

Education and Incarceration in the Jim Crow South: Evidence from Rosenwald Schools¹

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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 dataset of incarcerated and non-incarcerated men to observe access to schooling in childhood. I find that exposure to one of the new primary schools built as part of the Rosenwald program reduces incarceration by 34 percent of the mean. I argue that most of 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 US.

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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 than white men. By 1923, the black-white incarceration ratio was 4.2 and it has grown to 6.4 as of 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 socio-economic disparities that give rise to different underlying levels of crime.² Recent work finds that today's racial incarceration gap is partly due to a discriminatory law enforcement system and changes in sentencing policies (e.g. 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.³

This paper collects a new dataset of the full universe of prisoners from the US Censuses between 1920 and 1940, a time period which has not previously been possible to study due to

² These disparities include differences in education levels and income, but also differential job opportunities/unemployment rates, urban residence rates, and family background between races.

³ 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 US. 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 micro-data. 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.

lack of data. I explore the role of one factor – disparities in education – in explaining the historical roots of the racial gap in incarceration.⁴ 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 which 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 non-prisoners, 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. Exposure to a Rosenwald school for seven years during childhood reduces the probability of being incarcerated for blacks by 0.84 percentage points or 34 percent of the mean black incarceration rate in 1940. I show that educational attainment is an important channel through which the probability of incarceration decreases, but also find an increased propensity to migrate to the higher wage north and increases in income. Finally, I collect data from non-census

⁴ In this paper, I use incarceration in the census as my measure of crime, while thinking about factors which 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. I also look at some data about actual crimes committed later in the paper.

sources in North Carolina to consider whether discrimination is driving the decrease in incarceration observed in the main sample.

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.⁵ 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

⁵ 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 2009, Aaronson et al 2014, Milligan et al 2004).

and whites.⁶ My results imply that one year of school reduces the probability of being incarcerated by 0.84 percentage points, an effect twice as large as Lochner and Moretti's estimate. The social returns to primary school (in terms of crime reduction) appear to be 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 and additional data. I conclude in Section VI.

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. In 1890, 3 out of 1000 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's numbers divide by the relevant population which includes both men and women of all ages. Given that about 90% of

⁶ Lochner and Moretti (2004) is the best known study; their results have been replicated and expanded in Sweden, the UK, and other European countries (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.

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 to 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

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 agreed to fund a pilot program supporting the construction of six black schools in 1913-14, with the promise of up to 100 more. The original schools were built primarily in Alabama; by 1920, the program supported 716 schools in eleven 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 up to 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

⁷ The Rosenwald School Initiative was not the only black schooling initiative in this time. The Jeanes Fund provided teacher training. Kreisman (2014) shows that this fund also increased school enrollment and literacy of black youth. Other philanthropic interventions are described in Donohue, Heckman and Todd (2002).

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 The location of the schools was not randomly assigned. Aaronson and Mazumder (2011) show that counties with higher white literacy rates were more likely to build schools. This is consistent with the idea that whites in the area were less threatened by black education if they were already educated themselves. I would ideally check whether early incarceration predicts 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 the 1902 Census of Government which reports county-level expenditures overall and for jails and courts separately. None of these variables directly predict the number of Rosenwald schools or the time until the first school within a county (Table A.1).

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 5 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.

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 concern at that time. John Roach Stratton (1900) argued that the “race problem,” i.e. the high crime rates and

⁸ I omit Missouri from my analysis because only 11 schools were built there.

“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 and W. E. B. Du Bois 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, out of which grew the Rosenwald Initiative.

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

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.

evidence that education increases voting and other civic behavior (Milligan et al 2004); these attitudes could also translate into lower willingness to commit crimes.

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 effect of education, income, and migration is directly testable with my data, there could be community level effects which 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 would expect higher levels of education in the local black population to have a negative effect on overall wages. This in turn could increase white criminality and incarceration, which would lead to overestimating the effect in the absence of this. However, I do not see any effect of Rosenwald exposure on whites, suggesting that this was not a major factor.

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. I show with data from North Carolina

that Rosenwald schools do lower the number of county court cases—a measure of the number of individuals caught.

III. Data

A. Measuring incarceration and constructing the primary sample

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 US census data for the years 1920-1940. To do so, I assemble a dataset that includes the full universe of Southern-born, male prisoners and a 20 percent sample of Southern-born, male non-prisoners in each relevant census. I restrict the sample to men between ages 18 and 35 who were born in one of the 14 Rosenwald states.

I identify prisoners in each census via a three-step process. First, I take men reporting a relationship to household head of “Prisoner” in the full census indexes from FamilySearch.org, a genealogical website. Second, many prisoners did not report this relationship to household head but instead reported “Inmate” or, often, the field was left blank; therefore, I identify any census images which contain a substantial number of inmates or blank relationship fields.¹¹ Third, I look-up by hand each of these images to determine whether the men are incarcerated (as opposed to living as inmates in a hospital or other facility). I define an individual as incarcerated if he is present in a state prison, but also include federal penitentiaries, county and city jails, convict

¹⁰ The FBI Uniform Crime Statistics do not become available for a substantial number of counties until the 1960’s. 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 non-immigrants living in select northern states in the 1900-1930 censuses.

¹¹ There doesn’t appear to be any systematic reason that some prisoners were called inmates. If anything, it was an enumerator specific decision. However, most inmates are not prisoners—inmates include soldiers in barracks, hospital residents, residence of old age homes, and sometimes individuals living in hotels.

camps, and chain gangs.¹² I identify 24,010, 65,201, and 83,262 southern-born, male prisoners between ages 18 and 35 in the 1920, 1930, and 1940 Censuses, respectively. Non-prisoners are drawn from the full indexes of each census. I construct a 20 percent sample of Southern-born men between ages 18 and 35 in each relevant census.

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/rural status to each town as of the relevant census year.¹³ Urban is equal to one if the community has 2500 residents or more; Aaronson and Mazumder (2011) argue that this is the definition that the Rosenwald Fund administrators also used. The goal is to find individuals living at home as children, so men aged 18 to 23 years old are matched to the previous census while those between 24 and 35 years old are matched over a twenty year period.¹⁴

To match individuals, I follow the procedure pioneered by Ferrie (1996) and used in Abramitzky, Boustan, and Eriksson (2012). I first standardize first and last names using the NYSIIS algorithm (Atack and Bateman 1992) which spells names with the same phonetic sound identically. Individuals are then matched by first name, last name, state of birth, race, and age

¹²While 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 2500 residents. As Aaronson and Mazumder (2011), this was likely also the definition used by Rosenwald Fund administrators when targeting Rosenwald funds to rural areas.

¹⁴ Results are robust to using different cutoffs. I have moved the cutoff as far as age 26. This cutoff was determined by calculating the age at which approximately 90 percent of individuals appear to be living at home in the 1910 census. The percentage living at home at young ages increases with later censuses. In 1910, 88.6 percent of black children and 92.4 percent of white children who are 16 years of age or younger report a relationship to household head of “child”, “grand-child”, “sibling”, or “other relative”.

across census waves. I allow individuals to misreport their age by up to two years in either direction. Inherent in any matching procedure is a trade-off between sample size and accuracy. To prioritize accuracy in order to minimize measurement error in my measure of Rosenwald exposure, I go one step further and require individuals to be unique by name/birthplace/race within a 5 year age band.¹⁵ If I do not require this more stringent form of uniqueness, I find smaller effects of Rosenwald exposure.¹⁶ Accuracy may, however, come at the cost of representativeness if the uniqueness of an individual's name is correlated with his socio-economic status. I explore this below in Section C.

Sample sizes and match rates are shown in Table 1. Match rates are somewhat lower than usual, averaging from 7 to 23 percent; note that I use a more restrictive match procedure than most papers—these rates are consistent with those found in Abramitzky et al. (2012) utilizing the same method. Match rates are higher for non-prisoners than prisoners. This could be because prisoners are less literate and so are less likely to report their age 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 are also about twice as high for white men as 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

¹⁵ That is, I require an individual's name/birthplace/race combination to be unique within their age plus or minus two years. I impose this restriction in both childhood and adult years.

¹⁶ One concern in this paper stems from assigning individuals an incorrect county of birth and therefore incorrect exposure to Rosenwald schools. If this is random, I expect attenuation bias in the coefficient of interest. In fact, coefficients decrease by about 1/3 when I relax the 5-year-band requirement, suggesting that the more stringent uniqueness requirement reduces measurement error substantially. Results are not shown here but are similar in sign.

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.

B. The location of Rosenwald schools and assigning Rosenwald exposure

Information about the Rosenwald school program is taken directly from Aaronson and Mazumder (2011). The dataset 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.¹⁷

The school-level cards do not contain information on the school's address within a county. Therefore, I assign exposure to a Rosenwald school as measured in Aaronson and Mazumder (2011): exposure to a Rosenwald school varies based on county and birth cohort. Children who were born before 1906 would have been too old to attend even the first Rosenwald school. As the program expanded, later birth cohorts enjoyed a higher likelihood of attending a Rosenwald school. However, the Rosenwald program did not build schools uniformly throughout the South; out of 1,168 counties in the states with Rosenwald schools, 76 percent received at least one school.

Aaronson and Mazumder (2011) find little correlation between pre-existing black socioeconomic characteristics and the placement of Rosenwald schools in a county but do find a relationship between white literacy and school construction. I find little relationship between Rosenwald school timing and county-level expenditures on courts and jails reported in the 1902

¹⁷ Only a few schools were built in Missouri so I omit Missouri from the analysis. My analysis therefore includes 14 states.

Census of Government (Table A.1). 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 discuss these and other potential threats to identification in Section IV. I also show results with county-year fixed effects to show that results are robust to accounting for factors changing within a county over time which affected all cohorts and both races.

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 his county. This measure is referred to as “School in County” in results tables. 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.¹⁸ Therefore, this measure is used in most analysis and is referred to as “Likely Seats”. 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 between ages 7 and 13 within a county in each census year and then extrapolated between years; I follow their assumption that a classroom could hold 45 students.

¹⁸ For example, if a school could fit half of the students in a county, then each individual living in that county would get $\frac{1}{2}$ a year of exposure.

C. Representativeness of the Matched Sample and Summary Statistics

A concern is whether the matched dataset is representative of the population. Most literature (Abramitzky, et al., 2012) finds that the matched sample has slightly higher socio-economic status than the full population. I explore this possibility in Table 2.

Panel A looks at outcomes in the adult census year. I weight all individuals by the inverse of the match rate within a prisoner/race/year cell so 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 1.7 percentage points less likely to be literate than the population. Matched individuals are, surprisingly, slightly less likely to be living outside of the South as an adult.

Panel B looks at whether matched individuals differ from non-matched individuals in childhood. I find that the matched sample is only slightly (0.06 pp) more likely to be urban. The difference between the matched sample and population is never consistently positive or negative and is very small although significant. Rosenwald exposure of the matched sample differs slightly from the population but the difference is well less than one percent of the mean.

Finally, Panel C finds no large differences between the matched sample and population based on age or state of birth. I conclude that the matched sample is similar to the population. Most differences are consistent across groups within the sample so shouldn't affect inference within the sample; any differences will only affect population inferences about the results.

Summary statistics are shown in Table 3. 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. Rates are slightly higher than the rates given in Figure 1, adjusted

for gender (multiplied by 1.8), probably because ages 18 to 35 have the highest risk of incarceration. Prisoners have higher levels of exposure to Rosenwald schools, probably because they are more likely to come from urban areas and larger counties; exposure is defined for urban and rural areas because it is a county-level characteristic, but the schools were targeted to rural areas within a county.

The probability of being found outside of the South as an adult is higher for prisoners than non-prisoners, a difference which is 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 non-prisoners. Black prisoners have on average 5.54 years of schooling compared to 6.12 for black non-prisoners. The gap is larger for whites: prisoners have 6.94 years of school compared to 9.05 years for non-prisoners. 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 1996). As expected, prisoners are less literate than non-prisoners for both races, although literacy rates are high at 80 to 96 percent.

D. Additional Data from North Carolina

One drawback to using census data on incarceration to measure crime is that they do not include information about the severity of the crime or length of the sentence. Furthermore, a count of individuals in prison at a point in time captures mostly new criminals, not all individuals who have been engaged in criminal activity in the past (who may have since been released from prison). I collect additional data from North Carolina, the only state where such rich data is available.

To measure the flow of prisoners rather than stock, I collect complete county-race-year counts of inmates admitted to the North Carolina State Prison from the State Prison reports between 1935 and 1955. I assign exposure in the same way as court cases above. The number of admissions increases almost tenfold between 1933 and 1935 so I restrict to post-1935.¹⁹ Prior to 1931, data is not available broken down by race.

To measure the number of individuals *accused* of a crime, I collect year-race-county totals of the number of individuals tried by the county court in that year. I then assign exposure based on the age distribution of admissions. That is, I calculate the number of years individuals of each age would have had a school in their county and then take the weighted average based on the age distribution of admissions which is available aggregated to the state level.

Because juvenile delinquents were so infrequently incarcerated, they are unlikely to show up in the census data.²⁰ Children had to be at least 16 years of age to be sent to a state prison. I use county-level data on juvenile court cases by race coded by Wiley Britton Sanders (1945) to estimate the effect of the Rosenwald program on juvenile delinquency. I have eight data points per county: separate counts for black and white children for the time periods 1919-29²¹, 1929-34, 1934-39 and 1939-44. Ideally, one would use annual data to trace out the effect of a new Rosenwald school but the underlying data from which Sanders constructed his series appears to be lost. I calculate average exposure for each county in the time period and assign this

¹⁹ This seems to be because in the early periods the state prison reports only reported people who were sentenced to the state prison and not to road camps; data after this includes prisoners at state prison camps and road construction gangs outside of the state prison which made up a large proportion of prisoners.

²⁰ Juvenile court systems were not organized on a systematic basis until the 1930's and any incarceration facilities were small at best. For example, North Carolina had only two "training schools" to which judges could send juvenile delinquents. One for whites, called the Morrison Training school, was established in 1918 but had only 30 places. It wasn't until 1925 that a facility was built for black children. The Stonewall Jackson school had room for 35 students.

²¹ I divide these counts by two to get a comparable 5 year count.

to the relevant data point. More specifically, the exposure measure is calculated as the number of years in this time period that the county had a school.

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, from age 7 to 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 built in 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.²²

My first estimating equation (1) is restricted to the black male sample only. Using a linear probability model, I estimate:

$$\begin{aligned} prisoner_{iasct} = & \alpha_c + \gamma_t + \theta_a + \pi_{st} + \beta_1 rural_{ict} + \beta_2 exposure_{ict} \\ & + \beta_3 exposure_{ict} * rural_{ict} + \varepsilon_{ict} \end{aligned}$$

where *Prisoner* equals 100 if individual *i* (of age *a* who lived in county *c* and state *s* in childhood at census date *t*) is incarcerated at the time of the adult census.²³ I scale the original indicator outcome variable by 100 so that coefficients can be interpreted as percentage point changes. I include state fixed effects interacted with childhood census year, childhood census year fixed effects, age and black times age fixed effects, and childhood county fixed effects. I also include a

²² I create sample weights that are inversely proportional to the match rate within a census year-race-prisoner status cell. I do this for two reasons: first, because I have a full 100 percent sample of prisoners but only a 20 percent sample from of non-prisoners, I must weight non-prisoners by a factor of five to achieve the correct incarceration rates; second, match rates differ by race, census-year and prisoner status so I weight by the inverse of the match rate.

²³ 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.

vector of childhood county-level covariates in all regressions which includes black and white literacy rates, homeownership rates, and manufacturing output per capita.

Because urban areas did not receive schools, the main effect of *exposure* controls for any trends within a county in the outcome variable that are correlated with exposure but which affect urban and rural areas similarly. Therefore, the coefficient of interest is β_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 which are changing over time within rural versus urban parts of counties and which 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 (2):

$$\begin{aligned} prisoner_{iarsct} = & \alpha_c + \gamma_{tr} + \theta_{ar} + \pi_{st} + \beta_1 black_i + \beta_2 exposure_{ict} + \beta_3 rural_{it} + \\ & \beta_4 black_i * rural_{it} + \beta_5 exposure_{ict} * rural_{it} + \beta_6 exposure_{ict} * black_i + \\ & \beta_7 rural_{it} * black_i * exposure_{ict} + \varepsilon_{ict} \end{aligned}$$

for individual of race r and the remaining same subscripts from Equation (1). I add race-specific childhood census year fixed effects, age fixed effects interacted with race, and birth state times childhood census year fixed effects. The coefficient of interest in this equation is β_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 and black urban youth.

To the extent that Rosenwald resources may have been diverted to rural white schools, β_5 picks up any effect on white rural individuals. It is possible that Rosenwald funds freed up money in the local budget which could then be 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.²⁴ For this reason, I control for the effect of Rosenwald exposure on whites (β_5) and interpret β_7 as the differential effect of Rosenwald schools on incarceration for blacks.

My estimate of β_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 which 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 so as 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 during childhood and then attitudes during adulthood towards incarceration 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. To

²⁴ 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.

handle the idea that variables which are changing over time could explain my results, I control for census year-county fixed effects and results are not affected.

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/literacy, income, and migration as possible channels through which Rosenwald schools reduced incarceration.

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

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 on 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 4, 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” that 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 his county between the ages of 7 and 13.

The coefficient of interest is on Exposure*Rural. Columns (1) through (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 0.851 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 which affect both races and rural/urban areas similarly. I find that

the coefficient is stable across specifications. Rosenwald exposure reduces incarceration by between 0.85 and 1 percentage points.

As expected, the coefficient on Rural is negative and significant—i.e. 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 which 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 0.322 and 0.469 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.

The coefficients are all relative to an average base incarceration rate for blacks of 2.48 percent in 1940; 1920 and 1930 have very few exposed cohorts but are useful to remove pre-trends that are correlated with exposure to Rosenwald exposure. This implies that full Rosenwald exposure would reduce the probability of incarceration by 34 percent of the mean by the first measure and 12 percent of the mean by the second measure. Note, however, that average exposure in the sample is 0.05 and 0.2 using the two measures. Therefore, the average level of exposure in my sample reduces incarceration rates by 1.7 and 2.4 percent of the mean.

In Table 5, 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 0.84 percentage points. This number falls to 0.43 percentage points when using the second measure of exposure. I do not see any effect on whites. The rest of the analysis in this paper uses whites as a control group.

B. Effects of Rosenwald exposure on education, income, 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 or education, income, and migration. The most direct mechanism is through education, which in turn increases income and the propensity to migrate.

Table 6 starts with data from all three adult census years in columns (1)-(3). I use literacy as reported in the census in 1920 and 1930; in 1940, I define an individual as literate if they report having completed three or more years of education (Collins and Margo 1996). Column (1) replicates the effect of Rosenwald exposure on incarceration from Table 5. In Column (2), we see that full Rosenwald exposure increased literacy rates of rural black men by 6.6 percentage points. I find large effects on migration 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 10.6 percentage points. This number is much larger than that found by Aaronson and Mazumder (2011) but I note a major difference: I look at migration over the whole period between childhood and adulthood whereas they only look at migration from 1935-1940.

In Columns (4) and (5), I restrict to the 1940 sample where education and income is available. I find that education levels in my sample increase by 0.99 years with full Rosenwald

exposure. This is similar to the 1.2 years found by Aaronson and Mazumder and is not statistically distinguishable. Finally, I find that income increases 16.1 percent with full Rosenwald exposure, but note that income in 1940 is only reported for wage earners so the sample size decreases substantially.

I calculate a Wald estimate of the social return to education based on 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 later-in-life incarceration by about 0.8 percentage points. By comparison, Lochner and Moretti find that an additional year of schooling reduces the likelihood of imprisonment by 0.4 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.

Finally, in Table 7, 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 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 becomes insignificant but remains large at 0.57. I take this to mean that literacy and migration are important channels but do not explain the full effect.

C. Replicating the results in unmatched data

While there are no meaningful differences between the matched sample and population, 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 5 years ago, race, and age. I use county of residence 5 years ago to assign Rosenwald exposure instead of relying on matched data. I note that this is imperfect because 22% of individuals had already left their state of residence by 1935, suggesting that I will understate the main result. Another drawback is that I do not have town with which to assign urban/rural status in 1935.²⁵

I show the results in Table 8, omitting the interactions and main effect of Rural. In this specification, full exposure to a Rosenwald school decreases black incarceration by 0.336 percentage points. While this is half as large as in the full matched sample, it is negative and significant, suggesting that the full matched sample is not selected along any important dimension. Furthermore, this coefficient is well within the confidence intervals of the coefficients from the matched sample.

D. Effects of Rosenwald schools in North Carolina

Table 9 restricts the analysis to the North Carolina subsample where I have additional data. North Carolina is poorer than most southern states at this time and had higher exposure to Rosenwald schools. Results are larger than in the whole sample. Using blacks only in Columns (1) and (2), Rosenwald exposure decreases incarceration by 1.7 and 0.63 percentage points for the “Likely Seats” and “School in County” measures, respectively. Using both races, the effects

²⁵ This was collected in 1940, but IPUMS decided not to code it because the number of strings and misspellings was unmanageable for the full count data.

are -1.69 and -0.75. These larger numbers are relative to a much larger base incarceration rate of 2.05 percent for blacks. Exposure is also substantially higher in North Carolina where more schools were built than any other state. In all regressions, exposure of whites to Rosenwald schools is small and insignificant, indicating that Rosenwald schools did not affect white incarceration.

The benefit of the census data used thus far is that it provides a picture of all prisoners in the full South. A drawback is that it does not contain information about type of crime or sentence length, two characteristics that provide indirect information on whether and to what extent the racial gap in incarceration is simply due to the discriminatory Jim Crow legal system, rather than to “true” differences in criminal activity. One may think that exposure to Rosenwald schools reduced discrimination, which then led to less imprisonment in the local area.

In Table 10, I present estimates from different data sources that allow me to look more in detail at which mechanism is more likely. I focus on North Carolina because this is one of the states in which data is available broken down by race, year, and county for the outcomes of interest. I first look at state prison reports which provide a flow measure of incarceration in each year. An additional benefit of this data is that state prisoners are more likely to have committed “real crimes,” rather than, for example, being imprisoned due to enforcement of vagrancy laws, which was more likely to happen in local jails. Second, I examine county-level counts of court cases; this is a measure of the number of individuals *accused* of a crime, rather than only those convicted and sentenced for a crime. If the main effects derive from discrimination in conviction rates, due to partial juries or sentence lengths due to biased judges, I will not find an effect of Rosenwald schools on the case load. Following this, I look at juvenile court case data; because juveniles are rarely incarcerated but instead are put on probation, census data does not allow me

to consider the contemporaneous effect of Rosenwald schools on youth activity. I create covariates based on the population census data in the decennial years and linearly extrapolate between years. Controls include the total black and white male populations, the total county population, the share of the population that is black, and the share of the population that lives in urban areas. I log the variables of interest but do not divide by population in order to allow the relationship between population and numbers of cases to be flexible.

Results from the State Prison Reports are shown in Column (1).²⁶ Exposure is a weighted average based on the age distribution of prisoners and the timing of school openings in counties.²⁷ I find that exposure to Rosenwald schools would reduce the black-white gap in incarceration at the county level by 19 log points (20.9 percent) for a mean gap of 54 log points in column (2).

An alternative measure of crime is the number of individuals accused of a crime in a given county in each year. I digitize county level court cases from the Attorney General reports from 1913-1935 (the data is not available in later years) and find that court cases against blacks also decrease by a similar magnitude with average exposure in a county. Results are shown in Column (2).

The results from the State Prison reports suggest that what I find in census data is not due to discrimination in sentence lengths or due to petty crimes. In fact, exposure to Rosenwald schools decreases the probability of being incarcerated in state prison. The fact that court cases go down as well suggests that arrests decrease with Rosenwald exposure. While I cannot rule out

²⁶ County of birth is not available in this data so I use county of arrest. To the extent that there is migration from rural counties to urban counties, I will understate exposure since urban areas had fewer Rosenwald seats per student.

²⁷ Because the mean of the dependent variable increases by a factor of ten between 1933 and 1935, I restrict to post-1935 in my analysis.

that court cases are related to discrimination at the arrest stage, this provides indirect evidence that my results are not due to discrimination at the sentencing stage.

I consider contemporaneous effects of Rosenwald schools on the criminal activity of youth in Column (3) of Table 10. The observed decline in adult crime might arise because additional years of schooling increase the opportunity cost of crime. Alternatively, the main effect could be due to persistence of criminality: offenders who commit crimes at young ages often commit crimes later in life as well. If Rosenwald schools reduce crime among juveniles, these individuals might also be less likely to commit crimes later.

I do not have individual level data, but rather county-race counts for four five year periods. I regress the number of juvenile court cases for blacks or whites in a given county within a five year period on exposure to Rosenwald schools and the same set of county controls as above. Exposure is measured as the number of years over the previous five year period the county had a school. Exposure to Rosenwald schools has a negative and significant effect in all specifications. The effect of exposure on black juveniles is a decrease of 9.4 court cases per five years from the average of about 50 court cases. The effect of Rosenwald schools on white outcomes is insignificant and almost always small.

The magnitude of the effect on juvenile crime is somewhat similar to that on adult crime in the census. However, most juvenile offenders do not reoffend (18 percent reoffend, Illinois 2007). Therefore, reductions in juvenile infractions are not insignificant but do not account for most of the reduction in incarceration in adulthood.

VII. Conclusion

This paper considers the social returns of a program which 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 0.8 percentage point decrease in incarceration, which is larger in magnitude to the literature but three times the size of contemporary estimates relative to the mean incarceration rate (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 and so my results are potentially more applicable to developing countries today. Furthermore, this intervention was large and affected approximately 1/3 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.

This paper's 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 paper 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 paper 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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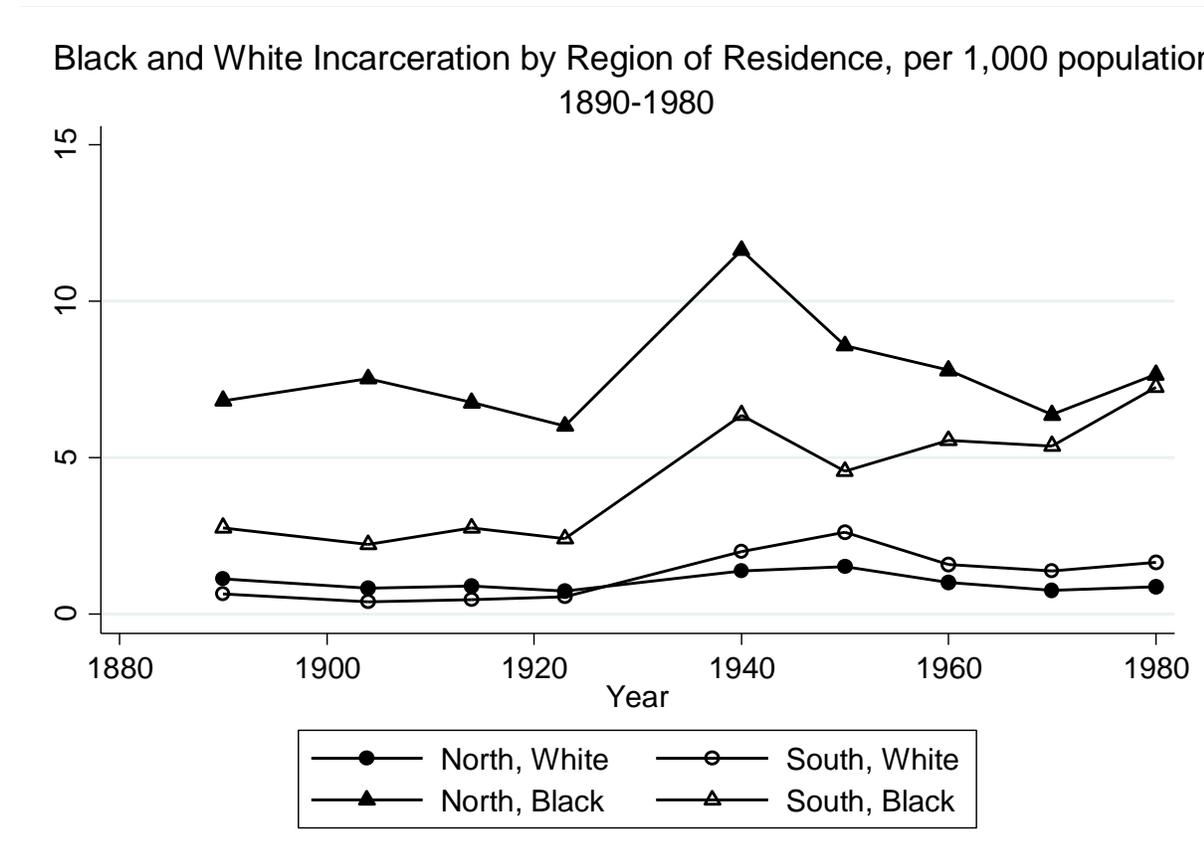
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Figures

Figure 1: Incarceration by Race and Region per 1,000 population, 1890-1980



Notes: Incarceration figures taken from US Department of Interior (1895), U.S. Department of Commerce and Labor (1907), US Department of Commerce (1914, 1926, 1943, 1955, 1963, 1973, 1983). Population (denominator) taken from IPUMS (Ruggles et al 2010). Figure depicts total number of prisoners by race/census region/year divided by relevant population, where population is interpolated between census years for non-census years. Men and women included—multiply figure numbers by 1.8 to calculate male incarceration rates.

Tables

Table 1: Sample Sizes and Match rates by Adult census year and Prisoner status

		(1)	(2)	(3)		
		Population	Matched	Match Rate		
1940	<i>Prisoners</i>	Black	31,034	2,630	0.084	
		White	30,254	4,863	0.161	
	<i>Non-Prisoners</i>	Black	307,784	31,043	0.101	
		White	908,946	216,416	0.238	
	1930	<i>Prisoners</i>	Black	26,797	2,231	0.083
			White	24,621	4,110	0.167
<i>Non-Prisoners</i>		Black	318,686	29,360	0.092	
		White	756,610	187,302	0.248	
1920		<i>Prisoners</i>	Black	14,685	1,033	0.070
			White	6,573	838	0.127
	<i>Non-Prisoners</i>	Black	231,774	24,735	0.107	
		White	617,045	132,811	0.215	

Notes: Prisoners and non-prisoners are taken from the census indexes provided by FamilySearch.org. I take a twenty percent sample of non-prisoners and a full 100 percent sample of prisoners. Individuals are matched based on name, age, state of birth, and race. I require men to be unique by name, birth state, and race within a five year age band (age plus or minus two years). The year in the table refers to the adult census year. I restrict to men aged 18 to 35 in the adult census year; men less than or equal to 23 are matched to the previous census while men 24 to 35 are matched to a census twenty years prior.

Table 2: Comparing the matched sample to the full population

	Black		White	
	Population	Difference: Matched - Population	Population	Difference: Matched - Population
<i>Panel A: Outcomes in adult census year</i>				
=1 if Literate	0.827 (0.001)	0.017*** (0.002)	0.959 (0.001)	0.009*** (0.001)
Years of Education (1940)	5.927 (0.008)	0.201*** (0.021)	8.718 (0.005)	0.323*** (0.009)
=1 if living outside South	0.186 (0.008)	-0.016*** (0.002)	0.089 (0.001)	-0.017*** (0.001)
<i>Panel B: Childhood census year characteristics</i>				
=1 if lives in Urban area	0.152 (0.001)	0.006*** (0.001)	0.188 (0.001)	0.007*** (0.001)
Exposure (“Likely Seats”)	0.052 (0.001)	0.001*** (0.000)	0.061 (0.001)	-0.001*** (0.000)
Exposure (“School in County”)	0.242 (0.001)	-0.000 (0.001)	0.217 (0.001)	-0.004*** (0.000)
<i>Panel C: Individual Characteristics</i>				
Age in Adult census	26.00 (0.0090)	-0.242*** (0.024)	25.61 (0.006)	0.042*** (0.011)
<i>State of Birth</i>				
Alabama	0.106 (0.001)	-0.003** (0.002)	0.072 (0.001)	0.000 (0.004)
Arkansas	0.051 (0.001)	0.003** (0.001)	0.066 (0.001)	0.001** (0.001)
Florida	0.026 (0.001)	-0.001* (0.001)	0.041 (0.001)	-0.002** (0.001)
Georgia	0.114 (0.000)	-0.002** (0.001)	0.069 (0.000)	0.003** (0.000)
Kentucky	0.032 (0.000)	-0.000 (0.001)	0.106 (0.000)	0.002** (0.001)
Louisiana	0.091 (0.001)	-0.008*** (0.002)	0.064 (0.000)	-0.004*** (0.000)
Maryland	0.027 (0.001)	0.002** (0.001)	0.052 (0.001)	-0.002** (0.001)
Mississippi	0.112 (0.001)	-0.002* (0.001)	0.049 (0.001)	0.001** (0.000)
North Carolina	0.099 (0.001)	0.005*** (0.001)	0.085 (0.001)	0.002*** (0.001)
Oklahoma	0.015 (0.000)	0.001** (0.000)	0.058 (0.001)	-0.004*** (0.000)
South Carolina	0.108 (0.001)	0.002 (0.002)	0.043 (0.000)	-0.000 (0.001)
Tennessee	0.056 (0.000)	-0.002* (0.001)	0.089 (0.000)	0.001* (0.001)
Texas	0.089 (0.000)	0.012*** (0.001)	0.144 (0.000)	0.001 (0.001)
Virginia	0.061 (0.003)	-0.008*** (0.001)	0.074 (0.001)	0.001* (0.000)

Notes: Sample includes male prisoners in 1920, 1930, and 1940 matched to childhood census county to assign Rosenwald exposure. I restrict to men between 18 and 35 years of age. 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. Regressions include robust standard errors.

Table 3: Summary Statistics

	Black		White	
	Prisoner	Non-Prisoner	Prisoner	Non-Prisoner
Sample Size	5,894	85,138	9,811	536,574
In Prison (weighted)				
1920		0.994		0.186
1930		1.656		0.647
1940		2.489		0.754
<i>Childhood Characteristics:</i>				
Exposure, Likely Seats	0.058 (0.129)	0.051 (0.141)	0.082 (0.178)	0.063 (0.159)
Exposure, School in County	0.320 (0.414)	0.262 (0.395)	0.283 (0.403)	0.230 (0.382)
=1 if Living in Urban Area	0.272 (0.335)	0.164 (0.370)	0.249 (0.432)	0.214 (0.410)
<i>Adult Outcomes:</i>				
Age	25.90 (5.323)	26.23 (4.906)	25.76 (5.206)	25.90 (4.642)
Living Outside the South	0.224 (0.417)	0.145 (0.352)	0.185 (0.389)	0.075 (0.263)
Education (1940 Only)	5.545 (3.232)	6.121 (3.265)	6.939 (3.132)	9.053 (3.354)
Literacy	0.803 (0.362)	0.844 (0.380)	0.923 (0.262)	0.969 (0.182)

Notes: N=620,374. Sample includes prisoners and non-prisoners 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 to 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.

Table 4: Reduced Form Results: Effect of Full Rosenwald Exposure on Incarceration, Blacks only

<i>Outcome:</i>	(1) =100 if in Prison	(2) =100 if in Prison	(3) =100 if in Prison	(4) =100 if in Prison
<i>Panel A: Likely Seats Measure of Exposure</i>				
Exposure*Rural	-0.833* (0.490)	-1.085** (0.406)	-1.134** (0.518)	-1.092* (0.651)
Exposure	0.644 (0.511)	1.176* (0.581)	1.267** (0.567)	1.386 (0.994)
Rural	-0.851*** (0.073)	-0.628*** (0.088)	-0.638*** (0.084)	-0.653*** (0.091)
Exposure Measure	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”
County Controls?	Yes	Yes	Yes	No
Fixed Effects	None	State	County	County-Year
Mean Exposure	0.051	0.051	0.051	0.051
Sample Mean, Black	1.406	1.406	1.406	1.406
N	89,797	89,797	89,797	89,797
<i>Outcome:</i>	(5) =100 if in Prison	(6) =100 if in Prison	(7) =100 if in Prison	(8) =100 if in Prison
<i>Panel B: School in County Measure of Exposure</i>				
Exposure*Rural	-0.441*** (0.147)	-0.455*** (0.140)	-0.469*** (0.089)	-0.322* (0.189)
Exposure	0.217 (0.182)	0.280* (0.133)	0.196 (0.182)	0.077 (0.315)
Rural	-0.761*** (0.076)	-0.553*** (0.096)	-0.562*** (0.019)	-0.622*** (0.102)
Exposure Measure	“School in County”	“School in County”	“School in County”	“School in County”
County Controls?	Yes	Yes	Yes	No
Fixed Effects	None	State	County	County-Year
Mean Exposure	0.201	0.201	0.201	0.201
Sample Mean, Black	1.406	1.406	1.406	1.406
N	89,797	89,797	89,797	89,797

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 labelled “Sample Mean, black”. Regressions include age, year, state*year, and county fixed effects, where year refers to the childhood census year, and county and state refer to the childhood census county and census state, respectively. I restrict to ages 18-35. Standard errors are clustered by childhood census county. Regressions used the “Likely Seats” measure of exposure. Sample includes prisoners and non-prisoners 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. ***p<0.01, **p<0.05, *p<0.10.

Table 5: Effect of Full Rosenwald Exposure on Incarceration, Both Races

	(1)	(2)
	=100 if in Prison	=100 if in Prison
Black*Rural*Exposure	-0.845* (0.504)	-0.427*** (0.154)
Exposure*Rural	0.063 (0.098)	-0.001 (0.010)
Black*Exposure	0.700 (0.495)	0.203 (0.182)
Black*Rural	-0.755*** (0.073)	-0.671*** (0.078)
Exposure	0.039 (0.098)	0.012 (0.035)
Black	1.114** (0.475)	1.068** (0.475)
Rural	-0.040** (0.019)	-0.041** (0.019)
Exposure Measure	“Likely Seats”	“School in County”
County Controls?	No	No
Fixed Effects	County	County
Mean Exposure	0.051	0.201
Sample Mean, black	1.406	1.406
R ²		
N	620,374	620,374

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 labelled “Sample Mean, black”. Regressions include age, black*age, year, black*year, state*year, and county fixed effects, where year refers to the childhood census year, and county and state refer to the childhood census county and census state, respectively. I restrict to ages 18-35. Standard errors are clustered by childhood census county. Regressions used the “Likely Seats” measure of exposure. Sample includes prisoners and non-prisoners 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. ***p<0.01, **p<0.05, *p<0.10.

Table 6: Effect of Rosenwald Exposure on Literacy and Migration, Full Sample

<i>Outcome:</i>	<i>All years</i>			<i>1940</i>	
	(1)	(2)	(3)	(4)	(5)
	=100 if in Prison	=1 if Literate	=1 if outside South as adult	Years of Education	Ln(Income)
Black*Exposure*Rural	-0.845* (0.504)	0.066*** (0.021)	0.106*** (0.023)	0.903** (0.360)	0.161* (0.085)
Exposure*Rural	-0.064 (0.097)	0.006 (0.004)	0.011* (0.006)	0.216 (0.144)	0.076** (0.037)
Black*Exposure	0.703 (0.537)	-0.031 (0.022)	-0.076*** (0.022)	-0.283 (0.387)	-0.053 (0.079)
Black*Rural	-0.755*** (0.073)	-0.047*** (0.005)	-0.057*** (0.005)	-0.370*** (0.100)	0.119*** (0.022)
Rural	-0.040** (0.019)	-0.011*** (0.001)	-0.016*** (0.002)	-1.359*** (0.052)	-0.283*** (0.012)
Exposure	0.039 (0.102)	-0.006 (0.004)	-0.008 (0.007)	-0.299** (0.141)	-0.094** (0.037)
Black	1.114** (0.476)	-0.154*** (0.036)	0.008 (0.030)	-2.829*** (0.403)	-0.2748*** (0.091)
Exposure Measure	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”
County Controls?	Yes	Yes	Yes	Yes	Yes
Fixed Effects	County	County	County	County	County
Mean Exposure	0.051	0.051	0.051	0.136	0.136
Sample Mean, black	1.406	6.120	0.153	2.489	2.489
R ²	0.006	0.075	0.083	0.188	0.251
N	620,374	593,056	620,374	236,086	149,446

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 labelled “Sample Mean, black”. Regressions include age, black*age, year, black*year, state*year, and county fixed effects, where year refers to the childhood census year, and county and state refer to the childhood census county and census state, respectively. I restrict to ages 18-35. Standard errors are clustered by childhood census county. Regressions used the “Likely Seats” measure of exposure. Sample includes prisoners and non-prisoners 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. ***p<0.01, **p<0.05, *p<0.10.

Table 7: Effect of Rosenwald Exposure on Incarceration, including covariates in main regression

	(1) Original	(2) With Covariates
Black*Rural*Exposure	-0.766* (0.362)	-0.572 (0.463)
=1 if Literate		-0.630*** (0.031)
=1 if living outside the South as adult		0.578*** (0.024)
N	593,056	593,056
R-squared	0.0057	0.0063
Exposure Measure	“Likely Seats”	“Likely Seats”
FE?	County	County
County covariates?	Y	Y

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 labelled “Sample Mean, black”. Regressions include age, black*age, year, black*year, state*year, and county fixed effects, where year refers to the childhood census year, and county and state refer to the childhood census county and census state, respectively. I restrict to ages 18-35. Standard errors are clustered by childhood census county. Regressions used the “Likely Seats” measure of exposure. Sample includes prisoners and non-prisoners 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. ***p<0.01, **p<0.05, *p<0.10.

Table 8: Results with Unmatched 1940 Census

	=100 if in Prison	Years of Education
Black*Exposure	-0.336*** (0.056)	1.512*** (0.021)
Exposure	-0.112*** (0.042)	-0.189*** (0.014)
Mean Exposure	0.145	0.145
Sample Mean, black	1.364	1.348
Exposure Measure	“Likely Seats”	“Likely Seats”
N	4,987,281	4,854,462

Notes: In the first column, outcome = 100 if in prison in the adult year. The binary outcome variable is multiplied by 100 so that the coefficients are expressed as percentages instead of proportions. Mean black incarceration rates are given in the row labeled “Sample Mean, black”. Regressions include age, black*age, and county fixed effects, where county is the county in 1935. I restrict to ages 18-35. Standard errors are clustered by county of residence in 1935. Regressions use the “Likely Seats” measure of exposure. ***p<0.01, **p<0.05, *p<0.10.

Table 9: Reduced Form Results, North Carolina sub-sample

	Black Only		Both Races	
	(1) =100 if in Prison	(2) =100 if in Prison	(3) =100 if in Prison	(4) =100 if in Prison
Exposure	3.468** (1.377)	1.000* (0.495)	-0.132 (0.198)	-0.092 (0.107)
Black			0.942** (0.368)	0.795** (0.335)
Rural	-0.632** (0.295)	-0.549** (0.304)	-0.122** (0.055)	-0.137** (0.059)
Black*Rural			-0.750** (0.269)	-0.565** (0.244)
Black*Exposure			3.322* (1.418)	1.131 (0.492)
Exposure*Rural	-1.778* (1.057)	-0.638 (0.411)	0.120 (0.181)	0.063 (0.076)
Black*Exposure*Rural			-1.691 (1.083)	-0.750* (0.431)
Exposure Measure	Likely Seats	School in County	Likely Seats	School in County
County Controls?	No	No	No	No
Fixed Effects	County-Year	County-Year	County-Year	County-Year
Mean Exposure	0.113	0.384	0.123	0.352
Sample Mean, black	1.496	1.496	1.496	1.496
R ²	0.008	0.008	0.006	0.007
N	15,377	15,377	55,780	55,780

Notes: Outcome = 100 if in prison in the adult year. The coefficients are interpreted as percentages rather than proportions. Black means of the outcome variable are given in the row labelled "Sample Mean, black". Regressions include age, black*age, year, black*year, state*year, and county fixed effects, where year refers to the childhood census year, and county and state refer to the childhood census county and census state, respectively. I restrict to ages 18-35 and to those living in North Carolina during the childhood census year. Standard errors are clustered by childhood census county. Regressions used the "Likely Seats" measure of exposure. Sample includes prisoners and non-prisoners in 1920, 1930, and 1940, linked to childhood census locations to assign Rosenwald exposure. ***p<0.01, **p<0.05, *p<0.10.

Table 10: Effect of Rosenwald Schools on Admissions to State Prison, County Court Cases, and Juvenile Court Cases

Dependent variable= Number of prisoners from county in each year, by race

<i>Outcome:</i>	(1) Ln(Admissions to State Prison)	(2) Ln(County Court Cases)	(3) Ln(Juvenile Court Cases)
Black*Exposure	-0.193*** (0.054)	-0.297** (0.135)	-0.344*** (0.046)
Exposure	0.052 (0.041)	0.098* (0.053)	-0.026 (0.101)
Black	0.543*** (0.156)	-0.345*** (0.135)	0.465*** (0.166)
Exposure Years	Likely Seats 1935-50	Likely Seats 1913-1935	Likely Seats 1919-1944
Sample Mean	0.488	0.525	0.407
Average exp.	2.310	0.495	0.421
R-squared	0.7876	0.5987	0.7349
N	3,064	3,534	733

Notes: Regressions include county and race-specific year fixed effects. Standard errors are clustered at the county level. Controls are included in all regressions: total black and white male populations, total population, share urban in the county, and share black in the county. ***p<0.01, **p<0.05, *p<0.10.

Appendix: Locations of Rosenwald Schools

Table A.1: Correlation between county characteristics and Rosenwald schools

	(1)	(2)	(3)	(4)
	Ever Had a School	Number of Schools by 1931	Had a school by 1919	Number of Years until First School
Jails Expenditures PC	0.2069 (0.181)	-5.2417* (2.604)	0.0008 (0.415)	2.5451 (5.417)
Court Expenditures PC	-0.1257 (0.127)	0.8198 (1.872)	-0.2380 (0.140)	0.5269 (0.816)
Total Expenditures PC	0.0323 (0.026)	-0.1275 (0.212)	0.0004 (0.017)	0.0810 (0.179)
Observations	740	740	740	593
R-squared	0.295	0.399	0.298	0.254

Notes: All regressions control for total population, the share of population that is black, population density, and industry shares by race and include state fixed effects. They also control for distance from the center of the county to Tuskegee Institute, Hampton, Virginia, Montgomery, AL, and Nashville, TN. Standard errors are clustered at the state level.