Access to Schooling and the Black-White Crime Gap in the Early 20th Century US South: Evidence from Rosenwald Schools

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Abstract: A large gap in incarceration rates between black and white men has been evident since the early 20th century. This paper examines the effect of access to schooling on black crime in this historic period. I use the construction of 5,000 new schools in the US south, funded by northern philanthropist Julius Rosenwald between 1913 and 1932, as a quasi-natural experiment which increased the educational attainment of southern black students. I match a sample of male prisoners and non-prisoners from the 1920-1940 Censuses backwards to their birth families in previous Census waves in order to assign exposure to Rosenwald schools based on birth cohort and childhood county of residence. I find that one year of access to a Rosenwald school decreased the probability of being a prisoner by 0.04-0.10 percentage points (10-15 percent of the mean). Using other data from archival and government sources, I show that Rosenwald schools affected juvenile crime and all categories of adult crime. Multiple identification strategies corroborate my results. I investigate the channels through which Rosenwald schools reduced crime, including educational attainment, school quality, and migration responses. These results contribute to a broader literature on causes of black-white differences in the US throughout the 20th century.

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I. Introduction

In the contemporary United States, black men are disproportionately more likely than white men to be arrested for crime and to be incarcerated. This racial gap in incarceration has been apparent in the data since the late nineteenth century. In 1910, blacks made up only thirteen percent of the population but twenty-seven percent of those in prison.[2] In the South in 1910, blacks comprised 30 percent of the population yet made up 60 percent of those incarcerated (US Census Report 1910).[3] In 2007, the Department of Justice estimated that victims of crimes face $15 billion in losses and that the prevention and punishment of crimes cost $179 billion for local, state and federal governments. Committing crimes also has long-run costs for the criminal; individuals with a criminal record often find it difficult to get a job, and those jobs they do get pay lower wages (Western, Kling, and Weiman 2001). The racial incarceration gap could have many causes, including discrimination in arrest and sentencing, differences in family background, lack of job opportunities for blacks, higher urbanization rates of blacks, and differences in educational attainment.

This paper explores the role of one of these factors – disparities in education – in explaining the historical roots of the racial gap in crime. In particular, I analyze the relationship between access to education and the probability of incarceration between 1920 and 1940 among southern-born black men in the United States. I use the construction of almost 5,000 schools in 15 southern states for rural black students between 1913 and 1931, sponsored in part by northern

[2] In 2009, this disparity was even larger. Blacks made up 13.9 percent of the population and 39.4 percent of those incarcerated. This is likely due to the emergence of a series of factors which have led to higher black incarceration rates such as stricter penalties for drug offenses.

[3] In the North, blacks were 1.8 percent of the population but 9.6 percent of those incarcerated.

philanthropist Julius Rosenwald, as a quasi-natural experiment which increased the educational attainment and literacy of blacks born in the South.

I build a new dataset that matches the full universe of southern-born male prisoners from the 1920-1940 US Censuses, along with a comparison sample of non-prisoners, to their birth families in a previous census earlier in life. Based on their location in childhood, I assign to these men their likely exposure to a Rosenwald school, defined as the number of years between the ages of seven and thirteen that a Rosenwald school was present in his county. I also construct an alternative measure of exposure, which is weighted by the probability that any child could have obtained a seat in the school in each year. That is, if a child had a school in his county for one year, but it could only serve half of the students, he would be assigned one half of a year by this measure.

I find that access to education reduces crime later in life among adults. Exposure to a Rosenwald school for one year during childhood reduces the probability of committing a crime for blacks by between 0.05 and 0.1 percentage points or 10-15 percent of the mean incarceration rate for blacks. This finding is robust to different comparison groups and alternative specifications. I find that most of the effect is among students whose county received a school when they were between seven and nine rather than between ages ten and thirteen.[4] I find that literacy is an important channel through which crime decreases, but also consider the idea that the returns to school also increased with exposure to Rosenwald schools.

I use county fixed effects in my estimation and utilize white men as a control group. Identification of the effect of Rosenwald schools on crime depends on variation in school construction across cohorts and races within a given county. I use county fixed effects in my

[4] This fits with a pattern of early childhood interventions being more important than ones in later childhood (Cunha, Heckman, Lochner, and Masterov 2006). Alternatively, it could be that children whose counties did not receive a school until later in childhood "missed the boat".

estimation and utilize white men as a control group. Potential threats to identification can arise from characteristics of counties that change over time in a way that was correlated with both the construction of new schools and with subsequent criminality. I address this concern by constructing a different measure of exposure which is based on the distance of a household from a school within a county; this design compares children within a certain distance from the school to those who did not grow up close to a school but who grew up in the same county.

A second concern might be that parental background differs among those who have exposure to schools and those who do not; for example, more literate parents could be migrating into certain counties as Rosenwald schools are being built. I provide evidence that this is likely not true but I also present estimates which compare brothers within the same family to control for family background differences. This identification strategy also addresses possible endogenous placement of schools within and across counties.

Finally, I collect data from non-census sources in North Carolina to look at some potential mechanisms which might explain my main result. I collect data on juvenile crime and categories of adult crime. I find that juvenile court cases decrease for black children in counties with exposure to Rosenwald schools. One year of access to a Rosenwald school reduces juvenile court cases by about 15 percent of the mean. These results present some suggestive evidence that schools reduced juvenile transgressions which could have led to criminal activity in adulthood. I also collected archival data from the state prison registers in the North Carolina State Prison which include information about type of crime committed. Within a multinomial logit framework, I show that Rosenwald schools reduce all categories of crime and that the effect is not restricted to the types of "nuisance" crimes for which blacks were regularly imprisoned in the Jim Crow South (Naidu 2010).

This paper contributes to two literatures. The first concerns the convergence in wages and other outcomes between blacks and whites over the 20th century. The educational attainment of three years on average between blacks and whites at the beginning of the 20th century, which was caused by poor investments in black schools and the legacy of slavery, declined between 1910 and 1940 (Margo 1990, Aaronson and Mazumder 2011). These improvements in black educational attainment have contributed to a decline in the black-white wage gap (Heckman, Lyons and Todd 2000, Smith and Welch 1977). My estimates suggest that the black-white incarceration gap should have declined by fifty percent over the 20th century 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. Future work will consider the role of migration to cities and the North as a counteracting force.

This paper also speaks to work on 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, with the most relevant being Lochner and Moretti (2004) who find, in a contemporary context, that the probability of incarceration for blacks and whites decreases following an additional year of school.[6] 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. I find that one year of school reduces the

[5] Other research has 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 2012, 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 2012) and finds a significant effect. Anderson (2012) shows that juvenile crime decreases with higher minimum dropout ages for students.

probability of being incarcerated by 0.25-0.5 percentage points which is similar to Lochner and Moretti who find a social return to school of 0.4 percentage points. Note, however, that the mean incarceration rate in contemporary data is 3.6 percent whereas in my historical context it is only 0.8 percent. Additionally, 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.

The structure of this paper is as follows. Section II provides historical background about black/white differences in crime and in schooling through the 20th century. In section III, I describe the data and exposure to Rosenwald schools. Section IV discusses my identification strategy and potential threats to identification. Section V presents results from my primary sample and additional data. Section VI examines robustness and alternative specifications. I conclude in Section VII.

II. Historical Background

A. Incarceration rates by race and region over time

The gap between white and black incarceration rates was evident as early as 1890. In 1890, blacks were 3.2 times more likely to be incarcerated than whites (Petersilia and Reitz 2012).[7] Contemporary observers attributed this difference in crime rates to a variety of factors: poverty, discrimination, lack of education, "moral degradation", etc (Sellin 1900). Figure 1 shows the evolution of black and white state and federal prison incarceration rates from 1926 to 1986 (Department of Justice, 1986). Panel A reveals that incarceration rates were higher for blacks than for whites in all years, with approximately eight per 10,000 blacks incarcerated nationally through the early 1960s compared to four whites. The racial gap is present in both the North and

[7] In 2010, this number was 6.4. It grew from 3.2 in 1890 to 4.3 in 1923, and was as high as 7.1 in 2000.

the South, despite higher incarceration rates in the North in the 1920s and 1930s. These graphs demonstrate 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 crime rates needs to take into account the historical patterns of incarceration. Most literature has focused on the evolution of this gap since the 1970's.

The debate about whether, and to what extent, education reduces crime 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 crime 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 at very little benefit to blacks or whites. In fact, Governor Vardaman of Mississippi used this reasoning when restricting funds for black schools in 1904 (Hollandworth 2008). On the other side of the argument, Booker T. Washington and W. E. B. Du Bois sought to explain higher black criminal behavior as a result of low wages and discrimination. The issue of high black incarceration rates was one motivation for Booker T. Washington's interest in improving black schools, out of which grew the Rosenwald Initiative.

B. Black schooling and Rosenwald schools

Blacks in the birth cohorts of 1880 and 1910 completed on average three fewer years of education than whites. Motivated by his concerns about the low funding levels for black education, Booker T. Washington, principal of the Tuskegee Institute in Alabama, reached out to

Northern philanthropist and businessman Julius Rosenwald.[8] Rosenwald agreed to fund a pilot program supporting the construction of six black schools in 1913-14, with the promise of up to 100 more. The original schools were built primarily in Alabama; by 1920, the program supported 716 schools in eleven southern states. The Rosenwald Fund explicitly targeted rural students.

Rosenwald believed that communities must "buy-in" to, or invest in, any endeavors. This view, coupled with Washington's belief in black self-reliance, led to the use of a matching grant approach, whereby local communities had to raise anywhere from 75 up to 90 percent of the funds for a new school. The early schools received about 25 percent 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 Section IV, I analyze the location and timing of school construction. In addition to building 4,983 schools by 1931, the Fund also provided some money for teacher training schools, homes, and shops.

By the end of the program, 76 percent of counties in the 15[9] southern states had a school and 92 percent of black students in these states lived in a county with a school. Aaronson and Mazumder (2011) estimate that the schools could serve 36 percent of rural black students. They show that the Rosenwald schools were a significant contributor to the narrowing of the black-white schooling gap by 1940. Black cohorts born through 1909 received three fewer years of school than white cohorts. This gap narrowed to half a year for cohorts born after 1940. The gap remained constant for cohorts born through the 1950s. Aaronson and Mazumder estimate that

[8] The Rosenwald School Initiative was not the only black schooling initiative in this time. The Jeanes Fund provided teacher training. Other philanthropic interventions are described in Donohue, Heckman and Todd (2002).

[9] I omit Missouri from my analysis because only 11 schools were built there.

Rosenwald schools increased school attendance by about 5 percentage points. They calculate that through this increase in enrollment, Rosenwald schools account for 40 percent, or one year, of the reduction in the racial schooling gap.

I estimate the effect of access to a Rosenwald school for one year during childhood. Implicit in this analysis is the assumption that the in the counterfactual world, students would not have access to a school; that is, Rosenwald schools increased access to schools because funds were not diverted to other uses. 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% of this gain accrued to white schools. They also find evidence of crowd-out in terms of teachers and school buildings. 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.

Finally, this school building program was taking place during a period of high levels of migration to the North. I consider this as a potential mechanism in my later analysis, but also note that it was a potential motivation for counties to make use of Rosenwald funds. Margo (1991) argues that investments in education were one way that southern governments could discourage migration to the North.

III. Data

A. Primary sample

My measure of crime is an individual-level propensity of a man who grew up in a given county or a town to be incarcerated later in life.[10] I calculate this measure from US Census data

[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

for the years 1920-1940. To do so, I assemble a sample of prisoners and non-prisoners in each relevant census. Prisoners are defined as men reporting a relationship to household head of "Prisoner" in the full census indexes from FamilySearch.org. A robustness check also includes men whose relationship to the household head is listed as "Inmate."[11] Traditionally, measures of crime do not include inmates of county jails, so in my analysis for North Carolina, I restrict to those incarcerated in State or Federal Prisons.[12] Non-prisoners are found either in the IPUMS 1% samples of 1920 and 1930 or from the full index of the 1940 Census, which has only recently been made publically available. I restrict the sample to men between ages 18 and 35 who were born in the South.

I identify the town or 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. Men less than 26 years old are matched to the previous census while those between 27 and 35 years old are matched over a twenty year period.[13] To match individuals, I follow the procedure pioneered by Ferrie (1996) and used in Abramitzky, Boustan, and Eriksson (2012). I first standardize first and last names using the NYSIIS algorithm (Atack and Bateman 1992) which spells names with the same phonetic sound identically. Individuals are then matched by first name, last name, state of birth, race, and age across Census waves. I allow individuals to

the first paper to collect individual data by race and for the full country. Moehling and Piehl (2011) collect individual data for immigrants living in select states in a similar time period.

[11] "Inmate" can refer to inmates of prisons but also can refer to inmates of mental institutions or other institutionalized group quarters (e.g. orphanages). To minimize the probability of falsely categorizing an individual as a criminal, I do not use inmates in my primary sample.

[12] The DOJ omits local jails and county detention centers from most statistics about crime, arguing that since most people are only in local jails for a few days to a few months, counting inmates at any one point in time will understate the number incarcerated in a given year. Furthermore, many inmates of jails are there for minor crimes (e.g. vagrancy) as opposed to more serious crimes warranting incarceration in state prisons.

[13] This cutoff was determined by calculating the age at which approximately 90% of individuals appear to be living at home in the 1910 census. The percentage living at home at young ages increases with later censuses. In 1910, 88.6% of black children and 92.4% of white children who are 16 years of age or younger report a relationship to household head of "child", "grand-child", "sibling", or "other relative". Results are robust to using a cutoff of 14 or 28 years old (these correspond to cutoffs of 24 and 28 years as adults) between the two matched samples.

misreport their age by up to two years in either direction. Inherent in any matching procedure is a trade-off between sample size and accuracy. A second sample prioritizes accuracy by requiring individuals to be unique by name/birthplace/race within a 5 year age band.[14] Accuracy may also come at the cost of representativeness if the uniqueness of an individual's name is correlated with his socio-economic status. The match rates in my study are consistent with the literature, averaging around 20 percent over a single Census decade and 19 percent over a 20 year period. Matching details and sample sizes are available in Appendix A.

In North Carolina, I collect additional information about my prisoners and non-prisoners. I collect the name of the prison by hand and restrict my dataset to consider only state and federal prisoners since inmates of local jails could be there for a number of reasons not related to criminality (e.g. vagrancy, etc). I also hand-collect literacy for the full sample of prisoners born in North Carolina. To allow for heterogeneous effects of the school-building program on crime, I collect household characteristics for the sample born in North Carolina. These variables include parents' literacy, home ownership, number of siblings, and the occupation of the household head.

Summary statistics are shown in Table 1. Black individuals are three times more likely than whites to be incarcerated in my sample (0.6 percent versus 0.2 percent). Prisoners tend to less literate, have less literate parents, and are more likely to have parents who are laborers rather than owner-occupier farmers. Finally, more prisoners are found outside of their state of birth than non-prisoners, but this pattern is mainly driven by migration of white prisoners (not shown).

Finally, I follow brother-pairs across censuses to control for any unobserved heterogeneity by household background which may account for my results. Both brothers were exposed to the same environment growing up: they presumably lived in the same town, had the same genetic characteristics, and were exposed to the same church environment. I find similar results for this

[14] Results are not shown here but are similar and are available in the online appendix.

sample. If anything, the effect of Rosenwald schools is stronger when accounting for household background characteristics.

B. Rosenwald schools

Information about the Rosenwald school program is taken from Aaronson and Mazumder (2012). 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 fifteen Southern states.[15]

The school-level cards do not, for the most part, contain information on the school's address within a county. For the North Carolina sub-sample of 775 schools, I used historical society documents and school directories (North Carolina Department of Education 1937, 1942) to determine the town in which the school was located. I was able to locate 720 schools. This procedure is limited to North Carolina because it is the only southern state with readily available school directories. One might worry that North Carolina is not representative of the south.[16] North Carolina was in fact one of the poorest states in 1910, with the lowest per capita income and low literacy rates. I expect that results might be stronger in this state than in other states.

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

[16] In 1910, North Carolina's per capita income was only $123, similar to South Carolina's $126 but lower than all other Southern states (Klein 2009). However, North Carolina's black pupil expenditures tracked those of other states between 1910 and 1950 while expenditures on white students were significantly lower than other states (Margo 1990). Furthermore, North Carolina had one of the lowest literacy rates in the region, with 81.6 percent of white men and 49.6 percent of black men between 20 and 64 being literate in 1900 (IPUMS 2010).

C. Calculating Exposure to Rosenwald Schools

Exposure to a Rosenwald school varies across towns and counties in the South as well as by birth cohort. Children who were born before 1906 would have been too old to attend even the first Rosenwald school. As the program expanded, later birth cohorts enjoyed a higher likelihood of attending a Rosenwald school. However, the Rosenwald program did not build schools uniformly throughout the South; out of 1,340 counties in the states with Rosenwald schools, 65 percent received at least one school. Aaronson and Mazumder (2011) find little correlation between pre-existing black socioeconomic characteristics and the placement of Rosenwald schools in a county but do find a relationship between white literacy and school construction. Carruthers and Wanamaker (2013) argue that schools were more likely to be built in larger counties with higher urbanization, per-pupil spending and enrollment of black youth. I discuss these and other potential threats to identification in Section IVB. I also control for observable time-varying county characteristics in my regressions.

For the full southern sample, I assign to individuals a measure of their likely exposure to a Rosenwald school based on their age and their 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; this varies between zero and seven. This measure is referred to as "School in County" in result 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. This measure is smaller than the first. The second measure is used in most analysis and is referred to as "Likely Seats".

I take advantage of the specific school locations in the North Carolina sample to create a potentially more exogenous measure of exposure. One concern with the full southern sample is that the placement of Rosenwald schools may not be truly exogenous. Even if the timing of school construction is not correlated with time-varying county-level characteristics, it could have been affected by unobservable factors like the degree of racial antipathy. This measure is calculated as the number of years a student had a school within a 5 mile radius of their town between the same ages as above. In this *within* county analysis, I compare individuals who were the same age and lived in the same county but who grew up different distances from a school.

Finally, in my within-brother analysis, I compare brothers who grew up in the same county, and who were the same race, but who were slightly different ages which accounts for their differential exposure to Rosenwald schools. For this analysis, I use the "Likely Seats" measure of exposure.

D. Additional North Carolina Data Sources: Prison Registers and Juvenile Court Cases

One drawback to using Census data on incarceration to measure crime rates is that they do not include information about the severity of the crime or length of the sentence. Furthermore, a count of individuals in prison at a point in time captures mostly new criminals, not all individuals who have been engaged in criminal activity in the past (who may have since been released from prison). For these reasons, I collected the full universe of prisoners admitted to the State Prison in North Carolina between 1910 and 1955 from the prison registers at the North Carolina State Archives. Available variables include date of admission, length of sentence, crime, county of birth, county of sentencing, age, gender, race, education, occupation, height, and weight.[17] I create a 10 percent random sample from this group of more than 30,000 prisoners, resulting in a

[17] For legal reasons, I was not allowed to copy the names of prisoners.

usable sample size of 2,765. I pool this data with my sample of non-prisoners and calculate exposure to a Rosenwald school based on the reported birth county and age as above. Outcome variables in the analysis include an indicator for being a prisoner, sentence maximum and minimum length, and the type of crime. The drawback of this data source is that it does not account for out-migration from North Carolina; approximately 1/4 of prisoners born in North Carolina were in jail outside of North Carolina in my primary sample.

One additional benefit of this data is that I can look at racial patterns or biases in sentencing for the same crime. I regress minimum or maximum sentence length on indicators for being black and available demographic characteristics. Without controlling for severity of crime, sentences of blacks are 1.1 years longer than for whites. However, this difference goes away when including fixed effects for type of crime, suggesting that higher sentences for blacks are driven not by discrimination but by the fact that blacks commit more severe crimes.[18]

I also collect complete county-race-year counts of inmates admitted to the North Carolina State Prison from the State Prison reports between 1930 and 1955. I assign exposure as the weighted average of exposure of individuals based on the age distribution of admissions. That is, I calculate the number of years individuals of each age would have had a school in their county and then take the weighted average based on the age distribution of admissions which is available aggregated to the state level. The number of admissions increases almost tenfold between 1933 and 1935 so I restrict to post-1935.[19] Prior to 1931, data is not available broken down by race.

[18] Note, however, that this does not provide definitive evidence that there is no discrimination in sentencing. It could be that blacks and whites commit the same crimes but that black crimes are "labeled" as more severe crimes in the prison register to justify higher sentences for blacks. If crimes had standard sentences, then a judge would have to convict a black person of a more serious crime to increase his sentence length.

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

Because juvenile delinquents were so infrequently incarcerated, they are unlikely to show up in the census data.[20] Children had to be at least 16 years of age to be sent to a state prison. I use county-level data on juvenile court cases by race coded by Wiley Britton Sanders (1945) to estimate the effect of the Rosenwald program on juvenile delinquency. I have eight data points per county: separate counts for black and white children for the time periods 1919-29[21], 1929-34, 1934-39 and 1939-44. Ideally, one would use annual data to trace out the effect of a new Rosenwald school but the underlying data from which Sanders constructed his series appears to be lost. I calculate average exposure for each county in the time period and assign this to the relevant data point. Specifically, the exposure measure is calculated as the number of years in this time period that the county had a school.

E. Heterogeneity

In addition to collecting information about household background characteristics, I use data from multiple other sources to examine whether the effect of Rosenwald schools depends on local county characteristics. Compulsory schooling laws might augment the effect of new school construction. Rosenwald schools likely made schools available in areas where children could not attend school so I expect that a Rosenwald school in an area with compulsory schooling would have a larger effect on literacy/human capital and therefore a larger effect on crime than one somewhere without compulsory schooling laws.

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

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

I also look at effects that differ by county economic characteristics such as share of employment in farming or manufacturing. Finally, I use data on lynching and voting behavior to see whether the effects differ between more and less "racist" counties.[22]

IV. Estimation Strategy

A. Reduced Form Estimation

I estimate the effect of being exposed to a Rosenwald school for one additional year on the probability of being a criminal, measured by incarceration, later in life. My estimation strategy exploits differences across counties in the number of Rosenwald schools as well as variation across cohorts within a county in exposure to a Rosenwald school at relevant ages. I also contrast black and white students in the same county and cohort. All regressions also include dummy variables for each age by race to allow for different white and black age-crime profiles. I create sample weights to account for the full population of prisoners but only one percent sample for the comparison sample; prisoners receive a weight of one while the comparison group receives a weight of 100.

My preferred estimation strategy within North Carolina makes use of variation in the length of exposure to Rosenwald schools and in the exact placement of these schools within counties. Here, *Exposure* is equal to the number of years that an individual could have attended a Rosenwald school within five miles of their childhood home. *Exposure* is equal to zero for men who lived too far away from a Rosenwald school or who were too old to take advantage of their local school.

My main estimating equation (1) is a linear probability model[23]:

[22] Heterogeneity results are not complete yet.

$$prisoner_{ict} = \alpha_c + \gamma_t + \beta_1 black_i + \beta_2 exposure_{ict} + \beta_3 black_i * exposure_{ict} + \epsilon_{ict}$$

where *Prisoner* equals one if the individual is incarcerated. I include county fixed effects, year fixed effects, childhood census year*state fixed effects, and age and age*black fixed effects.[24]

The coefficient of interest in this equation is β_3 which measures the additional effect of a year of exposure to a local Rosenwald school on black youth over any effect there may be on white youth. The total effect of exposure on black incarceration is $\beta_2+\beta_3$.

This equation uses multiple sources of variation to address potential concerns about bias in the estimate of β_3. I compare blacks and whites to control for any shocks that are correlated with Rosenwald school construction but which affected both races equally. I look within a county with county fixed effects to control for any differences between counties which received schools and those that did not. I also implicitly compare older (unaffected) cohorts to younger cohorts who were exposed to a school. The estimate will only be biased if there are local events that are correlated with the timing of construction of Rosenwald schools, correlated with trends in incarceration, and which affect the older and younger cohorts differentially.

Without county fixed effects we might expect the estimate to be biased downwards (more negative) since it is plausible that counties with Rosenwald schools had positive attitudes towards their black populations which might also translate into lower incarceration rates. Furthermore, using whites as a comparison group for blacks allows me to wash out anything which affects both blacks and whites and which is correlated with Rosenwald schools and crime. Any confounding factors must affect only black residents but not neighboring whites. Finally,

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[23] Results from a probit regression are quantitatively similar. However, probit regression is inconsistent in a regression with fixed effects so I prefer the linear probability model.

[24] Results are robust to including county-year census covariates such as literacy, home ownership, % farming (not shown).

note that Rosenwald school exposure is measured in childhood but crime is measured at least ten years later so any county-level confounding factor would have to be constant throughout this gap.[25]

B. Rosenwald schools and Literacy

Thus far, my main interest has been the direct effect of Rosenwald schools on crime. One likely channel through which Rosenwald schools reduced criminality is by increasing the literacy rates and educational attainment of its black pupils. As a result, I also present quasi-first stage estimates of the effect of Rosenwald schools on both literacy and educational attainment for my sample. These results complement Aaronson and Mazumder (2011), who show that Rosenwald schools increased school enrollment of affected cohorts and improved educational attainment of WWII enlistees.

My equation follows the format of equation (1) above. I estimate:

$$education_{ict} = \alpha_c + \gamma_t + \beta_1 black_i + \beta_2 exposure_{ict} + \beta_3 black_i * exposure_{ict} + \epsilon_{ict}$$

where β_3 is the effect of Rosenwald schools on black literacy versus white literacy. *Education* is measured either as literacy or as years of education in 1940. For 1940, I translate years of education into literacy based on the assumption that individuals with three or more years of schooling qualify as literate.[26]

I use the first stage estimate to calculate a Wald estimate of the effect of literacy on incarceration. That is, what is the predicted difference in incarceration rates between someone

[25] Note that household characteristics are constant from the perspective of the individual so if parental education is correlated with opening of the schools, then there could still be a problem. I address this by (a) controlling for parental education and other characteristics in some regressions; (b) arguing in Appendix B that education of blacks does not seem to predict when a Rosenwald school opens; (c) constructing within household estimates of the return to Rosenwald schools.

[26] Collins and Margo (2006) argue that literacy is the equivalent of 1-3 years of school. Results are not sensitive to the choice of two or three years as the cutoff.

who is literate and someone who is not? This is calculated by dividing the reduced form estimate by the first stage estimate. In order for this to be interpreted as an IV estimate, I must be willing to assume that Rosenwald schools only affected incarceration through literacy or learning. In fact, access to schooling may have reduced criminality in other ways, principally by keeping children occupied during the day.

C. Identification

One main concern about identification may be that counties who received schools are fundamentally different from those which did not receive schools. I address this concern by using county fixed effects so that something which would cause my estimates to be biased must be changing over time within a county and be correlated with the introduction of schools *and* with crime or incarceration. In addition, these changes would have to affect exposed and non-exposed cohorts differentially. For example, we might think that some counties have 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.

Following Aaronson and Mazumder (2011), I test whether a variety of county-level variables predict school locations. I find that counties with higher literacy rates of blacks and whites or with a higher percentage of black farmers tend to have schools and to receive them earlier than other counties. There is some evidence of a positive correlation between the percentage foreign born and receiving a school. See Appendix B for details.

We might also be worried that similar concerns plague the timing of school building within counties. This problem would not be eliminated with county fixed effects. Particularly in North Carolina, almost all counties ended up with at least one school by the end of the program, so it might not be across-county differences that are a problem, but that certain counties decided to build schools earlier or later. In Appendix B, I show that that black literacy, increases in the share foreign born, and increases in population density predict the length of time from the beginning of the program in 1913 to the date a county first built a school. I control for census year county characteristics in some regressions and results are not affected.

A final identification issue is that it is possible that families migrated towards counties with Rosenwald schools, either because they anticipated good schools for their children or as a response after the schools were built. I try to address this by assigning exposure based on the earliest census in which I can find the child. If the family migrated later, then the individual still gets assigned exposure from the previous county. Further, I regress literacy of men ages 30-50 on Rosenwald exposure in the past ten years; the idea is that if families migrated towards Rosenwald schools, then average literacy of men who are likely to be parents of exposed children should increase as Rosenwald exposure increases. I find no evidence that this was the case; I also find no evidence that average incomes of the same ages increased.

V. Results

A. Reduced form results

My empirical analysis begins in Table 2 by presenting estimates from equation (2) for the full sample of southern counties. In columns (1) and (2), I define my first measure of exposure as "Likely Seats", referring to the number of years a school was in a county, weighted by the probability of having a seat. In columns (3) and (4), exposure is "School in County" which is measured as the number of years a student had a school anywhere in his county between the ages of 7 and 13.

Using the first measure of exposure, one year of exposure to a Rosenwald school reduces the probability of being a prisoner by 0.096 percentage points, or just under 20 percent of the average propensity of a black individual to be imprisoned. This number falls to 0.05 percentage points when using the second measure of exposure. This pattern 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). Columns (2) and (4) restrict the sample of prisoners to those living in enumeration districts that are likely to contain state and federal prisons, namely, those enumeration districts where there are 10 or more prisoners in the census.[27] Estimates decrease slightly, suggesting that part of the effect might be coming from inmates of local jails.

Table 3 restricts the analysis to the North Carolina subsample. As noted above, North Carolina is poorer than most southern states at this time and had higher exposure to Rosenwald schools. Results are similar with both measures of county-level variation and with local-level variation. Recall that the local variation measures a child as exposed if they lived within a five mile radius of a Rosenwald school. One year of exposure to a school reduces the probability of being a criminal by between 0.04 and 0.05 percentage points. This is equivalent to reducing the probability of being incarcerated by about 10% on a base of 0.55 percent for blacks. In all regressions, exposure of whites to Rosenwald schools is small and insignificant, indicating that Rosenwald schools did not affect white crime.

Finally, in table 4, I turn to within-household estimates. I show that, comparing brothers from the same household who only were exposed to different numbers of years of a Rosenwald

[27] The name of the prison or institution is not available in my FamilySearch.org data. I have manually looked up the institution for all individuals in the North Carolina sub-sample but not in the full sample. Therefore, I assume that enumeration districts with 10 or more prisoners are more likely to be state and federal jails than enumeration districts with less than 10 prisoners.

school do to being born earlier or later, estimates are similar. If anything, there is a slightly larger effect of Rosenwald schools on incarceration when comparing within households. The effect of having a school in the county is 0.36 percentage points while the effect of having a school *and* having a seat for an extra year is about 0.1 percentage points.

B. "First stage" estimates and Wald Estimates

The above results suggest that Rosenwald schools reduced the criminality of black students later in life. The most likely mechanism for this decline in crime is that educational attainment raises the opportunity cost of engaging in criminal activity by increasing literacy rates and therefore wages. Other mechanisms include the effect of education on migration, if youth move to locations with lower crime rates, or the "incapacitation" effect, whereby staying in school keeps children occupied, preventing them from entering a life of crime. I look at these explanations below, but in this section I estimate the effect of Rosenwald schools on literacy. I run an identical regression to above but the dependent variable is whether the individual is literate; results are reported in Table 5. In the full southern sample, exposure to one more year of a Rosenwald school raises the probability of being literate by between 3.1 and 3.8 percentage points. These numbers are larger than the number from Aaronson and Mazumder who find that the probability of being literate increased by 1.8 percentage points for one year of exposure.[28] Literacy is not yet available for the North Carolina subsample.[29]

[28] This is most likely because Aaronson and Mazumder also use urban students as a comparison group. They find effects on these urban students so adding the two coefficients (black*exposure*rural and black*exposure) would give a similar number. My estimates also could be larger because they assume that the county in which the student is living is where they were educated. For older individuals, this could be inaccurate and lead to measurement error in the exposure variable.

[29] I am in the process of collecting literacy of all prisoners in North Carolina.

I calculate Wald estimates of the social return to literacy in Table 6. The Wald estimate is the reduced form coefficient divided by the first stage.[30] By this estimate, teaching a black man to read would reduce his likelihood of later-in-life incarceration by between 1.2 and 3.8 percentage points. In comparison, Lochner and Moretti find that an additional year of schooling reduces the likelihood of imprisonment by only 0.4 percentage points. I cannot calculate the effect of the Rosenwald program on education levels because education is only available for 1940. If, however, I use the estimate of 0.2 years of education for one year of exposure to a school from Aaronson and Mazumder, I can calculate the effect of another year of education on crime. I find that an additional year of schooling reduces imprisonment by 0.25 to 0.4 percentage points,[31] a value that is much closer to Lochner and Moretti.

These estimates would be valid two-sample instrumental variables estimates if the only mechanism through which Rosenwald schools affected crime was through literacy (Angrist and Krueger 1992; Solon and Inoue 2010). However, it is likely that the Rosenwald program affected criminality through multiple channels; work is ongoing to disentangle these mechanisms. Wald estimates are presented here simply to give an idea of the magnitude of the coefficients that are estimated in the reduced form analysis.[32]

C. North Carolinian additional data sources

The benefit of the Census data used thus far is that it provides a picture of all prisoners in the full South. A drawback is that it does not contain information about type of crime or sentence

[30] I calculate standard errors using the delta method. Standard errors presented assume that the coefficients are uncorrelated but the standard errors are similar and the coefficients are still significant letting the correlation range between -1 and 1.

[31] These Wald estimates comes from dividing the first stage coefficient (0.04-0.1) by the 0.2 years of schooling first stage coefficient.

[32] I also plan to add results considering heterogeneous effects of exposure, as well as estimates which compare within households to account for any omitted household background characteristics. Data collection for both of these is currently ongoing.

length, two characteristics that provide indirect information on whether and to what extent the racial gap in incarceration is simply due to the discriminatory Jim Crow legal system, rather than to “true” differences in criminal activity. One may be concerned that exposure to Rosenwald schools reduced discrimination, which then led to less imprisonment in the local area. Another potential issue is that census data measures a stock of crime in a census year, not the flow of prisoners into prisons; if whites are given shorter sentences than blacks, then we might overstate black crime. The first alternative hypothesis is unlikely because prisoners are rarely incarcerated in their county of birth; about 20 percent of prisoners are outside of their state of birth with another 15 percent also within the same state but outside of the county of birth.

In this section, I present estimates from different data sources that address concerns about discrimination in the southern legal system or measurement problems in census data. First, the state prison reports provide a flow measure of incarceration in each year. Another benefit of this data is that state prisoners are more likely to have committed “real crimes,” rather than, for example, being imprisoned due to enforcement of vagrancy laws, which was more likely to happen in local jails. Second, I examine county-level counts of court cases; this is a measure of the number of individuals *accused* of a crime, rather than only those convicted and sentenced for a crime. If the main effects derive from discrimination in conviction rates, due to partial juries, or sentencing lengths, due to biased judges, I will not find an effect of Rosenwald schools on the case load. Following this, I look at juvenile court case data; because juveniles are rarely incarcerated but instead are put on probation, Census data does not allow me to consider the contemporaneous effect of Rosenwald schools on youth activity. Finally, I incorporate micro-data from the state prison registers about the type of crime committed to determine whether education is more likely to prevent violent or property-based crime.

i. State Prison Reports and Attorney General Reports

Results from the State Prison Reports are shown in Table 7. Average exposure and sample means are given at the bottom of the table. I create covariates based on the population census data in the decennial years and linearly extrapolate between years. Controls include the total black and white male populations, the total county population, the share of the population that is black, and the share of the population that lives in urban areas. I use the total number of prison intakes in a county and the log of intakes of the specific race as the outcome variables.[33] Exposure is a weighted average based on the age distribution of prisoners and the timing of school openings in counties.[34] I find that exposure to Rosenwald schools for one year would reduce the black-white gap in incarceration at the county level by 5 prisoners relative to a mean of 73 prisoners in column (1) or by 19 log points percent for a mean gap of 54 log points in column (2).

An alternative measure of crime is the number of individuals accused of a crime in a given county in each year. I digitize county level court cases from the Attorney General reports from 1913-1935 (the data is not available in later years) and find that court cases against blacks also decrease by a similar magnitude with average exposure in a county. Results are shown in Table 8.

The results from the State Prison reports suggest that what I find in census data is not due to discrimination in sentence lengths or due to petty crimes. In fact, exposure to Rosenwald schools decreases the probability of being incarcerated in state prison. The fact that court cases

[33] County of birth is not available in this data so I use county of arrest. To the extent that there is migration from rural counties to urban counties, I will understate exposure since urban areas had fewer Rosenwald schools.
[34] Because the mean of the dependent variable increases by a factor of ten between 1933 and 1935, I restrict to post-1935 in my analysis.

go down as well suggests that arrests decrease with Rosenwald exposure. While I cannot rule out that court cases are related to discrimination at the arrest stage, this provides indirect evidence that my results are not due to discrimination at the sentencing stage.

ii. Juvenile Court Data

I consider contemporaneous effects of Rosenwald schools on the criminal activity of youth in Table 9. The observed decline in adult crime might arise because additional years of schooling increase the opportunity cost of crime. Alternatively, the main effect could be due to persistence of criminality: offenders who commit crimes at young ages often commit crimes later in life as well. If Rosenwald schools reduce crime among juveniles, these individuals might also be less likely to commit crimes later. I regress the number of juvenile court cases for blacks or whites in a given county within a five year period on exposure to Rosenwald schools and the same set of county controls as above. Exposure is measured as the number of years over the previous five year period the county had a school. Exposure to Rosenwald schools has a negative and significant effect in all specifications. The effect of Rosenwald schools on white outcomes is insignificant and almost always small as we would hope. The effect of exposure on black juveniles is a decrease of 9.4 court cases per five years from the average of about 50 court cases.

The magnitude of the effect on juvenile crime is very similar to that on adult crime in the census. If anything, it is larger. About 36 percent of juvenile offenders commit a crime at some point later in life (Illinois 2007). If we assume that these will happen in the next ten years and will require on average five years of incarceration (the average sentence length in the North Carolina state prison), then census data should capture 18 percent of them; incarceration in census data therefore should decrease by about 2 percent of the mean (=0.18*0.1). That means that the decrease in incarceration in census data could be partly due to the fact that kids have been kept out of a life of crime early on. However, 80% of the 10 percent decrease in crime is likely due to less non-juvenile offenders.

iii. State Prison Registers

In Table 10, I turn to individual-level prison register data. It is hard to assess the social return to the decline in crime documented in the main results without knowing the distribution of crimes affected. If educational attainment only reduces rates of petty theft, the monetary return to the Rosenwald Program would be lower than if schooling reduced violent or other severe crimes. Furthermore, any decreases in crime could have come from reductions in the number of black individuals picked up for vagrancy (Naidu 2010).

I estimate a multinomial logit by pooling my state prison data with the comparison samples of non-prisoners. I categorize the types of crime into four groups: property crimes, violent non-fatal crimes, violent fatal crimes, and other crimes. Property crimes include robbery, burglary, and housebreaking. Violent non-fatal crimes are mainly rape and assault. Fatal crimes are manslaughter and murder. Within the "other" category, there are many white-collar crimes such as forgery and embezzlement.

I find that exposure to a Rosenwald school decreases the likelihood that black men commit any of these crimes to a roughly equal degree. The relative risk ratios are all less than 1, indicating that a year of exposure to Rosenwald schools reduces that category of crime relative to the category of not being in prison.[35] These results suggest that education reduces crime of all types.

[35] I have also carried out similar analysis using only the prison population. It seems like the crimes that are reduced the most are property crimes and non-fatal violent crimes. Breaking into more categories makes the estimates less

VI. Robustness and Alternative Specifications

In this section I turn to robustness measures and alternative specifications. I first discuss potential concerns about the sample and sources of variation that are driving my results. Then I investigate non-linear effects of exposure and the hypothesis that there is a critical "age window" in which exposure to a Rosenwald school has the strongest effect on later-life outcomes. All regressions use the "likely seats" measure of exposure which is the number of years a child had a school in his county, weighted by the probability of having a seat available.

Table 11 contains results for five robustness samples. One potential worry is that there are county time trends that might affect schooling and lower crime later in life. For example, if parents are becoming more literate over time in Rosenwald counties, then this could explain part of the effect. In columns (2) and (3) I address this issue. In column (2), I restrict to ages less than 30 since 30 to 35 year olds might not be a good comparison group for an 18 year old. In column (3), I only keep a sample of exposed individuals and those who "just missed" exposure in the sense that they were too old but there was a school built between the ages of 14 and 17. The effect of exposure stays the same in these two samples so it does not appear that the results are sensitive to the ages of the comparison sample.

Another issue could be that if there are peer effects which *do* affect the "just missed" group through their slightly younger peers being more educated, then the estimate of the effect of exposure might be too small. This could work through either direct peer effects as children or higher wages in Rosenwald counties for everyone as a function of the average increase in

precise but there is some weak evidence that embezzlement and forgery increase with exposure, consistent with the idea that schooling increases skills that are useful for certain types of crimes.

literacy due to Rosenwald schools.[36] I therefore restrict to those who were exposed and those who did not just miss. That is, either individuals were exposed for a certain number of years between ages 7 and 13 or they did not have a school in their county until after age 18. The coefficient remains the same, indicating that the effect does not come only through peer effects or effects on county-level wages.

Another question is whether whites are a good comparison group for blacks. If there were things affecting the county judicial system which affected whites and blacks differently, then maybe whites are not a good comparison. I argue that, first, the coefficient on exposure for whites is never statistically or economically significant. Second, in column (5), I restrict to blacks only and regress the probability of being a prisoner on exposure alone. Note that in this case, I cannot include county-level fixed effects because much of my variation is coming off of cross-race variation. With state-level fixed effects, the coefficient is much the same as before albeit insignificant.

Finally, one might wonder whether the variation that is driving the results is from counties with Rosenwald schools compared to counties without schools. In column (6), I show that this is not the case; if anything, the coefficient is larger when restricting to counties which built a school at some point.

The results thus far have relied on the assumption that the effect of exposure is linear. In Table 12, I consider alternative specifications. First, I look at whether the result is robust to simply comparing those who are exposed by any amount to those that are not. The average effect of any exposure is a 0.22 percentage point decrease in the likelihood of being a prisoner for

[36] I plan to examine the county-level effects of Rosenwald schools in more detail in another paper. Work to examine this issue for this paper is ongoing.

blacks. This number is larger than the previous 0.09 percentage points per year of exposure, suggesting that the effect is not coming all from exposed versus unexposed groups.

Second, I allow the effect of one year of school to vary based on the age of the individual. It might be that older children have "missed the boat" if a school is built in their county at age 10 or 11. I do find some evidence that this is the case in column (2) where I include the exposure between age 7 and 9 and between ages 10 and 13 separately. The coefficient on the first time period is negative and significant but the coefficient on ages 10 to 13 is small and marginally significant. This suggests that new schools had a larger affect at younger ages. I also allow the effect of a year of school, at any age, to be non-linear. In column (3) we see that there do not appear to be diminishing effects.[37]

VII. Conclusion

This paper considers the social returns of a program which increased the schooling of black children between 1920 and 1940. Rosenwald schools increased schooling and literacy, which resulted in lower crime rates. I find a social return to school of about a 0.4 percentage point decrease in crime, which is similar to the literature.

My results are relevant to a developing economy today, such as South Africa, where inequality between blacks and whites is similar with respect to incarceration rates, education, and wages. This is the first paper to consider social returns to education in a historical, developing economy environment; previous literature has considered social returns to school primarily in the United States and Europe where inequality is lower, institutions are stronger, and incomes and education levels are higher.

[37] I have also tried regressions with dummy variables for exposure at each age and dummy variables for one through seven years of exposure. Both regressions are imprecisely estimated but there is a similar pattern of higher values at younger ages and diminishing returns to exposure after 5 years.

Further work will examine migration responses to Rosenwald schools and the relationship between migration and black-white crime gaps. The historical gap between blacks and whites in incarceration is not well explained and is hardly documented in current literature. Education is one factor that helps explain this gap, but the evolution of the gap over the 20th century is not well understood and is a subject for future research.

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Appendix A: Matching

The matching procedure starts with the base year of 1920, 1930, or 1940 and matches backwards to either 10 or 20 years prior. The procedure is as follows:

(1) I begin by standardizing the first and last names of men in the later year 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 1900 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 same birth year, the observation is thrown out;

(c) if I do not find a match at this first step, I try matching within a one-year band (older and younger) and then with a two-year band around the reported birth year; we 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 28,455 prisoners and 36,972 non-prisoners. I can successfully match 20.3 percent of all prisoners and 21.5 of non-prisoners. The difference between these match rates is most likely attributable to lower education levels among prisoners (e.g. they are less likely to report the correct age or to spell their names consistently enough to match). I have lower match rates for blacks than for whites, which is also consistent with lower education levels.

Individuals can fail to match due to (i) non-unique name-birthplace-race combinations; (ii) multiple matches in the earlier year; (iii) mis-reporting of age; and (iv) 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 in 1940 account for 40% of match failures. Multiple matches account for 12%. Allowing individuals to match within a 10 year age range gains an additional 10% so these are likely misreported ages. Finally, individuals who cannot be found because of differences in name spellings account for the remaining 38%. Note that this could be because the individual misspelled their name (likely correlated with education, etc) or because the enumerator or the modern transcriber misspelled it (random). In fact, the correlation between the census indexes of Ancestry.com and FamilySearch.org is quite low, suggesting that a lot of the differences in spelling are due to transcription differences in the index.

Appendix B: Locations of Rosenwald Schools

B. Relationship between county characteristics and Rosenwald schools

I consider the relationship between pre-Rosenwald county characteristics and the existence of a school. In Table B.1, I regress four outcomes on a set of race-specific socioeconomic variables and other demographic county variables. The outcomes are (a) whether the county ever had a Rosenwald school; (b) The total number of schools built in the county by the end of the program in 1931; (c) whether the county had a school before 1919; and (d) the number of years until the first school was built for counties which did get a school. I use literacy, school enrollment, and average occupational score as my socioeconomic variables. I also consider the effect of population density, the share of the population living in urban areas, the county's share of black and foreign born men, and the percentage of households that own a home. To account for the idea that distance from the Tuskegee Institute or other important historical black institutions might matter for the outcome variables, I include the distance from the center of the county to the Tuskegee Institute, Montgomery, AL, Nashville, TN, and Hampton, VA. Regressions also include state fixed effects and industry share controls by race. Finally, I consider the level of all of these variables in 1900 and 1910 and also the change between 1900 and 1910; that is, the share of the population that is urban might matter, but urbanizing counties might also be more or less likely to build a school.

In Panel A, I find a positive relationship between black literacy and ever having a Rosenwald school. A ten percentage point increase in black literacy in 1910 would increase the probability of having a school by 2.2 percentage points. The coefficients on white literacy are almost twice as large but insignificant. There is also some evidence that counties with higher population density and urbanizing counties with increasing urban share between 1900 and 1910 were more likely to get a school. In Panel B, the number of schools built by 1931 is positively correlated with population density, black literacy, the percent of farmers who are black, and the total number of farms in the county. Counties which built a school before 1919, in Panel C, tended to have a lower percentage of black farms, higher *white* literacy, a higher percentage of black men, and lower percentage of black owner-occupier farmers. Counties where home ownership or the percentage of black farmers were growing were less likely to get a school before 1919. Finally, in Panel D, we see that counties with increasing population density or increases in the share foreign born waited fewer years before a school was built. Those with higher black literacy also waited fewer years. The percentage of black farmers is positively correlated with the number of years until the first school was built.

C. Family migration decisions and Rosenwald schools

Another concern is that families who were more literate or of higher socio-economic status may have migrated towards counties with Rosenwald schools. In that case, we would expect average income and literacy of individuals not directly affected by the Rosenwald schools to increase in a county *after* the introduction of the schools. That is, average literacy in a county with high exposure to the schools should increase more than that of counties with low exposure to the school if literate parents migrate towards schools for their children. If literate parents have children who are unlikely to be criminals, this might bias my results.

To examine this, I regress literacy of men ages 30 to 50 on indicators for county Rosenwald exposure in various years of the previous decade. Because the data is noisy, I group it into three categories: exposure 1-3 years earlier (*rose1)*, exposure 4-6 years earlier (*rose2*), and exposure 7-9 years earlier (*rose3*). I interact each exposure measure with a dummy variable for being black. I would worry about selective migration if these interaction terms are significant. The results are shown in Table B.2. I find no significant evidence that there was selective in-migration to Rosenwald counties of more literate families between census years. There is some weak evidence (p=0.09) that Rosenwald exposure in the previous three years is associated with higher occupational scores of men in this age range, but this test is less clean; it is reasonable that Rosenwald schools could have increased wages for men so this is not direct evidence of selective migration

Table B.1: Correlation between 1900 and 1910 county characteristics and Rosenwald schools

	(a) Ever had a Rosenwald school			(b) Number of schools, 1931		
	1900	1910	Change	1900	1910	Change
Pop density	0.075	0.329**	0.085	3.038***	5.273**	3.071*
	(0.137)	(0.115)	(0.076)	(0.980)	(1.981)	(1.581)
% Black	1.305	1.144**	0.371	3.753	-0.088	13.373**
	(0.363)	(0.424)	(0.439)	(2.971)	(3.195)	(5.057)
% Urban	-0.0005	0.116	0.526***	1.111	1.282	7.438**
	(0.109)	(0.087)	(0.161)	(2.105)	(1.708)	(2.831)
% Foreign-born	-0.245	0.572	0.843	-4.473	-2.940	9.643
	(0.669)	(0.662)	(0.846)	(4.871)	(5.905)	(16.76)
% Own home	0.564	0.715***	-0.128	-3.556	2.404	4.180
	(0.103)	(0.154)	(0.259)	(3.195)	(3.421)	(3.134)
% Lit White	0.714	0.745	-0.869	1.427	1.051	-6.861*
	(0.369)	(0.455)	(0.623)	(5.291)	(7.369)	(3.668)
%Lit Black	0.462	0.224**	-0.138	3.056*	3.507*	2.449
	(0.114)	(0.095)	(0.101)	(1.551)	(1.881)	(1.743)
# farms (1000s)	0.0053	0.004**	0.005	0.124	0.124***	0.264***
	(0.0021)	(0.001)	(0.003)	(0.034)	(0.020)	(0.056)
% farms black	-0.344	-0.436	-0.497***	3.396***	10.712	-10.949***
	(0.151)	(0.319)	(0.116)	(2.054)	(3.971)	(2.116)
% black farmers	-0.083	-0.062	-0.046	-1.425	-0.754	0.365
who own	(0.087)	(0.046)	(0.084)	(0.852)	(0.683)	(0.724)
% white farmers	-0.228	-0.513*	-0.061	7.556**	-1.375	-7.571
who own	(0.221)	(0.235)	(0.262)	(2.645)	(3.291)	(4.419)
Sample Mean	0.65	0.65	0.65	3.495	3.495	3.495
N	1,088	1,154	1,067	1,088	1,154	1,067
R-squared	0.3998	0.3929	0.3593	0.4214	0.4548	0.4226

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

Table B.1: Correlation between 1900 and 1910 county characteristics and Rosenwald schools

	(c) Has a school by 1919			(d) Years until first school		
	1900	1910	Change	1900	1910	Change
Pop density	0.162	0.277	0.125	0.261	2.460	-5.988**
	(0.110)	(0.167)	(0.083)	(1.809)	(3.648)	(2.298)
% Black	0.781***	0.340	0.205	-5.061	-2.664	-1.297
	(0.259)	(0.377)	(0.402)	(4.319	(2.905)	(1.946)
% urban	0.166	0.217**	0.278	-0.292	-0.429	-1.329
	(0.122)	(0.097)	(0.192)	(0.931)	(0.764)	(2.231)
% Foreign-born	-0.487	-0.298	0.448	5.780	1.365	-13.026*
	(0.301)	(0.355)	(1.235)	(4.107)	(6.544)	(7.144)
% Own home	0.151	-0.011	-0.482**	-2.026	-2.034	2.500
	(0.112)	(0.289)	(0.222)	(1.845)	(1.683)	(1.891)
% Lit White	0.225	0.534***	-0.504	1.211	2.851	4.051
	(0.231)	(0.170)	(0.391)	(3.207)	(2.441)	(5.819)
%Lit Black	0.154	0.093	0.196	-2.179	-3.932*	-3.458*
	(0.112)	(0.158)	(0.195)	(2.541)	(1.866)	(1.930)
# farms (thousands)	0.005**	0.004**	0.006**	-0.052***	-0.042***	-0.058*
	(0.003)	(0.002)	(0.003)	(0.014)	(0.013)	(0.029)
Proportion farms black	-0.166*	0.033	-0.459***	0.157	-1.842	3.031***
	(0.085)	(0.265)	(0.129)	(1.475)	(2.414)	(0.598)
% black farmers who own	-0.141**	-0.016	-0.012	-0.670	-0.573	-0.084
	(0.059)	(0.044)	(0.039)	(0.808)	(0.541)	(1.639)
% white farmers who own	0.232	-0.110	-0.337	-2.091	-0.202	2.516
	(0.188)	(0.213)	(0.188)	(1.643)	(1.845)	(2.148)
Sample Mean	0.225	0.225	0.225	3.153	3.153	3.153
N	1,088	1,154	1,067	782	767	781
R-squared	0.2963	0.3003	0.2789	0.2682	0.2685	0.2553

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

Table B.2: Selective migration of literate and higher occupational status families to Rosenwald Counties

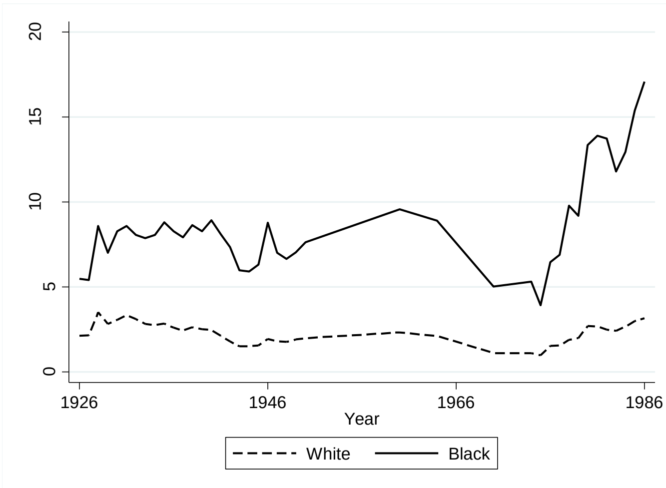
Outcome:	Literacy	Literacy	Occupational Score	Occupational Score
Black*Rose1	0.011	-0.017	0.927	2.097*
	(0.018)	(0.018)	(0.769)	(1.221)
Black*Rose2	-0.005	0.031	-0.145	-0.900
	(0.029)	(0.032)	(0.855)	(1.245)
Black*Rose3	0.004	-0.037	-0.632	-0.619
	(0.025)	(0.036)	(0.502)	(0.686)
Rose1	-0.002	0.006	0.042	-0.746
	(0.007)	(0.006)	(0.257)	(0.490)
Rose2	0.013	0.004	-0.249	0.846
	(0.010)	(0.010)	(0.405)	(0.505)
Rose3	-0.019**	-0.011	0.169	-0.302
	(0.008)	(0.010)	(0.358)	(0.302)
Ages	30-50	30-40	30-50	30-40
R-squared	0.1473	0.1443	0.1183	0.1001
N	51,850	29,807	51,850	29,807

Notes: regressions include fixed effects for age, black*age, year, state*year, and county. I restrict to men living in rural areas in the census year since schools were targeted to rural areas.

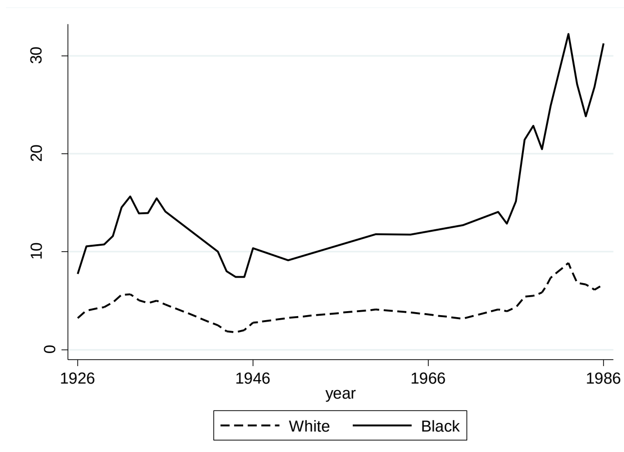
Figures

Figure 1: Incarceration Rates by Race and Region, 1926-1986

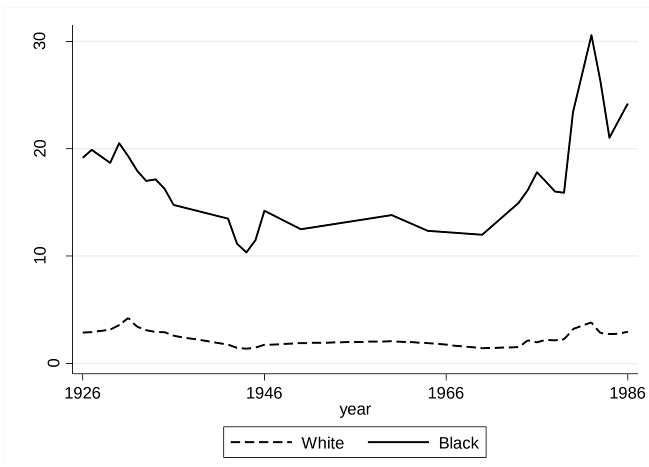
A: Whole US



B: Southern States



C. Northern States



Tables

Table 1: Summary Statistics

	Prisoner	Non-Prisoner
Full South		
Percentage (Black subsample)	0.688	99.31
Percentage (White subsample)	0.186	99.81
Age	25.18	25.09
	(4.651)	(4.949)
Average Exposure (coverage)	0.270	0.410
	(0.800)	(1.029)
Average Exposure	1.318	1.894
	(2.833)	(2.815)
Percentage living outside state of birth	21.19	14.34
North Carolina		
Literate	0.822	0.921
	(0.382)	(0.270)
Both Parents Literate	0.688	0.778
	(0.463)	(0.415)
Father = Owner-Occupier Farmer	0.259	0.340
	(0.439)	(0.474)
Father = Tenant Farmer	0.283	0.275
	(0.451)	(0.447)

Notes: N=76,639. North Carolina statistics are calculated from available (incomplete) data. North Carolina subsample restricts to those living in North Carolina in the childhood census.

Table 2: Reduced Form Results: Whole South

Exposure:	Likely Seats		Presence of School	
	Prisoner	Prisoner, likely state & federal	Prisoner	Prisoner, likely state & federal
Black*Exposure	-0.0939***	-0.0760***	-0.0427***	-0.0327***
	(0.0122)	(0.0120)	(0.0050)	(0.0049)
Exposure	0.0031	-0.0009	0.0020	-0.0027
	(0.00003)	(0.0041)	(0.0022)	(0.0022)
Black	0.5139***	0.4339***	0.5967***	0.4946
	(0.0003)	(0.0352)	(0.0039)	(0.0371)
County FE	Y	Y	Y	Y
Age, Age*Black FE	Y	Y	Y	Y
Mean Exposure	0.410	0.410	1.717	1.722
Sample Mean	0.550	0.553	0.553	0.556
Number of counties	1,321	1,112	1,321	1,112
N	65,885	62,351	65.885	62,351

Notes: Coefficients are multiplied by 100 to be expressed as percentages instead of proportions. Regressions include year, and state*year fixed effects. In columns (2) and (4), I restrict to enumeration districts in which there were at least 10 prisoners in the census of that year; these prisons were more likely to be state and federal prisons than county jails. I restrict to ages 18-35.

Table 3: Reduced Form Results, North Carolina

	(1)	(2)	(3)
Black*Exposure	-0.0495***	-0.0462***	-0.0469*
	(0.0112)	(0.0111)	(0.0464)
Exposure	-0.0002	-0.0076	0.0011
	(0.0631)	(0.0500)	(0.0566)
Black	0.2611***	0.2581***	0.0492***
	(0.0612)	(0.0613)	(0.0045)
Mean Exposure	3.469	1.001	0.6827
Sample Mean	0.0032	0.0032	0.0032
Exposure	Likely Seats	School in County	Local Radius
Black*age and age FE	Y	Y	Y
County FE	Y	Y	Y
Number of counties	100	100	100
N	9,008	8,363	8,145

Notes: Coefficients are multiplied by 100 to represent percentages instead of proportions. Regressions include age, black*age, county, year, and state*year fixed effects. I restrict to ages 18-35 and to those living in North Carolina in the childhood census.

Table 4: Reduced Form Results, Within Household Estimates

	(1)	(2)
Black*Exposure	-0.105***	-0.036***
	(0.003)	(0.003)
Exposure	-0.015**	-0.008**
	(0.007)	(0.004)
Exposure	Likely Seats	School in County
Black*age and age FE	Y	Y
County FE	Y	Y
N	7,994,918	7,994,918

Notes: Coefficients are multiplied by 100 to represent percentages instead of proportions. Regressions include age, black*age, county, year, and state*year fixed effects. I restrict to ages 18-35. These regressions control for household fixed effects so that the coefficients compare outcomes brothers who grew up in the same household. Standard errors in parentheses are clustered by birth county.

Table 5: Effect of Rosenwald Exposure on Literacy

Sample:	(1) Full Ipums	(2) North Carolina Ipums	(3) North Carolina Ipums
Black*Exposure	0.0368**	0.0390**	0.0218*
	(0.0145)	(0.0155)	(0.0132)
Exposure	-0.0006	0.0020	0.0010
	(0.0010)	(0.0031)	(0.0043)
Black	-0.1245***	-0.1323***	-0.1295***
	(0.0341)	(0.0568)	(0.0556)
Mean Exposure	1.023	1.234	0.655
Sample Mean, black	0.842	0.816	0.818
Exposure	Likely Seats	Likely Seats	Local Radius
R-Squared	0.1044	0.0955	0.0801
N	31,456	4,563	4,345

Notes: Coefficients are multiplied by 100 to be expressed as percentages instead of proportions. Regressions include year and state*year fixed effects. I restrict to ages 18-35.

Table 6: Wald Estimates of the Effect of Literacy on Incarceration

	(1) Full South	(2) North Carolina	(3) North Carolina
Literate	-2.471***	-1.210**	-1.512*
	(1.104)	(0.0667)	(0.887)
Mean Literacy, black	0.842	0.816	0.816
Exposure Measure	Likely Seats	Likely Seats	Local Radius
Black*age and age FE	Y	Y	Y
County FE	Y	Y	Y
N	31,466	4,563	4,355

Notes: Coefficients are multiplied by 100 to be expressed as percentages instead of proportions. Regressions include year and state*year fixed effects.. I restrict to ages 18-35. Wald Estimates are calculated as the reduced form coefficient on black*exposure divided by the first stage coefficient on the same variable. Standard errors are calculated using the delta method. Sample sizes are presented for the second stage sample.

Table 7: Effect of Rosenwald Schools on County/Race Admissions to State Prison

Dependent variable= Number of prisoners from county in each year

Outcome:	(1) Level	(2) Log
Black*exposure	-5.305***	-0.193***
	(1.094)	(0.054)
Exposure	2.349	0.052
	(1.937)	(0.041)
Black	40.71***	0.543***
	(8.711)	(0.156)
Exposure	Likely Seats	Likely Seats
Years	1935-50	1935-50
Sample Mean	73.26	3.420
Average exp.	2.335	2.310
R-squared	0.7350	0.7876
N	3,200	3,064

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

Table 8: Effect of Rosenwald Exposure on Attorney General Reports of County Court Cases

Dependent variable= Number of court cases in county by year

	(1) Level	(2) Log
Black*exposure	-17.60***	-0.297**
	(6.791)	(0.135)
Exposure	7.803	0.098*
	(4.977)	(0.053)
Black	-23.43***	-0.345***
	(6.429)	(0.135)
Exposure	Likely Seats	Likely Seats
Sample Mean	69.68	3.644
Average exp.	0.496	0.495
R-squared	0.6986	0.5987
N	3,600	3,534

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

Table 9: Results from North Carolina Juvenile Crime Data

	(1) Level	(2) Log
Black*Exposure	-9.484***	-0.344***
	(2.564)	(0.046)
Exposure	1.802	-0.026
	(5.62)	(0.101)
Black	17.21*	0.465***
	(9.272)	(0.166)
Exposure	Likely Seats	Likely Seats
Sample Mean	50.54	2.833
Avg. Exposure	0.425	0.421
R-squared	0.8529	0.7349
N	800	733

Notes: All regressions include county and race-specific year fixed effects. Controls include total black male population, total white male population, total county population, county share urban, and county share black in the respective year.

Table 10: Effects on Type of Crime, North Carolina State Prison Registers

Outcome:	Not in prison	Property	Violent-fatal	Violent-nonfatal	Other
Black*Exposure	1	0.707***	0.808*	0.799*	0.754**
		(0.063)	(0.112)	(0.109)	(0.101)
Exposure	1	0.716**	0.743**	0.707***	0.672***
		(0.044)	(0.082)	(0.087)	(0.080)
Black	1	10.129***	271.64***	146.85***	0.355
		(6.329)	(327.76)	(172.35)	(0.459)
Sample proportion		0.667	0.103	0.112	0.118

Notes: N=7,074. Coefficients are Relative Risk Ratios from one multinomial regression. The proportion of crimes in each cell is given in the last line. Controls include age, black*age, and county and year fixed effects.

Table 11: Robustness, Full Sample

Restriction	Coefficient on Black*Exposure
1. None	-0.0939***
	(0.0122)
2. Ages 18-30	-0.0911***
	(0.0124)
3. Exposed and “Almost Exposed”	-0.0909***
	(0.0124)
4. Exposed and “Never Exposed”	-0.0951***
	(0.0156)
5. Black Only	-0.1357
	(0.1477)
6. Rosenwald Counties Only	-0.1077***
	(0.0146)

Notes: Regressions include age, black*age, state*year, year, and county fixed effects. Restrictions are explained in detail in the text.

Table 12: Alternative Specifications

	(1)	(2)	(3)
Black*Exposed	-0.2318***		
	(0.0308)		
Exposed	0.0309**		
	(0.0105)		
Black*EarlyYears		-0.2205***	-0.2208***
		(0.0306)	(0.0316)
Black*LaterYears		-0.0704	-0.3047***
		(0.0803)	(0.0582)
EarlyYears		0.0237	0.0324
		(0.0156)	(0.0166)
LaterYears		-0.0114	0.0011
		(0.0108)	(0.0201)
Early Years =		Ages 7-9	Exposed 1-3 years
Later Years =		Ages 10-13	Exposed 4-7 years

Notes: N=64,885. Fixed effects include county, age, age*black, state*year, and year. “Exposed” is defined as one if exposure is greater than zero.