Education and Incarceration in the Jim Crow South: Evidence from Rosenwald Schools[1]

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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 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 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 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 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 previously been difficult to study due to lack of

[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.

micro-level 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. Full exposure to a Rosenwald school for seven years between ages seven and thirteen reduces the probability of being incarcerated for blacks by 1.96 percentage points; 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

[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.

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 and whites.[6] 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

[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).

[6] 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.

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 20^{th} 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 which reshuffles Rosenwald school exposure. Section VII concludes.

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 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

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 "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 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

[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).

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 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 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 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.

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

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.

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 which would confound my results. My identification strategy

[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.

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 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 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 four-step process. The process uses the Restricted Full Count Census data from 1920-1940 available on the NBER server along with IPUMS

[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.

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]

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 which uses the strategy above, the “Group Quarters” measure which only uses the group quarters and group quarters type variables to identify prisoners, and the “Group Quarters plus Relationship” measure which 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]

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[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.

[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,

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 which was classified as non-incarcerated by IPUMS (due to the Institution name not being entered) but which is from a city jail in Mobile, Alabama. 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/rural status to each town as of the relevant census year.[13] The goal is to find individuals 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.

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:

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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) argue, this was likely also the definition used by Rosenwald Fund administrators when targeting Rosenwald funds to rural areas.

(1) For all censuses which will 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 and Bateman 1992). I also recode any common nicknames to standard first names (e.g. Will becomes William). I restrict my attention to men in the later census that are unique by first and last name, birth year, race, and state of birth. I do so because, for non-unique 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.

My matching procedure generates a final sample of 31,129 prisoners and 4,342,124 non-prisoners. Sample sizes and match rates are shown in Table 3. Match rates are consistent with the literature, averaging from 15 to 31 percent. 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 for whites are also about one and a 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. I present unweighted estimates in Table 12 in Section VI.

Individuals can fail to match due to (i) non-unique name-birth state-race combinations; (ii) mis-reporting 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. Non-unique combinations account for 52 percent of match failures. Allowing individuals to match within a 10 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 socio-economic 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 D below and reweight my main results later in Table 12 to be representative of the population.

My robustness samples examine the sensitivity of my results to methods which produce lower match rates but also lower false positive rates. Recently, Bailey et al (2017) 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 dataset. 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 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.[14]

The location of the schools was not randomly assigned. 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. 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

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

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 2 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 which 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 which predicts coverage in 1919 is lynching: counties which 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 which already had higher black enrollment built less 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 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.[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 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.

D. Representativeness of the Matched Sample and Summary Statistics

A concern with any matching procedure 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 4. The first and fourth columns present the means for the relevant population. The second and fifth columns show the raw difference between the matched sample and population. The third and sixth columns show the differences between the matched sample and population,

[15] For example, if a school could fit half of the students in a county, then each individual living in that county would get ½ a 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}.

reweighting the matched sample using Inverse Probability Weights so that it matches the population (Bailey et al. 2017).

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 1.9 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 reweighting the data, 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 to both ever 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, these patterns flip if anything and remain significant only at the 10 percent level.[17]

Panel C looks at whether matched individuals differ from non-matched 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 inverse probability

[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).

weights. 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 socio-economic backgrounds than the population, with more literate household heads and household heads which 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 to 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]

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 5.41 years of schooling compared to 6.09 for black non-prisoners. The gap is larger for whites: prisoners have 7.01 years of school compared to 9.05 years for non-prisoners. Literacy is available in the 1920 and 1930

[18] They include ages 7 to 17 in 1900 through 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.

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 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, 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.[19]

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

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

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

[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, with state fixed effects, and with county fixed effects.

year including home ownership status, literacy of the household head, and indicators for household head occupation categories.

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 $\beta_3$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 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):

$$prisoner_{iarsct} = \alpha_c + \gamma_{tr} + \theta_{ar} + \pi_{st} + \beta_1 black_i + \beta_2 exposure_{ict} + \beta_3 rural_{it} +$$

$$\beta_4 black_i * rural_{it} + \beta_5 exposure_{ict} * rural_{it} + \beta_6 exposure_{ict} * black_i +$$

$$\beta_7 rural_{it} * black_i * exposure_{ict} + \varepsilon_{ict}$$

for individual of race *r* and the remaining same subscripts from 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 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 or 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.[22] 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. If incarceration rates were rising differentially in counties which built Rosenwald schools, then we expect β_6 to be positive.

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

[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.

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 which 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/literacy 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.[23] My estimating equation follows the 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

[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% IPUMS sample.

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” 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 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 which affect both races and rural/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—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; 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 which built Rosenwald schools 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.

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 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 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 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 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 OLS coefficient from regressing incarceration on education (controlling for age and birth state fixed effects) is -0.33 for blacks. However, this hides non-linearities: 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 in outside of 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 was not the only mechanism through which Rosenwald schools reduced incarceration. For example, higher

[24] The authors only find a significant effect of Rosenwald exposure on migration for 17 to 21 year olds; 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.

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 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 do not explain the full effect.

VI. Robustness of Main Results

The main results illustrate large negative effects of Rosenwald exposure on incarceration. This section shows results with alternative specifications, different matching procedures, different definitions of incarceration, and different weighting schemes. Next, I show that results are similar in unmatched data, although that data has many drawbacks. Finally, I conduct a permutation exercise which 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.

A. Robustness to Specification, Matching, Measure of Incarceration, and Weighting

In Table 10, I include different specifications in the main regressions. I start by reproducing the main result. One main worry with this specification is that it might not be fully controlling for different urban and rural age trends which are correlated with exposure to Rosenwald schools. While controlling for rural birthplace times black fixed effects is likely over-controlling because the outcomes of the exposed cohorts will be partially absorbed, I do this in Column (2) in order to see if the coefficient remains negative. While it becomes significant only at the 10% significance level for both measures of exposure, it remains negative and large. In the next columns, I control for rural birthplace times birth year race-specific trends, and then rural birth state times birth year times race fixed effects. Finally, I remove the previous controls but use county times rural times childhood census year fixed effects in the regression. 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.

Table 11 presents results with different matching procedures. The first column replicates the original results. Column (2) requires individuals to be unique within a five year age band (plus or minus two years) to reduce the chance of false positive matches. The match rate is much lower in this sample at 18.6 percent relative to 27.7 percent in the main sample, but the results are almost identical in magnitude. The third column returns to the iterative method but allows individuals to only match with only up to one year discrepancy in age; again, the larger two year discrepancies in age are more likely to be incorrect matches. The results are almost identical with this sample as well. Columns (4) and (5) follow Columns (1) and (2) but match on exact names instead of standardized names and also find similar results, if slightly smaller at 1.4 percentage points and 1.77 percentage points when requiring uniqueness within a five year age band. Note that the samples in these are over 50 percent smaller than with standardized names, likely because of spelling differences of names that sound phonetically the same. Finally, in Column (6), I show that introducing *more* false positive matches by allowing individuals to match up to five years off in age reduces the coefficient by introducing measurement error. The coefficient falls to an insignificant 1.18 percentage points.

Table 12 shows that results are insensitive to the choice of incarceration measure described in Section III. While the preferred method uses the group quarters and relationship variables from IPUMS, it also adds individuals in prison who had blank group quarters and relationship strings but who were coded by hand. Using a more hands off method, the second column uses only the group quarters variables in IPUMS to assign incarceration status. Column (3) fixes obvious mistakes in the IPUMS coding by coding individuals as non-incarcerated if they are household heads or children, and adding individuals to the incarcerated category if they have relationship strings of “Prisoner” or “Convict”. 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 inverse probability weights. My main results reweight by prisoner, race, and year, but do not correct for the differences in covariates described in Table 4. Therefore, in Table 13, I show that results with different weighting schemes. In the second column, I start by removing all weights. Results become smaller in magnitude because the mean incarceration rate falls. The effect of full exposure using the “Likely Seats” measure falls in magnitude to 1.45 percentage points. Column (3) uses the inverse probability weights estimated based on the childhood census year covariates from Table 4, while Column (4) uses weights calculated based on the adult census year characteristics from Table 4. Results using both of these 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. I use my preferred measure of incarceration and use county of residence in 1935 to assign Rosenwald exposure instead of relying on matched data. I note that this is imperfect because 25% of individuals from these birth cohorts had already left their state of birth by 1935 (11% had left the South entirely), suggesting that I may understate the main result due to measurement error in exposure. Furthermore, many prisoners were likely already incarcerated in 1935 which will also lead to incorrect assignment of exposure if they are incarcerated outside of their county of residence. Finally, residence five years ago is disproportionately missing for prisoners versus non-prisoners (13% versus 3%).

I use the “migcity” variable in IPUMS to assign rural status to individuals in 1935. I also restrict to men living in one of the 14 Rosenwald states in 1935 to compare results between the unmatched and matched data.

I show the results in Table 13. The first two columns use unmatched data, the next two columns used matched data but assign Rosenwald exposure using residence five years ago, and the final two columns use matched data and exposure based on childhood residence. The difference between the first and second sets of results is entirely due to selection differences between unmatched and matched data; the variable for residence in 1935 is reported in the census in 1940 so there is no information being used from the pre-1940 census. There are two differences between the samples used in the second and third set of results. First, I assign Rosenwald exposure based on childhood location instead of location in 1935. This requires matching so measurement error could be introduced here due to false matches. Second, I am able to use the full matched sample because I have residence in childhood for the full sample. This is key because the incarceration rates are much lower in data which restricts to living in the south in 1935 (the first two sets of results). Therefore, the difference between the second and third results is ambiguous in terms of having more or less measurement error in the measurement of actual Rosenwald exposure during childhood.

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 suggest 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 and proceed as follows:

Because the relative populations of counties differs widely across the south, I do not redistribute schools and then recalculate my measure of exposure. Instead, I assume that the “Likely Seats” measure would be the same when reshuffling the schools but reshuffle the counties in terms of the order they receive schools. For example, if County A received its first school in 1920 which could seat 20% of students and received another county in 1925 which increased the county’s capacity to 40% of students, I hold these numbers fixed. When reshuffling, I move County A’s exposure by birth year to a random County B. This happens for all counties which were exposed. Then, I recreate Exposure, Black*Exposure, Rural*Exposure, and Black*Rural*Exposure for each iteration.

I do the previous procedure 1,000 times and plot the coefficients in Figure 3. 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% of coefficients when using only receiving counties, and 0.3% 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 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 1.1

percentage point decrease in incarceration, which is larger in magnitude 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 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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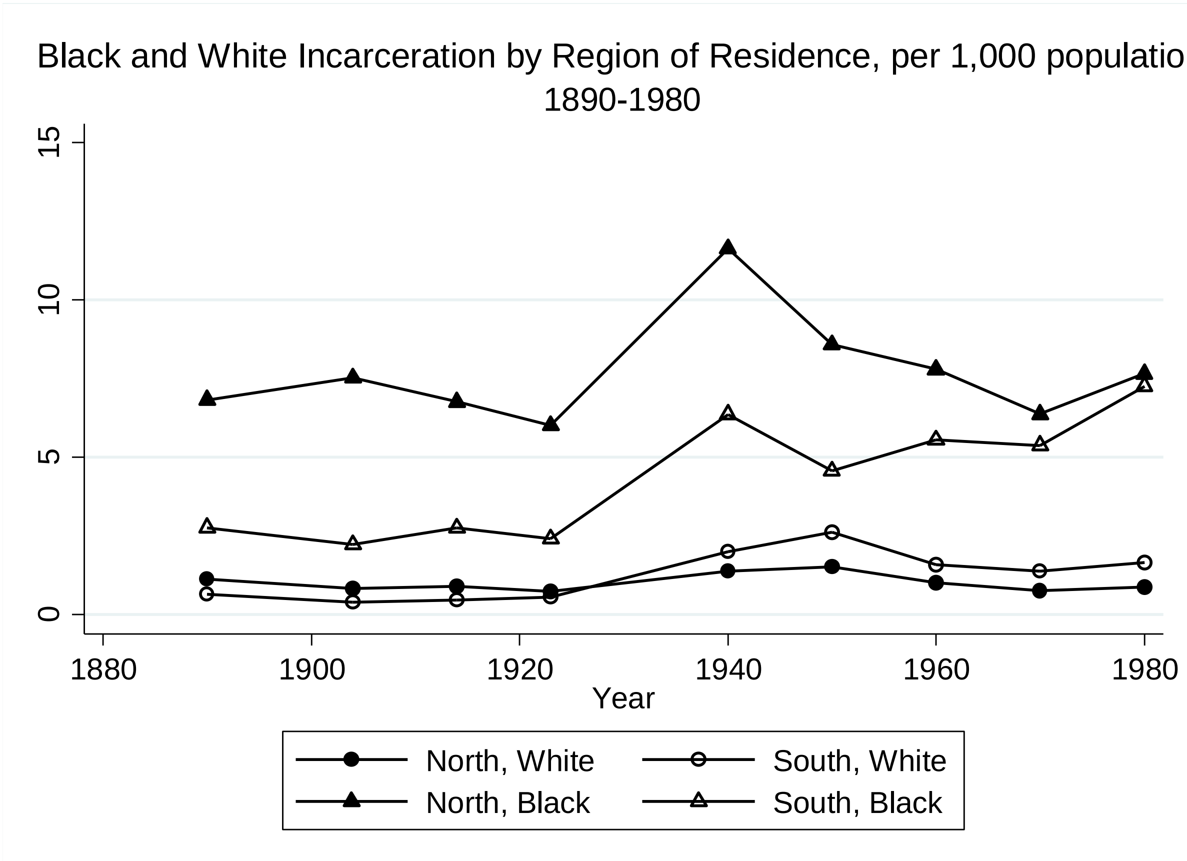
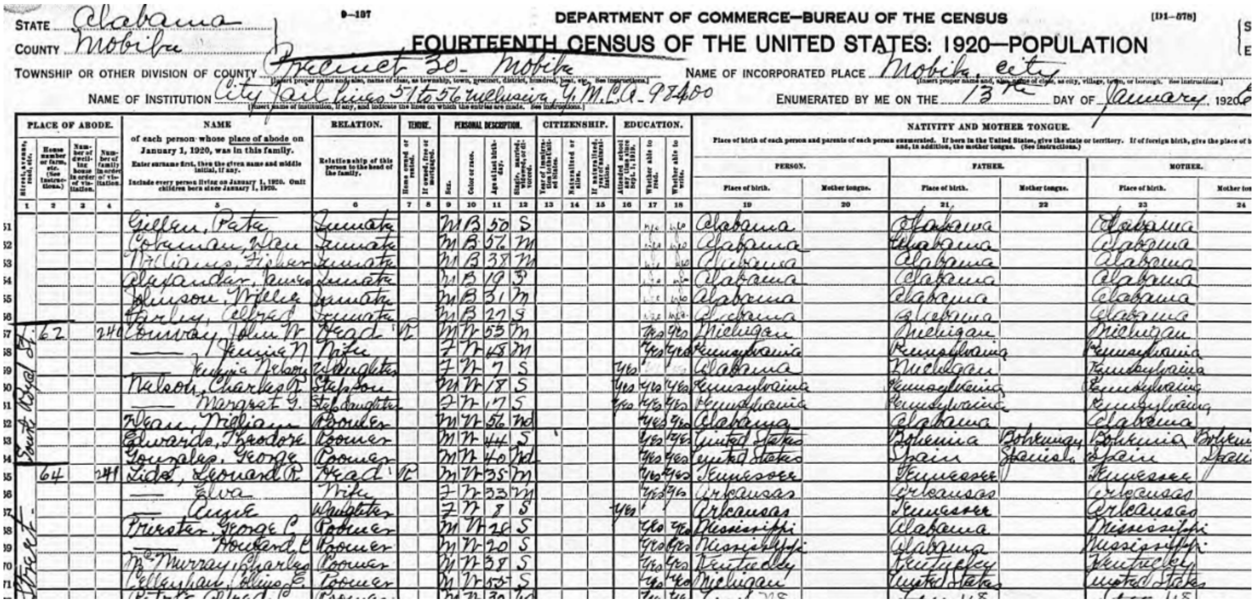
Figures

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 approximate male incarceration rates.

Figure 2: Example Census Image which is Incorrectly Coded by IPUMS



STATE Alabama — COUNTY Mobile — TOWNSHIP OR OTHER DIVISION OF COUNTY Precinct 30 - Mobile — NAME OF INSTITUTION City Jail lines 51 to 56 inclusive, Y.M.C.A. 98400

9—107 DEPARTMENT OF COMMERCE—BUREAU OF THE CENSUS [D1—878]

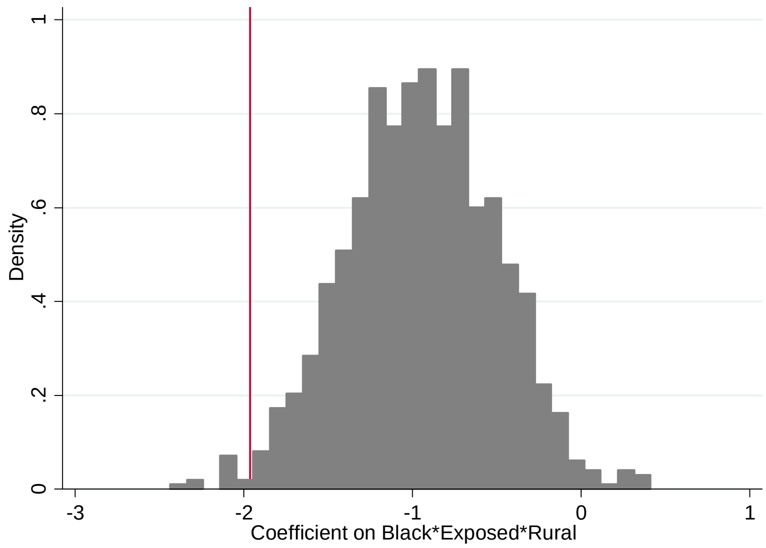
FOURTEENTH CENSUS OF THE UNITED STATES: 1920—POPULATION

NAME OF INCORPORATED PLACE Mobile, city — ENUMERATED BY ME ON THE 13th DAY OF January, 1920

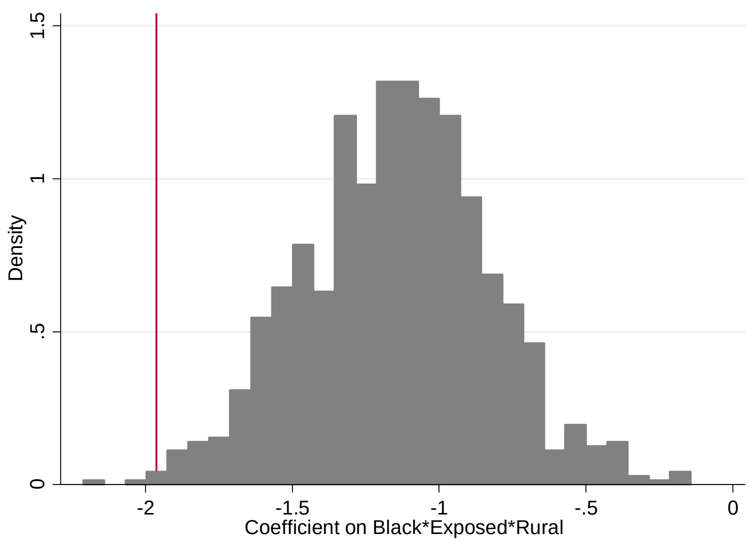
Line	Street (1)	House no. (2)	Dwelling no. (3)	Family no. (4)	Name (5)	Relation (6)	Home owned or rented (7)	Sex (9)	Color or race (10)	Age (11)	Marital (12)	Attended school (16)	Read (17)	Write (18)	Place of birth, person (19)	Place of birth, father (21)	Mother tongue, father (22)	Place of birth, mother (23)	Mother tongue, mother (24)
51					Gillen, Pate	Inmate		M	B	50	S		No	No	Alabama	Alabama		Alabama	
52					Coleman, Dan	Inmate		M	B	57	M		No	No	Alabama	Alabama		Alabama	
53					Williams, Fisher	Inmate		M	B	38	M		No	No	Alabama	Alabama		Alabama	
54					Alexander, [illegible]	Inmate		M	B	19	S		No	No	Alabama	Alabama		Alabama	
55					Johnson, Willie	Inmate		M	B	31	M		No	No	Alabama	Alabama		Alabama	
56					Farley, Alfred	Inmate		M	B	27	S		No	No	Alabama	Alabama		Alabama	
57	South Royal St.	62		240	Conway, John W	Head	R	M	W	55	M		Yes	Yes	Michigan	Michigan		Michigan	
58					—— Jennie N	Wife		F	W	48	M		Yes	Yes	Pennsylvania	Pennsylvania		Pennsylvania	
59					Eugenia Nelson	Daughter		F	W	7	S	Yes			Alabama	Michigan		Pennsylvania	
60					Nelson, Charles T.	Stepson		M	W	18	S	Yes	Yes	Yes	Pennsylvania	Pennsylvania		Pennsylvania	
61					—— Margaret G.	Stepdaughter		F	W	17	S	Yes	Yes	Yes	Pennsylvania	Pennsylvania		Pennsylvania	
62					Dean, William	Boarder		M	W	56	Wd		Yes	Yes	Alabama	Alabama		Alabama	
63					Edwards, Theodore	Roomer		M	W	44	S		Yes	Yes	United States	Bohemia	Bohemian	Bohemia	Bohemian
64					Gonzales, George	Roomer		M	W	40	Wd		Yes	Yes	United States	Spain	Spanish	Spain	Spanish
65		64		241	Lide, Leonard R.	Head	R	M	W	35	M		Yes	Yes	Tennessee	Tennessee		Tennessee	
66					—— Elva	Wife		F	W	33	M		Yes	Yes	Arkansas	Arkansas		Arkansas	
67	Frank St.				—— Annie	Daughter		F	W	8	S	Yes			Arkansas	Tennessee		Arkansas	
68					Priester, George C.	Roomer		M	W	28	S		Yes	Yes	Mississippi	Alabama		Mississippi	
69					—— Howard C.	Roomer		M	W	20	S		Yes	Yes	Mississippi	Alabama		Mississippi	
70					McMurray, Charles	Roomer		M	W	38	S		Yes	Yes	Kentucky	Kentucky		Kentucky	
71					Callaghan, Collins E.	Roomer		M	W	55	S		Yes	Yes	Michigan	United States		United States	

Figure 3: Permutation Test

Panel A: Assign Rosenwald Exposure among eventually treated counties only



Panel B: Assign Rosenwald Exposure among all Southern Counties



Notes: Figures plot estimated coefficients from 1000 placebo regressions after reshuffling Rosenwald school timing across eligible Southern counties. In Panel A, procedure only assigns Rosenwald exposure to those counties which will eventually be treated. In Panel B, procedure reassigns Rosenwald exposure to all Southern counties in the 14 Rosenwald states. Regressions use the “Likely Seats” measure of exposure where the share of students exposed to a school by birth year is held constant but the county with the schools is changed.

Tables

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

"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 individuals from the previous definition 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.

Table 2: Correlation between county characteristics and Rosenwald schools

	(1) Coverage 1919	(2) Coverage 1931	(3) Coverage 1931, Conditional on 1919	(4) Change in Coverage, 1919-1931	Sample Mean, X
Log White Enrollment, 1910	-0.009 (0.011)	0.023 (0.023)	0.029 (0.022)	0.033 (0.023)	4.256
Log Black Enrollment, 1910	0.001 (0.008)	-0.072*** (0.015)	-0.073*** (0.015)	-0.735*** (0.015)	3.743
Log Jail Expenditure, 1902	-0.001 (0.002)	0.001 (0.005)	0.001 (0.004)	0.001 (0.005)	-4.985
Log Court Expenditure, 1902	-0.014* (0.007)	0.006 (0.015)	0.014 (0.014)	0.020 (0.014)	-3.721
=1 if Lynching 1900-1920	-0.045** (0.013)	-0.037 (0.025)	-0.013 (0.025)	0.008 (0.025)	0.566
=1 if Execution 1900-1920	0.006 (0.009)	-0.018 (0.018)	-0.021 (0.017)	-0.024 (0.018)	0.521
=1 if Dry in 1910	0.004 (0.003)	0.009* (0.005)	0.007 (0.005)	0.005 (0.004)	0.882
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 is 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. Dry counties are defined as in ICPSR #8343 and Executions are taken from ICPSR #8451.

Table 3: Sample Sizes and Match rates by Adult census year and Prisoner status

			(1) Population	(2) Matched	(3) Match Rate
1940					
	Prisoners				
		Black	42,208	7,237	17.1
		White	37,215	7,293	19.5
	Non-Prisoners				
		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
	Non-Prisoners				
		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
	Non-Prisoners				
		Black	1,158,870	257,455	22.2
		White	3,085,221	843,898	27.4

Notes: Prisoners and non-prisoners 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 to men aged 18 to 35 in the adult census year; men less than or equal to 22 are matched to the previous census while men 23 to 35 are matched to a census twenty years prior.

Table 4: Comparing the matched sample to the full population

	(1)	(2)	(3)	(4)	(5)	(6)
		Black			**White**	
	Population	Difference: Matched – Population	Difference, weighted	Population	Difference: Matched – Population	Difference, weighted
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)
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 HH 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 non-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. Columns (3) and (6) replicate (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 non-prisoners because of lack of consistent wage reporting for prisoners. Regressions include robust standard errors. ***p<0.01, **p<0.05, *p<0.10.

Table 5: Summary Statistics

	Black		White	
	Prisoner	Non-Prisoner	Prisoner	Non-Prisoner
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 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 6: Reduced Form Results: Effect of Full Rosenwald Exposure on Incarceration, Blacks only

	(1)	(2)	(3)	(4)
Outcome:	=100 if in Prison	=100 if in Prison	=100 if in Prison	=100 if in Prison
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

	(5)	(6)	(7)	(8)
Outcome:	=100 if in Prison	=100 if in Prison	=100 if in Prison	=100 if in Prison
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
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 labelled "Sample Mean, black". Regressions include age, year, and county fixed effects in (3) and county-year fixed effects in (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 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 Full Rosenwald Exposure on Incarceration, Both Races

	(1)	(2)
	=100 if in Prison	=100 if in Prison
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 labelled "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 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: Effect of Rosenwald Exposure on Literacy, Education, and Migration, Full Sample

	(1)	(2)	(3)	(4)	(5)
Years:		*1920-1940*			*1940*
Outcome:	=100 if in Prison	=1 if Literate	=1 if outside South as adult	=100 if in Prison	Years of Education
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 labelled "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 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. 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

	(1) Original	(2) With Covariates
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 labelled "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 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 10: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
Weights:	Year FE Only	Preferred Specification	Rural*Birthyear Trends	Rural*Birthyear FE	County*Rural* Year FE
Black*Exposure*Rural	-1.963***	-1.227*	-0.971	-0.812	-1.145*
	(0.609)	(0.693)	(0.704)	(0.718)	(0.689)
Exposure Measure	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”
Mean Exposure	0.059	0.059	0.059	0.059	0.059
Sample Mean, black	2.105	2.105	2.105	2.105	2.105
Black*Exposure*Rural	-0.618***	-0.416*	-0.224	-0.174	-0.394
	(0.168)	(0.243)	(0.265)	(0.296)	(0.248)
Exposure Measure	“School in County”	“School in County”	“School in County”	“School in County”	“School in County”
Mean Exposure	0.232	0.232	0.232	0.232	0.232
Sample Mean, black	2.105	2.105	2.105	2.105	2.105

Notes: N = 4,373,395. 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 labelled “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 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 11: Robustness to Matching

	(1)	(2)	(3)	(4)	(5)	(6)
Matching Method:	Original	Unique within 5 years	Restrict age to up to one year discrepancy	Match on exact names	Match on exact name + unique within 5 years	Allow to match within 10 year band
Black*Exposure*Rural	-1.963***	-2.085**	-2.153**	-1.405**	-1.773**	-1.179
	(0.702)	(0.865)	(0.763)	(0.708)	(0.770)	(0.913)
Exposure Measure	"Likely Seats"	"Likely Seats"	"Likely Seats"	"Likely Seats"	"Likely Seats"	"Likely Seats"
Mean Exposure	0.059	0.059	0.059	0.059	0.059	0.059
Sample Mean, black	2.105	2.105	2.105	2.105	2.105	2.105
Black*Exposure*Rural	-0.618***	-0.577**	-0.698***	-0.711***	-0.807**	-0.330
	(0.197)	(0.260)	(0.209)	(0.207)	(0.218)	(0.279)
Exposure Measure	"School in County"	"School in County"	"School in County"	"School in County"	"School in County"	"School in County"
Mean Exposure	0.232	0.232	0.232	0.232	0.232	0.232
Sample Mean, black	2.105	2.105	2.105	2.105	2.105	2.105
Match Rate	27.5	18.6	25.1	15.1	6.9	35.6
N	4,373,395	2,961,621	3,983,265	2,405,506	1,096,587	5,666,636

Notes: Sample sizes and match rates in last two rows apply to both panels. See Table 7 notes for sample and regression specification. Original matching procedure uses iterative match procedure of Abramitzky, Boustan, and Eriksson (2012); method requires exact match on (NYSIIS standardized) name, birth state, and race and iteratively allows up to two years of discrepancy in age. Column (2) requires individuals to be unique within a five year (plus or minus two years) age band in each year. Column (3) allows individuals to only match with up to one year discrepancy in age. Columns (4) and (5 replicate (1) and (2) but use raw name strings instead of the NYSIIS standardization. Column (6) allows individuals to match iteratively with up to five years discrepancy in age. ***p<0.01, **p<0.05, *p<0.10.

Table 12: Robustness to Incarceration Measure

	(1)	(2)	(3)
Incarceration Measure:	Preferred	Group Quarters	Group Quarters + Relationship
Black*Exposure*Rural	-1.963***	-1.888*	-1.742***
	(0.700)	(0.659)	(0.667)
Exposure Measure	"Likely Seats"	"Likely Seats"	"Likely Seats"
Mean Exposure	0.059	0.059	0.059
Sample Mean 1940, black	2.105	1.655	1.769
Black*Exposure*Rural	-0.618***	-0.521***	-0.409**
	(0.197)	(0.187)	(0.209)
`Exposure Measure	"School in County"	"School in County"	"School in County"
Mean Exposure	0.232	0.232	0.232
Sample Mean, black	2.105	1.655	1.769

Notes: N=4,373,395. See Table 7 notes for sample and regression specification. Regressions are weighted to have correct incarceration rates within each year and race cell. Incarceration status refers to status in adult years of 1920, 1930, and 1940. "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 individuals from the previous definition 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. ***p<0.01, **p<0.05, *p<0.10.

Table 13: Robustness to Weighting

	(1)	(2)	(3)	(4)
Weights:	Prison/Race only (Original)	No Weights	Childhood Census Year	Adult Census Year
Black*Exposure*Rural	-1.963***	-1.449***	-1.862**	-2.095***
	(0.700)	(0.548)	(0.620)	(0.697)
Exposure Measure	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”
Mean Exposure	0.059	0.059	0.059	0.059
Sample Mean, black	2.105	2.105	2.105	2.105
Black*Exposure*Rural	-0.618***	-0.450***	-0.417***	-0.621***
	(0.197)	(0.157)	(0.153)	(0.204)
Exposure Measure	“School in County”	“School in County”	“School in County”	“School in County”
Mean Exposure	0.232	0.232	0.232	0.232
Sample Mean, black	2.105	2.105	2.105	2.105

Notes: N = 4,373,395. See Table 7 notes for sample and regression specification. Regressions include county times childhood census year fixed effects. Original weights correct for differential match rates within prison/year/race cells to recover original population incarceration rates. Column (2) removes weights. Column (3) uses an inverse proportional weighting procedure to reweight the matched sample to the population based on childhood observable characteristics: Rosenwald exposure, birth state, race, age, household head literacy, school attendance, and household farm status. Column (4) uses an inverse probability weighting procedure to reweight the sample to the population based on adult observable characteristics: years of education, indicators for no education and less than eight years of education, state of residence, state of birth, race, prison status, and age. ***p<0.01, **p<0.05, *p<0.10.

Table 14: Results with Unmatched 1940 Census

	(1)	(2)	(3)	(4)	(5)	(6)
	Unmatched Data			*Matched Data*		
	=100 if in Prison	Education	=100 if in Prison	Education	=100 if in Prison	Education
Black*Exposure*Rural	-1.884**	0.731**	-1.346	0.992***	-1.429*	1.277***
	(0.842)	(0.288)	(0.863)	(0.317)	(0.772)	(0.348)
Exposure*Rural	-0.402**	-0.249**	-0.479**	-0.190*	0.078	-0.095
	(0.189)	(0.112)	(0.215)	(0.107)	(0.158)	(0.110)
Black*Exposure	0.837	-0.093	0.659	-0.101	1.871*	-0.305
	(0.763)	(0.345)	(0.775)	(0.374)	(0.757)	(0.363)
Black*Rural	-0.154	-0.091	-0.269	-0.323***	-1.167***	-0.324***
	(0.213)	(0.098)	(0.235)	(0.108)	(0.148)	(0.118)
Rural	-0.129*	-1.602***	-0.128*	-1.585***	0.081*	-0.675***
	(0.077)	(0.045)	(0.073)	(0.044)	(0.045)	(0.039)
Exposure	0.322	0.176*	0.333	0.069	0.032	0.659***
	(0.200)	(0.107)	(0.217)	(0.098)	(0.166)	(0.110)
Black	1.117***	-2.726***	1.263***	-2.629***	2.781***	-2.074***
	(0.207)	(0.121)	(0.256)	(0.131)	(0.146)	(0.119)
Prison Measure	Preferred		Preferred		Preferred	
Exposure Measure	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”	“Likely Seats”
Exposure + Fixed Effects	County in 1935	County in 1935	County in 1935	County in 1935	County in Childhood	County in Childhood
Mean Exposure	0.127	0.127	0.129	0.129	0.131	0.131
Sample Mean, black	5.519	1.832	5.848	1.893	2.552	6.120
R^2	0.264	0.030	0.244	0.038	0.255	0.008
N	5,018,955	5,150,853	1,484,749	1,521,436	1,730,760	1,775,391

Notes: Table uses the previous matched data, restricted to outcome year 1940, for the first two columns and the full count unmatched 1940 census for the last two columns. The first two columns further restrict individuals to still be living in one of the Rosenwald states in 1935 to be consistent with the restriction in the unmatched data. The “Group Quarters” prison measure is used to be consistent with the unmatched data where information is not available to calculate the other two measures. The binary outcome variable for incarceration is multiplied by 100 so that the coefficients are expressed as percentages instead of proportions. Mean for blacks in 1940 are given in the row labeled “Sample Mean, black”. Regressions use the “Likely Seats” measure of exposure. Regressions include age, black*age, and county fixed effects, where county is the county in 1935 (cols 1 - 2) or county in childhood (cols 5 and 6). For Columns (1)-(4), I restrict to those living in the South in 1935; in Columns (5) and (6), I restrict to those living in the South in 1935 and those aged 18 to 35 in 1940. Standard errors are clustered by county. Columns (1) – (4) use the “migcity” variable in IPUMS to assign rural status in 1935. “Rural” refers to living in a rural place in childhood in Columns (5) and (6). ***p<0.01, **p<0.05, *p<0.10.