Parental Minimum Wages, Children's Education, and Racial Inequality*

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Abstract

We test whether having a parent covered by minimum wages improves long-term education outcomes. We exploit variation in exposure to the 1966 Fair Labor Standards Act by child birth cohort and predetermined parental occupation. Parental minimum wage coverage during children's teenage years increases children's completed education. This effect is larger among black children, contributing to lowering educational inequality. This comes primarily from relaxed household budget constraints. First, education effects are larger for groups experiencing larger first-generation wage increases. Second, suggestive evidence indicates reduced teenage labor force participation and a reduction in dropout due to financial difficulties, especially for black children.

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1 Introduction

A key channel through which inequality in one generation leads to inequality in the next generation is through unequal investments in human capital (Bulman et al., 2021; Guryan et al., 2023). Policies successfully curbing inequality today may thus start a virtuous intergenerational cycle of equal opportunities. One of the most commonly used tools to reduce income inequality are mandated minimum wages. Yet we do not know whether the children of those who benefit from these mandates gain more education. In the US, extending the minimum wage to industries where black workers were over-represented accounts for over a fifth of the racial income inequality decline observed in the US during the civil rights era (Derenoncourt & Montialoux, 2021).[1] In this paper, we ask whether this large reduction in income inequality between workers (first generation) reduced human capital inequality among their children (second generation).

We first test whether the children of parents working in industries newly covered by minimum wage regulation complete more education in the long run. To do so, we exploit variation in second-generation exposure to minimum wage mandates by parental occupation and child cohort in a difference-in-differences design. The first source of variation comes from variation in implementation across industries, which we match to parental occupations using census data. In February 1967, the 1966 FLSA extended the 1938 FLSA minimum wage coverage to the agricultural sector, hotels, restaurants, schools, hospitals, nursing homes, entertainment and other services, as demanded by the Civil Rights movement (Smythe & Hsu, 2023). We therefore compare children whose parents, prior to the implementation of the 1966 FLSA, worked in industries newly covered in the 1966 reform to children whose parents worked in industries covered in the original 1938 act. The second source of variation comes from variation by age of the child relative to minimum school leaving age laws, which at the time ranged from 16 to 18—leading to sharp declines

[1]The newly covered industries accounted for around one-third of black workers, compared to only 18% of white workers (Derenoncourt & Montialoux, 2021).

in enrollment around this age.[2] In our sample of individuals aged 14 to 24 in 1966, those who were 14 to 18 years old in 1966 are thus classified as "post-treatment" cohorts while those who were 19 to 24 act as "pre-treatment" cohorts. We then study the heterogeneous effects of the minimum wage extension on children's completed education and shed light on its impact on second-generation inequality.

We implement our research design using data from the "Young Men" sample of the National Longitudinal Survey. The fortuitous timing of the survey and a question about parental occupation allows us to observe the parents' occupation when the child was age 14 irrespective of the child's age in 1966, and contains information on other pre-reform controls collected just before the 1966 FLSA was implemented. Since the youngest respondent in this survey is 14 in 1966, we hypothesize that the minimum wage mandate relaxes the household budget constraint at the bottom of the income distribution and thus enables children to stay in school for longer.

We find that the 1966 minimum wage reform increased completed years of education by 0.79 years, on average. This effect combines impacts at both ends of the completed years of education distribution, as we find a 10 percentage points decrease in dropout prior to completing 12th grade as well as a 8 percentage points increase in college attainment. Our heterogeneity analysis shows that the magnitude of the overall effect on completed education is much larger among black children than among white children. In terms of years of education, our estimates indicate that the effect among black children is an additional 0.92 years, while it is only 0.46 years among white children. Combined with the over-representation of black workers in newly covered industries, this leads to a 20.5 percent second-generation decrease in racial inequality in educational attainment. This improvement is driven by the larger reduction in high-school dropout among black children—nearly twice as large as that among white children.

Our results are robust to a number of sensitivity tests. We first test for parallel pre-

[2]See Figure A1, which shows the school enrollment rate by age in the publicly available 1960 Population Census extract (Ruggles et al., 2024).

treatment trends through a placebo test splitting pre-treatment cohorts into those aged 19-21 and 22-24 in 1966, and find no difference in changes in outcomes between these two cohorts by treated- versus control parental industries. An event study showing cohort-by-birth changes also supports this conclusion. Second, we test the robustness of our findings to alternative definitions of exposure to the 1966 FLSA. Because we observe parental occupation but not parental industry, we assign an occupation's treatment status based on the treatment status of the most common industries for this occupation. We do so based on 1960 Census microdata in which we observe both occupation and industry, and where we show that occupations can be successfully matched to industry treatment status in 79% of cases. Our results are robust to alternative specifications such as using a continuous treatment variable (namely, the share of employees in treated industries by occupation) and excluding from the sample those occupations where the share of workers in treated industries is between 20 and 80%, where the match error rate is likely higher. Third, we find that the effects we observe come from children whose parents had lower education levels and were therefore more likely to benefit from the minimum wage policy, which suggests that the effects we estimate are not picking up differential trends across industries unrelated to the minimum wage extension.

We test our hypothesized mechanism—a relaxation of the household budget constraint reducing the need for older children to earn income—in several ways. First, we replicate Derenoncourt and Montialoux (2021)'s estimated positive effects of FLSA 1966 on contemporaneous, first-generation wages using annual March Current Population Survey (CPS) data (1962-1981) for a similar sample and specification to those used in our second-generation education analysis. Second, we show that this contemporaneous effect on first-generation wages is heterogeneous across various sub-samples in a way that aligns closely with the heterogeneous effects on second-generation educational outcomes. Third, still using CPS data, we study the effect of household heads' exposure to FLSA 1966 on the contemporaneous labor force participation and school enrollment of their teenage sons, and find suggestive evidence of decreased labor force participation and increased enrollment in the black sample. Finally, we study the self-reported reason for dropping out before completing a high-school or college degree in our NLS sample and find that treated black drop-outs are less likely to report having dropped out for financial reasons while we find a smaller, statistically insignificant effect for white drop-outs.

In addition to finding evidence in support of a relaxation of budget constraints as the likely leading mechanism driving our results, we also find that the second-generation's education goals and occupational aspirations are immediately impacted by the 1966 FLSA, especially in the case of black children. While it is possible that individuals rapidly and rationally revised their expectations of future parental earnings, it is also possible that part of the effect we observe comes from a direct aspiration channel of the 1966 FLSA, whereby an improvement in parental working conditions raised aspirations among their children in addition to any effect of increased parental earnings on human capital goals.

This paper makes two main contributions. First, we provide the first estimates of a parent's minimum wage coverage on the education of their children. Previous literature has investigated the effects of minimum wage mandates on the labor market outcomes of working-age adults in the US and elsewhere (see, e.g., Card et al., 1993; Dube et al., 2010; Cengiz et al., 2019; Dube, 2019; Bailey et al., 2021; Derenoncourt & Montialoux, 2021; Dustmann et al., 2022; Engbom & Moser, 2022). But we do not know if these effects on working-age adults affect investments in the education of their children. A few recently published articles have estimated the contemporaneous (i.e., same-year) effect of changes in the mandated value of state minimum wages on aggregate education outcomes at the state level (Regmi, 2020; Smith, 2021; Schanzenbach et al., 2024). And in a study developed independently to ours, Araujo et al. (2023) estimate the effect on completed education of growing up in states where FLSA 1966 had more "bite" by exploiting between-state variation in the share of workers earning below $1.60 before the reform. We estimate the effect on an individual's educational outcomes of the minimum wage coverage of their own parents. Doing so has two key advantages relative to exploiting variation in exposure across states. First, we are able to isolate the positive effect of increased parental income on educational attainment—which only applies to the children of those exposed—net of the negative incentive effect which minimum wages could have on education—which applies to all young people.[3] The combination of these two opposing forces may contribute to explaining why prior studies have reached mixed conclusions, namely positive effects of higher minimum wage floors in Smith (2021) and Araujo et al. (2023) but a negative effect in Regmi (2020), and mixed effects in Schanzenbach et al. (2024). Second, studying the effect of own parent's exposure to a minimum wage mandate sharpens our ability to investigate heterogeneous impacts by individual characteristics, such as race.

Our second contribution is to study the effect of an exogenous improvement in black parents' relative incomes on racial inequality in the human capital of their children. This complements prior work on racial inequality in socioeconomic outcomes. Large persistent racial inequality in education and incomes has become one of the most significant dimensions of economic inequality in the United States and has been the focus of much prior literature (see Derenoncourt & Montialoux, 2021; Levine & Ritter, 2022, and literature reviewed therein). Although inequalities persist (Chetty et al., 2018, 2020; Collins & Wanamaker, 2022), since the 1960s there have been sizeable reductions in black-white income inequality and pre-college educational inequality, which have been explained by, among others, federal anti-discrimination activities (Donohue & Heckman, 1989; Heckman & Payner, 1989), the Voting Right Act of 1965 (Aneja & Avenancio-Leon, 2019), improved school quality (Card & Krueger, 1992), income transfers (Butler & Heckman, 1977), and minimum wage mandates (Derenoncourt & Montialoux, 2021). Here we show that, in addition to its direct, intended equity-enhancing effect on wages, the 1966 FLSA addi-

[3] In the National Longitudinal Survey Young Men sample on which we carry out our analysis, the Pearson coefficient of correlation between a parent working in an industry covered by the 1966 FLSA when the child was age 14 and the child himself working in an industry covered by the 1966 FLSA (when aged 29-39) is only 0.15. The negative incentive effect should therefore apply largely equally to the children of workers covered by the 1966 FLSA and to the children of other workers.

tionally contributed to reducing second-generation racial human capital inequality.

This article also pertains more broadly to the literature relating children's educational outcomes to parental income, which has focused on alternative sources of changes in income such as lottery wins (Bleakley & Ferrie, 2016; Bulman et al., 2021), the Norwegian oil boom (Løken et al., 2012), housing prices (Lovenheim, 2011), tax credits (Dahl & Lochner, 2012; Bastian & Michelmore, 2018), and government transfers (Akee et al., 2010; Milligan & Stabile, 2011; Bastian & Michelmore, 2018; Braga et al., 2020), and which has tended to focus on exposure of children at younger ages.

The rest of the paper is organized as follows. Section 2 presents the data used in this article, while Section 3 discusses the empirical strategy. Section 4 presents our results on second-generation educational outcomes and their implications for second-generation racial inequality. Section 5 discusses mechanisms through which mandated minimum wages may have contributed to improved second-generation education outcomes. Section 6 concludes.

2 Data

Our main source of data is the National Longitudinal Survey of Young Men (NLSYM), which we use to analyze the effects of the 1966 reform on the second generation's completed education. We focus on the 'Original Cohorts - Young Men' sample (Bureau of Labor Statistics, U.S. Department of Labor, 1992) because it meets the following four requirements. First, it contains pre-determined parental work information, which allows us to identify whether individuals' parents worked in treated or control industries before the 1966 FLSA came into force in 1967. Second, we observe parental occupation when all individuals were aged 14, irrespective of the age of respondents at the time the 1966 FLSA was implemented and hence irrespective of their treatment status. Third, we observe the characteristics of the home environment in 1966 and hence prior to the implementation of the 1966 FLSA. Fourth, the panel nature of the survey means that we observe the completed education levels of the respondents. Only the NLS Young Men sample meets all these requirements. A "Young Women" sample was first surveyed in 1968, so that for this female sample, we do not observe family income and other control variables prior to the 1966 FLSA implementation, and for the youngest cohorts, parental occupation at age 14 is observed after implementation, leading to endogeneity concerns. We therefore focus our analysis on males, who are the gender group for whom racial gaps in education and income are the most persistent, as documented in Chetty et al. (2020), and report estimates for the female sample in the appendix (see section 4.4).

The NLSYM includes 5,225 males who were 14 to 24 years old in 1966. After the first interview in 1966, this longitudinal survey followed the original respondents until 1981.[4] Parental occupation and some baseline household characteristics when the child was 14 years old are recalled in 1966, while the outcome variables are measured in various waves. Namely, we use the most up-to-date information about completed education available, and ensure that the high-school dropout outcome is measured after the age of 19, that the indicator for having some college education is measured after the age of 22, and that the number of completed years of education is measured after the age of 22. Other socioeconomic characteristics such as parental education, household income, receipt of welfare or public assistance, and other location and demographic characteristics are measured in 1966, before the extension of the minimum wage. The details of the definition of each variable are provided in Table A1 and variable means are shown in Table A2.

Similar to Derenoncourt and Montialoux (2021), we compare individuals (whose parents worked) in industries covered in 1966 versus those covered in the original 1938 FLSA and drop observations pertaining to other industries, whose treatment status is fuzzier due to smaller reforms in the relevant time period. Also, since we are specifically examining racial inequality between black and white people, individuals of other races, who

[4]There are 12 waves in 1966, 1967, 1968, 1969, 1970, 1971, 1973, 1975, 1976, 1978, 1980 and 1981, respectively.

constitute 1% of the full sample, are excluded. This results in a main regression sample of 3,513 individuals or 858 black individuals and 2,655 white individuals, reflecting the fact that black young men were oversampled in the NLSYM to allow the construction of reliable statistics disaggregated by race.

We collect information on the industries covered in the 1938 Fair Labor Standards Act and 1966 amendment from documents from the U.S. Department of Labor Wage and Hour Division.[5] We then assign treatment status as follows. The NLSYM collects information on parental occupation when the child was age 14, but not parental industry.[6] We therefore need to match parental occupations to the treatment status of industries. We do so using IPUMS' 1960 Census 1 percent extract, in which we calculate, for each occupation, the share employed in industries covered by the minimum-wage law in 1967 (% 1967), covered in 1938 (% 1938) and covered in neither (% others). We then define treatment status for each occupation using the following formula:

$$\text{Covered } 1967_j = \begin{cases} 1, & \text{if } \%\ 1967_j > \text{Max}\{\%\ 1938_j, \%\ \text{others}_j\} \\ 0, & \text{if } \%\ 1938_j > \text{Max}\{\%\ 1967_j, \%\ \text{others}_j\} \\ \text{Missing}, & \text{if } \%\ \text{others}_j > \text{Max}\{\%\ 1967_j, \%\ 1938_j\} \end{cases} \tag{1}$$

As shown in Figure A2, occupations are concentrated at the two extremes of the distribution of the share of individuals working in treated industries. To validate our approach, we take the 1960 census data and assign each individual worker to the FLSA 1966 group or FLSA 1938 group using Formula 1. We then compare the predicted assignment to FLSA 1966 based on occupation against the FLSA 1966 coverage of the worker's actual industry. For 78.6% of occupations (87.4% of workers), the predicted FLSA 1966 coverage matches actual coverage. For most occupations, the share of workers in treated industries is either

[5] Source: https://www.dol.gov/sites/dolgov/files/WHD/legacy/files/FairLaborStandAct.pdf.

[6] The exact survey question is "What kind of work was your father (or the head of the household) doing when you were 14 years old?" The youngest cohort in the sample was 14 years old in 1966, ensuring that this occupation was determined prior to the 1966 FLSA implementation in February 1967.

more than 80% or less than 20%. Findings are robust to alternative approaches such as excluding occupations with a share of workers in treated industries above 20% but below 80%, where assignment is more uncertain, and using the share assigned to FLSA 1966 industries instead of a binary indicator, as discussed in Section 4.2. Further details regarding how we assign treatment status based on parental occupation are provided in appendix B.1.

When studying causal pathways, we also use CPS microdata to estimate the effect of the 1966 reform on first-generation wages—replicating estimates from Derenoncourt and Montialoux (2021)—and to estimate contemporaneous effects of the household head's exposure to the 1966 reform on the school enrollment and work of children aged 15-18 living in the same household.[7]

3 Empirical Strategy

We use a difference-in-differences research design exploiting differences in treatment exposure across predetermined parental occupation and child birth cohort. More specifically, we compare changes in outcomes between birth cohorts, for children whose parents worked in occupations found predominantly in newly covered industries relative to children whose parents worked in occupations found predominantly in industries covered since 1938.

The younger ("post-treatment") cohorts are between 14 and 18 years old in 1966, while the older ("pre-treatment") cohorts are between 19 and 24 years old in 1966. The 18 year old cutoff comes from the legal minimum age of leaving school, which was between 16 and 18 years old depending on state. For our high school dropout outcome, individuals aged 19 and above in 1966 should be minimally affected. For our other outcomes (completed years of education and college education), individuals aged 19 and above might

[7]We use CPS waves 1962 to 1981 (Flood et al., 2023), which provide wage information from 1961 to 1980, with the exception of wave 1963 which we do not use because it lacks demographic information.

still benefit (and hence bias our estimates towards zero), but these benefits are likely to be small.[8] To confirm this, in a robustness check we use as pre-treatment cohorts only those aged 21-24 in 1966 and find similar results (Table A3).

Our key identification assumption is that our outcomes of interest would have evolved similarly across birth cohorts, between children whose parents were employed in treated and control occupations, absent the 1966 reform. We carry out a placebo test comparing time trends between pre-treatment cohorts and, reassuringly, find no evidence of differential changes between the 19 to 21 cohorts and 22 to 24 cohorts between parents working in treated occupations versus control occupations (see Table A4). In Figure A3, we also depict cohort-by-cohort differences in years of education in the treated versus control parental industry group relative to the last cohort prior to treatment (Age 19). Although cohort-by-cohort samples are small and estimates imprecise, we find no indication of diverging trends prior to treatment.

We estimate a difference-in-differences model with linear two-way fixed effects—which, given that our treatment is binary, that there is no variation in the timing of the treatment, and no treatment reversal, is a special case in which a linear two-way fixed effects model gives an unbiased estimate of the average treatment effect on the treated (ATT) as long as the parallel trends assumption holds (De Chaisemartin & d'Haultfoeuille, 2023, p. C4). More specifically, we estimate models of the form:

$$y_{ijc} = \alpha + \beta_1 \text{Covered } 1967_j \times \text{post-treatment cohorts}_c + \sum_{j=2}^{14} \delta_j + \sum_{c=2}^{11} \delta_c + \mathbb{X}_i' \Gamma + \varepsilon_{ijc} \tag{2}$$

where y_{ijc} denotes the education outcome of individual i in the last wave in which it is observed.[9] The dummy variable Covered 1967_j equals 1 if, when the individual was 14

[8] In Figure A1, we show the steep decrease in school enrollment prior to age 18.

[9] We test for differential attrition as follows. We estimate Equation 2 using as the dependent variable a variable equal to 1 if an individual surveyed in 1966 and meeting our sample restriction criteria (for race and parental occupation) is included in our main regression samples (i.e., those used in Table 1), and 0 otherwise. When doing so, the coefficient associated with the interaction term (Covered 1967_j $\times$ post-treatment cohorts$_c$) is statistically insignificant (p-values: 0.749 and 0.805, Table A5). Table A6 further

years old, the individual's father or, if not living with the child at that time, the household head, had an occupation matched to an industry covered in 1967 and 0 if they had an occupation matched to an industry covered in 1938. post-treatment cohorts$_c$ equals 1 if the individuals' age c is equal or less than 18 in 1966, and 0 otherwise. δ_j are occupation fixed effects. Parental exposure to the 1966 FLSA reform is assigned on the basis of the parent's 3-digit occupation code. However, some occupations have a very small number of observations, which leads to the loss of observations if using 3-digit occupation fixed effects when we explore treatment effect heterogeneity in sub-samples. To avoid losing observations, in our baseline specification, we aggregate occupations and include 2-digit-occupation-by-treatment-status fixed effects (i.e., two fixed effects by 2-digit occupation, one for all treated 3-digit occupations within the 2-digit occupation code and one for all control 3-digit occupations within the 2-digit occupation code).[10] Our results remain robust when using 3-digit occupation fixed effects, as discussed in Section 4.2, under "Robustness to alternative controls". δ_c are year of birth fixed effects. An important role played by year of birth fixed effects here is to control for "war on poverty" policies and civil rights era reforms common to all children irrespective of parental occupation, although careful consideration of 1960s social protection and educational programs and interventions suggests that the cohorts included in our analysis are largely too old to be affected (Table A7). We also control for the following baseline (i.e., pre-1967) individual-level characteristics in the vector $\mathbb{X}_i$, all relating to 1966 unless stated otherwise: ethnicity, number of siblings, parental education, family income, whether received welfare or public assistance, whether a library card was available in the household when the index individual was aged 14; rural location; labor market size of the region where the individual lives; and whether the individual lives in a southern state.[11] However, including control vari-

reports tests of differential attrition when splitting the sample by sub-samples of interest. Out of 12 tests, we only marginally reject equal attrition in one case (namely among the urban sample).

[10]We also include a category named 'all other treated' to collect together rarely occurring 2-digit-occupation-by-treatment-status, again to avoid losing observations in subsample analysis.

[11]See Table A1 for precise definitions of these variables. We control for each of these characteristics through categorical variables including one "missing value" category. For instance, we control for father's

ables makes little difference to the coefficients of interest, suggesting negligible selection into treatment on observable characteristics. We report standard errors clustered at the occupation level at which we assign treatment status to allow for arbitrary correlation of the error terms within occupation.

When studying causal pathways through which the 1966 FLSA impacted second-generation long-term educational outcomes, we use CPS data for the years 1962 to 1981, and run regressions of the form:

$$y_{ijt} = \alpha + \sum_{k=2}^{3} \beta_k \text{Covered 1967}_j \times \mathbb{1}[t = k] + \sum_{j=2}^{16} d_j + \sum_{t=2}^{3} d_t + \mathbb{Z}_i' G + \varepsilon_{ijt} \tag{3}$$

where y_{ijt} denotes the wage of parent i in year t or the contemporaneous school enrollment/work status of children aged 15-18 who live with them.[12] The dummy variable Covered 1967$_j$ equals 1 if the father or, if not living with the child, the household head, worked in an industry covered in 1967 as of the time of the survey and 0 if they worked in an industry covered in 1938. Following Derenoncourt and Montialoux (2021), we split the sample period into three sub-periods k: a pre-treatment period from 1961 to 1966, an immediate post-period from 1967 to 1972, and a later post-period from 1973 to 1980. d_j and d_t are industry and year fixed effects, respectively. $\mathbb{Z}_i$ is a set of parental characteristics, namely: binary indicators for race, education level, full-time/part-time work, weeks worked per year, hours worked per week, marital status and occupation, and the linear, quadratic and cubic terms of working experience, similar to the covariates used in Derenoncourt and Montialoux (2021), and further controls for being in a southern state and for rural status.

education by including 19 binary indicators for each possible number of completed years of education (from 0 to 18) plus one additional binary indicator equal to 1 when father's education is missing, and to 0 otherwise.

[12]Year t corresponds to the calendar year during which income was earned, that is one year prior to the survey wave (e.g., 1961 earnings for CPS 1962).

4 The Impact of Minimum Wage Mandates on Second-Generation Completed Education

In this section, we first present estimates of the overall average treatment effects on the treated of minimum wage coverage on second-generation long-term educational outcomes. We then report on a number of robustness checks before exploring heterogeneous treatment effects by race and their implications for racial inequality.

4.1 Average Treatment Effect of FLSA 1966 on the Children of the Treated

The two first columns of Table 1 report estimates for the effect of parental exposure to the 1967 minimum-wage extension on completed years of education. Our preferred specification, which controls for predetermined household- and labor market characteristics, shown in column (2), indicates that the average years of education for children whose parents worked in occupations covered in the 1966 reform is 0.79 years higher than those whose parents worked in control occupations for post-treatment cohorts compared to pre-treatment cohorts (or around 28% of a standard deviation). The estimate is only slightly smaller when controls are excluded (column (1)). As shown in the rest of the table, gains in education come both from a reduction in dropout (by 10 percentage points) and an increase in college education (by 8 percentage points).

4.2 Robustness Checks

Quality of the matching. Based on census data, in which we observe both 3-digit occupation (as in the NLSYM survey) and industry, the probability of correctly assigning an individual's industry treatment status based on their occupation is 87.4% when weighting occupations by the number of workers in each occupation (and 78.6% if giving equal weight to each occupation).

Table 1: Impact on Second Generation's Education Outcomes

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.738***	0.788***	0.068**	0.080***	-0.103***	-0.103***
	(0.208)	(0.216)	(0.027)	(0.029)	(0.032)	(0.032)
Mean	13.18	13.18	.49	.49	.19	.19
S.D.	2.82	2.82				
Obs	3,513	3,513	3,513	3,513	3,726	3,726
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Still, we carry out several robustness checks regarding matching. First, we exclude occupations with a share of workers in treated industries between 20% to 80%. As would be expected in this sample where treatment status is more sharply identified, Table A8 shows that the results after restricting the sample are similar to the main results in Table 1 with slightly larger coefficients. Also, we repeat the event study analysis with this restricted sample, which confirms that there is no indication of diverging trends prior to treatment (Figure A4). Alternatively, we use a continuous treatment variable, namely the share of employees working in the treated industries by occupation, rather than the binary treatment status. Results in Table A9 suggest the same conclusion as the baseline results, with more precisely estimated and slightly larger estimated effects (as would be expected since these correspond to the estimated effect of going from 0% to 100% of the parental occupation working in a FLSA 1966 industry).

Robustness to excluding 17 and 18 year olds. Excluding cohorts who were 17 or 18 years old in 1966 and may already have dropped out of school in some states yields similar results (Table A10).

Robustness to using sampling weights. Black individuals were purposefully over-sampled in the NLSYM survey, so that weighting observations using sampling weights in Table A11 puts more weight on white observations than in the unweighted estimates. As a result, we find qualitatively similar results across all outcomes; but for years of education and high-school drop out, estimates are smaller in magnitude since treatment effects are smaller for the white sample, as shown in the next subsection.

Effects by parents' education. The minimum wage reform should not affect children whose parents have high earnings. Introducing a wage floor may have sizeable indirect effects on lower earnings who are above the mandated minimum to ensure that differential skills and experience are rewarded, but these positive indirect effects should disappear at the top of the income distribution (see Gregory & Zierahn, 2022, and citations therein). To check that the data align with this prediction, we estimate Equation (2) for two sub-samples based on a proxy for parental earnings. The NLSYM does not collect individual parental wages, so instead we use completed years of education as a proxy. Following Derenoncourt and Montialoux (2021), we split the sample based on whether the household head dropped out before finishing high school (completed years of education less than or equal to 11 versus higher). Table 2 suggests that the effects are primarily driven by children whose household head has a lower level of education. The coefficients for the sample of children with a household head who has a higher level of education are insignificant and close to zero. These results are reassuring in that they indicate that our empirical strategy does not capture differential shocks that affected all workers in the occupations covered in this reform.

Robustness to alternative controls Table A12 shows that our results are robust to alternative fixed effects and controls for time-varying unobserved factors. Columns (1), (5), and (9) in Table A12 replicate our baseline results in Table 1 for each main outcome, respectively, to compare with other specifications.

In columns (2), (6), and (10), we first show that the results are largely unchanged if we use 3-digit occupation fixed effects instead of 2-digit occupation-by-treatment status fixed effects.

Another concern is that our estimates may be biased by the convergence in educational attainment between children from Southern states and those from other states, given that the newly covered industries are concentrated in Southern states. To address this concern, we add an interaction term between the indicator for living in a Southern state and a linear birth cohort trend to our baseline specification in columns (3), (7), and (11). Although the magnitude of the coefficients is slightly smaller than the baseline results, the effects remain large and statistically significant at least at the 5% significance level. We reach the same conclusion when we add Southern state-by-cohort fixed effects in columns (4), (8), and (12).

Table 2: Impact on Second Generation's Education Outcomes by Household Head's Education Level

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.776***	0.265	0.088***	0.040	-0.123***	-0.015
	(0.187)	(0.333)	(0.033)	(0.050)	(0.036)	(0.033)
Parental education	$\leq$ **11y**	$>$ **11y**	$\leq$ **11y**	$>$ **11y**	$\leq$ **11y**	$>$ **11y**
Mean	12.37	14.55	.35	.71	.27	.06
S.D.	2.69	2.39				
Obs	1,890	1,343	1,890	1,343	2,023	1,405
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes by parental (household head) education level. Based on Derenoncourt and Montialoux (2021), we split sample based on whether the household head has 11 years of schooling or less versus those with more than 11 years of schooling. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

4.3 Treatment Effect Heterogeneity and Implications for Racial Inequality in Education Outcomes

To explore treatment effect heterogeneity, we estimate Equation (2) using different subsamples. We focus on three potential sources of treatment effect heterogeneity: i) ethnicity; ii) living in the US South; and iii) rural location. We focus on these characteristics because minimum wage mandates are more likely to be binding for black workers and workers in southern states and in rural areas, whose average wages are lower. African Americans are also over-represented in the industries impacted by the 1966 FLSA and these industries are more prevalent in Southern states and in rural areas, but since we are estimating average treatment effects *on the treated* (ATT) and our definition of treatment is conditioned on parental occupation, a larger share of children defined as treated in a subsample should not mechanically translate into a higher ATT. Table 3 presents the results of sub-sample estimates: each column refers to a sub-group, and each panel refers to a dependent variable.[13] Note that Table A13 reports tests of differential trends for each subsample, which show no significantly different trends in cohorts aged 19 and older in 1966.

Columns (1) and (2) report estimates by race. Panel A shows that the ATT of the 1966 reform on years of education is significantly different from zero for both black and white individuals, but the magnitude of the estimate is much smaller (about half) for the white sample.[14] Similarly, the treatment effects on years of education are also much more pronounced in the South and somewhat more pronounced in rural areas.

In Panels B and C, we consider two extensive margins separately: high school dropout (panel C) and the acquisition of at least some college education (panel B). This reveals that the heterogeneous treatment effects observed for overall years of education are driven by reductions in high-school dropout. While there are heterogeneous effects running in the opposite direction for college education at the extensive margin, this is insufficient to counter the patterns observed at the lower end of the distribution of schooling.[15]

[13]Here we use a linear regression model and test for heterogeneous treatment effects along variables of particular interest. We come to qualitatively similar conclusions using a non-linear machine learning method ('causal forest') in which we let the data identify variables along which treatment effects are heterogeneous in Chen (2024).

[14]Table A14 shows that our results decrease only slightly in magnitude when excluding individuals who lived with their mother but not their father at age 14, although this decrease, combined with larger standard errors due to a smaller sample size, lead to insignificant coefficients when splitting the sample by race. These results suggest that the heterogeneous treatment effects we observe only partially reflect racial differences in the share of female-headed households (24.8% of black families vs. 14.8% of white families in the 1960 Census).

[15]In Table A15, we replace the dependent variables of Table 3 Panels B and C with the number of years of college education and the number of years which would have been needed to complete high school, respectively, therefore combining extensive and intensive margins. This shows that, although the treatment effect on having any college education is larger and only significant for the white sample, the effect on total number of college years completed is, if anything, larger in the black sample, and the magnitude of the effects on years of high school education are at least twice as large as the effects on years of college education for both samples.

We estimate that FLSA 1996 increased second-generation years of education by 0.33 (0.17) of a standard deviation among black (white) children, and increased high school completion by 10.2 (6.3) percentage points. An extensive review of the literature found no directly comparable estimates for three main reasons. First, in many studies of the effect of parental income on children's long-term educational outcomes, the treated group is treated at a much younger age and therefore the mechanisms at play are very different (investments in children rather than opportunity cost of the child's time). Second, among studies where treatment occurs during adolescence (Bastian & Michelmore, 2018; Akee et al., 2010), the treatment has not only an effect on parental income but also on labor supply (Bastian & Michelmore, 2018) or on incentives to complete high school (Akee et al., 2010), which may or not be similar to any effect of FLSA 1966 on labor supply.[16] Third, the time- and demographic setting is very different to ours (individuals born 1967-1995 in Bastian and Michelmore (2018) and American-Indians born in the 1980s in Akee et al. (2010)). Bearing these caveats in mind, we carry out back-of-the envelope comparisons extrapolating previous estimates to the increases in parental earnings due to FLSA 1966 and find consistent estimates with ours, namely a 9.8 pp predicted increase in high school graduation for black males using estimates in Bastian and Michelmore (2018) and a 6.5 pp increase for poor American-Indians using estimates in Akee et al. (2010). See Appendix B.2 for details.

Implications for Second-Generation Racial Inequality. Two margins contribute to a reduction in the racial gap in completed years of education in the next generation. First, the over-representation of black workers in newly covered industries (resulting in a larger proportion of black children than of white children being treated). Second, the larger educational attainment effects of FLSA 1966 on the treated in the black sample compared to

[16]Derenoncourt and Montialoux (2021) find that FLSA 1966 had no effect on hours worked and a near-zero effect on employment even for black workers, whereas Bailey et al. (2021) report no effect overall but a significant disemployment effect for black men. For a thorough discussion of how different theoretical models of firm behavior lead to different implications regarding the effect of minimum wages on employment, and evidence of no employment effect in Germany, see Dustmann et al. (2022).

Table 3: Treatment Effect Heterogeneity

	Blacks	Whites	South	Non-South	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			Years of Education			
Covered in 1967 ×						
Treated Cohorts	0.920**	0.461*	0.938***	0.255	0.510*	0.711***
	(0.380)	(0.237)	(0.290)	(0.201)	(0.280)	(0.231)
Mean	11.89	13.54	12.44	13.64	13.55	12.68
S.D.	2.79	2.72	3.1	2.52	2.63	2.98
Obs	858	2,655	1,442	2,070	1,890	1,616
Panel B:			College			
Covered in 1967 ×						
Treated Cohorts	0.044	0.075*	0.062	0.080***	0.090**	0.056
	(0.055)	(0.042)	(0.053)	(0.030)	(0.045)	(0.042)
Mean	.31	.54	.4	.54	.53	.42
Obs	858	2,655	1,442	2,070	1,890	1,616
Panel C:			Dropout			
Covered in 1967 ×						
Treated Cohorts	-0.102**	-0.063**	-0.160***	-0.001	-0.025	-0.128***
	(0.047)	(0.026)	(0.047)	(0.033)	(0.043)	(0.044)
Mean	.37	.14	.3	.13	.16	.24
Obs	964	2,761	1,532	2,193	2,022	1,697

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the heterogeneous effects on second-generation educational outcomes. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for Panel A, B, and C, respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. Each column refers to a sub-sample. All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

the white sample.

To quantify the overall reduction, we use a counterfactual analysis to investigate the contribution of FLSA 1966 to the overall reduction in the unconditional total racial gap in completed years of education, using the same approach as Derenoncourt and Montialoux (2021) employ to analyse the reform's effect on first-generation income inequality.

The counterfactual racial gap is computed under two key assumptions. First, that in the absence of the 1966 reform, the racial gap among post-treatment cohorts would have evolved similarly to that of pre-treatment cohorts; and second, that the gap between post- and pre-treatment cohorts among black children would have evolved similarly to the gap among white children. Under these two assumptions, the average racial gap can be written as follows:

$$\text{Total Racial Gap} = s_w^c \bar{X}_w^c + s_w^t \bar{X}_w^t - (s_b^c \bar{X}_b^c + s_b^t \bar{X}_b^t) \tag{4}$$

where s_b^c, s_b^t, s_w^c and s_w^t denote the share of workers in control and treated industries for black and white workers, respectively.[17] $\bar{X}_b^c$, $\bar{X}_b^t$, $\bar{X}_w^c$ and $\bar{X}_w^t$ is the average completed years of education among control and treated industries for black and white children, respectively. So, the first two terms in Equation (4) measure the average completed years of education of white children, while the last two terms measure the average years of education of black children. The education racial gap is $G^c = \bar{X}_w^c - \bar{X}_b^c$ among children of workers in control industries and $G^t = \bar{X}_w^t - \bar{X}_b^t$ among children of workers in treated industries ; while the gap between control and treated industries among black children is $G_b^{ct} = \bar{X}_b^c - \bar{X}_b^t$. We can thus re-write the total racial gap in Equation (4) as:[18]

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

We first plug in actual, observed parameters into Equation (5) to calculate the actual racial gap for each cohort group. Given our limited sample size, we split our birth cohorts (1942

[17]For clarity, $s_w^c + s_w^t = 1$ and $s_b^c + s_b^t = 1$. We estimate these shares using the March CPS 1962–1966. Approximately 44.5% of Black employees work in treated industries, while 55.5% work in control industries. Regarding White employees, only 26.1% work in treated industries, with 73.9% in control industries.

[18]Using the share of workers rather than the share of children means that we do not take into account potential differences in family size between workers of different groups defined by industry and race.

to 1952) into three groups rather than into single birth year cohorts to improve precision, resulting in two pre-FLSA 1966 cohorts (1942–1944 and 1945–1947) and one post-FLSA cohort (1948–1952).

Next, we construct the counterfactual racial gap among treated industries ($G^t_{\text{counterfactual}}$) for each cohort group. For pre-treatment cohorts (1942 to 1947), $G^t_{\text{counterfactual}}$ is the same as the actual total racial gap G^t_{actual}. For post-treatment cohorts (1948 to 1952), we use the actual racial gap among control industries minus the actual average pre-treatment difference between the racial gap among control and treated industries: $G^t_{\text{counterfactual}} = G^c_{\text{actual}} - \frac{1}{2}\sum_{k=1}^{2}(G^c_{\text{actual,k}} - G^t_{\text{actual,k}})$, where k denotes pre-FLSA 1966 cohort groups. We use the same approach to construct the counterfactual of G^{ct}_b. To calculate the counterfactual total racial gap, we replace G^t and G^{ct}_b in Equation (5) by their counterfactual, and all other parameters are the same as when calculating actual total racial gap.

Figure A5 plots the actual racial gap (black bar) and the racial gap in the absence of 1966 reform (counterfactual, gray bar), respectively. We find that the 1966 reform contributed to a reduction of around 20.5% of the racial gap in the completed years of education among post-treatment cohorts, or a 0.3 years reduction in the gap from 1.46 years to 1.16 years among the post-FLSA 1966 cohorts.

Next we use an alternative approach, which conditions on all the regressors we include in the regressions reported in Table 3 and makes clear the relative contribution of the two margins (higher share treated and larger ATT for the black sample) to the reduction in the education racial gap. More specifically, we compute the predicted change in average years of education in the black and white samples separately based on our estimates and then take the difference between the two. Namely, we compute $\Delta b = s^t_b \times \beta_{1,b}$ and $\Delta w = s^t_w \times \beta_{1,w}$ where $\beta_{1,b}$ ($\beta_{1,w}$) is the ATT estimate from Equation 2 in the black (white) sample and s^t_w and s^t_b still denote the race-specific shares of treated workers. We estimate $\Delta b \approx 0.409$ and $\Delta w \approx 0.120$ or a difference of about 0.29 and hence nearly identical to the 0.3 difference between the actual and counterfactual gaps obtained above. Indeed:

$$\Delta b = \overbrace{0.445}^{\substack{\text{Share}\\\text{treated}}} \times \overbrace{0.920}^{\text{ATT}} \approx \overbrace{0.409}^{\substack{\text{Predicted}\\\text{change}}}$$

$$\Delta w = 0.261 \times 0.461 \approx 0.120$$

$$\text{Difference} \qquad \approx 0.289 \text{ years of schooling}$$

4.4 Young Women Sample Results

Before turning to an investigation of the channels underpinning the effects of the FLSA 1966 on male education outcomes, we summarize the findings we obtain when replicating our analysis on the Young Women's sample, bearing in mind the limitations we highlighted in Section 2. Tables A16, A17 and A18 report our findings for the full sample (A16), high/low parental education samples (A17) and by race, South/non-South and Urban/Rural location (A18), showing a similar pattern as for the young men sample but with slightly smaller magnitudes, which could be due to genuine heterogeneity in treatment effects by gender, or because of more measurement error in the women's dataset.

5 Mechanisms

We explore two potential channels which may lead to the positive effects of the newly introduced minimum wage mandate on second generation's education outcomes and to the treatment effect heterogeneity we observe: i) the relaxation of the household's budget constraints due to the increase in the wages of the first generation induced by the 1966 reform (the "income channel"); and ii) the effect of FLSA 1966 on the second generation's education aspirations (the "aspirations channel"), which may in part be due to (i), but may also arise independently.

5.1 The Income Channel

First, we investigate the extent to which the 1966 reform affects first-generation wages and whether the pattern of heterogeneity in first-generation wages matches the one we observe for the overall effect on years of education.

Columns (1) and (2) of Table 4 essentially replicate the estimates in Derenoncourt and Montialoux (2021) with minor alterations to match the specifications we use in the rest of the analysis. We use the same data (March CPS) and the same specification as Derenoncourt and Montialoux (2021) except that we add a few regressors and restrict the sample to household heads. We also split post-treatment periods into two intervals, namely 1967 to 1972 and 1973 to 1980, as Derenoncourt and Montialoux (2021). Given our focus on second-generation human capital, the post-1972 effects on parental wages are much less relevant because they are unlikely to affect much the second generation's education outcomes—indeed, the youngest cohort in our analysis was already 20 years old in 1972.[19] Echoing findings in Derenoncourt and Montialoux (2021), the point estimates reported in Columns (1) and (2) indicate a 44% larger effect for black workers than white workers for the relevant period for our analysis (1967-1972).

In addition, we find that the effects on first-generation wages are much larger in the South (Columns (3) and (4)) and among low-education parents (Columns (7) and (8)), while the effect on rural wages is also larger but less markedly so (and not quite statistically significant with a p-value of 0.122, Columns (5) and (6)).

As shown in the bottom panel of Table 4, this aligns well with the patterns observed in Tables 2 and 3, which gives support to the idea that the new minimum wages mandate increased educational attainment by relaxing the household's budget constraint, since the

[19] Table A19 starts by replicating the findings of Derenoncourt and Montialoux (2021) in Columns (1) and (2), then controls for whether in a Southern state and rural/metro area in Columns (3) and (4) before restricting the sample to household heads in Columns (5) to (8) since in our main analysis the treatment status is based on the occupation of the household head specifically, leading to qualitatively similar conclusions. The findings reported in Columns (1) and (2) of Table 4 correspond to the specification matching most closely our education analysis (Columns (7) and (8) of Table A19).

more this budget constraint was relaxed, the larger the effect on overall educational attainment.

Table 4: Heterogeneous Effects on First Generation's Wages

	Blacks	Whites	South	non-South	Urban	Rural	Parental Education $\leq 11y$	Parental Education $> 11y$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Covered in 1967 ×								
1967-1972	0.059**	0.041*	0.077**	0.034	0.043**	0.054	0.088***	0.026*
	(0.023)	(0.020)	(0.032)	(0.020)	(0.020)	(0.033)	(0.027)	(0.014)
1973-1980	0.011	0.009	0.064	-0.002	0.008	0.031	0.087*	-0.010
	(0.041)	(0.038)	(0.051)	(0.034)	(0.034)	(0.057)	(0.049)	(0.034)
Mean	21,882.2	40,786.93	30,215.73	41,232.3	41,244.32	31,284.94	23,373.6	45,478.47
S.D.	15,164.76	27,947.11	22,674.42	28,379.67	28,962.16	22,057.21	16,228.07	28,734.71
Obs	23,682	232,862	73,361	183,187	182,871	73,677	73,641	182,906
Two-way FEs	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
South & Rural	Y	Y	Y	Y	Y	Y	Y	Y
	Corresponding effects on years of education							
Covered in 1967 ×								
Treated Cohorts	0.920**	0.461*	0.938***	0.255	0.510*	0.711***	0.776***	0.265
	(0.380)	(0.237)	(0.290)	(0.201)	(0.280)	(0.231)	(0.187)	(0.333)

Notes: March CPS 1962–1981 (Flood et al., 2023). This table shows the heterogeneous effects on first generation's wages. The dependent variable is log annual earnings. We report mean and standard deviation of the annual earnings among the control group. 'Two-way FEs' refers to industry and time period fixed effects. Control variables includes dummies of ethnicity, education, full-time/part-time, weeks working per year, hours working per week, marital status and occupation, and the linear, quadratic and cubic terms of working experience. Additional controls include southern region and rural status. The year 1962 is excluded and set to zero. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level. The last row of this table shows the corresponding estimates of the impact on second generation's completed years of education for each sub-sample, respectively (equivalent to Table 3 Panel A).

We next zoom in on racial differences in the effect of the 1966 FLSA and specifically test whether the larger increases in wages experienced by black household heads translated into a contemporaneous reduction in teenage labor force participation and an increase in teenage school enrollment. To do so, we focus on the sample of 15-18 year old children of a household head employed in either an FLSA 1966 industry or a FLSA 1938 industry and compare the probability of being in the labor force after FLSA 1966 relative to before (Equation 3) for children of household heads working in FLSA 1966 industries vs. FLSA 1938 industries.[20] One important caveat of this exercise is that "attending school" is a residual category in the CPS variable we use ("EMPSTAT") and there is no additional information on current enrollment in the CPS for the relevant time period. More specifically, the CPS classifies as "at work" anyone "doing any work at all for pay or profit, or working at least fifteen hours without pay in a family business or farm", even if they are also attending school. Only those who are not doing any paid work and not working at least 15 hours a week on a family enterprise and attending school are recorded as attending school. In addition, several broader issues with the CPS such as low-, race-specific coverage rates lead to individuals with low educational level being underrepresented, especially so for minorities (Heckman & LaFontaine, 2010). These data limitations are consequential, as demonstrated by the unexpected direction of the difference in school enrollment rates between black and white individuals in the control group (see "Mean" row in Table 5). With these caveats in mind, Table 5 reports our results. The first two columns suggest that black boys whose household head parent works in an FLSA 1966 industry are much more likely to be outside the labor force, although this effect is too imprecisely estimated to be statistically significant. This effect, however, is both statistically insignificant and very small in magnitude for white boys. Likewise, Columns (3) and (4) suggest that this effect is driven by an increase in (exclusive) school attendance, although this large effect (6 percentage points) is again statistically insignificant.

[20]We start the sample at age 15 because the relevant questions are only asked from age 15 in all the CPS waves used in the analysis.

Table 5: Impact on Child Labor

	Not in Labor Force		In School	
	Blacks	Whites	Blacks	Whites
	(1)	(2)	(3)	(4)
Covered in 1967 ×				
1967-1972	0.096	-0.017	0.061	-0.000
	(0.065)	(0.015)	(0.071)	(0.020)
1973-1980	0.069	-0.014	0.013	0.003
	(0.075)	(0.016)	(0.082)	(0.020)
Mean	.79	.69	.72	.66
Obs	6,177	64,136	6,181	64,173
Two-way FEs	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Notes: March CPS 1962–1981 (Flood et al., 2023). This table shows the effects on children's labor force and employment status. The sample includes males age 15 to 18 who are not the household head and living with parents. We drop observations in 1966 for potential anticipating effects. In columns (1) and (2), the dependent variable equals 1 if the child is not in the labor force, and 0 otherwise. In columns (3) and (4), the dependent variable equals 1 if the child is 'in school', and 0 otherwise. We report the mean of all dependent variables among the control group. 'Two-way FEs' refers to industry and time period fixed effects. Control variables includes dummies of southern region, rural status, education, full-time/part-time, weeks working per year, hours working per week, marital status and occupation, and the linear, quadratic and cubic terms of working experience of the corresponding household head. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are clustered at the industry level.

Finally, in Table 6 we test whether it is the case that individuals who drop out before graduating are less likely to do so due to financial constraints once the head of their household is covered by a minimum wage mandate, and whether any race-differentiated effects align with what would be expected given the larger effects of FLSA 1966 on the completed education of black children. To implement this test, we take advantage of the fact that, in the first survey wave, the NLSYM asked individuals who were no longer enrolled in school why they had decided to end their education when they did. In addition, in following waves, the survey asked respondents who were enrolled in education in the previous wave if they are still enrolled, have completed their degree or dropped out and, if they have dropped out before graduating, why they did so. These data have clear limitations, as reasons for ending studies are self-reported, and the exact questions and sample are selected based on prior enrollment. Despite these caveats, in the black sample, we see a larger, marginally significant reduction in the likelihood of reporting dropping out for financial reasons—i.e., of answering that they dropped out because of "financial difficulties, couldn't afford" or because they "needed to work", while we see an increase in reporting dropping out due to lack of interest (p-value: 0.102, column (3)) or to join the military (column (5)). Meanwhile, estimates in the white sample go in the same direction and are about half the size of the estimates for the black sample.

Table 6: Impact on Self-Reported Reasons for Dropping Out

	Financial		Tastes or Ability		Military	
	Blacks (1)	Whites (2)	Blacks (3)	Whites (4)	Blacks (5)	Whites (6)
Covered in 1967 × Treated Cohorts	-0.097*	-0.042	0.087	0.040	0.052**	0.011
	(0.055)	(0.029)	(0.053)	(0.045)	(0.021)	(0.036)
Mean	.39	.23	.12	.18	.04	.08
Obs	630	1,532	630	1,532	630	1,532

Notes: NLS: Original Cohort - Young Men 1966-1981. This table reports heterogeneous effects (by ethnicity) on (second-generation) reasons for ending education in the sample of individuals who either (i) were no longer enrolled in education at the first interview or (ii) in subsequent waves, were enrolled in education in the previous wave but have since dropped out. The dependent variables are binary variables equal to 1 if the respondent says that they dropped out for these reasons and 0 otherwise. We assign responses to types of reasons as follows. Financial reasons (Columns 1 and 2): "financial difficulties, couldn't afford", "needed to work". Tastes or Ability (Columns 3 and 4): "lack of ability, poor grades, or wasnt accepted"; "disliked school, wasnt interested, or problems with school personnel or peers"; "interest changed, former school did not offer desired course of study"; "goal changed, chose to attend different school". Military (Columns 5 and 6): "military". We report the mean of all dependent variables (Mean) among the control group. All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

5.2 The Aspirations Channel

In Section 5.1, we present several pieces of evidence in support of our hypothesis that minimum wage mandates, by increasing the earnings of workers in industries newly covered by the 1996 FLSA, relaxed their household's budget constraint, thus allowing teenagers to stay in school for longer. Indeed, we show that subgroups whose parents benefit the most in terms of wages also make the largest educational gains; that the large, long-term effects on completed education for the black sample (in the NLSYM) are matched by suggestive short-term effects on school attendance for teenagers living with household heads working in newly covered industries (in the CPS); and that children of parents covered by minimum wage mandates from 1967 onwards appear less likely to self-report dropping out of education due to financial reasons, and especially so for subgroups enjoying the largest first-generation wage gains and second-generation educational gains.

We now complement this evidence by documenting the effect of FLSA 1966 on the aspirations of the children of those working in newly covered industries. These aspirations could be interpreted as an intermediate outcome in the causal chain running from relaxed budget constraints to higher realized educational attainment. Certainly, as they realize that increased parental income may now permit them to stay in education for longer, young people are bound to update their educational objectives. It is however also possible that the FLSA 1966 may have had a direct impact on the aspirations of the children of workers in previously neglected occupations, especially so for black families given the direct racial discrimination embodied by the 1938 FLSA exclusions. And, in turn, higher aspirations may have had a direct effect on realized educational outcomes.[21]

In Panel A of Table 7, we first estimate the impact of the 1966 FLSA on the educational goals of children of parents working in treated occupations. The first three columns report our estimates for the 1967 wave, i.e. the first survey wave carried out after implementation,

[21]Guyon and Huillery (2021), for instance, find that pupils from socially disadvantaged families undervalue their abilities, and lower aspirations for education are correlated with poorer educational outcomes.

before repeating this analysis for 1968. The last three columns repeat the estimates we obtain for actual completed years of education, so we can compare impacts on aspirations and realized outcomes. We find positive effects on the educational goals of the children of parents working in treated occupations and these effects are larger (but imprecisely estimated) for the black sample. Interestingly, the magnitude of the effect on education goals is closely aligned with the magnitude of effects on completed years of education despite an overall tendency of aspirations to exceed realized education (as can be seen by comparing the mean of the dependent variables for aspirations vs. completed years of education).

Could FLSA 1966 have a direct effect on aspirations? We cannot rule out that the increase in educational aspirations simply reflects rational expectations by the children of workers newly covered by minimum wage mandates. However, given the speed at which aspirations are raised, it seems reasonable to assume that having a parent newly covered by FLSA 1966 may have had a direct effect on their children's aspirations, and more so in the case of black individuals who would have been aware of the racial discrimination FLSA 1966 put an end to.

In addition to asking respondents about their education objectives, NLSYM also asked about the occupation that young people would like to have in the future ("What kind of work would you like to be doing when you are 30 years old?"), and converts these occupation aspirations in terms of an occupation "prestige" index—namely the Duncan Socioeconomic Index (Duncan, 1961).[22] Panel B of Table 7 report our estimates of Equation 2 using this prestige index as dependent variable. Although estimates are imprecisely estimated and we lose close to 40% of the sample to attrition when measuring the prestige index of actual occupation in 1981 (i.e., when observed at age 29-39), similar conclusions

[22]The Duncan Socioeconomic Index assigns prestige based on the education and earnings of people employed in an occupation using weights taken from regressions of prestige scores assigned by survey respondents interviewed in the 1947 National Opinion Research Center survey on the occupational education and occupational earnings of male workers in the 1950 Census.

can be drawn from our analysis of educational and professional aspirations.[23] Namely, aspirations are raised rapidly among the children of those newly covered by mandated minimum wages, especially so for black children, and these raised aspirations largely materialize into realized outcomes.

[23]Attrition is balanced for education outcomes (see Table A6 showing attrition estimates corresponding to Table 3). For labor market outcomes, however, which given the age of respondents are only meaningfully observed in the late NLSYM survey waves, attrition is not only high, it is also much more pronounced for the black sample, as illustrated in Table 7 by the attrition of 35% for the white sample vs. 53% for the black sample when going from columns (2)-(3) to columns (8)-(9).

Table 7: Impact on Aspirations

	Desired in 1967			Desired in 1968			Realized		
	Full	Blacks	Whites	Full	Blacks	Whites	Full	Blacks	Whites
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:	Educational Goals and Completed Education								
Covered in 1967 ×									
Treated Cohorts	0.674***	0.955	0.294	0.695***	0.920	0.517***	0.788***	0.920**	0.461*
	(0.234)	(0.696)	(0.184)	(0.257)	(0.814)	(0.179)	(0.216)	(0.380)	(0.237)
Mean	13.93	12.75	14.27	14.24	13.42	14.47	13.18	11.89	13.54
S.D.	3.1	3.39	2.93	2.82	3.01	2.73	2.82	2.79	2.72
Obs	3,270	804	2,465	3,026	728	2,297	3,513	858	2,655
Panel B:	Professional Aspirations and Actual Occupation in 1981								
Covered in 1967 ×									
Treated Cohorts	5.539**	7.246*	4.519**	5.263***	8.479*	3.823**	6.569***	7.218**	3.535
	(2.177)	(3.748)	(2.133)	(1.936)	(4.925)	(1.891)	(1.633)	(3.222)	(2.234)
Mean	53.28	45.09	55.75	53.47	45.78	55.72	45.28	31.01	48.54
S.D.	25.6	25.74	25.06	24.95	25.46	24.35	26.02	23.76	25.41
Obs	2,923	767	2,155	2,864	727	2,136	2,366	498	1,867
Two-way FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impacts on children's educational goals (Panel A) and professional aspirations (Panel B), measured respectively as their goal in terms of completed years of education and the Duncan Socioeconomic Index of their desired occupation. The dependent variables are educational goal/professional aspirations in 1967 in columns (1) to (3), educational goal/professional aspirations in 1968 in columns (4) to (6), and completed years of education/Duncan Socioeconomic Index of the actual occupation in columns (7) to (9). We report mean and standard deviation of all dependent variables among the control group. Within each dependent variable, each column refers to a different sample, respectively. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definition of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

6 Conclusion

This paper investigates the causal effect of the Fair Labor Standards Act (FLSA) of 1966, which extended mandated minimum wages to industries in which African American workers were over-represented, on second-generation long-term education outcomes, with a focus on implications for racial inequality.

Using data from the National Longitudinal Survey of Young Men (NLSYM), we exploit variation in exposure to parental minimum wage coverage and age of exposure in a difference-in-differences research design. We show that the minimum wage reform had an economically substantial and statistically significant effect on second-generation educational attainment, and especially so for black children, thus contributing to reducing racial inequalities. The main driver of the black-white difference in the effect of FLSA 1966 on completed years of education is the larger decline in high school drop out among black children of workers in the treated industries.

Given the age of our post-treatment cohorts, who were aged between 14 and 18 immediately prior to the implementation of the new minimum wage mandate, we hypothesize that the latter affected second-generation educational outcomes mainly by relaxing the household's budget constraint and hence enabling older children to stay in education for longer. We find support for this mechanism from the alignment between subgroups who benefit the most in terms of wage increases and subgroups whose children see their educational outcomes improve the most, as well as from direct suggestive evidence of a contemporaneous reduction in teenage labor force participation and a reduction in the share of individuals who report financial difficulties as the reason for not completing their high-school or college degree. We also find that second-generation education and professional aspirations increase immediately and broadly in line with future outcome realizations.

All in all, our findings suggest that minimum wage mandates can, and the 1966 FLSA in particular did kick-start a virtuous circle of equality in the next generation, thus adding an intergenerational dimension to the long-neglected "racial justice component to the minimum wage laws” (Smythe & Hsu, 2023).

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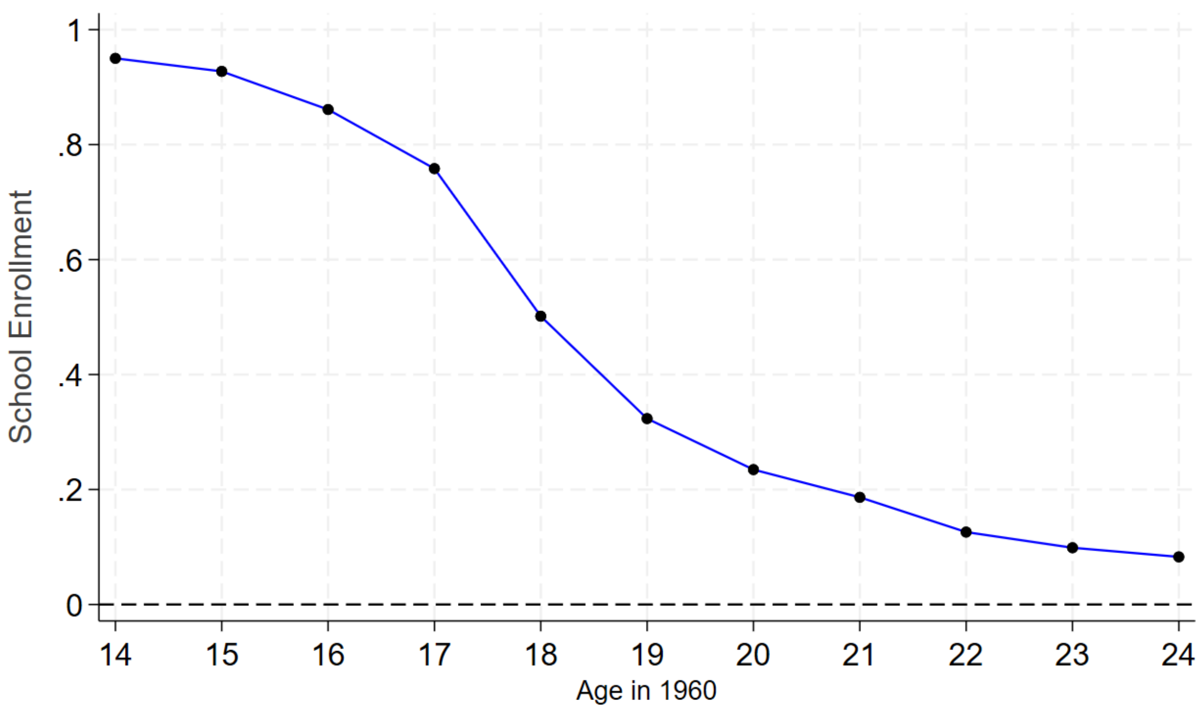
Online Appendix

Appendix A Figures and Tables

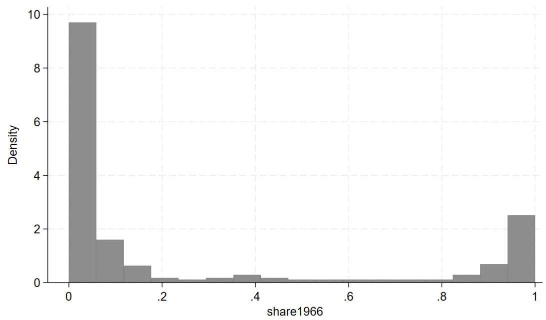
Figure A1: School Enrollment Rate by Age in 1960



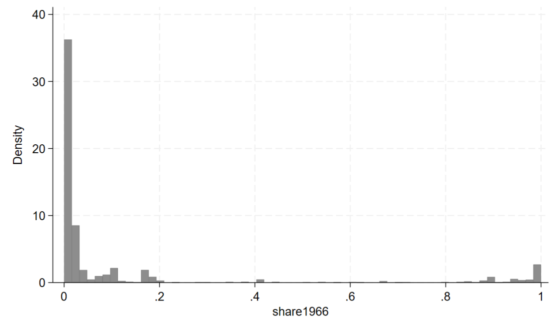
Data source: Census 1960 micro data by IPUMS (Ruggles et al., 2024).

Figure A2: Distribution of 'Share of Workers in Treated Industries'

(a) Each occupation given equal weight

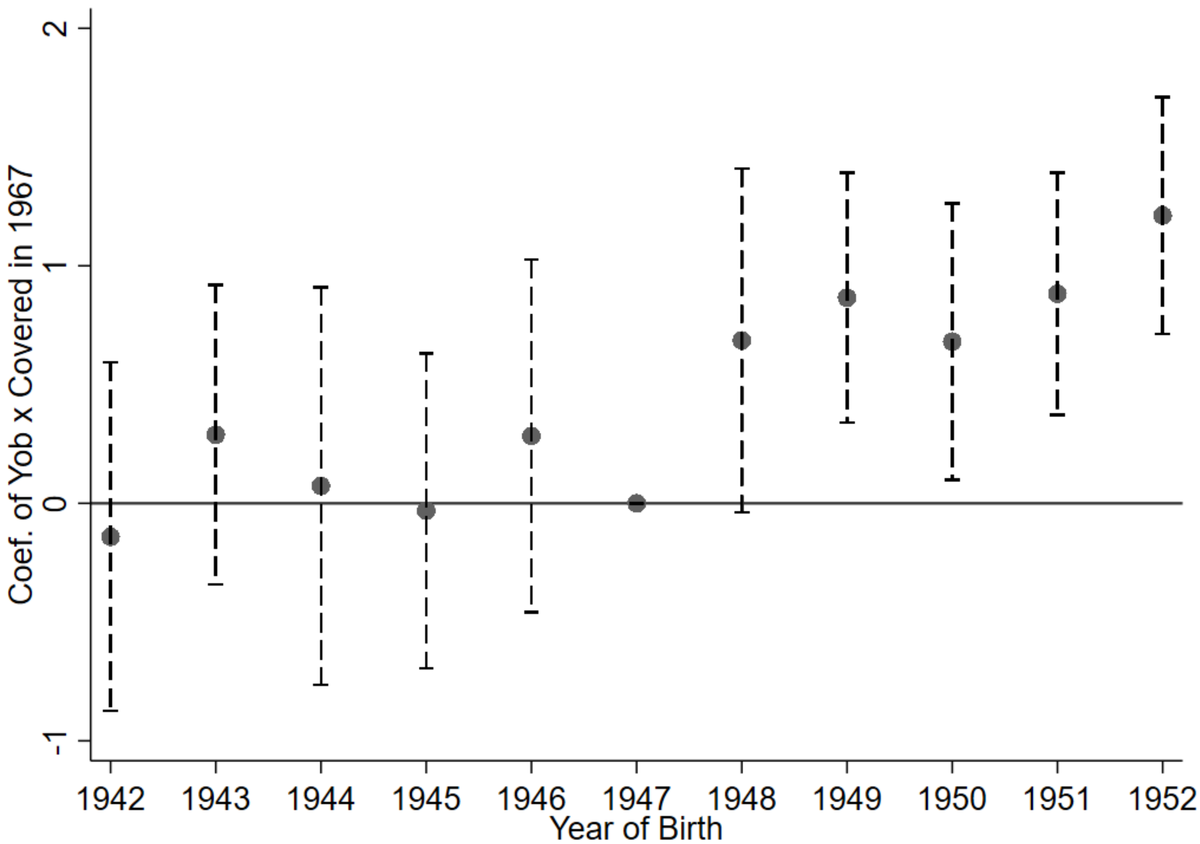


(b) Occ. weighted by num. of workers



Notes: This figure shows the distribution of the share of workers in treated industries by occupation. Data source: Census 1960 micro data by IPUMS (Ruggles et al., 2024).

Figure A3: Dynamic Effect by Age in 1966

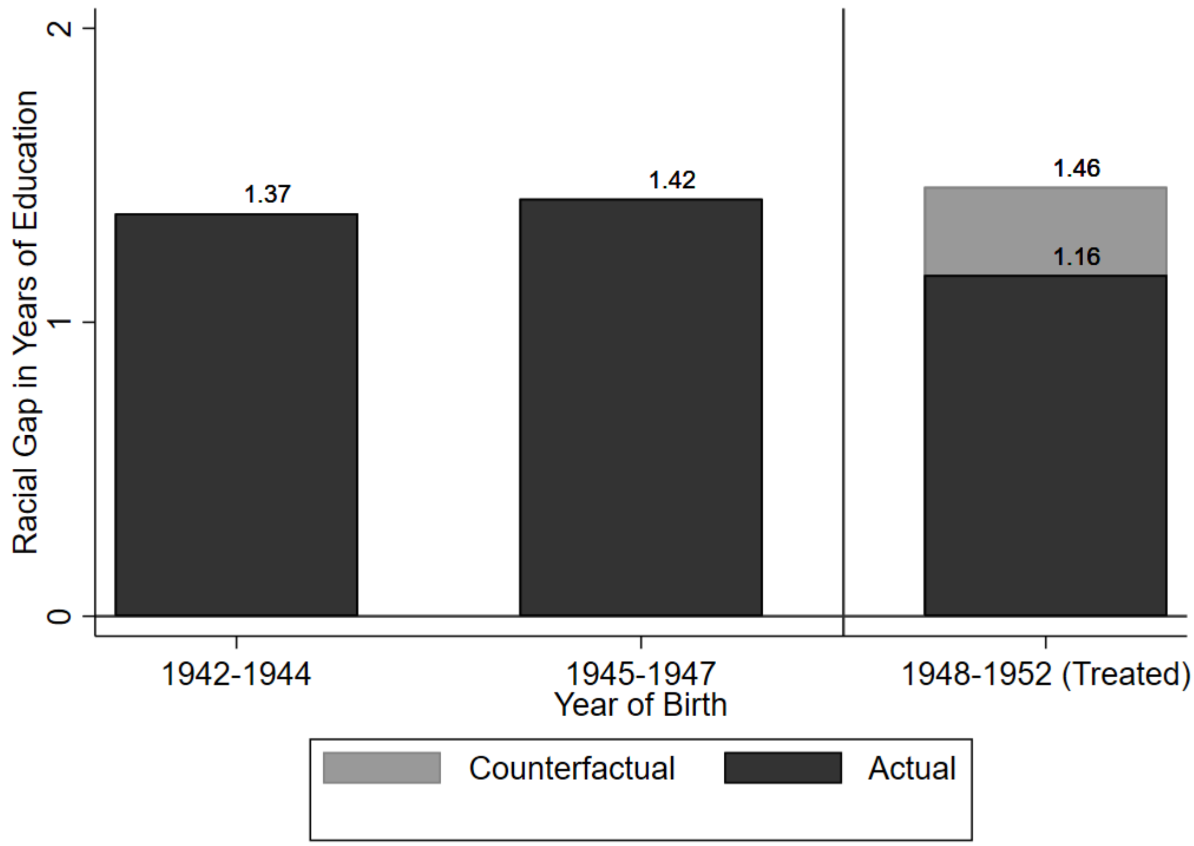


Notes: NLS: Original Cohort - Young Men 1966-1981. This figure shows the dynamic effect of the 1966 reform on the second generation's completed year of education by year of birth (cohort) using 1947 cohort (age 19 in 1966) as the reference group. This figure plots point estimates with 95% confidence interval.

Figure A4: Dynamic Effect by Age in 1966 with Restricted Sample

Notes: NLS: Original Cohort - Young Men 1966-1981. This figure shows the dynamic effect of the 1966 reform on the second generation's completed year of education by year of birth (cohort) using 1947 cohort (age 19 in 1966) as the reference group. Sample excludes occupations with a share of workers in treated industries between 20% to 80%. This figure plots point estimates with 95% confidence interval.

Figure A5: Actual versus Counterfactual Racial Gap in Education



Notes: Data source for the racial gap and control versus treated industries gap in years of education: NLS: Original Cohort - Young Men 1966-1981. Data source for the share of black and white workers in treated and control industries: March CPS 1962-1966, except for wave 1963, which lacks demographic information (Flood et al., 2023). This figure shows the un-adjusted (i.e., not controlling for observable characteristics) racial gap in years of education by cohort groups. The black vertical line split cohorts into two categories: pre-treatment (left-hand-side) and post-treatment (right-hand-side).

Table A1: Variables in NLSYM

Variable	Wave	Values	Definition
Parental Occupation	1966	3-digit 1960 codes	What kind of work was your father (or head of the household) doing when you were 14 years old?
Age	1966	14 to 24	Age in 1966.
Black	1966	0/1	Black or White.
Rural	1966	1 to 8	See Table notes.
South	1966	0/1	Live in South or not.
Labor Market Size	1966	1 to 8	See Table notes.
Family Income	1966	1 to 11	See Table notes.
Subsidies	1966	0/1	Did anyone in this family receive any welfare or public assistance in the last 12 months?
Siblings	1966	0 to 5+	We code '5 and over' as one category.
Library Card	1966	0/1	Did you or your parents have a library card when you were about 14 years old?
Years of Education	1966-1981	0 to 18	We track individuals' education records from 1966 to 1981 to calculate the highest grade completed by age 22 or over.
Dropout	1966-1981	0/1	Years of education is strictly smaller than 12 or not, by age 19 or over.
College Attainment	1966-1981	0/1	Years of education is strictly greater than 12 or not by age 22 or over.
Educational Goal	1967 & 1968	0 to 18	Years of education goal based on the survey question: 'How much more education would you like to get?'.
Desired Occupation	1967 & 1968	3-digit 1960 codes	Duncan Index of the Occupation desired at age 30.

Notes: : Categories are as follows: **Rural**: Urbanized area - 3,000,000 or more, urbanized area - 1,000,000 to 2,999,999, urbanized area - 250,000 to 999,999, urbanized area - under 250,000, urban places 25,000 - outside urbanized areas, urban places - 10,000 to 24,999, urban places 2,500 to 9,999, and rural. **Labor market size**: Less than 50,000, 50,000 to 199,999, 200,000 to 399,999, 400,000 to 499,999, 500,000 to 799,999, 800,000 to 999,999, 1,000,000 to 2,999,999, and more than 3,000,000. **Family income**: Under $1,000, $1,000 - $1,999, $2,000 - $2,999, $3,000 - $3,999, $4,000 - $4,999, $5,000 - $5,999, $6,000 - $7,499, $7,500 - $9,999, $10,000 - $14,999, $15,000 - $24,999, $25,000 and over.

Table A2: Variable means

	All	Black	White
Panel A: Household characteristics in 1966			
Household Heads' schooling (years)	9.83	7.67	10.43
Family income (11 categories from 1 (<$1,000) to 11 (≥ $24,000))	6.92	5.03	7.63
Number of siblings (count)	2.83	3.67	2.56
Urban status (8 categories, from low to high)	5.17	4.95	5.24
Labor market size (8 categories, from low to high)	3.15	3.04	3.18
Family received welfare/public assistance (binary)	0.06	0.18	0.02
Library card was available age 14 (binary)	0.65	0.42	0.73
Lived in South (binary)	0.41	0.74	0.30
Panel B: Treatment status			
Black (binary)	0.24	1.00	0.00
Treated by 1967 reform (binary)	0.27	0.42	0.22
Observations	3,513	858	2,655

Notes: This table shows variable means across the three samples. The 11 categories for family income are 1: under $1,000; 2: $1,000 - $1,999; 3: $2,000 - $2,999; 4: $3,000 - $3,999; 5: $4,000 - $4,999; 6: $5,000 - $5,999; 7: $6,000 - $7,499; 8: $7,500 - $9,999; 9: $10,000 - $14,999; 10: $15,000 - $24,999; 11: $25,000 and over. The 8 categories for urban status are 1: urbanized area - 3,000,000 or more. ; 2: urbanized area - 1,000,000 to 2,999,999 ; 3: urbanized area - 250,000 to 999,999 ; 4: urbanized area - under 250,000 ; 5: urban places 25,000 - outside urbanized areaS ; 6: urban places 10,000 - 24,999 outside urbanized areas; 7: urban places 2,500 - 9,999 outside urbanized areas; 8: rural based on 1960 census classification. The 8 categories for labor market size are 1:less than 50,000; 2:50,000 to 199,999; 3:200,000 to 399,999; 4:400,000 to 499,999; 5:500,000 to 799,999; 6:800,000 to 999,999; 7:1,000,000 to 2,999,999; 8:3,000,000 and more.

Table A3: Excluding Age 19 and 20 in 1966

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 ×						
Treated Cohorts	0.690***	0.822***	0.052	0.079**	-0.117***	-0.123***
	(0.226)	(0.241)	(0.035)	(0.036)	(0.029)	(0.031)
Mean	13.17	13.17	.48	.48	.18	.18
S.D.	2.77	2.77				
Obs	3,043	3,043	3,043	3,043	3,215	3,215
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. Sample: excludes individuals aged 19 or 20 in 1966. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A4: Placebo Test for Educational Outcomes

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 ×						
Placebo Cohorts	0.038	-0.044	-0.000	-0.004	-0.033	-0.014
	(0.297)	(0.307)	(0.052)	(0.052)	(0.044)	(0.046)
Mean	12.92	12.92	.44	.44	.26	.26
S.D.	3.1	3.1				
Obs	1,535	1,535	1,535	1,535	1,584	1,584
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the placebo test of the impact on second-generation educational outcomes. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. The 'Placebo Cohort' compares old cohorts (age 19 to 21 in 1966) versus very old cohorts (age 22 to 24 in 1966). 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A5: Test for Differential Attrition Rate (full sample)

	Years of Edu & College		Dropout	
	(1)	(2)	(3)	(4)
Covered in 1967 ×				
Treated Cohorts	-0.006	-0.010	0.006	0.005
	(0.029)	(0.032)	(0.020)	(0.020)
Mean	.9	.9	.95	.95
Obs	3,931	3,931	3,931	3,931
Two-way FEs	Y	Y	Y	Y
Controls	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table tests the differential attrition rate for educational outcomes (years of education and college attainment in Panel A and dropout before finishing high school in Panel B). The dependent variable equals 1 if an individual is in main regression sample (e.g., observed after age 22 for years of education and college, and observed after age 19 for 'dropout'), 0 otherwise. All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A6: Test for Differential Attrition Rate by Sub-samples

	Blacks	Whites	South	non-South	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	Years of Edu & College					
Covered in 1967 ×						
Treated Cohorts	0.042	-0.018	-0.018	-0.012	-0.078*	0.013
	(0.074)	(0.021)	(0.036)	(0.043)	(0.040)	(0.026)
Mean	.82	.93	.91	.9	.89	.91
Obs	1,051	2,879	1,611	2,319	2,140	1,784
Panel B:	Dropout					
Covered in 1967 ×						
Treated Cohorts	0.038	-0.001	0.004	-0.005	-0.040	0.022
	(0.039)	(0.019)	(0.017)	(0.032)	(0.029)	(0.016)
Mean	.92	.96	.96	.95	.95	.96
Obs	1,051	2,879	1,611	2,319	2,140	1,784

Notes: NLS: Original Cohort - Young Men 1966-1981. This table tests the differential attrition rate for educational outcomes (years of education and college attainment in Panel A and dropout before finishing high school in Panel B). The dependent variable equals 1 if an individual is in main regression sample (e.g., observed after age 22 for years of education and college, and observed after age 19 for 'dropout'), 0 otherwise. We report the mean of the dependent variable. Each column refers to a sub-sample. All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A7: 1960s Social Protection and Educational Programs and Interventions

	Program	Dates (Coverage)	Relevant Cohorts	References/Details
1	Food Stamps/SNAP	1964 to 1975 (county roll-out)	Born after 1960	Hoynes et al. (2016) (no effect for children exposed after age 4)
2	Head Start	1965 onwards (by county application)	Born 1960 or later	Ludwig and Miller (2007)
3	Medicare	July 1966 (US-wide)	All	Almond et al. (2006)
4	Title I School Funding (poverty formula)	1965 onwards (US-wide)	Born 1947 or later	Cascio et al. (2013) (reduced drop out of Whites, not that of Blacks)
5	School desegregation orders	1965 onwards	Born 1960 or later	Cascio et al. (2010) Anstreicher et al. (2022) (no significant effect of court orders after age 5)
6	Extended school leaving age	(2 states only) between 1966-1969	Born 1948 onwards	Simon and Grant (1966a) Simon and Grant (1966b) Oreopoulos (2009) 1966-1969: Texas 1971-1972: Virginia, Wisconsin 1966-1972: Hawaii

Notes: "Relevant cohorts" correspond either to recipient cohorts where eligibility is effectively restricted by age or to cohorts for which prior literature studying effects by timing of exposure shows significant effects. In the latter case, further detail is provided in the last column.

Table A8: Excluding Occupations with 20-80% Treated Industries

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.823***	0.847***	0.078***	0.086***	-0.111***	-0.106***
	(0.189)	(0.201)	(0.024)	(0.027)	(0.032)	(0.032)
Mean	13.17	13.17	.48	.48	.19	.19
S.D.	2.83	2.83				
Obs	3,407	3,407	3,407	3,407	3,606	3,606
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. Sample: excludes occupations with a share of workers in treated industries between 20% to 80%. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A9: Continuous Treatment: Probability of FLSA 1966 Coverage

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Prob. Covered in 1967 × Treated Cohorts	0.788***	0.825***	0.078***	0.091***	-0.100***	-0.098***
	(0.228)	(0.237)	(0.029)	(0.031)	(0.035)	(0.034)
Mean	13.18	13.18	.49	.49	.19	.19
S.D.	2.82	2.82				
Obs	3,513	3,513	3,513	3,513	3,726	3,726
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. The treatment variable, 'Prob. Covered in 1967', measures the share of employees working in the treated industries by occupation. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are clustered at the occupation level.

Table A10: Excluding Age 17 and 18 in 1966

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.828***	0.794***	0.099*	0.097**	-0.104***	-0.095***
	(0.271)	(0.256)	(0.052)	(0.046)	(0.032)	(0.034)
Mean	13.05	13.05	.46	.46	.21	.21
S.D.	2.86	2.86				
Obs	2,777	2,777	2,777	2,777	2,926	2,926
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. Sample: excludes individuals aged 17 or 18 in 1966. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A11: The Impact on Second Generation's Education Outcomes (Weighted)

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.575***	0.615***	0.077**	0.091**	-0.059*	-0.062**
	(0.213)	(0.207)	(0.036)	(0.037)	(0.033)	(0.032)
Mean	13.18	13.18	.49	.49	.19	.19
S.D.	2.82	2.82				
Obs	3,513	3,513	3,513	3,513	3,726	3,726
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. All regressions are weighted by sample weights. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A12: Alternative Specifications

	Years of Edu				College				Dropout			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Covered in 1967 × Treated Cohorts	0.788***	0.828***	0.652***	0.600***	0.080***	0.083***	0.063**	0.066**	-0.103***	-0.112***	-0.083***	-0.077***
	(0.216)	(0.208)	(0.197)	(0.185)	(0.029)	(0.028)	(0.029)	(0.029)	(0.032)	(0.033)	(0.031)	(0.030)
Mean	13.18	13.18	13.18	13.18	.49	.49	.49	.49	.19	.19	.19	.19
S.D.	2.82	2.82	2.82	2.82								
Obs	3,513	3,480	3,513	3,513	3,513	3,480	3,513	3,513	3,726	3,696	3,726	3,726
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
2-Digit OCC by T FE	Y	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y
3-Digit OCC FE	N	Y	N	N	N	Y	N	N	N	Y	N	N
South linear trend	N	N	Y	N	N	N	Y	N	N	N	Y	N
South by cohort FE	N	N	N	Y	N	N	N	Y	N	N	N	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes using alternative specifications. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) to (4), (5) to (8), and (9) to (12), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. All regressions include cohort (children) fixed effects and control variables, including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Columns (1), (5), and (9) use our baseline specification, which are equivalent to columns (2), (4), and (6) in Table 1. In columns (2), (6) and (10), we use 3-digit occupation fixed effects instead of 2-digit by treatment status occupation fixed effects used in the baseline specification. In columns (3), (7), and (11), we add southern region by cohort linear trend to our baseline specification. In columns (4), (8), and (12), we add southern region by cohort fixed effects to our baseline specification. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A13: Placebo Test: Sub-samples

	Blacks	Whites	South	non-South	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			Years of Edu			
Covered in 1967 ×						
Placebo Cohorts	-0.018	-0.000	-0.225	0.140	-0.305	-0.052
	(0.348)	(0.362)	(0.527)	(0.348)	(0.348)	(0.615)
Mean	11.23	13.41	11.84	13.62	13.47	12.23
S.D.	3	2.94	3.28	2.76	2.94	3.16
Obs	341	1,191	599	930	865	666
Panel B:			College			
Covered in 1967 ×						
Placebo Cohorts	-0.007	0.018	-0.057	0.046	-0.010	-0.054
	(0.061)	(0.066)	(0.069)	(0.078)	(0.065)	(0.088)
Mean	.23	.5	.32	.52	.51	.36
Obs	341	1,191	599	930	865	666
Panel C:			Dropout			
Covered in 1967 ×						
Placebo Cohorts	-0.030	-0.007	0.012	-0.044	0.015	-0.046
	(0.096)	(0.039)	(0.109)	(0.038)	(0.060)	(0.098)
Mean	.52	.18	.4	.16	.2	.33
Obs	359	1,222	615	963	899	681

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows results of placebo tests on second-generation educational outcomes for sub-samples of interest. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for Panel A, B, and C, respectively. Each column refers to a sub-sample. The 'Placebo Cohort' compares old cohorts (age 19 to 21 in 1966) versus very old cohorts (age 22 to 24 in 1966). All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A14: Gender of Household Head

	Full sample		Blacks		Whites	
	All	ex. only with mother	All	ex. only with mother	All	ex. only with mother
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.788***	0.617**	0.920**	0.733	0.461*	0.388
	(0.216)	(0.280)	(0.380)	(0.447)	(0.237)	(0.272)
Mean	13.18	13.26	11.89	12.08	13.54	13.57
S.D.	2.82	2.79	2.79	2.74	2.72	2.72
Obs	3,513	3,258	858	720	2,655	2,537
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on second-generation educational outcomes. The dependent variable is 'completed years of education'. In columns (2), (4), and (6), we exclude children who lived with their mother but not their father at age 14. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A15: Impact on Combined Extensive and Intensive Margin of College and Dropout

	Years of College Edu			Years to Finish High School		
	Full	Black	White	Full	Black	White
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.290**	0.388	0.118	-0.518***	-0.676***	-0.318***
	(0.146)	(0.246)	(0.205)	(0.102)	(0.194)	(0.080)
Mean	1.7	.92	1.91	.54	1.05	.38
S.D.	2.09	1.66	2.15	1.38	1.79	1.18
Obs	3,513	858	2,655	3,726	964	2,761
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: NLS: Original Cohort - Young Men 1966-1981. This table shows the impact on intensive margin of 'college' and 'dropout' among the second-generation. The dependent variables are 'years of college education' and 'further years of education required to finish high school' for columns (1) to (3) and (4) to (6), respectively. Each column refers to a different sample. We report the mean and standard deviation of all dependent variables among the control group. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A16: Impact on Second Generation's Education Outcomes (Women Sample)

	Years of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 × Treated Cohorts	0.403	0.349	0.034	0.035	-0.079	-0.077*
	(0.263)	(0.227)	(0.040)	(0.036)	(0.055)	(0.042)
Mean	12.91	12.91	.4	.4	.17	.17
S.D.	2.49	2.49				
Obs	3,574	3,574	3,574	3,574	3,769	3,769
Two-way FEs	Y	Y	Y	Y	Y	Y
controls	N	Y	N	Y	N	Y

Notes: NLS: Original Cohort - Young Women 1968-1983. This table shows the impact on second-generation educational outcomes. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A17: Household Head's Education Level: cutoff 11 (Women Sample)

	Years of Edu		College		Dropout	
	≤ 11	> 11	≤ 11	> 11	≤ 11	> 11
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967 $\times$ Treated Cohorts	0.604***	-0.020	0.007	0.093	-0.099**	-0.037
	(0.171)	(0.391)	(0.040)	(0.074)	(0.045)	(0.027)
Parental education	$\leq 11y$	$> 11y$	$\leq 11y$	$> 11y$	$\leq 11y$	$> 11y$
Mean	12.2	13.95	.26	.6	.23	.06
S.D.	2.31	2.32				
Obs	1,829	1,497	1,829	1,497	1,923	1,585
Two-way FEs	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: NLS: Original Cohort - Young Women 1968-1983. This table shows the impact on second-generation educational outcomes by parental (household head) education level. We split sample based on whether the household head had college attainment (completed years of education strictly greater than 12 or not). The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for columns (1) and (2), (3) and (4), and (5) and (6), respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. 'Two-way FEs' refers to occupation (parents) and age in 1966 (children) fixed effects. Control variables include ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A18: Treatment Effect Heterogeneity (Women Sample)

	Blacks	Whites	South	non-South	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			Years of Edu			
Covered in 1967 ×						
Treated Cohorts	0.581*	0.196	0.465*	0.035	0.005	0.428*
	(0.306)	(0.234)	(0.261)	(0.266)	(0.295)	(0.232)
Mean	12.32	13.1	12.5	13.16	13.08	12.71
S.D.	2.48	2.46	2.65	2.35	2.5	2.46
Obs	943	2,628	1,438	2,135	1,785	1,789
Panel B:			College			
Covered in 1967 ×						
Treated Cohorts	0.128	0.006	0.001	0.035	0.021	0.044
	(0.078)	(0.033)	(0.054)	(0.041)	(0.058)	(0.043)
Mean	.32	.43	.35	.43	.43	.37
Obs	943	2,628	1,438	2,135	1,785	1,789
Panel C:			Dropout			
Covered in 1967 ×						
Treated Cohorts	-0.067	-0.077***	-0.081	-0.023	0.023	-0.092**
	(0.078)	(0.026)	(0.053)	(0.038)	(0.053)	(0.046)
Mean	.25	.14	.24	.12	.15	.18
Obs	997	2,769	1,504	2,265	1,890	1,879

Notes: NLS: Original Cohort - Young Women 1968-1983. This table shows the heterogeneous effects on second-generation educational outcomes. The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for Panel A, B, and C, respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education. Each column refers to a sub-sample. All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are clustered at the occupation level.

Table A19: Effect on First Generation's Wages (Alternative Specifications and Samples)

	Full Sample				Only Household Heads			
	Blacks	Whites	Blacks	Whites	Blacks	Whites	Blacks	Whites
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Covered in 1967 ×								
1967-1972	0.095***	0.054**	0.070***	0.050*	0.085***	0.044**	0.059**	0.041*
	(0.022)	(0.023)	(0.023)	(0.024)	(0.023)	(0.020)	(0.023)	(0.020)
1973-1980	0.078*	0.036	0.039	0.030	0.049	0.012	0.011	0.009
	(0.037)	(0.042)	(0.039)	(0.042)	(0.042)	(0.038)	(0.041)	(0.038)
Mean	20,025.52	31,728.47	20,030.81	31,728.37	21,882.2	40,783.1	21,882.2	40,786.93
S.D.	13,981.36	24,486.58	13,981.01	24,490.14	15,164.76	27,941.07	15,164.76	27,947.11
Obs	37,768	370,052	37,026	348,604	24,154	246,462	23,682	232,862
Two-way FEs	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
South & Rural	N	N	Y	Y	N	N	Y	Y

Notes: This table shows the effects on first generation's wages using alternative specifications and samples. The dependent variable is log annual earnings. In columns (1) and (2), we replicate results of Model (1) in Table V in Derenoncourt and Montialoux (2021). In columns (3) and (4), we add two additional controls variables, namely South and rural status. In columns (5) to (8), we restrict the sample to household heads. We report mean and standard deviation of the annual earnings among the control group. 'Two-way FEs' refers to industry and time period fixed effects. Control variables includes dummies of ethnicity, education, full-time/part-time, weeks working per year, hours working per week, marital status and occupation, and the linear, quadratic and cubic terms of working experience. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

Appendix B Further Information

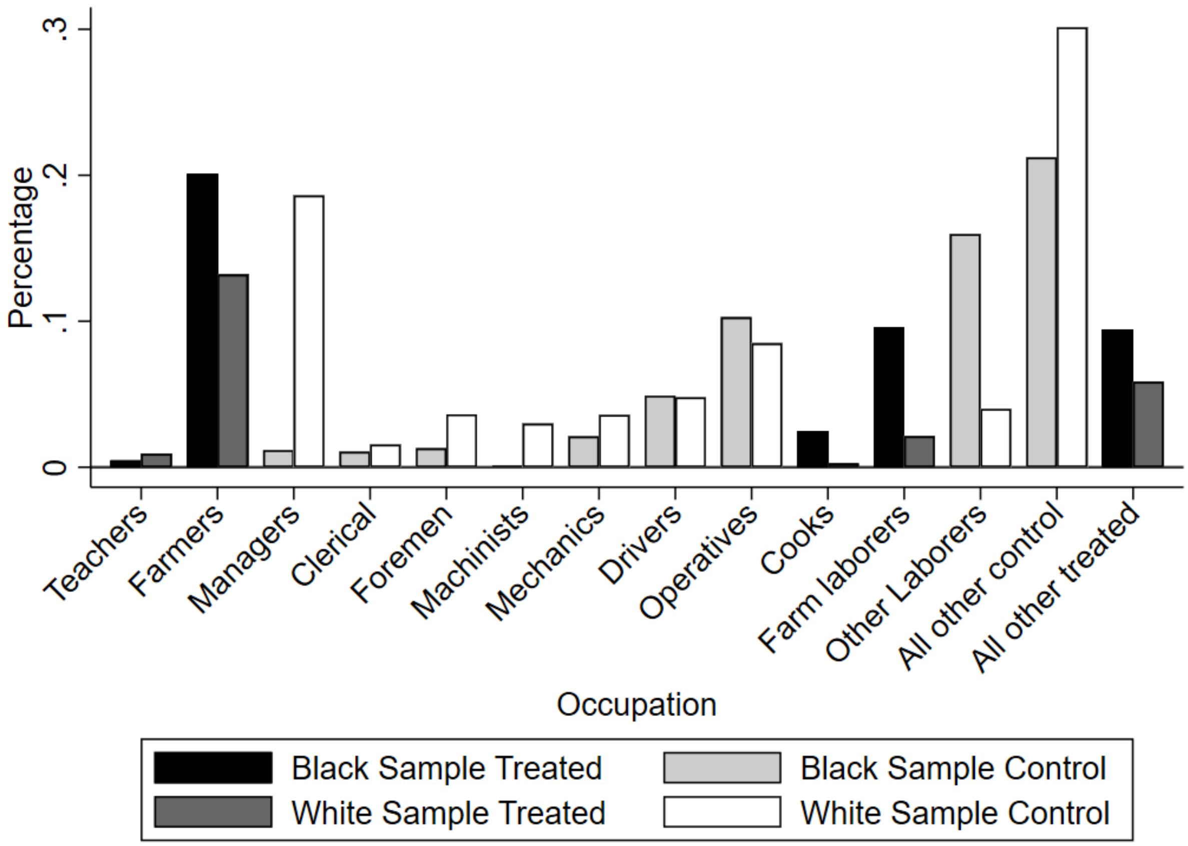
B.1 Treatment Assignment Based on Parental Occupation

As explained in Section 2, we match (3-digit) parental occupation when the respondent was 14 years old to industries using Formula (1).

In figure B1, we show the distribution of (3-digit) occupations, separately for the black and white samples, and whether or not these occupations are classified as covered by FLSA 1966 using our matching procedure—showing that a larger share of black parents are treated (42% vs 22.3% of white parents).

The NLSYM does not include information on whether the household head was self-employed, so that we are unable to exclude those who are self-employed from the sample. To assess the sensitivity of our findings to keeping the self-employed in the analysis, we repeat the estimation after dropping randomly selected observations up to the share of self-employed workers in each occupation according to CPS data. More precisely, we first compute the share of self-employed workers within occupation $\times$ race $\times$ parental education cells using data from the March CPS 1967-1970 (Flood et al., 2023) and merge these shares with our NLSYM dataset. In a second step we randomly drop a share of the NLSYM dataset corresponding to the share of self-employed workers in that occupation $\times$ race $\times$ parental education cell and re-estimate equation (2). We repeat this 100 times and report the average coefficients across these 100 iterations. We bootstrap the entire process to obtain standard errors that take the additional sample variation into account. The results are shown in Table B1. Despite standard errors increasing by more than 50% in most regressions, this exercise confirms our main conclusions and suggests larger differences in treatment effects between black and white children.

Figure B1: Distribution of Parental Occupation (3-Digit) by Ethnicity



Notes: Data Sources: NLS: Original Cohort - Young Men 1966-1981 ('Years of Education' sample). This figure shows the distribution of 3-digit occupation by treatment status for the black and white sample. All the bars corresponding to the black sample add up to one, and all the bars corresponding to the white sample add up to one. There are 41 occupations in the 'All other treated' category and 131 occupations in the 'All other control' category.

B.2 Back of the envelope comparisons with estimates from prior studies

First, we use Derenoncourt and Montialoux (2021)'s figures on the average annual earnings for the treated group reported in their Table II, namely $20,854 for black workers (in $2017), and our estimates of the effect of the FLSA 1966 on the earnings of household heads (0.059 or a 5.9 percent increase, Table 4 column 1) to calculate the annual

Table B1: Adjusting the Sample for the Share of Self-Employed

	All	Race		Parental Education	
		Blacks	Whites	$\leq 11y$	$> 11y$
	(1)	(2)	(3)	(4)	(5)
Panel A:			Years of Education		
Covered in 1967 ×					
Treated Cohorts	0.628*	1.219***	0.039	0.802**	0.114
	(0.372)	(0.402)	(0.441)	(0.401)	(0.509)
Mean	13.18	11.89	13.54	12.37	14.55
S.D.	2.82	2.79	2.72	2.69	2.39
Panel B:			College		
Covered in 1967 ×					
Treated Cohorts	0.044	0.106	-0.018	0.046	0.063
	(0.040)	(0.073)	(0.047)	(0.044)	(0.093)
Mean	.49	.31	.54	.35	.71
Panel C:			Dropout		
Covered in 1967 ×					
Treated Cohorts	-0.092	-0.137*	-0.014	-0.114	-0.041
	(0.057)	(0.078)	(0.061)	(0.070)	(0.058)
Mean	.19	.37	.14	.27	.06

Notes: NLS: Original Cohort - Young Men 1966-1981. Here we randomly drop observations based on the share of self-employed workers within occupation × race × parental education cells using March CPS 1967-1970 (Flood et al., 2023). The dependent variables are 'completed years of education', 'college attainment', and 'dropout before finishing high school' for Panel A, B, and C, respectively. We report the mean of all dependent variables among the control group and the standard deviation of completed years of education, before we randomly drop observations. Each column refers to a different sample. All regressions include 'Two-way FEs', namely occupation (parents) and age in 1966 (children) fixed effects; and 'Controls', including ethnicity, urban status, labor market size, southern region, parental education, family income, whether the family received welfare/public assistance, number of siblings, and library card availability. The definitions of these variables are in Table A1. We also include a set of dummies for each control variable to indicate whether it is missing or not. Significant at *** p<0.01, ** p<0.05, * p<0.1. Bootstrapped standard errors are clustered at the occupation level.

($20,854×0.059≈$1230.4) and cumulative additional earnings for our treatment group who was aged between 14 and 18 at the time of the reform, for an average exposure prior to high school completion of 3.12 years ($1230.4×3.12≈$3,839).

Second, we take estimates for an additional \$1,000 cumulative transfers exposure from previous studies in which the treated group was exposed during adolescence. In Bastian and Michelmore (2018), an additional \$1,000 cumulative EITC exposure (in \$2013) between the ages of 13 and 18 is estimated to increase the probability of high school graduation by 0.027 for black males (Table 3, column 5). Using the Bureau of Statistics CPI inflation deflator, this translates into a 0.0256 increase per \$1,000 cumulative exposure in \$2017. In Akee et al. (2010), the treated (control) cohorts are aged 9 (13) in 1993, hence around ages 12 (16) when transfers started in 1996 (see p.91 in Akee et al., 2010). There is therefore a difference in four years at \$4,000 annual exposure between the treated and pre-treatment cohorts, resulting in an additional \$16,000 cumulative exposure in \$2000 or \$23,016 in \$2017 using the Bureau of Statistics CPI inflation deflator. Taking the estimated effect per American Indian parent obtained by Akee et al. (2010) (Table 5, column 2) for poor households, who are arguably more likely to resemble our treated group given the lower living standards prevalent in the 1960s compared to the 1990s, the effect of an additional \$1,000 (in \$2017) casino transfer on the probability of high school completion is equal to $0.391 \div 23.016 \approx 0.017$. Note however that the estimated effect would be much smaller for this latter study if we instead considered the estimate for the overall American Indian sample (0.156, Table 4 column (2)) rather than that applying to poor households.

Finally, we multiply the estimated effects for \$1,000 cumulative transfers exposure by the additional income of 3.839 thousand \$ received by our post-treatment cohorts and obtain estimates of $0.0256 \times 3.839 \approx 0.098$ for black males based on Bastian and Michelmore (2018) estimates and $0.017 \times 3.839 \approx 0.065$ for poor American Indians based on Akee et al. (2010).