Minimum Wages, Education, and Inter-generational Inequality*

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February, 2024

Preliminary Draft

Abstract

Can equality-enhancing wage regulations initiate a virtuous cycle of social mobility? We study the effect of the 1966 Fair Labor Standards Act on the completed education and labor market outcomes of children whose parents worked in industries newly covered by mandated minimum wages. Exploiting variation across cohorts and industry in exposure to minimum wages, we find that minimum wages reduced racial inequality in completed education and in the skill content of jobs of the next generation. However we find a statistically insignificant effect on wage inequality—with no compensating differential improvement on a range of other outcomes. Importantly, black children exposed to the reform age 18 or below appear much less likely to migrate away from Southern states after 1966, suggesting that the minimum wage expansion may have failed to improve black intergenerational mobility in earnings by reducing push factors away from the South.

JEL Codes: D04, I24, J15, J38, O15.

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1 Introduction

A key channel through which inequality begets 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 inter-generational cycle of equal opportunities. A commonly used tool to reduce income inequality are mandated minimum wages. Despite controversy regarding the potential unintended consequences of minimum wage policies (Mincer 1976), Derenoncourt and Montialoux (2021) show that the Fair Labor Standards Act (FLSA) of 1966, which extended the 1938 FLSA act to new 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.[1] In this paper, we ask whether this large reduction in income inequality successfully reduced inequality in the next generation.

We first test whether the children of parents in industries newly covered by minimum wage regulation complete more education and have better labor market outcomes as a consequence. To do so, we exploit variation in parental exposure to minimum wage mandates by industry and child cohort in a difference-in-differences design. The 1966 FLSA extended the 1938 FLSA minimum wage coverage to the agricultural sector, hotels, restaurants, schools, hospitals, nursing homes, entertainment and other services. We 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 1938 reform. In addition, we exploit 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 in enrollment around this age.[2] Children who were aged 14 to 18 years old in 1966 are thus considered treated while those aged 19 to 24 act as controls.

We then test for heterogeneous effects of the minimum wage extension on children's completed education and labor market outcomes to shed light on its impact on inter-generational inequality. Heterogeneous treatment effects may arise either due to heterogeneous intensity of treatment, as some groups are over-represented in the industries newly covered by the 1966 FLSA, or because, for a given treatment intensity, some groups experience larger changes in outcomes, or both. These sources of heterogeneity may lead to rich, and poten-

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

[2] See Figure A1, which shows the school enrollment rate by age in the publicly available 1960 Population Census extract.

tially unexpected heterogeneity patterns. Therefore, in addition to testing for heterogeneous effects by characteristics disproportionately represented in the industries targeted by the 1966 FLSA (black, South, rural) using a conventional linear two-way fixed effect approach, we also explore heterogeneous treatment effects more systematically using causal forests (Athey, Tibshirani, et al. 2019).

We implement our research design using data from the 1979 National Longitudinal Survey of Youth, which interviewed young people every one to two years between 1966 and 1981. The timing of the survey is serendipitous in that it allows us to observe the parents' occupation and other pre-reform controls in 1966, hence just before the 1966 FLSA was implemented in February 1967. In the main analysis, we measure labor market outcomes in the last wave, which took place in 1981 when the youngest individuals in our sample were 29 years old and therefore well into their working life.

We find that the 1966 minimum wage reform increased completed years of education by 0.57 years, on average. This effect combines impacts at both ends of the completed years of education distribution, as we find a 7.0 percentage points decrease in dropout prior to completing 12th grade as well as a 8.1 percentage points increase in college attainment. Our heterogeneity analysis (whether "traditional" or using causal forests) shows that the magnitude of the education effect is much higher among black children than among white children. In terms of years of education, linear two-way fixed effect (causal forest) estimates indicate that the effect among black children was an additional 1.2 (0.59) years, while it was only 0.32 (0.23) years among white children, and the dropout reduction among black children is about three times as large as that among white children. However, we find more homogeneous effects by race on college attainment. Overall, the 1966 reform contributed to racial inequality reduction in years of education by 19% among the treated cohorts compared to a counterfactual in which we fix the post-treatment difference between treated and control industries at its average level across pre-reform cohorts.

Turning to children's labor market outcomes, we consider the effect on employment as well as on a range of pecuniary and non-pecuniary outcomes which could act as substitutes: wages, whether employed in the public sector, fringe benefits, job characteristics such as indicators of job "prestige", and job satisfaction. We find that parental exposure to the 1966 minimum wage extension age 18 or below had no effect on employment, fringe benefits or public sector employment for either black or white children, and the effect on wages is not large enough to be statistically significant. Overall, we conclude that the educational gains of children whose parents benefited from the 1966 FLSA do not appear to have improved their

labor market outcomes. This is true even for the large educational gains observed among black children, so that the initial reduction in racial inequality in earnings did not succeed in starting a self-sustaining equalizing inter-generational cycle. Interestingly, we also observe improvements in job satisfaction among white second-generation beneficiaries of the 1966 FLSA, but a worsening among Blacks, as may be expected from the fact that their large gains in educational outcomes are not accompanied by an improvement of labor market outcomes. Further analysis suggests that reduced geographical mobility away from Southern states specifically may explain the lack of dynamic racial inequality reduction.

Our results are robust to a number of tests. We first test for parallel pre-treatment trends through a placebo test splitting control 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 analysing changes birth cohort-by-birth cohort also supports this conclusion. Second, we test the robustness of our findings to alternative definitions of exposure to the 1966 FLSA. Indeed, we observe parental occupation but not parental industry and so we have to match occupations to industries based on 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 (namely, the share of employees in treated industries by occupation) and excluding occupations where the share of workers in treated industries is more intermediate (i.e., between 20 and 80%, where the match error rate is likely higher) from the sample.[3] 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. Finally, we find qualitatively similar results when using a linear two-way fixed effects specification and random forests, which serves as a specification check for our main estimates.

This paper relates to three strands of literature. First, it contributes to the minimum wage literature, which, in addition to investigating the effects of minimum wage mandates on the labor market outcomes of working-age adults, has also extensively documented their effect on child health outcomes (see, e.g., Bailey et al. 2021; Card, Katz, et al. 1993; Cengiz et al. 2019; Dube 2019; Dube et al. 2010; Engbom and Moser 2022; Neumark 2023). A few studies have

[3]We also show that the matching varies little across states.

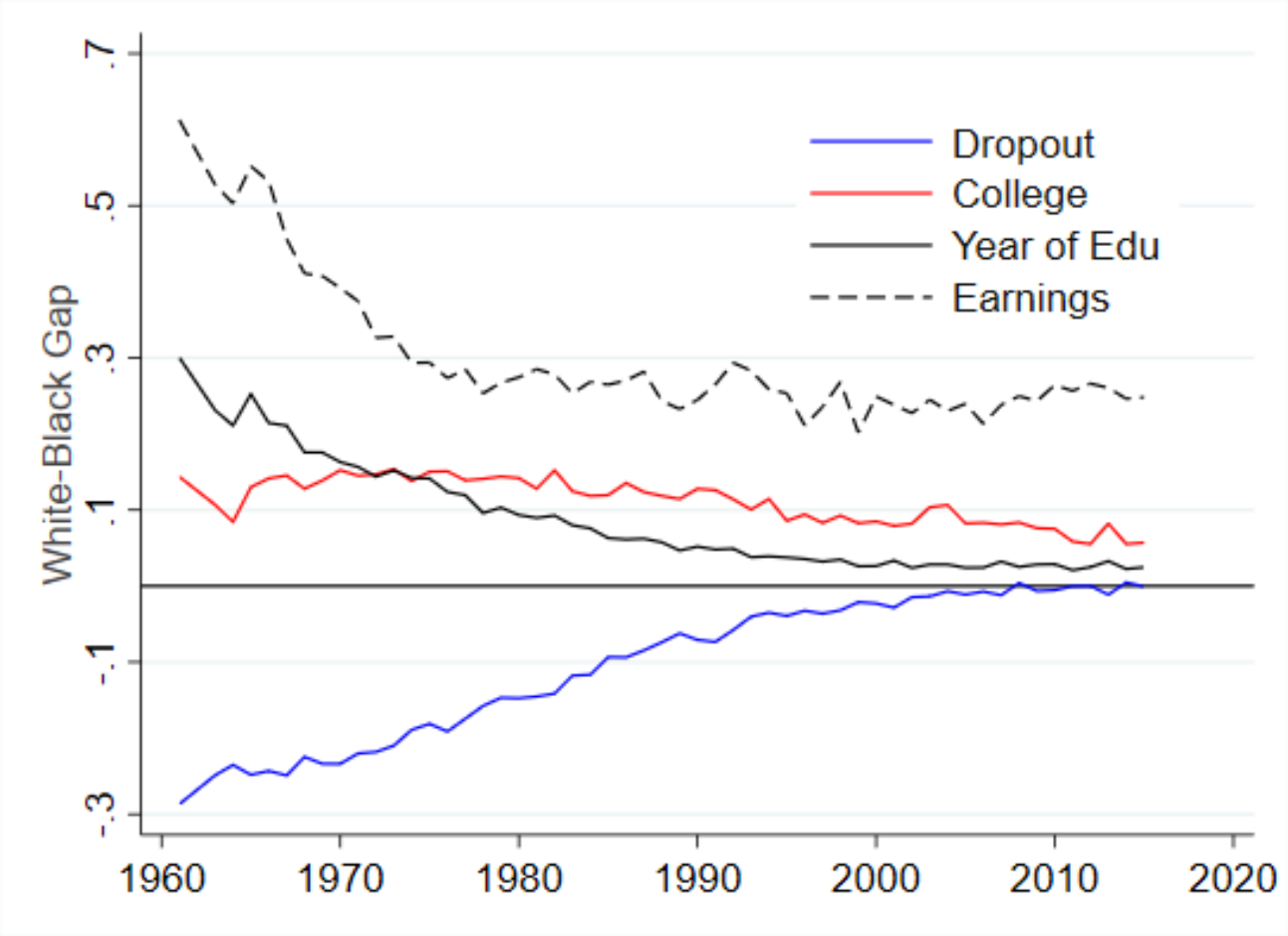
recently also estimated the instantaneous (i.e., same-year) effect of changes in the mandated value of minimum wages on changes in high school dropout (Smith 2021) and cognitive test scores (Regmi 2020), and have reached mixed conclusions. Smith (2021) finds that a higher minimum wage reduces high school dropout but Regmi (2020) concludes that increasing the minimum wage reduces children's math scores. Our contribution to the literature on the effect of mandated minimum wages is to provide novel estimates of their effect on long-term inter-generational human capital and labor market outcomes.

Second, this study relates more broadly to the literature relating children's educational outcomes to parental income, which except for Smith (2021) and Regmi (2020), has focused on alternative sources of changes in income such as lottery wins (Bleakley and Ferrie 2016; Bulman et al. 2021), housing prices (Lovenheim 2011) and government transfers (Akee et al. 2010; Milligan and Stabile 2011; Braga et al. 2020).

Third, our work relates to the literature studying racial inequality in socioeconomic outcomes. Large persistent racial inequality in education and incomes (see Figure 1) 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 and Montialoux 2021; Levine and Ritter 2022, and literature reviewed therein). Descriptively, since the 1960s, there have been sizeable reductions in black-white income inequality and pre-college educational inequality (Figure 1), which have been explained by, among others, federal anti-discrimination activities (Donohue and Heckman 1989; Heckman and Payner 1989), the Voting Right Act of 1965 (Aneja and Avenancio-Leon 2019), improved school quality (Card and Krueger 1992), income transfers (Butler and Heckman 1977), and minimum wage mandates (Derenoncourt and Montialoux 2021). Despite this progress, racial inequalities persist and recent work has emphasized barriers to black social mobility, i.e. that African-Americans have had systematically lower earnings even *conditional on parental earnings* (Chetty et al. 2020; Collins and Wanamaker 2022). Our contribution is to document how an exogenous move up in the distribution of parental earnings fails to translate into better labor market outcomes in the next generation despite achieving a reduction in human capital inequality | as would be predicted by studies finding that African-American men have enjoyed lower returns to education (Bayer and Charles 2018).

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. Section 5 investigates heterogeneous effects on educational outcomes and implications for racial inequality. Section 6 discusses mechanisms through

Figure 1: Racial Gap in Education and Wage



Notes: Data source is the Current Population Survey micro file from 1961 to 2016. The sample includes all black and white workers employed in industries covered in 1966 or 1938 reform from 25 to 65 years old.
This figure shows an unadjusted racial gap in educational outcomes and wages (not controlling for any observable characteristics).

which mandated minimum wages may have contributed to improved second-generation education outcomes.Section 7 estimates reform impacts on second-generation labor market outcomes. Section 8 concludes.

2 Data

Our main source of data is the National Longitudinal Survey of Youth 1979, which we use to analyse the effects of the 1966 reform on the second generation's education and labor market outcomes. We use the 'Original Cohorts - Young Men (NLSY79YM)' sample (Bureau of Labor Statistics, U.S. Department of Labor 1992) for the following two reasons. One key data

requirement for our analysis is that we need pre-determined parental work information to identify whether individuals' parents worked in treated or control industries before the 1966 FLSA came into force in 1967. We have this information only for the NLSY79YM sample. The Young Women sample was first surveyed in 1967, so we cannot identify pre-reform parents' working industries, leading to potential endogeneity. In constraining our analysis to males, the data however allows us to focus on the gender group for whom racial gaps in education and income are the most persistent, as documented in (Chetty et al. 2020).

The NLSY79YM includes 5225 males who were 14 to 24 years old in 1966. It is a panel survey started in 1966 and which lasted until 1981.[4] Parental industry/occupation and baseline household characteristics are observed in 1966 while the dependent variables come predominantly from the 1981 wave. The details of the definition of each variable are provided in Table A1. We compare individuals whose parents worked in industries covered in 1966 versus those covered in the original 1938 FLSA. Individuals whose parents worked in all other industries are dropped from the analysis. Also, since we are specifically examining racial inequality between Blacks and Whites, individuals of other races, which comprise 1% of the full sample, are excluded. This results in a main regression sample of 3876 individuals.

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 NLSY79YM collects information on parental occupation when the child was age 14, but not parental industry.[6] We therefore need to match parental occupations to industries. We do so using Census 1960 microdata, in which we calculate, for each occupation, the share employed in industries covered in 1967 (% 1967), covered in 1938 (% 1938) and covered in neither (% others). We then define treatment status for each occupation using the following equation:

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

This procedure leads to a 79-87% correct match rate (depending on whether we weight occupations by their size), and findings are robust to alternative approaches, as discussed in Section 4.2).

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

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

[6]The exact survey question is 'OCCUPATION OF FATHER OR HEAD OF HOUSEHOLD WHEN R [the respondent] WAS AGE 14'? 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

We also use the Current Population Survey microdata to estimate the effect of the 1966 reform on first-generation wages, replicating estimates from Derenoncourt and Montialoux (2021). Finally, we use IPUMS' 1960 Census 1 percent extract, mostly to match parental occupations to industries and construct occupational indexes to match with the occupation of the second generation.

3 Empirical Strategy

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

The younger (treated) cohorts are between 14 and 18 years old in 1966, while the older (control) cohorts are between 19 and 24 years old in 1966. The 18 years old cutoff comes from the legal minimum age of leaving school, which is between 16 and 18 years old depending on state. For our high school dropout outcome, individuals aged 19 and above in 1966 should not be affected. For our other outcomes (completed years of education and college education), individuals aged 19 and above might still benefit (and hence bias our estimates towards zero), but these benefits are likely to be marginal.[7]

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 industries, 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 industries versus control industries (see Table A2).[8]

we estimate the following difference-in-differences model with two-way fixed effects:

$$y_{ijc} = \alpha + \beta_1 \text{Covered 1967}_j \times \text{Treated Cohorts}_c + \sum_{j=2}^{161} \delta_j \text{OCC}_j + \sum_{c=2}^{11} \delta_c \text{Cohort}_c + \mathbb{X}_i' \Gamma + \varepsilon_{ijc} \quad (2)$$

where y_{ijc} denotes the education outcome of individual i in 1981. The dummy variable Covered 1967_j equals 1 if the individual's father or, if not living with the child, the household

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

[8] In Figure A2, we also depict cohort-by-cohort differences in years of education in the treated vs. 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.

head, worked in an industry covered in 1967 when the individual was 14 years old and 0 if they worked in an industry covered in 1938. Treated Cohorts$_c$ equals 1 if the individuals' age c is equal or less than 18 in 1966, and 0 otherwise. δ_j and δ_c are occupation and year fixed effects, respectively. We control for the following baseline (before 1967) individual-level characteristics contained in the vector $\mathbb{X}_i$: ethnicity, number of siblings; family income, whether received welfare or public assistance, whether library card was available in the household; rural location and labor market size of the region the individual lived in; and whether the individual lived in southern states.[9] However, including control variables does not affect the coefficient of interest, suggesting negligible selection into treatment on observable characteristics. We report standard errors clustered at the occupation level to allow the arbitrary correlation within industries Abadie et al. (2023).

4 Second-Generation Education Results

4.1 Average Treatment Effects

The two first columns of Table 1 report estimates for completed years of education. Our preferred specification, which controls for predetermined household- and labor market characteristics (column 2) indicates that the average years of education for children whose parents worked in industries covered in the 1966 reform is 0.57 years higher than those whose parents worked in control industries for treated cohorts compared to control cohorts (or around 20% of a standard deviation). The estimate is only slightly larger when controls are excluded (column 1). As shown in the rest of the table, gains in education come both from a reduction in drop out (by 7 percentage points) and an increase in college education (by 8.1 percentage points).

4.2 Robustness Checks

Quality of the matching. Based on census data, 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). This is not surprising since, as shown in Figure A3, occupations are concentrated at the two extremes of the distribution of the share of individuals working in treated industries. For most occupations, the share of workers in treated industries is indeed

[9]See Table A1 for definition of these variables.

Table 1: Impact on Second Generation's Education Outcomes

D.V.	Year of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967	0.641**	0.569**	0.089**	0.081*	-0.077**	-0.070**
× Treated Cohorts	(0.268)	(0.269)	(0.039)	(0.042)	(0.031)	(0.031)
Num.Obs.	3876	3876	3876	3876	3876	3876
R2	0.227	0.331	0.189	0.269	0.125	0.210
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Mean D.V.	12.99		0.46		0.22	
S.D. D.V.	2.86		0.50		0.41	

Notes: 'D.V.' refers to the dependent variable. 'S.D.' refers to the standard deviation. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. The definition of these variables are in Table A1. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

either more than 80% or less than 20%.[10]

Still, we carry out several robustness check 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 A4 shows that the results after restricting the sample are similar to the main results in Table 1 with a larger magnitude of coefficients. Also, we repeat the event study analysis with this restricted sample, which shows that there is no indication of diverging trends prior to treatment (Figure A5). 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 A5 suggest the same conclusion as the baseline results, with more precisely estimated and slightly larger estimated effects.

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 A6).

[10]Since we do not observe in which state the individual lived in 1966, we cannot match occupations to industries at state-level in the NLSY79YM. However, we check whether an occupation matches to different industries in different states and find that it largely does not. Figure A4 shows that occupations are defined as having the same treatment status in over 80% states as in the country-level matching.

Effects by parents' education and income. The minimum wage reform should not affect children whose parents' earnings are at the top of the distribution. 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 indirect effects should be weak to non-existent at the top of the income distribution. To check that the data align with this prediction, we estimate Equation 2 for four sub-samples corresponding to the quartiles of a proxy for parental earnings. The NLSY79YM does not collect parental education levels or individual parental wages. We instead proxy for parental wages using the parent's occupational education score, which measures the percentage of people who had completed one or more years of college in each occupation. In the Census 1960, the share of workers earning less than US$1500 per year by quartile of occupational education score is, respectively: 34.47%, 32.9%, 28.51% and 16.21%, indicating that our proxy effectively identifies the top quartile as a group who stands to benefit a lot less from a wage floor than the others, but does not discriminate very well between differential exposure to the reform among the three lower quartiles. Table A7 reports our results. For all three outcomes, namely the years of education, 'college attainment', and 'dropout', there is no effect in the top quartile. The relative magnitude of the effect for the four quartiles is as expected except for the third quartile. Since our quartiles are imperfect proxies for very low wages, it is possible that a larger share of parents in the third quartile benefit from the minimum wage reform, and/or the income *elasticities* may increase with quartile.

5 Treatment Effect Heterogeneity and Racial Inequality in Education Outcomes

In this section, we first examine treatment effect heterogeneity using a linear two-way fixed effect approach for specific sub-samples or allowing for interactions between treatment status and observable characteristics. We then use a non-linear machine learning method ('causal forest') as a robustness check.

5.1 Heterogeneity with the Two-Way Fixed Effects Approach

To explore treatment effect heterogeneity, we estimate Equation 2 using different sub-samples. We focus on three potential sources of treatment effect heterogeneity: i) ethnicity; ii) living in the US South; and iii) rural location, since African Americans are over-represented in the industries impacted by the 1966 FLSA and since these industries are more prevalent in

Table 2: Treatment Effect Heterogeneity with Linear DID Approach

Sample	Black	White	South	non-South	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			D.V.: Year of Education			
Covered in 1967	1.202***	0.316	0.916***	0.214	-0.278	0.737***
X Treated Cohorts	(0.378)	(0.316)	(0.239)	(0.301)	(0.370)	(0.183)
Mean	11.71	13.40	12.25	13.45	13.31	12.64
S.D.	2.74	2.78	3.10	2.60	2.72	2.97
Panel B:			D.V.: College			
Covered in 1967	0.054	0.071	0.104*	0.070	-0.007	0.111***
X Treated Cohorts	(0.058)	(0.052)	(0.055)	(0.044)	(0.052)	(0.030)
Mean	0.28	0.52	0.38	0.52	0.51	0.41
S.D.	0.45	0.50	0.48	0.50	0.50	0.49
Panel C:			D.V.: Dropout			
Covered in 1967	-0.132**	-0.048	-0.154***	-0.008	0.042	-0.110**
X Treated Cohorts	(0.066)	(0.031)	(0.039)	(0.036)	(0.045)	(0.043)
Mean	0.40	0.16	0.32	0.16	0.19	0.25
S.D.	0.49	0.37	0.47	0.36	0.39	0.43
Num.Obs.	1040	2836	1597	2279	1889	1987
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effects. 'Controls' include binary indicators for ethnicity, urban location, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. The definition of these variables are in Table A1. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Southern states and in rural areas. Table 2 presents the results of sub-sample estimates: each column refers to a sub-group, and each panel refers to a dependent variable.

Columns (1) and (2) report estimates by ethnicity. Panels A and C show that the effect of the 1966 reform on years of education and drop out is significantly different from zero for Blacks but insignificant for Whites. Also, the magnitude of the effect for Blacks is much higher (around three times more) than Whites. However, Panel B suggests much more homogeneous effects on college attainment across ethnicities. As expected, treatment effects are also much more pronounced, and only statistically significant, in the South and in rural areas.

5.2 Heterogeneity Analysis using Causal Forests

We now turn to a causal forest analysis of treatment effect heterogeneity. This serves as a useful robustness check since (i) we relax the functional form assumptions made in the two-way linear specification (thus, e.g., allowing for complex interactions between different sources of heterogeneity), (ii) this approach has built-in validation checks, and (iii) the procedure provides doubly-robust treatment effect estimates.

Overview of Method. First, we use a regression forest to 'residualize' the treatment (defined as the interaction variable 'Covered in 1967 $\times$ Treated Cohorts') and the three education outcomes (completed years of education, 'college attainment' and 'dropout'). I.e., we purge the treatment and outcome variables from variation coming from: occupation and age fixed effects, ethnicity, south, rural, and labor market size. Second, we estimate a causal forest allowing several potential sources of treatment effect heterogeneity, using the residuals from the first step as the outcome. In addition to black, south and rural, we also allow treatment effect heterogeneity by occupation dummies, age, labor market size, family income, whether the household receives subsidies, library card availability and the number of siblings. The causal forest is composed of 2000 individual trees. Each tree contains a random 50% sample of the dataset, and the leaves are partitioned according to the values of variables used in this step. The partitioning algorithm aims to maximize treatment heterogeneity between these leaves while minimizing treatment effect variance within each leaf by combining the values of these variables (Athey and Imbens 2016).

To ensure robustness and unbiased performance, the algorithm is fine-tuned according to cross-validation without any researcher input.[11] causal forest procedure.

Causal forest fit. Table 3 Columns (1) and (2) present the results of omnibus tests following Chernozhukov et al. (2018) for a non-experimental setting and using the procedure by Tibshirani et al. (2022). These two columns show the coefficient of a linear regression of the predicted individual treatment effect on the predicted average treatment effect (coefficient of 'mean forest prediction') and the deviation of predicted individual treatment from the predicted average treatment effect ('coefficient of differential forest prediction'). These two

[11] we use the grf package in R (Tibshirani et al. 2022) to build the causal forest, and all parameter settings are selected based on cross-validation. We use 50% of the sample to grow each tree. The splitting structure of the trees is determined on a 50% sub-sample of the tree sub-sample, after which the other 50% populates the tree to estimate the treatment effects.

Table 3: Causal Forest Fit & Average Treatment Effects

	mean forest prediction (1)	differential forest prediction (2)	Doubly Robust Average Treatment Effect (3)	Plain DID (4)
Panel A:		**Year of education**		
	1.006*	0.959*	0.273***	0.569**
	(0.732)	(0.743)	(0.087)	(0.269)
Panel B:		**Dropout**		
	0.894*	0.984*	-0.048***	-0.070**
	(0.551)	(0.661)	(0.011)	(0.031)
Panel C:		**College**		
	0.938*	0.564	0.054***	0.081*
	(0.676)	(0.745)	(0.017)	(0.042)

Notes: Columns (1) and (2) show the results for the omnibus test inspired by equation 3.1 in Chernozhukov et al. (2018) modified to the observational setting and implemented through the test calibration function from the grf library in R. Column 3 shows the Augmented Inverse-Propensity weighted (AIPW) Average Treatment Effects. Column 4 shows the linear DID estimations from Table 1. Standard errors are in parentheses. Significant at *** p<0.01, ** p<0.05, * p<0.1. Following Tibshirani et al. (2022), the significance levels in columns (1) and (2) are for the one-sided tests.

coefficients should be 1 if the forest captures both average and heterogeneous treatment effects well. Panel 1 and 2 of Table 3 show that both two coefficients are very close to 1 in magnitude (the confidence interval includes 1 as well) and significantly different from zero at 10% significance level using completed years of education or 'dropout' as the outcome. Panel 3 however shows that, for the 'college attainment' outcome, performance is not very good and therefore we will focus on the two first outcomes in the remaining causal forest analysis.

Average treatment effects Column (3) of Table 3 shows the Augmented Inverse-Propensity weighted (AIPW) Average Treatment Effects based on the causal forest. The AIPW is a doubly robust estimator that combines propensity score weighting and outcome regression modelling. The estimator is consistent provided one of these two variables (treatment status or outcome) are modelled correctly. AIPW estimates lead to similar conclusions as the 'plain' difference-in-differences estimates, although the magnitude of AIPW estimates is smaller than linear DID estimates.

Table 4: Covariate means across tertiles of CATEs (Selected Variables)

	First Tertile (N=1170)		Third Tertile (N=1169)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Panel A:			D.V.: Year of Education			
CATE	0.058	0.101	0.547	0.170	0.490	0.000
Black	0.221	0.415	0.368	0.483	0.147	0.000
South	0.159	0.366	0.706	0.456	0.546	0.000
Rural	3.509	2.476	6.636	2.151	3.127	0.000
Panel B:			D.V.: Dropout			
CATE	-0.099	0.032	-0.011	0.016	0.088	0.000
Black	0.400	0.490	0.236	0.425	-0.164	0.000
South	0.818	0.386	0.060	0.238	-0.758	0.000
Rural	7.103	1.633	2.663	2.031	-4.440	0.000

Notes: 'D.V.' refers to the dependent variable. The table shows covariate means for the first and third tertile of the sample based on the estimated CATEs. The definition of variables are in Table A1. See Table A8 for all variables.

What observable characteristics drive treatment effect heterogeneity? We estimate individual Conditional Average Treatment Effects (CATEs), i.e., the predicted average treatment effect for an individual with a given set of observable characteristics, rank individuals by their CATE, and then compare the characteristics of individuals found in the lowest and highest CATE groups (Table 4). Given the sign of the estimates, the largest educational gains are found in the top tertile for years of education and in the lowest tertile for dropout. Regarding completed years of education (dropout), there are 14.7 (16.4) percentage points more Blacks in the largest gains tertile. Similar to the linear difference-in-differences, the random forests estimates indicate that black children, children in Southern states and in more rural areas benefitted more than white children and children outside the South and in less rural settings.[12]

To further illustrate treatment effect heterogeneity by covariates, we estimate AIPW using sub-samples and compare them with plain DID results in Table 2. Table 5 shows that the AIPW estimates are consistent with plain DID estimates, although the magnitude of AIPW

[12]Regarding other observable characteristics, Table A8 shows the means of all variables across tertiles of CATEs. Consistent with the pattern for 'rural', individuals in the higher effect group come from smaller labor market size areas compared to those in the lower effect group. Regarding family income, individuals from lower income families are more likely to be found in the higher effect group. We also observe treatment effect heterogeneity by whether received subsidies regarding completed years of education. In terms of family characteristics, the higher effect group has more individuals with more siblings and no library card availability for both two outcomes.

Table 5: AIPW by Sub-samples

Sample	Black (1)	White (2)	South (3)	non-South (4)	Urban (5)	Rural (6)
Panel A:			Year of education			
AIPW	0.589***	0.231***	0.656***	0.089	0.141	0.403***
	(0.148)	(0.096)	(0.159)	(0.103)	(0.129)	(0.115)
Panel B:			Dropout			
AIPW	-0.090***	-0.042***	-0.117***	-0.015	-0.016	-0.079***
	(0.032)	(0.012)	(0.018)	(0.014)	(0.017)	(0.015)

Notes: 'D.V.' refers to the dependent variable. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

estimates is generally smaller than plain DID estimates. The AIPW of the black sample is still more than twice as large as the AIPW of the white sample regarding both completed years of education and 'dropout'. Similarly, AIPW estimates show larger treatment effects among southern states and rural areas.

5.3 Racial Inequality

A key finding from our analysis of treatment effect heterogeneity is that the effect of the 1966 reform on black children is much larger in magnitude than that for white children. But to what extent did the 1966 reform reduce the racial gap? To answer this question, we use a counterfactual analysis to investigate the unconditional racial gap in completed years of education by cohort, using the same method as Derenoncourt and Montialoux (2021) for first-generation income inequality.

There are two underlying assumptions to build the counterfactual racial gap. In the absence of the 1966 reform, the racial gap among treated cohorts would have evolved similarly to that of control cohorts; second, the gap between treated and control cohorts among black children would have evolved similarly to white children. Under these two assumptions, the average racial gap can be written as follows:

$$\text{Total Racial Gap} = s_w^c \bar{X}_w^c + s_w^t \bar{X}_w^t - (s_b^c \bar{X}_b^c + s_b^t \bar{X}_b^t) \tag{3}$$

where s_w^c, s_w^t, s_b^c and s_b^t denote the share of worker in control and treated industries for white and black, respectively ($s_w^c + s_w^t == 1$ and $s_b^c + s_b^t == 1$). $\bar{X}_w^c$, $\bar{X}_w^t$, $\bar{X}_b^c$ and $\bar{X}_b^t$ is the average

completed years of education among control and treated industries for white and black, respectively. So, the first two terms measure the average completed years of education of white children, while the last two terms measure the average 'years of education of black children. Moreover, the racial gap among control industries is $G^c = \bar{X}^c_w - \bar{X}^c_b$; among treated industries $G^t = \bar{X}^t_w - \bar{X}^t_b$; and the gap between control and treated industries among Blacks is $G^{ct}_b = \bar{X}^c_b - \bar{X}^t_b$. Then, we can re-write the total racial gap in Equation 3 as:

$$\text{Total Racial Gap} = s^c_w G^c + s^t_w G^t + G^{ct}_b (s^c_w - s^c_b) \tag{4}$$

We plug in actual (observed) parameters into Equation 4 to calculate the actual racial gap for each cohort (the cohorts in the sample correspond to those born from 1942 to 1952).

Next, we construct the counterfactual racial gap among treated industries ($G^t_{\text{counterfactual}}$) for each cohort. Regarding control cohorts (1942 to 1947), $G^t_{\text{counterfactual}}$ is the same as the actual total racial gap G^t_{actual}. Regarding the treated cohorts (1948 to 1952), we use the actual racial gap among treated industries minus the actual average pre-trend of the difference between the racial gap among control and treated industries: $G^t_{\text{counterfactual}} = G^t_{\text{actual}} - \frac{1}{6}\sum_{k=1942}^{1947}(G^c_{\text{actual}} - G^t_{\text{actual}})$. To calculate the counterfactual total racial gap, we replace G^t in Equation 4 by $G^t_{\text{counterfactual}}$ and all other parameters are the same as when calculating actual total racial gap.

Figure 2 plot the actual racial gap (black line) and the racial gap in the absence of 1966 reform (blue line), respectively. It shows that the average counterfactual racial gap among the treated cohorts is 1.6 years, while the average actual racial gap is 1.3. Therefore, the 1966 reform contributed to a reduction of around 19% of the racial gap in the completed years of education among treated cohorts.

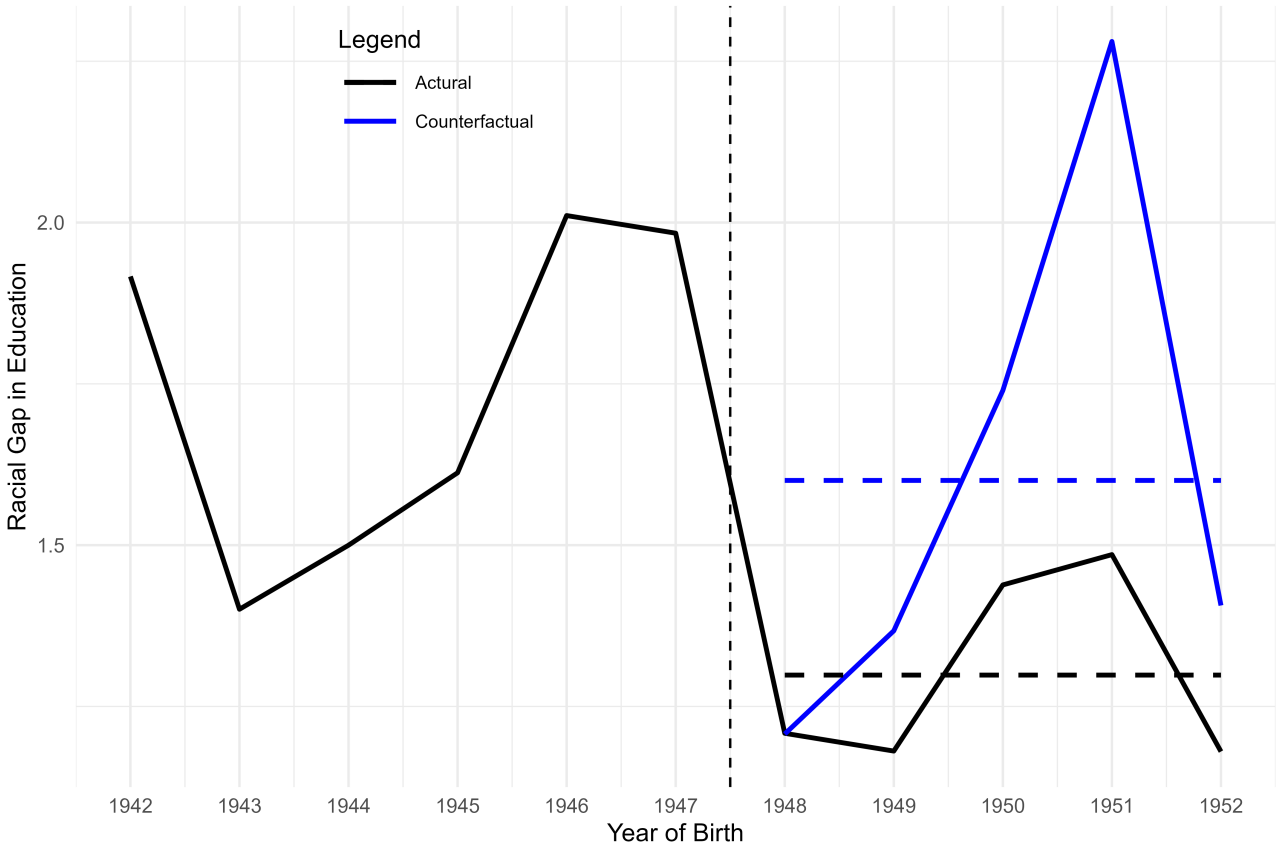
6 Potential Mechanisms

Several channels may lead to the positive effects on the minimum wage policy on second generation's education outcomes and to the treatment effect heterogeneity we observe. Here we focus on two channels: i) the income benefits from the 1966 reform on the first generation; and ii) the effect on the second generation's education aspirations, which may in part be due to (i), but may also arise independently.

6.1 The Effect of the 1966 Reform on the First Generation's Wages

First, we investigate to what extend the 1966 reform affect first-generation wages and whether it is different for black and white workers. Since these findings essentially replicate the

Figure 2: Actual versus Counterfactual Racial Gap in Education



Notes: This figure shows an un-adjusted (not controlling for any observable characteristics) racial gap in years of education by cohort. The red vertical line split cohorts into two categories: untreated (left-hand-side) and treated (right-hand-side).

estimates in Derenoncourt and Montialoux (2021) with only a few minor alterations to match the specifications we use in the rest of the analysis, we only present these briefly. We use the same data (CPS) and the same setting as Derenoncourt and Montialoux (2021). 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 20 years old in 1972.

Table 6 presents the results. First, using the full sample, there are positive effects on first generation's wages, which are statistically different from zero at least at 10% significance level. Moreover, the comparison between Columns (1) and (2) suggests that the magnitude of

Table 6: The Effect on First Generation's Wages

Sample	Full Sample				Only Household Heads			
Ethnicity	Black	White	Black	White	Black	White	Black	White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Covered in 1966								
X 1967-1972	0.095***	0.054**	0.070***	0.049*	0.085***	0.043**	0.059**	0.040*
	(0.022)	(0.023)	(0.023)	(0.023)	(0.023)	(0.020)	(0.023)	(0.019)
X 1973-1980	0.078*	0.036	0.039	0.030	0.049	0.011	0.011	0.007
	(0.037)	(0.042)	(0.039)	(0.042)	(0.042)	(0.038)	(0.041)	(0.038)
'Chow Test'								
X 1966-1970								
Prob > chi2	[0.020]**		[0.125]		[0.017]**		[0.284]	
X 1971-1980								
Prob > chi2	[0.235]		[0.690]		[0.286]		[0.907]	
Observations	37,768	370,053	37,026	348,605	24,154	246,463	23,682	232,863
R-squared	0.494	0.596	0.528	0.606	0.467	0.462	0.507	0.479
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
South & Rural	7	7	✓	✓	7	7	✓	✓

Notes: The dependent variable is log annual earnings. 'Controls' 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. 'Prob > chi2' refers to the 'Chow test' that do coefficients of 'Covered in 1966 X 1967-1970' vary between groups? Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

treatment effects among Blacks is significantly larger than Whites. After controlling for South and rural, Columns (3) and (4) show that the difference becomes smaller (and insignificant post 1972). However, the point estimates still indicate a 43% larger effect for Blacks than Whites for the relevant period for our analysis (1967-1972). Second, since treatment status is based on the occupation of the household head, we also present results restricting the sample to household heads in Columns (5) to (8), leading to qualitatively similar conclusions.

6.2 The Effect of the 1966 Reform on the Second Generation's Aspirations

A second potentially important channel of differential effects among black and white children is the effect it may have had on children's aspirations. Guyon and Huillery (2021) find that pupils from socially disadvantaged families undervalue their abilities, and lower aspirations

Table 7: Impacts on Education Goals in 1967

Sample	Black (1)	White (2)	South (3)	non-South (4)	Urban (5)	Rural (6)
Covered in 1967	1.189***	0.140	0.712***	0.070	0.208	0.301
X Treated Cohorts	(0.447)	(0.216)	(0.271)	(0.203)	(0.476)	(0.212)
Mean	12.87	14.24	13.15	14.39	14.35	13.44
S.D.	3.35	2.95	3.49	2.73	2.87	3.27
Num.Obs.	953	2595	1481	2067	1701	1847
R2	0.414	0.362	0.465	0.315	0.376	0.372
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is the education goal in 1967. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effects. 'Controls' include binary indicators for ethnicity, urban location, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. The definition of these variables are in Table A1. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

for education are correlated with poorer educational outcomes. We then test whether improved parental earnings directly or indirectly impacted children's education goals in 1967, separately for black and white children. Columns (1) and (2) of Table 7 show a considerable difference in the treatment effect between ethnic groups, with black children found to increase their intended completed number of years of education by 1.19 years, whereas the estimate is close to zero and insignificant for white children. A similar, although less marked pattern of heterogeneity applies to children in- and outside the South, whereas the effect on aspirations is statistically insignificant and small in magnitude in both rural and urban areas as a whole.

Finally, we check that differences in treatment effects between groups, and especially between Blacks and Whites, are not simply due to differences in household head gender composition. Indeed, treatment status is assigned based on the household head's occupation if the father is absent or not employed. And the 1960 Census data indicates that there are 24.8% black families with female household heads compared to only 14.8% of white families. The NLSY79YM dataset does not provide information on the gender of the household head. However, it provides information on whether children only live with their mothers. Table A9 shows that, for all three outcomes and different samples (full, only Black and only White), results excluding individuals who only live with their mother suggest the same conclusion that there is a large difference in the treatment effects between Blacks and Whites regarding

completed years of education and 'dropout'. It therefore suggests that differences in the gender of the household head is not the driver of treatment effect heterogeneity.

Overall, our findings in this subsection suggest that larger first-generation income benefits from mandated minimum wages contributed to larger second-generation educational gains. Interestingly, however, the exact way in which these income benefits translated into educational gains varies across groups, with black children revising upwards their educational goals much more strongly than other groups where large educational gains are observed.

7 Second-Generation Later Life Outcomes

7.1 Labor Market Outcomes

Using the same research design (industry-by-cohort DID), we estimate the effect of the 1966 reform on the labor market outcomes of children whose educational attainment stood to benefit from parental eligibility for minimum wages.[13]

Column (1) of Table 8 indicates that there is no effect on the probability of being employed. There is also no treatment effect heterogeneity by race (as captured by the interaction term between the treatment variable and ethnicity). Results in Column (2) report estimates for the logarithm of wages. The point estimates are sizeable and positive, but too imprecisely estimated to be significant. However, it is noteworthy that the treatment effects are very similar for second-generation Blacks and Whites despite previous sections having shown much larger effects on the educational outcomes of black children.

We then test for changes in non-wage impacts such as job stability and prosocial value as proxied by working in the government sector, and other measures of job quality. We find no effect on the probability of working in the public sector, either for Blacks or for Whites. Concerning fringe benefits, in general, the 1966 reform did not improve outcomes for second-generation beneficiaries, and, if anything, marginally statistically significant estimates indicate that it negatively affected Blacks compared to Whites in the case of pecuniary benefits. Finally, in Column (6), it is shown that the 1966 reform makes second-generation Whites more likely to say that they would choose the same job if they could select any job, while the effect for Blacks is significantly lower and, in fact, the total effect on this measure of job satisfaction among Blacks is *negative* and significantly different from zero—as would be expected from

[13]See Table A3 for placebo tests for these outcomes. These indicate no significantly differential trends in 1966 FLSA vs. 1938 FLSA parental industry between cohorts aged 22-24 vs. 19-21 in 1966, except for the "public sector employment" outcome.

Table 8: Effects on Labor Market Outcomes

D.V.	Employed	Log wage	Gov	Fringe Benefits		Want the
				non-pecuniary	pecuniary	same job
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967	0.016	0.110	0.010	0.028	0.012	0.189***
X Treated Cohorts	(0.015)	(0.199)	(0.033)	(0.031)	(0.034)	(0.043)
DID X Black	-0.002	-0.015	0.025	-0.115	-0.150*	-0.369***
	(0.045)	(0.327)	(0.081)	(0.081)	(0.089)	(0.133)
'Wald Test'						
Pr(>Chisq)	[0.467]	[0.856]	[0.847]	[0.342]	[0.118]	[<0.001]***
Num.Obs.	2426	1970	2352	1990	1990	2336
R2	0.184	0.318	0.152	0.167	0.183	0.149
Mean	0.97	7.99	0.16	0.29	0.80	0.40
S.D.	0.17	1.98	0.37	0.24	0.28	0.49
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effects. 'Controls' include binary indicators for ethnicity, urban location, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. The definition of these variables are in Table A1. **Fringe Benefits**: non-pecuniary fringe benefits include training/educational opportunity, profit sharing, stock option, free/discounted meals, free/discounted merchandise, and flexible work hour; while pecuniary fringe benefits include insurance coverage away from job, life insurance coverage away from job, retirement program, paid sick leave, and paid vacation. 'Wald Test' is the test of H0: total effect for Blacks equals 0. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

the much larger increases in education among Blacks that are not matched by significant wage and non-wage benefits.

Recalling that the larger educational gains of black children we identify come mostly from larger reductions in high school drop out, our results echo the finding that, since 1940, increased educational attainment has benefited black men more at the top of the income distribution than at the bottom (Bayer and Charles 2018).

7.2 Effect on Type of Occupation

Next, we investigate the second-generation effects of the 1966 reform on several occupation indexes, including earnings score, education score, Hauser and Warren Socioeconomic Index (H&W Index), and two measures of segregation by occupation.

Table 9, Column (3), shows that second-generation black reform beneficiaries are more likely to be employed in a more prestigious occupation, while there is no effect among Whites. However, the increase in prestige is mostly driven by the education score rather than the earnings score component of the H&W Index, as indicated by Columns (1) and (2). In other words, second-generation black reform beneficiaries are more likely to have occupations with a significantly higher share of employees with college attainment, but these occupations do not have a significantly higher share of employees whose income is beyond the median.[14] In Column (4), we measure segregation by race across occupations using the absolute difference in the share of Blacks and Whites in each occupation. In Column (5), we use another measure, which is the absolute difference in the share of Blacks between the total labor force and each occupation. The results suggest that there is no effect on being employed in a more- or less- segregated occupation among either Whites or Blacks.

These results imply that job dissatisfaction among treated Blacks is likely driven by their wages and pecuniary fringe benefits. Even though second-generation black reform beneficiaries are more likely to be employed in more prestigious occupations, there are no significant effects on either earnings or other benefits potentially compensating wages.

7.3 Geographical Mobility

Geographical mobility has been an important contributor to racial inequality reduction. For example, Collins and Wanamaker (2014) find that African Americans who migrated away from Southern states during the Great Migration enjoyed large gains in earnings, but the direction of black migration flows reversed in the 1970s (Boustan 2016).[15]

The Fair Labor Standards Act of 1966 may be a potential driver of changes in the direction of Black migrants. The newly covered industries are overrepresented in Southern states, and these industries are generally lower-paid compared to others. The increase in wages in treated industries may reduce the push factor away from working in the South and make individuals more likely to choose to stay in- or even move to Southern states, especially for second-generation beneficiaries experiencing a positive education shock.

In Table 10, we estimate the effect of the 1966 FLSA on second-generation migration between 1966 and 1981. Column 1 (3) report estimates where the dependent variable is an

[14] While our findings using "public sector" as the dependent variable indicate that this is not mediated by an increased share of Blacks working in the public sector.

[15] See also Table A10 documenting a disadvantage in earnings for those working in Southern states. Columns (2) and (3), in particular, reveal that racial inequality in earnings is significantly greater in Southern states compared to the rest of the country.

Table 9: The Effect on Occupational Indexes

D.V.	Earn	Edu	H&W	Segregation by Race	
				with-in	population
	(1)	(2)	(3)	(4)	(5)
Covered in 1966	-0.008	-0.018	0.008	0.013	-0.003
X Treated Cohorts	(0.042)	(0.035)	(0.016)	(0.009)	(0.003)
DID X Black	0.081	0.164***	0.065*	0.003	-0.002
	(0.077)	(0.048)	(0.035)	(0.029)	(0.008)
Num.Obs.	2313	2313	2313	2313	2313
R2	0.145	0.181	0.266	0.256	0.168
Mean	0.60	0.38	0.35	0.83	0.05
S.D.	0.36	0.33	0.15	0.10	0.03
Two-way FEs	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Notes: 'Two-way FEs' refers to occupation (parents) and age (children) fixed effects. 'Controls' include binary indicators for ethnicity, urban location, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. The definition of these variables are in Table A1. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level. **Outcomes**: (1) share of employees whose earnings are above the median; (2) share of employees with college attainment; (3) Hauser and Warren Socioeconomic Index, is a measure of prestige ratings; see details. I constructed two indexes for segregation by race across occupations. Column (4) measures the absolute difference between share of Blacks and Whites for each occupation; Column (5) measure the absolute difference between share of Blacks in each occupation with the share of Blacks among all labor force.

indicator equal to one if the index individual has moved between states (counties) at least once since 1966, while in columns 2 (4) we consider a dependent variable equal to the number of such moves. We find no statistically significant effects of the reform and no evidence of heterogeneous effects between Blacks and Whites. Similarly, Column (5) indicates that the 1966 reform does not lead treated children of either black or white treated parents to move to areas with a larger labor market size (more opportunities). The last two columns, however, show that the absence of impacts on the extensive or intensive margins of moves hides a key difference in the composition of moves: regardless of whether an individual already lived in Southern states before the reform or not, black second-generation beneficiaries of the reform are much more likely to live or work in Southern states in 1981, while there is no effect on white second-generation beneficiaries.

Table 10: The Effect on Geographical Mobility

D.V.	Inter-state		Inter-County		Lab Mkt	End in	Stay
	Binary (1)	Num. (2)	Binary (3)	Num. (4)	Size (5)	South (6)	South (7)
Covered in 1966	0.002	0.008	0.022	0.072	-0.027	-0.029	-0.105
	(0.056)	(0.101)	(0.043)	(0.146)	(0.039)	(0.024)	(0.085)
DID X Black	-0.029	0.053	-0.101	0.010	0.021	0.186***	0.307***
	(0.073)	(0.112)	(0.076)	(0.168)	(0.063)	(0.051)	(0.106)
Mean	0.25	0.36	0.50	0.94	0.16	0.33	0.87
S.D.	0.43	0.72	0.50	1.21	0.37	0.47	0.34
Num.Obs.	2589	2589	2589	2589	2589	2589	1061
R2	0.159	0.166	0.144	0.155	0.194	0.831	0.343
Two-way FEs	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓

Notes: 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effects. 'Controls' include binary indicators for ethnicity, urban location, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. The definition of these variables are in Table A1. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

8 Conclusion

This paper investigates the causal effect of the Fair Labor Standards Act of 1966, which extended mandated minimum wages in newly covered industries in which African American workers were over-represented, on second-generation education and labor market outcomes, with a particular focus on implications for racial inequality.

Using data from males surveyed in the National Longitudinal Survey or Youth 1979, we implement and industry-by-cohorts difference-in-differences research design and first show that the minimum wage reform had an economically substantial and statistically significant effect on educational attainment.

Exploring differential effects by race, we find that racial inequality in educational outcomes declined not only because black workers are over-represented in treated industries but also because treatment effects are larger for African Americans. We reach this conclusion whether analyzing heterogeneous treatment effects through linear DID or non-linear causal forests. On average, we find that the reform contributed 19% of the reduction in racial inequality in education among treated cohorts.

The main driver of the black-white difference in the average treatment effect in completed years of education is the larger decline in high school drop out among black children of workers in the treated industries | in contrast, the effects on college education are similar for Blacks and Whites. Previously documented low returns to education for black males in the lower part of the distribution may contribute to explaining why we then find that even the large improvements in educational outcomes we estimate did not significantly improve employment or earnings for black second-generation reform beneficiaries. It shifted the composition of jobs held by Blacks towards occupations with a larger share of college-educated workers but this failed to translate into a significant earnings premium. Overall, job satisfaction increased among white second-generation reform beneficiaries but *decreased* among black second-generation beneficiaries. And we find no evidence that the lack of improved earnings was compensated by an increase in job security (captured by public sector employment), fringe benefits, or changes in the racial composition of jobs. Importantly, we find that the 1966 reform reduced mobility away from the low-wage South among treated Blacks, which constitutes a plausible pathway for the lack of intergenerational reduction in racial income disparities.

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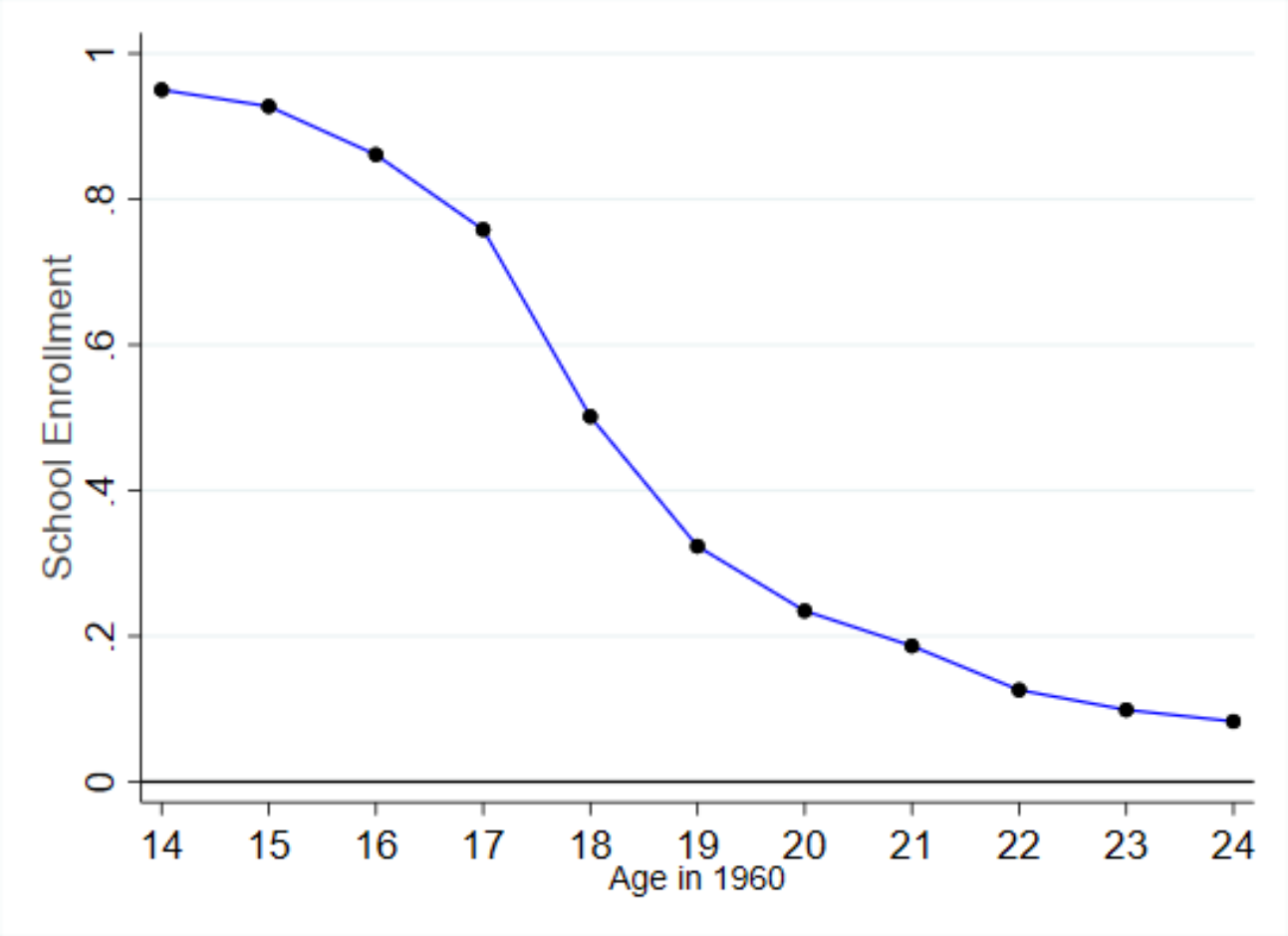
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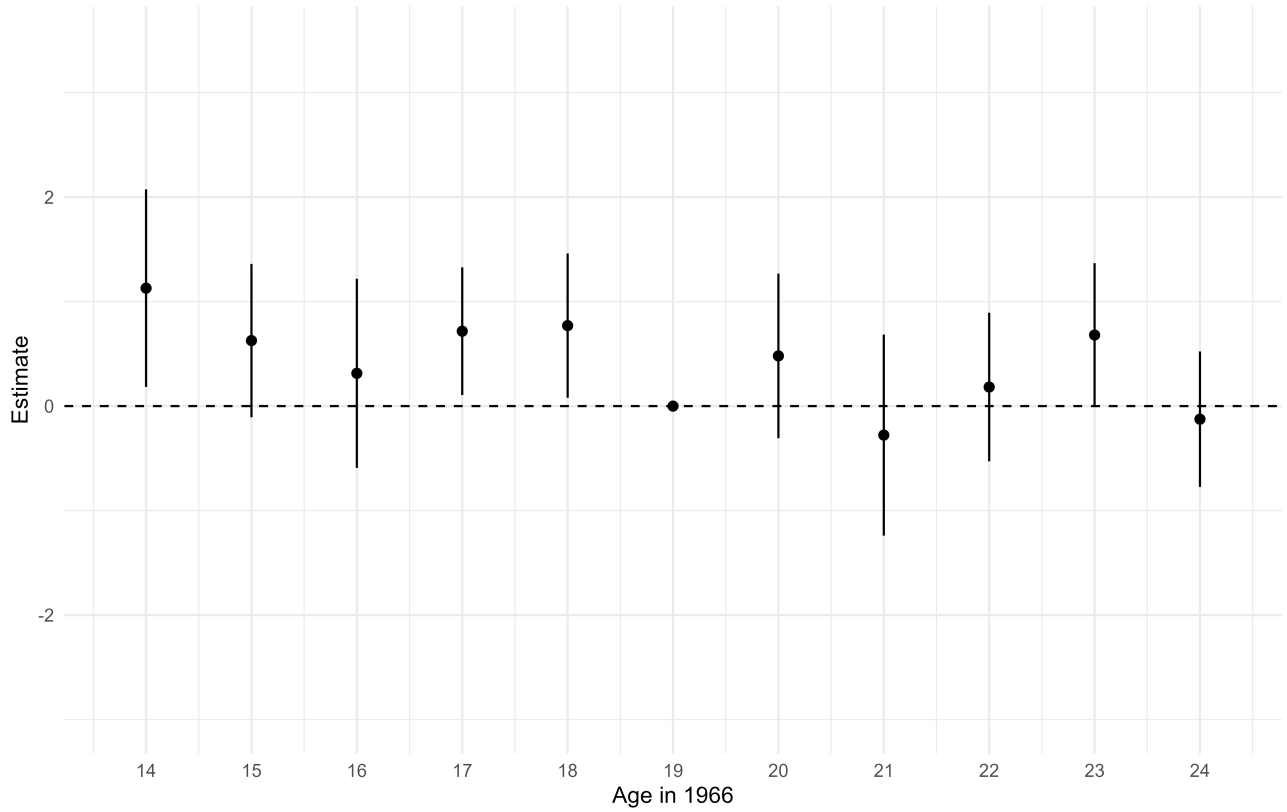
Appendix A Figures and Tables

Figure A1: School Enrollment Rate by Age in 1960



Notes: This figure shows the school enrollment rate by age in 1960 using US Census micro data.

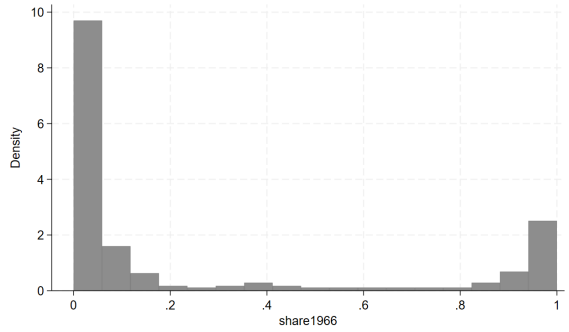
Figure A2: Dynamic Effect by Age in 1966



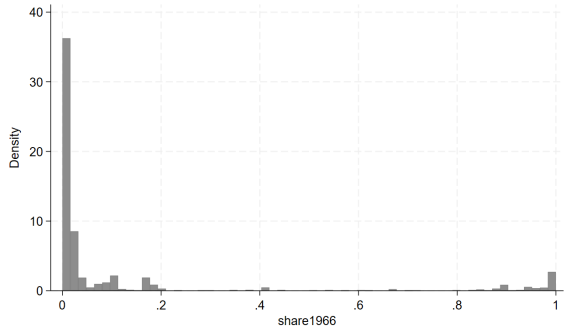
Notes: This figure shows the dynamic effect of the 1966 reform on the second generation's completed year of education by age in 1966 using 19 cohort as the reference group. This figure plots point estimations with 95% confidence interval.

Figure A3: Distribution of 'Share of Workers in Treated Industries'

(a) Equal weight

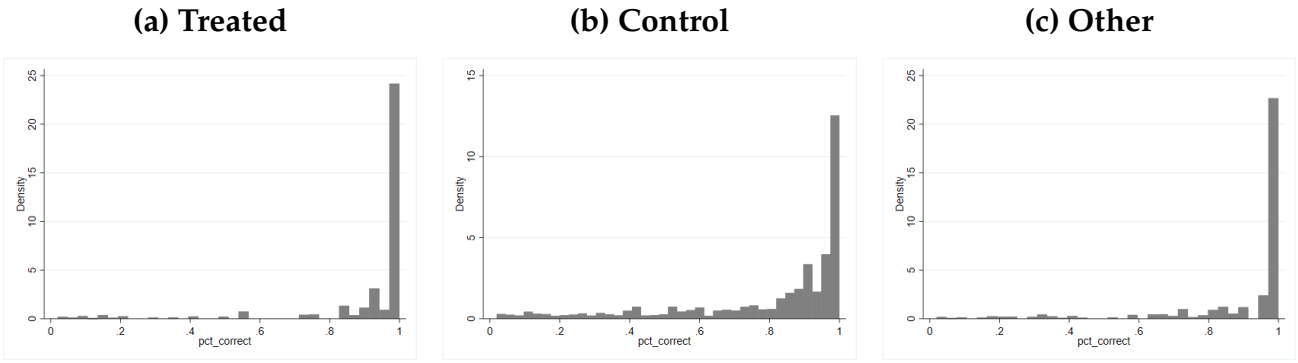


(b) weighted by Num. of Workers



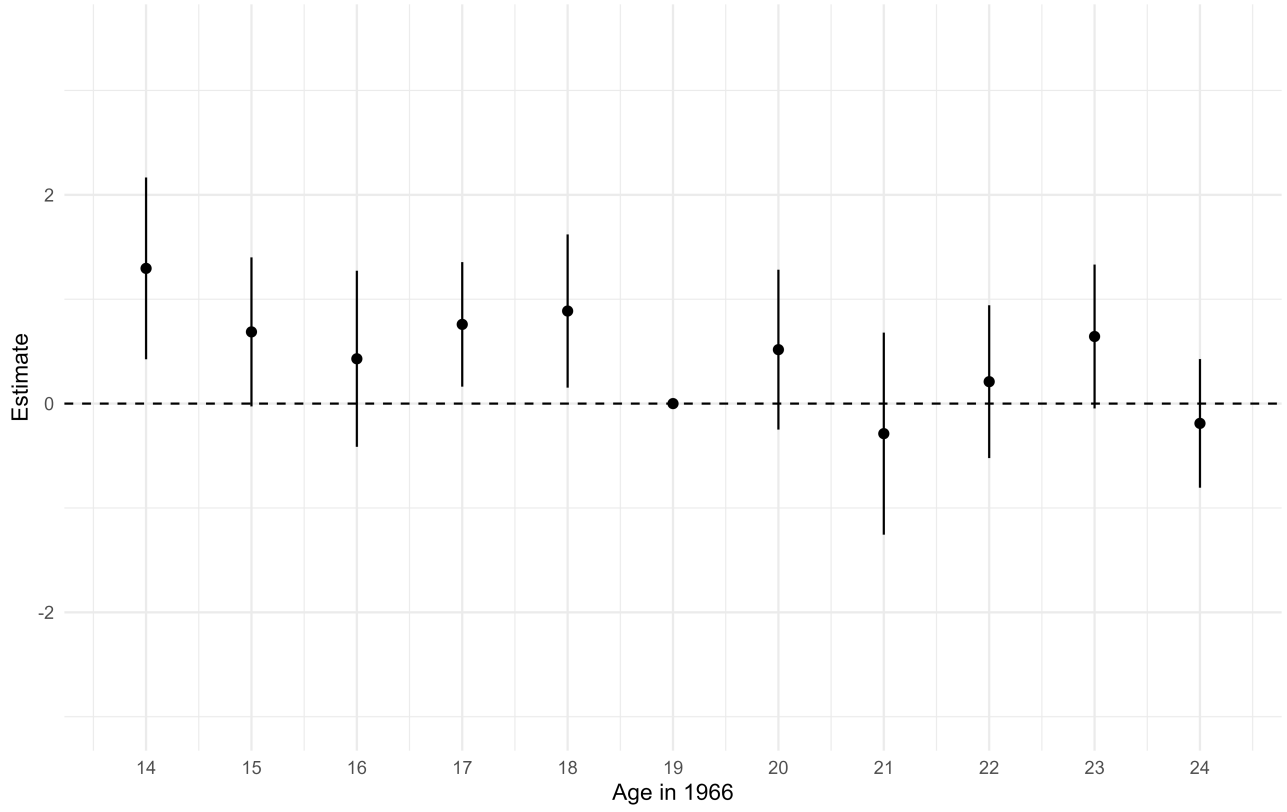
Source: Census 1960 micro data by IPUMS.
Notes: This figure shows the distribution of 'share of workers in treated industries' by occupations.

Figure A4: Distribution of 'Share of States having the same Matched Treatment Status with Matching using Full Sample'



Notes: These figures show the distribution of share of states have the same matched treatment status at state level and country level by occupation for treated, control, and other occupations, respectively.

Figure A5: Dynamic Effect by Age in 1966 with Restricted Sample



Notes: This figure shows the dynamic effect of the 1966 reform on the second generation's completed year of education by age in 1966 using 19 cohort as the reference group. This figure plots point estimations with 95% confidence interval.

Table A1: Variables in NLSY79YM

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 and 1981 to calculate the highest grade completed.
Dropout	1966-1981	0/1	Years of education is strictly smaller than 12 or not.
College Attainment	1966-1981	0/1	Years of education is strictly greater than 12 or not.
Educational Goal	1967	0 to 18	
Employment	1981	0/1	One for with a job and zero for looking for work.
Wage	1981	10 to 50,001	Altogether, how much (do/did) you usually earn at this job before deductions?
Fringe Benefits	1981	0 to 1	See Table notes.
Job Satisfaction	1981	0 to 1	If you were free to go to any type of job you wanted, what would it be? One for yes and zero for others.
Occupation	1981	3-digit 1960 codes	What kind of work were you doing?

Notes: This Table shows the variables in NLSY79YM. **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. **Fringe Benefits**: non-pecuniary fringe benefits include training/educational opportunity, profit sharing, stock option, free/discounted meals, free/discounted merchandise, and flexible work hour; while pecuniary fringe benefits include insurance coverage away from job, life insurance coverage away from job, retirement program, paid sick leave, and paid vacation.

Table A2: Placebo Test for Educational Outcomes

D.V.	Year of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967	-0.183	-0.108	-0.019	-0.007	0.000	0.006
X Placebo Cohorts	(0.305)	(0.365)	(0.053)	(0.065)	(0.030)	(0.033)
Num.Obs.	1564	1564	1564	1564	1564	1564
R2	0.314	0.439	0.269	0.365	0.203	0.307
5						
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓

Notes: 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labor market size, south, family income, family received welfare/public assistance, number of siblings and library card availability. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A3: Placebo Test for Labor Market Outcomes

D.V.	Employed	Log wage	Gov	Fringe Benefits		Want the
				non-pecuniary	pecuniary	same job
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967	-0.016	0.086	0.121**	0.013	-0.009	-0.101
X placebo Cohorts	(0.015)	(0.282)	(0.053)	(0.037)	(0.026)	(0.065)
Num.Obs.	1006	798	979	801	801	973
R2	0.273	0.410	0.203	0.190	0.236	0.208

Notes: 'D.V.' refers to the dependent variable. All regressions include two-way fixed effects and controls, including binary indicators for ethnicity, urban location, labor market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability, as well as interaction with Blacks. The definition of these variables are in Table A1. **Fringe Benefits**: non-pecuniary fringe benefits include training/educational opportunity, profit sharing, stock option, free/discounted meals, free/discounted merchandise, and flexible work hour; while pecuniary fringe benefits include insurance coverage away from job, life insurance coverage away from job, retirement program, paid sick leave, and paid vacation. Approximately 16% of the data for wage and fringe benefits is missing. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level.

Table A4: Robustness Check: Drop potential Mis-Matched Occupations

D.V.	Year of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1967	0.751***	0.681***	0.106***	0.100***	-0.088***	-0.081***
X Treated Cohorts	(0.242)	(0.240)	(0.033)	(0.036)	(0.029)	(0.029)
Num.Obs.	3747	3747	3747	3747	3747	3747
R2	0.226	0.334	0.185	0.268	0.126	0.214
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓

Notes: The sample only includes parents' occupations in which share of workers in treated industries is wither higher than 80% or less than 20%. 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labor market size, south, family income, family received welfare/public assistance, number of siblings and library card availability. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A5: Robustness Check: Continuous Treatment by Occupation

D.V.	Year of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Probability of Covered in 1966						
× Treated Cohorts	0.767***	0.697**	0.110***	0.103**	−0.086**	−0.078**
	(0.278)	(0.273)	(0.039)	(0.041)	(0.033)	(0.033)
Num.Obs.	3876	3876	3876	3876	3876	3876
R2	0.228	0.332	0.189	0.269	0.125	0.210
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓

Notes: 'D.V.' refers to the dependent variable. 'S.D.' refers to the standard deviation. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labour market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A6: Robustness Check: Excluding Cohorts Age 17 or 18 in 1966

D.V.	Year of Edu		College		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Covered in 1966	0.668*	0.617*	0.112*	0.107*	−0.065**	−0.059*
X Cohorts 14-16	(0.346)	(0.325)	(0.063)	(0.063)	(0.032)	(0.033)
Num.Obs.	3045	3045	3045	3045	3045	3045
R2	0.243	0.351	0.199	0.283	0.146	0.228
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓

Notes: 'D.V.' refers to the dependent variable. 'S.D.' refers to the standard deviation. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labour market size, south, family income, the family received welfare/public assistance, number of siblings and library card availability. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A7: Effect by Parents' Education Level

Quantile	1	2	3	4
% College	0-4%	4%-7.3%	7.3%-21.5%	21.5%-98.6%
	(1)	(2)	(3)	(4)
Panel A:		D.V.: Year of Education		
Covered in 1967	0.506**	0.305	1.395***	-0.308
X Treated Cohorts	(0.221)	(0.669)	(0.405)	(0.295)
Panel B:		D.V.: College		
Covered in 1967	0.034	0.011	0.176**	0.013
X Treated Cohorts	(0.030)	(0.092)	(0.067)	(0.049)
Panel C:		D.V.: Dropout		
Covered in 1967	-0.095**	0.038	-0.148***	-0.022
X Treated Cohorts	(0.044)	(0.093)	(0.037)	(0.029)
Observations	927	789	823	970
R^2	0.26	0.28	0.23	0.16
Two-way FEs	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labor market size, south, family income, family received welfare/public assistance, number of siblings and library card availability. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the occupation level.

Table A8: Covariate means across tertiles of CATEs (All Variables)

	First Tertile (N=1170)		ThirdTertile (N=1169)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Panel A:			D.V.: Year of Education			
cate	0.058	0.101	0.547	0.170	0.490	0.000
Age	17.694	3.149	19.033	3.156	1.338	0.000
Black	0.221	0.415	0.368	0.483	0.147	0.000
South	0.159	0.366	0.706	0.456	0.546	0.000
Rural	3.509	2.476	6.636	2.151	3.127	0.000
Lab Mkt Size	4.525	2.412	1.961	1.663	-2.563	0.000
Family income	6.675	2.434	6.440	2.671	-0.235	0.043
Subsidies	0.105	0.307	0.062	0.241	-0.043	0.000
No Subsidies	0.797	0.402	0.894	0.308	0.097	0.000
Library card	0.836	0.370	0.384	0.487	-0.452	0.000
Siblings	2.535	1.606	3.330	1.652	0.795	0.000
Panel B:			D.V.: Dropout			
cate	-0.099	0.032	-0.011	0.016	0.088	0.000
Age	18.429	3.175	18.193	3.183	-0.235	0.060
Black	0.400	0.490	0.236	0.425	-0.164	0.000
South	0.818	0.386	0.060	0.238	-0.758	0.000
Rural	7.103	1.633	2.663	2.031	-4.440	0.000
Lab Mkt Size	1.700	1.320	5.032	2.372	3.332	0.000
Family income	5.568	2.718	7.525	2.218	1.958	0.000
Subsidies	0.084	0.278	0.071	0.257	-0.013	0.212
No Subsidies	0.840	0.367	0.847	0.360	0.007	0.626
Library card	0.368	0.483	0.831	0.375	0.463	0.000
Siblings	3.434	1.638	2.390	1.548	-1.044	0.000

Notes: 'D.V.' refers to the dependent variable. The table shows covariate means for the first and third tertile of the sample based on the estimated CATEs.

Table A9: Effect by Gender of the Household Head

Sample	Full		Black		White	
	All	ex. only with mother	All	ex. only with mother	All	ex. only with mother
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			D.V.: Year of Education			
Covered in 1967	0.569**	0.567*	1.202***	1.187***	0.316	0.386
X Treated Cohorts	(0.269)	(0.297)	(0.378)	(0.411)	(0.316)	(0.319)
Num.Obs.	3876	3573	1040	869	2836	2704
R2	0.331	0.325	0.347	0.342	0.326	0.320
Panel B:			D.V.: College			
Covered in 1967	0.081*	0.090*	0.054	0.053	0.071	0.088*
X Treated Cohorts	(0.042)	(0.046)	(0.058)	(0.059)	(0.052)	(0.051)
Num.Obs.	3876	3573	1040	869	2836	2704
R2	0.269	0.264	0.278	0.289	0.272	0.266
Panel C:			D.V.: Dropout			
Covered in 1967	-0.070**	-0.090***	-0.132**	-0.161*	-0.048	-0.078***
X Treated Cohorts	(0.031)	(0.026)	(0.066)	(0.085)	(0.031)	(0.024)
Num.Obs.	3876	3573	1040	869	2836	2704
R2	0.210	0.198	0.262	0.269	0.196	0.188
Two-way FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: 'D.V.' refers to the dependent variable. 'Two-way FEs' refers to occupation (parents) and age (children) fixed effect. 'Controls' includes ethnicity, urban, labor market size, south, family income, family received welfare/public assistance, number of siblings and library card availability. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are clustered at the occupation level.

Table A10: Baseline Regional Earning Gap

Sample	Full	Black	White
	(1)	(2)	(3)
South	-0.164***	-0.345***	-0.140***
	(0.015)	(0.046)	(0.012)
'Chow test'		Prob > chi2 = [<0.001]***	
Observations	68,058	6,426	61,619
R-squared	0.596	0.550	0.587
FEs & Controls	✓	✓	✓

Notes: Data: CPS. Time period: 1961 to 1966, except for 1962. The dependent variable is log annual earnings. Significant at *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the industry level. FEs includes industry and year fixed effects; controls include dummies of sex, ethnicity (Column 1), 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.