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The Impact of Rosenwald Schools on Black Achievement

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Abstract

The Black-White gap in schooling among Southern-born men narrowed sharply between the World Wars. From 1914 to 1931, nearly 5,000 schools were constructed as part of the Rosenwald Rural Schools Initiative. Using Census data and World War II records, we find that the Rosenwald program accounts for a sizable portion of the educational gains of rural Southern Blacks. We find significant effects on school attendance, literacy, years of schooling, cognitive test scores, and Northern migration. The gains are highest in the most disadvantaged counties, suggesting that schooling treatments have the largest impact among those with limited access to education.

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I. Introduction

Since the path breaking work of Schultz (1961) and Becker (1964), it has been well-recognized that investment in human capital is a primary vehicle for economic development. Indeed, over the last 50 years, developing countries have made the improvement of basic education a principal goal in their effort to enhance living standards. Yet, despite substantial progress, lack of access to schools and poor school quality remains a persistent problem (UN Millennium Project 2005). For example, in many developing countries, schools often lack basic infrastructure like sanitation (UNICEF 2011) and educational equipment like textbooks, blackboards and desks (Glewwe and Kremer 2006) and rates of teacher absenteeism are often very high (Banerjee and Duflo, 2006). Some of these problems can be traced to ineffective or corrupt governments (Lewis and Pettersson 2009), exacerbated by a lack of political influence among the rural poor. There is also some uncertainty about whether providing more educational inputs improves learning and other long-term outcomes (Glewwe and Kremer 2006). In this paper, we present new evidence based on a large American schooling intervention from nearly a century ago that may shed light on current efforts to improve school access and school quality in developing countries today.

At the turn of the 20th century, the educational opportunities available to rural Blacks living in the segregated American South were quite similar to what is available to the rural poor in many countries today: inadequate school buildings, classrooms, and equipment. Moreover, White-run public institutions were not held accountable for these failures since Blacks lacked political representation. As a consequence, Blacks born in the South between 1880 and 1910 completed three fewer years of schooling than their White counterparts. While both groups made absolute gains, Blacks experienced no *relative* progress over this thirty year period.

However, for cohorts born during a relatively short period between the World Wars, the Southern racial education gap improved dramatically (see Figure 1).[1] Within a generation, the racial gap in the South declined to well under a year and was comparable in size to the racial gap in the North, which remained roughly unchanged for cohorts born between 1880 and 1940.

The Rosenwald Rural Schools Initiative was a program explicitly designed to narrow racial schooling gaps in the South during this period of Black relative progress.[2] The Initiative was the result of a collaboration between Booker T. Washington, the principal of the Tuskegee Institute in Alabama, and Chicago businessman and philanthropist Julius Rosenwald. The two men developed a matching grant program that, between 1913 and 1931, facilitated the construction of almost 5,000 schoolhouses for Southern rural Black children. By the time the program ended, we estimate that approximately 36 percent of Southern rural Blacks of school-age could have attended a Rosenwald school.

In addition to making schooling more accessible, the program represented a sea change in the quality of schools. The buildings were constructed based on modern designs that ensured adequate lighting, ventilation and sanitation. Classrooms were required to be fully equipped with books, chairs, desks, blackboards and other materials to ensure an adequate learning environment. A number of other initiatives -- including minimum teacher salaries, newly built teacher homes, and training programs often in concert with other philanthropic efforts like the Jeannes Fund[3] -- were introduced to recruit and prepare teachers. While historical data on these dimensions of school quality unfortunately do not exist,[4] we can examine the overall impact of these various programs on long-term measures of human capital.

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[1] For research on the Black-White schooling and income gap during the 20th century, see Smith (1984), Smith and Welch (1989), Margo (1990), Donohue and Heckman (1991), Collins and Margo (2006), and Neal (2006).

[2] Donohue, Heckman, and Todd (2002) discuss the role of Northern Philanthropists, and Rosenwald in particular, on Black school attendance in the early 20th century. Card and Krueger (1992) study the effects of improvements in school quality experienced by Blacks from 1915 to 1966, but do not explicitly look at the effects of the Rosenwald schools. For historical descriptions of the Rosenwald Rural Schools Initiative, see McCormick (1934), Embree (1936), Ascoli (2006), and Hoffschwelle (2006).

[3] The Jeannes Fund, established in 1907, primarily funded supervisors who helped train teachers in Black rural and urban Southern schools. Underscoring the close link to the Rosenwald School Initiative, Booker T. Washington served on the Jeannes Fund Board.

[4] As we discuss later, available measures that have been used in prior research to proxy for school quality (e.g. school expenditures, teacher counts) will generally not capture the school quality improvements of the Rosenwald schools and can lead to misleading inferences regarding the effects of the program.

We do this by linking newly uncovered records of the location and the timing of the Rosenwald schools to large samples drawn from the Decennial Censuses and World War II enlistment records. Our main finding is that rural Black students with access to Rosenwald schools completed over a full year more education than rural Black students with no access to Rosenwald schools, a magnitude that, in the aggregate, explains close to 40 percent of the observed Black-White convergence in educational attainment in the South for cohorts born between 1910 and 1925. We find similarly large effects on literacy rates and evidence that exposure to the schools increased rates of migration to the North among rural Blacks, potentially fueling further economic gains. Accounting for the full cost of building and maintaining the schools, we estimate that the additional human capital acquisition generated by the Rosenwald schools implies an internal real rate of return of about 7 to 9 percent.

Our estimates also provide insight into the relative importance of school quality versus school access in explaining the effects of the program. We estimate that the program increased the rate of school attendance by roughly 5 percentage points, well below the 36 percent of students who could have attended a Rosenwald school in 1932. Some simple calculations suggest that about one-third of the overall improvement in years of completed schooling is due to an increase in the fraction of students attending school. The remaining two-thirds is due to gains in schooling among students who would have attended school even in the absence of the Rosenwald program. This suggests that the bulk of the gain in human capital investment is likely due to improvements in school quality which would have led many students to stay in school longer than they would have in a pre-Rosenwald school.[5]

A unique aspect of our analysis is that we use a measure of cognitive ability, the Army General Classification Test (AGCT), a precursor to the modern Armed Forces Qualifying Test (AFQT). We find that childhood access to the schools raised the AGCT scores of rural Blacks by about 0.2 to 0.45 standard deviations. By comparison, the Black-White test score gap among World War II enlistees was about 1.1

[5] It is difficult to attribute exactly how much of the gain in completed schooling is due to school quality. Some of the gain in years of schooling among students who would have attended school in the absence of the Rosenwald program could have been due to lower costs of schooling rather than to improvements in school quality. On the other hand, improvements in school quality may have also encouraged some children who would not have otherwise attended school, to enroll in a Rosenwald school.

standard deviations. Further, we find that the test score effect is eliminated when we control for educational attainment, suggesting that the pathway for the improvement in cognitive skills is through schooling. This finding is consistent with other studies that show that racial gaps in test scores are not immutable and can be influenced by interventions (Neal and Johnson 1996; Hansen, Heckman, and Mullen 2004; Cascio and Lewis 2006; Chay, Guryan, and Mazumder 2009).

Across all outcomes, we find that the gains are largest in counties with large fractions of former slaves. This suggests that schooling interventions targeted at historically deprived communities may yield especially large returns, consistent with a view in the development literature that introducing schools in disadvantaged rural areas disproportionately benefits students facing the highest cost of attending school and where rudimentary school inputs are lacking (e.g. Duflo 2001; Glewwe and Kremer 2006).

Identifying causal effects of the Rosenwald Initiative presents an empirical challenge because school location decisions were not random. In order to receive matching grants, local citizens were required to donate substantial resources for school construction. Consequently, counties with greater demand for educational resources may have more likely received a grant. It is plausible that students in those high-demand counties may have experienced better outcomes even in the absence of the Rosenwald program. However, we find no empirical evidence to support this conjecture. We show that Black school attendance rates in 1910, and the change in those rates between 1900 and 1910, were similar between counties that received a school and those that did not, and that the prevalence of Rosenwald schools across counties was not systematically related to observable measures of Black socioeconomic conditions prior to the Rosenwald Fund's creation.

Nevertheless, we use several empirical strategies to address any concerns about the selective location of schools. Our research design exploits both the geographic and temporal variation in the location of schools, allowing comparisons of cohorts born within the same county by using county fixed effects. Repeated cross-sections of Census data for some outcomes allow us to control for county-specific time trends. We also take advantage of the explicit targeting of the program to rural Blacks allowing us to

use rural Whites and urban Blacks as control groups in a difference-in-difference framework. As an additional robustness check, we conduct a separate exercise where we only exploit variation arising from idiosyncratic factors that influenced the location decisions of some of the earliest schools built in Alabama that preceded the large-scale rollout of the program. The estimates from this exercise are very similar to what we find with our full sample. This strategy also addresses concerns regarding the possible selective migration of families with a strong preference for education to the Rosenwald counties. Finally, we directly address concerns about selective migration by recalculating Rosenwald exposure rates based on county of *birth* rather than on county of residence for a sample of WWII enlistees and find similar results.

The next section provides background on the Rosenwald schools and sketches out our analytical framework. Section III describes the data. Section IV outlines our empirical strategy. Section V presents our Census results on school attendance and literacy. Section VI discusses our results on adult outcomes, including educational attainment and test scores using the World War II enlistment data and migration using the 1940 Census. Section VII describes how these results differ across pre-Rosenwald community characteristics. Section VIII explains how we compute an internal rate of return for the Rosenwald Initiative. Brief conclusions are offered in section IX.

II. The Rosenwald Rural Schools Initiative

A. Historical Background

The Rosenwald Rural Schools Initiative arose from the collaboration of Booker T. Washington, the principal of the Tuskegee Institute in Alabama, and Julius Rosenwald, a Chicago area businessman. Frustration with the disbursement of funds by local county education boards and the general inadequacy of Black schools[6] led Washington to seek out Northern philanthropists, including but not limited to

[6] For accounts of early 20th century Black schooling, see Bond (1934), McCormick (1934), Myrdal (1944), Margo (1990), and Hoffschwelle (2006). One contemporary description appears in the South Carolina Superintendent of Education's 1917-18 school report: "The school buildings are in the most instances wretched, the terms short, and salaries low, practically no equipment, and the preparation and fitness of the teachers generally very inferior." The

Rosenwald, to build new schools for rural Blacks. After the opening of the first six schools near Tuskegee around 1913-14, Rosenwald agreed to partly fund up to 100 additional schools, primarily in Alabama. Thereafter, the program spread quickly and, by the end of the decade, 716 schools were built in 11 states. Figure 2a displays a map of the number of Rosenwald schools by county as of the 1919-20 school year, the first year in which we know the complete spatial distribution of school buildings. Early schools were primarily clustered in Alabama, as well as in parts of Louisiana, Tennessee, Kentucky, North Carolina and Virginia. Three final states – Florida, Oklahoma, and Texas – were approved for funding in 1920, boosting the final list of eligible states to the 14 shown in Figure 2a.

Rosenwald school construction accelerated in the 1920s, growing by 18 percent per year. Figures 2b and 2c illustrate this expansion by registering the number of schools by county as of 1925 and 1932. When the initiative closed in 1932,[7] 76 percent of counties with rural Black children had a Rosenwald school and 92 percent of rural Black children in the 14 states lived in a county with at least one Rosenwald school. Nevertheless, in most counties the number of Rosenwald schools was insufficient to serve all potential students. Figure 2d displays our estimate of the fraction of school-age rural Black children that could be accommodated by Rosenwald schools by county as of school year 1931-32. These calculations take the ratio of the count of Rosenwald teachers multiplied by an assumed student teacher ratio of 45, which was typical of the time, to the count of 7 to 17 year old rural Blacks derived from the 100 percent Census manuscripts. We estimate that roughly 36 percent of the Southern rural Black school-age population, and 25 percent of all Southern Black school-age children, could have attended Rosenwald schools by 1930.[8]

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report also describes overcrowded classrooms, lack of blackboards, seats, and windows, and "in many cases…superintendents (who) did not even know the location of many African-American schools" (Brannon 1919 as described in Weathers 2008). Racial funding inequities are also described bluntly in Washington's letters to Rosenwald and catalogued more systematically in Johnson (1941) and Margo (1990). For example, the data reported in Johnson (1941) indicates that the average county spent 39 cents on Black school salaries per capita for every dollar on White school salaries per capita in 1930.

[7] The Fund voluntarily closed within a year of Rosenwald's death in 1932. Its closure was hastened by a significant fall in Sears' stock value, which made up two-thirds of the Fund's assets prior to the market crash (McCormick 1934).

[8] Fund documents, starting with the name of the Initiative, are clear that the program was intended for rural communities. For example, a well-publicized experiment to build five urban high schools in the late 1920s was

The program's varied timing and geographic development provides the basis for our main identification strategy. Figure 3 plots the mean and the inter-quartile range of Rosenwald rural coverage across counties over the 1919 to 1930 period, highlighting the substantial amount of cross-county variation. The temporal coverage of Rosenwald schools varied substantially *within* counties as well. For example, although Oklahoma was among the last states to be funded by Rosenwald, by 1930 it had the second highest share of rural Black coverage. In contrast, although Alabama hosted the first schools, by 1930 its coverage was among the lowest.

B. A Framework for Understanding the Effect of the Rosenwald Schools

The canonical model of the school investment decision (Becker 1967; Card 1995) provides the conceptual basis for understanding how the Rosenwald schools improved student outcomes. In this framework, the optimal amount of schooling for individual i is determined by equating the marginal economic return (in terms of future wages) of an additional year of schooling, B_i, to the marginal cost, C_i. The marginal return to a year of school is a positive function of the quality Q of school s ($B_i = B_i(Q_s)$). The marginal cost is a function of foregone contemporaneous earnings and the direct cost of attending school.

Now imagine a population that does not have access to the socially efficient number of schools either because of political oppression or other institutional restrictions. One clear prediction of the model is that by increasing the Q_s that pertains to this group, individuals will choose to spend more time in school and thus ultimately improve their long-term skills. The Rosenwald Initiative aimed to improve Q_s among rural Blacks along several important dimensions. First, the program subsidized the construction of modern facilities that were conducive to learning. This included building designs that provided for adequate lighting, ventilation, and sanitation. The program also required adequate provision of school

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short-lived and very small-scale. That said, a small number of counties with sizable cities, notably Shelby County in Tennessee which contains Memphis, appear to have a high degree of Rosenwald coverage. In some of the analysis to follow, we make sharp restrictions on the definition of rural and urban that ensures that no urban (living in a town or incorporated place with over 2,500 residents) students are included. See Sections III.C. Elsewhere, when we rely on self-reports of rural status, any effect from an urban building will be picked up on urban Blacks and lead us to sometimes underestimate the effect of Rosenwald on our main treatment group, rural Blacks. See Section IV.

equipment (e.g. desks and blackboards)[9] and directly-funded school libraries that ensured supplies of books. All of these key features were lacking in the previous schools that served rural Blacks. Second, the Rosenwald program made an effort to improve teacher quality by first and foremost improving the physical environment for teaching,[10] but also providing for teacher training schools and programs,[11] incentivizing communities to raise teacher salary standards, and in some cases, subsidizing the construction of teacher homes. Third, the program actively sought to increase the length of the school term.

The model also indicates that a second channel by which the intervention may have increased educational attainment among rural Blacks is by reducing the marginal cost, C_i, of attending school. The construction of new schools made access easier for those who lived far from existing school buildings.[12] This is particularly true for high school instruction, which was virtually nonexistent prior to the Fund's involvement at this level starting in 1926.[13] The reduction in costs of schooling has two implications. First, among those families for whom the costs of schooling their children were so high that they would

[9] "School equipment received the same careful scrutiny to ensure that the building could have the greatest impact on its occupants. Blackboards along three walls served the teacher for instruction and students for practice assignments. Modern patent desks replaced the rough wooden slabs, pews, and benches typical of many other Black schools. Often African American community members found it difficult to pay for patent desks in addition to their contribution to the building and asked to be relieved of this burden. White school officials would have preferred to transfer used furnishings from White schools over to Black ones. However, the Rosenwald Fund remained firm and refused to make final payment on buildings that did not meet its standards for the exterior or interior." From http://www.preservationnation.org/travel-and-sites/sites/southern-region/rosenwald-schools/development-of-rosenwald-plans/community-school-plans.html.

[10] Numerous letters from key participants make clear the difficult conditions hampering teaching and the importance of the buildings in improving this situation. A poignant example is Booker T. Washington's June 12, 1912 letter to Rosenwald that concludes: "Many of the places in the South where the school are now taught are as bad as stables, and it is impossible for the teacher to do efficient work in such places."

[11] This included county training schools, summer training programs, and actively working with other philanthropies, most notably the Jeannes Fund, to help train teachers. See, for example, Reed (2004) who discusses the training standards that applied to Rosenwald instructors at one particular school in North Carolina.

[12] Indeed, in 1929, the Fund began offering three year grants to support bus services, conditional on term length and minimum teacher salary standards (Hoffschwelle 2006). Reducing distance from school may also increase attendance of teachers (Kremer, Chaudhury, Rogers, Muralidharan, and Hammer 2005; Chaudhury, Hammer, Kremer, Muralidharan, and Rogers 2006; Das, Dercon, Habyarimana, and Krishnan 2007) and thereby work on the school quality margin.

[13] Although the high school movement was well underway in the North by the mid-1920s (Goldin and Katz 1999), public high schools for Southern Blacks were scarce. Alabama and South Carolina contained no four-year accredited Black public high schools, and Florida had only two that offered any high school instruction. However, by 1932, Rosenwald Fund pamphlets claimed that roughly 10 percent of their schools offered at least two years of high school instruction (Donohue, Heckman, and Todd 2002).

have opted for no schooling in the absence of the program, the presence of a new school nearby may have led them to obtain some schooling. In addition to this effect at the "extensive margin", the lower costs of schooling among those who were already enrolled in school could have led them to obtain more schooling.

Finally, during this era, school finances were determined by county school boards that were dominated by Whites. If this politically dominant group could implicitly tax the Rosenwald subsidy, it is possible that White students in Rosenwald counties may have gained from gifts intended for rural Black schools. Indeed, the diversion of fungible financial resources to White schools was an endemic problem in the South during this period (Bond 1934; Margo 1990; Ascoli 2006). Moreover, anecdotes suggest that the success of the Rosenwald schools led to additional demand for schooling among Whites, which could have accelerated the diversion of resources.[14] A possible implication is that the Rosenwald program may have also benefitted rural Whites, an issue which we return to later when we discuss our differencing strategy in section IV and our results in section V.

Similarly, given the fungibility of money and the institutional environment of the time, it is likely that some of the public financial resources that were targeted to the Rosenwald schools "crowded out" funding that would have otherwise gone to existing Black schools. To counteract both the possibility of crowd out as well as the diversion of funds to White schools, the Rosenwald Fund deliberately focused on providing *physical resources* (e.g. better buildings, equipment, and teachers) that could not be easily expropriated. This implies that traditional measures that have been used to proxy for school quality in previous research (e.g. school expenditures, counts of teachers), would not only fail to capture improvements in actual school quality but may even lead to counter-intuitive conclusions regarding the effects of the Rosenwald schools on racial gaps in educational resources.

C. Matching Grants and School Location Selection

[14] See e.g. McCormick (1934). In a 1919 letter to Rosenwald, Robert Moton, the Principal of Tuskegee wrote: "Let me repeat that there is no movement in America that is doing more, not only in providing larger and more satisfactory school equipment for the Negro race, but doing equally as much in stimulating White people towards making more adequate provisions for the education of their own children, so that you are not only helping Negroes but Whites as well."

The strategies pursued by the Rosenwald Fund reflected the experiences and visions of both Washington and Rosenwald. Washington felt strongly that in the wake of slavery, the Black community could develop only through self-reliance. Rosenwald, meanwhile, was a strong proponent of using matching grants to foster community support; indeed, he had employed the concept of "buy-in" for his previous philanthropic efforts. Consequently, the Fund was unambiguous in its requirement that it would provide financial backing conditional on local support, succinctly summarized by the Fund's refrain: "Help only where help was wanted, when an equal or greater amount of help was forthcoming locally, and where local political organizations co-operated" (McCormick 1934; Hoffschwelle 2006).[15] Local Blacks and state and county governments provided the majority of the funding, particularly after construction was complete (see Table A1).[16] Over time, matching became even more critical, with the Rosenwald share of contributions falling from around 25 percent for the earliest schools to the 10 to 15 percent range during the last five years of the program.[17]

This funding mechanism suggests that individuals from communities that were particularly open to improving Black schools, and thus were able to convince the Fund to invest in their community, might have experienced better outcomes even in the absence of the Rosenwald program. We briefly provide several pieces of evidence regarding the possibility of selective school location. In Figure 4, we show that the initial counties where Rosenwald schools were built by 1919 had slightly *lower* levels and weaker trends in Black rural school attendance in the pre-Rosenwald period (1900 to 1910) than those counties

[15] Letters available at the Fund archives at Fisk University provide a few examples of communities that struggled to obtain the required resources and were therefore denied funding.

[16] Table A1does not include nonmonetary donations of time, materials, and land from local citizens. Comprehensive records of in-kind donations do not exist. Anecdotal evidence from the archives suggests that in-kind donations were particularly important for the earliest schools.

[17] Although the Rosenwald Fund ultimately only covered a small share of the building expenses, it played a crucial role in providing the prestige and credibility to garner the necessary financial and nonfinancial support of local White and Black communities. For example, the Fund hired canvassers to explain available opportunities and guide local Black leaders through the fundraising process (Hoffschwelle 2006). The Fund consulted with and, to varying degrees, gained the support of White government officials who acted as the state agents for Black schools. Rosenwald money also likely helped buy local White acquiescence, including county education board approval for maintaining schools post-construction (Donohue, Heckman, and Todd 2002).

that never received a school.[18] Thus, with respect to a key outcome, we find no evidence of positive selection for the earliest schools. Nevertheless, this concern also motivates our analysis in section VI where we isolate idiosyncratic variation in the location of some of the earliest schools built in Alabama.

In Appendix A, we investigate the extent to which pre-Rosenwald county characteristics affected school location decisions. We find that pre-existing Black socio-economic conditions (e.g. school attendance, literacy, occupational status) and trends in these conditions cannot predict the timing or intensity of initial Rosenwald school locations in a statistically or economically significant way (Table A4).[19] However, we do find some evidence that counties with higher levels of White literacy, irrespective of White occupational structure, were more likely to build an initial school in the program's early years. This result is consistent with Washington's strategy, perhaps continued by the Fund after his untimely death in 1915, of avoiding areas that might lead to White backlash.[20] In any event, we remove any bias that is generated by the matching grant strategy or selection on White characteristics, by using county fixed effects and county fixed effects interacted with Census year. The latter approach further limits our variation to differences in exposure across birth cohorts *within* counties in a given year.

III. Data

A. Rosenwald Schools

Through an agreement with the caretaker of the Rosenwald Fund's archives -- Fisk University in Nashville, Tennessee -- we received digital versions of the index cards used to track the Fund's 4,972 construction projects. These cards are the only complete database of the individual Rosenwald schools. Each card contains a description of a school, teacher home, or industrial shop, or some combination

[18] If we go back even further to 1880, the counties which built the initial Rosenwald schools had virtually identical levels of Black rural school attendance as the counties that never obtained a Rosenwald school.
[19] Black occupational status has a marginally statistically significant effect in some specifications involving the schools built during the 1920s but the size of the effect is not qualitatively large.
[20] Neither Washington nor the Rosenwald Fund challenged segregation, which almost surely increased White support for the schools. The view within the Fund echoed Washington's well-known belief that education and economic needs had to be addressed first —a strategy that led to deep conflicts with other activists, notably W.E.B Du Bois and the NAACP.

thereof. Information is limited to the location (state and county), year of construction, school name, number of teachers (or home/shop rooms), number of acres of land, insurance valuation, and construction cost. Cost is broken down by four possible funding sources: the Rosenwald Fund, local Black individuals, local White individuals, and local public governments. Room additions, as well as complete destructions due to fire or weather, are recorded in handwriting ex-post although it is difficult to know how complete these adjustments ultimately are. However, for completeness, we include all recorded additions and rebuilds in the relevant year that they take place. Appendix Table A1 provides basic statistics about all Rosenwald school construction projects. Our analysis uses a database that includes 4,932 schools with the capacity to hold 13,746 teachers in 888 counties.[21]

B. Census (1900-1930)

We pool cross-sectional samples drawn from the 1900 to 1930 decennial Censuses using the Integrated Public Use Microdata Series (or IPUMS, see Ruggles, Alexander, Genadek, Goeken, Schroeder, and Sobek 2010). In particular, we use the 1 percent sample for 1900, the 1.4 percent sample for 1910, the 1 percent sample for 1920, and an early version of the 1930 5 percent sample.[22] These data are linked to the Rosenwald schools by county of residence and birth year. Importantly, we can also distinguish between those living in rural or urban areas within a county.[23]

The two relevant outcomes available in these Censuses are school attendance and literacy. School attendance refers to whether an individual attended or enrolled in school between September 1 and the Census date (June 1 in 1900, April 15 in 1910, January 1 in 1920, and April 1 in 1930). For this measure, we construct a pooled sample of over 640,000 Black and White children between the ages of 7

[21] Official Rosenwald Fund records tally 4,977 schools in 883 counties. Our database starts with 4,972 index cards. We delete 36 of these cards because the project did not involve a schoolhouse (22 cases), contained missing information on cost or teachers (10 cases), or was never built (4 cases). Additionally, we drop the four Missouri projects. We also exclude county training schools because of uncertainty as to whether they housed students or were used for their original purpose to train teachers. However, our results are impervious to the inclusion of county training schools.

[22] Since the 1910 Census oversamples certain groups, we utilize sample weights in our main estimates.

[23] It may be the case that the Rosenwald Fund's vision of a rural community differs from the technical Census definition of less than 2,500 people, adding attenuation bias to our estimates. However, in internal documents, the Fund often used the Census definition for data organization and evaluation.

and 17. Literacy, which is asked of individuals 10 and older, refers to the ability to both read and write in any language. Collins and Margo (2006) find that this measure acts as a proxy for completing 1 to 3 years of schooling. They also show that literacy rises with age. To abstract from literacy effects due to schooling at young ages, we restrict the sample to those who are at least 15. We also restrict the sample to those under 23 to avoid spurious correlation arising from the possibility that adults with high literacy moved into Rosenwald counties for their children's schooling.[24] This sample of 15 to 22 year olds totals over 430,000 persons.

Appendix Table A2 presents descriptive statistics of these samples. Of note, the rural Black-White school attendance gap was 21 percentage points in 1910 but narrowed to 9 percentage points by 1930. In urban areas, the Black-White attendance gap fell from 13 to 7 percentage points. The table also illustrates the striking racial differences in measures of family background such as parent literacy and home ownership. For example, as late as 1930, the Black-White gap in father's literacy was about 20 percentage points. With this detailed individual-level data, we are able to control for such factors in our empirical analysis.

C. World War II Enlistment Records

We also draw from records of US Army World War II enlistees, available from the National Archives and Records Administration. Our overall sample includes roughly 1.8 million men born between 1910 and 1928 and living in Rosenwald states when they joined the Army between 1940 and 1946.[25] We use age and county of residence at enlistment to link soldiers to their potential access to Rosenwald schools. Unlike the Census, however, we do not know rural status. Instead, we use the 1910 through 1930 Censuses to classify counties based on their rural composition. Since the Census uses a population cutoff of 2,500 for defining whether a city or incorporated place is urban, we classify a county as rural if it had no individuals living in places with a population greater than 2,500 in the 1910 through

[24] We find generally weaker effects when the age range is expanded to 15 to 30 year olds suggesting that selective migration is not a significant concern. Sections VI, VII.D.3, and VII.E provide more evidence on this latter point.
[25] Records are available for enlistees starting in 1938 but the samples are very small. See Feyrer, Politi, and Weil (2008) for more description of the data.

1930 Censuses. This ensures that we do not include any urban students in our treatment group. We define a county as urban if more than half of the county's residents from 1910 through 1930 lived in cities containing greater than 2,500 people. It is possible therefore that some treated individuals may be contained in our control groups. These restrictions produce a sample of nearly 1 million men with nearly equal numbers living in our definition of rural and urban counties.[26]

A key advantage of this data, relative to the early Censuses, is that it provides measures of human capital during adulthood. This includes completed years of schooling beyond grammar school[27] from which we also calculate indicators for attending and completing high school. A unique feature of the data is that, for a brief period in 1943, we have scores from the Army General Classification Test (AGCT) used to determine military occupation. Since these test scores were thought lost to history, we describe them in Appendix B. In addition, for 1941 and 1942, there are data on height, which we use as a validity check since height is unlikely to be impacted by access to schools after age 5 (Martorell, Schroeder, Rivera, and Kaplowitz 1995; Behrman and Hoddinott 2005).

A clear concern with the enlistment records is that there may be selection into who was inducted into the Army. We discuss a variety of ways in which we address potential selection bias in the next section. In one such exercise, we use a smaller subsample of about 17,000 WWII enlistees, for whom we have determined the county of birth by matching individuals' name, state of birth, and year of birth to the Social Security Death Master File (DMF). We obtained the place of birth (county or city) from a match provided to us from the Social Security Administration (SSA) using their NUMIDENT file.[28]

[26] Our primary classification excludes counties with a rural share between 50 and 100 percent. We discuss the implications of using alternative rural and urban thresholds in section VII.A.

[27] For those who did not complete grammar school, we impute years of schooling based on race, birth year, and state economic area from the 1940 Census and exclude individuals who had completed fewer than four years of schooling, the military's requirement in 1941-1942 (Perrott 1946). Our results are not sensitive to small changes in imputation methods.

[28] We drew three samples from the WWII data to match to SSA data: Blacks living in Rosenwald counties, individuals who enlisted in 1941 and 1942 for whom we have height, and individuals for whom we have AGCT scores. There are many sources of possible selection with this sample: 1) Individuals must die between 1965 and 2007; 2) they must enter the Social Security System and own a Social Security Number; 3) they must be uniquely identified; and 4) they must provide a clearly recognized county or city name for their place of birth that can be matched to the Rosenwald database.

Summary statistics of the WWII enlistee samples are presented in Appendix Table A3. The Black-White gap in years of schooling for the full sample is about 2.2 years. The racial difference in AGCT scores is about 25 points which is equivalent to 1.1 standard deviations based on the test score distribution for the full sample.

IV. Empirical Strategy

A. Regression Model

For our analysis using the Census data, the main specification is of the following form:

$$(1) \quad y_{ibct} = \alpha + female_i + black_i + rural_i + blackrural_i + \gamma_0 ROSE_{bct} + \gamma_1(black_i \times ROSE_{bct}) + \gamma_2(rural_i \times ROSE_{bct}) + \gamma_3(blackrural_i \times ROSE_{bct}) + \beta X_{ibct} + \theta_{st} age_i + year_t + county_c + \varepsilon_{ibct}$$

where y_{ibct} is school attendance or literacy for individual *i* born in year b living in county *c* in Census year *t, female*, *black*, *rural* and *blackrural* are indicators of being in one of those demographic categories, X_{ibct} is a vector of family background characteristics including mother's literacy, father's literacy, father's occupational status and father's home ownership, *age* is interacted with state *s* and Census year *t*, *county* represents county fixed effects, and ε_{ibct} is an error term. Standard errors are clustered at the county level.[29]

Our first measure of Rosenwald treatment ("ROSE"), R_{ct}, is an indicator of whether a Rosenwald school was present in an individual's county *c* as of Census year *t*. In our tables, we refer to this measure as "Rosenwald presence." R_{ct} is limited in at least two ways: it fails to distinguish differences in Rosenwald exposure between birth cohorts within a county[30] and it does not adjust for the breadth of Rosenwald coverage within a county. Despite these drawbacks, we start with R_{ct} because it provides a straightforward approach that is easy to interpret.

[29] Clustering at the state level, as in Bester, Conley, and Hansen (2010), has minimal impact on our inferences.
[30] For example, a 13 year old in 1930 living in a county that first opened a Rosenwald school in 1928 has only two years of exposure but is treated the same as a 13 year old living in a county that built a Rosenwald school in 1924 and has 6 years of exposure.

Our second, more comprehensive measure of "Rosenwald exposure," E_{bc}, estimates the average Rosenwald coverage that each student born in year b and living in county c experienced over ages 7 to 13.[31] Specifically for individuals 13 or older as of a Census, $E_{bc} = \frac{1}{7}\sum_{t=b+7}^{t=b+13} \frac{T_{ct} \times 45}{\mathrm{N}_{ct}}$, where T_{ct} is the number of Rosenwald teachers in county c in year t and N_{ct} is the number of rural Blacks between the ages of 7 and 17 in the county in each year. For the numerator of our coverage measure, we assume a class size of 45 students per teacher, a standard at the time (Johnson 1941) and an assumption used by the Rosenwald Fund in internal and published documents. The denominator is computed from the digitized 100 percent 1920 and 1930 Census manuscript files available through ancestry.com and interpolated for 1919 and 1921 through 1929. For individuals under 13 as of a Census, we average the coverage rates up to and including the year of the Census. That is, for 10 year olds: $E_{bc} = \frac{1}{4}\sum_{t=b+7}^{t=b+10} \frac{T_{ct} \times 45}{\mathrm{N_{ct}}}$. Since E_{bc} takes on values between 0 and 1, the γ coefficients can be interpreted as the effect of going from no Rosenwald exposure in one's county to complete exposure (i.e. every rural school-age Black child could have attended a Rosenwald school).[32] However, when considering economic magnitudes, it may be particularly revealing to describe the effects at the mean level of exposure in order to account for population-wide changes in racial gaps over particular time periods or cohorts.

For our analysis using the World War II data, the main specification is of the following form:

$$(2)\ y_{ibcq} = \alpha + black_i + rural_c + blackrural_{ic} + \gamma_0 E_{bc} + \gamma_1(black_i \times E_{bc}) + \gamma_2(rural_c \times Ebc + \gamma 3 blackruralic \times Ebc + \beta Xc + agei + agei \times blacki + enlqtrq + enlqtrq \times blacki + countyc + \varepsilon ibcq$$

There are a few differences in (2) compared to the Census-specific specification (1). We rely exclusively on E_{bc} as our measure of ROSE since most of the WWII enlistees attended school in the late 1920s and 1930s when the vast majority of counties with Black children had at least one Rosenwald school.

[31] Since we cannot identify which schools built after 1926 were high schools, we confine our analysis to the effects of exposure during the ages of 7 to 13. However, our results are robust to defining exposure over the ages of 7 to 17.

[32] In some cases, the exposure measure exceeds 1; in such cases, we topcode values at 1. Rosenwald coverage rates exceed 1 for 6 counties in 1919, 48 counties in 1925 and 109 counties in 1930.

However, even with county fixed effects, there remains significant cross-cohort variation in the timing of the construction of schools to exploit. X_c includes the same *average* family background characteristics as (1) but, because these measures do not exist for individuals in the WWII data, it is measured at the county level using the Census year when a cohort was between the ages of 0 and 9. As described in section III.C, *rural* is also defined at the county level. To control for age, we include a set of age dummies for each race. We also include a set of race-specific time dummies, *enlqtr*, to control for the 28 quarters (indexed as q) from 1940 Q1 to 1946 Q4 in which men could have enlisted. Finally, the sample is composed entirely of men and therefore the gender indicator is dropped.

In both (1) and (2), we interact our Rosenwald measures with race and rural status to take advantage of the explicit targeting of the treatment to rural Blacks while allowing the other groups (e.g. rural Whites) to potentially serve as controls. Consequently, the γs enable us to construct "differenced" estimates. For example, to calculate the effect of complete exposure versus no exposure on Black rural children we would sum γ_0, γ_1, γ_2, and γ_3. The sum of γ_2 and γ_3 provides the difference in effects between rural and urban Blacks (labeled "Black, Rural-Urban" in the tables). Similarly, the sum of γ_1 and γ_3 estimates the effect on the Black-White gap in rural areas ("Black-White Rural"). Finally, γ_3 taken alone, provides an estimate of the "triple difference," which describes the effect of Rosenwald exposure on the Black-White gap in rural school attendance relative to the Black-White gap in urban school attendance.

The advantage of differencing across groups is that common factors that may be correlated with Rosenwald school exposure are removed. In particular, suppose that in the absence of the interactions, the error term is structured as $\varepsilon_{ict} = \omega_{ct} + \omega_{ct,race} + \omega_{ct,rural} + \omega_{ct,race,rural}$. By interacting rural with ROSE, any correlation between ROSE and the error component $\omega_{ct,rural}$ is absorbed. This design accounts for factors, such as a positive shock to the rural economy, that benefited both rural Whites and rural Blacks and were also correlated with the formation of the schools. Similarly, $\omega_{ct,race}$ can account for race-specific factors that happened to be coincident with the construction of Rosenwald schools that would also affect urban Blacks. Finally, we can difference out both race and rural status, thereby eliminating the correlation between ROSE and $\omega_{ct,race}$ and $\omega_{ct,rural}$. In this case, our identification rests on the assumption

that there are no unobservable factors, $\omega_{ct,race,rural}$, that are correlated with ROSE that only affect rural Blacks. Put differently, any alternative explanation for our findings must utilize variation that had no effect on rural Whites or urban Blacks.

Each of our four key estimators -- Rural Black; Black, Rural-Urban; Black-White Rural; triple difference -- requires different assumptions about how unobservables vary by demographic group. Moreover, the possibility that Rosenwald resources were diverted to White schools is not only theoretically plausible (see Section II.B) but visible in the actions and words of Fund leaders (see footnote 13). Consequently, any positive effect on Whites would imply Black-White estimators may understate the gains for rural Blacks. Additionally, for the Census specifications that rely on self-reported rural status, if Rosenwald schools were built in areas that exceeded 2,500 residents, our comparison of rural and urban Blacks would understate the gains for rural Blacks. Therefore, we consider all four estimators, including the simple rural Black estimator, in assessing the range of effects of Rosenwald on rural Black schoolchildren. In practice, the estimates are typically tightly bunched, a consequence of the control groups revealing little systematic selection favoring rural communities or Black children, the Fund's building being concentrated in areas deemed rural by the Census, and diversion of key resources to White schools being of minor importance.

In addition to differencing, we exploit variation in Rosenwald school coverage either over time (for R_{ct}) or across cohorts (for E_{bc}) to control for unobserved county characteristics by using county fixed effects. With the exposure measure, E_{bc}, we can also specify separate county fixed effects *for each Census year* (i.e. add *county* × $year_{ct}$ to equation 1) to address any long-term (e.g. 10 year) time trends that are county-specific. This is because even within a particular county in a particular Census year, there is sufficient variation in Rosenwald exposure across birth cohorts due to the timing of school construction. This variation allows us to overcome threats to identification that arise from the possibility that Rosenwald schools were built in counties with particular characteristics at a point in time (see Appendix A) or that were exhibiting certain trends over long periods of time. Moreover, this framework accounts for concurrent policy changes at the state or national level, such as the introduction and

expansion of compulsory schooling and child labor laws, as well as more general trends such as improvements in health (e.g. disease eradication) or the lessening in importance of "intergenerational drag" from slavery (Margo 1990).[33] Coupled with our differencing strategy, the fixed effects approach implies that any alternative explanation for our findings must be rely on within-county variation that only affected rural Blacks and happened to coincide with the timing of school construction for cohorts who were of school-age at the time the schools were built.

To address remaining concerns related to unobservables, and in particular, to their potential impact on initial nonrandom residential household location decisions, we also provide three additional sets of results: a) family fixed effects[34], b) estimates based on the initial Alabama schools, and c) estimates based on exposure in one's location at birth rather than contemporaneous residential location. In the latter two cases, residential location precedes large-scale rollout of the Rosenwald program and, by construction, is unlikely to be tainted by endogenous migration decisions.

B. Selection in World War II Data

The World War II sample introduces an additional concern about potential bias from nonrandom induction into the Army. Prospective enlistees were screened on mental and physical characteristics. Moreover, these criteria were lowered when manpower needs rose after the US entered the war (Lew 1944). Since our sample includes both men who were drafted through lotteries as well as volunteers, mean characteristics such as years of education may differ from the overall population.

Of particular concern for our purposes is the extent to which selection into the military might bias our parameter estimates of the effects of Rosenwald exposure. A positive bias could arise, for example, if counties where Rosenwald schools were most effective at improving educational outcomes happened to have other characteristics that led to greater recruitment and higher induction rates of rural Blacks. Alternatively, as manpower needs became the preeminent concern for the military, and standards were

[33] We note that Lleras-Muney (2002) finds no impact of compulsory schooling and child labor laws on Black education. Likewise, the eradication of hookworm disease (Bleakley 2007) predates our cohorts, and primarily impacted Whites living in coastal areas (Coelho and McGuire 2006; Keller, Leathers, and Densen 1940).
[34] We can only use family fixed effects for studying effects of school attendance with the 1900-30 Census samples.

loosened for more recent cohorts, recruitment might have become more intense in counties where Rosenwald schools were less effective, possibly leading to a negative bias.

A useful way to gauge whether selection into the sample may have been related to the success of the schools is to compare the probability of selection with Rosenwald exposure. If the measures are correlated, selection would be of greater concern. We estimate the probability of selection by taking the ratio of the number of actual male enlistees by county, birth year, and race, to an estimate of the corresponding population of each group. For the numerator, we use counts from the population of male WWII enlistees; the denominator is derived from digitized records of the complete 1930 Census manuscript files available at ancestry.com. We use 1930 as a baseline because the 1940 data are not yet available. There are a total of 26,448 cells for each race (1,392 counties × 19 cohorts).[35]

Figure 5 shows the scatter plot of the probability of selection for Blacks against Rosenwald exposure E_{bc}, along with a regression line for a subsample of 20,494 observations for counties that had a positive number of Black residents in the 1910 Census and non-missing values for exposure and probability of selection. Although visually there appears to be very little relationship between the variables, the regression coefficient is positive, 0.036 (0.009) and statistically significant (standard errors are clustered at the county level). However, once we include county fixed effects and cohort dummies, the coefficient is reduced to 0.004 and is no longer quantitatively important or statistically significant.[36]

Therefore, our first approach to dealing with selection is to incorporate county fixed effects, age dummies interacted with race, and quarter of enlistment dummies interacted with race in our specifications. The use of county fixed effects should eliminate any selection bias that is associated with time invariant county level characteristics that may be common to the selection of both Whites and Blacks. The age-by-race dummies and quarter of enlistment-by-race dummies eliminate selection

[35] The estimated probabilities are greater than 1 for 124 cells for Whites and 363 cells for Blacks. In these cases, we topcode the values at 1.

[36] The coefficient is further reduced to -0.0006 (0.007) if we exclude 298 cells where the probability of selection is 0.6 or greater. Such high probabilities of selection are likely to be implausible, particularly since the denominator contains both males and females. The estimate is reduced to 0.002 (0.007) if we simply exclude 184 cells where the probability of selection is estimated to be 1 or more.

operating at the *national* level that affect some ages or time periods differently by race. For example, if military recruiters began to target Blacks of all ages in greater numbers across all counties as the War heated up in late 1942 and 1943, this form of selection would be absorbed by these indicators.

These controls, however, do not account for county-specific factors that might have changed over time, across birth cohorts, or differed by race. To build on the last example, the increasing demand for soldiers as the War intensified may have led recruiters to look for volunteers among 18 year old Blacks living in counties that had Rosenwald high schools. This particular form of selection involves the "interaction" of county-by-cohort-by-race and therefore is not absorbed by our fixed effects, though it is worth noting that our earlier exercise found no evidence suggesting that selection at this level was a problem. In any case, we cannot sweep out effects at this level of interaction through indicator variables and maintain identification since our exposure measure, E_{bc}, is defined at the county-by-cohort level.

Therefore, our second strategy is to directly account for the probability of selection at the county-by-cohort-by-race level using Inverse Probability Weighting (IPW). This approach involves weighting our regressions by the inverse of the probability of enlistment for each observation based on their cell. Define p to be the true likelihood that a given individual will enlist, and $\hat{p}$ to be an estimate of that likelihood. Then, weighting the regression equations by $w_i = \frac{1}{\hat{p}}$ removes any selection bias, as long as the observables used to estimate the probabilities account for all sample selection within cells (e.g. Hirano, Imbens, and Ridder 2003; Wooldridge 2002; Chay, Guryan, and Mazumder 2009). Note that unlike studies that use a selection equation (e.g. propensity score) with a sample to estimate $\hat{p}$, we have the universe of World War II enlistees and the full population counts from the 1930 Census.

Any sources of selection that operate *within* cells defined by county-by-cohort-by-race would not be addressed with our IPW strategy. For example, suppose it were the case that among a set of rural Black men attending Rosenwald schools who were the same age and lived in the same county, only those who were the most successful students chose to volunteer for the Army. The use of weights derived from probabilities estimated at the county-by-cohort-by-race level would not address this source of bias since

the probabilities differed within cells. Likewise, any other characteristics operating below the level of county-by-cohort-by-race that influenced selection also would not be addressed by IPW.

To address the possibility that individual preferences for military service could be correlated with the effectiveness of the Rosenwald schools, our third strategy confines the sample to individuals who were drafted in the year in which they first became age-eligible for the draft. When conscription was first instituted in October 1940, all men between the ages of 21 and 35 were required to register. In November 1942, the draft age was reduced to 18. Therefore, we construct a sample that for each year includes only draftees who were either of the minimum draft age or one year older (to account for the fact that we do not know the exact date of birth but only the *year* of birth). Thus the sample includes 21 and 22 years old who were drafted in 1940 and 1941, 20 and 21 year olds drafted in 1942, and 18 and 19 year olds drafted during 1943 to 1946.[37] Since there is unambiguous scope for 20 to 22 year old draftees to have volunteered at younger ages, we also report results solely for the small subsample of 18 and 19 year old draftees during 1943 to 1946.[38]

Finally, we describe results on similar education outcomes using the 1940 Census, which does not contain the potential selection problems related to military enlistment but requires using a more blunt measure of Rosenwald exposure.

V. Census Results on School Attendance and Literacy

A. School Attendance

Table 1 provides results for school attendance using the indicator of county Rosenwald presence, R_{ct}. The top panel displays the γ coefficients from equation (1). Below that, we report a series of estimates based on combinations of the γs: 1) the implied effects for each of the four demographic groups

[37] According to our data, 95 percent of draftees were at least 21 in 1941. In 1942, 9.2 percent of draftees were 20 and 90 percent were 21 or older. During this time, few 18 to 20 year olds volunteered. After the draft age was lowered to 18, the age distribution shifted markedly. Between 1943 and 1946, 25 to 30 percent of draftees were 18 or 19. Note that draftees made up 75 percent of the military during the War, peaking at 92 percent in 1943.

[38] For that sample, we cannot include county fixed effects since there is no variation within county in E_{bc} for the relevant cohorts.

– Black rural, White rural, Black urban, and White urban, 2) difference-in-difference estimates between Black rural and Black urban, White rural and White urban, Black rural and White rural, and Black urban and White urban; and 3) the triple differenced estimate of Black-White rural less Black-White urban. We start with a minimal specification in column (1) that includes only year effects to show the basic patterns in the data. Naturally, other important factors, like family background characteristics, are likely to affect school attendance. In Column (2), we add controls for gender, age, parents' literacy, father's occupation score, father's homeownership, state fixed effects, and the White literacy rate in the county in 1910. In column (3), we omit these controls but add county fixed effects. Column (4) includes both the demographics and the county fixed effects.

Beginning with the most parsimonious specification, we find an economically and statistically significant effect on rural Blacks; the presence of a Rosenwald school in one's county boosted school attendance among potentially eligible children by 8.9 (0.7) percentage points. By comparison, there is no effect on either rural Whites or urban Whites. There is a smaller, but statistically significant, effect on urban Blacks that could reflect the possibility that some Blacks classified as urban by the Census attended Rosenwald schools. Alternatively, the effect on urban Blacks may reflect that there were higher school attendance rates among all Blacks living in Rosenwald counties. When we difference out the common effect of being Black (Black, Rural-Urban) or the common effect of living in a rural county (Black-White, rural), the Rosenwald effect remains economically large and highly significant. Finally, differencing the rural Black-White effect and the urban Black-White effect, we find that a Rosenwald school is estimated to raise school attendance by 6.7 (1.1) percentage points.[39]

Concentrating on the most complete specification in column (4), the four different estimates of the treatment effects on rural Blacks (Black rural, Black rural – Black urban, Black rural – White rural, and the triple difference) range from 4 to 7 percentage points and are all statistically significant at the 1 percent level. The magnitudes of these estimates are economically important. For brevity, we focus on what the effects of the program imply for the Black-White gap among rural students across our outcomes

[39] We get extremely similar results if we use those attending school *and not working* as an outcome.

in the various samples, but in principle we can calculate similar statistics for the rural – urban gap among Blacks or the triple difference. Over the 1910 to 1930 period, the Black-White rural difference in school attendance fell by about 11.5 percentage points from 1910 to 1930. Our estimates suggest that Rosenwald schools can account for 5.8 percentage points, or 50 percent, of this decline.[40]

Table 2 reports school attendance results using the more refined Rosenwald exposure measure, E_{bc}. Column (1) begins with a sparse specification and shows large and significant effects of complete exposure on rural Blacks using any of our four estimates. We find that adding county fixed effects (column 2) and further adding age interactions by state and year (column 3) has relatively little effect on our four key estimators. For example, the simple un-differenced estimate of "Black Rural" is about 0.12 in all three cases.

However, when we include county fixed effects by Census year (column 4), we estimate a large effect on urban Whites (γ_0). As we discussed in section II.B, this might reflect the possible diversion of public funds to White schools. If on the other hand, this reflects some form of positive selection regarding the placement of schools in counties that would have experienced school attendance gains even without the schools, our differenced estimators may remove this bias. Indeed, we find that our three differenced estimates yield statistically significant effects that are only slightly lower in magnitude than we find in column (3).[41] As we will show later, we do not find statistically significant positive effects for Whites in any of our other results.

Overall, moving from the blunt Rosenwald treatment measure shown in Table 1 to the more nuanced measure in Table 2 lowers the implied aggregate effects of the program. For example, specification (4), which includes the baseline controls along with county-by-year fixed effects and age interactions by state and Census year, suggests that going from no exposure to Rosenwald schools (E_{bc}=0)

[40] We take the effect of 6.3 percentage points on Black-White Rural and scale this down to 5.8 percentage points since 92 percent of rural Black school-age children in 1930 were living in a county with a Rosenwald school.
[41] We also find that the results are similar if we use age dummies rather than a linear term, include county-specific age (cohort) trends, interact the state- and Census year-specific age trends with race or rural status, use alternative IPUMS samples, use alternative weighting methods, or construct our exposure measure using both urban and rural Blacks.

to the mean level of Rosenwald exposure for rural Blacks in 1930 (E_{bc}=0.27)[42] raised school attendance of rural Blacks relative to rural Whites by about 3.1 percentage points. That estimate accounts for 27 percent of the reduction in the gap between 1910 and 1930.

Finally, in columns (5) and (6), we allow for family fixed effects (within Census year). As was the case with column (4), we find a positive effect on urban Whites but also find that the differenced point estimates are similar to what we find in other specifications. However, the standard errors rise sharply. Consequently, only the effects on the Black-White rural gap remain statistically significant at reasonable confidence levels. Nevertheless, by moving to a comparison of siblings who may have had different levels of exposure to schools simply because of the timing of their birth relative to the construction of the schools further narrows the scope for alternative explanations of our findings.

It is important to note that relative to the full capacity of the Rosenwald schools, the gains in school attendance were relatively modest, suggesting that most of the students who were attending Rosenwald schools would have attended another school in the absence of the program. To be concrete, in 1932, approximately 36 percent or 650,000 of the 1.8 million school-age rural Blacks were in Rosenwald schools. If we assume that Rosenwald schools increased school attendance by roughly 5 percentage points, which is within the range of our estimates, then 560,000 (1.8 million x (1 – 0.05)) of the 650,000 Rosenwald students would have attended school in the absence of the program. While Rosenwald schools may have "crowded out" other schools, the program nonetheless appears to have been beneficial in increasing attendance and, as we document below, other human capital measures. This success was likely a consequence of improvements in both quantity, via lowering the cost of attending school, and quality relative to the documented inadequacy of the pre-Rosenwald rural Black schools.

B Literacy

Table 3 reports results for literacy using our Census sample of 15 to 22 year olds. Using our preferred specification for R_{ct} (column 2), we find that the presence of a Rosenwald school in the county raises rural Black literacy rates by 9.3 (0.6) percentage points. As with school attendance, we estimate no

[42] Exposure for all Blacks (rural and urban) was 0.28 in 1930 (see Table A2).

effect for rural Whites and a small positive effect on urban Blacks. This leads to difference-in-difference estimates of 7.2 (0.8) percentage points for the rural-urban Black difference and 9.3 (0.5) percentage points for the Black-White rural difference. The triple difference is somewhat lower at 5.3 (0.9) percentage points. However, some of this attenuation is due to an estimated decline in literacy rates among urban Whites in Rosenwald counties. This may be driven by sampling error rather than by a true decline in literacy since literacy rates were already close to 100 percent among urban Whites by 1910.

In columns (3) and (4), we turn to the exposure measure. Although we again find quantitatively large and statistically significant effects for rural Blacks, we now find larger negative effects for urban Whites and small but statistically significant negative effects for rural Whites. These results are difficult to reconcile with the framework from section II.B that suggested that, if anything, the effects on Whites could be positive. Nevertheless, using the differenced estimators, our preferred specification in column (4) shows that complete exposure to Rosenwald schools improved Black literacy relative to Whites in rural areas by just under 25 percentage points. Similarly, the effect on literacy for rural Blacks relative to urban Blacks is nearly 18 percentage points. The estimated effect of complete exposure on the difference between the Black-White rural gap and the Black-White urban gap is to narrow this difference by 16.5 percentage points. Focusing on the aggregate implications of the program, we estimate that access to the Rosenwald schools accounted for 55 percent of the closing of the Black-White rural gap in literacy among 15 to 22 year olds from 1910 to 1930.

C. Earliest Schools Built in Alabama

Thus far, we rely on fixed effects and differences between treatment and control groups to address any potential selection driven by local demand for education. This section presents an alternative approach based on the location of the earliest schools.

The Rosenwald Fund archival records supply clues that the initial schools were heavily clustered in specific geographic areas (see Figure 2a) for idiosyncratic reasons that were largely unrelated to

economic or educational circumstances.[43] We focus on the first schools in Alabama where the evidence for exogenous school location is most transparent and compelling. In particular, Booker T. Washington found himself located in Alabama due to happenstance [44] and archival records suggest that he sought to build the initial schools there in order to quickly and efficiently develop a model for the future rollout of the program:

> "*At present, it is thought wise to confine the schoolhouse building to the State of Alabama with the view of getting experience that will enable us to render the best service for the least money and in the shortest time possible.*" [45]

Moreover, we know from future construction activity (e.g. Figures 2b-d), school expenditure data (Johnson 1941), and anecdotes[46] that there is little to suggest that Alabama's underlying demand for Black schooling was high relative to the rest of the South. Therefore, we estimate the effects of the Alabama schools built between 1913 and 1920 on school attendance rates of 7 to 17 year olds as of the 1920 Census, using data drawn from the 1900 to 1920 Censuses.

Specifically, we compare the effects of the program on children who lived in Alabama counties along the state border to a control group consisting of children who lived in contiguous counties on the

[43] The cluster of schools in Eastern Virginia and North Carolina could be explained by their proximity to Virginia's Hampton Institute where Washington was trained. The clusters in Louisiana, North Carolina, and Tennessee may have been related to the presence of certain individuals, particularly the state agents for Black schools or county officials who happened to be sympathetic to the Rosenwald program. Washington specifically considered exploiting such friendly contacts in a June 1912 letter to Rosenwald:

> "The wisest plan would be…to get…a half dozen county superintendents and county boards who are in thorough sympathy with the plan, get them to work in their county, and in this way it would soon attract the attention of other county officials…"

During 1918 and 1919, when the Fund began to divert more resources out of Alabama, strong letters of interest were received from the Boards of Education of Louisiana, North Carolina, and Tennessee. Appendix A also highlights that the initial school sites were not strongly related to observable characteristics of the counties.

[44] Washington was born into slavery in Virginia where he became trained as a teacher at the Hampton Institute. Local citizens in Tuskegee contacted the Hampton Institute in Virginia in search of a founding director who they expected would be White. The principal of the Hampton Institute recommended Washington and this was the basis of his location in Alabama.

[45] Source: "Plan for Erection of Rural Schoolhouses," date and author unknown, Fisk archives.

[46] Bond (1969) describes how Alabama's school superintendent noted in 1911 that local school boards were averse to building or repairing Black schools, even with funds remaining after all work had been completed on White schools.

other side of the border (see Figure A1 for a map of the counties included).[47] Because there were few large urban areas along the border and sample sizes are limited, we have insufficient power to estimate differences by rural status. Further, due to lack of power, we only use R_{ct}, the indicator of Rosenwald presence in one's county. Since R_{ct} does not take into account the intensity of treatment -- the fact that 12 year olds in 1920 who were treated will have had, on average, more years of exposure to the schools than 7 year olds in 1920-- and since we do not know the year that schools were built prior to 1919, we also estimate the effects for a slightly older sample of 9 to 17 year olds. Our regression specification is the following:

$$(3)\ y_{ict} = \alpha + female_i + black_i + \gamma_0 R_{ct} + \gamma_1 (black_i \times R_{ct})$$
$$+\delta (black_i \times R_{ct} \times Non\_Alabama_s) + \beta X_{ict} + \theta_{st} age_i + county_c + year_t + \varepsilon_{ict}$$

where γ_1, the estimated effect on Blacks in Alabama relative to Whites, is our coefficient of interest.

Table 4 reports the results. We find that when we combine all four (Georgia, Florida, Mississippi, and Tennessee) borders, the presence of a Rosenwald school increases the likelihood of school attendance of Blacks relative to Whites by 5.1 (3.2) percentage points. The effect rises to 7.6 (3.5) percentage points among 9 to 17 year olds. These estimates are quite similar to those reported in Tables 1 and 2.[48]

Another potential threat to the validity of our inferences is that the presence of a Rosenwald school in a county might have prompted families who placed a high value on their children's schooling to migrate to or stay in these counties.[49] A possible implication of such selective migration may be to overstate the effects of the program since outcomes for these children might be higher even in the absence of the program. It is difficult to directly assess the potential magnitude of this bias since the rate of

[47] In general, the Alabama counties had lower levels of schooling and poorer socioeconomic outcomes than the adjacent counties outside of Alabama. However, we use county fixed effects to sweep out that source of variation.
[48] Rosenwald exposure was around 10 percent in Alabama around 1919. Therefore, the point estimates suggest that over half of the Rosenwald students in Alabama would not have attended another school in the absence of the Initiative, suggesting far less crowd-out than during the remainder of the program's existence.
[49] Emmett Scott of the Tuskegee Institute in 1918 noted that the presence of the schools appeared to be valued by families who stayed in the South: "Of the rural Black people who choose to remain in the South, many will tell you that they are content because they have a good school for their children to attend, a friendlier understanding with their White neighbors, and a brighter outlook because of the Rosenwald rural school." (McCormick 1934).

migration across geographic areas is generally unavailable prior to the 1940 Census. However, the earliest schools are also largely purged of location choices since the rapid building of the pilot schools in Alabama preceded the large-scale rollout of the program throughout the South. It is unlikely that migration would have responded quickly to the pilot program. We provide further evidence on selective migration in section VI.D.

VI. Results on Adult Outcomes from World War II Enlistment Records and the 1940 Census

A. Years of Schooling

Table 5 reports a variety of results using the WWII enlistment records. The first two columns examine the effects of Rosenwald exposure on completed years of schooling, with the columns differentiated by whether inverse probability weighting (IPW) corrects for nonrandom sample selection. Using the preferred IPW specification (column 2), we find that complete exposure raised Black rural schooling levels by 1.2 years and had no meaningful effect on the other three groups, including urban Blacks. Consequently, complete exposure to Rosenwald narrowed the Black-White rural difference, the Black rural-urban difference, and the triple difference by 1.2 to 1.4 years.[50] Based on these estimates, Rosenwald exposure accounted for nearly 40 percent of the narrowing of the Black-White gap in completed schooling for cohorts born between 1910 and 1925.[51]

These results are extremely similar to what we obtain when we use the 1940 Census, which does not contain potential selection problems related to military enlistment. Unfortunately, county geocodes are not currently available in the 1940 IPUMS and therefore we must rely on state economic areas

[50] As explained in section III.C, our sharper classification of rural and urban counties eliminates those with rural share greater than or equal to 50 percent but less than 100 percent. For this sample, we find complete Rosenwald exposure effects on Blacks of: 0.74 (0.15) on years of education; 0.11 (0.02) on some high school; 0.04 (0.02) on completed high school and 5.57 (1.97) on AGCT scores. In all cases, we find no significant effects for Whites. In short, the results are always statistically significant but around 50 to 60 percent of the effect sizes estimated for the Black-White rural differences, consistent with the attenuation we expect in mixed rural-urban counties.

[51] The Black-White gap for Southern born men is about 3 years for the pre-Rosenwald cohorts born between 1905 and1909 (see Figure 1) for whom E_{bc} averaged only 0.01 and closes to a gap of 1.8 years for the 1925 birth cohort for whom $E_{bc} = 0.36$. If we use Table 6's average estimated effect from full Rosenwald exposure of 1.3 years, the effect at the mean is $1.3 \times (0.36 - 0.01) = 0.46$. Therefore, Rosenwald explains about 38 percent (0.46/(3.0-1.8)) of the closing of the gap across these cohorts.

(SEAs), aggregations of contiguous counties with similar economic characteristics developed by the Census Bureau. We also cannot distinguish between rural and urban areas within an SEA. Nevertheless, by using a cohort similar in age to WWII enlistees, specifically those aged 18 to 25 in 1940, we find that complete exposure raised completed years of schooling of Blacks relative to Whites by 1.36 (0.18) years.

Our earlier findings on school attendance imply that about one-third of the gain in completed schooling can be attributed to students who would not have attended any school in the absence of the program. Specifically, in section V.A we estimated that the Rosenwald program caused approximately 90,000 additional students to attend schools during the early 1930s. If we further assume that these students gained, on average, an additional 3 years of schooling, this would imply that increases in school attendance contributed about 0.4 years to the overall increase of 1.2 to 1.4 years of completed schooling that we estimate in Table 5.[52] The remainder of the estimated increase in completed schooling, therefore, can be attributed to greater time spent in school among those students who would have attended a non-Rosenwald school even in the absence of the program. We think that this latter gain most likely reflects the sharp improvement in school quality that the Rosenwald schools represented.

B. High School Matriculation and Completion

In columns (3) and (4), we focus on the program's effect on attending and completing high school (hereafter, we only show the IPW results). The estimates suggest that full exposure to Rosenwald schools is associated with a 17.0 percentage point increase in the probability of attending some high school amongst rural Blacks. Again, we estimate no significant effects for urban and rural Whites and urban Blacks. The gain relative to urban Blacks is 13.3 percentage points and the gain relative to rural Whites is 18.6 percentage points. The triple difference estimate is 20.4 percentage points and, like all of the key estimates, highly significant. The aggregate effect of the program would be to raise high school attendance of rural Blacks by between 4.6 to 7.1 percentage points for cohorts born in 1925 compared to

[52] We use three years in our calculation, because this was the average gain in education among Southern Blacks born in 1925-29 relative to 1905-09. Our calculation is as follows: 90,000/650,000 × 3 + 560,000/650,000 × 0 = 0.42.

those born just prior to 1910. Raising high school attendance leads to a notable increase in high school graduation. Among rural Blacks, complete exposure raises the probability of high school completion by 8.3 percentage points and by 8 to 9 percentage points relative to our control groups. This translates into an aggregate increase in high school completion across cohorts of rural Blacks of about 3 percentage points.

C. AGCT Scores

Column (5) presents the results on AGCT scores. Recall that we are relegated to using a vastly smaller sample of individuals who enlisted over a short period in 1943 so estimates are less precise and could be more susceptible to selection. Nevertheless, we again find quantitatively large and in some cases statistically significant effects across our estimates of rural Blacks ranging from 5 to 10 points. For example, the triple difference estimate is 8.0 (4.0) points. Given that the standard deviation of AGCT scores for the full sample of Blacks and Whites is 23.5 points, our estimates suggest that full exposure improved Black test scores by between 0.2 to 0.45 standard deviations and, in the aggregate, would have led to an improvement of between 0.08 to 0.15 standard deviations for cohorts born in 1925 compared to those born just prior to 1910.

Since the publication of Herrnstein and Murray (1994), several studies show that environmental factors influence the Black-White test score gap (Neal and Johnson 1996, Hansen, Heckman, and Mullen 2004, Cascio and Lewis 2001, Chay, Guryan, and Mazumder 2009). Our results confirm that a very straightforward intervention, the provision of schools, has a sizable effect on test scores. Further confirming that schooling influences these scores, we also find that the Rosenwald test score effect disappears when we include educational attainment as a covariate in the regressions (column 6).

We note some concern over the surprising marginally significant *negative* effects on rural and urban Whites. However, as we discuss below, these effects are largely eliminated when we turn to the "young draftee" sample where selection is less of a concern.

D. Further robustness checks

1. Height

One potential validity check is to measure the Rosenwald effect on outcomes for which we expect additional schooling to have little, and perhaps no, influence. A good candidate is height, since effective interventions on height are believed to be confined mostly to the early life period, well before children enter school (Martorell, Schroeder, Rivera, and Kaplowitz 1995; Behrman and Hoddinott 2005). Since the Rosenwald Initiative targeted children beyond the early life period and moreover was not designed to treat childhood nutrition or health,[53] we expect Rosenwald exposure to have no impact on height. That is indeed what we report in column (7). These results do not suggest any obvious remaining confounding factor.

2. Young Draftees

As we discuss in section IV.B, there may be concern that our initial strategies of using a wide array of fixed effects and inverse probability weighting may not sufficiently remove all potential sources of selection, especially those operating at the individual level where unobserved preferences may matter. Therefore, to address this potential source of bias, we re-estimate our regressions for two outcomes, education and AGCT scores, using the sample of young draftees described earlier. The results are shown in columns (8) and (9). In short, we find the same general pattern of results as in the full sample. Complete exposure to Rosenwald raised rural Black schooling levels by over 1.5 years and narrowed the Black-White rural gap, the Black rural-urban gap, and the triple difference by between 0.8 to 1.3 years.

For test scores, the samples are considerably smaller since they include only 18-19 year old draftees who enlisted over a 10 week period in 1943 and do not include county fixed effects. We find that the simple undifferenced "Black Rural" estimate is now slightly larger at 8.4 percentage points using this specification compared to column (5) and significant at the 10 percent level. We also find that the negative effect on urban Whites is eliminated and the negative effect on rural Whites is greatly reduced.

[53] While there is no historical documentation of health initiatives in the primary or secondary Rosenwald schools, Julius Rosenwald had interest in health initiatives (Ascoli 2006). The Rosenwald school plans embraced some school hygiene issues, including lighting, ventilation, and bathrooms (see www.preservationnation.org/travel-and-sites/sites/southern-region/rosenwald-schools).

Our differenced estimates, however, indicate very similar-sized magnitudes as with our larger sample. The triple difference estimate suggests that full exposure increased test scores by 7.8 percentage points (5.7), nearly identical to the 8.0 point estimate for the full test score sample.

We also estimate the regressions (not shown) on just the subsample of 18 to 19 year old draftees who enlisted from 1943 through 1946 for whom there was virtually no scope for volunteering prior to their draft. Once more, we find similar results. For example, our four key estimates of the education effect on rural Blacks range from 0.8 to 1.5 and are highly statistically significant. Similarly for attending high school, our estimates range from 0.13 to 0.22 and for completing high school range from 0.02 to 0.07. Finally, the positive AGCT score effects already described in the previous paragraph are estimated using this sample.

3. Exposure Based on County of Birth

To further address concerns about selective migration, we reran our WWII analysis on a select subsample of WWII enlistees where a source of exposure determined prior to the time of school attendance, namely exposure based on one's county of birth, was available. The regression specification is similar to equation (2):

$$(4)\ y_{ibcq} = \alpha + black_i + \gamma_0 E_{bc} + \gamma_1 (black_i \times E_{bc}) + \beta X_c + age_i + age_i \times black_i + enlqtr_q + enlqtr_q \times black_i + county_c + \varepsilon_{ibcq}$$

but E_{bc} is computed for county of birth rather than county of enlistment and counties are not differentiated by their rural status. The results are shown in Table 6. For each outcome (years of completed education, some high school, completed high school, and AGCT score), we display a first column of results that use county of enlistment and the full sample and a second that is based on enlistment county but restricts the sample to those we know county of birth. The third column reports results that use county of birth exposure. We find statistically indistinguishable effects whether we use county of enlistment or county of birth, regardless of the outcome. Consequently, we again find no support for concern that our results are contaminated by selective migration.

E. South-to-North Migration

Although we find little evidence of parents moving to counties with higher levels of Rosenwald exposure, their children's improved human capital could offer greater opportunity to relocate to superior labor markets, which at the time chiefly meant Northern cities (e.g. Bowles 1970, Margo 1990, Card and Krueger 1992). To study this question, we use the 1940 Census, the first to ask about migration, to run the following regression:

$$(5)\ y_{i,SEA} = \alpha + female_i + black_i + female_i \times black_i + \gamma_0 E_{b,SEA} + \gamma_1 (black_i \times E_{b,SEA}) + F(age_i) + F(age_i) \times black_i + \varepsilon_{i,SEA}.$$

The outcome $y_{i,SEA}$ is either South-to-North or South-to-South migration, computed from a comparison of the individual's 1940 residential state economic area (SEA) to her residential SEA five years prior. The sample consists of residents of Rosenwald states in 1935. The regression covariates include a female indicator, a Black indicator, an indicator for Black and female, and a quadratic in age interacted with race and 1935 state of residence.

Results for both migration measures are presented by age cohort in Table 7. We find that complete Rosenwald exposure increased the likelihood of Northern migration among 17 to 21 year old Blacks relative to 17 to 21 year old Whites by 2.5 (1.2) percentage points (column 2). By contrast, we find no Black-White difference on within-South migration for this same age cohort (column 5). We also find no Black-White difference within the school-age population or within those individuals aged 22 to 30, who were likely finished with school and potentially raising their own children. Indeed, the only group of Southerners in the late 1930s that responded with their feet to higher Rosenwald exposure was Blacks finishing school, and they increased their propensity to move North by over 60 percent, from 1.4 to 2.3 percent at a Rosenwald exposure rate typical for this cohort.

While we are certainly not the first to link education to the Great Migration, we believe our results are the first to use a potentially exogenous source of educational improvement to make a causal claim about the importance of human capital to Black Northern migration at that critical time.[54]

VII. Heterogeneous Effects

A. By initial school conditions

Given the extremely poor educational conditions facing many rural Blacks prior to the Rosenwald program and the inability to secure financing for schools through existing institutional arrangements, it seems plausible that the introduction of the Rosenwald program disproportionately benefited those students with high costs of schooling and with especially high marginal rates of return. We explore this possibility by re-estimating our models using samples that are stratified by county rates of Black school attendance in the period prior to the initiation of the Rosenwald program.

The first two columns of Panel A of Table 8 report these results.[55] Regardless of which of the three differenced estimators we report, the effects are statistically and economically larger for those residing in counties where the 1910 Black attendance rate was at or below the median. Indeed, when we split the sample into quartiles (unreported), the effects monotonically decline with higher initial Black school attendance. By contrast, there is no consistent pattern when we stratify by pre-Rosenwald White school attendance rates (columns 3 and 4). These results suggest that the program's influence was largest where the opportunity for Blacks to invest in schooling was lacking and therefore where there was substantial room for progress.

In Panel B, we show comparable results for education and AGCT scores in the World War II data stratified by 1920 Black school attendance rates.[56] For years of completed education, we again see sharply higher estimates in the counties that were at or below the median. For AGCT scores, we find

[54] See also Duflo (2004) for a similar result following the Indonesian school construction program.
[55] For this exercise, we use our baseline specification from column (3) of Table 2, where the full sample effect of Rosenwald exposure on rural Blacks is 0.124, the Black rural-urban gap is 0.070, and the Black-White rural gap is 0.127. The triple difference estimate is 0.076. The coefficients are reported in Appendix Table A5.
[56] We used 1920 for stratification since over 80 percent of the WWII sample entered school in 1920 or later.

considerable effects in the bottom half of the Black school attendance distribution and no effect for those in the top half of the distribution.

B. By Black Population Share and Past Slave Share

Researchers starting with Bond (1934) argue that Blacks historically fared worse in counties where they were more numerous because Whites were successfully able to divert educational resources by taking advantage of the political exclusion of Blacks (see also Margo 1990 and Card and Krueger 1996). These conditions arose in areas where there were large populations of slaves due to agricultural conditions favoring certain crops (Fogel and Engerman 1974). The Rosenwald program might have potentially overcame this exclusionary system by providing actual physical infrastructure that could not be diverted. Therefore, we next examine how the effects of the schools varied over various direct measures that reflect these historically deprived communities.

We first split the sample based on the Black share of the 1910 county population (Panel A, columns 5 and 6). We find that the effects were significantly larger in the counties with high Black population shares. In columns 7 and 8, we stratify by the share of 1890 county land that was used to cultivate one of four labor intensive crops -- cotton, cane sugar, rice and tobacco -- using data from Chay and Munshi (2011). Here, we find larger point estimates in the counties that had high shares of plantations, though the differences are not as stark as with high Black population shares. Finally, we stratify by the slave share of the population in 1860 for counties that had at least some slaves using data from Haines (2010). Columns 9 and 10 report sharply higher effects in those counties that had the most slaves prior to the Civil War. In columns 11 and 12, we further stratify by whether a county was in the top quartile in both plantation land *and* slave share. Again, the effects are particularly large for the counties in which rural Blacks were most likely to have been historically disadvantaged.

Overall, the evidence in Table 8 strongly suggests that students schooled in the most disadvantaged communities, as measured by conditions prone to slavery or by pre-Rosenwald levels of Black schooling, benefited the most from access to the Rosenwald schools.[57]

VIII. Real Internal Rate of Return

In order to gauge the success of the program as a public policy investment, we calculate its real internal rate of return. Since this requires many simplifying assumptions, the results of this exercise should be viewed with some caution. The precise details of the calculation are provided in Appendix C. We assume that the schools were gradually phased out by 1950[58] and therefore only cohorts of rural Blacks born in Rosenwald states between 1906 through 1942 received benefits from the program. The size of each cohort is estimated from the 1910 to 1950 Censuses.

We calculate the stream of yearly benefits over the 1926 to 2002 period by aggregating the labor earnings experienced by all cohorts in that year. For each cohort, we estimate the implied effects of the program on completed years of schooling based on their childhood exposure to the schools and our Table 5 estimates. We assume that the return to a year of education was either 5 or 7 percent and that these returns were earned when individuals were between the ages of 20 and 60.[59] The typical lifecycle earnings profile to which we apply these returns is computed for Southern–born Blacks from the 1940 to 1980 Censuses. We calculate the stream of costs incurred between 1913 and 1950 using the total costs of Rosenwald school construction and our estimates of teacher salaries, other school maintenance costs, and the foregone contemporaneous earnings that Rosenwald students would have received during the

[57] We have also run the results separately by age and gender. There appears to be some evidence, though not uniformly consistent, that the impact of Rosenwald is larger for older students. This could reflect the possibility that the effects have a cumulative effect over time. We find that the program's effects are statistically similar for males and females.

[58] Since we have no information about the timing of school closures, we assume that the buildings were used for about 20 years. There are some anecdotes of schools that remained open well into the 1950s.

[59] The estimated return to a year of education for Southern Blacks in the 1940 and 1950 Census is 5 percent. We suspect that the returns may have been even larger for those exposed to Rosenwald schools. This is based on an analysis of the effects of the program on the earnings of Blacks who remained in the South in the 1940 and 1950 Census in an earlier draft of this paper where estimated returns were in excess of 10 percent.

additional 1.2 years that they were in school. Earnings and costs are deflated to 1925 dollars using the Consumer Price Index.

We estimate that the real internal rate of return was between 7 and 9 percent. By way of comparison, the average real yield on 8 to 12 year US treasury bonds was 5 percent during the 1919 to 1932 period.[60] Moreover, this calculation does not include other potential benefits such as improvements in family planning (Aaronson, Lange, and Mazumder 2011), health or intergenerational linkages.[61]

IX. Conclusions

At the turn of the twentieth century, the education infrastructure available to American Southern Blacks, particularly those living in rural areas, resembled the conditions faced by some rural communities in developing nations today. For example, rates of literacy and school enrollment in some developing countries are as low or even lower today than they were among rural Blacks in 1910. But over a moderately short period between the World Wars, the Southern racial education gap declined markedly. While no single explanation likely accounts for this rapid convergence, we show that the Rosenwald Rural Schools Initiative is a significant contributor, explaining about 40 percent of the narrowing of the racial education gap among the cohorts that we study. Moreover, the program stimulated migration to better labor market opportunities in the North. In sum, the Rosenwald Initiative highlights the large productivity gains that can arise when substantial improvements to school quality and access are introduced to relatively deprived environments. This conclusion is accentuated by the especially large gains measured in communities that were contending with the worst pre-Rosenwald educational conditions.

[60] While the Rosenwald Initiative ended in 1932, many schools remained open well beyond. Therefore, a more appropriate comparison could be to a longer period. For example, the average real yield between 1919 and 1939, still well before WWII interest rate caps were in place, is under 3 percent. We take nominal yields (8 year U.S. bonds from 1919 to 1925 and 12 year U.S. bonds thereafter) from the National Bureau of Economic Research's macro database (series 13033). Inflation expectations do not exist for this period. Instead, we use the average 8 or 12 year realization of future inflation rates, computed from the NBER macro database's Consumer Price Index (series m04128).

[61] Aaronson and Mazumder (2008) estimate the intergenerational income elasticity to be roughly 0.4 to 0.5 for the *children* of Rosenwald era students (including White and Black, North and South, born between 1930 and 1950).

Left somewhat unresolved are the channels by which the Rosenwald program improved student outcomes. We find significant gains in school attendance suggesting progress along the quantity of schools margin. Yet we speculate that the bulk of the gains in human capital may be attributable to improvements in the quality of the schools that rural Black students attended. However, systematic data do not exist for many of the relevant dimensions of school quality (e.g. teacher quality, curriculum, physical infrastructure). Further, traditional measures such as school spending may not adequately capture school quality, especially in light of the persistent institutional inequities that accompanied segregated schools.

More generally, the gains in human capital acquisition due to school improvements likely had implications for economic development in the 20th century South, as well as for the US economy in general. While beyond the scope of the current paper, we view this link as an important future research question and the Rosenwald program as a useful contributor towards understanding the causal relationship between human capital acquisition and economic progress.

In assessing the overall importance of the Rosenwald schools, one may reasonably argue that racial convergence in educational standards with the North was inevitable. Indeed, by the time of *Brown vs. Board of Education* in 1954, common measures of Black-White educational resource gaps had mostly been closed (Card and Krueger 1992, Donohue, Heckman, and Todd 2002). Yet many observers -- notably Booker T. Washington, but modern researchers such as Margo (1990) and Donohue, Heckman, and Todd (2002) as well -- point to the fundamental funding inequities driven partly by institutional discrimination, and likely exacerbated by liquidity constraints, to argue that major investments in Black schools required outside intervention in the early part of the 20th century. The racial convergence that occurs in relatively short-order after the introduction of the Rosenwald program seems to validate the view that some prodding was necessary. Subsequent progress in Black educational attainment and cognitive skill development has likewise been significantly aided by a series of private and public interventions, including but not limited to NAACP litigation (Donohue, Heckman, and Todd 2002),

desegregation of schools and hospitals (Welch and Light 1987, Guryan 2004, Chay, Guryan, and Mazumder 2009), and civil rights legislation (Donahue and Heckman 1991).

Our results may also inform the historical literature concerning the nature of Black economic progress in the first half of the 20th century. Margo (1990) presents a framework in which a combination of human capital (supply side), institutional factors (demand side), and "intergenerational drag" all played a role in keeping the relative earnings of adult Black men flat from 1900 to 1940. We show that an expansive schooling intervention in the South did in fact have a sizable effect on the level of human capital of Blacks born after 1910. Our study suggests that the relative importance of the supply of human capital may have played a more consequential role than previously thought in accounting for early 20th century Black economic progress.

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Table 1: School Attendance Effects of Rosenwald School Presence in County

	(1)	(2)	(3)	(4)
γ_0	0.011	0.014	0.001	0.010
	[0.007]	[0.006]**	[0.007]	[0.007]
γ_1	0.024	0.017	0.034	0.022
	[0.010]**	[0.008]**	[0.009]***	[0.008]***
γ_2	-0.013	-0.012	0.004	-0.001
	[0.007]*	[0.006]**	[0.006]	[0.005]
γ_3	0.067	0.047	0.055	0.041
	[0.011]***	[0.010]***	[0.010]***	[0.010]***
	Differences (Rose minus no Rose)			
Black Rural	0.089	0.066	0.094	0.072
($\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3$)	[0.007]***	[0.007]***	[0.007]***	[0.007]***
White Rural	-0.002	0.002	0.005	0.008
($\gamma_0 + \gamma_2$)	[0.004]	[0.004]	[0.006]	[0.005]
Black Urban	0.034	0.031	0.036	0.032
($\gamma_0 + \gamma_1$)	[0.008]***	[0.008]***	[0.009]***	[0.009]***
White Urban	0.011	0.014	0.001	0.010
(γ_0)	[0.007]	[0.006]**	[0.007]	[0.007]
	Difference In Difference			
Black, Rural-Urban	0.054	0.035	0.059	0.040
($\gamma_2 + \gamma_3$)	[0.009]***	[0.009]***	[0.009]***	[0.009]***
White, Rural-Urban	-0.013	-0.012	0.004	-0.001
(γ_2)	[0.007]*	[0.006]**	[0.006]	[0.005]
Black-White Rural	0.091	0.065	0.089	0.063
($\gamma_1 + \gamma_3$)	[0.006]***	[0.006]***	[0.006]***	[0.006]***
Black-White Urban	0.024	0.017	0.034	0.022
(γ_1)	[0.010]**	[0.008]**	[0.009]***	[0.008]***
	Triple Difference			
B-W Rural - B-W Urban	0.067	0.047	0.055	0.041
(γ_3)	[0.011]***	[0.010]***	[0.010]***	[0.010]***
Controls	**N**	**Y**	**N**	**Y**
County Fixed Effects	**N**	**N**	**Y**	**Y**
N	650167	650167	650167	650167

Notes: Samples include children between the ages of 7 and 17 in the 1900, 1910, 1920 and 1930 IPUMs. Dependent variable is school attendance. Columns 1 and 3 only include year dummies. The controls in columns 2 and 4 include year dummies, age, female dummy, father's and mother's literacy, father's occupational score and father's home ownership. Column 2 also includes state fixed effects and county White literacy rate in 1910. Estimates use Census sampling weights. Standard errors clustered on county are shown in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2: School Attendance Effects of Rosenwald Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
γ_0	0.005	-0.002	0.002	0.138	0.147	0.137
	[0.010]	[0.012]	[0.015]	[0.021]***	[0.028]***	[0.028]***
γ_1	0.045	0.055	0.052	0.05	0.029	0.030
	[0.013]***	[0.013]***	[0.013]***	[0.013]***	[0.077]	[0.075]
γ_2	-0.025	-0.016	-0.006	-0.008	0.006	-0.001
	[0.009]***	[0.009]*	[0.011]	[0.009]	[0.033]	[0.033]
γ_3	0.093	0.081	0.076	0.065	0.057	0.043
	[0.016]***	[0.016]***	[0.017]***	[0.016]***	[0.089]	[0.088]
	Difference (Effect of Complete Exposure)					
Black Rural	0.119	0.119	0.124	0.245	0.239	0.209
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.012]***	[0.014]***	[0.014]***	[0.022]***	[0.042]***	[0.042]***
White Rural	-0.020	-0.017	-0.003	0.13	0.153	0.136
$(\gamma_0 + \gamma_2)$	[0.006]***	[0.008]**	[0.009]	[0.019]***	[0.018]***	[0.018]***
Black Urban	0.051	0.053	0.054	0.188	0.177	0.166
$(\gamma_0 + \gamma_1)$	[0.016]***	[0.019]***	[0.022]**	[0.024]***	[0.071]**	[0.070]**
White Urban	0.005	-0.002	0.002	0.138	0.147	0.137
(γ_0)	[0.010]	[0.012]	[0.015]	[0.021]***	[0.028]***	[0.028]***
	Difference In Difference					
Black, Rural-Urban	0.069	0.066	0.070	0.056	0.062	0.042
$(\gamma_2 + \gamma_3)$	[0.017]***	[0.018]***	[0.021]***	[0.015]***	[0.083]	[0.082]
White, Rural-Urban	-0.025	-0.016	-0.006	-0.008	0.006	-0.001
(γ_2)	[0.009]***	[0.009]*	[0.011]	[0.009]	[0.033]	[0.033]
Black-White Rural	0.139	0.136	0.127	0.115	0.086	0.073
$(\gamma_1 + \gamma_3)$	[0.011]***	[0.012]***	[0.012]***	[0.011]***	[0.045]*	[0.046]
Black-White Urban	0.045	0.055	0.052	0.050	0.029	0.030
(γ_1)	[0.013]***	[0.013]***	[0.013]***	[0.013]***	[0.077]	[0.075]
	Triple Difference					
B-W Rural - B-W Urban	0.093	0.081	0.076	0.065	0.057	0.043
(γ_3)	[0.016]***	[0.016]***	[0.017]***	[0.016]***	[0.089]	[0.088]
Baseline Controls	**Y**	**Y**	**Y**	**Y**	**N**	**N**
Age-St.-Yr	**N**	**N**	**Y**	**Y**	**N**	**N**
County F.E	**N**	**Y**	**Y**	**N**	**N**	**N**
County by Year F.E.	**N**	**N**	**N**	**Y**	**N**	**N**
Family F.E	**N**	**N**	**N**	**N**	**Y**	**Y**
Birth Order	**N**	**N**	**N**	**N**	**N**	**Y**
N	643284	643284	643284	643284	482346	482346

Notes: Samples include children between the ages of 7 and 17 in the 1910, 1920 and 1930 IPUMs. Dependent variable is school attendance. Estimates show the effect of complete exposure (exposure = 1) to Rosenwald schools between the ages of 7 and 13 relative to no exposure (exposure=0). The controls include year dummies, age, female dummy, father's and mother's literacy, county White literacy rate in 1910 (column 1 only), father's occupational score and father's home ownership and state dummies (column 1 only). Estimates use Census sampling weights. Standard errors clustered on county are shown in brackets except for columns 5 and 6 which cluster on families.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Literacy Effects of Rosenwald School Presence in County, or Rosenwald School Exposure

	(1)	(2)	(3)	(4)
	Rosenwald Presence		Rosenwald Exposure	
γ_0	-0.030	-0.018	-0.058	-0.051
	[0.005]***	[0.005]***	[0.009]***	[0.011]***
γ_1	0.052	0.039	0.086	0.083
	[0.008]***	[0.007]***	[0.017]***	[0.016]***
γ_2	0.020	0.018	0.029	0.012
	[0.005]***	[0.004]***	[0.008]***	[0.008]
γ_3	0.064	0.053	0.182	0.165
	[0.010]***	[0.009]***	[0.022]***	[0.020]***
	Differences (Rose minus no Rose)		Difference (Effect of Exposure)	
Black Rural	0.106	0.093	0.239	0.209
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.007]***	[0.006]***	[0.017]***	[0.015]***
White Rural	-0.010	0.000	-0.029	-0.039
$(\gamma_0 + \gamma_2)$	[0.003]***	[0.003]	[0.008]***	[0.008]***
Black Urban	0.022	0.021	0.028	0.032
$(\gamma_0 + \gamma_1)$	[0.008]***	[0.007]***	[0.014]*	[0.011]***
White Urban	-0.03	-0.018	-0.058	-0.051
(γ_0)	[0.005]***	[0.005]***	[0.009]***	[0.011]***
	Difference In Difference		Difference In Difference	
Black, Rural-Urban	0.084	0.071	0.211	0.177
$(\gamma_2 + \gamma_3)$	[0.010]***	[0.008]***	[0.020]***	[0.017]***
White, Rural-Urban	0.020	0.018	0.029	0.012
(γ_2)	[0.005]***	[0.004]***	[0.008]***	[0.008]
Black-White Rural	0.116	0.092	0.268	0.248
$(\gamma_1 + \gamma_3)$	[0.006]***	[0.005]***	[0.018]***	[0.016]***
Black-White Urban	0.052	0.039	0.086	0.083
(γ_1)	[0.008]***	[0.007]***	[0.017]***	[0.016]***
	Triple Difference		Triple Difference	
B-W Rural - B-W Urban	0.064	0.053	0.182	0.165
(γ_3)	[0.010]***	[0.009]***	[0.022]***	[0.020]***
Controls	**N**	**Y**	**Y**	**Y**
County F.E.	**N**	**Y**	**Y**	**N**
County by Year F.E.	**N**	**N**	**N**	**Y**
N	431976	431976	425115	425115

Notes: Samples includes individuals between the ages of 15 and 22 in the 1900, 1910, 1920 and 1930 IPUMs. Dependent variable is literacy. Estimates in columns 1 and 2 show the effect of the presence of a Rosenwald school in one's county as of the Census year. Estimates in columns 3 and 4 show the effect of complete exposure (exposure = 1) to Rosenwald schools between the ages of 7 and 13 relative to no exposure (exposure=0). The sample sizes are lower in columns 3 and 4 because there are a few Rosenwald counties for which we cannot calculate exposure. The controls include year dummies, age, female dummy, father's and mother's literacy, father's occupational score and father's home ownership. Specifications without county fixed effects also include state fixed effects and county White literacy rate in 1910. Estimates use Census sampling weights. Standard errors clustered on county are shown in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: School Attendance Effects of Rosenwald County Presence Using Alabama Borders

Panel A: 7 to 17 year olds

	(1) AL/GA Border	(2) AL/FL Border	(3) AL/MS Border	(4) AL/TN Border	(5) All Borders
γ_0	0.044	0.096	-0.044	-0.023	0.012
(White)	[0.034]	[0.085]	[0.034]	[0.041]	[0.024]
γ_1	0.096	0.031	0.032	0.100	0.051
(Black-White)	[0.050]*	[0.065]	[0.056]	[0.100]	[0.032]
N	4878	1505	4188	2391	12438

Panel B: 9 to 17 year olds

	(1) AL/GA Border	(2) AL/FL Border	(3) AL/MS Border	(4) AL/TN Border	(5) All Borders
γ_0	0.053	0.088	-0.055	-0.040	0.007
(White)	[0.037]	[0.069]	[0.027]*	[0.051]	[0.025]
γ_1	0.117	-0.014	0.070	0.105	0.076
(Black-White)	[0.042]***	[0.106]	[0.059]	[0.126]	[0.035]**
N	3905	1194	3340	1909	9929

Notes: The sample uses 7 to 17 year olds or 9 to 17 year olds in the 1900, 1910 and 1920 Censuses living in any of the counties on either side of Alabama's borders to estimate the effect of the presence of a Rosenwald school in an Alabama county by 1920 on White school attendance (γ_0) and the Black-White difference in school attendance (γ_1) by specific border and for all borders pooled together. Note that certain Alabama counties may appear in more than one border comparison. For this reason, the sample in column 5 is not equal to the sum of the samples in columns 1 to 4. The controls include year dummies, age, female dummy, father's and mother's literacy, father's occupational score, father's home ownership and county fixed effects. Regressions also control for the presence of a Rosenwald school in non-Alabama counties interacted with being black. Estimates use Census sampling weights. Standard errors clustered on county are shown in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Effects of Rosenwald Exposure on Outcomes in World War II Data

	All ages and years, Volunteers and Draftees							*"Young Draftees"*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Education		Some H.S.	Complete H.S.	AGCT scores	AGCT incl. Ed.	Height	Education	AGCT scores
γ_0	0.061	0.048	0.056	0.003	-2.275	-1.769	-0.027	1.077	0.025
	[0.131]	[0.119]	[0.019]	[0.018]	[1.198]*	[1.054]*	[0.139]	[0.635]	[1.372]
γ_1	-0.017	-0.131	-0.017	-0.007	2.008	-0.328	0.033	-0.271	2.775
	[0.298]	[0.256]	[0.043]	[0.028]	[1.971]	[2.796]	[0.110]	[0.312]	[3.134]
γ_2	-0.146	-0.100	-0.071	-0.004	-2.867	-0.010	-0.051	-0.561	-2.258
	[0.164]	[0.157]	[0.024]	[0.022]	[3.170]	[2.512]	[0.177]	[0.831]	[1.831]
γ_3	1.186	1.377	0.204	0.090	8.033	-1.986	-0.191	1.335	7.832
	[0.367]***	[0.339]***	[0.056]***	[0.036]***	[4.006]**	[3.941]	[0.175]	[0.411]***	[5.714]
	Difference (Effect of Complete Exposure)								
Black Rural	1.084	1.193	0.171	0.083	4.899	-4.094	-0.235	1.580	8.374
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.232]***	[0.228]***	[0.039]***	[0.024]***	[4.156]	[3.352]	[0.155]	[0.600]***	[4.615]*
White Rural	-0.085	-0.053	-0.015	0.000	-5.142	-1.779	-0.078	0.516	-2.232
$(\gamma_0 + \gamma_2)$	[0.097]	[0.102]	[0.015]	[0.014]	[2.935]*	[2.282]	[0.109]	[0.535]	[1.213]*
Black Urban	0.044	-0.083	0.038	-0.003	-0.267	-2.097	0.007	0.806	2.800
$(\gamma_0 + \gamma_1)$	[0.279]	[0.244]	[0.043]	[0.024]	[2.243]	[2.549]	[0.158]	[0.692]	[2.744]
White Urban	0.061	0.048	0.056	0.003	-2.275	-1.769	-0.027	1.077	0.025
(γ_0)	[0.131]	[0.119]	[0.019]	[0.018]	[1.198]*	[1.054]*	[0.139]	[0.635]	[1.372]
	Difference In Difference								
Black, Rur-Urb	1.040	1.276	0.133	0.086	5.166	-1.996	-0.242	0.774	5.574
$(\gamma_2 + \gamma_3)$	[0.362]***	[0.334]***	[0.058]***	[0.034]***	[4.723]	[4.230]	[0.222]	[0.915]	[5.369]
White, Rur-Urb	-0.146	-0.100	-0.071	-0.004	-2.867	-0.010	-0.051	-0.561	-2.258
(γ_2)	[0.164]	[0.157]	[0.024]	[0.022]	[3.170]	[2.512]	[0.177]	[0.831]	[1.831]
B-W Rural	1.169	1.246	0.186	0.083	10.041	-2.314	-0.158	1.064	10.606
$(\gamma_1 + \gamma_3)$	[0.215]***	[0.221]***	[0.037]***	[0.022]***	[3.487]***	[2.747]	[0.137]	[0.267]***	[4.779]**
B-W Urban	-0.017	-0.131	-0.017	-0.007	2.008	-0.328	0.033	-0.271	2.775
(γ_1)	[0.298]	[0.256]	[0.043]	[0.028]	[1.971]	[2.796]	[0.110]	[0.312]	[3.134]
	Triple Difference								
BW Rur - BW Urb	1.186	1.377	0.204	0.090	8.033	-1.986	-0.191	1.335	7.832
(γ_3)	[0.367]***	[0.339]***	[0.056]***	[0.036]***	[4.006]**	[3.941]	[0.175]	[0.411]***	[5.714]
County F.E	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**N**
Inverse Prob. Wts	**N**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**
N	980020	980020	980020	980020	50239	50239	464698	196930	18693

Notes: Sample is drawn from World War II enlistment records and includes men born between 1910 and 1928 who enlisted between 1940 and 1946 and who lived in either entirely rural or predominantly urban counties based on the 1910-1930 Census (see text for details). Estimates show the effect of complete exposure (exposure = 1) to Rosenwald schools between the ages of 7 and 13 relative to no exposure (exposure=0). The controls include quarter of enlistment dummies interacted with race (except for columns 5, 6 and 9), age dummies interacted with race, and county fixed effects. Columns 2 through 9 use the inverse of the probability of being in the military by race, county and year of birth. Standard errors clustered by county are shown in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Effects of Using Rosenwald Exposure Based on County of Birth

	(1)	(2)	(3)	(4)	(5)	(6)
	Education			Some High School		
γ_0	-0.033	-0.337	-0.466	0.016	0.006	-0.071
(White)	[0.052]	[0.281]	[0.355]	[0.008]*	[0.045]	[0.059]
γ_1	0.534	0.951	0.668	0.071	0.145	0.094
(Black-White)	[0.188]***	[0.260]***	[0.209]***	[0.027]***	[0.042]***	[0.032]***
County Exp. Measure	Residence	Residence	**Birth**	Residence	Residence	**Birth**
Sample type	Full	Restricted	Restricted	Full	Restricted	Restricted
N	2035356	17550	17371	2035356	17550	17371

	(7)	(8)	(9)	(10)	(11)	(12)
	Completed High School			AGCT Score		
γ_0	-0.001	-0.018	-0.038	-0.223	-2.110	1.702
(White)	[0.009]	[0.040]	[0.051]	[0.738]	[7.339]	[7.949]
γ_1	0.031	0.069	0.089	4.91	7.448	4.991
(Black-White)	[0.019]*	[0.036]*	[0.031]***	[1.653]***	[3.619]**	[3.408]
Cty. Exp. Measure	Residence	Residence	**Birth**	Residence	Residence	**Birth**
Sample type	Full	Restricted	Restricted	Full	Restricted	Restricted
N	2035356	17550	17371	92971	4129	4071

Notes: Sample uses the World War II enlistment records and includes men born between 1910 and 1928 who enlisted between 1940 and 1946. The "restricted" samples use only a subset of men who were residing in Rosenwald counties who also could be matched to SSA death records, and who provided SSA a place of birth that is easily matched to a county.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Effects of Rosenwald Exposure on Migration (1940 Census)

	(1)	(2)	(3)	(4)	(5)	(6)
	South to North Migration			South to South Migration		
	Age in 1940			Age in 1940		
	8 to 16	17 to 21	22 to 30	8 to 16	17 to 21	22 to 30
γ_0	0.005	0.000	0.009	0.013	0.014	0.002
(White)	[0.006]	[0.007]	[0.010]	[0.013]	[0.015]	[0.011]
γ_1	0.004	0.025	-0.014	0.008	0.000	0.011
(Black-White)	[0.007]	[0.012]**	[0.013]	[0.018]	[0.019]	[0.025]
N	68044	35750	54521	68044	35750	54521
Mean for Blacks	0.008	0.014	0.022	0.035	0.052	0.070

Notes: The sample uses individuals between the ages of 8 and 30 in the 1940 IPUMS who lived in a Rosenwald state in 1935. Controls in Panel B include a female dummy, female*Black dummy, a quadratic in age interacted with state interacted with race. Standard errors clustered on state economic area are shown in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Results Based on Stratified Samples

Panel A: Census School Attendance Results Stratified by…

	1910 Black School Attendance		*1910 White School Attendance*		*1910 Black Population Share*		*1890 Plantation Share of Land*		*1860 Slave Share (if >0)*	
	at/below Median	above Median	at/below Median	above Median	at/below Median	above Median	at/below Median	above Median	at/below Median	above Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black, Rur-Urb ($\gamma_2 + \gamma_3$)	0.122 [0.025]***	0.013 [0.019]	0.091 [0.032]***	0.058 [0.019]***	0.014 [0.027]	0.090 [0.025]***	0.042 [0.029]	0.086 [0.027]***	0.019 [0.025]	0.084 [0.021]***
B-W Rural ($\gamma_1 + \gamma_3$)	0.207 [0.019]***	0.058 [0.015]***	0.119 [0.016]***	0.148 [0.016]***	0.067 [0.022]***	0.138 [0.013]***	0.108 [0.018]***	0.130 [0.015]***	0.063 [0.021]***	0.147 [0.015]***
B-W Rural - B-W Urban (γ_3)	0.146 [0.028]***	0.015 [0.020]	0.072 [0.025]***	0.087 [0.021]***	0.027 [0.028]	0.081 [0.020]***	0.063 [0.029]**	0.084 [0.021]***	0.044 [0.028]	0.078 [0.023]***
N	255887	296191	337151	296440	281651	352418	203511	333198	226559	288338

	Top quart. Slave Share AND Plantation	*excluding those in* (11)	*Age*			*Sex*	
			7 to 10	11 to 13	14 to 17	Male	Female
	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Black, Rur-Urb ($\gamma_2 + \gamma_3$)	0.082 [0.042]*	0.048 [0.020]**	0.073 [0.017]***	0.123 [0.018]***	0.045 [0.030]	0.060 [0.026]**	0.077 [0.020]***
B-W Rur. ($\gamma_1 + \gamma_3$)	0.224 [0.037]***	0.096 [0.015]***	0.096 [0.013]***	0.129 [0.014]***	0.176 [0.019]***	0.126 [0.014]***	0.129 [0.013]***
B-W Rural - B-W Urban (γ_3)	0.104 [0.061]*	0.061 [0.019]***	0.047 [0.019]**	0.102 [0.019]***	0.141 [0.033]***	0.075 [0.022]***	0.075 [0.019]***
N	83940	398919	250214	170990	222059	324141	319122

Panel B: World War II Education and AGCT Results Stratified by 1920 Black School Attendance…

	Education		AGCT Scores	
	at/below Median	above Median	at/below Median	above Median
	(1)	(2)	(3)	(4)
Black, Rur-Urb ($\gamma_2 + \gamma_3$)	1.567 [0.498]***	0.767 [0.423]**	15.499 [8.598]*	-10.577 [6.44]*
B-W Rur. ($\gamma_1 + \gamma_3$)	1.584 [0.336]***	0.953 [0.314]***	20.889 [6.137]***	-0.751 [4.163]
B-W Rural - B-W Urban (γ_3)	1.997 [0.552]***	0.894 [0.394]**	22.677 [7.341]***	-3.073 [4.596]
N	306693	521279	15053	27983

Notes: For Panel A the samples include children between the ages of 7 and 17 in the 1900, 1910, 1920 and 1930 IPUMs. The dependent variable is school attendance. Sample sizes vary across columns due to different rates of non-missing values of the variable used for stratification. The specification corresponds to column (3) in Table 2. Estimates use Census sampling weights. For Panel B, the samples are drawn from World War II enlistment records and includes men born between 1910 and 1928 who enlisted between 1940 and 1946 who lived in either entirely rural or predominantly urban counties (see text for details) based on the 1910-1930 Census. The specification corresponds to that shown in Table 6 and includes inverse probability weights of being in the military by race, county and year of birth. Standard errors clustered on county are shown in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

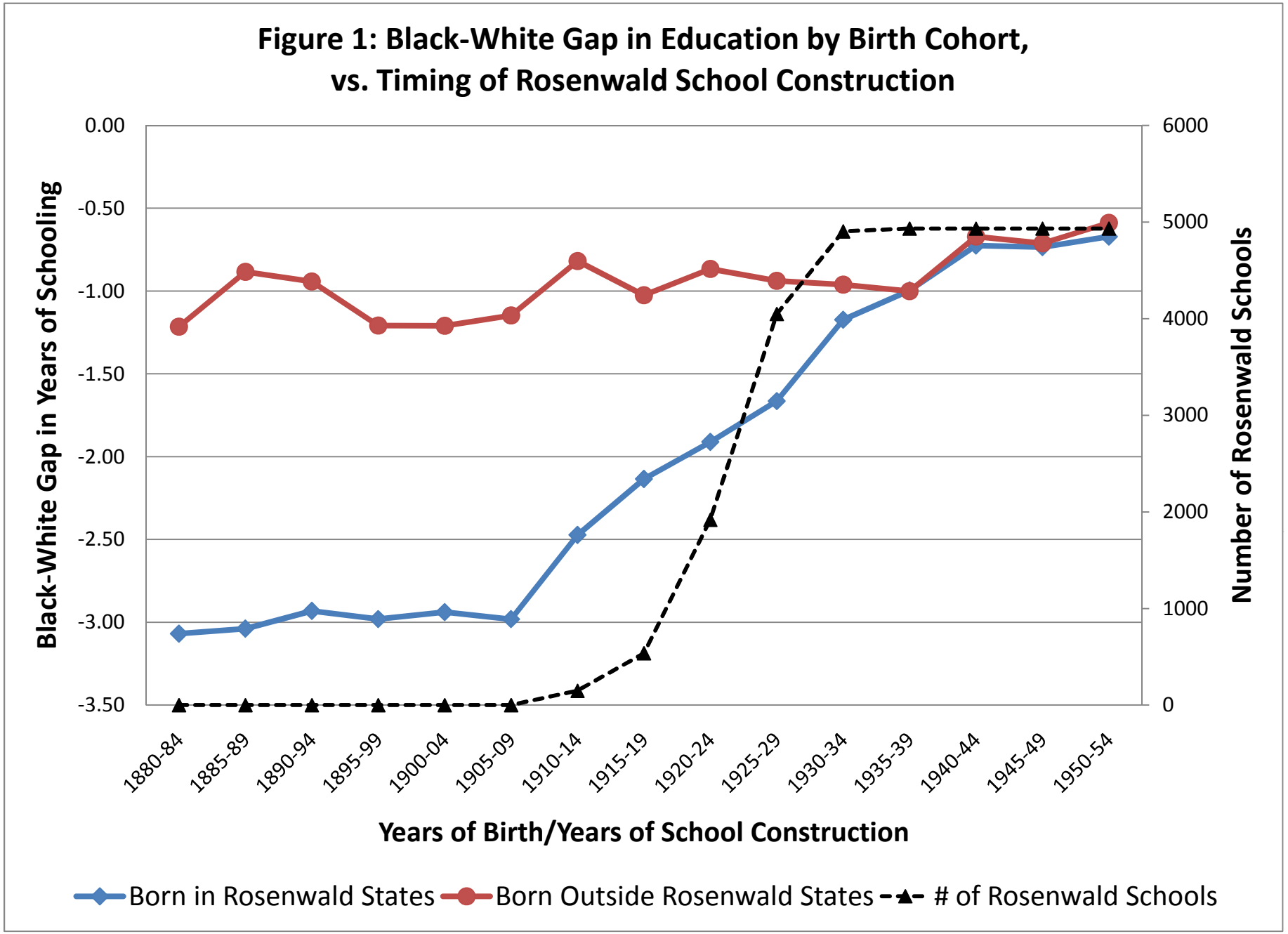


Figure 1: Black-White Gap in Education by Birth Cohort, vs. Timing of Rosenwald School Construction
Black-White Gap in Years of Schooling
0.00
-0.50
-1.00
-1.50
-2.00
-2.50
-3.00
-3.50
Number of Rosenwald Schools
6000
5000
4000
3000
2000
1000
0
1880-84
1885-89
1890-94
1895-99
1900-04
1905-09
1910-14
1915-19
1920-24
1925-29
1930-34
1935-39
1940-44
1945-49
1950-54
Years of Birth/Years of School Construction
Born in Rosenwald States
Born Outside Rosenwald States
# of Rosenwald Schools

Figure 2a: Number of Rosenwald Schools, as of 1920

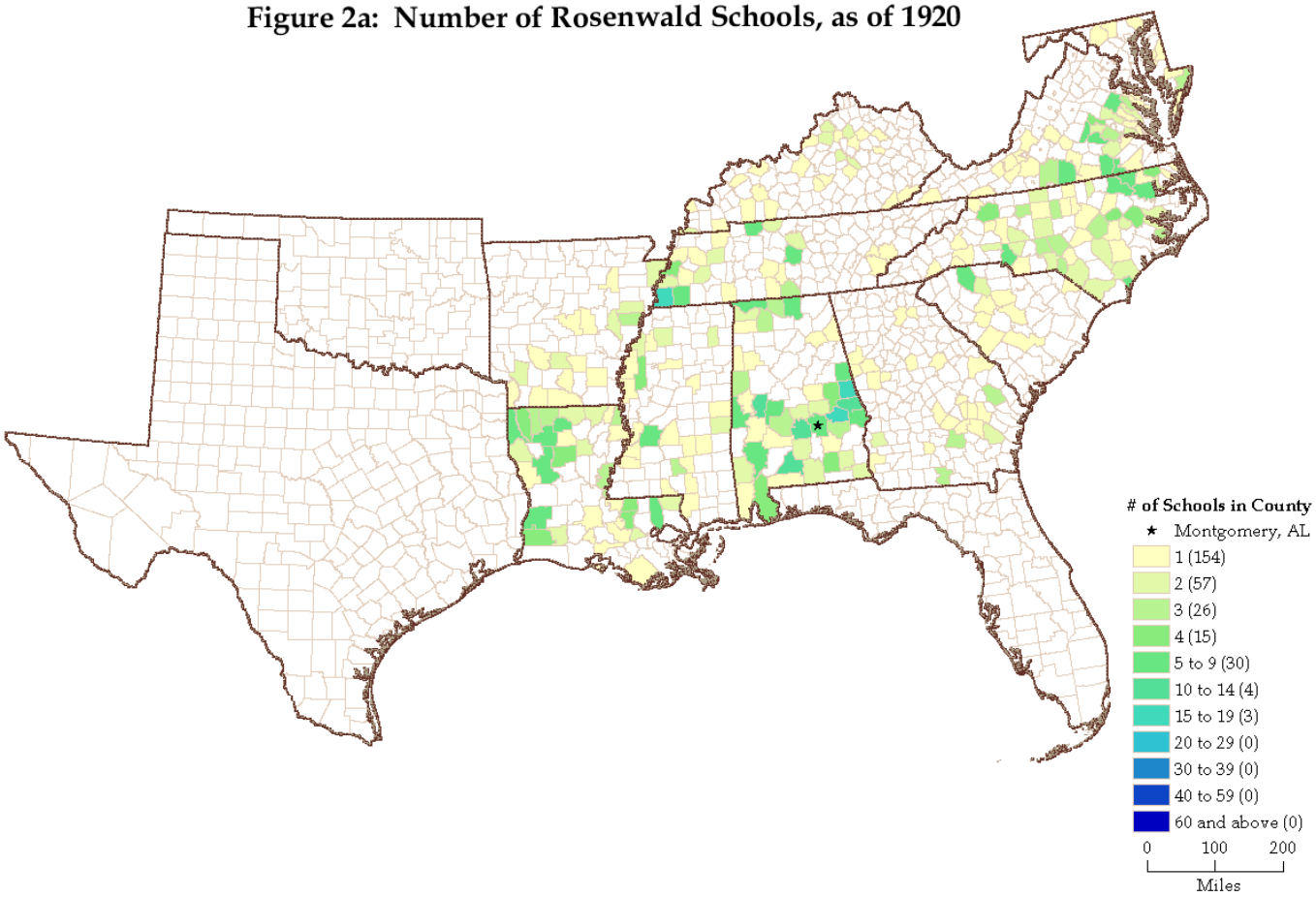


Figure 2b: Number of Rosenwald Schools, as of 1925

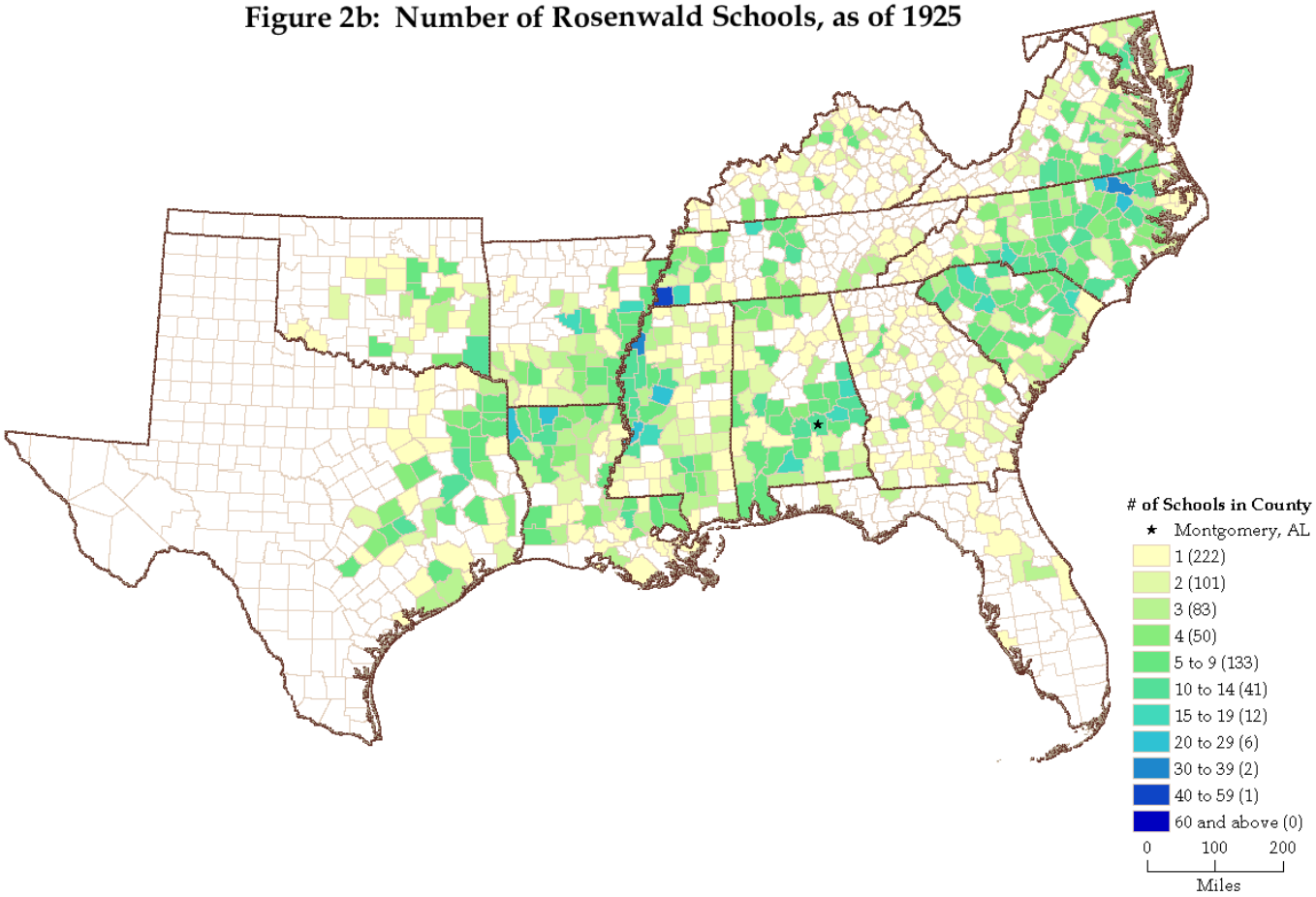


Figure 2c: Number of Rosenwald Schools, as of 1932

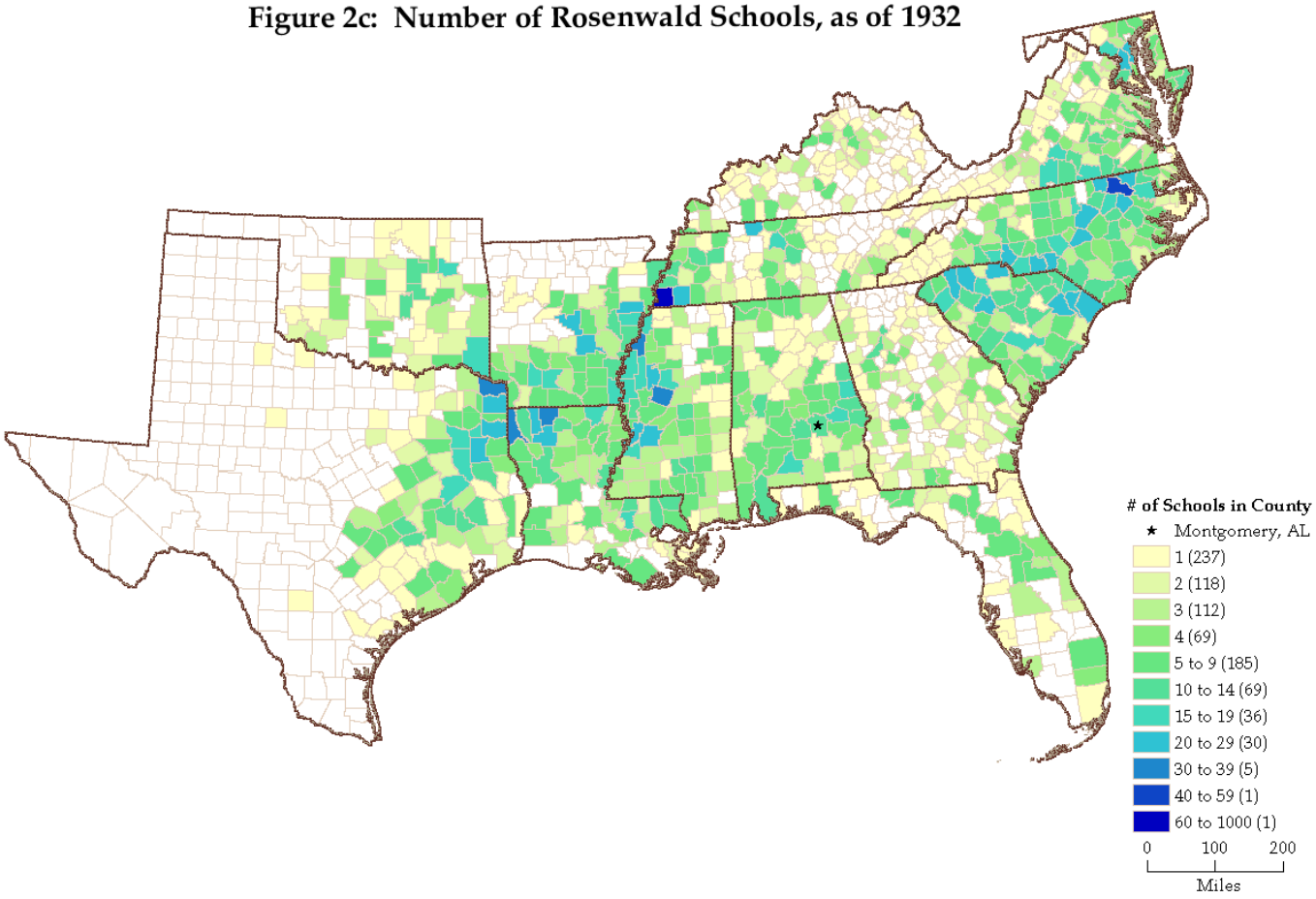


Figure 2d: Share of Black Rural School Age Children in Rosenwald Schools, as of 1932

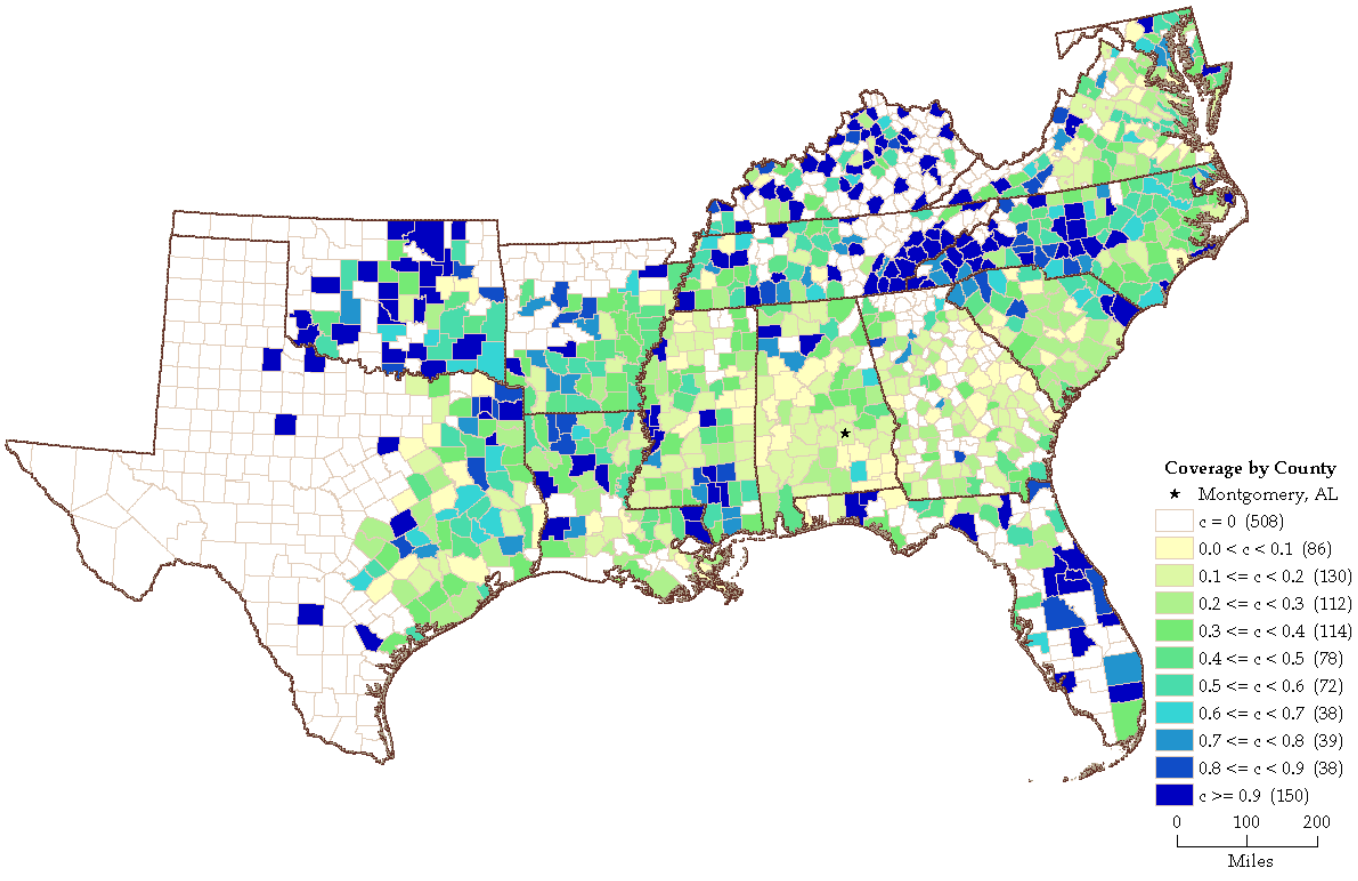
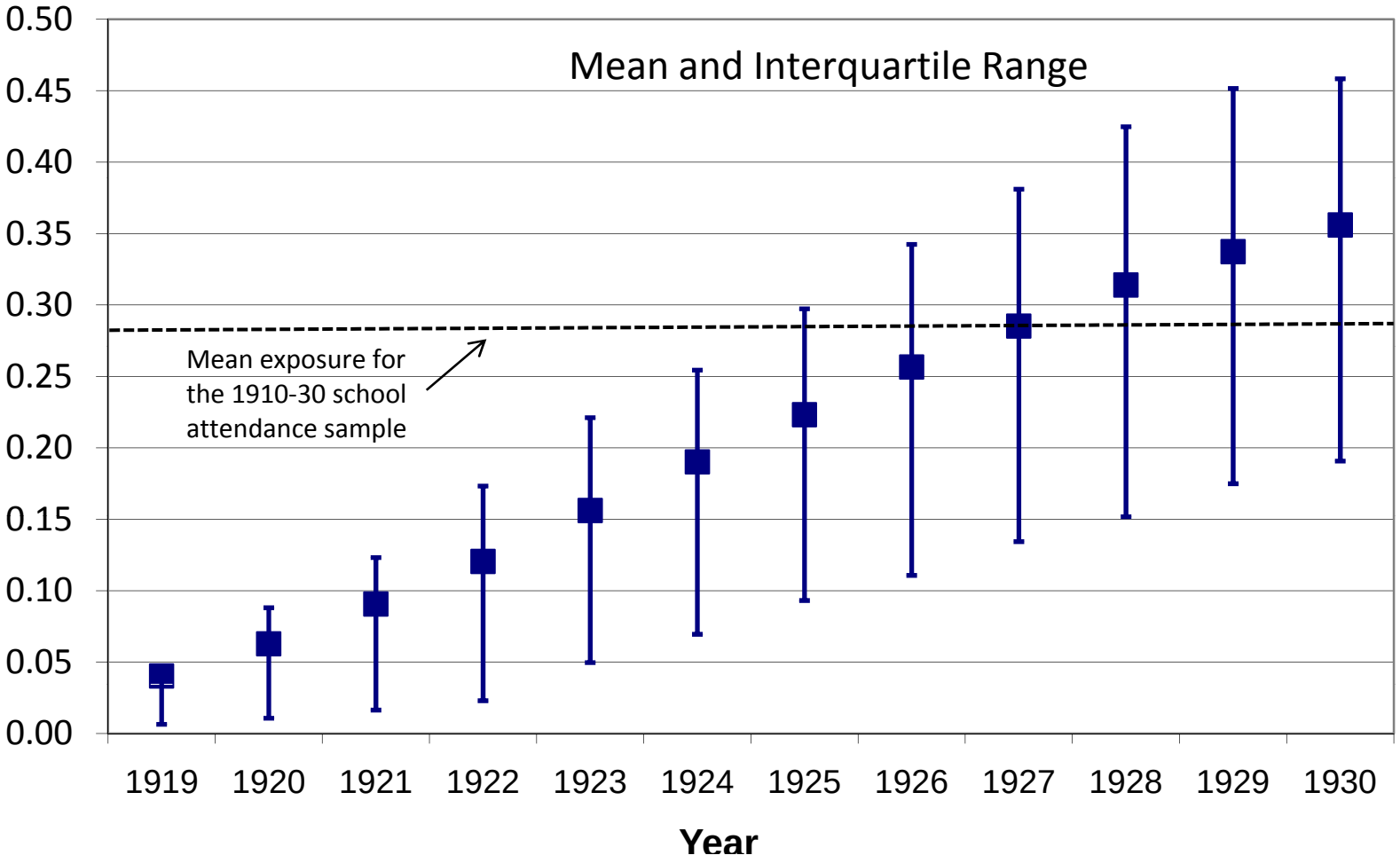


Figure 3: Distribution of Rosenwald Share of Rural Black School Age Children Across Counties



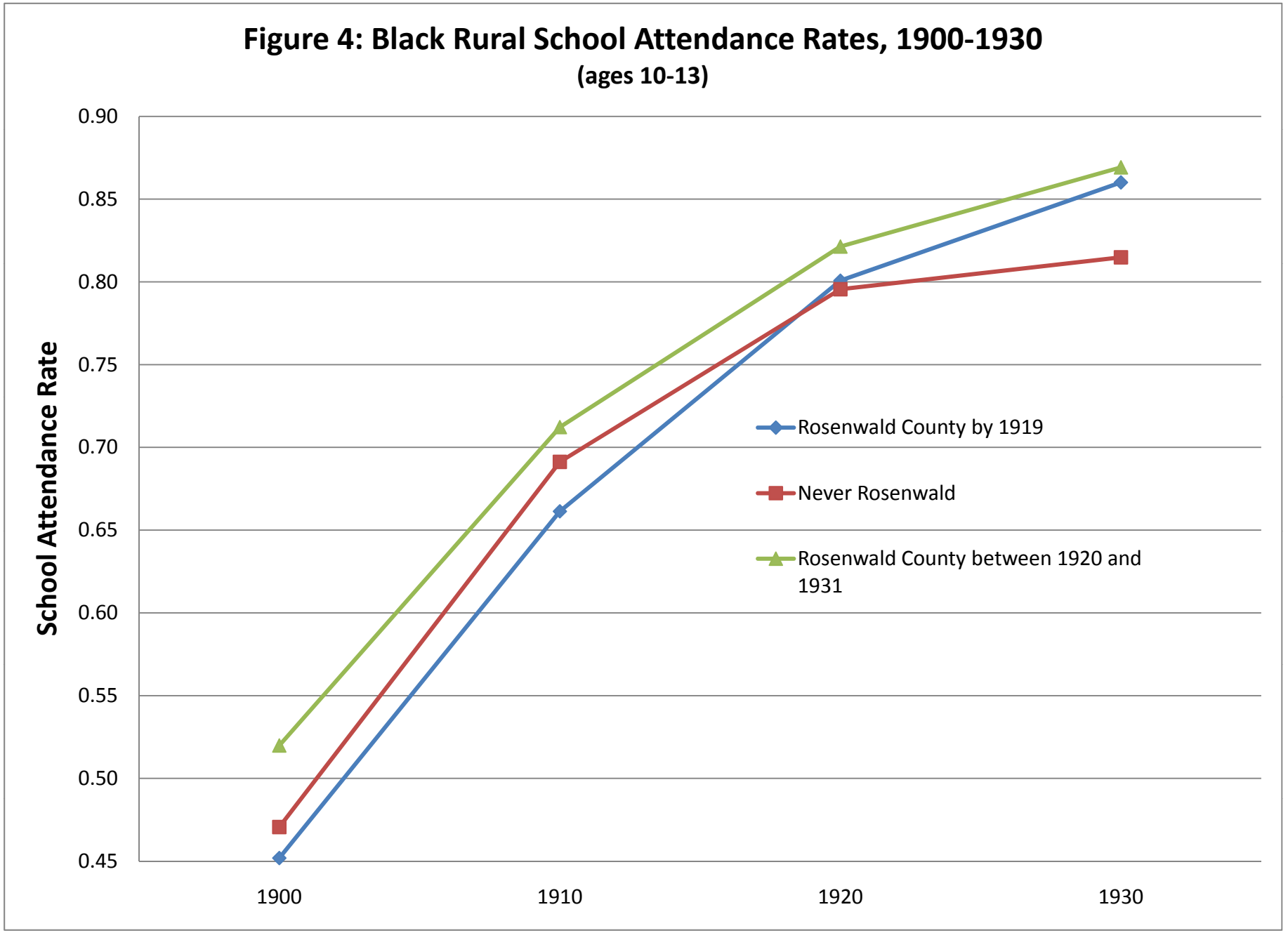
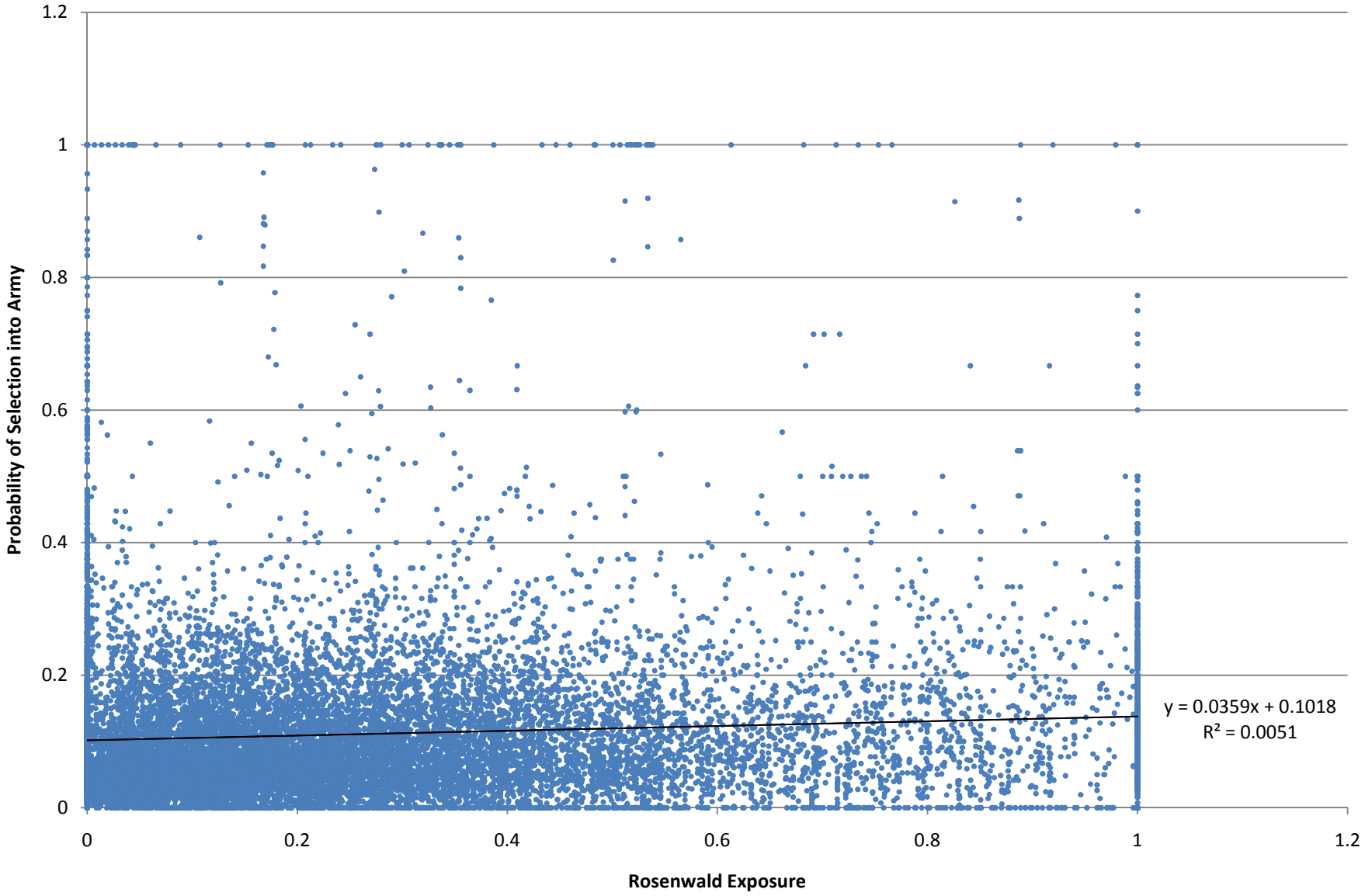


Figure 4: Black Rural School Attendance Rates, 1900-1930
(ages 10-13)
School Attendance Rate
0.90
0.85
0.80
0.75
0.70
0.65
0.60
0.55
0.50
0.45
1900
1910
1920
1930
Rosenwald County by 1919
Never Rosenwald
Rosenwald County between 1920 and 1931

Figure 5: Probability of Black Enlistment vs. Rosenwald Exposure

Each Observation is a county-birth year (N = 20494)



Appendix A: School Location Selection

This appendix describes a set of county-level regressions that show the association between pre-Rosenwald county characteristics and school location decisions. The regressions take the following form:

$$(6) \qquad ROSE_c = \alpha + \beta county_c + \beta X_c + state + \varepsilon_c$$

where $ROSE_c$ is defined as (a) whether a Rosenwald school was built in county *c* by 1919, (b) the share of the school age population covered by Rosenwald schools in *c* by 1919, and (c) the analogously defined Rosenwald coverage rate in *c* by 1931. The pre-Rosenwald county characteristics, $county_c$, come from the 1900 and 1910 Census and include race-specific measures of educational and economic status such as school enrollment, literacy, and occupational status.[1] We define these variables to be either as of 1910 or to be the change between 1900 and 1910.[2] The regressions also incorporate state fixed effects, industry share controls, plantation land as of 1890, and political participation measures as of 1880. The plantation land and political participation measures were generously supplied by Kenneth Chay and Kaivan Munshi (2011).[3]

Table A4 shows the results. In the first three columns, we regress the various Rosenwald measures on the 1910 level of county characteristics. We find no evidence that 1910 Black socio-economic characteristics can predict the location of the earliest schools. We also find no statistically significant, or economically large, effects of these characteristics on the final rates of coverage in 1931. Moreover, using changes in county characteristics between 1900 and 1910, rather than 1910 levels, columns (4) to (6) report no evidence that Black socio-economic *progress* can predict the location of the Rosenwald schools.[4] This provides some comfort that our key outcomes when measured prior to the

[1] The occupational status measures are provided by IPUMS and are based on 1950 levels of income and education by occupation.
[2] We have also run specifications that allow us to incorporate both levels and changes. The broad contours of the results are similar to what we report in Table A4.
[3] When we include this data we drop 172 counties from the sample. We find virtually identical results if we include these counties and drop the plantation land and political participation measures.
[4] In columns 4 through 6 we have also included the 1910 level of the rural black population. When we add 1900 data we drop 5 counties due to missing data.

Rosenwald period, do not appear to be significantly correlated with the location of where schools were built, especially in the 1910s, suggesting limited scope for the notion that our results could be due to reverse causality.

That said, one intriguing finding is that a 10 percentage point increase in a county's 1910 White literacy rate is associated with a 4.5 percentage point increase in the probability that a Rosenwald school is built in the county by 1919. It is not clear what this relationship reflects. From letters in the Rosenwald archives, we know that Washington believed that the program had important racial implications, and in a variety of ways sought to minimize White backlash as much as possible. These results seem consistent with that strategy. Moreover, the results are not eliminated after controlling for lynchings and political participation using the Chay and Munshi data. Alternatively, areas with higher White literacy may have been more prosperous and had a higher demand for more skilled Black labor. However, neither the level nor the change in industrial composition of the White workforce has a statistically or economically significant impact on school location and the White literacy results are highly robust to these controls.

Although we believe that school selection was fairly idiosyncratic prior to 1919, as time passed, there is perhaps some suggestive evidence that schools were concentrated in areas with better socio-economic characteristics. This can be seen from a comparison of columns (2) to (3). Column (2) reports regressions of the Rosenwald coverage rate in 1919 on pre-Rosenwald Census characteristics. Again, we find little evidence that Black or White observables matter with the exception of White literacy. But by 1931 (column 3), there is some marginally statistically significant evidence that Rosenwald coverage was higher in counties with higher Black occupational status in 1910. Further, the point estimates on Black school attendance and literacy in column (3) are positive and slightly higher than they are in column (2), though they remain statistically and economically insignificant. The positive effects of Black socioeconomic characteristics are also not robust to looking at changes in these measures between 1900 and 1910 (columns 5 and 6).

In any case, our econometric strategy (use of county fixed effects and county-by-year fixed effects) is robust to sources of bias at the county-level. The fact that we often find precisely estimated "zero" effects for our control groups once we include county fixed effects, is reassuring on this point as well. Moreover, the results in Table A4 provide further support for using the location of pre-1919 Alabama schools and state and county of birth (see sections VI and VII.D) to identify causal Rosenwald effects.

Appendix B: Army General Classification Test (AGCT) scores

Although the enlistment records database does not appear to contain test scores, Joe Ferrie discovered ,through National Archives documentation, that a May 1943 Army training manual (TM-12-305, May 1, 1943) instructed punch card operators to input AGCT scores into the weight field (Ferrie, Rolf, and Troesken 2009). Specifically the instructions read "Weight (AGCT will be punched in this field) 76-78." An examination of the data confirms that for a period from March 1943 to May 1943, the weight field was occupied by test scores. Specifically, we plotted the mean and standard deviation of the data contained in the "weight" field for a 40 week period in 1943 for all enlistees across all large enlistment cities. In New York City, for example, it is apparent that the mean value of weight abruptly changes from around 150 to 100 starting in March 1943. The mean stays at around 100 for the following 10 weeks and thereafter becomes noisy.

Based on an evaluation of the means and standard deviations of the weekly data in the weight field in the period beginning in March 1943, we were able to classify about 98,000 of the weight observations for men in the Rosenwald states as actually representing test scores. We also confirmed that our data replicates the distributions of weight and tests scores from previous historical studies using other samples of World War II enlistees. Figure A2 plots separate kernel density estimates for weight and test scores and compares this to data from previous historical studies (Staff, Personnel Research Section, The Adjutant General's Office 1947; Karpinos 1958). We find that AGCT scores have a lower fat tail and peak at around 110 while weight peaks at around 140 pounds, consistent with these other historical sources.

Finally, we note that prior to March 1943 the correlation between the data in the weight field and completed schooling was only about 0.06. For the sample in which we are convinced the data contains test scores, the correlation with schooling is roughly 0.60.

Appendix C: Details of the Internal Rate of Return Calculation

Cohort Size: We want to calculate benefits for cohorts of rural Southern Blacks who survived to age 7 and could have attended a Rosenwald school. We estimate the number of rural Blacks born in Rosenwald states who were 7 years old in the 1910, 1920, 1930, 1940 and 1950 Census and use this as our estimate of the size of the 1903, 1913, 1923, 1933 and 1943 cohorts. For example, we estimate the size of the 1913 cohort to be about 161,000. The size of other cohorts is a linear extrapolation of these counts. Unfortunately, in 1940 and 1950 we do not know rural status. Therefore, to get the count of rural Blacks in those years, we extrapolate forward the rate of the decline in the rural share of the Southern Black population from 1910 to 1930.

Earnings Stream: To calculate a base earnings level on which to apply the return to education, we estimate the lifecycle earnings for a representative cohort born in 1920. In particular, we calculate the mean earnings of Blacks born in Rosenwald states in 1920 in the 1940, 1950, 1960, 1970 and 1980 Censuses and then interpolate earnings at other ages. It is not possible to estimate the earnings stream for younger cohorts because earnings was not asked in the Census prior to 1940. Earnings are deflated to 1925 dollars using the CPI-U. We also subtract 10 percent of the value of earnings under the assumption that rural-born Blacks had lower earnings than urban-born Blacks ("rural penalty"). This suggests that for rural Blacks born in the South in 1920, total real earnings from age 20 to 60 was about $33,000 in 1925 dollars.[5]

Costs: We take the total construction costs of primary buildings, replacement buildings, rehabilitated buildings, teacher homes and industrial shops from the Rosenwald database and apply them to the school year in which they were built. For teacher salaries, we use data from Margo (1990) on salaries of Black teachers in 7 southern states in 1910 and 1936. We convert these to 1925 dollars and add a 10 percent premium on the conservative assumption that Rosenwald teachers were paid more on average. We then interpolate teacher salaries for the other years based on these values. We use the Rosenwald database to estimate the number of teachers based on the number of classrooms. We assume that maintenance costs were about 20 percent of the total of variable costs (teacher salaries being the other variable cost). Finally, we assume that the foregone contemporaneous earnings of attending an additional 1.2 years of school (see Table 5, column 2) was $40 in 1925 dollars.[6]

[5] Ideally we would like our earnings trajectory to only include individuals who did *not* attend a Rosenwald school and who presumably had a lower earnings base. Unfortunately, there is no way to address this directly. Among our sensitivity checks we have increased the rural penalty to 20 percent. This can be viewed as an alternative way of lowering the baseline level of earnings for our calculation.

[6] We estimate that the foregone contemporaneous earnings of an additional 1.2 school years was approximately $30 to $35 in 1925 dollars. We choose $40 to be conservative. We come to this estimate in three independent ways. First, we use the 1940 Census and estimate weekly earnings, by age, of 14 to 17 year old Blacks in Rosenwald states. Since the majority of working Black children in the early 20th century rural South were employed on the farm, we restrict the sample to those who report their industry as agriculture. Since earnings was not asked of those younger than 14, we interpolate the weekly earnings of 7 to 13 year olds and then calculate an average for 7 to 17 year olds. We then calculate earnings over 26 weeks (assuming a 6 month school term length) for each age. We further inflate this by a factor of 1.2 to account for our effect on "years" of education and finally convert to 1925 dollars using the CPI. This method yields an estimate of foregone earnings of $31. Our second approach also uses the 1940 Census. By age, we calculate the difference in annual earnings between Southern Blacks who

Baseline Estimates: If we assume that the effect of Rosenwald exposure is to increase schooling by 1.2 years, the low end of our point estimates, and that the return to education is 5 percent, we find that the internal rate of return (IRR) is 7.2 percent. If, instead, we assume an effect of 1.4 years on completed schooling, the upper end of our point estimates, and a rate of return of 7 percent, the estimated IRR rises to 9.3 percent.

Sensitivity Analysis: We experiment with varying the "rural penalty" from 0 to 20 percent. If we increase the rural penalty to 20 percent, our estimates fall to between 6.7 and 8.8 percent. Reducing the rural penalty to 0 increases our estimates to between 7.6 and 9.8 percent. We also experiment with increasing the share of maintenance costs as a fraction of variable costs from 20 to 33 percent. This modification lowers the range of estimates to between 6.7 and 8.7 percent. Increasing the amount of the construction costs has little effect. For example, raising such costs by 20 percent to better account for the donation of land, labor, and materials and the possibility that some capital costs are not well recorded by the Fund's index cards, lowers the IRR by only two tenths of a percentage point. If we assume that there were no foregone contemporaneous earnings among Rosenwald students, the range of our IRR estimates increases to 7.9 to 10.0 percent. On the other hand, if the foregone contemporaneous earnings was 50 percent higher than the baseline assumption ($60), our range of estimates falls to 6.9 to 9.0 percent. The IRR estimate appears to be most sensitive to the assumed rate of return of a year of schooling. For example, raising the rate of return from 5 to 9 percent raises the IRR by about 2.5 percentage points. If we use our most conservative set of assumptions in combination, the IRR is estimated to be 5.8 percent.

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work in agriculture but do not attend school and Southern Blacks who work in agriculture while simultaneously attending school. That difference could be attributed to the lost earnings implied by school attendance. Again, we multiply this amount by 1.2 years of schooling and deflate to 1925 dollars using the CPI. This method results in an estimate of foregone earnings of $34. Finally, we convert the estimates of the contribution to farm income by children from Craig (1993) to 1925 dollars. His estimates, which are based on Northern farmers from the 1860 Census, suggest that forgone earnings was about $31.

Table A1: Descriptive Statistics about Rosenwald School Projects by School

	Mean	Std dev	Min	Max
School Building Details (n=4,968)				
Rosenwald school	0.993			
Rosenwald teacher homes	0.044			
Rosenwald shops	0.035			
Country Training Schools	0.049			
Fraction of schools with additions	0.062			
Fraction of schools rebuilt	0.007			
Fraction of schools burned	0.016			
Cost of Rosenwald Schools (n=4,932)				
Real Cost of original schools	5,343	7,854	583	169,761
borne by local Blacks	882	1,021	0	16,528
borne by local Whites	221	1,226	0	39,375
borne by local government	3,425	7,193	0	163,473
borne by Rosenwald Fund	815	569	58	7,859
Fraction of schools with positive contributions from:				
local Blacks	0.92			
local Whites	0.29			
local government	0.96			
Rosenwald Fund	1.00			
Real cost, including changes to schools (additions/rebuilds)	5,604	8,176	583	169,761

Notes:
Excludes 4 Missouri projects. Counts Rosenwald schools with known number of teachers and date of construction.
Costs deflated to 1925 dollars.

Table A2: Summary statistics of 1900-1930 IPUMS samples

School Enrollment Sample of 7 to 17 year olds (N= 650167)

	1900		1910		1920		1930	
	Blacks	Whites	Blacks	Whites	Blacks	Whites	Blacks	Whites
School Enrollment								
All Ages	0.38	0.59	0.60	0.80	0.74	0.85	0.75	0.84
Age 7 to 10	0.36	0.55	0.61	0.82	0.78	0.91	0.81	0.90
Age 11 to 14	0.49	0.72	0.69	0.87	0.81	0.92	0.85	0.93
Age 15 to 17	0.28	0.46	0.45	0.66	0.55	0.65	0.52	0.63
Male	0.37	0.58	0.57	0.80	0.72	0.85	0.73	0.83
Female	0.40	0.60	0.64	0.81	0.76	0.86	0.77	0.84
Rural	0.37	0.58	0.59	0.80	0.72	0.85	0.74	0.83
Urban	0.48	0.61	0.68	0.81	0.82	0.87	0.79	0.86
Family Characteristics								
Father literate	0.39	0.85	0.53	0.87	0.63	0.90	0.73	0.93
Mother literate	0.30	0.81	0.49	0.87	0.66	0.91	0.80	0.95
Father Occ. Score	15.37	18.60	15.01	19.81	15.47	20.34	15.70	20.63
Father Owned home	0.26	0.58	0.27	0.57	0.27	0.53	0.27	0.47
Rosenwald Measures								
Presence in County	0.00	0.00	0.00	0.00	0.49	0.29	0.91	0.73
Exposure (ages 7 to 13)	0.00	0.00	0.00	0.00	0.02	0.02	0.28	0.30
Geography								
Rural	0.87	0.85	0.86	0.82	0.82	0.77	0.78	0.74
City Population	9101	19922	9963	20733	17021	30530	26542	35335
Alabama	0.11	0.07	0.10	0.06	0.11	0.07	0.10	0.07
Arkansas	0.05	0.07	0.05	0.06	0.06	0.06	0.05	0.06
Florida	0.03	0.02	0.03	0.02	0.03	0.03	0.04	0.04
Georgia	0.13	0.08	0.15	0.08	0.14	0.08	0.13	0.07
Kentucky	0.03	0.12	0.02	0.11	0.02	0.10	0.02	0.09
Louisiana	0.08	0.05	0.08	0.05	0.08	0.05	0.08	0.05
Maryland	0.02	0.06	0.03	0.05	0.02	0.04	0.02	0.05
Mississippi	0.12	0.04	0.12	0.04	0.11	0.04	0.11	0.04
North Carolina	0.08	0.08	0.09	0.08	0.10	0.08	0.11	0.10
Oklahoma	0.01	0.04	0.01	0.08	0.01	0.09	0.02	0.08
South Carolina	0.11	0.04	0.12	0.04	0.12	0.04	0.10	0.04
Tennessee	0.06	0.10	0.05	0.09	0.04	0.09	0.05	0.08
Texas	0.09	0.17	0.09	0.17	0.08	0.17	0.09	0.16
Virginia	0.08	0.07	0.07	0.07	0.07	0.07	0.08	0.07
Number of observations	21082	39887	28390	71358	17680	52188	115146	304436
Literacy Sample of 15 to 22 year olds (N = 431976)								
Literacy								
All ages	0.61	0.90	0.71	0.94	0.80	0.96	0.88	0.98
Age 15 to 17	0.60	0.89	0.72	0.94	0.82	0.96	0.89	0.98
Age 18 to 22	0.61	0.91	0.71	0.93	0.79	0.96	0.86	0.98
Rural	0.57	0.89	0.68	0.92	0.77	0.95	0.85	0.97
Urban	0.79	0.97	0.85	0.98	0.90	0.98	0.94	0.99
Rosenwald Measures								
Presence in County	0.00	0.00	0.00	0.00	0.46	0.29	0.91	0.73
Exposure (ages7 to 13)	0.00	0.00	0.00	0.00	0.00	0.00	0.13	0.13
Number of observations	13056	24620	19146	51027	11998	33623	78477	200029

Table A3: Summary statistics of WWII enlisted men sample

	All Counties						Rural Counties					
	Blacks			Whites			Blacks			Whites		
	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N
Outcomes												
Education	7.4	2.7	400168	9.7	2.9	1440294	7.1	2.6	103906	9.2	2.852	397726
Some H.S.	0.33	0.47	400168	0.62	0.49	1440294	0.28	0.45	103906	0.546	0.498	397726
Completed H.S.	0.12	0.32	400168	0.36	0.48	1440294	0.09	0.29	103906	0.301	0.459	397726
AGCT Score	65.1	16.4	11816	90.8	23.0	75545	65.5	16.8	3476	87.24	23.07	21050
Height	68.1	3.2	176352	68.7	4.4	688865	68.0	3.2	48387	68.53	4.037	193329
Demographics												
Age	23.2	4.0	400168	22.97	4.07	1440294	23.0	3.9	103906	22.86	3.944	397726
Drafted	0.86	0.35	400168	0.64	0.48	1440294	0.88	0.33	103906	0.699	0.459	397726
Enlisted, 1940	0.01	0.08	400168	0.06	0.24	1440294	0.01	0.07	103906	0.048	0.214	397726
Enlisted, 1941	0.09	0.28	400168	0.12	0.32	1440294	0.11	0.31	103906	0.137	0.344	397726
Enlisted, 1942	0.35	0.48	400168	0.36	0.48	1440294	0.36	0.48	103906	0.349	0.477	397726
Enlisted, 1943	0.26	0.44	400168	0.19	0.39	1440294	0.24	0.43	103906	0.185	0.388	397726
Enlisted, 1944	0.10	0.30	400168	0.11	0.31	1440294	0.11	0.31	103906	0.112	0.316	397726
Enlisted, 1945	0.13	0.33	400168	0.12	0.32	1440294	0.13	0.33	103906	0.12	0.325	397726
Enlisted, 1946	0.06	0.24	400168	0.05	0.21	1440294	0.06	0.23	103906	0.049	0.216	397726
Enlist Prob.	0.20	0.20	400168	0.22	0.18	1440294	0.29	0.34	103906	0.276	0.294	397726
Rosenwald Exposure												
All cohorts	0.33	0.30	400168	0.33	0.34	1440294	0.24	0.22	103906	0.251	0.292	397726
born <1915	0.18	0.23	75810	0.17	0.23	274574	0.14	0.15	17546	0.145	0.216	70703
born 1916-22	0.35	0.30	205351	0.35	0.35	748228	0.25	0.23	55133	0.264	0.293	209531
born 1923-28	0.39	0.31	119007	0.40	0.37	417492	0.27	0.24	31227	0.289	0.313	117492
Geography												
% Rural	0.71	0.28	400168	0.72	0.28	1440294	1.00	0.00	103906	1.00	0.00	397726
Alabama	0.10	0.30	400168	0.08	0.27	1440294	0.09	0.29	103906	0.074	0.261	397726
Arkansas	0.03	0.16	400168	0.03	0.18	1440294	0.01	0.12	103906	0.03	0.172	397726
Florida	0.07	0.25	400168	0.04	0.20	1440294	0.03	0.16	103906	0.022	0.146	397726
Georgia	0.11	0.31	400168	0.09	0.29	1440294	0.10	0.30	103906	0.089	0.285	397726
Kentucky	0.02	0.15	400168	0.08	0.28	1440294	0.01	0.12	103906	0.113	0.316	397726
Louisiana	0.06	0.24	400168	0.03	0.16	1440294	0.05	0.22	103906	0.024	0.154	397726
Maryland	0.03	0.16	400168	0.04	0.19	1440294	0.07	0.26	103906	0.082	0.274	397726
Mississippi	0.14	0.34	400168	0.05	0.22	1440294	0.18	0.38	103906	0.067	0.251	397726
No. Carolina	0.11	0.32	400168	0.11	0.31	1440294	0.09	0.28	103906	0.077	0.266	397726
Oklahoma	0.02	0.14	400168	0.07	0.25	1440294	0.00	0.07	103906	0.031	0.173	397726
So. Carolina	0.08	0.28	400168	0.05	0.21	1440294	0.06	0.23	103906	0.019	0.138	397726
Tennessee	0.07	0.25	400168	0.11	0.31	1440294	0.03	0.18	103906	0.108	0.31	397726
Texas	0.10	0.31	400168	0.17	0.38	1440294	0.06	0.23	103906	0.109	0.311	397726
Virginia	0.06	0.24	400168	0.05	0.23	1440294	0.21	0.41	103906	0.156	0.363	397726

Table A3: Summary statistics of WWII enlisted men sample (continued)

	Urban Counties						Non-Rural, Non-Urban counties					
	Blacks			Whites			Blacks			Whites		
	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N
Outcomes												
Education	8.1	2.8	112957	10.5	2.7	372180	7.2	2.7	183305	9.5	2.894	670388
Some H.S.	0.41	0.49	112957	0.74	0.44	372180	0.30	0.46	183305	0.60	0.49	670388
Completed H.S.	0.16	0.37	112957	0.46	0.50	372180	0.11	0.31	183305	0.347	0.476	670388
AGCT Score	66.6	16.3	4725	97.3	21.9	21109	63.0	15.8	3615	88.86	22.75	33386
Height	68.1	3.3	45678	68.6	4.9	181464	68.2	3.1	82287	68.76	4.25	314072
Demographics												
Age	23.7	4.3	112957	23.32	4.26	372180	23.0	3.9	183305	22.83	4.023	670388
Drafted	0.84	0.36	112957	0.59	0.49	372180	0.86	0.35	183305	0.637	0.481	670388
Enlisted, 1940	0.01	0.09	112957	0.06	0.24	372180	0.01	0.08	183305	0.068	0.251	670388
Enlisted, 1941	0.08	0.27	112957	0.12	0.32	372180	0.08	0.27	183305	0.108	0.31	670388
Enlisted, 1942	0.32	0.47	112957	0.37	0.48	372180	0.37	0.48	183305	0.361	0.48	670388
Enlisted, 1943	0.30	0.46	112957	0.21	0.40	372180	0.25	0.43	183305	0.183	0.387	670388
Enlisted, 1944	0.10	0.30	112957	0.10	0.30	372180	0.10	0.30	183305	0.11	0.313	670388
Enlisted, 1945	0.12	0.33	112957	0.10	0.31	372180	0.13	0.34	183305	0.12	0.326	670388
Enlisted, 1946	0.06	0.23	112957	0.04	0.20	372180	0.06	0.24	183305	0.049	0.217	670388
Enlist Prob.	0.22	0.10	112957	0.24	0.10	372180	0.14	0.11	183305	0.174	0.092	670388
Rosenwald Exposure												
All cohorts	0.38	0.37	112957	0.36	0.39	372180	0.35	0.27	183305	0.362	0.339	670388
born <1915	0.22	0.29	27214	0.17	0.24	82696	0.18	0.19	31050	0.174	0.227	121175
born 1916-22	0.41	0.38	54358	0.39	0.39	190232	0.37	0.27	95860	0.383	0.337	348465
born 1923-28	0.46	0.39	31385	0.46	0.41	99252	0.41	0.28	56395	0.439	0.356	200748
Geography												
% Rural	0.32	0.10	112957	0.31	0.10	372180	0.80	0.12	183305	0.79	0.12	670388
Alabama	0.15	0.36	112957	0.09	0.29	372180	0.08	0.27	183305	0.081	0.273	670388
Arkansas	0.01	0.11	112957	0.02	0.14	372180	0.04	0.20	183305	0.043	0.203	670388
Florida	0.12	0.33	112957	0.09	0.29	372180	0.05	0.22	183305	0.029	0.167	670388
Georgia	0.16	0.37	112957	0.10	0.30	372180	0.08	0.27	183305	0.083	0.275	670388
Kentucky	0.03	0.18	112957	0.09	0.29	372180	0.02	0.14	183305	0.062	0.241	670388
Louisiana	0.04	0.19	112957	0.01	0.12	372180	0.08	0.27	183305	0.036	0.187	670388
Maryland	0.01	0.07	112957	0.02	0.15	372180	0.01	0.12	183305	0.02	0.141	670388
Mississippi	0.05	0.22	112957	0.02	0.15	372180	0.17	0.38	183305	0.054	0.226	670388
No. Carolina	0.08	0.27	112957	0.07	0.26	372180	0.15	0.36	183305	0.144	0.351	670388
Oklahoma	0.02	0.13	112957	0.05	0.22	372180	0.03	0.16	183305	0.098	0.297	670388
So. Carolina	0.05	0.21	112957	0.02	0.14	372180	0.12	0.33	183305	0.077	0.267	670388
Tennessee	0.13	0.34	112957	0.13	0.34	372180	0.04	0.21	183305	0.091	0.287	670388
Texas	0.14	0.35	112957	0.25	0.43	372180	0.11	0.31	183305	0.165	0.371	670388
Virginia	0.01	0.08	112957	0.01	0.11	372180	0.02	0.13	183305	0.018	0.132	670388

Table A4: Determinants of Location of Rosenwald Schools Using Pre-Rosenwald County Characteristics

	Indep. Vars use 1910 levels			Indep. Vars use Change 1900-10		
	(1) School by 1919	(2) Coverage 1919	(3) Coverage 1931	(4) School by 1919	(5) Coverage 1919	(6) Coverage 1931
Rural Black Population	0.017	0.000	-0.004	1.414	4.444	8.744
	[0.004]***	[0.001]	[0.003]	[5.698]	[1.820]**	[4.208]**
Black School Enrollment	-0.003	-0.003	0.052	0.019	0.015	0.045
	[0.064]	[0.021]	[0.048]	[0.043]	[0.014]	[0.032]
Black Literacy	0.031	0.022	0.060	0.006	0.021	-0.065
	[0.084]	[0.027]	[0.063]	[0.068]	[0.022]	[0.050]
Black Occ. Status	0.006	0.002	0.007	-0.001	0.001	0.004
	[0.005]	[0.002]	[0.004]*	[0.004]	[0.001]	[0.003]
Black Occ. Ed. Score	0.004	0.001	-0.007	0.000	-0.001	-0.005
	[0.006]	[0.002]	[0.004]*	[0.003]	[0.001]	[0.002]**
White School Enrollment	0.064	0.047	0.099	0.119	0.036	0.178
	[0.122]	[0.039]	[0.091]	[0.067]*	[0.022]	[0.050]***
White Literacy	0.447	0.101	0.443	-0.031	0.010	0.086
	[0.213]**	[0.069]	[0.159]***	[0.163]	[0.052]	[0.120]
White Occ. Status	-0.014	-0.004	0.004	-0.005	-0.001	0.005
	[0.008]*	[0.003]*	[0.006]	[0.006]	[0.002]	[0.004]
White Occ. Ed. Score	0.012	0.004	0.003	0.002	0.000	-0.004
	[0.007]*	[0.002]	[0.005]	[0.004]	[0.001]	[0.003]
% Teachers	1.881	-0.006	0.561	1.147	0.393	2.127
	[1.481]	[0.478]	[1.109]	[0.923]	[0.294]	[0.681]***
Fraction Repub. Voters	-0.054	-0.018	-0.068	-0.072	-0.018	-0.056
(1880)	[0.091]	[0.029]	[0.068]	[0.091]	[0.029]	[0.067]
Ever elect, Black St. Rep.	-0.017	-0.006	-0.051	-0.018	-0.003	-0.039
(before 1880)	[0.045]	[0.014]	[0.033]	[0.046]	[0.015]	[0.034]
Ever elect, Black St. Sen.	-0.067	0.005	-0.015	-0.080	0.008	0.002
(before 1880)	[0.060]	[0.019]	[0.045]	[0.060]	[0.019]	[0.045]
Plantation Share of Land	-0.073	-0.041	0.149	0.152	-0.015	0.107
(1880)	[0.314]	[0.101]	[0.234]	[0.290]	[0.093]	[0.214]
N	873	868	868	873	868	868

*Notes: S*ample includes counties with rural blacks. All regressions also include indicators for missing any of the following black school white literacy. Regressions also include indicators for state and control for the fraction of workers in agriculture, nondurable manufact Columns 4 through 6 also control for the 1910 level of the rural Black population. Standard errors are in brackets. * significant at 10%;

Table A5: Coefficients from Regressions Based on Stratified Samples

Panel A: Census School Attendance Results Stratified by…

	1910 Black School Attendance		*1910 White School Attendance*		*1910 Black Population Share*		*1890 Plantation Share of Land*		*1860 Slave Share (if >0)*	
	at/below Median	above Median	at/below Median	above Median	at/below Median	above Median	at/below Median	above Median	at/below Median	above Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
γ_0	-0.015	0.025	-0.023	0.026	0.028	-0.028	0.025	-0.018	0.025	-0.005
	[0.021]	[0.013]*	[0.025]	[0.012]**	[0.011]**	[0.026]	[0.014]*	[0.027]	[0.017]	[0.018]
γ_1	0.060	0.044	0.048	0.062	0.040	0.057	0.044	0.046	0.019	0.069
	[0.023]***	[0.015]***	[0.019]**	[0.017]***	[0.017]**	[0.016]***	[0.022]**	[0.017]***	[0.018]	[0.019]***
γ_2	-0.024	-0.001	0.020	-0.028	-0.013	0.008	-0.022	0.003	-0.025	0.006
	[0.016]	[0.010]	[0.018]	[0.010]***	[0.009]	[0.017]	[0.012]*	[0.019]	[0.013]*	[0.014]
γ_3	0.146	0.015	0.072	0.087	0.027	0.081	0.063	0.084	0.044	0.078
	[0.028]***	[0.020]	[0.025]***	[0.021]***	[0.028]	[0.020]***	[0.029]**	[0.021]***	[0.028]	[0.023]***
N	255887	296191	337151	296440	281651	352418	203511	333198	226559	288338

	Top quart. Slave Share AND Plantation	*excluding those in* (11)	*Age*			*Sex*	
			7 to 10	11 to 13	14 to 17	Male	Female
	(11)	(12)	(13)	(14)	(15)	(16)	(17)
γ_0	0.012	0.010	-0.032	-0.043	0.049	0.015	-0.01
	[0.043]	[0.014]	[0.011]***	[0.011]***	[0.023]**	[0.016]	[0.015]
γ_1	0.121	0.034	0.050	0.028	0.035	0.052	0.054
	[0.046]***	[0.016]**	[0.015]***	[0.014]**	[0.028]	[0.019]***	[0.013]***
γ_2	-0.022	-0.013	0.026	0.022	-0.096	-0.015	0.002
	[0.045]	[0.010]	[0.009]***	[0.008]**	[0.019]***	[0.012]	[0.012]
γ_3	0.104	0.061	0.047	0.102	0.141	0.075	0.075
	[0.061]*	[0.019]***	[0.019]**	[0.019]***	[0.033]***	[0.022]***	[0.019]***
N	83940	398919	250214	170990	222059	324141	319122

Panel B: World War II Education and AGCT Results Stratified by 1920 Black School Attendance…

	Education		AGCT Scores	
	at/below Median	above Median	at/below Median	above Median
	(1)	(2)	(3)	(4)
γ_0	0.155	-0.046	5.52	-3.566
	[0.302]	[0.119]	[3.114]*	[1.390]**
γ_1	-0.413	0.059	-1.788	2.322
	[0.437]	[0.233]	[3.815]	[2.065]
γ_2	-0.43	-0.127	-7.177	-7.504
	[0.380]	[0.218]	[6.367]	[5.557]
γ_3	1.997	0.894	22.677	-3.073
	[0.552]***	[0.394]**	[7.341]***	[4.596]
N	306693	521279	15053	27983

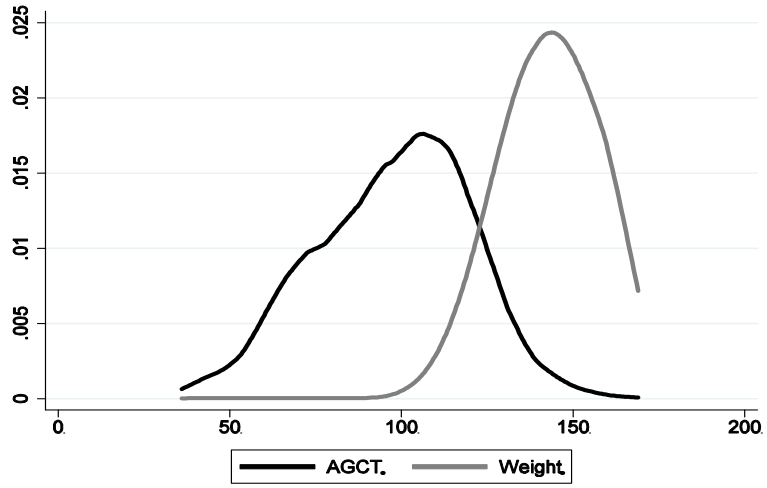
Notes: See notes accompanying Table 8 for details. Standard errors, clustered on county, are shown in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure A1

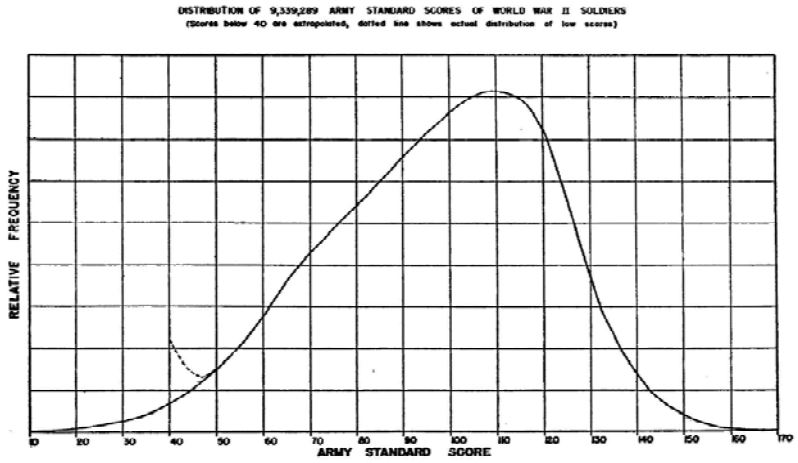
Alabama Border Counties Used for Table 4A

Figure A2: Comparison of AGCT and Weight Data to Historical Sources

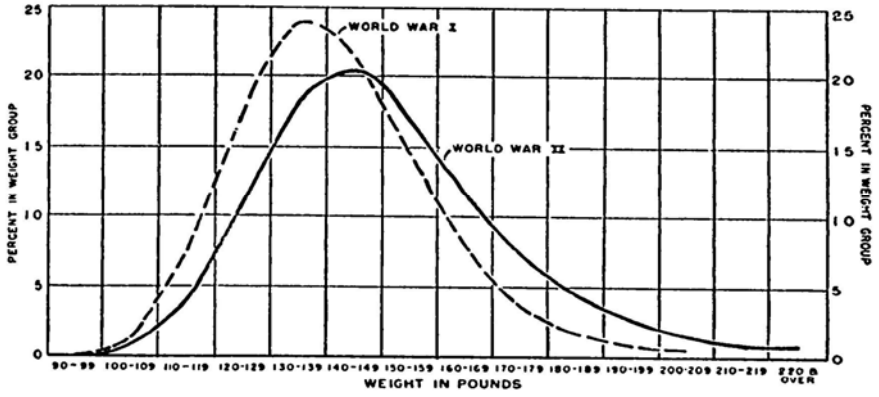
Panel A: Kerenel Density of AGCT scores and Weight



Panel B: AGCT scores from Staff, Personnel Research Section, The Adjutant General's Office (1947)



Panel C: Descriptive statistics for weight for WWI and WWII enlistees from Karpinos (1958)



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