# Education and Incarceration in the Jim Crow South

## Evidence from Rosenwald Schools

**Katherine Eriksson**

ABSTRACT

*This paper examines the effect of childhood access to primary schooling on adult black incarceration in the early 20th century. I construct a linked census data set of incarcerated and nonincarcerated men to observe access to schooling in childhood. I find that full exposure to one of the new primary schools built as part of the Rosenwald program reduces the probability of incarceration by 1.9 percentage points. I argue that the reduction in incarceration comes from increased opportunity costs of crime through higher educational attainment. These results contribute to a broader literature on racial gaps in social outcomes in the United States.*

## I. Introduction

In the contemporary United States, black men are disproportionately more likely than white men to be arrested and incarcerated. This racial gap in incarceration is not new. In 1890, black men were 3.1 times more likely to be incarcerated

*Katherine Eriksson is assistant professor of Economics at the University of California–Davis, a faculty research fellow at the NBER, and a research associate at the University of Stellenbosch.* *The author thanks Ancestry.com and FamilySearch.org for access to data for this project and Bhash Mazumder and Seth Sanders for additional data. The author acknowledges financial support from the Center for Economic History at UCLA. She is grateful for advice from her dissertation committee, Leah Boustan, Dora Costa, Christian Dippel, and Walker Hanlon, and for beneficial conversations with, among others, Marianne Bitler, Scott Carrell, Marianne Page, Giovanni Peri, and participants of the NBER Development of the American Economy 2012 summer session, the Economic History Association 2012 and 2013 annual meetings, the Southern Economic Association 2014 meeting, SoCCAM 2014, the 2014 CSWEP/CEMENT workshop, SOLE 2015, and seminar participants at many universities. She also thanks Roy Mill for advice and help and undergraduate research assistants, especially Ashvin Gandhi, for help with data collection and analysis. The data used in this paper is restricted access data made available by IPUMS through the NBER secure data server. For questions about how to access the restricted data, contact IPUMS. The author is willing to assist (kaeriksson@ucdavis.edu).*
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than white men. By 1923, the black/white incarceration ratio was 4.2, and it grew to 6.4 by 2010 (Petersilia and Reitz 2012). High rates of incarceration in the past may contribute to black imprisonment today. For example, evidence suggests that children who grow up with fathers in prison are more likely to have behavioral problems, drop out of school, be unemployed, and even be incarcerated themselves (Johnson 2007).

Explanations for the racial gap in incarceration fall into three categories: discrimination by the police and courts, sentencing policies, and socioeconomic disparities that give rise to different underlying levels of crime.[1] Recent work finds that today's racial incarceration gap is partly due to a discriminatory law enforcement system and changes in sentencing policies (for example, three-strikes laws) since 1980 (Alexander 2012; Raphael and Stoll 2007), but education and income differences have also been found to be a large driver of incarceration in recent decades (Lochner and Moretti 2004). Yet, little is known about the relative determinants of incarceration in the early 20th century as the racial gap in imprisonment emerged.[2]

This paper collects a new data set of the full universe of prisoners from the U.S. Censuses between 1920 and 1940, a time period that has previously been difficult to study due to a lack of micro-level data. I explore the role of one factor—disparities in education—in explaining the historical roots of the racial gap in incarceration.[3] In particular, I analyze the relationship between access to primary education and the probability of incarceration as an adult between 1920 and 1940 among southern-born black men in the United States. I use the construction of almost 5,000 schools in 14 southern states for rural black students between 1913 and 1931, sponsored in part by northern philanthropist Julius Rosenwald, as a quasi-experiment that increased the supply of schooling for black children and therefore the educational attainment and literacy of blacks born in the South (Aaronson and Mazumder 2011).

Using a linked census sample of prisoners and nonprisoners, I assign men their likely exposure to a Rosenwald school according to their county of residence as children. Rosenwald schools were specifically targeted to rural black students. Therefore, I identify the effect of exposure to a Rosenwald school by comparing rural black children to rural whites, to blacks in urban areas within the same county, and to black children born before the Rosenwald program began.

I find that access to education significantly reduces incarceration later in life among adults. Full exposure to a Rosenwald school for seven years between ages 7 and 13 reduces the probability of being incarcerated for blacks by 1.96 percentage points;

1. These disparities include differences in education levels and income, but also differential job opportunities and unemployment rates, urban residence rates, and family background between races.

2. One exception is Moehling and Piehl (2014), who look at immigrants in the first three decades of the 20th century and find that immigrants assimilated towards natives between 1900 and 1930; that is, immigrants were unlikely to be incarcerated upon first arrival, but became more so after spending more time in the United States. Another is Muller (2012), who finds that migration from the South to the North was partly responsible for increased black incarceration rates during the Great Migration; however, his paper uses aggregate data from census publications, not census microdata. Finally, Feigenbaum and Muller (2016) collect city-level homicide rates in the early 20th century and show that the use of lead pipes increased homicide rates.

3. In this paper, I use incarceration in the census as my measure of crime, while thinking about factors that should affect actual criminality. Incarceration and criminality are by no means the same thing, particularly in the highly discriminatory environment of the Jim Crow South.

average exposure in the most exposed cohorts reduces incarceration by up to 8.8 percent of the 1940 mean incarceration rate. I show that educational attainment is an important channel through which the probability of incarceration decreases, finding no statistically significant effects on migration.

This paper contributes to two literatures. The first concerns the convergence in wages and other outcomes between blacks and whites over the 20th century. In 1910, blacks lagged behind whites in completed schooling by three years on average, a legacy of slavery and of poor investments in southern black schools (Margo 1990; Aaronson and Mazumder 2011). The racial gap in schooling diminished substantially by 1940, contributing to the decline in the black–white wage gap over the 1940s (Heckman et al. 2000; Smith and Welch 1989).

My estimates suggest that the black–white incarceration gap should have been cut in half by 1980 due to these relative increases in black educational attainment. The fact that black incarceration rates have not only remained persistently high but also have increased further since the mid-1970s suggests that other factors have counteracted the forces of educational convergence. Specifically, in the earlier period, the Great Migration of blacks from the South to the North, as well as migration to cities within the South, increased black incarceration rates due to higher incarceration rates in urban areas (Muller 2012). Furthermore, state prison capacity was growing through the 1930s and 1940s as states increased their use of free or cheap convict labor as a major revenue source (Larsen 2016).

This paper also adds to our understanding of the social returns to education. One of the social returns to education is a significant reduction in criminality.[4] The relationship between education and crime has been extensively studied in a modern context. Lochner and Moretti (2004) find that an additional year of school reduces the probability of incarceration for blacks and whites.[5] My results imply that one year of school reduces the probability of being incarcerated by 1.1 percentage points, an effect almost three times as large as Lochner and Moretti's estimate. One possible explanation is that the social returns to primary school (in terms of crime reduction) are larger than the social returns to high school.

The structure of this paper is as follows. Section II provides historical background about black–white differences in incarceration and in schooling through the 20th century. In Section III, I describe the data and exposure to Rosenwald schools. Section IV discusses my estimation strategy and potential threats to identification. Section V presents results from my primary sample. Section VI shows that results are robust to different matching procedures, weighting, and incarceration definitions; it also presents results with unmatched data as well as a permutation test that reshuffles Rosenwald school exposure. Section VII concludes.

4. Other research has also shown that education contributes to improvements in health, more targeted fertility, and increases in voting and civic behavior (Lleras-Muney 2005; Clark and Royer 2010; Aaronson et al. 2014; Milligan et al. 2004).

5. Lochner and Moretti (2004) is the best known study; their results have been replicated and expanded in Sweden, the UK, and other European counties (Hjarlmarsson et al. 2010; Machin et al. 2011; Meghir et al. 2011). Other work has looked at the relationship between school quality and crime (Deming 2011) and finds a significant effect. Anderson (2014) shows that juvenile crime decreases with higher minimum dropout ages for students.

## II. Historical Background and Theoretical Framework

### *A. Incarceration Rates by Race and Region over Time*

In historical data for the United States, incarceration rates for blacks have always been higher than those of whites. Figure 1 graphs the number of incarcerated individuals per capita by race and region from 1890 to 1980.[6] In 1890, three out of 1,000 blacks were incarcerated; the black incarceration rate was 3.1 times as high as the white incarceration rate. The black/white incarceration ratio grew to 4.8 in 1940, before falling back to 3.1 by 1950. Thereafter, the ratio grew through 1980. Rates for blacks living in the North were higher than for those living in the South throughout the period—blacks in the more urban North were between two and three times more likely to be incarcerated than those in the rural South. The figure shows the numbers incarcerated divided by the relevant population, which includes both men and women of all ages. Given that about 90 percent of prisoners were male, multiplying by 1.8 would give the rate for men. For example, the incarceration rate of black men in the South in 1923 was about 0.43. Incarceration rates for the most often incarcerated ages of 18–45 are even higher.

Historical evidence suggests that the initial racial gap in incarceration rates (circa 1890) may have been, in part, the result of a discriminatory system that was set up to incarcerate black men. Following the Civil War, many southern states passed a series of laws, referred to as "Black Codes," designed to control the mobility and restrict the economic opportunities of black freedmen. One subset of these laws criminalized vagrancy and allowed prisons to lease out their inmates as low-cost labor to local farms (Naidu 2010). Convict leasing became an increasingly large income source for state prisons, leading to a system that has been called "slavery by another name" (Blackmon 2008). As leasing convicts to private citizens became illegal in most states by the end of the 19th century, states realized they could profit directly from convict labor in prisons. For example, the Brushy Mountain Penitentiary was built in Tennessee in 1896 to house prisoners who worked at the prison-run stone quarry. Parchman Farm in Mississippi is known as one of more brutal examples of large-scale farming using free labor (Oshinsky 1996). The fact that states could gain free labor from convicts provided incentives to lock away black men for minor infractions.

Figure 1 demonstrates that the current black–white incarceration gap is not a recent phenomenon but, rather, has been present throughout the 20th century. Any explanation of differences in incarceration rates needs to take into account the historical patterns of incarceration. Most literature has focused on the evolution of this gap since the 1970s. This paper is one of the first to examine incarceration in the first half of the 20th century. In light of the discriminatory Jim Crow system present in the historical South, one question is the extent to which educational investments reduced criminal behavior in this context.

### *B. Black Schooling and Rosenwald Schools*

The debate about whether, and to what extent, education reduces crime and therefore incarceration goes back to the early 20th century and was in fact a central topic of

6. Data are taken from published census statistics. See figure notes for sources.

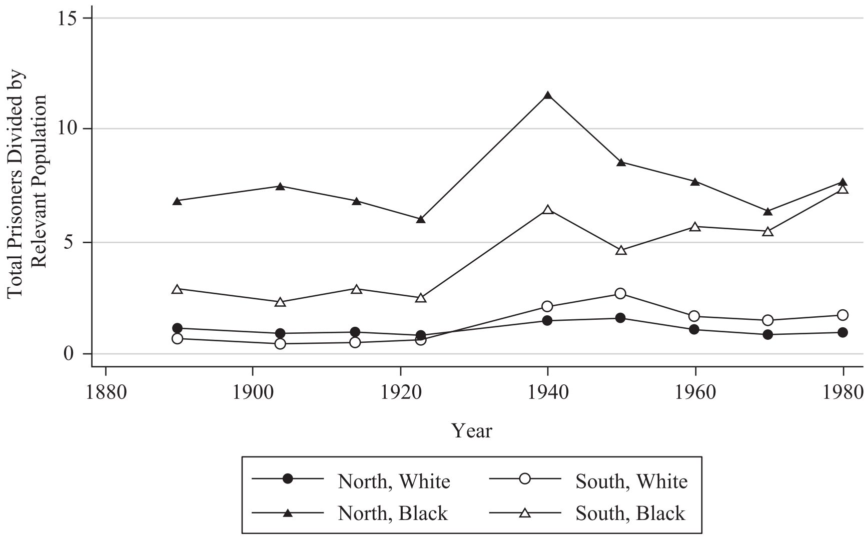


**Figure 1**

*Incarceration by Race and Region per 1,000 Population, 1890–1980*

Notes: Incarceration figures taken from U.S. Department of Interior (1895), U.S. Department of Commerce and Labor (1907), U.S. Department of Commerce (1914, 1926, 1943, 1953, 1963, 1973, 1983). Population (denominator) taken from IPUMS (Ruggles et al. 2010). Figure depicts total number of prisoners by race, census region, and year divided by relevant population, where population is interpolated between census years for noncensus years. Men and women included—multiply figure numbers by 1.8 to calculate approximate male incarceration rates.

concern at that time. John Roach Stratton (1900) argued that the "race problem," that is, the high crime rates and "immorality" of blacks, could not be solved by education. Stratton thought that the positive correlation between increasing black incarceration and increasing levels of black education between the end of the Civil War and 1900 showed that education actually increased criminality. He argued that allowing blacks to gain education and move from farms to cities to find work increased crime rates at very little benefit to blacks or whites. In fact, Governor Vardaman of Mississippi used this reasoning when restricting funds for black schools in 1904 (Hollandworth 2008). On the other side of the argument, Booker T. Washington sought to explain higher black criminal behavior as a result of low wages and discrimination. The issue of high black incarceration rates was one motivation for Booker T. Washington's interest in improving black schools (Washington 1900), out of which grew the Rosenwald Initiative.

Blacks born between 1880 and 1910 completed on average three fewer years of education than whites. Motivated by his concerns about the low levels of funding for black education, Booker T. Washington, principal of the Tuskegee Institute in Alabama, reached out to northern philanthropist and businessman Julius Rosenwald.[7] Rosenwald

7. The Rosenwald School Initiative was not the only black schooling initiative in this time. The Jeanes Fund provided teacher training. Kreisman (2017) shows that this fund also increased school enrollment and literacy of black youth. Other philanthropic interventions are described in Donohue, Heckman, and Todd (2002).

agreed to fund a pilot program supporting the construction of six black schools in 1913–1914, with the promise of up to 100 more. The original schools were built primarily in Alabama; by 1920, the program supported 716 schools in 11 southern states. By 1931, the Fund had supported the building of 4,983 schools explicitly targeting rural students.

Rosenwald believed that, in order to be successful, communities needed to "buy-in" to, or make investments in, any educational endeavors. This view, coupled with Washington's belief in black self-reliance, led to the use of a matching grant approach, whereby local communities had to raise anywhere from 75–90 percent of the funds for a new school. The early schools received about 25 percent of the cost in grant money, whereas this number fell to 10–15 percent by the later years of the program. On average, local school districts contributed about half of the funds for the school, with about 20 percent coming from black citizens and 4 percent from white citizens. After the schools were built, they were reliant on the local community and the state for funding. The program ended in 1931 with Rosenwald's death and the decreased value of fund assets after the collapse of the stock market. In addition to helping to build schools, the fund also provided some money for teacher training schools, teacher homes, and shops. By the end of the program, 76 percent of counties in 14 southern states had a school, and 92 percent of black students in these states lived in a county with a school.[8]

In an earlier evaluation of the direct effects of the program, Aaronson and Mazumder (2011) find that the schools could serve 36 percent of rural black students by 1931. They show that the Rosenwald schools were a significant contributor to the narrowing of the black–white schooling gap by 1940. In particular, Aaronson and Mazumder estimate that Rosenwald schools increased school attendance by about five percentage points. Using years of education reported on World War II draft cards, they find that full exposure (seven years) to a Rosenwald school also increased educational attainment by 1.2 years.

### *C. Conceptual Framework*

I measure the reduced form effect of having access to a Rosenwald school during childhood on incarceration as an adult. This effect could come through multiple channels, including education, income, and migration.

The most important direct mechanism through which Rosenwald schools likely reduced black incarceration was increased educational attainment of exposed black cohorts, where educational attainment raises the opportunity cost of engaging in criminal activity by increasing wages. Alternatively, more time in school could act through the "incapacitation effect" whereby staying in school keeps children occupied, preventing them from entering a life of crime.[9] Finally, education could reduce incarceration through what students learn in school—there is evidence that education increases voting and other civic behavior (Milligan et al. 2004); these attitudes could also translate into lower willingness to commit crimes.

8. Following Aaronson and Mazumder, I omit Missouri from my analysis because only 11 schools were built there.

9. The individuals in my sample as adults will not be directly incapacitated because they are too old to still be in school, but they could have begun criminal behavior later if they were more exposed to Rosenwald schools. Given that there is a strong correlation between early offenses and later incarceration, this is one way through which schools could have decreased incarceration.

Another major mechanism through which education could affect incarceration is through migration; there is evidence that migration from the South to the North was somewhat positively selected on education (Collins and Wanamaker 2013). Furthermore, incarceration rates were higher in the North than South, so moving to a northern city might increase the propensity to be incarcerated. If Rosenwald schools increased the probability of migrating, I would be understating the effect of Rosenwald schools on incarceration in the absence of migration. I directly test these mechanisms below.

While the effects of education and migration are directly testable with my data, there could be community-level effects that would confound my results. My identification strategy compares races, cohorts, and rural–urban individuals to estimate the individual impact of Rosenwald schools, but they could have had impacts on the communities overall. In a competitive labor market, we might expect higher levels of education in the local black population to have a negative effect on overall wages.

Finally, being incarcerated is the outcome of committing a crime, being caught, being convicted, and being enumerated in prison. One way through which Rosenwald schools decreased incarceration could be through the ability to avoid getting caught or avoid being convicted after being caught. Particularly in this time period, being employed lowered the probability of being at risk to be picked up for vagrancy.

## III. Data

### *A. Measuring Incarceration*

I am interested in estimating the effect of access to a Rosenwald school on adult outcomes, particularly on the likelihood of committing a crime or being imprisoned. Lacking historical data on crime or arrest rates, I instead rely on individual-level data on incarceration as of the census date.[10] I calculate this measure from U.S. census data for the years 1920–1940. To do so, I assemble a data set that includes the full universe of southern-born, male prisoners and nonprisoners in each relevant census. I restrict the sample to men ages 18–35 who were born in one of the 14 Rosenwald states.

I identify prisoners in each census via a four-step process. The process uses the Restricted Full Count Census data from 1920–1940 available on the NBER server, along with IPUMS coding of group quarters combined with images looked up by hand. This procedure is necessary to calculate correct incarceration rates because IPUMS codes some men as incarcerated who are not but also misses a substantial number of prisoners.[11]

10. The FBI Uniform Crime Statistics do not become available for a substantial number of counties until the 1960s. Crime statistics are only available from census reports or for major cities prior to the start of the FBI UCR. This is the first paper to collect individual data on incarceration by race and for the full country. Moehling and Piehl (2014) collect individual data for immigrants and nonimmigrants living in select northern states in the 1900–1930 censuses.

11. While one should be able to construct correct incarceration rates using the *gq* and *gqtype* variables from IPUMS, there are known problems with this variable. Personal correspondence with IPUMS acknowledged that the "Institution" variable was not consistently entered in the full count censuses. Furthermore, entire census pages are coded as in prison even if only two men are in a county jail. By looking up images by hand, I try to rectify these problems.

I identify prisoners in 1920, 1930, and 1940 using a four-step process. First I extract all men with group quarters type (*gqtype*) variable equal to 2 and group quarters (*gq*) variable equal to 3, 4, or 5, as well as all men with a relationship to household head of "prisoner," "convict," "inmate," or with a blank relationship variable. Second, I code as incarcerated anyone with this group quarters status or who has a relationship to household head of "prisoner" or "convict." Third, I identify images to look up by hand where there is no group quarters string variable, and the relationship to household head is "inmate" or blank. Finally, I code by hand incarceration status for men who are on the images identified in the previous step.

I look up by hand 1,025, 1,410, and 1,765 images from 1920, 1930, and 1940, respectively. This identifies an additional 9,308, 18,083, and 19,016 prisoners in the respective years. I identify three incarceration measures: my preferred measure that uses the strategy above, the "group quarters" measure that only uses the group quarters and group quarters type variables to identify prisoners, and the "group quarters plus relationship" measure that uses the "group quarters" but adds all individuals with relationship strings of "prisoner" or "convict." I define an individual as incarcerated if he is present in a state prison, but also include federal penitentiaries, county and city jails, convict camps, and chain gangs.[12]

Table 1 shows the incarceration rates by race, year, and method. In all three years, the incarceration rates is highest in my preferred sample and lowest in the second sample. Rates are similar, but, for example, range from 1.86 to 2.55 for blacks in 1940. Figure 2 shows the top left of a census image that was classified as nonincarcerated by IPUMS (due to the institution name not being entered) but which is from a city jail in Mobile, AL. While incarceration rates differ across measures, I show in Section VI that my main results are not sensitive to the definition of incarceration used.

### *B. Constructing the Primary Sample*

I identify the town and county in which sample individuals grew up by matching all men to the relevant census one or two decades earlier to find the individual living in their birth family. I assign childhood county and town of residence to each individual after matching and attach urban or rural status to each town as of the relevant census year.[13] The goal is to find individuals as children, so men aged 18–23 years are matched to the previous census, while those 24–35 years are matched over a 20-year period.

12. While Moehling and Piehl (2014) restrict only to those in state and federal prisons, I do consider those in jail in my primary analysis. One main reason is that the state prison systems in the South were less developed than in the North in this time period. In the North, 86.4 percent of prisoners were in state or federal prisons in this census, but in the South it was only 80.5 percent. These numbers are more different in previous census waves. Also, in the South, average jail sentence lengths were about two years, which suggests jails were used to house long-term prisoners (Oshinsky 1996). I do not include individuals in mental institutions or state hospitals, even though it was a common practice in this period for courts to send individuals to these rather than prison. Future work could look at the determinants of being in these types of institutions.

13. I follow the census in defining as rural any incorporated place with more than 2,500 residents. As Aaronson and Mazumder (2011) argue, this was likely also the definition used by Rosenwald Fund administrators when targeting Rosenwald funds to rural areas. Not all schools were necessarily in rural areas, but it is impossible to match schools with towns without more information, so I assume that the majority of schools were not in urban centers.

**Table 1**
*Incarceration Rates with Three Different Measures*

| | Incarceration Measure | | |
|---|---|---|---|
| | Preferred | Group Quarters | Group Quarters + Relationship |
| 1940 | | | |
| Black | 2.55 | 1.86 | 2.00 |
| White | 0.72 | 0.53 | 0.57 |
| 1930 | | | |
| Black | 2.23 | 1.79 | 1.95 |
| White | 0.68 | 0.54 | 0.59 |
| 1920 | | | |
| Black | 1.35 | 1.11 | 1.16 |
| White | 0.21 | 0.17 | 0.18 |

Notes: "Preferred" incarceration measure uses all individuals with relationship string "prisoner" and "convict," as well as individuals with blank or "inmate" relationships to household head that were determined by hand to be in a prison or jail. "Group quarters" measure uses those identified by IPUMS to be in a correctional facility by the variable "gqtype." The "group quarters + relationship" definition removes from the previous definition individuals who are household heads or other family members and adds individuals with relationship strings of "prisoner" and "convict" who were not identified by the "gqtype" variable in IPUMS. See Section III for details.

To match individuals, I follow the procedure pioneered by Ferrie (1996) and used in Abramitzky, Boustan, and Eriksson (2012). The matching procedure starts with the base year of 1920, 1930, or 1940 and matches backwards to either 10 or 20 years prior. The procedure is as follows:

1. For all censuses to be matched, I begin by standardizing the first and last names of men to address orthographic differences between phonetically equivalent names using the NYSIIS algorithm (Atack et al. 1992). I also recode any common nicknames to standard first names (for example, Will becomes William). I restrict my attention to men in the later census who are unique by first and last name, birth year, race, and state of birth. I do so because, for nonunique cases, it is impossible to determine which of the records should be linked to potential matches in the earlier year.
2. I match observations backwards from the later year to the earlier year using an iterative procedure. I start by looking for a match by first name, last name, race, state of birth, and exact birth year. There are three possibilities:
   (a) if I find a *unique* match, I stop and consider the observation "matched";
   (b) if I find multiple matches for the individual, the observation is thrown out;
   (c) if I do not find a match at this first step, I try allowing the individual's age to be "off" by one year in either direction. Then, if this does not result in a match, I allow the age to be "off" by two years in either direction. I only accept unique matches. If none of these attempts produces a match, the observation is discarded as unmatched.

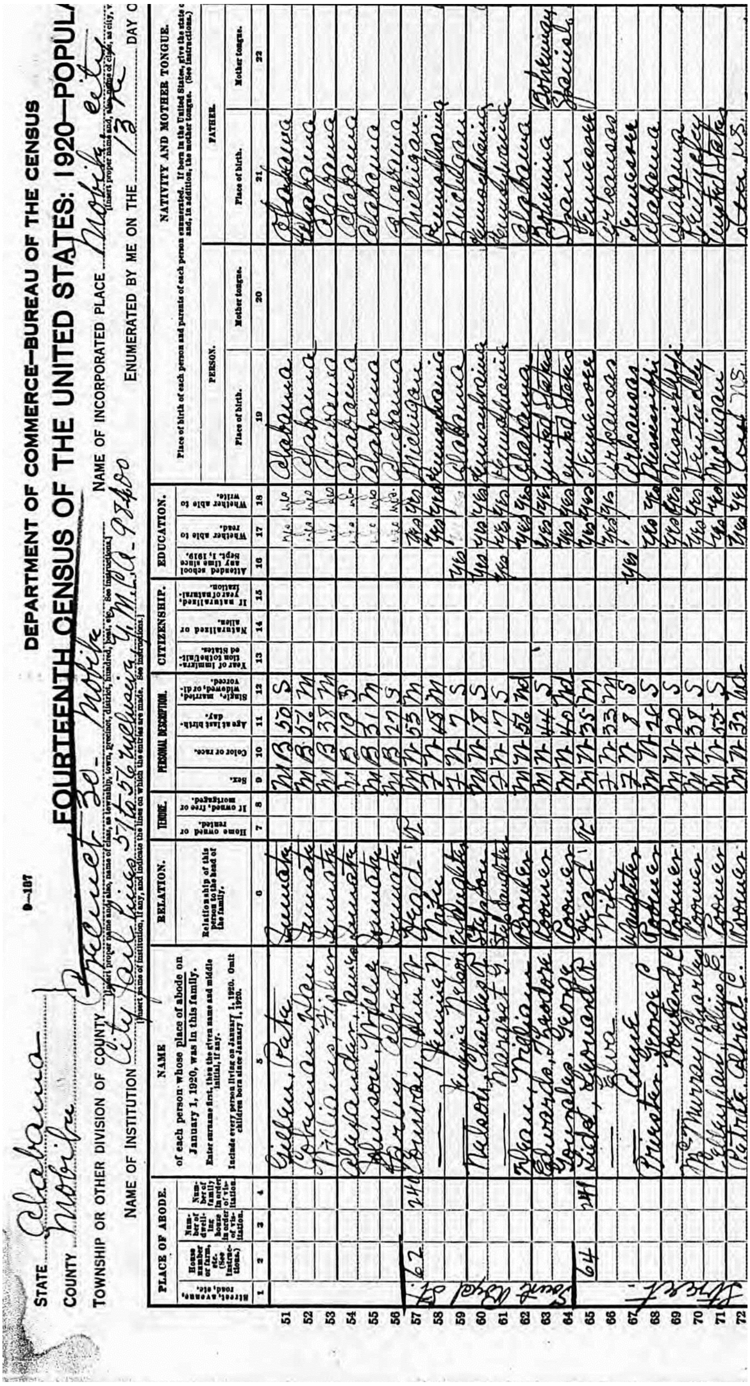


**Figure 2**
*Example Census Image That Is Incorrectly Coded by IPUMS*

**Table 2**
*Sample Sizes and Match Rates by Adult Census Year and Prisoner Status*

| | Population (1) | Matched (2) | Match Rate (3) |
|---|---|---|---|
| 1940 | | | |
| Prisoners | | | |
| Black | 42,208 | 7,237 | 17.1 |
| White | 37,215 | 7,293 | 19.5 |
| Nonprisoners | | | |
| Black | 1,538,921 | 346,284 | 22.5 |
| White | 4,544,734 | 1,414,577 | 31.1 |
| 1930 | | | |
| Prisoners | | | |
| Black | 37,119 | 5,769 | 15.5 |
| White | 31,621 | 5,398 | 17.1 |
| Nonprisoners | | | |
| Black | 1,593,426 | 290,455 | 18.2 |
| White | 3,783,047 | 1,189,455 | 31.4 |
| 1920 | | | |
| Prisoners | | | |
| Black | 19,954 | 3,031 | 15.2 |
| White | 14,951 | 2,401 | 16.1 |
| Nonprisoners | | | |
| Black | 1,158,870 | 257,455 | 22.2 |
| White | 3,085,221 | 843,898 | 27.4 |

Notes: Prisoners and nonprisoners are taken from the full count census indexes provided by Ancestry.com and available on the NBER server. Individuals are matched based on standardized name, age, state of birth, and race. I require exact matches on name, state of birth, and race and require individuals to report age no more than two years off in either direction. The year in the table refers to the adult census year. I restrict the data to men aged 18–35 in the adult census year; men less than or equal to 22 are matched to the previous census, while men 23–35 are matched to a census 20 years prior.

My matching procedure generates a final sample of 31,129 prisoners and 4,342,124 nonprisoners. Sample sizes and match rates are shown in Table 2. Match rates are consistent with the literature, averaging 15–31 percent. Match rates are higher for nonprisoners than prisoners. This could be because prisoners are less literate and so are less likely to report their ages correctly or consistently spell their names. It is also possible that errors in spelling and age are more prevalent in the prisoner sample because the prison warden often reported the names and ages of all prisoners to the census enumerator. Match rates for whites are also about one and one-half times those for black men, also possibly due to literacy and numeracy differences. Collins and Wanamaker (2015) are also less successful at matching black men than white men in a similar time period. To account for the substantial differences in match rates across race and years, I create sample weights equal to the inverse of the match rates by prisoner status, year, and race; this enables me to interpret the coefficients relative to the correct incarceration rate for each year and race.

Individuals can fail to match due to (i) nonunique name–birth state–race combinations, (ii) misreporting of age, and (iii) complete misspellings of the name. Note that mortality cannot account for any failure to match due to starting with the later year and matching backwards. Nonunique combinations account for 52 percent of match failures. Allowing individuals to match within a ten-year age range gains an additional 10 percent, so these are likely misreported ages. Finally, individuals who cannot be found because of differences in name spellings account for the remaining 38 percent. Note that this could be because the individual misspelled their name (likely correlated with socioeconomic status) or because the enumerator or the modern transcriber misspelled it (random).

There is an inherent tradeoff in any matching procedure between match rates, false-positive rates, and representativeness of the resulting sample. I explore representativeness of my primary sample in Section III.D below and reweight my main results later to be representative of the population.

My robustness samples examine the sensitivity of my results to methods that produce lower match rates but also lower false-positive rates. Recently, Bailey et al. (forthcoming) have shown that the standard iterative match procedure from Abramitzky et al. (2012) results in false-positive rates of up to 23 percent. The authors also show substantially lower false-positive rates (around 12 percent) for the Abramitzky et al. (2012) conservative method, which requires individuals to be unique within a five-year band (plus or minus two years in age) in each data set. Therefore, I create a robustness sample in which men are required to be unique within this five-year age band. This results in match rates approximately 30 percent lower but hopefully reduces the number of false links. In additional robustness samples, I also restrict individuals to match only up to one year in age in either direction, match on original names instead of standardized names, and finally allow individuals to match up to five years in age in either direction. This final sample *increases* match rates and false-positive rates and, unsurprisingly, results in insignificant and smaller coefficients.

### *C. The Location of Rosenwald Schools and Assigning Rosenwald Exposure*

Information about the Rosenwald school program is taken directly from Aaronson and Mazumder (2011). The data set of 4,983 schools was compiled from school-level index cards archived at Fisk University. Information available includes school name, county location, year of construction, and some information about funding sources and the size of the school. The earliest Rosenwald schools were located in Alabama in 1913; by 1932, schools had been built in 15 southern states.[14]

The location of the schools was not randomly assigned. Aaronson and Mazumder (2011) find little correlation between preexisting black socioeconomic characteristics and the placement of Rosenwald schools in a county but do find a relationship between

14. Only a few schools were built in Missouri, so I follow Aaronson and Mazumder (2011) and omit Missouri from the analysis. My analysis therefore includes 14 states.

**Table 3**
*Correlation between County Characteristics and Rosenwald Schools*

| | Coverage 1919 (1) | Coverage 1931 (2) | Coverage 1931, Conditional on 1919 (3) | Change in Coverage, 1919–1931 (4) | Sample Mean, *X* |
|---|---|---|---|---|---|
| Log white enrollment, 1910 | –0.009 | 0.023 | 0.029 | 0.033 | 4.256 |
| | (0.011) | (0.023) | (0.022) | (0.023) | |
| Log black enrollment, 1910 | 0.001 | –0.072*** | –0.073*** | –0.735*** | 3.743 |
| | (0.008) | (0.015) | (0.015) | (0.015) | |
| Log jail expenditure, 1902 | –0.001 | 0.001 | 0.001 | 0.001 | –4.985 |
| | (0.002) | (0.005) | (0.004) | (0.005) | |
| Log court expenditure, 1902 | –0.014* | 0.006 | 0.014 | 0.020 | –3.721 |
| | (0.007) | (0.015) | (0.014) | (0.014) | |
| = 1 if lynching 1900–1920 | –0.045** | –0.037 | –0.013 | 0.008 | 0.566 |
| | (0.013) | (0.025) | (0.025) | (0.025) | |
| = 1 if execution 1900–1920 | 0.006 | –0.018 | –0.021 | –0.024 | 0.521 |
| | (0.009) | (0.018) | (0.017) | (0.018) | |
| = 1 if dry in 1910 | 0.004 | 0.009* | 0.007 | 0.005 | 0.882 |
| | (0.003) | (0.005) | (0.005) | (0.004) | |
| Coverage in 1919 | | | 0.542*** | | |
| | | | (0.066) | | |
| $R^2$ | 0.1483 | 0.3488 | 0.3980 | 0.3360 | |
| Mean *Y* | 0.048 | 0.377 | 0.377 | 0.329 | |
| *N* | 879 | 879 | 879 | 879 | |

Notes: Regressions control for total population in 1910, rural population in 1910, population density, the proportion of land that was farmed as plantations, and state fixed effects. When data are not available for certain counties, that observation is replaced as zero, and a dummy for missing is included in the regression. County-level jail and court expenditure taken from the Census of Government in 1902. White and black enrollment is taken from Carruthers and Wanamaker (2013). Lynchings are from the Project Hal database (2004). Dry counties are defined as in ICPSR 8343 (Sechrist 2012), and executions are taken from ICPSR 8451 (Espy and Smykla 2004).

white literacy and school construction. Carruthers and Wanamaker (2013) argue that schools were more likely to be built in larger counties with higher urbanization, per-pupil spending, and enrollment of black youth. I would ideally check whether early incarceration rates predict whether a county has a school, but incarceration rates by county are not published, and individuals incarcerated would have to be collected by hand in the 1900 and 1910 censuses. I do, however, look at a number of variables related to law enforcement in the years leading up to the establishment of Rosenwald schools.

Table 3 investigates three measures of Rosenwald coverage: coverage in 1919, coverage in 1931, and the change in coverage between 1919 and 1931. By coverage

I refer to the percent of black rural children who could attend a Rosenwald school. As the first explanatory variable, I look at school enrollment in 1910, by race. Variables that might be correlated with crime or the justice system in general include the log of jail and court expenditures from the 1902 Census of Government, indicators for whether there was a lynching or an execution in the county between 1900 and 1920, and an indicator for whether the county was dry by 1910. The only variable that predicts coverage in 1919 is lynching—counties that had a lynching had students who were 4.5 percentage points less likely to have access to a Rosenwald school in 1919 than those which did not, and the effect is statistically significant. The effect of lynching does not persist to explain coverage by 1931 or the change in coverage. Black school enrollment in 1910 negatively predicts coverage in 1931 and the change in coverage between 1919 and 1931. This means that counties that already had higher black enrollment built fewer schools. Other variables do not predict Rosenwald coverage.

For the full southern-born sample, I assign to individuals a measure of their likely exposure to a Rosenwald school based on their age and county of residence during childhood. Following Aaronson and Mazumder, I calculate two measures of exposure. The first is a simple count of the years between ages 7 and 13 in which a child had a Rosenwald school in his county; I then scale this so that it lies between 0 and 1 to measure the proportion of the relevant childhood period during which a child had a school in their county. This measure is referred to as "school in county" in the tables of results. The second measure, which takes into account that Rosenwald schools were not large enough for all students, is the proportion of black students in the county who could be served by a Rosenwald school added over the years during which the child was between 7 and 13; I also scale this to lie between 0 and 1. This measure is smaller than the first, but is a better measure of how likely a student was to attend a Rosenwald school.[15] Therefore, this measure is used in most analysis and is referred to as "likely seats."[16] The counts of potential students in a county are taken from Aaronson and Mazumder (2011), who used the census indexes on Ancestry.com to count all rural children ages 7–13 within a county in each census year and then extrapolated between years. I follow their assumption that a classroom could hold 45 students.

### *D. Representativeness of the Matched Sample and Summary Statistics*

A concern with any matching procedure is whether the matched data set is representative of the population. Most literature (Abramitzky et al. 2012) finds that the matched sample has slightly higher socioeconomic status than the full population. I explore this possibility in Table 4. The first and fourth columns present the means for the relevant population. The second and fifth columns show the differences between the matched sample and population, weighted by the inverse of the match rate within prison–race–census year cells. The third and sixth columns show the differences between the matched sample and population, reweighting the matched sample using inverse probability

15. For example, if a school could fit half of the students in a county, then each individual living in that county would get one-half year of exposure.

16. The first measure, "school in county," corresponds to the Aaronson and Mazumder $Rose_{bct}$ measure, while the second measure, "likely seats," corresponds to $E_{bc}$.

**Table 4**
*Comparing the Matched Sample to the Full Population*

| | Black | | | White | | |
|---|---|---|---|---|---|---|
| | Population (1) | Difference: Matched – Population (2) | Difference, Weighted (3) | Population (4) | Difference: Matched – Population (5) | Difference, Weighted (6) |
| **Panel A: Outcomes in Adult Census Year** | | | | | | |
| = 1 if literate | 0.766 | 0.019*** | –0.0004 | 0.947 | 0.009*** | –0.0001 |
| | | (0.001) | (0.001) | | (0.0001) | (0.0002) |
| Years of education (1940) | 5.863 | 0.237*** | –0.001 | 8.828 | 0.226*** | 0.0005 |
| | | (0.006) | (0.008) | | (0.004) | (0.004) |
| = 1 if living outside South | 0.117 | –0.019*** | 0.001* | 0.100 | –0.031*** | 0.001* |
| | | (0.0005) | (0.001) | | (0.0002) | (0.001) |
| Age | 25.81 | –0.223** | 0.010 | 25.83 | –0.164*** | 0.001 |
| | | (0.007) | (0.008) | | (0.003) | (0.001) |
| **Panel B: Other Moments of Distributions** | | | | | | |
| Return to education (1940) | 0.081 | –0.002 | 0.006* | 0.117 | –0.0000 | 0.0002** |
| | | (0.002) | (0.005) | | (0.001) | (0.001) |
| = 1 if never attended school | 0.0517 | –0.010*** | 0.001* | 0.016 | –0.006*** | 0.0003* |
| | | (0.0004) | (0.001) | | (0.0001) | (0.0001) |
| = 1 if completed at least 8 years of education | 0.306 | 0.015*** | –0.017 | 0.645 | 0.021*** | –0.008* |
| | | (0.001) | (0.001) | | (0.001) | (0.005) |

(*continued*)

**Table 4** (*continued*)

| | Black | | | White | | |
|---|---|---|---|---|---|---|
| | Population (1) | Difference: Matched – Population (2) | Difference, Weighted (3) | Population (4) | Difference: Matched – Population (5) | Difference, Weighted (6) |
| **Panel C: Childhood Census Year Characteristics** | | | | | | |
| Exposure ("likely seats") | 0.051 | 0.0009** | 0.0008* | 0.060 | 0.002*** | 0.0001 |
| | | (0.0005) | (0.0005) | | (0.0003) | (0.0005) |
| Exposure ("school in county") | 0.257 | –0.008** | 0.0006 | 0.226 | 0.002 | 0.001 |
| | | (0.001) | (0.001) | | (0.002) | (0.002) |
| = 1 if lives in urban area | 0.133 | –0.011*** | –0.004* | 0.144 | 0.002*** | –0.006*** |
| | | (0.002) | (0.002) | | (0.001) | (0.001) |
| = 1 if farm household | 0.566 | 0.017*** | 0.005** | 0.628 | 0.016*** | 0.009** |
| | | (0.002) | (0.002) | | (0.002) | (0.005) |
| = 1 if head owns house/farm | 0.239 | 0.022*** | 0.001 | 0.523 | 0.047*** | 0.006*** |
| | | (0.002) | (0.002) | | (0.001) | (0.002) |
| = 1 if household head is literate | 0.536 | 0.035*** | –0.002 | 0.880 | 0.039*** | –0.0006 |
| | | (0.002) | (0.002) | | (0.001) | (0.001) |
| = 1 if enrolled in school | 0.262 | 0.044*** | 0.0002 | 0.399 | 0.055*** | 0.003*** |
| | | (0.002) | (0.002) | | (0.001) | (0.001) |

Notes: $N = 4{,}845{,}279$ (black); $N = 10{,}628{,}887$ (white). Sample includes male prisoners and nonprisoners in 1920, 1930, and 1940 matched to childhood census county to assign Rosenwald exposure. I restrict to men aged 18–35 years. Columns 1 and 3 report means and standard deviations from the population. Coefficients in Columns 2 and 4 are from a regression of the outcome of interest on a dummy for being in the matched sample. Columns 3 and 6 replicate Columns 2 and 4 but use weights created using the inverse probability weights. The return to education is estimated by regressing log wage on education, an indicator for matched, and the interaction between education and matched. The coefficient on education is reported in the "population" column and the interaction reported in the "difference" columns. For this regression, I restrict to nonprisoners because of the lack of consistent wage reporting for prisoners. Regressions include robust standard errors. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

weights (IPW) so that it matches the population on observable characteristics (Bailey et al., forthcoming).

Panel A looks at outcomes in the adult census year. My main regressions weight all individuals by the inverse of the match rate within a prisoner–race–year cell, so I do not compare prisoner status between the population and matched sample. I find that individuals in the matched sample are slightly more literate and have higher levels of education. Matched whites are 0.09 percentage points more likely to be literate (less than 1 percent of the mean), and matched blacks are 1.9 percentage points less likely to be literate than the population (2.4 percent of the mean). Those who are matched have about 0.23 more years of school than the population. Matched individuals are slightly less likely to be living outside of the South as an adult; lower match rates among migrants likely comes from individuals who change their name upon moving North or who are less able to remember their age correctly when there is no other family member to consult with when the census enumerator arrives. Differences are small and insignificant when using the IPW, which shows that the IPW procedure was successful.

Panel B looks beyond means to other moments of the distribution of education as well as the return to education. I find that matched men were more likely both ever to have attended school and to have completed at least eight years of education. The return to school is statistically the same for the two groups. When reweighting with IPW, these patterns flip, if anything, and remain significant only at the 10 percent level.[17]

Panel C looks at whether matched individuals differ from nonmatched individuals in childhood. Rosenwald exposure using the "likely seats" measure is slightly higher among the matched sample than the population, but this difference is erased with the IPW. Exposure for blacks using "school in county" is slightly lower for the matched sample than the population. None of the differences are large in magnitude. I find that the matched sample is only slightly (0.11 pp) less likely to be urban. Matched individuals come from higher socioeconomic backgrounds than the population, with more literate household heads and household heads who are more likely to own their house or farm. The men themselves in the matched sample were more likely to be enrolled in school in the childhood year. Again, these differences become much smaller in magnitude and mostly insignificant after reweighting the data.

Summary statistics are shown in Table 5. Black individuals are more than three times more likely than whites to be incarcerated in my sample in all years. Incarceration rates for both races are increasing over time. The incarceration rate for black men peaks at 2.55 percent in 1940. Rates are slightly higher than the rates given in Figure 1, adjusted for gender, because ages 18–35 have the highest risk of incarceration. Prisoners have slightly higher levels of exposure to Rosenwald schools. The overall average exposure of 0.059 ("likely seats") or 0.232 ("school in county") is similar to that of Aaronson and Mazumder because the cohorts in my sample are almost identical to theirs.[18]

17. The matched sample also reproduces Aaronson and Mazumder (2011) results on school attendance, suggesting the relationship between exposure and school attendance is similar in the matched sample and the population (results not shown).

18. They include ages 7–17 in 1900–1930. Their oldest cohort therefore was born in 1883 and the youngest in 1923. My oldest cohort (35 year olds in 1920) was born in 1885, and my youngest (18 year olds in 1940) was born in 1922.

**Table 5**
*Summary Statistics*

| | Black | | White | |
|---|---|---|---|---|
| | Prisoner | Nonprisoner | Prisoner | Nonprisoner |
| Sample size | 15,722 | 894,194 | 15,407 | 3,448,072 |
| In prison (weighted) | | | | |
| 1920 | 1.350 | | 0.206 | |
| 1930 | 2.232 | | 0.683 | |
| 1940 | 2.552 | | 0.724 | |
| Childhood characteristics: | | | | |
| Exposure, likely seats | 0.069 | 0.059 | 0.076 | 0.059 |
| | (0.138) | (0.129) | (0.175) | (0.154) |
| Exposure, school in county | 0.336 | 0.290 | 0.268 | 0.216 |
| | (0.422) | (0.408) | (0.397) | (0.373) |
| = 1 if living in urban area | 0.237 | 0.156 | 0.223 | 0.198 |
| | (0.425) | (0.363) | (0.416) | (0.399) |
| Adult outcomes: | | | | |
| Age | 25.62 | 25.71 | 25.59 | 25.77 |
| | (4.769) | (5.077) | (4.700) | (5.139) |
| Living outside the South | 0.223 | 0.134 | 0.217 | 0.086 |
| | (0.417) | (0.340) | (0.412) | (0.281) |
| Education (1940 Only) | 5.405 | 6.086 | 7.008 | 9.054 |
| | (3.137) | (3.323) | (3.160) | (3.364) |
| Literacy | 0.801 | 0.841 | 0.931 | 0.968 |
| | (0.398) | (0.365) | (0.253) | (0.173) |

Notes: $N = 4{,}373{,}395$. Sample includes prisoners and nonprisoners in 1920, 1930, and 1940, linked to childhood census locations to assign Rosenwald exposure. I restrict to men born in the South but living anywhere as an adult and to ages 18–35 in the adult census year. Urban is defined as living in a place with more than 2,500 residents in the childhood census year. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

The probability of being found outside of the South as an adult is higher for prisoners than nonprisoners, a difference due to higher incarceration rates outside of the South. For the 1940 census only, I can examine education levels. I find, as expected, that prisoners are less educated than nonprisoners. Black prisoners have on average 5.41 years of schooling compared to 6.09 for black nonprisoners. The gap is larger for whites: prisoners have 7.01 years of school compared to 9.05 years for nonprisoners. Literacy is available in the 1920 and 1930 censuses. For the 1940 census, I define an individual as literate if they have three or more years of school (Collins and Margo 2006). As expected, prisoners are less literate than nonprisoners for both races, although literacy rates are high at 80–97 percent.

## IV. Estimation Strategy

### *A. Reduced Form Estimation of the Effect of Rosenwald Schools on Incarceration*

I estimate the effect for men of being exposed to a Rosenwald school for seven years, for ages 7–13, on the probability of being incarcerated as an adult. My estimation strategy exploits variation across cohorts within a county in exposure to a Rosenwald school at relevant ages and the fact that Rosenwald schools were targeted to rural areas. In most of my analysis, I also contrast black and white students in the same county, cohort, and with the same rural residence status.[19]

Equation 1 is restricted to the black male sample only. Using a linear probability model, I estimate:

$$(1)\quad prisoner_{iact} = \alpha_c + \gamma_t + \theta_a + \beta_1 rural_{ict} + \beta_2 exposure_{ict} + \beta_3 exposure_{ict} * rural_{ict} + X_i B + \varepsilon_{ict}$$

where *prisoner* equals 100 if individual *i* (of age *a* who lived in county *c* in childhood at census date *t*) is incarcerated at the time of the adult census.[20] I scale the original indicator outcome variable by 100 so that coefficients can be interpreted as percentage point changes. I include child and adult census year fixed effects, age fixed effects, and childhood county times childhood census year fixed effects.[21] Finally, I control for household-level characteristics in the childhood year, including home ownership status, literacy of the household head, and indicators for household head occupation categories.

Because urban areas were not meant to receive schools, the main effect of *exposure* controls for any trends within a county in the outcome variable that are correlated with exposure but that affect urban and rural areas similarly. Therefore, the coefficient of interest is $\beta_3$, which measures the change in incarceration for an extra year of exposure to a Rosenwald school, and is identified by comparing cohorts in the same county who were exposed to the new school with those who were too old to benefit.

One concern with this specification is that there might be factors that are changing over time within rural versus urban parts of counties and that affect cohorts differentially. To address this, I add white men as a comparison group. This allows me to account for any local factors that are changing within a county over time, that affect rural and urban areas differently, but that have similar effects on whites and blacks.

My main estimating equation therefore is as follows:

$$(2)\quad prisoner_{iarsct} = \alpha_c + \gamma_{tr} + \theta_{ar} + \pi_{st} + \beta_1 black_i + \beta_2 exposure_{ict} + \beta_3 rural_{it} + \beta_4 black_i * rural_{it} + \beta_5 exposure_{ict} * rural_{it} + \beta_6 exposure_{ict} * black_i + \beta_7 rural_{it} * black_i * exposure_{ict} + \varepsilon_{ict}$$

19. I create sample weights that are inversely proportional to the match rate within a census year–race–prisoner status cell because match rates differ by race, census year, and prisoner status.

20. Results from a probit regression are quantitatively similar. However, probit regression is inconsistent (Greene 2004) in a regression with fixed effects, so I prefer the linear probability model.

21. In my first results table (Table 6), I also show results with no geographical fixed effects and with state fixed effects, and with county fixed effects.

for individual of race *r* and with the remaining subscripts as in Equation 1. I add race-specific childhood census year fixed effects, and age fixed effects interacted with race. I continue to control for childhood county times childhood census year fixed effects. The coefficient of interest in this equation is $\beta_7$, which measures the additional effect of a year of exposure to a local Rosenwald school on black rural youth above any effect that there may be of Rosenwald schools on white rural youth or black urban youth.

To the extent that Rosenwald resources may have been diverted to rural white schools, $\beta_5$ picks up any effect on white rural individuals. It is possible that Rosenwald funds freed up money in the local budget that was then siphoned off to white schools. Carruthers and Wanamaker (2013) find significant crowd-out of the Rosenwald initiative. An additional dollar of Rosenwald spending was associated with another \$2.12 of public spending for black and white schools, but 63 percent of this gain accrued to white schools.[22] For this reason, I control for the effect of Rosenwald exposure on whites ($\beta_5$) and interpret $\beta_7$ as the differential effect of Rosenwald schools on incarceration for blacks. If incarceration rates were rising differentially in counties with Rosenwald schools, then we expect $\beta_6$ to be positive.

My estimate of $\beta_7$ can still be biased if there are local events that are correlated with the timing of construction of Rosenwald schools, are correlated with trends in incarceration, and affect the older (unaffected) and younger cohorts differentially. Additionally, by comparing white and black children, these factors must affect the two races differently. Finally, this factor must affect rural and urban areas in different ways. However, it is hard to conceive of omitted variables that meet all of these criteria. For example, we might think that some counties have more racist attitudes, which would lead them not to build schools and to also tend to incarcerate black men more often, but these attitudes would have to be changing over time to affect the two cohorts differently. Finally, note that Rosenwald school exposure is measured in childhood, but incarceration is measured at least ten years later, so any county-level confounding factor in terms of attitudes towards incarceration during childhood and then attitudes during adulthood would have to be constant throughout this gap. Furthermore, a majority of prisoners commit crimes outside of their childhood county of residence, so it is unlikely that county-level trends in police expenditures, for example, would be a confounding factor. By including county–year fixed effects, I control for anything happening at a county level and changing over time that could confound the results.

### *B. Estimating the Effect of Rosenwald Schools on other Outcomes*

Thus far, my main interest has been the direct effect of Rosenwald schools on incarceration. One likely channel through which Rosenwald schools reduced incarceration is by increasing the educational attainment of its black pupils. They could also have encouraged migration to the higher wage North where incarceration rates were higher. As a result, I consider education and literacy as well as migration as possible channels through which Rosenwald schools reduced incarceration.

22. In light of these findings, they argue that Aaronson and Mazumder's results are consistent with higher marginal returns to school spending on black schools.

These results complement Aaronson and Mazumder (2011), who show that Rosenwald schools increased school enrollment of affected cohorts and improved educational attainment of World War II enlistees.[23] My estimating equation follows Equation 2 above, where *prisoner* is replaced with the adult outcome of interest among education, literacy, and migration status.

I use the first-stage estimates here to calculate a Wald estimate of the effect of education on incarceration—that is, what is the predicted change in incarceration rates for someone who obtains an extra year of school? This is calculated by dividing the reduced form estimate of the effect of Rosenwald schools on incarceration by the first-stage estimate of the effect of Rosenwald schools on years of education. In order for this to be interpreted as an instrumental variable estimate, I must be willing to assume that Rosenwald schools only affected incarceration through years of education. In fact, access to schooling may have reduced incarceration in other ways, namely by keeping children occupied during the day or increasing school quality.

This school-building program was taking place during a period of high levels of migration to the North. I consider migration as a potential mechanism for reductions in incarceration in my later analysis, but I also note here that high rates of black out-migration was a potential motivation for counties to make use of Rosenwald funds despite the overwhelming representation of whites on local school boards. Margo (1990) argues that investments in education were one way that southern governments could discourage migration to the North.

## V. Results

### *A. Reduced Form Effects of Rosenwald Exposure on Incarceration Later in Life*

My empirical analysis begins by estimating the relationship between exposure to a Rosenwald school and incarceration later in life. In Table 6, I start by estimating Equation 1, which restricts to black individuals only. Given that incarceration rates were so dissimilar for whites and blacks in this time period, and that whites and blacks were treated differently by justice systems in the Jim Crow era, whites may not be a good comparison group for blacks. Panel A uses the first measure of exposure, "likely seats," which refers to the proportion of the time a school was in a county between ages 7 and 13, weighted by the probability of having a seat. Panel B defines exposure using the "school in county" measure, which is measured as the proportion of time a student had a school anywhere in their county at ages 7–13.

The coefficient of interest is on *exposure*rural*. Columns 1–4 gradually add fixed effects. The first column has no childhood location fixed effects. Full Rosenwald exposure during childhood reduces the probability of being incarcerated as an adult by 1.92 percentage points. Column 2 adds childhood state fixed effects, Column 3 adds childhood county fixed effects, and Column 4 adds childhood county times year fixed effects to account for factors changing in a county over time that affect both races

23. Aaronson and Mazumder's paper did not use the 1940 census to look at effects on education because county of residence in 1935 was not available in the 1 percent IPUMS sample.

**Table 6**
*Reduced Form Results, Effect of Full Rosenwald Exposure on Incarceration, Blacks Only*

| | Outcome | | | |
|---|---|---|---|---|
| | = 100 if in Prison (1) | = 100 if in Prison (2) | = 100 if in Prison (3) | = 100 if in Prison (4) |
| **Panel A: Likely Seats Measure of Exposure** | | | | |
| Exposure*rural | –1.921*** | –2.096*** | –2.530*** | –2.507*** |
| | (0.668) | (0.593) | (0.614) | (0.672) |
| Exposure | 2.140** | 2.350** | 2.683** | 2.787** |
| | (0.708) | (0.599) | (0.639) | (0.793) |
| Rural | –0.581** | –0.552*** | –0.353*** | –0.336*** |
| | (0.099) | (0.073) | (0.081) | (0.080) |
| Exposure measure | "Likely seats" | "Likely seats" | "Likely seats" | "Likely seats" |
| County–year controls? | Yes | Yes | Yes | No |
| Fixed effects | None | State | County | County–year |
| Mean exposure | 0.059 | 0.059 | 0.059 | 0.059 |
| Sample mean, black | 2.105 | 2.105 | 2.105 | 2.105 |
| $R^2$ | 0.003 | 0.004 | 0.006 | 0.010 |
| *N* | 906,563 | 906,563 | 906,563 | 906,563 |
| | = 100 if in Prison (5) | = 100 if in Prison (6) | = 0 if in Prison (7) | = 0 if in Prison (8) |
| **Panel B: School in County Measure of Exposure** | | | | |
| Exposure*rural | –0.657*** | –0.744*** | –0.783*** | –0.764*** |
| | (0.178) | (0.147) | (0.153) | (0.179) |
| Exposure | 0.540** | 0.747*** | 0.721*** | 0.444* |
| | (0.227) | (0.169) | (0.182) | (0.258) |
| Rural | –0.477*** | –0.437*** | –0.269*** | –0.254*** |
| | (0.107) | (0.073) | (0.083) | (0.090) |
| Exposure measure | "School in county" | "School in county" | "School in county" | "School in county" |
| County–year controls? | Yes | Yes | Yes | No |
| Fixed effects | None | State | County | County–year |

(*continued*)

**Table 6** (*continued*)

| | = 100 if in Prison (5) | = 100 if in Prison (6) | = 0 if in Prison (7) | = 0 if in Prison (8) |
|---|---|---|---|---|
| Mean exposure | 0.291 | 0.291 | 0.291 | 0.291 |
| Sample mean, black | 2.105 | 2.105 | 2.105 | 2.105 |
| $R^2$ | 0.003 | 0.004 | 0.006 | 0.011 |
| *N* | 906,563 | 906,563 | 906,563 | 906,563 |

Notes: Outcome = 100 if in prison in the adult year. The coefficients in column are interpreted as percentages rather than proportions. Black means of the outcome variable are given in the row labeled "Sample mean, black." Regressions include age, year, and county fixed effects in Column 3 and county–year fixed effects in Column 4, where year refers to the childhood census year, and county refers to the childhood census county. I restrict to ages 18–35. Standard errors are clustered by childhood census county. Sample includes prisoners and nonprisoners in 1920, 1930, and 1940, linked to childhood census locations to assign Rosenwald exposure. Rural is defined as living in a place with less than 2,500 inhabitants in the childhood census year. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

and rural and urban areas similarly. I find that the coefficient is quite stable across specifications. Rosenwald exposure reduces incarceration by between 1.9 and 2.5 percentage points.

As expected, the coefficient on *rural* is negative and significant—that is, men who grow up in rural areas are less likely to be incarcerated as adults, probably because they are less likely to live in urban places as adults. The main effect of Exposure is positive and sometimes significant. This is likely because counties with more Rosenwald schools also had upward trends in incarceration, and this was more pronounced in more urban counties; this is picked up by the main effect of exposure.

In Panel B, I find that having a Rosenwald school in the county of residence as a child for seven years reduces incarceration by between 0.65 and 0.78 percentage points. The fact that the "likely seats" measure produces larger points estimates is what we would expect if we think about these two measures as "intent to treat" measures. The first measure is twice as likely to lead to an extra year of schooling (Aaronson and Mazumder 2011). It is also a better measure of the likelihood that a student attends a Rosenwald school.

In Table 7, I add whites to the regression as an additional comparison group. Now the effect of Rosenwald exposure on rural black men is the coefficient on *black**rural*exposure*. Using the first measure of exposure, full exposure to a Rosenwald school reduces the probability of being a prisoner by 1.96 percentage points. This number falls to 0.62 percentage points when using the second measure of exposure. As in Table 4, the effect of Rosenwald exposure on urban blacks is positive, suggesting that places where Rosenwald schools were built had upward trends in incarceration rates. I do not see any effect on whites. The rest of the analysis in this paper uses whites as a control group.

**Table 7**
*Effect of Full Rosenwald Exposure on Incarceration, Both Races*

| | = 100 if in Prison (1) | = 100 if in Prison (2) |
|---|---|---|
| Black*rural*exposure | −1.963*** | −0.618** |
| | (0.700) | (0.197) |
| Exposure*rural | 0.028 | 0.001 |
| | (0.140) | (0.05) |
| Black*exposure | 1.896* | 0.424 |
| | (0.791) | (0.259) |
| Black*rural | −0.960*** | −0.867*** |
| | (0.102) | (0.120) |
| Exposure | 0.133 | −0.004 |
| | (0.187) | (0.078) |
| Black | 1.349*** | 1.271*** |
| | (0.121) | (0.129) |
| Rural | 0.081*** | 0.079** |
| | (0.029) | (0.031) |
| Exposure measure | "Likely seats" | "School in county" |
| County controls? | No | No |
| Fixed effects | County–year | County–year |
| Mean exposure | 0.059 | 0.232 |
| Sample mean, black | 2.105 | 2.105 |
| $R^2$ | 0.009 | 0.009 |
| $N$ | 4,363,109 | 4,362,109 |

Notes: Outcome = 100 if in prison in the adult year. The coefficients in column are interpreted as percentages rather than proportions. Black means of the outcome variable are given in the row labeled "Sample mean, black." Regressions include age, black*age, year, black*year, and county–year fixed effects, where year refers to the childhood census year, and county refers to the childhood census county. I restrict to ages 18–35. Standard errors are clustered by childhood census county. Sample includes prisoners and nonprisoners in 1920, 1930, and 1940, linked to childhood census locations to assign Rosenwald exposure. Rural is defined as living in a place with less than 2,500 inhabitants in the childhood census year. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

The coefficients are all relative to an average base incarceration rate for blacks of 2.55 percent in 1940 or 2.1 percent for all three years. This implies that full Rosenwald exposure would reduce the probability of incarceration by up to 100 percent of the 1940 mean by the first measure and 33 percent of the mean by the second measure. Note, however, that average exposure in the sample is 0.05 and 0.29 using the two measures. Therefore, the average level of exposure in my sample reduces incarceration rates by 8.8 and 4.9 percent of the 1940 mean.

### B. Effects of Rosenwald Exposure on Education and Migration

The above results suggest that Rosenwald schools reduced the criminality of black students later in life. I look at three main mechanisms in this section: literacy, completed years of education, and migration. The most direct mechanism is through education, which increases the opportunity cost of crime by increasing labor market opportunities.

Table 8 begins with data from all three adult census years in Columns 1–3. I use literacy as reported in the census in 1920 and 1930. For 1940, I define an individual as literate if they report having completed three or more years of education (Collins and Margo 2006). Column 1 replicates the effect of Rosenwald exposure on incarceration from Table 7. In Column 2, we see that full Rosenwald exposure increased literacy rates of rural black men by 5.8 percentage points. In Column 3, I define the outcome variable equal to one if the individual is living outside of the South in 1940. Full Rosenwald exposure increases the probability of living outside the South as an adult by an insignificant 0.7 percentage points. This is consistent with the small effects on migration found by Aaronson and Mazumder (2011).[24]

In Columns 4 and 5, I restrict to the 1940 sample where education is available. I find that full Rosenwald exposure reduces the probability of incarceration by 1.42 percentage points in this sample. Education levels increase by 1.277 years with full Rosenwald exposure. This is similar to the 1.2 years found by Aaronson and Mazumder.

I calculate a Wald estimate of the social return to education on the basis of the results above by dividing the reduced form coefficient by the first-stage coefficient. By this estimate, one more year of school would reduce the likelihood of incarceration later in life by about 1.1 percentage points. This estimate would be valid two-sample instrumental variables estimate if the only mechanism through which Rosenwald schools affected incarceration was through education (Angrist and Krueger 1992; Solon and Inoue 2010). However, it is likely that the Rosenwald program affected criminality through multiple channels. Wald estimates are presented here simply to give an idea of the magnitude of the coefficients that are estimated in the reduced form analysis.

To compare, the ordinary least squares (OLS) coefficient from regressing incarceration on education (controlling for age and birth state fixed effects) is –0.33 for blacks. However, this hides nonlinearities: the effect is the largest for low levels of school (–0.45 for education equal to zero) and becomes as large as –0.77 for black men living outside the South, where incarceration rates are higher. While the OLS coefficient is always larger in magnitude than the Wald estimate, they are both negative and large. Furthermore, this suggests that years of educational attainment is not the only mechanism through which Rosenwald schools reduced incarceration. For example, higher school quality would mean that an additional year of school reduces incarceration more for those attending Rosenwald schools than those not attending schools. Finally, if anything, the OLS estimate could place a lower bound on the social return to school in this context.

Next, in Table 9, I ask whether the effects on literacy and migration are large enough to explain the full coefficient in the main incarceration regression. I add migration and

24. The authors only find a significant effect of Rosenwald exposure on migration for those 17–21 years old; furthermore, their results pool both genders. My own analysis of the data provided finds that these results are driven by women and that there is no statistically significant effect for men.

**Table 8**
*Effect of Rosenwald Exposure on Literacy, Education, and Migration, Full Sample*

| | 1920–1940 | | | 1940 | |
|---|---|---|---|---|---|
| Outcome: | = 100 if in Prison (1) | = 1 if Literate (2) | = 1 if Outside South as adult (3) | = 100 if in Prison (4) | Years of Education (5) |
| Black*exposure*rural | –1.963*** | 0.058** | 0.007 | –1.429* | 1.277*** |
| | (0.700) | (0.014) | (0.023) | (0.772) | (0.348) |
| Exposure*rural | 0.023 | 0.005 | 0.031** | 0.078 | –0.095 |
| | (0.140) | (0.005) | (0.006) | (0.158) | (0.110) |
| Black*exposure | 1.896** | 0.010 | –0.007 | 1.871* | –0.305 |
| | (0.736) | (0.022) | (0.022) | (0.757) | (0.363) |
| Black*rural | –0.960*** | –0.046*** | –0.033*** | –1.167*** | –0.324*** |
| | (0.102) | (0.005) | (0.006) | (0.148) | (0.118) |
| Rural | 0.081*** | –0.002** | –0.009*** | 0.081* | –0.675*** |
| | (0.029) | (0.001) | (0.002) | (0.045) | (0.039) |

| | | | | | |
|---|---|---|---|---|---|
| Exposure | 0.133 | –0.002 | –0.024** | 0.032 | 0.659*** |
| | (0.187) | (0.004) | (0.008) | (0.166) | (0.110) |
| Black | 1.349** | –0.088*** | 0.116* | 2.781*** | –2.074*** |
| | (0.121) | (0.006) | (0.008) | (0.146) | (0.119) |
| Exposure measure | "Likely seats" | "Likely seats" | "Likely seats" | "Likely seats" | "Likely seats" |
| Fixed effects | County–year | County–year | County–year | County | County |
| Mean exposure | 0.059 | 0.059 | 0.059 | 0.131 | 0.131 |
| Sample mean, black | 2.105 | 0.785 | 0.153 | 2.552 | 6.120 |
| $R^2$ | 0.009 | 0.088 | 0.072 | 0.255 | 0.008 |
| $N$ | 4,362,109 | 4,362,109 | 4,362,109 | 1,730,760 | 1,775,391 |

Notes: Outcome = 100 if in prison in the adult year. The coefficients in column are interpreted as percentages rather than proportions. Black means of the outcome variable are given in the row labeled "Sample mean, black." Regressions include age, black*age, year, black*year, and county–year fixed effects, where year refers to the childhood census year, and county refers to the childhood census county. I restrict to ages 18–35. Standard errors are clustered by childhood census county. Regressions use the "Likely seats" measure of exposure. Sample includes prisoners and nonprisoners in 1920, 1930, and 1940, linked to childhood census locations to assign Rosenwald exposure. Rural is defined as living in a place with less than 2500 inhabitants in the childhood census year. Columns 4 and 5 restrict to 1940 where education is available. ***$p<0.01$, **$p<0.05$, *$p<0.10$.

**Table 9**
*Effect of Rosenwald Exposure on Incarceration, Including Covariates in Main Regression*

| | Original (1) | With Covariates (2) |
|---|---|---|
| Black*rural*exposure | –1.963*** | –1.962** |
| | (0.700) | (0.707) |
| = 1 if literate | | –0.869*** |
| | | (0.039) |
| = 1 if living outside the South as adult | | 1.142*** |
| | | (0.036) |
| Exposure measure | "Likely seats" | "Likely seats" |
| County controls? | No | No |
| Fixed effects | County–year | County–year |
| Mean exposure | 0.059 | 0.059 |
| Sample mean, black | 2.105 | 6.120 |
| $R^2$ | 0.009 | 0.010 |
| $N$ | 4,362,109 | 4,362,109 |

Notes: Outcome = 100 if in prison in the adult year. The coefficients in column are interpreted as percentage points rather than proportions. Black means of the outcome variable are given in the row labeled "Sample mean, black." Regressions include age, black*age, year, black*year, and county–year fixed effects, where year refers to the childhood census year, and county refers to the childhood census county. I restrict to ages 18–35. Standard errors are clustered by childhood census county. Sample includes prisoners and nonprisoners in 1920, 1930, and 1940, linked to childhood census locations to assign Rosenwald exposure. Rural is defined as living in a place with less than 2,500 inhabitants in the childhood census year. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

literacy to the regression with incarceration as the outcome, reporting the main effect of Rosenwald exposure and individual effects of migration and literacy. Unsurprisingly, being literate is associated with a 0.63 percentage point lower probability of being incarcerated. Migration is associated with higher incarceration rates of 0.578 percentage points. The main effect of Rosenwald exposure on incarceration does not change, remaining 1.96 and significant. I take this to mean that literacy and migration are important channels, but they do not explain the full effect.

## VI. Robustness of Main Results

### *A. Robustness to Specification, Matching, Weighting, and Definition of Incarceration*

The main results illustrate large negative effects of Rosenwald exposure on incarceration. In Online Appendix A, I show that these results are robust to alternative specifications, different matching procedures, different definitions of incarceration, and different weighting schemes. Next, I show that results are similar in unmatched data, although those data have many drawbacks. Finally, I conduct a permutation exercise that reshuffles Rosenwald exposure randomly throughout eligible counties and show that the main coefficient is larger in magnitude than almost any other assignment of Rosenwald schools.

In Online Appendix Table A.1, I include different specifications in the main regressions. I control for a variety of different trends and interactions of census year, urban–rural, race, and birth year fixed effects. The coefficients all stay negative and large, though lose significance sometimes. I view the smallest of these, –0.81 ("likely seats") or –0.17 ("school in county") as the lower bound of the true effects of Rosenwald exposure on incarceration.

In Online Appendix Table A.2, I present results with different matching procedures. I reduce the likelihood of false matches at a cost of a lower match rate by (i) requiring individuals to be unique within a five-year age band, (ii) also allowing individuals to only misreport age by up to one year, (iii) using the iterative match but allowing only up to a one-year age discrepancy, and (iv) matching on exact instead of standardized names (Bailey et al., forthcoming). Coefficients are almost identical to those from the original matched sample. Finally, I introduce more false-positive matches by allowing individuals to misreport their age by up to five years and find that this attenuates the main coefficient.

Online Appendix Table A.3 shows that results are insensitive to the choice of incarceration measure described in Section III. Results are consistent across the three categorizations, with the effect of full exposure between −1.74 and −1.96 percentage points using the "likely seats" measure and between –0.409 and –0.618 using the "school in county" measure.

I showed in Table 4 that the matched sample was not representative of the population but that the two are indistinguishable after using IPW. My main results reweight by prisoner, race, and year, but do not correct for the differences in covariates described in Table 4. Therefore, in Online Appendix Table A.4, I show the results with different weighting schemes. Results are almost identical in magnitude to the original results, suggesting that observable differences between the matched sample and population do not explain the results.

### *B. Replicating the Results in Unmatched Data*

While there are no meaningful differences between the matched sample and population, and reweighting the results does not meaningfully affect the results, one might still worry that the matched sample differs from the population along unmeasurable characteristics. To alleviate this concern, I use the 1940 Full Count census from IPUMS, which has prisoner status, birth state, county of residence in 1935, race, and age. Details are explained in Online Appendix A.

I show the results in Online Appendix Table A.5. The main coefficients of interest for each outcome are not statistically distinguishable from each other. However, I do find that full Rosenwald exposure increases years of education by between 0.73 and 1.28 years. Full exposure reduces the probability of incarceration by between 1.34 and 1.88 percentage points. This suggests that using matched data is not driving the primary results.

### *C. Permutation Tests*

Finally, I use a permutation test to reshuffle Rosenwald exposure among eligible southern counties. By doing this, I argue that the effects found in this paper are not just due to random chance. I randomize across counties in two ways: first, among only those who did get a school and, second, among all southern counties. I keep the distribution of school opening dates fixed to match the actual opening dates of the schools. I use the "likely seats" measure of exposure.

I do the previous procedure 1,000 times and plot the coefficients in Online Appendix Figure A.1. The coefficient in the main sample is plotted as a vertical line in the histograms. While there is large variation in the estimated coefficients in the placebo regressions, my coefficient is larger in magnitude than all by 1.1 percent of coefficients when using only receiving counties, and 0.3 percent of coefficients when randomizing across the entire South. This suggest that the results found were not due to random chance but the actual placement of the schools.

## VII. Conclusion

This study considers the social returns of a program that increased the schooling of black children between 1920 and 1940. The program was responsible for one year of the three-year decrease in the black–white education gap in this time period. I show that the program also resulted in lower incarceration rates. I find a social return to a year of school of about a 1.1 percentage point decrease in incarceration, which is larger in magnitude compared to the literature (Lochner and Moretti 2004).

Papers looking at other social returns to school have found that the institutional context matters. I focus on a poorer, highly unequal society, so my results are potentially more applicable to developing countries today. Furthermore, this intervention was large and affected approximately one-third of black children; effects of such a large program might differ from estimates using compulsory schooling laws, which only affect education levels at the individual level. All papers in this area focus primarily on secondary schooling, using variation induced by compulsory schooling laws, whereas I focus on elementary school. If there are decreasing returns to education, we might expect results to be stronger at these younger ages.

These results imply that the black–white incarceration gap should have decreased by half between 1910 and 1940. However, incarceration rates of blacks were increasing over this time period, due to countervailing forces such as migration to the North (Muller 2012) and migration to cities within the South. In fact, Rosenwald schools themselves resulted in migration to the North, dampening the effect of education on incarceration.

My results contribute to the broader scholarship about causes of black–white differentials in the 20th century, as well as to the literature on social returns to education. This is the first work to consider an important social return to education in a historical period. Previous literature has considered social returns to schooling primarily in the contemporary United States and Europe, where inequality is lower, institutions are stronger, and incomes and education levels are higher.

The historical gap between blacks and whites in incarceration is not well understood. This work shows that differences in education were one factor that contributed to racial differences in crime and incarceration. Exploring the other causes of this gap would be a fruitful subject for future research.

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