by region. Each series shows uninterrupted increases over the 1880 to 1940 period. From 1940 to 1970, however, the growth in black teen attendance rates slows substantially. This stagnation has implications for the racial gap in teen school attendance rates by region.

Figure A2 shows the unadjusted white-black school attendance gap for teenagers aged

[2]Matched samples of fathers and sons can be constructed from pre-1950 censuses using first and last name and state of birth, allowing one to construct measures of the intergenerational transmission of occupation score or literacy prior to 1940 and income and educational transmission in 1940. However, matching techniques typically do not allow daughters to be matched across censuses, and match rates are notoriously worse for African Americans. For work documenting the changing geography of intergenerational occupation score mobility for white fathers and sons, see Tan (2018).

[3]The literacy rate for black Americans aged 14 and older improved from 30% in 1880 to 84% by 1930. See "120 Years of Literacy," National Center for Education Statistics.

[4]These measures can be constructed for 1870 as well, but estimates of black school attendance by region for these years are noisy due to the very small number of black children living outside the south.

14-17 from 1880 to 2010,[5] estimated from the linear probability model in equation A.1 below:

$$\text{Attend}_{irzt} = \sum_{1880}^{2010} \beta_t \text{North}_i \times \text{Black}_i \times \delta_t + \delta_r + \delta_z + \varepsilon_{irzt} \tag{A.1}$$

The outcome Attend_{irzt} is a binary indicator of school attendance for teen i of race r in region z (where $z = \{\text{North}, \text{South}\}$), and decade t.

During the first part of the 20th century, racial gaps in educational attainment are much higher in the South than in the North, though this measure likely understates the true gap.[6] Using this procedure, I estimate the gap in teen school attendance to be about 20 percentage points in the south and 10 percentage points in the North, thus gaps are about half as large in the North. In 1900, this difference in racial inequality peaks, with the north having a 15 percentage point smaller gap in teen school attendance. The next several decades show both continual improvements in the racial teen school attendance gap and substantial southern catch-up.[7] By 1940, the racial gap in teen school attendance in the North has disappeared and the southern gap has shrunk to about 8 percentage points.

The patterns in racial inequality after 1940 are of great interest. Between 1940 and 1960, the apparent northern premium in black educational attainment relative to the South vanished. Although improvements in relative black outcomes in the South over this period play a role, what is remarkable is the re-emergence of the racial gap in teen school attendance in the North. This re-emergence holds true even when conditioning on parent socioeconomic status. Including household head occupation score and the state of birth of both parents and children does not alter the basic post 1940 upward trend in racial inequality in teen school attendance in the North.[8]

[5]These figures show attendance rates for boys and girls together. The patterns are similar when estimated for boys and girls separately.

[6]In the first part of the 20th century, de jure segregation of southern schools and the much lower quality of black schools generated large gaps in the outcomes of black and white children (Margo, 1990).

[7]The trend in the unadjusted gap shows a continued improvement in the south, which experienced major improvements in educational access and quality for black children over this period.

[8]The regional gaps in racial inequality resulting from this alternative estimation strategy are smaller, but the broad pattern of increasing gaps in the North after 1940 remains true. The adjusted gaps show increases in racial inequality in both the South and the North after 1940, but as early as 1950, the gap in the North exceeds

A1.2 Long run change, 1940-2015

I provide an additional piece of evidence in changing social mobility patterns for black children. I correlate a more detailed measure of educational upward mobility in 1940 with income upward mobility in 2015. Following a method similar to Card *et al.* (2018), I measure the fraction of black teenagers from households in which the household head has 5-8 years of schooling[9] who obtain at least 9 years of education. I then correlate this measure with a measure of income upward mobility in 2015 for children from different racial backgrounds. The measure consists of estimated average adult income rank at the commuting zone level, for children from different parent income percentiles, where adult income is measured between the ages of 32 and 37.[10] Section 1.3.1 describes these data in much greater detail.

Figure 1.1 illustrates these two measures on a map of US commuting zones. The top panel shows the historical measure while the bottom panel shows the contemporary measure. Darker green represents higher upward mobility while lighter green represents lower upward mobility. In Table A1, I report the correlation coefficients between historical and contemporary upward mobility measures separately by race and gender. For white men and women, historically educational upward mobility is positively correlated with income upward mobility across commuting zones today. However, for black men and women, these measures are virtually uncorrelated. This racial difference is particularly pronounced among men. Figures A4 shows the correlation between the historical measure and the contemporary measure for black men in the top panel and for white men in the bottom panel.

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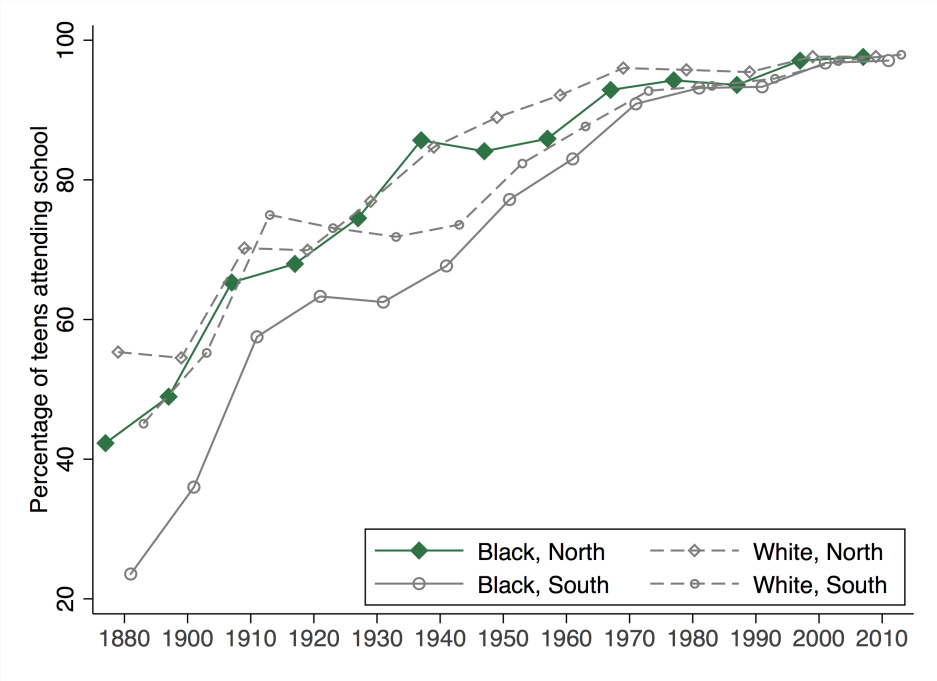
that of the South.

[9] Approximately the median of adult education in 1940.

[10] The children come from 1980s birth cohorts[11].

FIGURE A1: *Teen school attendance by subgroup*



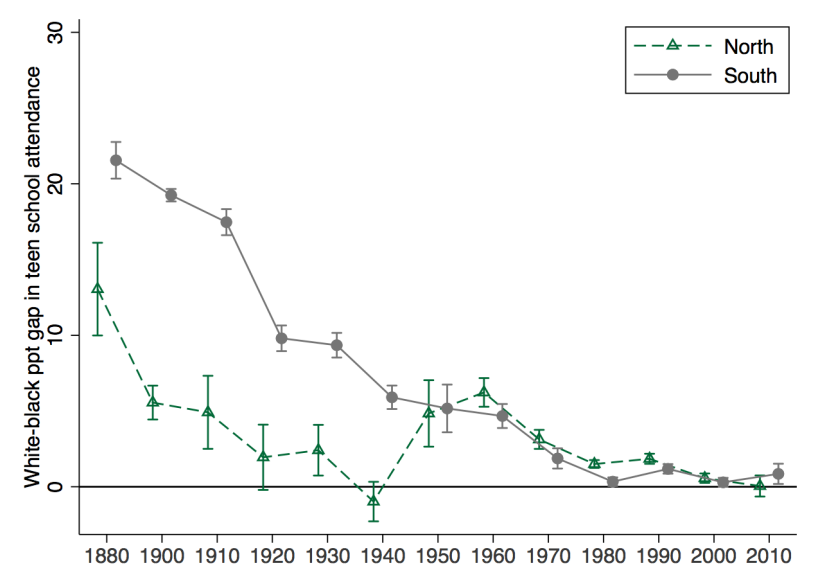
Notes: Teen school attendance by region (South and Non-South ("North")). The sample is 14-17 year-old boys and girls living in households. Source: IPUMS.

Table A1: *Correlation between historical and contemporary upward mobility measures, by race and gender*

	Men	Women
Black	-.09	.11
White	.46	.43

Correlation coefficients between 1940 and 2015 upward mobility, by race and gender. The sample is the set of CZs within each gender for which both black and white upward mobility measures can be computed.

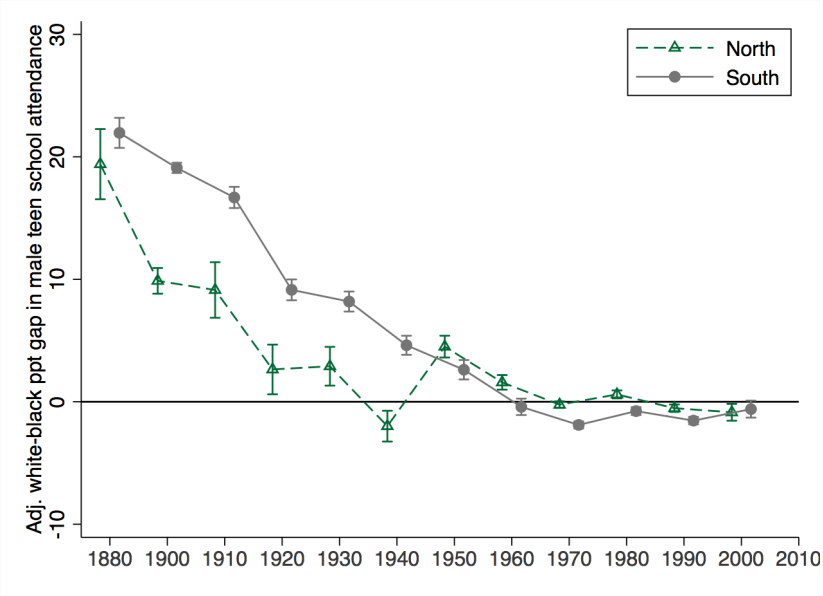
FIGURE A2: *White-Black Gap in Teen School Attendance*



Notes: This figure shows the unadjusted racial gap in teen school attendance by region (South and Non-South ("North")). Plotted above are yearly regression coefficients from estimating equation A.1 with the mean effect of black added in and multiplied by -1 *to generate the white-black gap in attendance. The sample is 14-17 year-old boys and girls living in households. Appendix Figure A3 depicts the series adjusted for household head occupation score and birth state as well as teen birth state. Appendix Figure A1 shows the raw black and white teen attendance rates separately by gender. Data sources: IPUMS.*

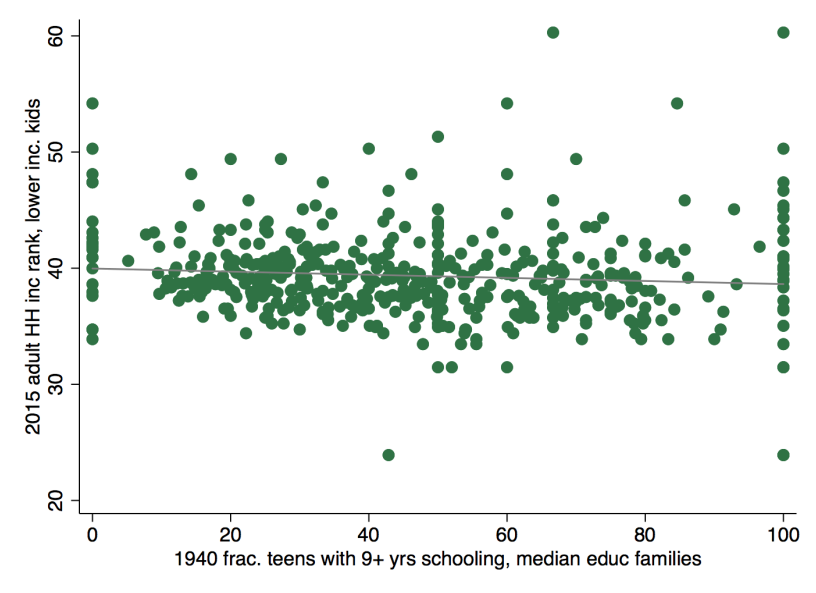
A2 Descriptive statistics

FIGURE A3: *White-black gap in teen school attendance, adjusted*



Notes: Racial gap in teen school attendance by region (South and Non-South ("North")) adjusted for household head occupation score and birth state as well as teen birth state. The sample is 14-17 year-olds living in households. Source: IPUMS.

FIGURE A4: *Correlation of 1940 & 2015 Upward Mobility*

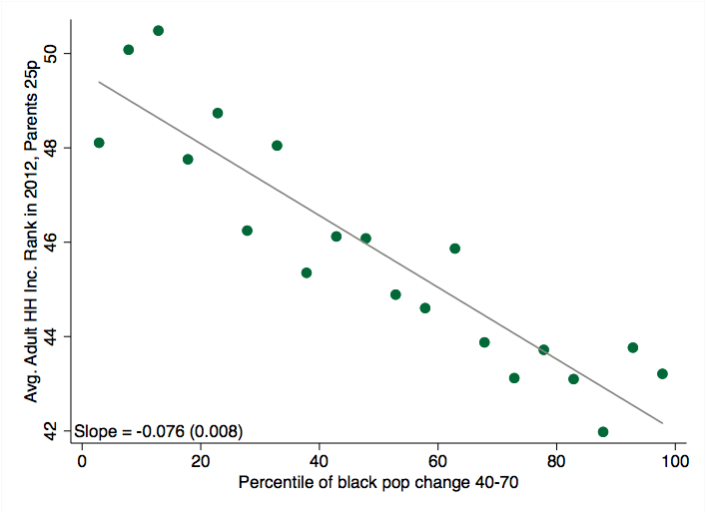


(a) Black men

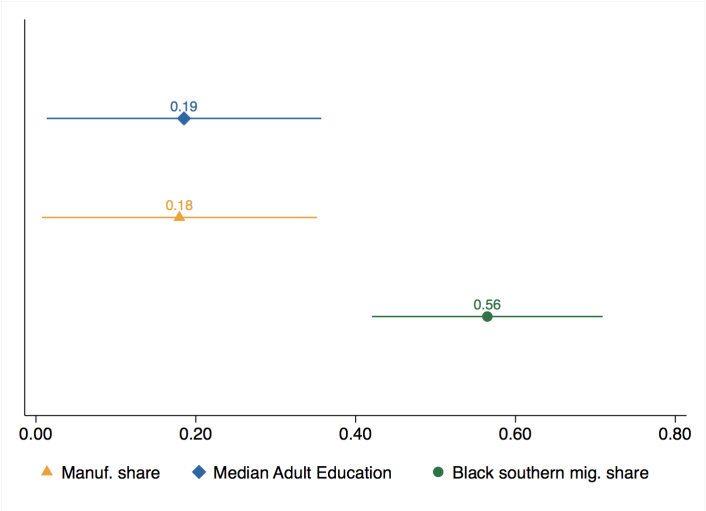
(b) White men

Notes: This figure depicts scatter plots of the relationship between historical upward mobility and contemporary upward mobility for black and white men. In panel (a), the right hand side ("RHS") is between 1940 educational upward mobility defined as fraction of 14-17 year old black boys who have at least 9 years of schooling, from families where the household head has 5-8 years of education. The left hand side ("LHS") is expected average individual adult income rank in 2015 of black men from 1978-1983 birth cohorts who come from families at the 25th percentile of the parent income distribution. Panel (b) shows the same relationship as in panel (a) for white men. The correlations between historical and contemporary upward mobility are reported for black and white women in Appendix Table A1.

FIGURE A5: *Descriptive evidence and summary statistics*



(a) Relationship between 1940-1970 black population change and upward mobility in 2012



(b) Correlation between black population change and baseline 1940 covariates

Notes: Panel (a) shows a binned scatterplot of the relationship between the percentile of actual black population increase between 1940 and 1970 and upward mobility in 2012. The right hand side variable is grouped into 20 bins (5 percentiles each). The unit of observation is a commuting zone. Panel (b) plots the correlation coefficient between percentile of black population increase between 1940 and 1970 and three baseline 1940 covariates: the share of the labor force in manufacturing, educational upward mobility in 1940 described in Section 1.3, and the share of the 1940 urban population made up of recent black southern migrants. Data sources: IPUMS complete count US census for 1940; CCDB.

Table A2: *Commuting Zones in Sample*

Phoenix, AZ	Rockford, IL	Joplin, MO	Youngstown, OH
Tucson, AZ	Springfield, IL	Kansas City, MO	Zanesville, OH
Bakersfield, CA	Center, IN	Springfield, MO	Eugene, OR
Fresno, CA	Concord, IN	St. Joseph, MO	Portland, OR
Los Angeles, CA	Evansville, IN	St. Louis, MO	Allentown, PA
Sacramento, CA	Fort Wayne, IN	Butte-Silver Bow, MT	Altoona, PA
San Diego, CA	Gary, IN	Great Falls, MT	Erie, PA
San Francisco, CA	Indianapolis, IN	Fargo, ND	Hagerstown, PA
San Jose, CA	Lafayette, IN	Lincoln, NE	Harrisburg, PA
Santa Barbara, CA	Muncie, IN	Omaha, NE	Philadelphia, PA
Colorado Springs, CO	South Bend, IN	Manchester, NH	Pittsburgh, PA
Denver, CO	Terre Haute, IN	Newark, NJ	Reading, PA
Pueblo, CO	Wayne, IN	Albuquerque, NM	Scranton, PA
Bridgeport, CT	Hutchinson, KS	Albany, NY	Williamsport, PA
Washington, DC	Topeka, KS	Amsterdam, NY	Providence, RI
Wilmington, DE	Wichita, KS	Buffalo, NY	Sioux Falls, SD
Burlington, IA	Louisville, KY	Elmira, NY	Salt Lake City, UT
Cedar Rapids, IA	Boston, MA	New York, NY	Burlington, VT
Clinton, IA	Pittsfield, MA	Poughkeepsie, NY	Bellingham, WA
Des Moines, IA	Springfield, MA	Syracuse, NY	Seattle, WA
Dubuque, IA	Baltimore, MD	Union, NY	Spokane, WA
Mason City, IA	Cumberland, MD	Watertown, NY	Yakima, WA
Ottumwa, IA	Bangor, ME	Canton, OH	Eau Claire, WI
Sioux City, IA	Portland, ME	Cincinnati, OH	Green Bay, WI
Waterloo, IA	Detroit, MI	Cleveland, OH	Kenosha, WI
Bloomington, IL	Grand Rapids, MI	Columbus, OH	La Crosse, WI
Chicago, IL	Jackson, MI	Dayton, OH	Madison, WI
Davenport, IL	Kalamazoo, MI	Lima, OH	Milwaukee, WI
Decatur, IL	Lansing, MI	Lorain, OH	Oshkosh, WI
Edwardsville, IL	Saginaw, MI	Mansfield, OH	Sheboygan, WI
Galesburg, IL	Duluth, MN	Scioto, OH	Wausau, WI
Peoria, IL	Minneapolis, MN	Steubenville, OH	
Quincy, IL	Rochester, MN	Toledo, OH	

Notes: Name refers to largest city in the commuting zone.

A3 Shift-share instrument for black population increases during the Great Migration

To estimate the causal impact of the Great Migration on upward mobility in cities in the North today, I instrument for 1940-1970 urban black population increases (normalized by the 1940 urban population) in northern commuting zones using variation in southern black migration patterns. Specifically, I interact variation in location choices of black southern migrants who moved prior to 1940 with variation in net-migration from southern counties between 1940 and 1970 predicted using southern economic variables. This appendix details the construction of the instrument from these two sources of variation, beginning with the construction of the shares from pre-1940 migrant location choices and following with the prediction of migration from southern counties using a machine learning approach.

A3.1 Pre-1940 black southern migrant shares

I measure black southern migrant shares using the complete count 1940 census. The 1940 census was the first census in which enumerators asked individuals to report their place of residence in 1935. There are several advantages to this approach of measuring pre-1940 black migration patterns. The first is that I am able to observe the universe of enumerated recent black southern migrants, generating a nearly complete picture of recent migration flows into northern cities. The second is that the census microdata allow me to observe fine geographies for individuals' 1935 place of residence, including city and county. I define a recent black southern migrant as a black individual who reported a southern county of residence in 1935, but was enumerated in a different county (whether southern or not) in 1940. There are over 340,000 such individuals.

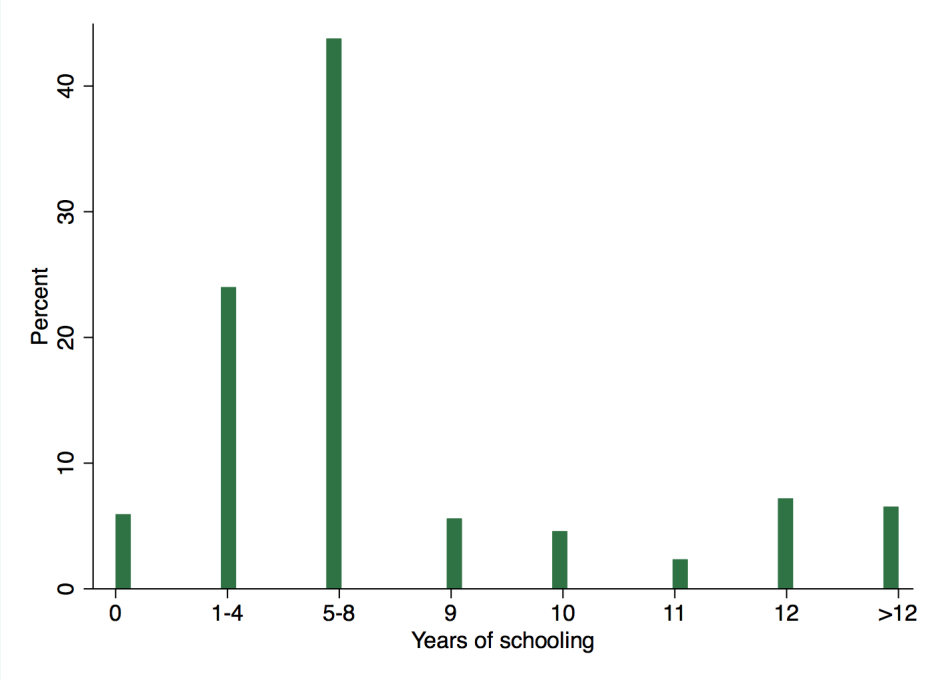
Using this population of recent black southern migrants, I construct the share of migrants from each 1935 southern county j who settled in a northern city c by 1940:

$$\omega_{jc}^{1935-1940} = \frac{b_{cj}}{b_j} \tag{A.2}$$

where b_j is the number of black individuals who listed j as their county of residence in 1935, and b_{cj} is the number of black individuals who were enumerated in city c.

Figure A8 depicts $\omega_{jc}^{1935-1940}$ for a select group of cities and southern counties. Depicted is the share of 1935-1940 black migrants from the largest sending county for each southern state who settled in the following cities: Baltimore, Boston, Chicago, Los Angeles, New York, Philadelphia, and Salt Lake City. The figure captures the immense heterogeneity in settlement patterns across and volume of migration into the cities in question. Figure A6 shows the educational distribution for 1935-1940 black southern migrants aged 25 plus.

FIGURE A6: *1935-1940 black southern migrant educational attainment*



Notes: Histogram of years of schooling for 1935-1940 black southern migrants aged 25 plus. Data sources: IPUMS Complete Count 1940 US Census.

A3.2 Post-LASSO prediction of southern county net migration

In a "zero stage," I predict net migration from southern counties using southern push factors:

$$m_{jt} = \beta_0 + \mathbb{Z}'_{jt-10}\beta_1 + \varepsilon_{jt},$$

$$\hat{m}_{jt} = m_{jt} - \varepsilon_{jt}$$

where m_{jt} is net migration for southern county j between decade $t-10$ and t, $\mathbb{Z}'_{jt-10}$ is the set of predictors measured in decade $t-10$, and $\hat{m}_{jt}$ is predicted net migration from county j.[12] I then generate predicted migration into northern cities by multiplying the share of pre-1940 migrants from each county by the predicted number of migrants leaving that county between 1940 and 1970:[13]

$$\hat{m}_{ct} = \sum_{j=1,..,1223} (\omega_{cj}^{1935-40} \cdot \hat{m}_{jt}) \tag{A.3}$$

where $\omega_{cj}^{1935-40}$ is the share of black migrants from southern county j living in city c.

$$\sum_{j \in S} \sum_{t=1950}^{1970} \hat{m}_{jt \text{ urban,CZ}} \tag{A.4}$$

Specifically, let m_{ct} be historical black in-migration into city c in decade t, and let ω_{cj} be the share of county j's outmigrants between 1935 and 1939 who reside in city c by 1940. Predicted in-migration $\hat{m}_{ct}$ is the sum of predicted outmigration from southern counties,

[12] Direct measures of county-level in-migration and out-migration is not available for this time period, so I use net migration estimates produced by Boustan (2010) and made available in Boustan (2016).

[13] Because the available figures are net migration figures, and some southern counties experienced positive net migration (in-migration) as opposed to negative (in-migration), this procedure may result in predicted *decreases* in the black population. This is the case for a small share of the commuting zones in the sample, particularly those in western states that are more likely to be connected to counties in Oklahoma or Texas, for example, some of which experienced net in-migration between 1940 and 1970.

weighted by ω_{cj}:

$$\hat{m}_{ct} = \sum_{j=1,..,1386} (\omega_{cj}^{1935-40} \cdot \hat{m}_{jt}) \text{ and}$$
$$\hat{m}_{c,t+10} = \hat{b}_{c,t} + \hat{m}_{c,t+10} \; \forall t > 1940.$$
$$\text{For } t = 1940, \; \hat{b}_{c,1940} = b_{c,1940}.$$

Under the assumption that county-level variation in southern economic indicators from 1940-1970 is uncorrelated with northern destination city characteristics for migrants from those counties, I view estimating southern county net-migration rates as a pure prediction problem. Belloni *et al.* (2011) propose a machine learning based estimation of the first stage in an instrumental variables context where the number of instruments is large relative to the number of observations. In my case, I use this approach to select predictors in the "'zero" stage prediction of migration out of southern counties.[14]

I choose the set of predictors by applying the Least Absolute Shrinkage and Selection Operator ("LASSO") algorithm to datasets of southern county net black migration estimates in 1950, 1960, 1970. For each decade, I use 5-fold cross-validation to choose the tuning parameter on the penalty term, the absolute value of the sum of the coefficients on each southern county covariate. I begin with a the set of predictors used in Boustan (2010) of net black migration rates from southern counties, where each predictor is measured in t and used to predict migration in $t + 10$. For migration from 1940-1950, 1950-1960, and 1960-1970, predictors are measured in 1940, 1950, and 1960, respectively.

Boustan (2010) uses the following variables in each year: the percent acreage in cotton; percent tenant farms; share of the labor force in agriculture; indicator for being in tobacco growing state, and the interaction between tobacco growing state and share in agriculture; WWII spending per capita; share of the labor force in mining, an indicator for being in a mining state (OK and TX), and the interaction between the two.

In my case, LASSO selects the following for each year:

[14]See Sequeira *et al.* (2019) where the authors first predict European outmigration using local weather shocks and then interact predicted migration flows with railway expansion across US counties.

Variables selected in 1940:

- Percent tenant farms
- Share of the labor force in agriculture
- WWII spending per capita
- Percent acreage in cotton
- Share of the labor force in agriculture $\times$ Tobacco growing state
- Indicator for mining state
- Indicator for mining state $\times$ Share of the labor force in mining

Variables selected in 1950:

- Percent tenant farms
- Share of the labor force in agriculture
- WWII spending per capita
- Percent acreage in cotton
- Percent acreage in tobacco
- Indicator for mining state
- Indicator for mining state $\times$ Share of the labor force in mining
- Share of the labor force in mining

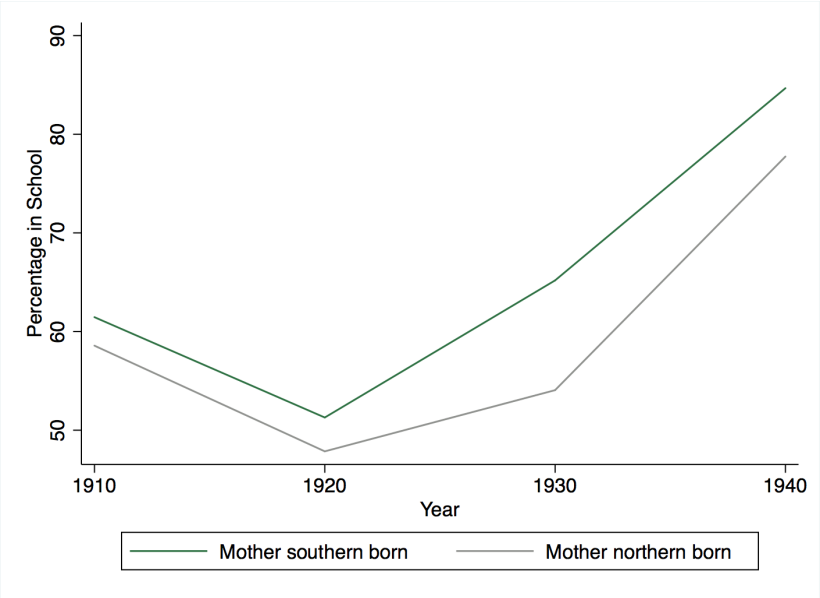
Variables selected in 1960:

- Percent tenant farms
- Share of the labor force in agriculture
- Indicator for tobacco growing state
- Share of the labor force in agriculture $\times$ Tobacco growing state
- Percent acreage in cotton

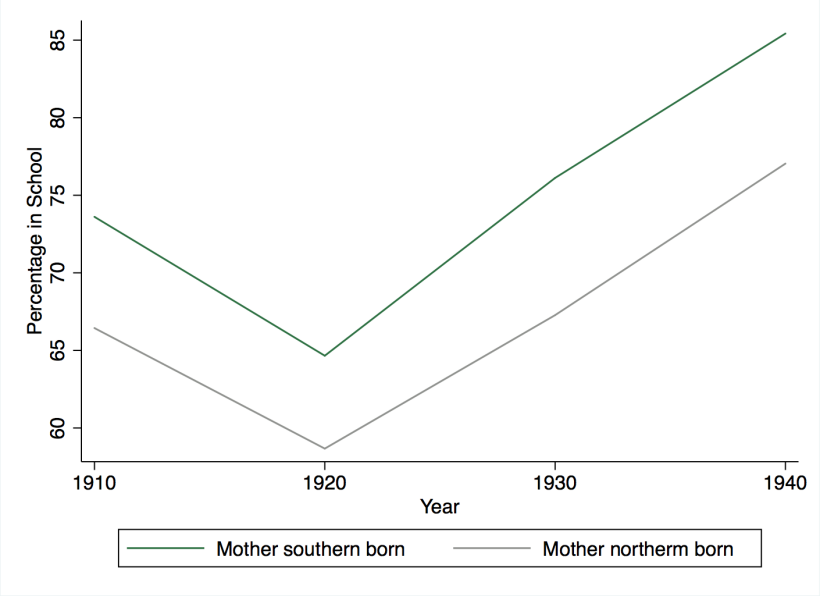
- Indicator for mining state
- Indicator for mining state $\times$ Share of the labor force in mining
- Share of the labor force in mining

Using LASSO-selected variables improves the F-statistic for county outmigration prediction from 1940-1950 from 11.56 to 14.78. The F-statistics in the models for county outmigration prediction from 1950-1960 and 1960-1970 are identical using the original set of variables in Boustan (2010) and the LASSO-selected set.

FIGURE A7: *School attendance rates of black teens in the North with southern- vs. northern-born mothers*



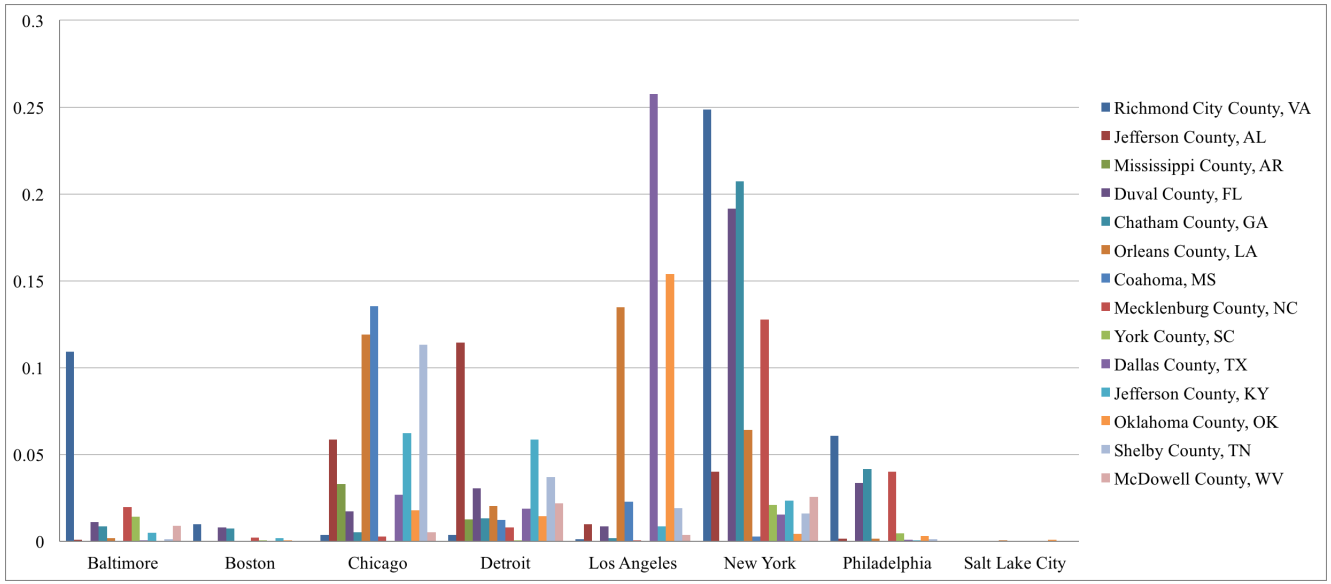
(a) Black teens with illiterate mothers



(b) Black teens with low occupation score fathers

Notes: 1910-1940 school attendance rates (in percentage points) for black 14-17 year-old boys and girls by mother birth region. Data sources: IPUMS Complete Count 1920-1940 US Censuses.

FIGURE A8: *Settlement patterns of 1935-1940 black southern migrants*



Notes: Share of black individuals reporting the indicated counties (legend to the right of chart) as their county of residence in 1935 who were enumerated in the cities displayed in 1940. The counties included are those with the largest net migration from the state out of all other counties in the state. Data sources: IPUMS Complete Count 1940 US Census.

A4 Appendix to upward mobility results

TABLE A3: *Upward mobility results summary*

	Average	Expos. Effects	Black, p25	Black, p75
GM Shock	-0.0299***	-0.00242***	-0.0248**	-0.0357**
	(0.00952)	(0.000628)	(0.0113)	(0.0167)
Edu. Upward Mobility 1940	-0.0735*	0.0000909	-0.0144	-0.0772
	(0.0416)	(0.00268)	(0.0492)	(0.0728)
LF in manuf. 1940	-0.144***	-0.00321*	-0.0816**	-0.0000935
	(0.0270)	(0.00192)	(0.0319)	(0.0472)
Black Southern Mig 1935-1940	-5.099***	-0.0720	-0.567	-2.208
	(1.387)	(0.0654)	(1.649)	(2.443)
Midwest	-0.261	0.100***	-1.391*	-0.656
	(0.610)	(0.0357)	(0.722)	(1.069)
South	-2.054	0.170**	-0.306	1.418
	(1.291)	(0.0752)	(1.527)	(2.262)
West	-1.756*	-0.103*	-1.379	-1.004
	(0.958)	(0.0546)	(1.143)	(1.693)
R-squared	0.493	0.249	0.185	0.116

Dependent variable is mean individual income rank, where income is measured from IRS tax returns for cohorts born between 1978 and 1983. Pooled income refers to household income, as opposed to individual income. Independent variable is predicted change in black population share between 1940 and 1970. Baseline controls include share of CZ population made up of 1935-1939 black southern migrants from any southern county, median education levels in 1940, and share of employment in manufacturing in 1940. Data from Chetty and Hendren (2018).

TABLE A4: *Great Migration impact on average upward mobility for black families in the 2000s (individual earnings)*

	First Stage				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
$\widehat{GM}$	0.304***	0.304***	0.304***	0.304***	0.304***
	(0.0750)	(0.0750)	(0.0750)	(0.0750)	(0.0750)
F-Stat	23.32	23.32	23.32	23.32	23.32
	Ordinary Least Squares				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	-0.0480***	0.0250***	-0.0538***	0.0307**	-0.0710***
	(0.00711)	(0.00795)	(0.00875)	(0.0155)	(0.0128)
R-squared	0.354	0.200	0.256	0.0872	0.213
	Reduced Form				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM Shock	-0.0301***	0.0132	-0.0366***	0.0250	-0.0464***
	(0.00772)	(0.00804)	(0.00927)	(0.0153)	(0.0134)
R-squared	0.213	0.155	0.138	0.0782	0.104
	Two-stage least squares				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	-0.0516**	0.0316	-0.0777**	0.112*	-0.112**
	(0.0253)	(0.0292)	(0.0318)	(0.0587)	(0.0480)
R-squared	0.416	0.228	0.299	0.0649	0.209
N	129	129	129	129	129
Outcome Mean	33.19	40.33	38.88	49.32	51.46
Outcome SD	2.747	2.763	3.151	5.036	4.481

Notes: This table reports the estimated impact of the Great Migration on average upward mobility in the 2000s for black men and women with high income parents. The unit of observation is a commuting zone. Dependent variable is expected mean individual income rank for individuals with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1978 and 1983. Independent variable is the percentile of black population increase during the Great Migration, or the instrument for the same. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: CCDB, IPUMS complete count US census for 1940; Boustan (2016); Chetty *et al.* (2018).

TABLE A5: *Great Migration impact on average upward mobility for white families in the 2000s (individual earnings)*

	First Stage				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
$\widehat{GM}$	0.304***	0.304***	0.304***	0.304***	0.304***
	(0.0750)	(0.0750)	(0.0750)	(0.0750)	(0.0750)
F-Stat	23.32	23.32	23.32	23.32	23.32
	Ordinary Least Squares				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	-0.0199**	-0.000804	-0.0121	-0.0105	-0.0114**
	(0.00970)	(0.00991)	(0.00814)	(0.00792)	(0.00556)
R-squared	0.0616	0.0769	0.0733	0.280	0.117
	Reduced Form				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM Shock	-0.0161	-0.00296	-0.0123	-0.00266	-0.00422
	(0.00975)	(0.00991)	(0.00813)	(0.00797)	(0.00564)
R-squared	0.0507	0.0775	0.0738	0.271	0.0911
	Two-stage least squares				
	Pooled HH Lower Inc	Women Lower Inc	Men Lower Inc	Women Higher Inc	Men Higher Inc
GM	0.0159	0.0338	-0.00803	0.0443	0.00344
	(0.0359)	(0.0376)	(0.0307)	(0.0320)	(0.0218)
R-squared	0.187	0.161	0.165	0.257	0.141
N	130	130	130	130	130
Outcome Mean	45.23	40.46	49.00	51.68	62.96
Outcome SD	3.163	3.259	2.669	2.950	1.869

Notes: This table reports the estimated impact of the Great Migration on average upward mobility in the 2000s for white men and women with high income parents. The unit of observation is a commuting zone. Dependent variable is expected mean individual income rank for individuals with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1978 and 1983. Independent variable is the percentile of black population increase during the Great Migration, or the instrument for the same. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Data sources*: CCDB, IPUMS complete count US census for 1940; Boustan (2016); Chetty *et al.* (2018).

A4.1 Childhood exposure effects

Chetty and Hendren (2018b) use variation in age of child at time family moves to purge place effect estimates of bias due to sorting on family unobservables, θ_i:

$$\begin{aligned} y_i &= \delta_c + \theta_i \\ &\downarrow \\ \Delta y_i &= \alpha_c \Delta t_i \end{aligned}$$

α_c is an unbiased estimate of effect of additional year of childhood exposure to location c on adult outcome y_i.

Scaling childhood exposure effects

Assuming muted effect for early years according to Figure A11, the effect of full childhood exposure for 23 years should be adjusted in the following manner:

$$\text{Years} = (23 - 13) + (17/40) * 13 = 15.525$$

A4.2 Additional robustness checks

TABLE A6: *Results on childhood exposure effects using southern-state-of-birth to define Great Migration shocks*

	First Stage on GM					
	1880	1910	1920	1930	1940	1935-40
$\widehat{GM}$	0.131	0.224**	0.609***	0.455***	0.327***	0.292***
	(0.0907)	(0.0956)	(0.113)	(0.106)	(0.0984)	(0.0652)
F-Stat	4.203	6.638	16.51	24.45	26.98	27.46
	Ordinary Least Squares					
GM	-0.00262***	-0.00277***	-0.00292***	-0.00310***	-0.00303***	-0.00265***
	(0.000610)	(0.000633)	(0.000691)	(0.000797)	(0.000844)	(0.000823)
R-squared	0.226	0.229	0.229	0.229	0.227	0.223
	Reduced Form					
GM Shock	-0.00108*	-0.00161**	-0.00301***	-0.00316***	-0.00255**	-0.00242***
	(0.000654)	(0.000721)	(0.000992)	(0.00103)	(0.000980)	(0.000628)
R-squared	0.128	0.143	0.179	0.196	0.190	0.249
	Two-stage least squares					
GM	-0.00830	-0.00718**	-0.00495***	-0.00696***	-0.00780**	-0.00830***
	(0.00597)	(0.00350)	(0.00158)	(0.00233)	(0.00319)	(0.00250)
R-squared	-0.324	-0.0778	0.175	0.0815	0.0233	-0.0774
N	130	130	130	130	130	130
Precision Wt	Y	Y	Y	Y	Y	Y
Mean Expos FX	-0.0160	-0.0160	-0.0160	-0.0160	-0.0160	-0.0160
SD Expos FX	0.172	0.172	0.172	0.172	0.172	0.172
SD GM	24.82	24.82	24.82	24.82	24.82	24.82

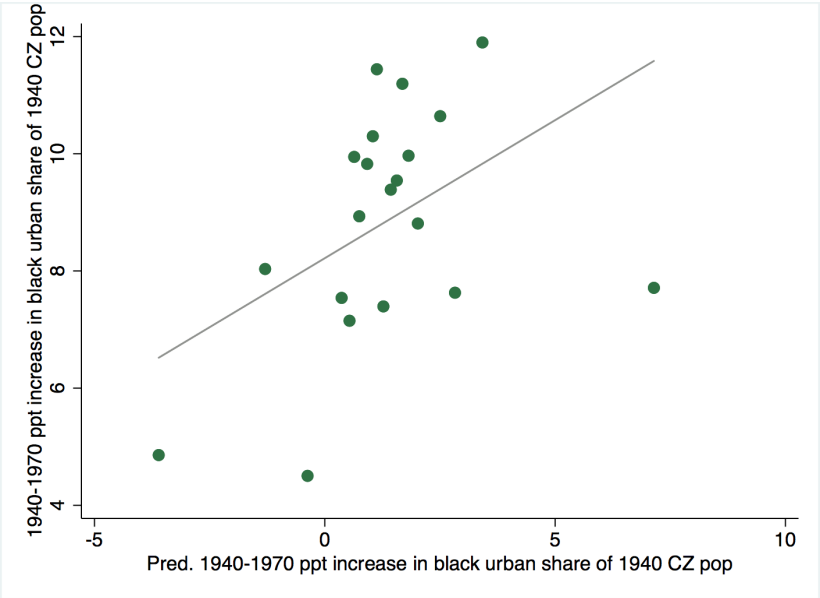
Notes: This table reports results on commuting zone childhood exposure effects using 1880-1940 southern-state-of-birth to construct the instrument for black population changes. The unit of observation is a commuting zone. Dependent variable is the estimated causal impact of one additional year of childhood in the commuting zone on adult household income rank for men and women with parents at the 25th percentile. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. Independent variable is the percentile of black population increase during the Great Migration, or the instrument for the same. Columns 1-5 use southern state of birth of the black population in northern cities in 1880, 1910, 1920, 1930 and 1940, respectively, to define black southern migrant shares. Predicted southern county outflows between 1940 and 1970 are aggregated to the state level and assigned to northern cities according to the share of the black population born in that southern state and living in the destination city in the year indicated. Column 6 uses 1935-1940 black migration patterns to define the shares and assigns predicted southern county outflows to northern cities based on these shares. Controls include share of urban population made up of 1880, 1910, 1920, 1930, 1940, or 1935-1940 black southern migrants respectively. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A7: *Results on black men's upward mobility using southern-state-of-birth to define Great Migration shocks*

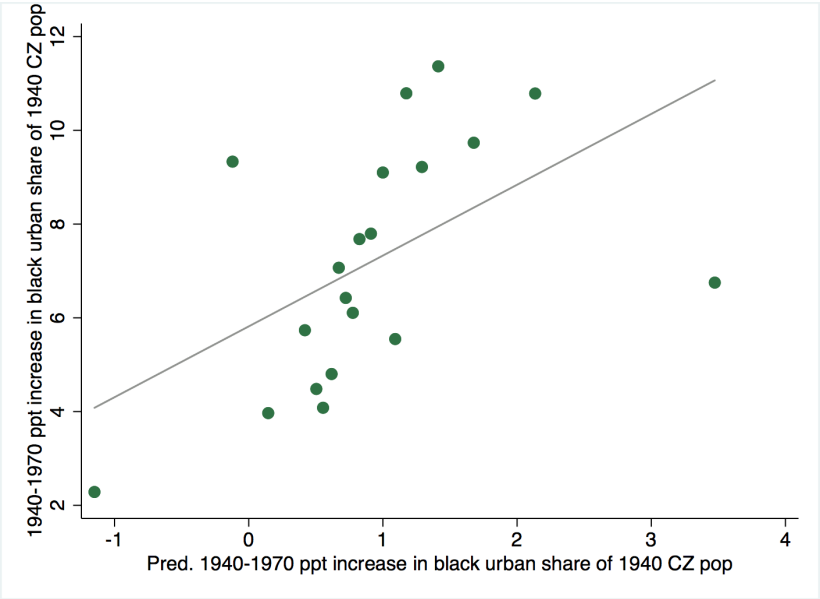
	First Stage on GM					
	1880	1910	1920	1930	1940	1935-40
GM Shock	0.133	0.140	0.353***	0.451***	0.403***	0.304***
	(0.0822)	(0.0970)	(0.103)	(0.0966)	(0.0935)	(0.0750)
F-Stat	5.631	7.580	13.70	22.65	24.84	23.32
	Ordinary Least Squares					
GM	-0.0474***	-0.0486***	-0.0507***	-0.0578***	-0.0643***	-0.0625***
	(0.00964)	(0.0101)	(0.0109)	(0.0118)	(0.0121)	(0.0120)
R-squared	0.289	0.287	0.288	0.298	0.312	0.309
	Reduced Form					
GM Shock	-0.0143	-0.0184	-0.0315**	-0.0585***	-0.0565***	-0.0248**
	(0.00942)	(0.0116)	(0.0134)	(0.0136)	(0.0137)	(0.0113)
R-squared	0.163	0.167	0.196	0.270	0.258	0.185
	Two-stage least squares					
GM	-0.112	-0.114	-0.0862**	-0.127***	-0.137***	-0.0777**
	(0.0767)	(0.0743)	(0.0349)	(0.0318)	(0.0351)	(0.0318)
R-squared	0.0296	0.0424	0.225	0.0993	0.110	0.299
N	129	129	129	129	129	129
Mean Dep Var	38.88	38.88	38.88	38.88	38.88	38.88
SD Dep Var	3.151	3.151	3.151	3.151	3.151	3.151
SD GM	28.80	28.80	28.80	28.80	28.80	28.80

Notes: This table reports results on black men's upward mobility using 1880-1940 southern-state-of-birth to construct the instrument for black population changes. The unit of observation is a commuting zone. Dependent variable is expected mean individual income rank for individuals with parents at the 25th percentile. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1978 and 1983. Independent variable is the percentile of black population increase during the Great Migration, or the instrument for the same. Columns 1-5 use southern state of birth of the black population in northern cities in 1880, 1910, 1920, 1930 and 1940, respectively, to define black southern migrant shares. Predicted southern county outflows between 1940 and 1970 are aggregated to the state level and assigned to northern cities according to the share of the black population born in that southern state and living in the destination city in the year indicated. Column 6 uses 1935-1940 black migration patterns to define the shares and assigns predicted southern county outflows to northern cities based on these shares. Controls include share of urban population made up of 1880, 1910, 1920, 1930, 1940, or 1935-1940 black southern migrants respectively. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE A9: *First stage in levels of 1940-1970 black population increase*



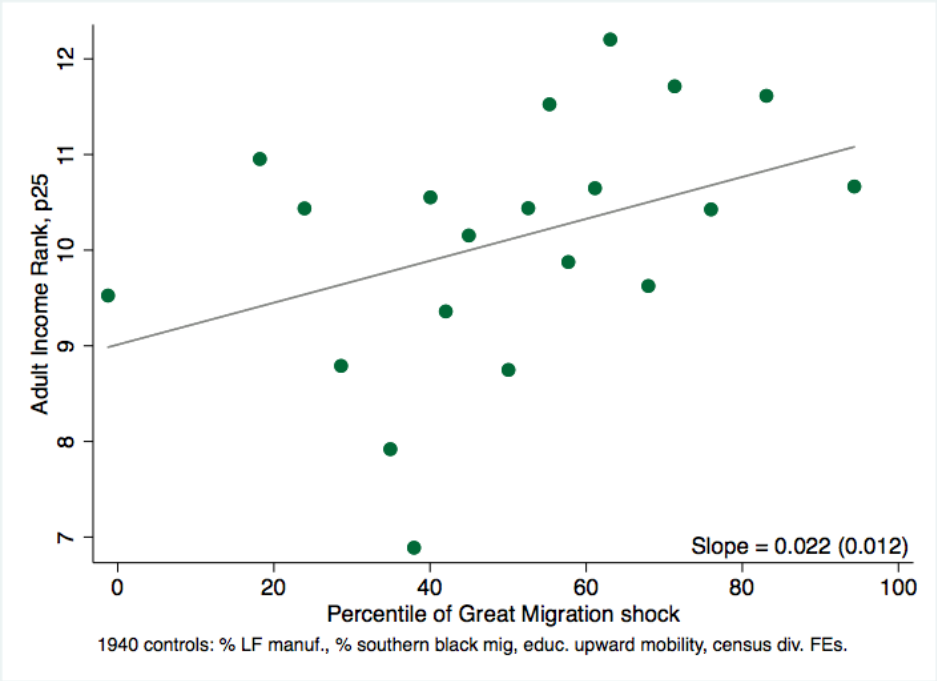
(a) Full sample



(b) Windsorized

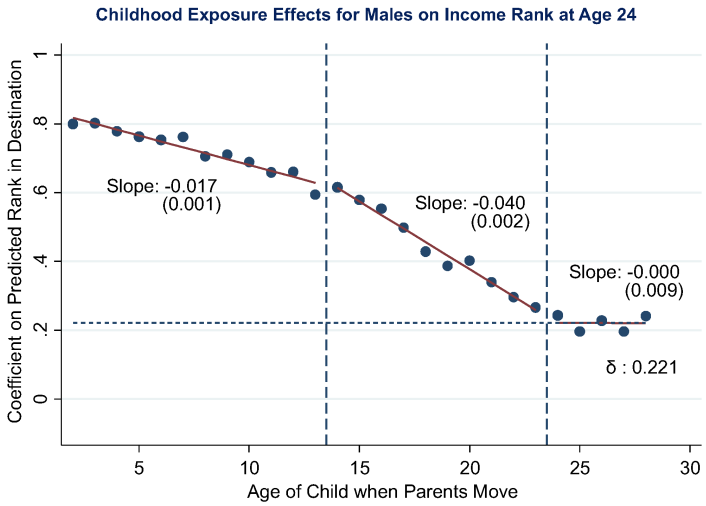
Notes: Panel (a) shows the relationship between actual urban black population increase from 1940-1970 as a share of the initial 1940 population and predicted increase of the same. Panel (b) shows this relationship after windsorizing the sample based on the 5th and 95th percentile of increases. Source: Complete count 1940 census; CCDB; Boustan (2016).

FIGURE A10: *GM Impact on racial gap between men, low income parents*



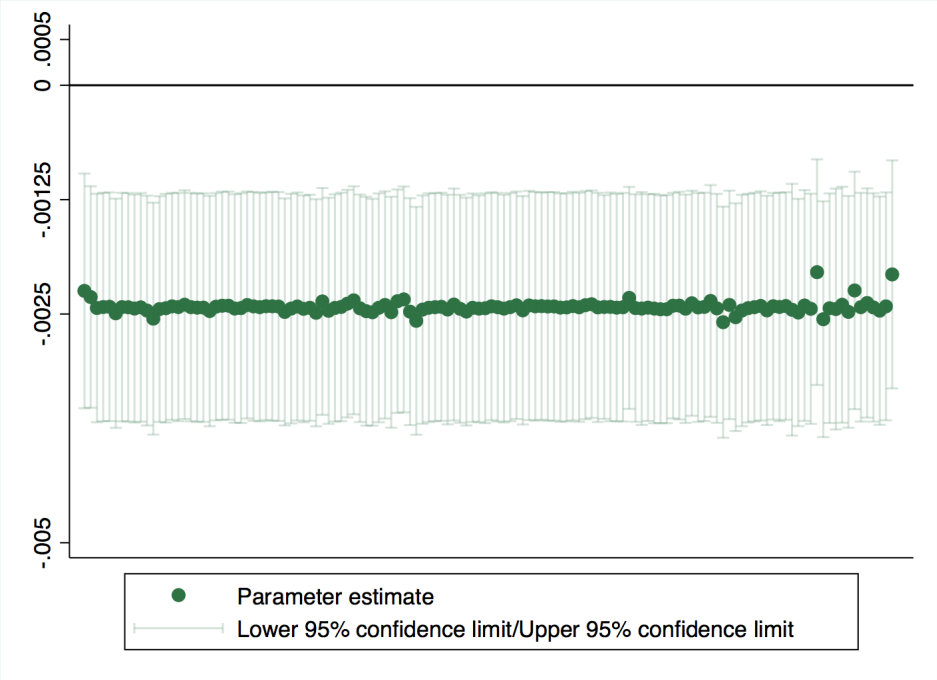
Notes: This binned scatterplot depicts the relationship between the racial gap in upward mobility among men in 2015 and the percentile of predicted black population change between 1940 and 1970 (in units of 30 percentile points). The unit of observation is a commuting zone. Upward mobility is defined as mean individual or household income rank by childhood commuting zone where income is measured from IRS tax returns for cohorts born between 1978 and 1983. Pooled income refers to household income, as opposed to individual income. Independent variable is predicted change in black population share between 1940 and 1970. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Chetty, Hendren, Jones, and Porter (2018); IPUMS Complete Count 1940 US Census; Boustan (2016).

FIGURE A11: *Heterogeneity in childhood exposure effects by age of child (Chetty et al., 2018)*



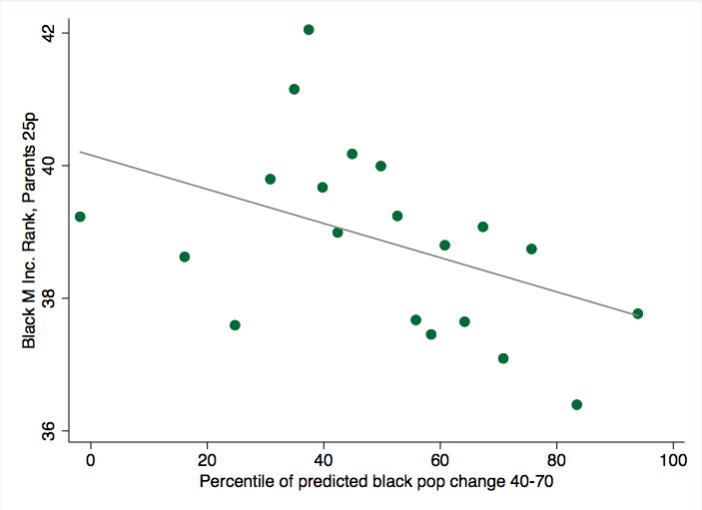
Notes: This image from Chetty et al. (2018) depicts heterogeneity in childhood exposure effects by age of exposure. Early years of childhood exposure have more muted impacts compared to teen years of exposure.

FIGURE A12: *Great Migration effect robust to leaving out each CZ once from sample*

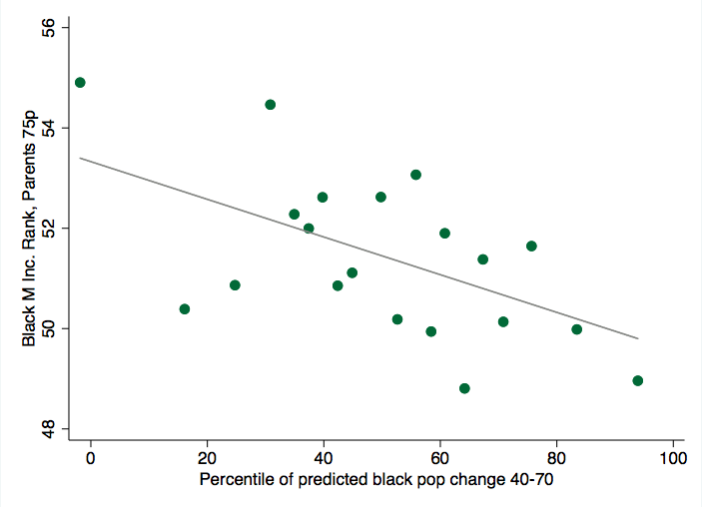


Notes: This figure plots the coefficient on percentile of predicted black population change in 130 separate regressions where each CZ in the sample has been left out of the regression once. 95% confidence intervals indicated.The unit of observation is a commuting zone. Dependent variable is the estimated causal impact of one additional year of childhood in the commuting zone on adult household income rank for men and women with parents at the 25th percentile of the parent income distribution. Income is measured from IRS tax returns for cohorts and parents of cohorts born between 1980 and 1986. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects.

FIGURE A13: *Great Migration impact on black men's outcomes, residualizing southern county net-migration on state FEs*



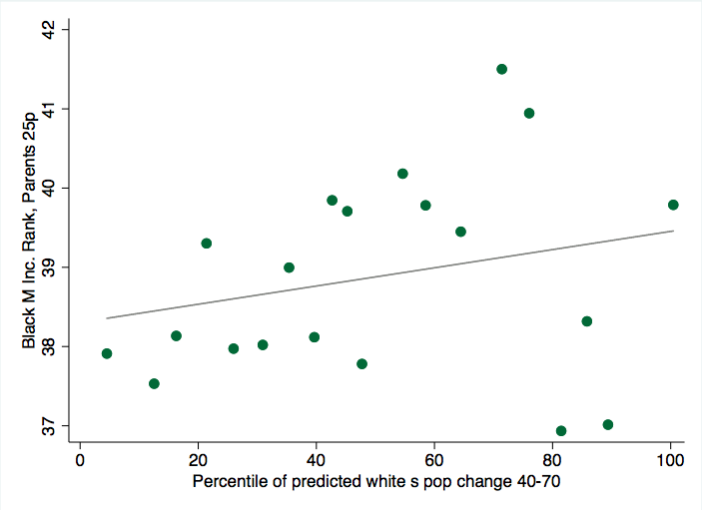
(a) 25th percentile



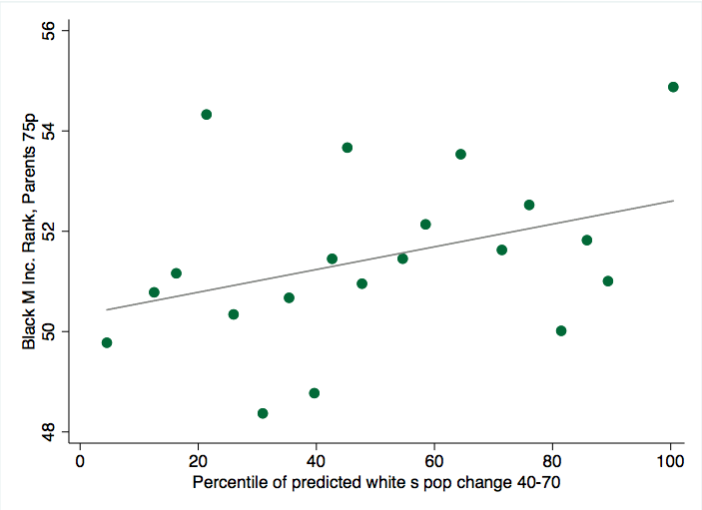
(b) 75th percentile

Notes: Panel (a) depicts a binned scatterplot of the relationship between the percentile of predicted black in-migration and black men's upward mobility (25th percentile of parent income distribution). In this definition of the Great Migration shock, southern county net-migration rates have first been residualized on southern state fixed effects. Panel (b) depicts the same for black men from the 75th percentile of parent income distribution. The right hand side variable is grouped into 20 bins (5 percentiles each). The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Upward mobility is defined as mean individual or household income rank by childhood commuting zone where income is measured from IRS tax returns for cohorts born between 1978 and 1983. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Chetty, Hendren, Jones, and Porter (2018); IPUMS Complete Count 1940 US Census; Boustan (2016).

FIGURE A14: *White southern migration impact on black men's outcomes*



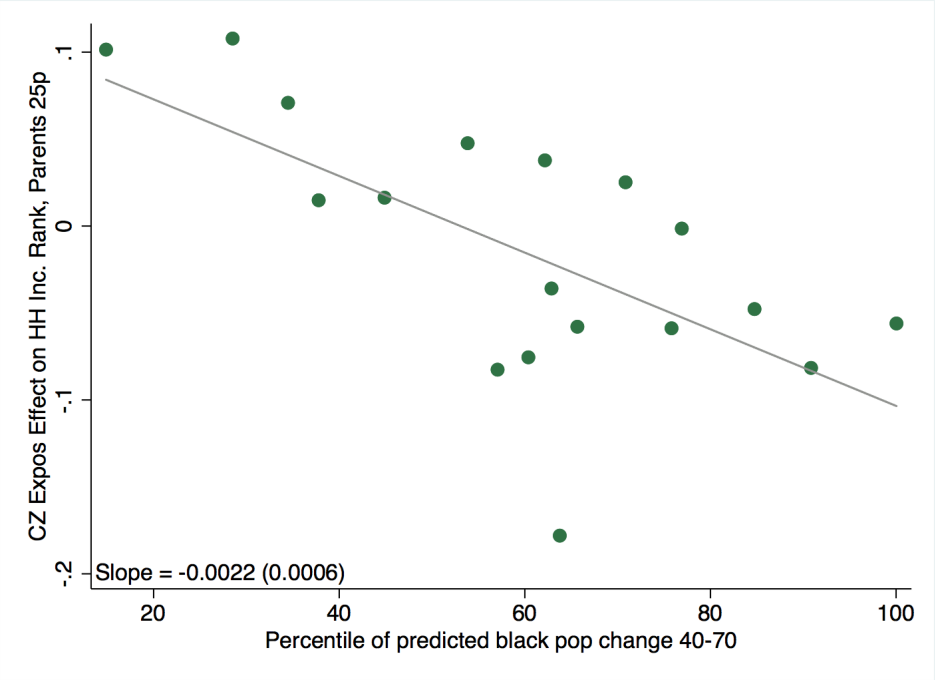
(a) 25th percentile



(b) 75th percentile

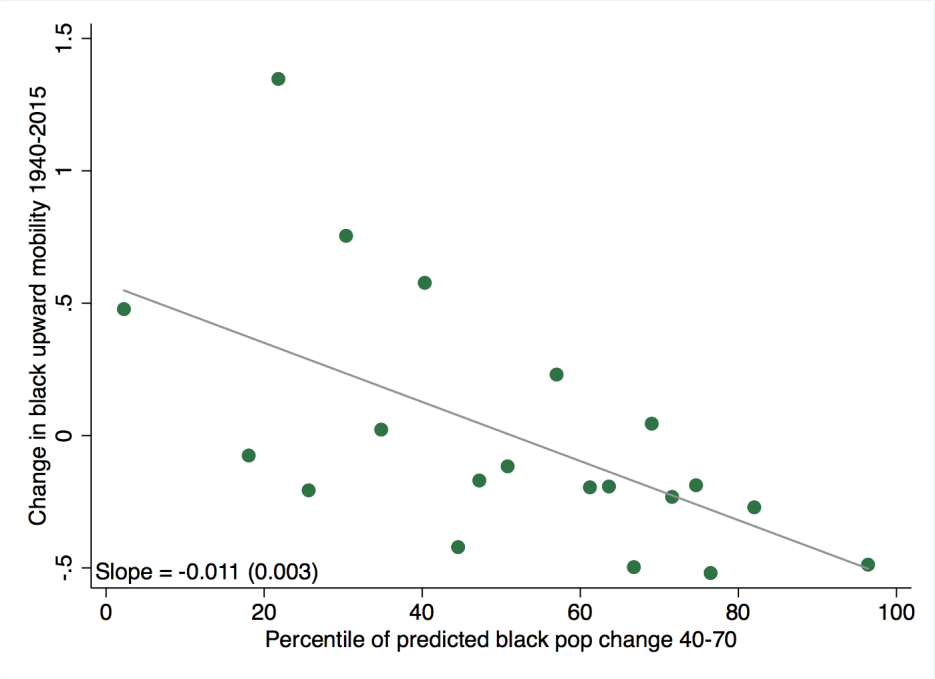
Notes: Panel (a) depicts a binned scatterplot of the relationship between the percentile of predicted white southern in-migration and black men's upward mobility (25th percentile of parent income distribution). Panel (b) depicts the same for black men from the 75th percentile of parent income distribution. The right hand side variable is grouped into 20 bins (5 percentiles each). The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Upward mobility is defined as mean individual or household income rank by childhood commuting zone where income is measured from IRS tax returns for cohorts born between 1978 and 1983. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Chetty, Hendren, Jones, and Porter (2018); IPUMS Complete Count 1940 US Census; Boustan (2016).

FIGURE A15: *Great Migration impact on CZ exposure effects, controlling for quartiles of black population share in 1940*



Notes: This figure depicts a binned scatterplot of the relationship between the percentile of predicted black population increase and the CZ childhood exposure effects for individuals from low income families. The unit of observation is a commuting zone. The right hand side variable is grouped into 20 bins (5 percentiles each). Both right hand side and left hand side variables have been residualized on the following controls from 1940: share of urban population made up of 1935-1940 black southern migrants, the share of labor force in manufacturing, census division fixed effects, and quartiles of the black population share in 1940. Data sources: Chetty, Hendren, Jones, and Porter (2018); IPUMS Complete Count 1940 US Census; Boustan (2016).

FIGURE A16: *Great Migration impact on change in black men's upward mobility, 1940-2015*



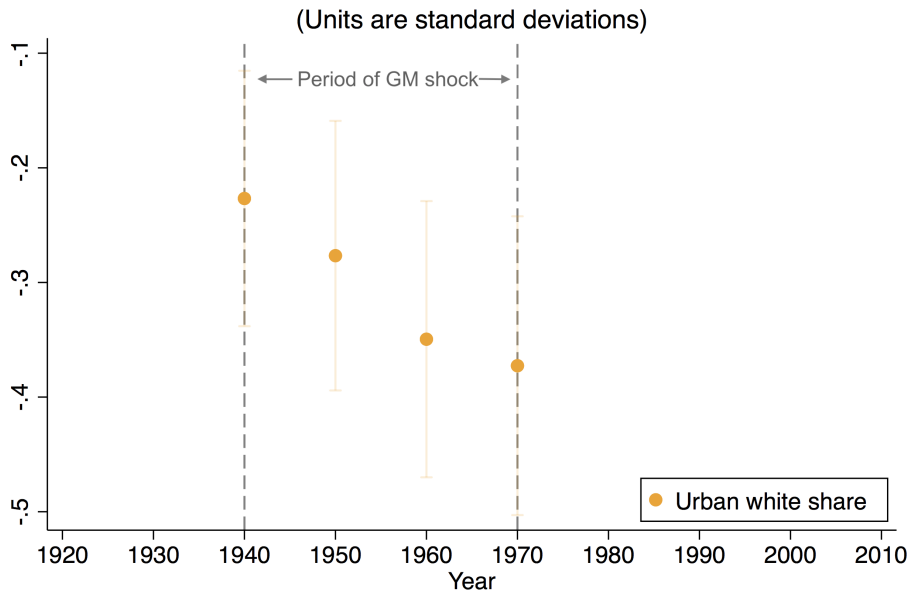
Notes: This figure depicts a binned scatterplot of the relationship between the percentile of predicted black population increase and the change in black men's upward mobility between 1940 and 2015. The unit of observation is a commuting zone. To construct the outcome variable, I take difference in the Z-score of black male income upward mobility in 2015 (for men from parents at the median of the national parent income distribution) and the Z-score of black male educational upward mobility in 1940 (for boys whose parents had 5-8 years of schooling, the national median for adults). I then standardize this difference, so that the units of outcome variables are standard deviations. The right hand side variable is grouped into 20 bins (5 percentiles each). Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, the share of labor force in manufacturing, and census division fixed effects. Data sources: Chetty, Hendren, Jones, and Porter (2018); IPUMS Complete Count 1940 US Census; Boustan (2016).

Appendix B

Appendix to Chapter 2

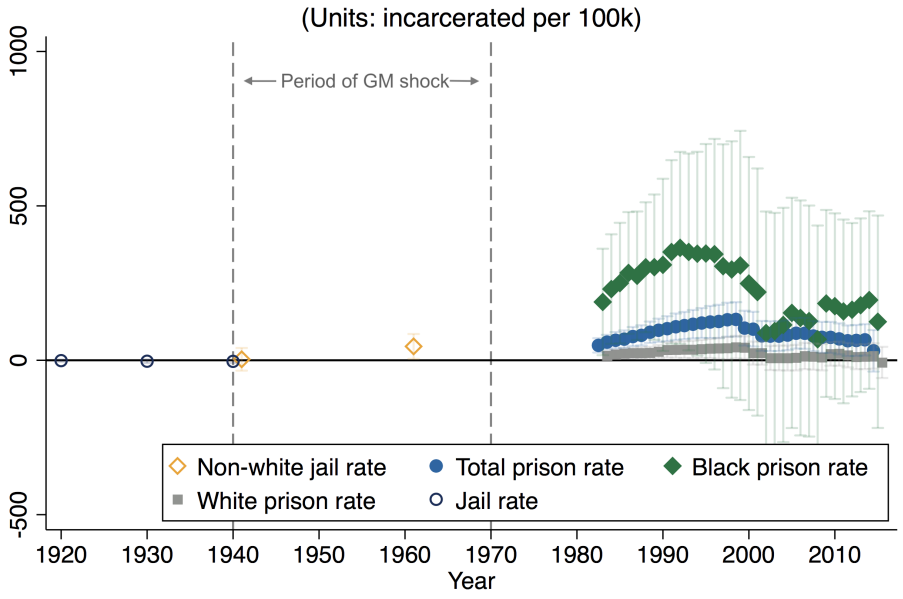
B1 Additional results on local mechanisms

FIGURE B1: *Great Migration impact on urban white share*



Notes: This figure plots the coefficient on percentile of predicted black population change (units are 30 percentile points, approximately 1 standard deviation) in separate regressions for each year where the dependent variable is the urban white population share. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: County Data Books 1947-1977.

FIGURE B2: *Great Migration impact on incarceration rates, levels*



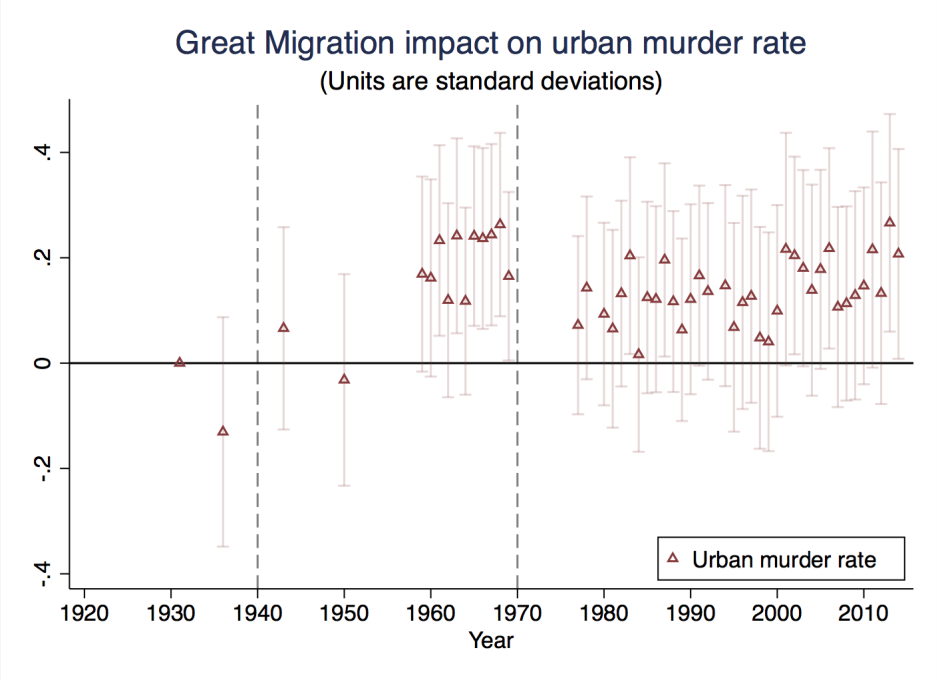
Notes: This figure plots the coefficient on percentile of predicted black population change (units are 30 percentile points, approximately 1 standard deviation) in separate regressions for each year where the dependent variable is county jail population per 100,000 (1940 and 1960) or federal and state prison population by 100,000 by county-of-commitment from 1983-2015. Each jail or prison population group is normalized by the population for that group. Federal and state prison rates are for black and white men aged 15-64. The unit of observation is a commuting zone. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: 1940 county jail rates come from US IPUMS complete count US census; 1960 county jail rates come from 1960 Census report on county correctional institution population; data on 1983-2015 federal and state prison population by county-of-commitment come from Vera Institute of Justice In Our Backyards Database.

TABLE B1: *Great Migration impact on housing supply restrictions*

	LPPI	SPII	SCII	LZAI	LPAI	LAI	DRI	OSI	EI	SRI	ADI	WRLURI
	Ordinary Least Squares											
GM	0.00550***	-0.000658	-0.00524**	-0.00124	0.00367**	-0.000553	0.000782	0.00189*	0.000150	0.00175	0.0296***	0.00656***
	(0.00141)	(0.00270)	(0.00221)	(0.00163)	(0.00155)	(0.000357)	(0.000704)	(0.000984)	(0.000641)	(0.00110)	(0.00585)	(0.00182)
R-squared	0.225	0.428	0.176	0.121	0.142	0.211	0.161	0.150	0.321	0.102	0.485	0.514
	Reduced Form											
GM Shock	0.00406**	-0.00203	-0.00807***	-0.00152	0.00246	-0.000804*	-0.000932	0.000996	0.000640	0.00361***	0.00598	0.00208
	(0.00171)	(0.00313)	(0.00254)	(0.00191)	(0.00187)	(0.000418)	(0.000841)	(0.00118)	(0.000759)	(0.00129)	(0.00718)	(0.00221)
R-squared	0.211	0.467	0.241	0.160	0.135	0.251	0.170	0.148	0.340	0.145	0.462	0.499
	Two-stage least squares											
GM	0.0114**	-0.00570	-0.0227***	-0.00427	0.00690	-0.00226**	-0.00262	0.00280	0.00180	0.0102***	0.0168	0.00584
	(0.00466)	(0.00855)	(0.00793)	(0.00536)	(0.00502)	(0.00115)	(0.00240)	(0.00318)	(0.00208)	(0.00388)	(0.0189)	(0.00588)
R-squared	0.206	0.460	-0.00491	0.101	0.148	0.234	0.0849	0.162	0.326	-0.0505	0.491	0.521
N	120	120	120	120	120	120	120	120	120	120	120	120
SD GM	28.11	28.11	28.11	28.11	28.11	28.11	28.11	28.11	28.11	28.11	28.11	28.11

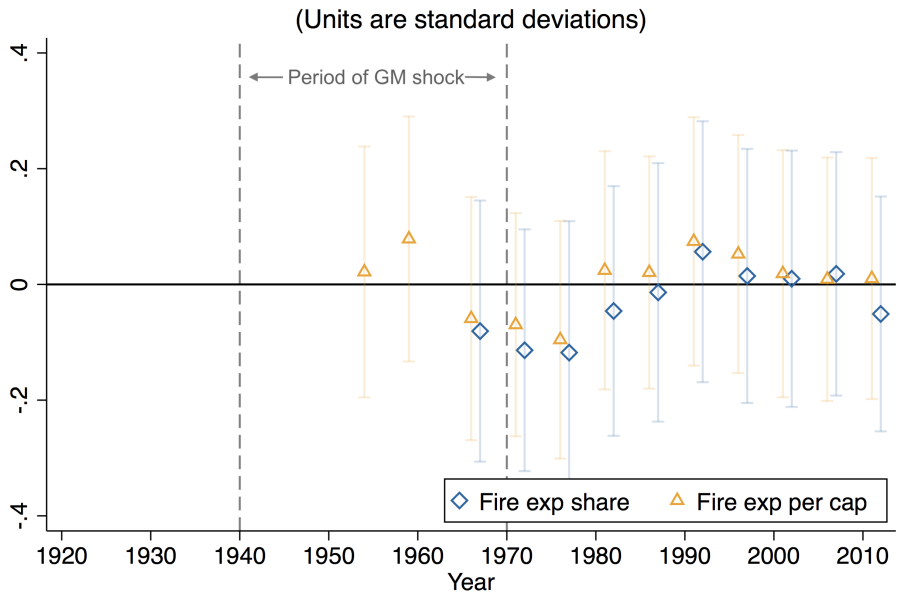
Notes: LPPI = Local Political Pressure Index; SPII = State Political Involvement Index; SCII = State Court Involvement Index; LZAI = Local Zoning Approval Index; LPAI = Local Project Approval Index; LAI = Local Assembly Index; DRI = Density Restrictions Index; OSI = Open Space Index; EI = Exactions Index; SRI = Supply Restrictions Index; ADI = Approval Delay Index; WRLURI = Wharton Residential Land Use Regulation Index. *Source*: Wharton Land Regulation Data.

FIGURE B3: *Great Migration impact on murder rates with pre-period control*



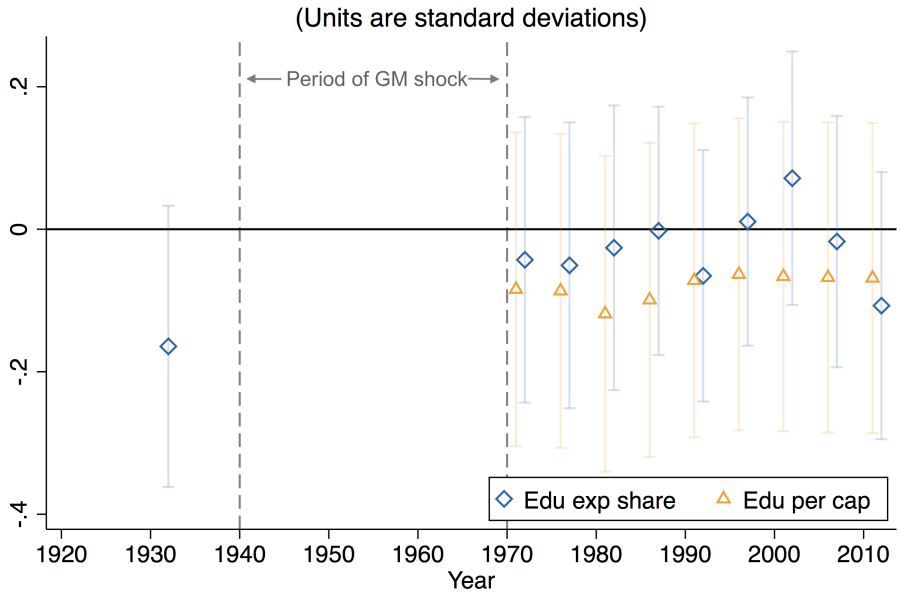
Notes: This figure plots the coefficient on percentile of predicted black population change (units are 30 percentile points, approximately 1 standard deviation) in separate regressions for each year where the dependent variable is urban murder rates per 100,000 in commuting zones. All regressions include controls for the 1931 murder rate. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data source: Uniform Crime Reports.

FIGURE B4: *Great Migration impact on fire-fighting investments*



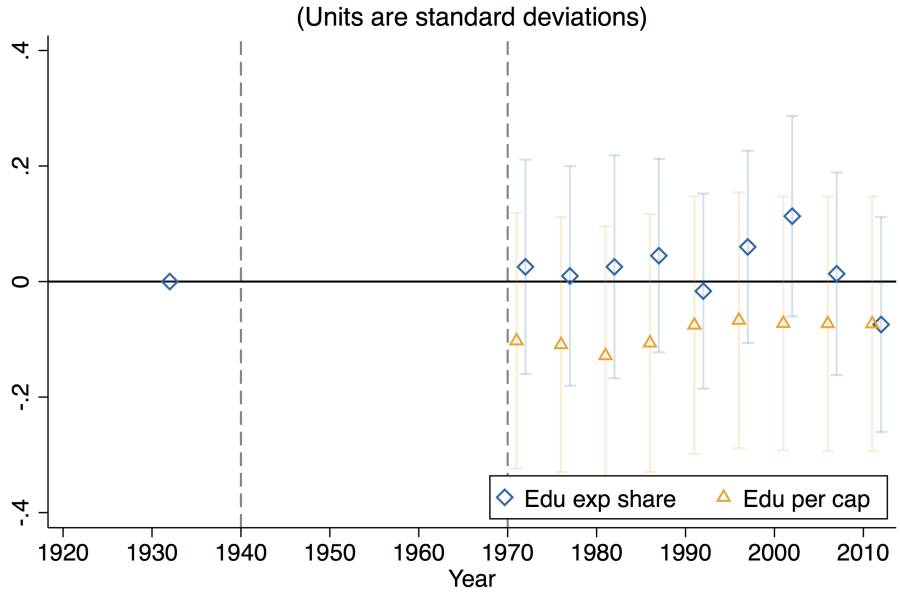
Notes: This figure plots the coefficient on percentile of predicted black population change (units are 30 percentile points, approximately 1 standard deviation) in separate regressions for each year where the dependent variable is either the share of local government expenditures on fire-fighting or fire-fighting expenditures per student. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: US Census Bureau Annual Survey of Local Governments (1967-2012).

FIGURE B5: *Great Migration impact on schooling investments*



Notes: This figure plots the coefficient on percentile of predicted black population change (units are 30 percentile points, approximately 1 standard deviation) in separate regressions for each year where the dependent variable is either the share of local government expenditures on education or education expenditures per student. Education expenditure data are for elementary and high school districts. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Data on education expenditure shares and per student spending come from Financial statistics of states and local governments, 1932; US Census Bureau Annual Survey of Local Governments (1967-2012).

FIGURE B6: *Great Migration impact on schooling investments, with pre-period control*



Notes: This figure plots the coefficient on percentile of predicted black population change (units are 30 percentile points, approximately 1 standard deviation) in separate regressions for each year where the dependent variable is either the share of local government expenditures on education or education expenditures per student. Education expenditure data are for elementary and high school districts. All regressions include controls for the 1932 share of local government expenditures on education. The unit of observation is a commuting zone. Units of outcome variables are standard deviations. Baseline 1940 controls include share of urban population made up of 1935-1940 black southern migrants, educational upward mobility, share of labor force in manufacturing, and census division fixed effects. Data sources: Data on education expenditure shares and per student spending come from Financial statistics of states and local governments, 1932; US Census Bureau Annual Survey of Local Governments (1967-2012).

TABLE B2: *Impact of Great Migration on neighborhood quality in 2000*

	Standardized variables measured in 2000			
	Murder Rate	Racial Segregation (Theil)	Percent Single Parent	Segregation of Poverty
GM Shock	0.269**	0.598***	0.418***	0.626***
	(0.126)	(0.157)	(0.137)	(0.160)
Baseline mean	3.963	0.192	0.205	0.065
Std Dev	3.219	0.101	0.027	0.026
Observations	125	129	129	129
State FEs	Y	Y	Y	Y
CZ 1940 Pop	Y	Y	Y	Y

This table reports the coefficient on percentile of predicted black population change in regressions where the dependent variable is the urban murder rate per 100,000; the Theil index of racial segregation across census tracts; the Theil index of segregation of below median income families across census tracts; and the percentage of single-parent households.

B2 Public Finance and Neighborhoods Database, 1920-2015

B2.1 Data sources and key measures

Private school enrollment rates

Data on private school enrollments come from two different sources depending on the time period. For pre-1940 statistics on private school enrollment, I use tabulations on city school systems from the 1922 Biennial Survey of Education report. This report contains the total number of elementary and high school students enrolled in private schools in that city as well as total enrollment in the city.

For 1970 onwards, I use county-level counts of private school enrollments from IPUMS National Historical Geographic Information System ("NHGIS"), which I aggregate up to the CZ level. Starting in 1970 through 2010, enrollment is also reported separately for elementary and high school students and separately by race from 1970 to 2000.

Incarceration rates

For 1920 and 1930, I use the complete count censuses to construct the percent of the population in a county that is incarcerated in jails or local correctional institutions. I do not include the federal or state prison in these estimates as it is not possible to allocate state and federal prisoners back to localities they came from. In 1940, I digitized data from a census report on the incarcerated population. For 1960, I digitized data from the 1960 US Census publication, which includes a table on the incarcerated population and reports the non-white and white incarcerated population by county separately.

For the post 1970 period, I use a rich new dataset from the Vera Institute of Justice In Our Backyards Symposium ("IOB"), which provides counts of federal and state prisoners by their county-of-commitment to federal and state prison. These data begin in the year 1983. These figures are available separately by race. Due to reliability issues for the local jail population in these data, I focus on total jail rates rather than jail population breakdowns by race.

Crime rates

For crime rates, I focus on murder rates as these are less subject to reporting bias than other crime categories, such as property crime or non-fatal violent crimes. I digitize murder rates for cities with 25,000 or more from the Uniform Crime Reporting publications ("UCR") of the FBI in 1931, 1943, and 1950.[1] For the years 1958 to 1969 I use city-level tabulations of murder rates from UCR available from ICPSR. Finally, for the post 1970 period, I use county-level tabulations of UCR murder rates available from the IOB database. In addition to looking at crime rates as a measure of neighborhood quality, I also use data on the intensity and duration of race riots in major cities in the 1960s.[2]

[1]Some large cities did not report to the FBI UCR series in these years. A notable case is New York City. For these cities, I supplement using data generously shared by Price Fishback.

[2]These data were generously shared by William Collins and Robert Margo. See Collins and Margo (2007).

Local government expenditures

Data on local government expenditures come from surveys of state and sub-state level governments conducted by the US Census Bureau. For each expenditure category, I focus on aggregate spending by various local governments in a county area. The advantage to this approach is that changes in which levels or types of government are responsible for providing a certain public good will not affect this measure of spending. The first full set of such data are available in the 1932 publication of *Financial statistics of states and local governments*. I digitize county aggregate and individual local government expenditures from this report.

For post-migration years, I rely on the US Census Bureau Annual Survey of Local Governments, which provides individual government expenditure data in digital format for roughly 15,000 local governments across the United States from 1967 to 2012. I also include data on city government expenditures available for intermittent years from 1948 to 1975 from the City Data Books available from ICPSR and for the year 1962, I include data available on different expenditure categories from the County Data Book also available from ICPSR. In the case of police expenditures, I supplement these two measures counts of police officers per capita using the complete count censuses available from IPUMS for the years 1920, 1930, and 1940 and US Census Bureau data surveying public sector employment in cities from 1951-2007.

For each data set, I construct commuting zone area aggregate expenditures for the expenditure categories of interest. I focus on expenditures per capita (or per student), and the share of total expenditures devoted to that expenditure category.

For example, for police spending, CZ-area local government expenditure share is defined as

$$\text{Pol. Exp. Share}_{CZ} = \frac{\text{\$Spent on Police by All Local Governments}_{CZ}}{\text{\$Spent by All Local Governments}_{CZ}}$$

and per capita expenditures at the CZ-area level are defined as

$$\text{Per Cap Pol. Exp.}_{CZ} = \frac{\text{\$Spent on Police by All Local Governments}}{\text{Population}_{CZ}}$$

Finally, I focus on categories of expenditures over which local governments have a large degree of discretion: police expenditures, education expenditures, and fire expenditures. Table F1 shows the the contribution of different levels of government (e.g., federal, state, county, etc.) to direct expenditures for each category of government spending.

TABLE F1: *Expenditure by government type by spending category*

Govt Type	Revenue	Rev (Own)	Dir Exp	Elem + HS	Fire Prot	Health + Hosp	Highway	Parks & Rec	Police	Pub. Welf	Sanitation
Fed	18.93%	24.90%	22.95%	0.00%	0.00%	6.12%	0.36%	3.24%	3.73%	6.27%	0.82%
State	42.92%	42.76%	34.17%	1.14%	0.00%	42.77%	60.37%	14.44%	13.31%	79.42%	5.13%
County	8.73%	7.40%	9.51%	7.84%	13.83%	26.71%	14.76%	16.76%	23.77%	10.33%	16.03%
Muni	12.45%	12.72%	13.90%	8.31%	67.90%	10.64%	19.41%	51.29%	54.80%	3.78%	56.82%
Town	1.19%	1.22%	1.31%	2.24%	6.29%	0.25%	3.79%	3.75%	4.36%	0.09%	5.40%
Spec. Dist.	4.15%	4.13%	4.79%	0.24%	11.99%	13.51%	1.32%	10.52%	0.03%	0.11%	15.81%
School Dist.	11.64%	6.87%	13.38%	80.23%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
Total	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%

Notes: This table shows the breakdown in spending by government type for different spending categories, averaged across all reporting years from 1967-2012. Column 1 lists the government types: federal, state, county, municipality, town, special district, and school district. Column 2 shows the total breakdown of government revenue by government type. Column 3 shows this number net of intergovernmental transfers. Column 3 shows total direct government expenditures by government type. Starting with Column 4, the categories of spending from left to right are education for elementary and high school districts; fire protection services; health and hospitals; highways; parks and recreation; police protection; public welfare; and sanitation. Sanitation spending includes sewage and waste management. *Source*: US Census Bureau Annual Survey of Local Governments (1967-2012).

Appendix C

Appendix to Chapter 3

B1 appendix

C1 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] Ours differs 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

[1]There are, to our knowledge, two main published minimum wage databases for research purposes: (i) Vaghul and Zipperer (2016) dataset (1974-2016), and (ii) Neumark (2018) dataset (1960-2017).

[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 C2 for more details.

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. 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.[4]

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. 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).

Classification of industries by date of FLSA coverage. Which industry is covered by which amendment of the Fair Labor Standards Act? Table C1 shows the list of industries available in CPS 1962-1981 (see section C2) 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. 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[5] and

[4]See Department of Labor, Wage and Hour Division, History of Federal Minimum Wage Rates Under the Fair Labor Standards Act, 1938-2009.

[5]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

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[6], we consider that the following industries are covered by the 1938 FLSA: mining, manufacturing (durable and non-durable), transportation, communication and other utilities[7], 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[8] with sales over $1m, and to small retailers under certain conditions[9]. They also increase coverage to construction enterprises with sales over $350,000. Retail trade establishments and construction are therefore only partially covered in 1961,

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for workers working overtime, varies by age, etc.

[6]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.

[7]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.

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

[9]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

and are further affected by the 1966 amendments, and subsequent amendments. [10] Since in the CPS we do not 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.[11]

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,[12], 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[13]. The minimum wage in

[10]The 1966 amendments extended coverage to retail trade entreprises with sales over $500,000. In 1969, this threshold was reduced to $250,000. It was further increased to $350,000 in 1981, and to $500,000 in 1990. See p.25 in Neumark, 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.

[11]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.

[12]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.

[13]There were four notable exemptions in agriculture: (i) employees of farms employing less than 500 mandays of nonexempt labor in the highest quarter of the 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

agriculture was introduced at a lower rate than the federal rate, and fully converges to the federal rate only ten years later (see 3.2).

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, 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

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.

1974. [14] 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).[15] Coverage was extended to state and local government workers from January 1, 1986 after U.S. Supreme Court reversal. [16]

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

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

The minimum wage by year y, month m, industry j, state-group S and gender g, denoted

[14] 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. 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.

[15] See Supreme Court in the National League of Cities v. Usery (6/24/76).

[16] 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.

Table C1: *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.

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:[17]

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

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:

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

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:

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

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

[17]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. 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.

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

Finally, we convert nominal minimum wage rates into real minimum wage rates using the CPI-U-RS.[18]

[18]The annual CPI-U-RS series are available since 1947 from Census.

C2 March CPS (1962-1981)

This paper uses data from the March Current Population Survey (CPS)[19] to analyze the effect of the 1966 Fair Labor Standards Act on annual earnings, employment and on racial inequality. As noted in IPUMS documentation, 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. In particular, the IPUMS version of the CPS early files has an harmonized industry variable.

C2.1 Sample of interest

Figure C1 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,[20] For more information on the evolution of the universe of CPS employment questions, see IPUMS.[21], 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 [22], 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

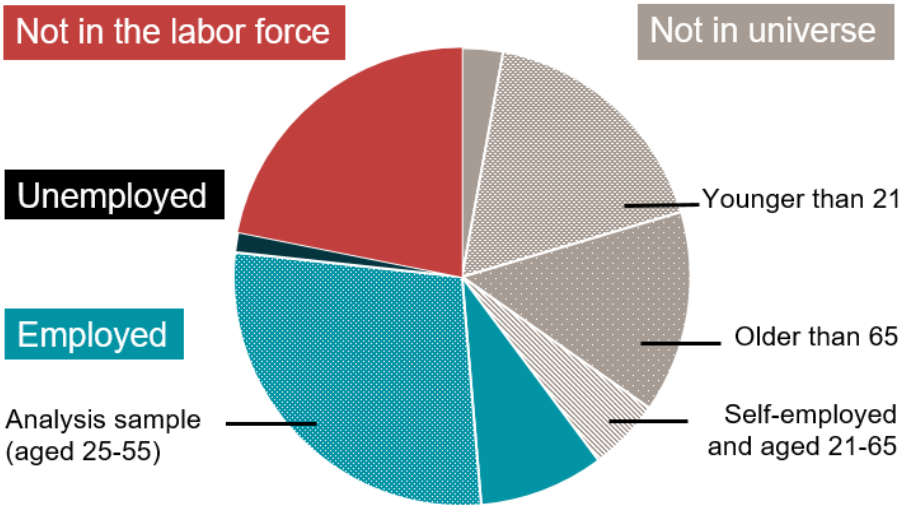
[19]Sarah Flood, Miriam King, Steven Ruggles, and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 5.0 [March CPS]. Minneapolis, MN: University of Minnesota, 2017.

[20]From March CPS 1962 to 1979, the lowest age cut-off for employment questions us 14. It is 15 starting in 1980.

[21]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.

[22]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 C1: *Analysis sample, before the reform (1966)*



Source: Authors' analysis of March CPS 1967.

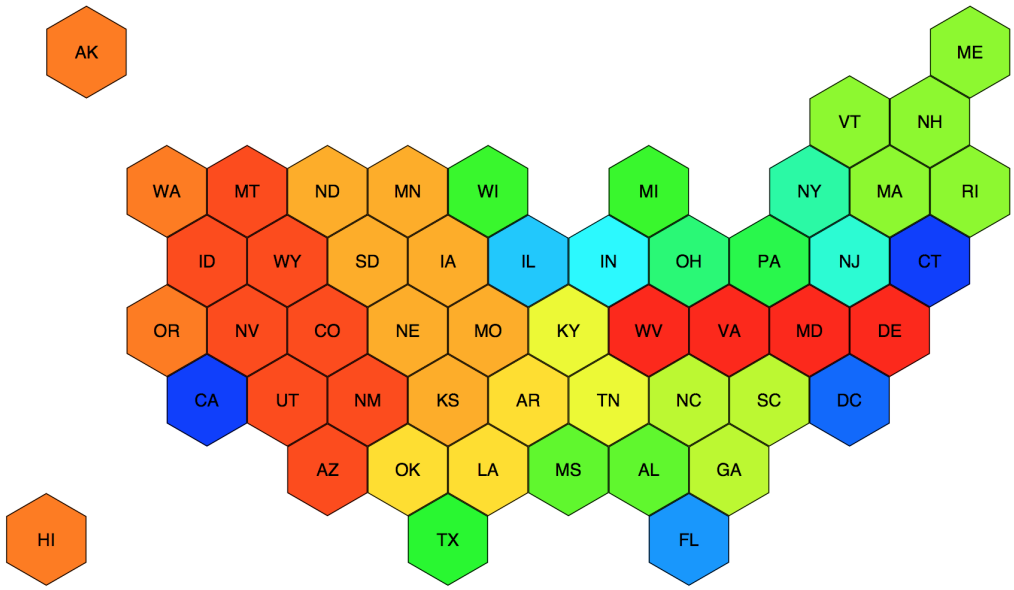
week or employed but not at work last week. Our analysis sample – the sample on which we conduct the bulk of our analysis of the effect of the 1966 reform on wages, and on the racial earnings gap (section 3.3), is conducted on prime age workers (25-55).

Unemployed or not in the labor force. When analyzing the employment effects of the 1966 reform (section 3.5), 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.

C2.2 State crosswalks

In some years, states are identified with their Federal Information processing standard (FIPS) state codes, and in some others (March CPS 1962, 1968-1971, 1972, and 1973-1976) some states are grouped together, 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

Figure C2: *State groups used in March CPS (1962-1980)*



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

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 C2). 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 C2). To a certain extent, the state groups share similar economic conditions.

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,[23] a few workers were in a state that was not identified: 25 in March CPS 1963 (0.2% of the representative sample of interest), 40 in March CPS 1964 (0.3%), and 13 in March CPS 1972 (0.04%).These observations are dropped from our analysis.

[23]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.

Table C2: *List of state groups used in March CPS (1962-1980)*

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.

Given the small number of workers involved, we believe this does not introduce any bias in our results.

C2.3 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, from March CPS 1962-1967, and IND1950, 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. 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" at IPUMS.org. 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 C1 above). Importantly, this classification allows us to disentangle industries covered by the Fair Labor Standards Act from those covered by its subsequent amendments.

C2.4 Topcoding

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

Before 1996, no replacement is provided in the CPS. We replace the topcoded values by 1.5 the value of the highest non-topcoded income. This replacement is done by industry

type (covered in 1938, 1961, 1966, 1974 or 1986). [24] Among employed individuals in March CPS 1962-1972,[25] less than 1% of the sample has topcoded incomes. This share increases progressively in the 1970s and reaches almost 5% in 1978, 8% in 1979, and peaks at 10% in 1980. Starting in 1981, this share is consistently below 5% (except for the years 1992-1994 where it is between 5% and 8%).

After 1996, topcoded values are replaced with values that vary with individual characteristics (gender, race, and full-time/part-time status). [26]

C2.5 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 C3. 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 C3 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 C3).

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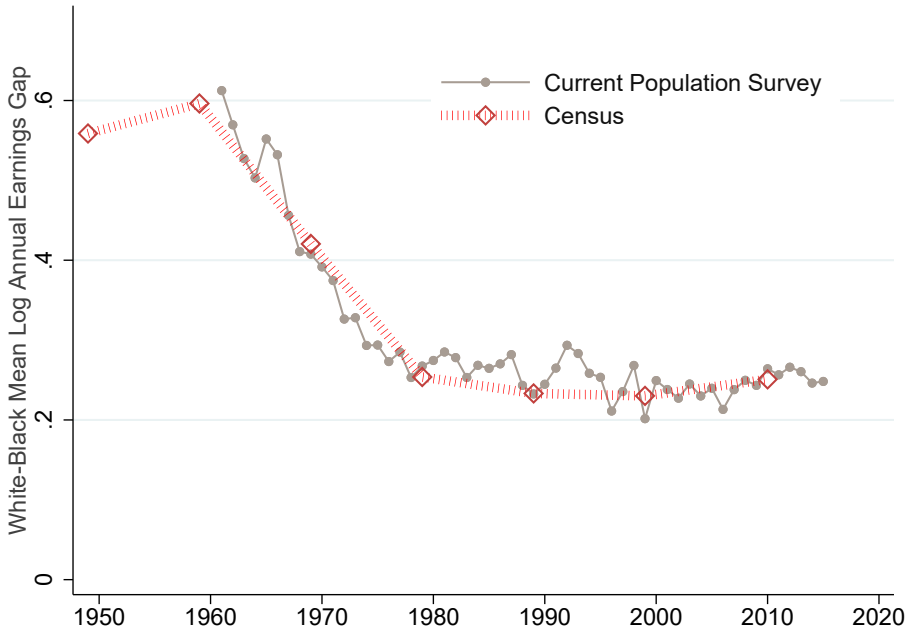
[24] This is consistent with assuming that the distribution of incomes Pareto distributed, with a pareto coefficient of 3, that is typically used in the literature on top-income earners (Piketty *et al.*, 2018).

[25] 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.

[26] For CPS samples starting in 1996, see replacement values at IPUMS for the variable INCWAGE.

Figure C3: *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 C3: *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	0.90	0.10	0.68	0.32	37,176	19,523	42,029	21,113
1963	9,638	22,277,274	0.90	0.10	0.68	0.32	37,607	18,865	42,412	21,267
1964	14,222	34,344,403	0.89	0.11	0.68	0.32	38,736	21,529	44,216	21,343
1965	14,126	34,637,727	0.89	0.11	0.68	0.32	39,677	22,997	45,379	22,158
1966	30,113	37,407,666	0.89	0.11	0.68	0.32	41,196	23,168	47,224	22,461
1967	19,191	38,490,848	0.89	0.11	0.68	0.32	42,575	24,522	49,036	23,091
1968	30,277	39,451,389	0.89	0.11	0.66	0.34	43,219	26,019	50,127	24,098
1969	30,808	40,044,846	0.89	0.11	0.66	0.34	44,575	28,242	52,070	24,935
1970	29,626	40,963,562	0.90	0.10	0.66	0.34	47,062	29,253	55,248	26,015
1971	29,130	40,594,657	0.89	0.11	0.65	0.35	47,563	30,486	55,870	26,946
1972	28,214	41,861,238	0.90	0.10	0.65	0.35	47,460	30,936	55,969	27,039
1973	28,025	42,659,268	0.89	0.11	0.64	0.36	49,744	33,601	59,060	28,255
1974	27,620	43,773,753	0.90	0.10	0.64	0.36	49,962	33,810	59,852	28,155
1975	26,474	43,108,371	0.90	0.10	0.63	0.37	48,364	34,284	58,235	27,912
1976	28,407	44,987,015	0.90	0.10	0.62	0.38	47,557	33,346	57,386	27,866
1977	33,944	46,526,101	0.90	0.10	0.61	0.39	48,197	34,215	58,382	28,390
1978	33,936	48,250,592	0.89	0.11	0.61	0.39	48,588	34,812	59,187	28,665
1979	34,468	50,109,925	0.90	0.10	0.60	0.40	48,789	36,335	59,923	29,044
1980	41,137	51,461,168	0.90	0.10	0.58	0.42	48,862	36,004	60,306	29,636
1981	41,859	53,389,185	0.90	0.10	0.58	0.42	47,624	34,640	58,541	29,490
US Census										
1960	1,662,241	33,244,820	0.90	0.10	0.69	0.31	35,857	19,429	40,231	20,684
1970	403,015	40,301,500	0.90	0.10	0.65	0.35	46,243	30,102	54,341	26,724
1980	2,613,374	52,267,480	0.89	0.11	0.58	0.42	46,870	36,367	57,205	29,905

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

Sample: Adults 25-55, 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.

C3 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:

$$\begin{aligned} G^{\text{total}} &= \frac{1}{N_w}\sum_i log(\omega_i^w) - \frac{1}{N_b}\sum_i log(\omega_i^b) \\ &= \bar{X_w} - \bar{X_b} \end{aligned} \tag{C.5}$$

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:

$$\begin{aligned} \bar{X_w} &= \frac{1}{N_w}\sum_i log(\omega_i^w) \\ &= \frac{N_w^c}{N_w}\cdot\frac{1}{N_w^c}\sum_{i,w} log(\omega_i^c) + \frac{N_w^t}{N_w}\cdot\frac{1}{N_w^t}\sum_{i,w} log(\omega_i^t) \\ &= s_w^c\cdot\frac{1}{N_w^c}\sum_{i,w} log(\omega_i^c) + s_w^t\cdot\frac{1}{N_w^t}\sum_{i,w} log(\omega_i^t) \end{aligned} \tag{C.6}$$

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:

$$\begin{aligned} G^{\text{total}} &= s_w^c\bar{X_w^c} + s_w^t\bar{X_w^t} - s_b^c\bar{X_b^c} - s_b^t\bar{X_b^t} \\ &= (s_w^c\bar{X_w^c} - s_b^c\bar{X_b^c}) + (s_w^t\bar{X_w^t} - s_b^t\bar{X_b^t}) \\ &= (s_w^c\bar{X_w^c} - s_w^c\bar{X_b^c}) + (s_w^t\bar{X_w^t} - s_w^t\bar{X_b^t}) + s_w^c\bar{X_b^c} - s_b^c\bar{X_b^c} + s_w^t\bar{X_b^t} - s_b^t\bar{X_b^t} \\ &= s_w^c G_c + s_w^t G_t + \underbrace{\bar{X_b^c}(s_w^c - s_b^c) + \bar{X_b^t}(s_w^t - s_b^t)}_{=\lambda} \end{aligned} \tag{C.7}$$

$$
\begin{aligned}
\lambda &= s_w^c G_c + s_w^t G_t + \bar{X}_b^c (s_w^c - s_b^c) + \bar{X}_b^t (s_w^t - s_b^t) \\
&= s_w^c \bar{X}_b^c - s_b^c \bar{X}_b^c + s_w^t \bar{X}_b^t - s_b^t \bar{X}_b^t \\
&= s_w^c \bar{X}_b^c - s_w^c \bar{X}_b^t + s_w^c \bar{X}_b^t - s_b^c \bar{X}_b^c + s_w^t \bar{X_b} t - s_b^t \bar{X}_b^t - (s_b^c \bar{X}_b^c - s_b^c \bar{X}_b^t + s_b^c \bar{X}_b^t - s_w^t \bar{X}_b^t) \\
&= s_w^c G_b^{ct} + s_w^c \bar{X}_b^t - s_b^t \bar{X}_b^t - (s_b^c G_b^{ct} + s_b^c \bar{X}_b^t - s_w^t \bar{X}_b^t) \\
&= s_w^c G_b^{ct} - s_b^c G_b^{ct} + \bar{X}_b^t \times (\underbrace{s_w^c + s_w^t}_{=1} - (\underbrace{s_b^c + s_b^t}_{=1})) \\
&= s_w^c G_b^{ct} - s_b^c G_b^{ct}
\end{aligned}
\tag{C.8}
$$

Therefore:

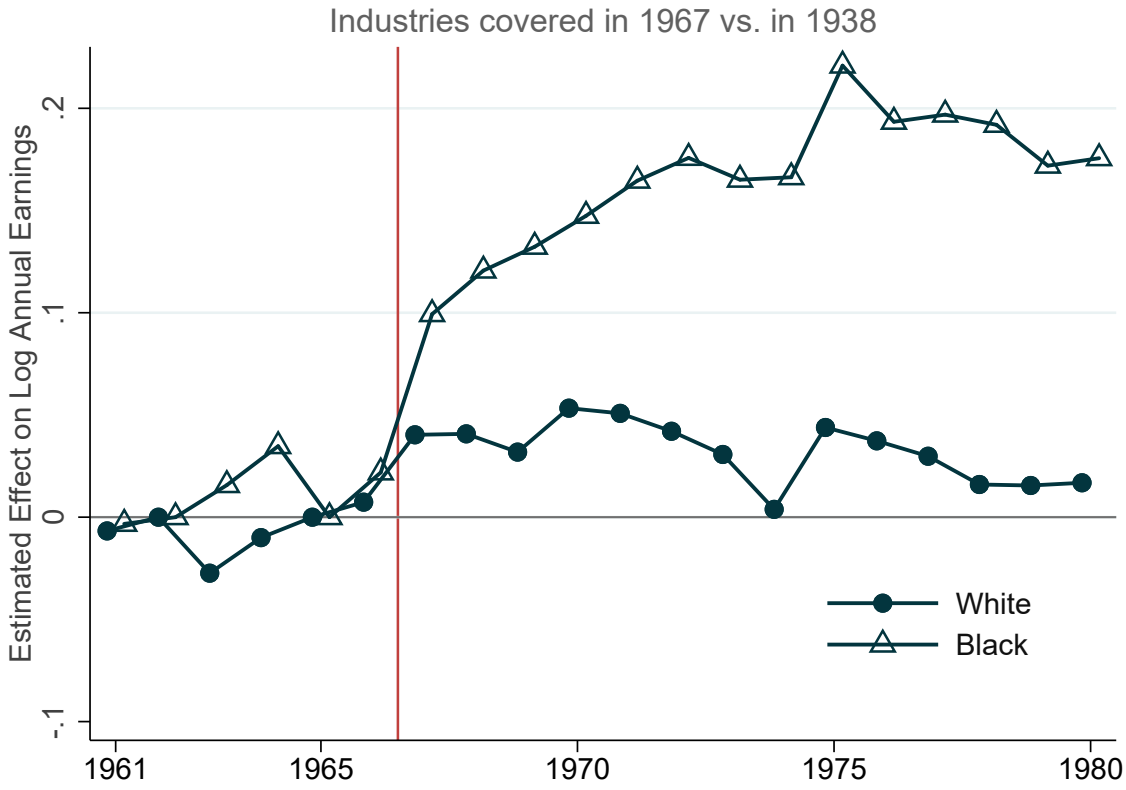
$$
G^{\text{total}} = s_w^c G_c + s_w^t G_t + G_b^{ct} (s_w^c - s_b^c) \tag{C.9}
$$

C4 Appendix Figures and Tables

Figure C4: *Impact of the 1966 FLSA on annual earnings by race*

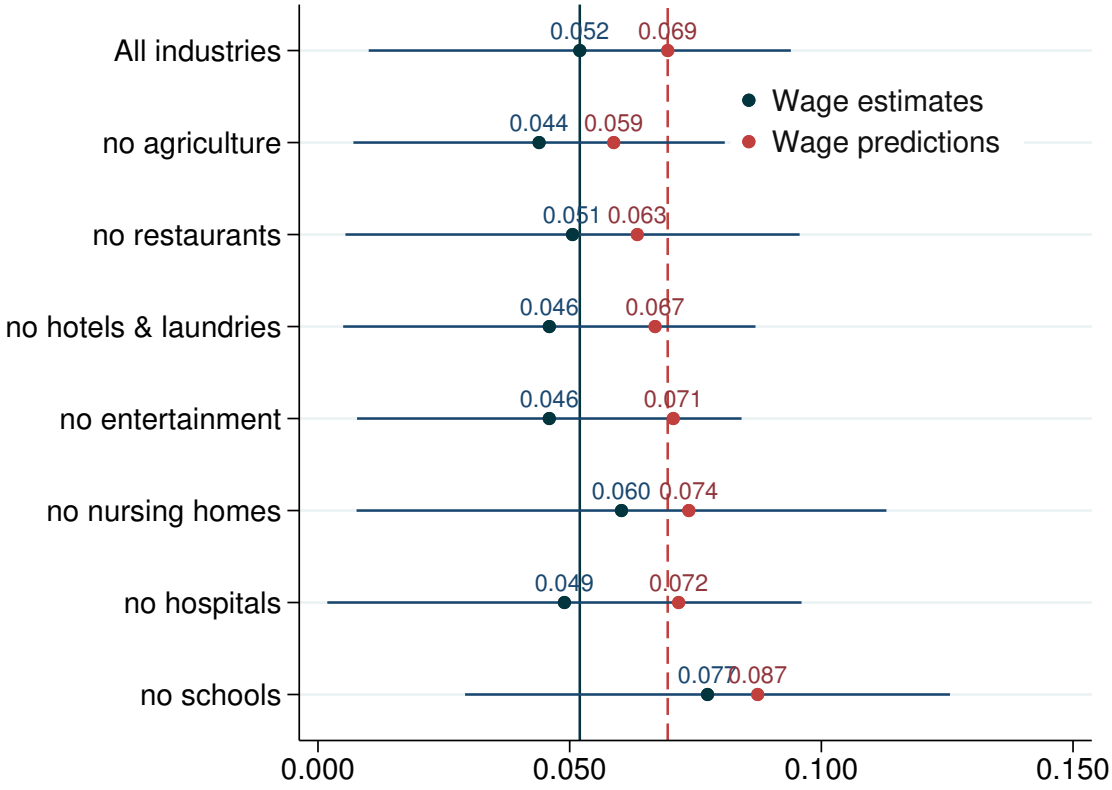
Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces.

Figure C5: *Impact of the 1966 FLSA on annual earnings by race*



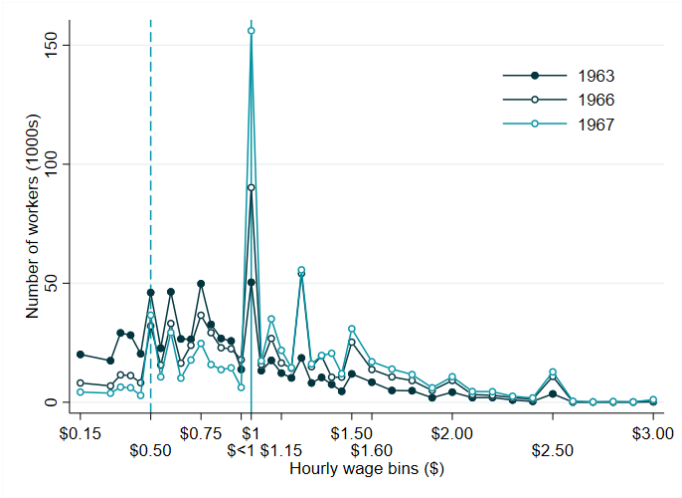
Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: This graphs differs from figure 3.8b: the control group for black workers is composed here by black and white workers in the industries covered in 1938, whereas in figure 3.8b, the control group for black workers is composed of black workers only in the industries covered in 1938.

Figure C6: *Wage estimates and wage predictions, by industry*

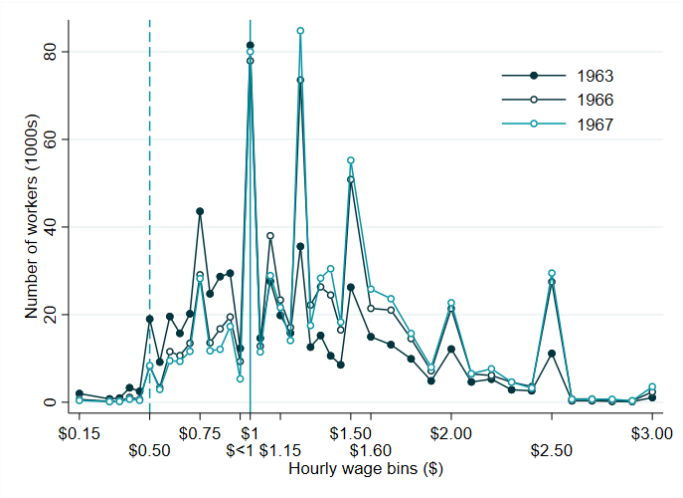


Source: CPS 1962-1980.
Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Wage estimates and wage predictions are for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

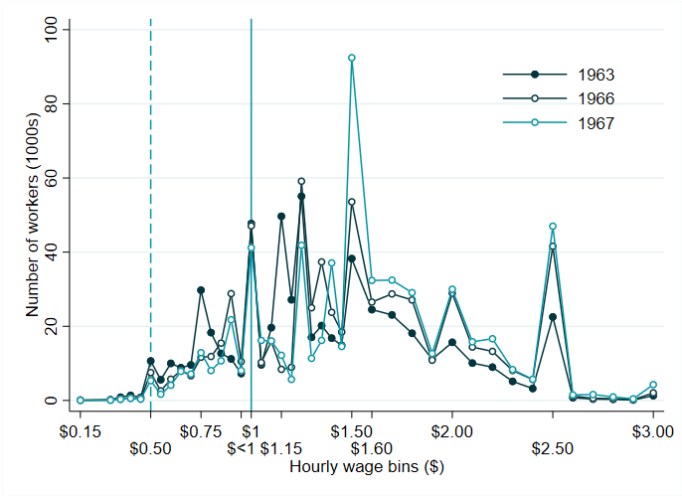
Figure C7: *Earnings distributions in hotels, restaurants and laundries, by region*



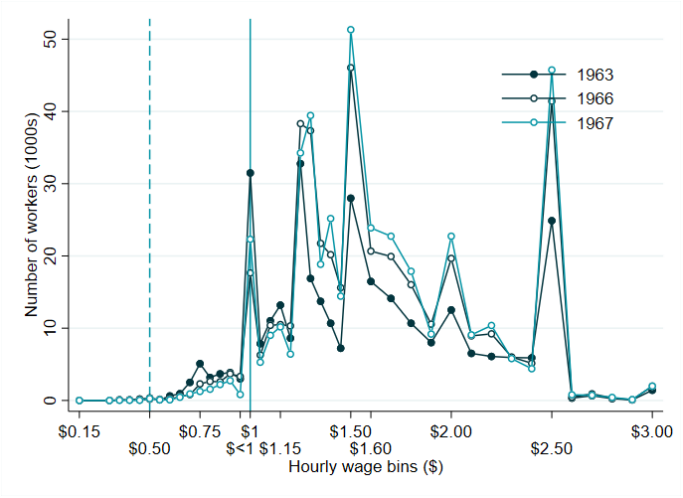
(a) *South*



(b) *Midwest*



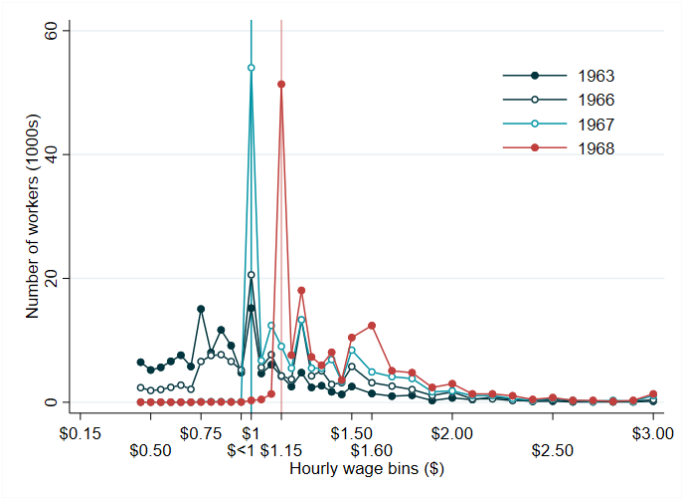
(c) *Northeast*



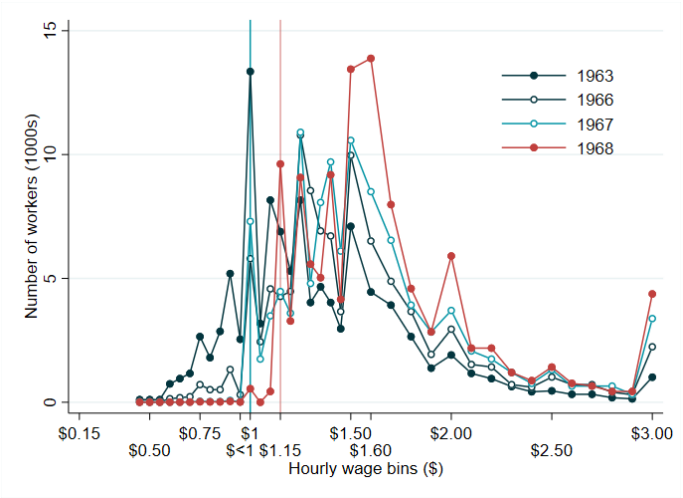
(d) *West*

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).

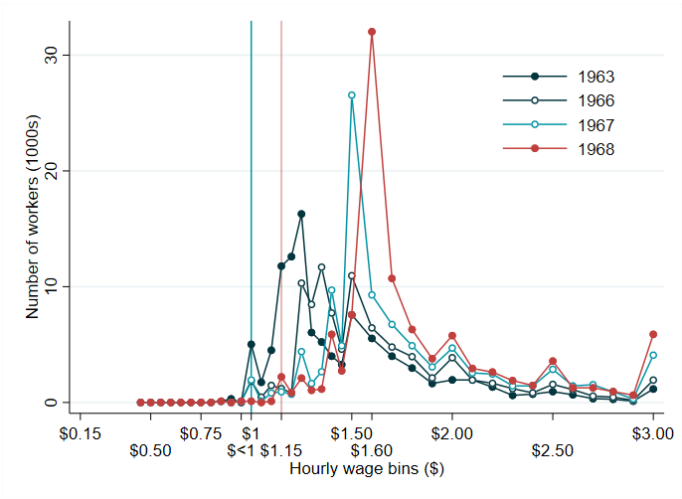
Figure C8: *Earnings distributions in laundries (inside plant workers), by region*



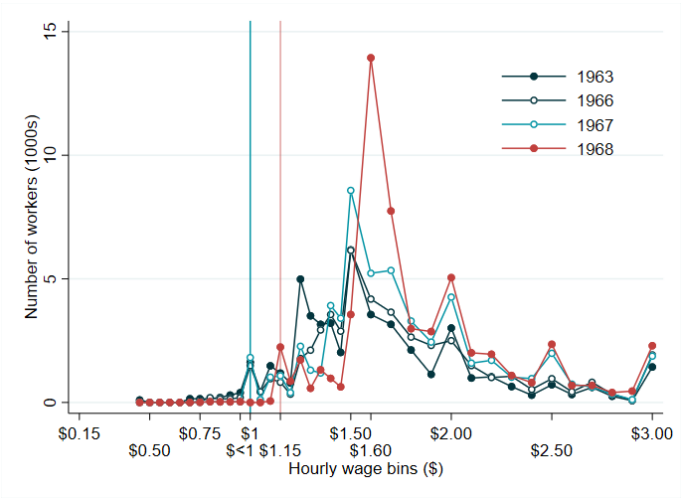
(a) *South*



(b) *Midwest*



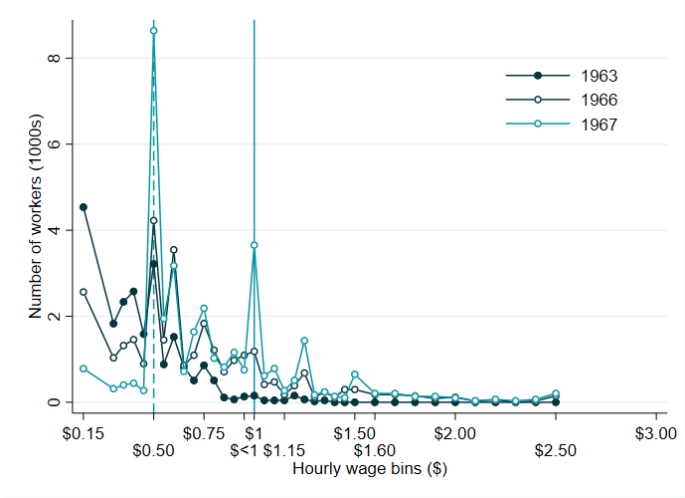
(c) *Northeast*



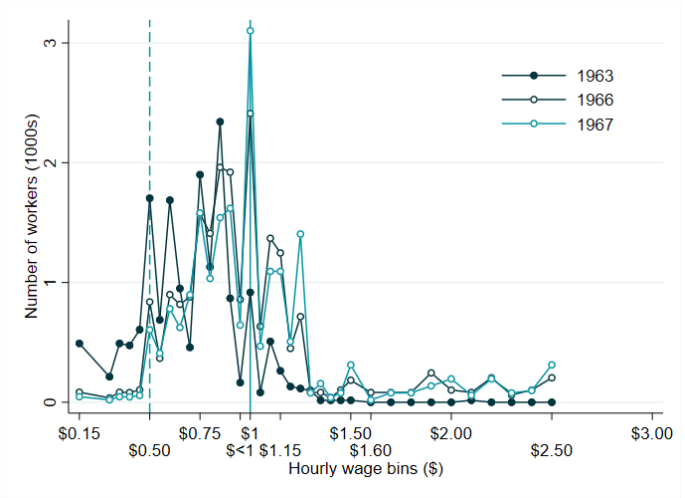
(d) *West*

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.

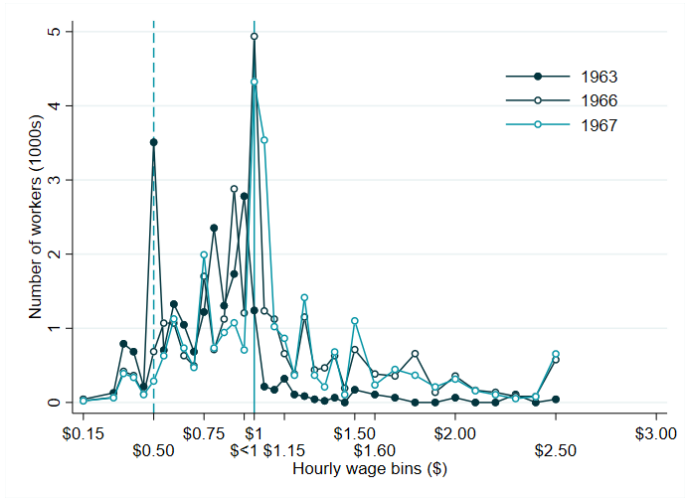
Figure C9: *Earnings distributions in hotels (tipped workers), by region*



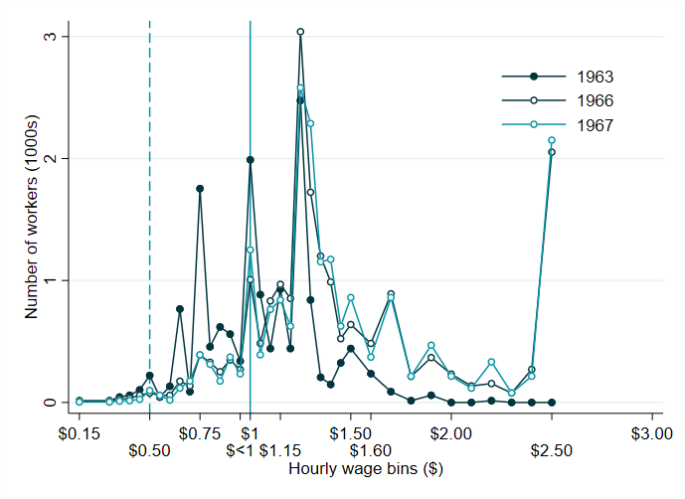
(a) *South*



(b) *Midwest*



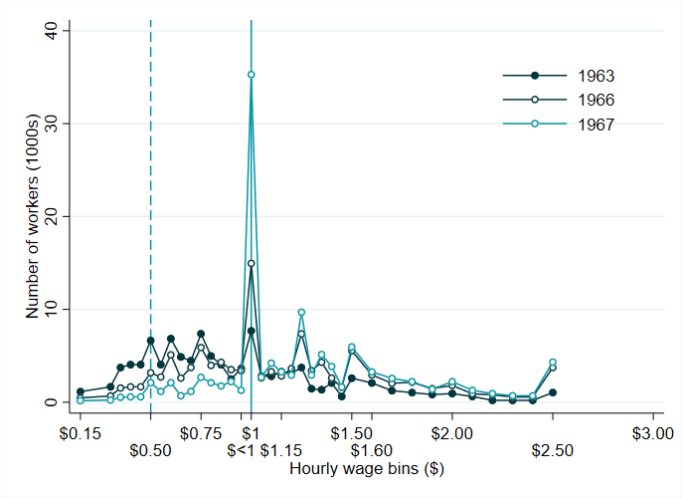
(c) *Northeast*



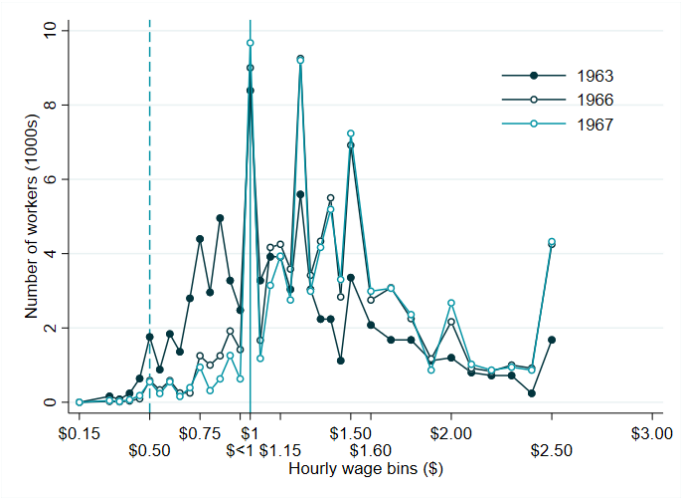
(d) *West*

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.

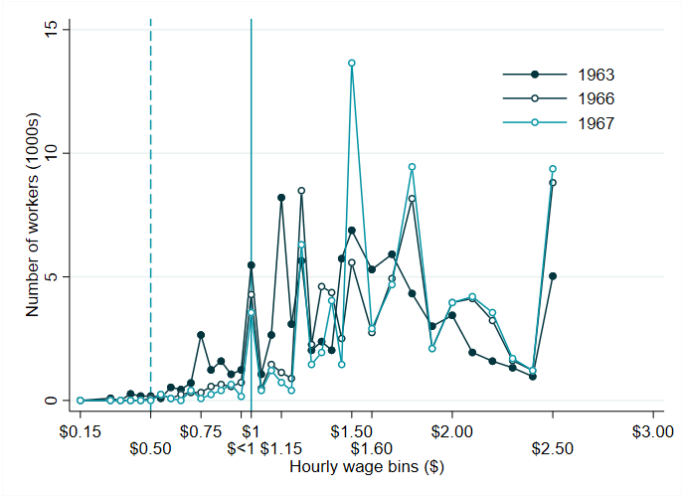
Figure C10: *Earnings distributions in hotels (non-tipped workers), by region*



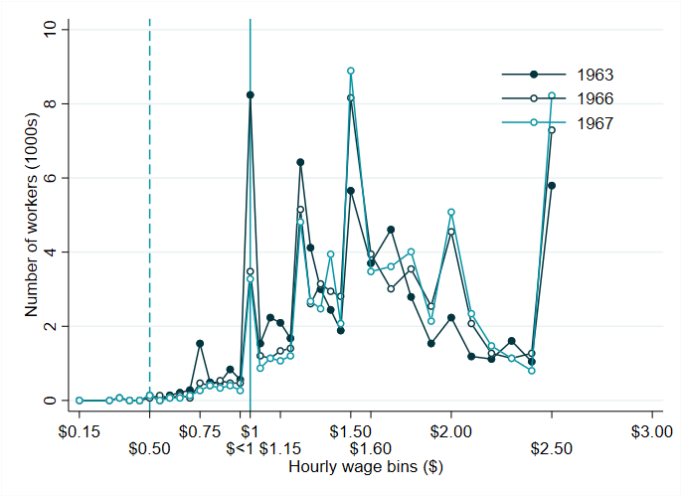
(a) *South*



(b) *Midwest*



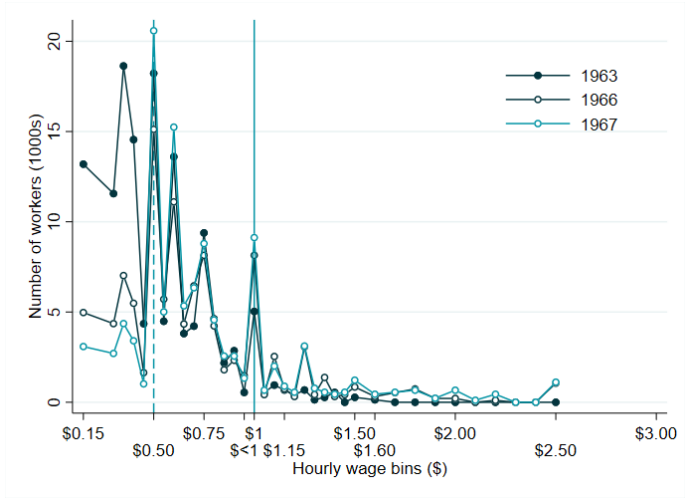
(c) *Northeast*



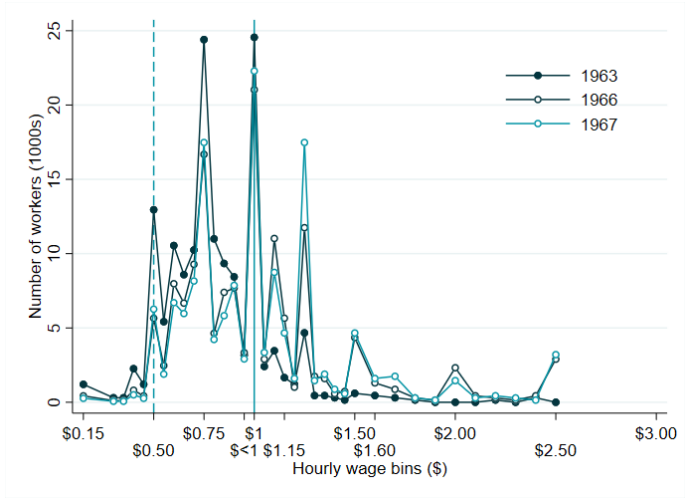
(d) *West*

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.

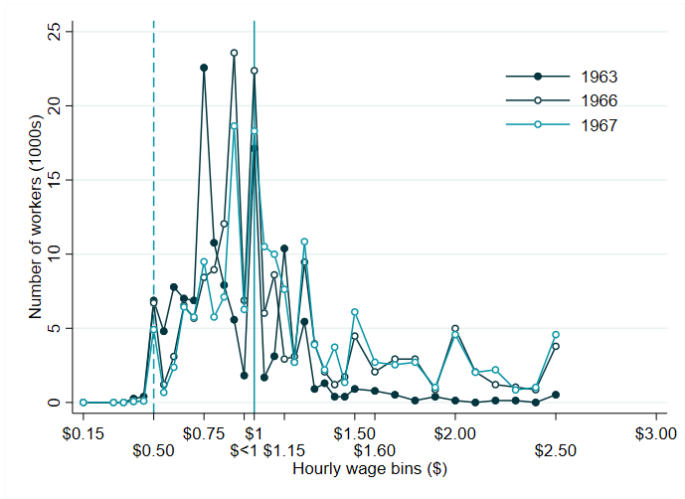
Figure C11: *Earnings distributions in restaurants (tipped workers), by region*



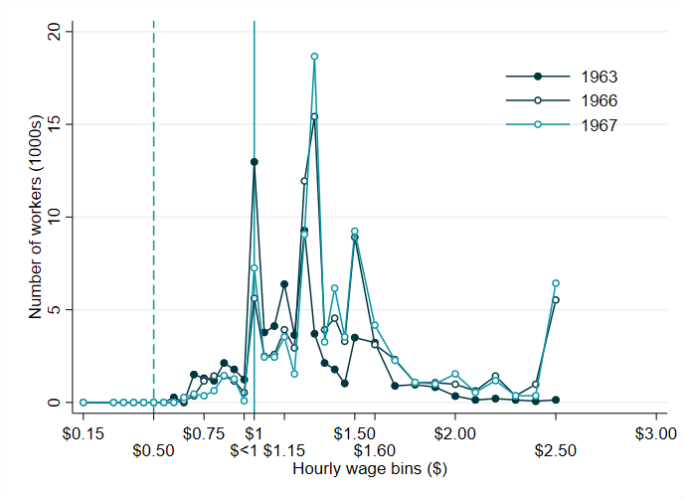
(a) *South*



(b) *Midwest*



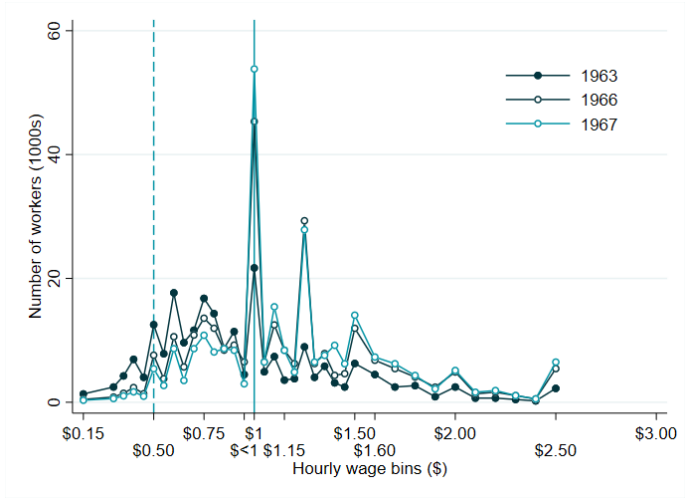
(c) *Northeast*



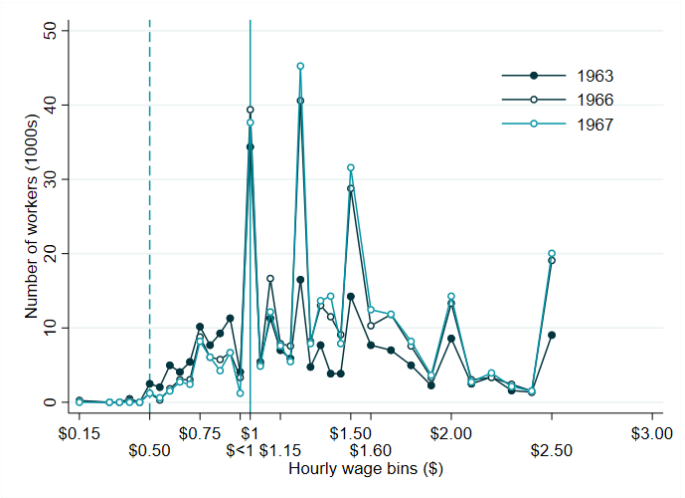
(d) *West*

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).

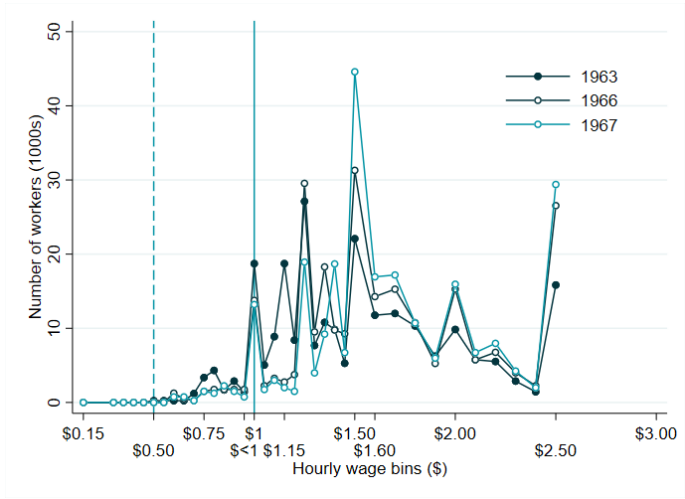
Figure C12: *Earnings distributions in restaurants (non-tipped workers), by region*



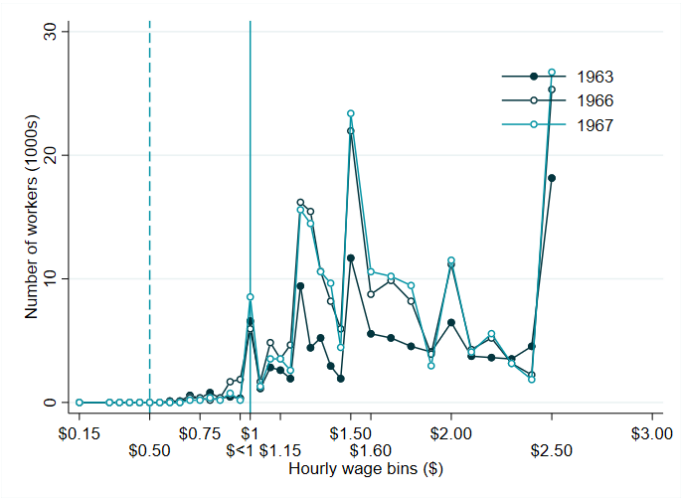
(a) *South*



(b) *Midwest*



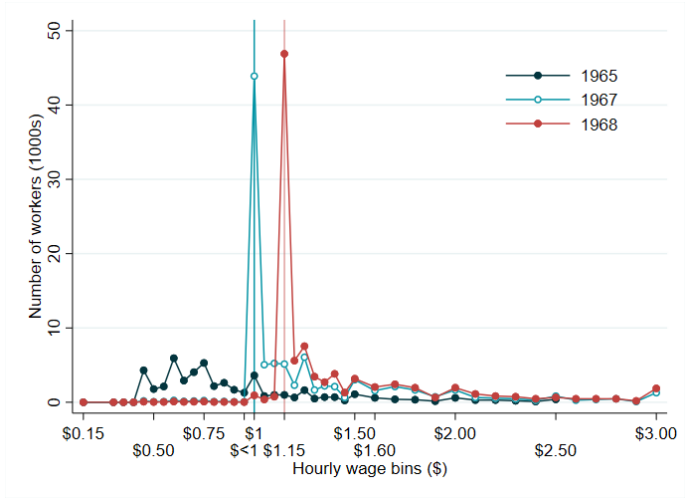
(c) *Northeast*



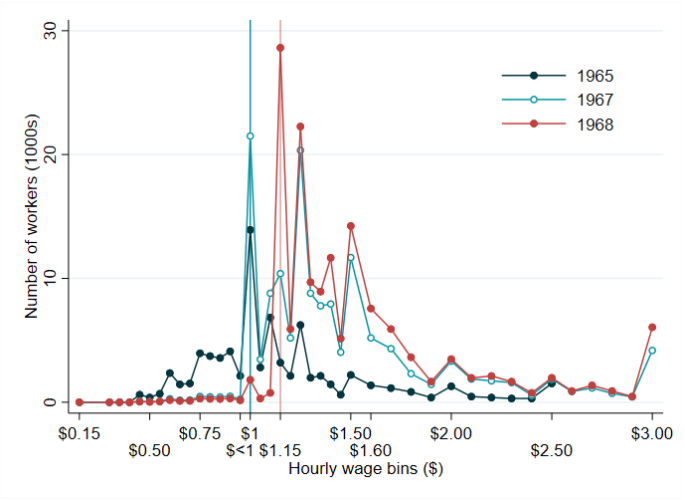
(d) *West*

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).

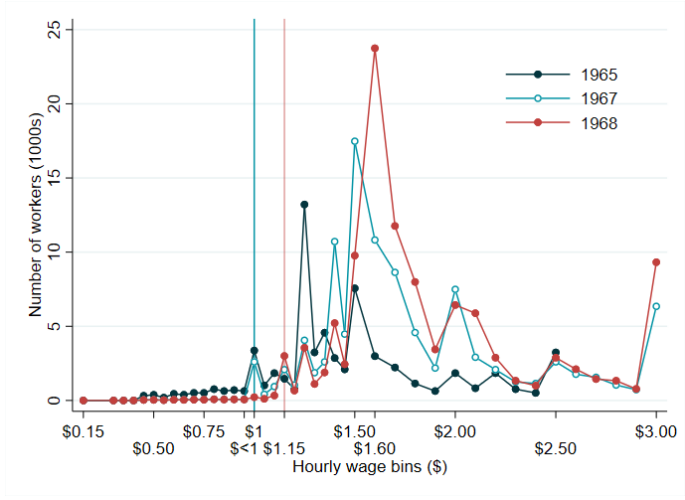
Figure C13: *Earnings distributions in nursing homes, by region*



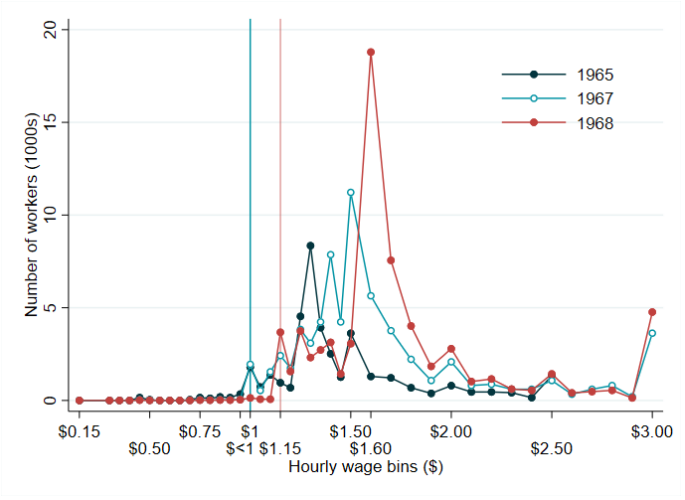
(a) *South*



(b) *Midwest*



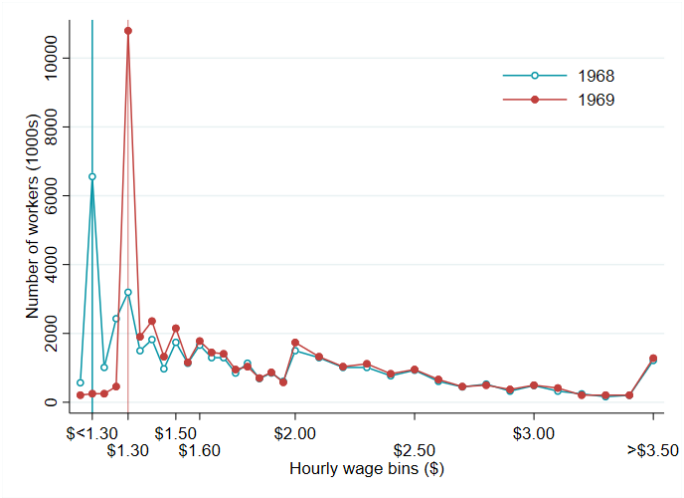
(c) *Northeast*



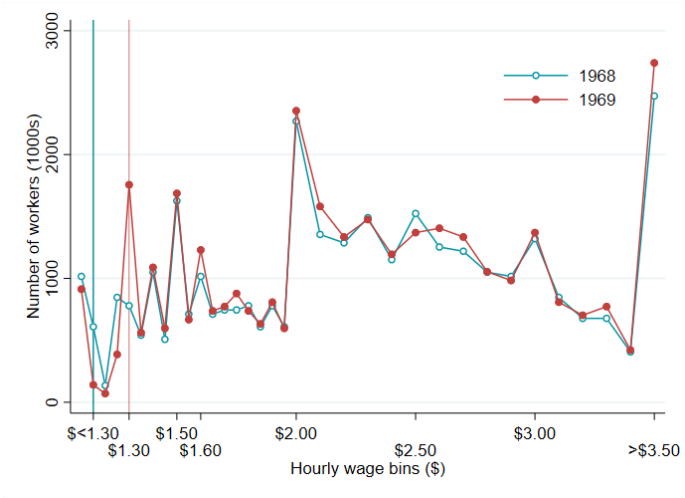
(d) *West*

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

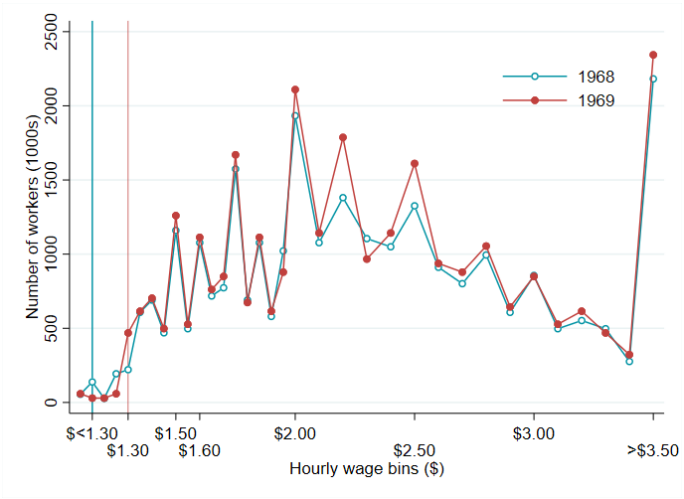
Figure C14: *Earnings distributions in schools, by region*



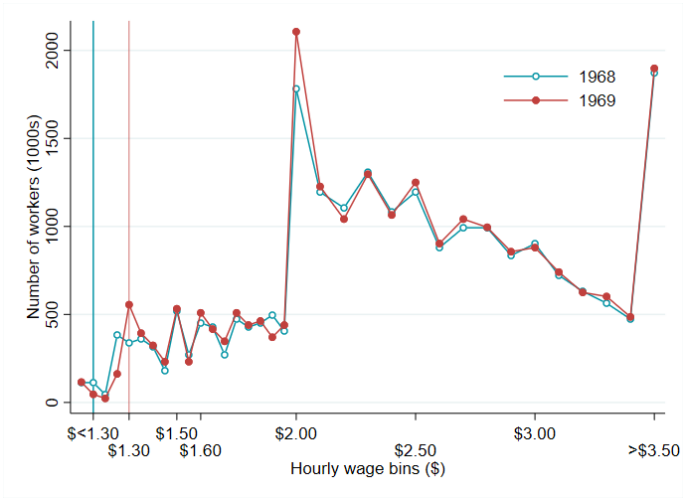
(a) *South*



(b) *Midwest*



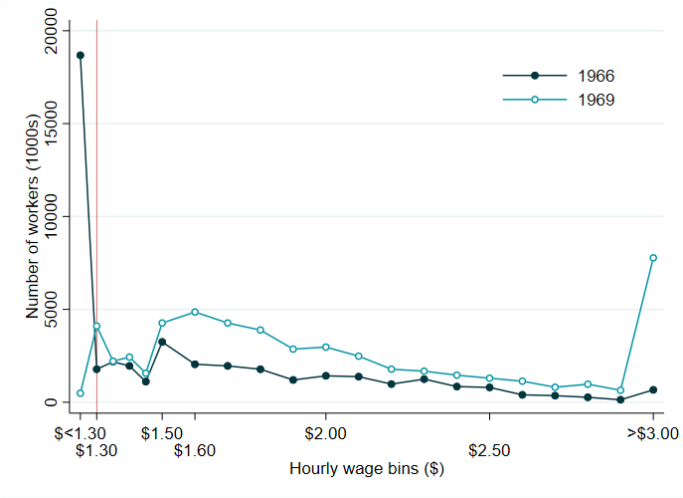
(c) *Northeast*



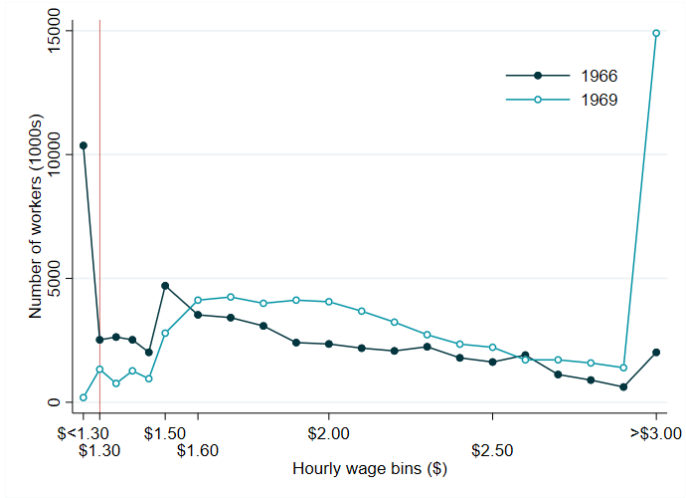
(d) *West*

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

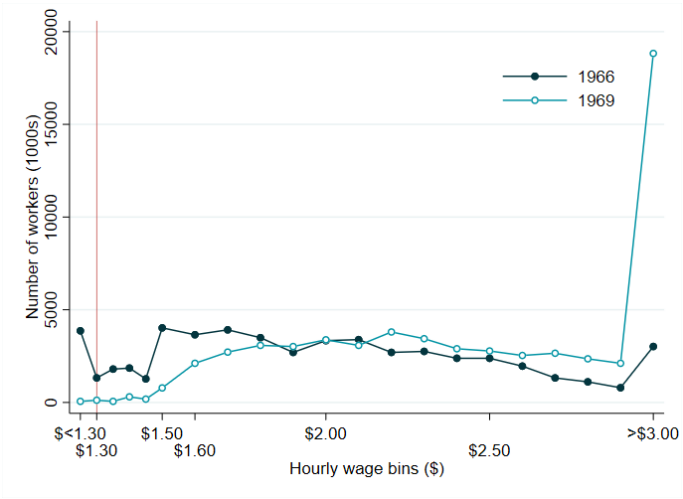
Figure C15: *Earnings distributions in hospitals, by region*



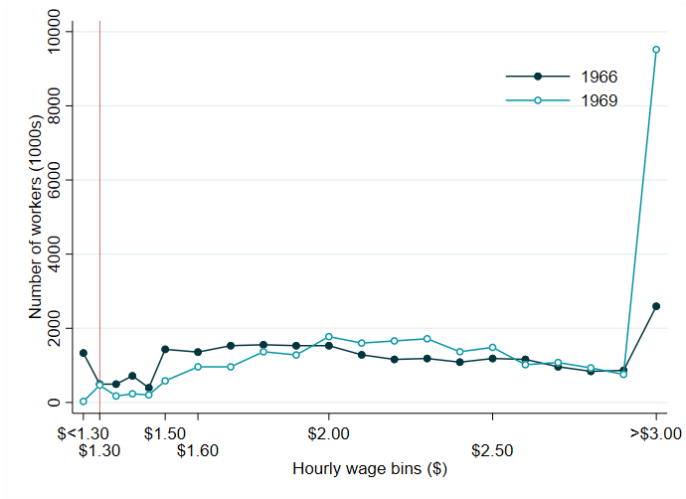
(a) *South*



(b) *Midwest*



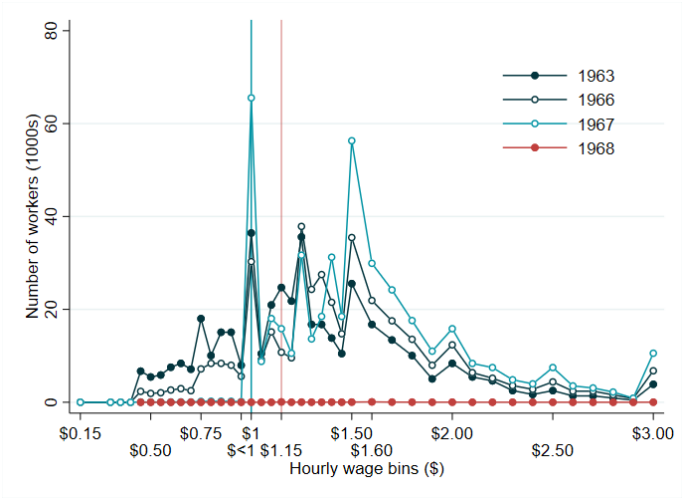
(c) *Northeast*



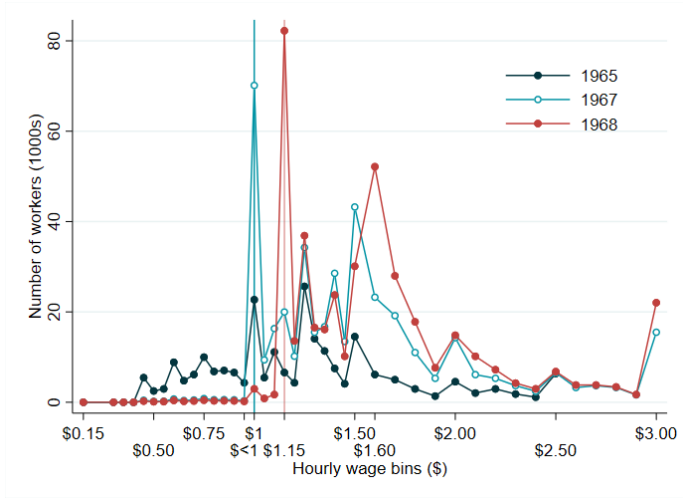
(d) *West*

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.).

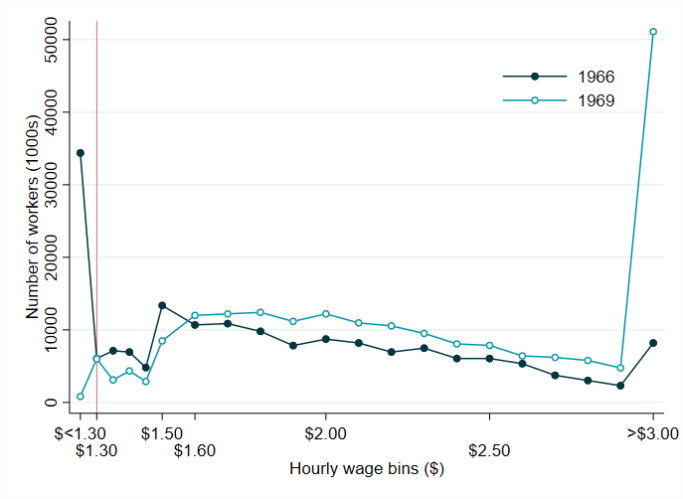
Figure C16: *Hourly earnings distributions*



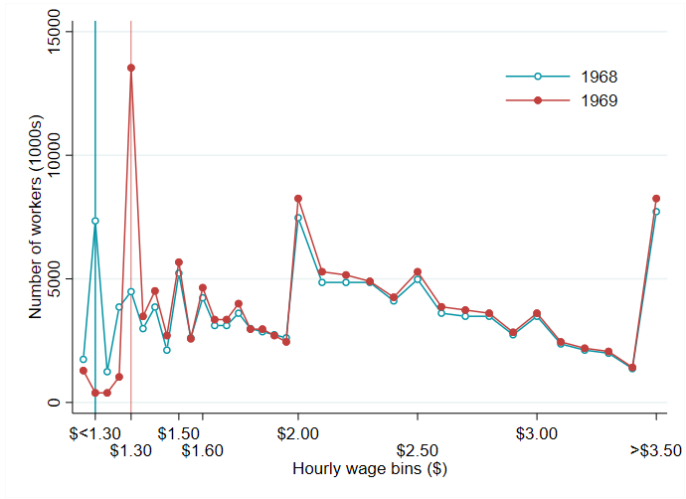
(a) *Laundries*



(b) *Nursing homes*



(c) *Hospitals*

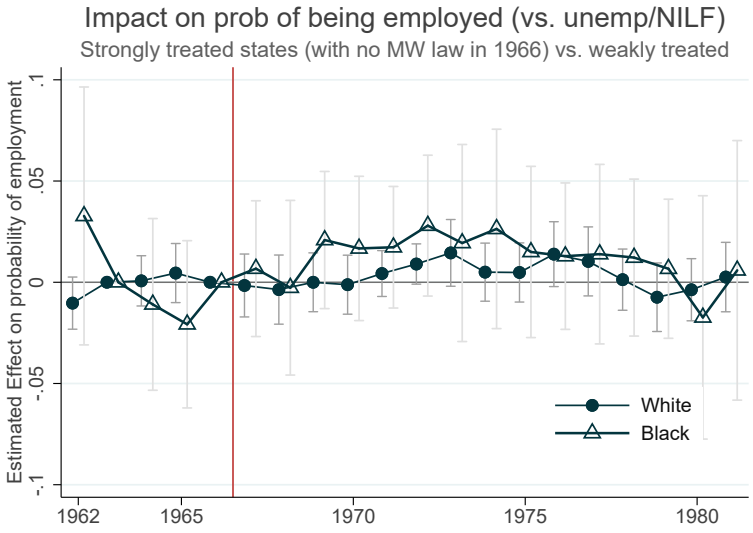


(d) *Schools*

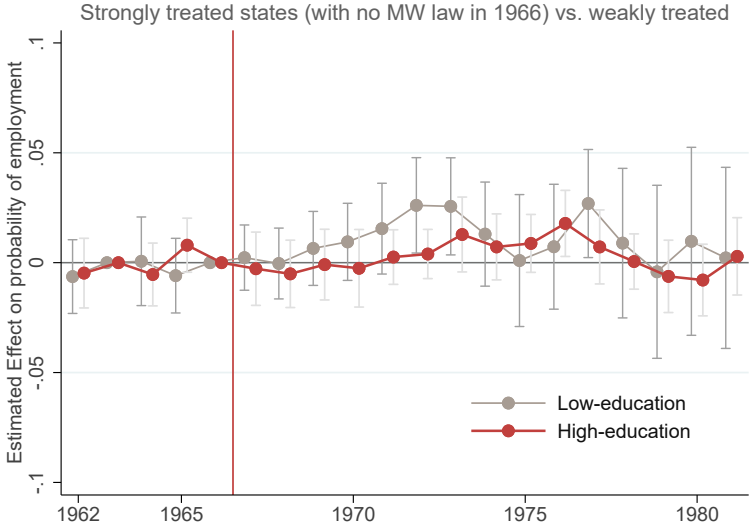
Source: BLS Industry Wage Reports. Sample: All nonsupervisory employees.

Figure C17: *Impact of the 1966 FLSA on probability of being employed (vs. not unemployed or not in the labor force)*

(a) *Black vs. white workers*



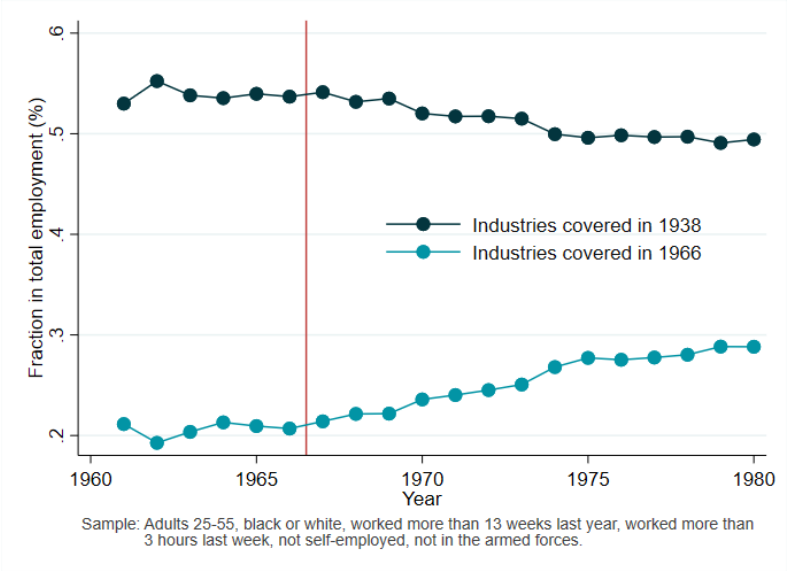
(b) *Low-education vs. high-education*



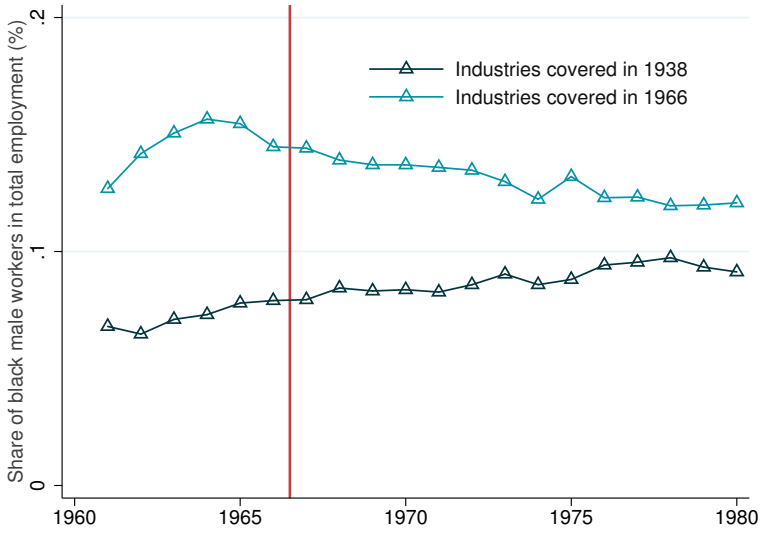
Source: CPS 1962-1980. Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.
Notes: Standard errors clustered at the industry and state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

Figure C18: *Evolution of Black and White employment in treated and control industries*

(a) *Employment shares in control vs. treated industries*

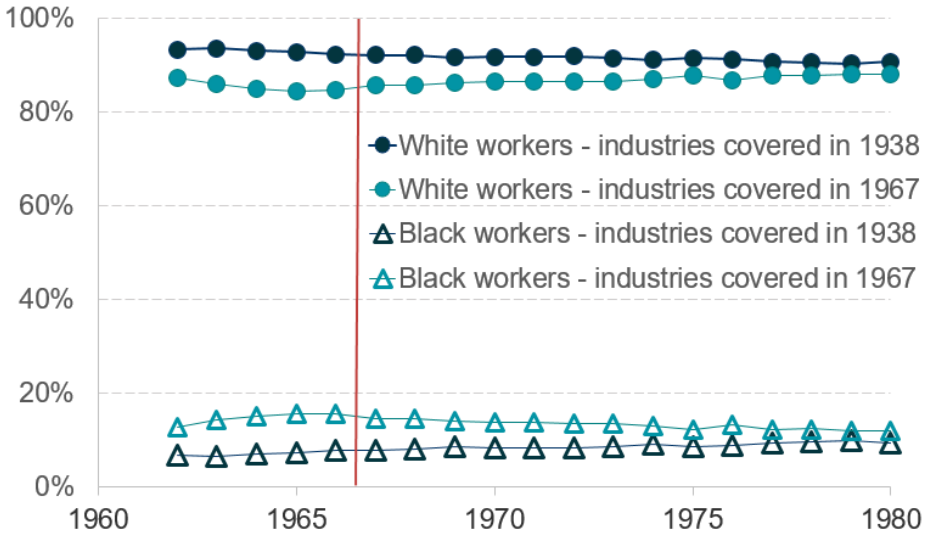


(b) *Black (vs. white) employment shares within 1938 and 1967 industries*



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.

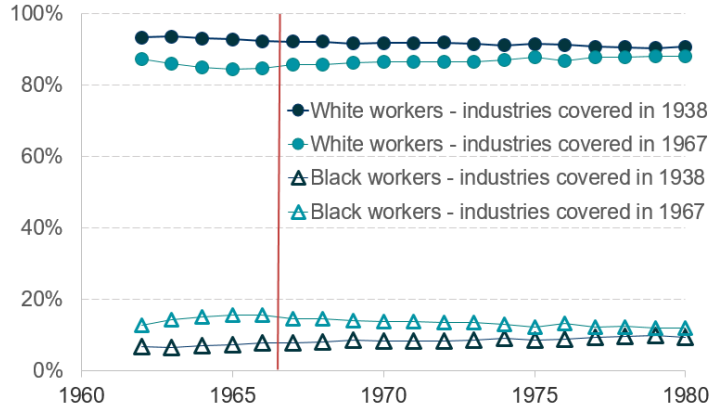
Figure C19: *Aggregate employment shares by industry type and by race*



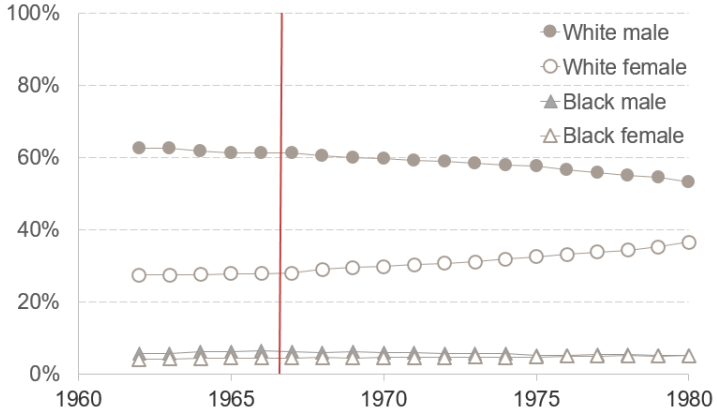
Source: CPS 1962-1980. Sample: Adults 25-55, black or white, worked more than 13 weeks last year, worked more than 3 hours last week, not self-employed, not in the armed forces. Excludes public sector, private households, retail trade and construction.

Figure C20: *Aggregate employment shares*

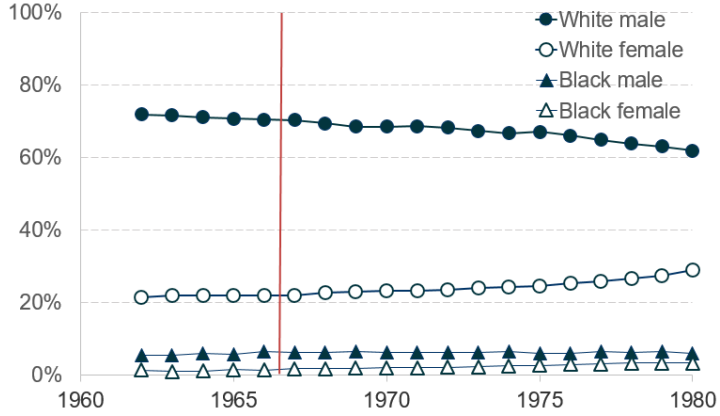
(a) *By industry type and by race*



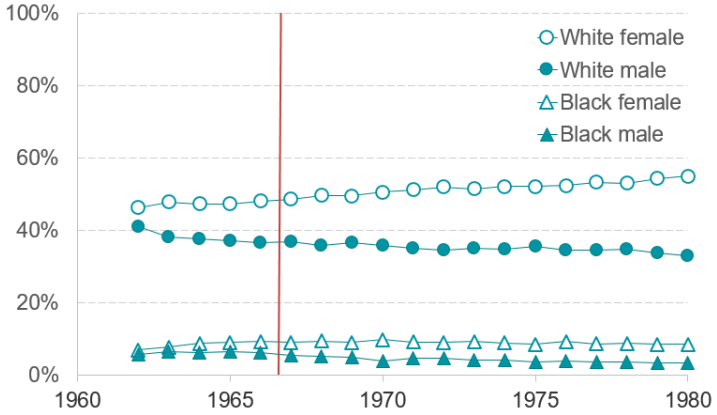
(b) *All industries, by race*



(c) *1938 industries, by race*

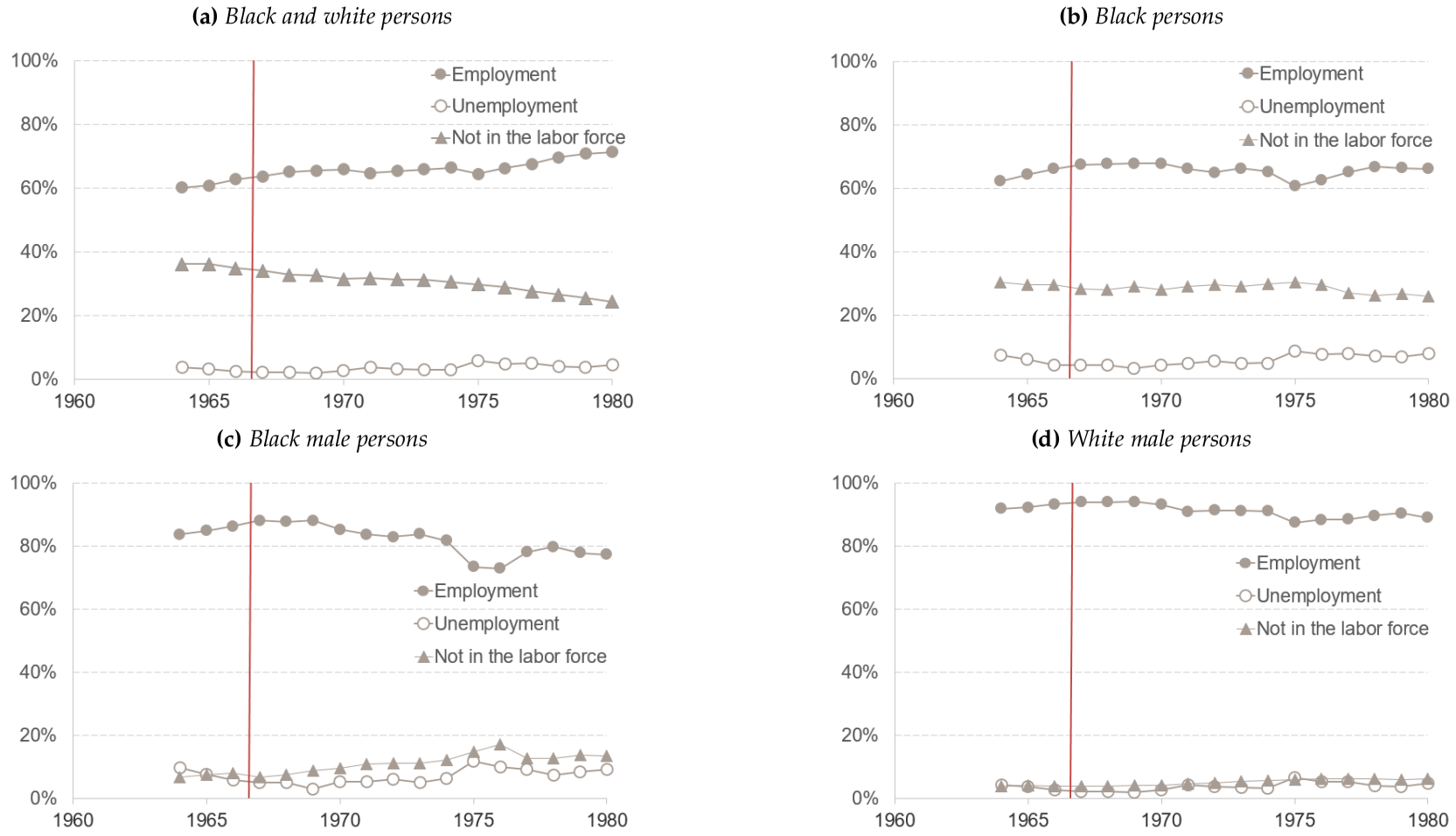


(d) *1967 industries, by race*



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 C21: *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.