THE IMPACT OF ROSENWALD SCHOOLS ON BLACK ACHIEVEMENT

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Abstract: The Black-White gap in completed schooling among Southern born men narrowed sharply between the World Wars after being stagnant for cohorts born between 1880 and 1910. We examine a large scale school construction project, the Rosenwald Rural Schools Initiative, which was designed to improve the educational opportunities for Southern rural Blacks. From 1914 to 1931, nearly 5,000 school buildings were constructed, serving approximately 36 percent of the Black rural school-aged Southern population by 1930. We use historical Census data and World War II enlistment records to analyze the effects of the program on school attendance, literacy, high school completion, years of schooling, earnings, hourly wages, and migration. We find that the Rosenwald program accounts for at least 30 percent of the sizable educational gains of Southern Blacks born during the 1910s and 1920s. Using scores on the Army General Classification Test (AGCT), a precursor to the AFQT, we also find that access to the schools significantly improved cognitive skills. In the longer run, exposure to the schools raised the wages of Blacks who remained in the South relative to Whites in the South by about 35 percent, implying a private rate of return to a year of additional schooling of about 17 percent. Moreover, Rosenwald significantly increased northbound migration of young adult Blacks, likely fueling further income gains. Across all outcomes, the improvements were highest in counties with the lowest levels of Black school attendance suggesting that schooling treatments can have a very large impact among those with limited access to education.

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I. Introduction

A vast body of economic research has investigated the links between human capital investment, individual earnings, and aggregate economic development. While there is general consensus that schooling has a causal effect in promoting individual economic well-being, the magnitude of the effects may be especially large and most convincing in the context of relatively deprived agrarian societies where rudimentary school inputs are often lacking (e.g. Glewwe and Kremer 2006). At the turn of the 20th century, the education infrastructure available to American Southern Blacks, particularly those living in rural areas, resembled what many developing nations face in modern times: often unsuitable classrooms, insufficient basic equipment, and a severe lack of political representation to overturn these deficiencies. Moreover, little progress relative to Whites had been made in the preceding thirty years. Racial schooling gaps remained persistently high compared to the North and funding inequities appeared to be growing (Margo 1990).

However, over a moderately short period between the World Wars, the Southern racial education gap improved dramatically (see Figure 1).[1] While Southern Blacks born between 1880 and 1910 obtained three fewer years of schooling than their White contemporaries, a generation later the racial gap was well under a year and comparable in size to the racial gap in the North, which remained roughly unchanged for cohorts born between 1880 and 1940. Significant South-North convergence occurred along other school quantity and quality measures as well (Card and Krueger 1992, Donohue, Heckman, and Todd 2002).

This paper evaluates a sizable yet largely unstudied school construction program, the Rosenwald Rural Schools Initiative, which was explicitly designed to narrow the racial education gap in the South. At the urging of Booker T. Washington, the principal of the Tuskegee Institute in Alabama, Chicago businessman and Tuskegee board member Julius Rosenwald supplied matching grants to help facilitate the construction of almost 5,000 schoolhouses for Southern Black rural children starting in 1913. The vast majority of the schools were built during the late 1910s and 1920s. As the last buildings were constructed in 1931, Rosenwald schools had the capacity to employ over 14,000 teachers, providing classrooms for over a third of the rural South's Black school-age population or roughly a quarter of all Southern Black school-age children.

[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).

The dearth of research on this program has been driven by lack of detailed information about who went to the schools and where the schools were even located.[2] Through the Rosenwald Fund archives at Fisk University in Nashville, Tennessee, we obtained access to digitized index cards that report location, timing, size, and cost of every Rosenwald building. We link this data to several large historical samples of potential students —the 1910 to 1950 Census samples (IPUMS) and enlistment records from World War II— to estimate the causal effect of the schools on a variety of outcomes.

We find economically and statistically significant effects of the program on Black rural school attendance, literacy, educational attainment, military test scores, earnings, and hourly wages. Rosenwald schools can explain about 30 percent of the Black-White convergence in educational attainment for native Southerners born between 1910 and 1924 and even larger portions of other education outcomes. Ultimately, we estimate that exposure to Rosenwald schools increased relative wages of Blacks that remained in the South by around 35 percent and that the private returns to an additional year of schooling induced by the program are approximately 17 percent. Moreover, these returns do not account for potential benefits of migration to better labor opportunities in the North, though verily, we find a notable increase in Northern migration for young Blacks exposed to the schools.

A unique aspect of our analysis is that we are also able to estimate the impact of the schools on a direct measure of cognitive ability by using test scores on the Army General Classification Test (AGCT), a precursor to the modern Armed Forces Qualifying Test (AFQT). We find that exposure to the schools increase Black test scores on average by about 6 points, or 0.25 standard deviations and would close about 25 percent of the Black-White test score gap. We find that the effect on test scores disappears when we control for educational attainment, adding further evidence to the growing literature that has shown that racial gaps in test scores are not immutable and can be affected by interventions (Neal and Johnson 1996; Hansen, Heckman, and Mullen 2004; Cascio and Lewis 2006; Chay, Guryan, and Mazumder 2009).

Across all of the outcomes, we find that the gains are largest for those with the most limited access to schooling opportunities. For example, the test score effects are particularly large, about 0.6 standard deviations,

[2] An exception is Donohue, Heckman, and Todd (2002), who discuss the role of Northern Philanthropists, and Rosenwald in particular, on Black school attendance in the early 20th century. For historical descriptions of the Rosenwald Rural Schools Initiative, see McCormick (1934), Embree (1936), Ascoli (2006), and Hoffschwelle (2006).

among counties with the lowest pre-Rosenwald Black school attendance rates. This suggests that schooling interventions targeted at the bottom of the skill distribution may have especially large returns. This is consistent with a view in the development literature that introducing schools in rural areas with few alternatives disproportionately benefits students that are facing the highest cost of attending school. For example, Duflo (2001) finds that the introduction of schools in Indonesia had larger effects in more sparsely populated regions and in regions with lower initial levels of schooling.

Our research design makes use of several sources of variation. First, we utilize the considerable geographic variation across counties in school location. Second, we also take advantage of the explicit targeting of the Rosenwald program to a single well-identified demographic group: rural Blacks. This allows us to use other demographic groups, particularly rural Whites and urban Blacks, to remove any confounding effect that might have been correlated with Rosenwald school exposure. In practice, our preferred specifications consistently find no direct impact of Rosenwald schools on rural or urban White students and mixed, but always significantly smaller effects on Black urban students. Finally, we exploit the well-observed variation in birth cohort access to schools, as in Duflo (2001). The Rosenwald schools were constructed over 18 years, introducing considerable disparity in school exposure across birth cohorts even within counties. Since we use multiple cross-sections in our Census samples, we are able to control for unobserved fixed and *time-varying* county characteristics. Additionally using the family data of the IPUMS, we can exploit sibling differences and control for unobservable family background characteristics through family fixed effects. We also demonstrate that our results are unaffected by the inclusion of county-specific cohort trends.

The ability to control for fixed and time-varying county effects is particularly important because school location decisions were certainly not random. This non-randomness is inherent in the way the matching grants were designed: local citizens were required to donate substantial funds for construction, sometimes including land and labor. Consequently, it is plausible that funds were disbursed to counties with higher demand for educational resources and hence likely facing better outcomes even in the absence of the Rosenwald program. However, we find little empirical evidence to support this conjecture. We show that Black school attendance rates and attendance trends in those rates, 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. The fact that we generally find no effects for our control groups also helps mitigate concerns of selection bias regarding where the schools were built.

To further address the potential bias from endogenous program placement, we use the idiosyncratic way that Booker T. Washington influenced site decisions in the program's earliest years, in particular in Alabama and in the counties surrounding the state capital of Montgomery, to confirm that our estimated Rosenwald effects are not due to school location choices.

A related concern with our identification strategy is that families with the highest demand for educational resources may have migrated to the counties where the Rosenwald schools were built. An exercise with the initial Alabama schools helps to address this concern since the rapid building of the pilot schools preceded the large-scale rollout of the program throughout the South. However, to further account for potential bias from selective migration, we recalculate Rosenwald exposure rates based on state of birth —and, for one of our exercises, county of birth— rather than county of residence. By construction, our cohorts were born prior to large-scale Rosenwald construction and therefore exposure rates are unrelated to family migration decisions. We find broadly similar results using these measures.

While we do not believe that selection drives our inferences, it is nevertheless likely that racial convergence in educational standards with the North was inevitable given the high rates of return to basic education that we uncover. 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 during the 20th century in Black educational attainment and cognitive skill development has likewise been significantly aided by a series of additional 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 help 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 a large schooling intervention in the South did in fact have a sizable effect on the levels of human capital and earnings of Blacks born after 1910. This suggests that the relative importance of the supply of human capital may have played a larger role than previously thought in accounting for early 20th century Black economic progress. Perhaps more speculatively, later cohorts also could have been impacted through intergenerational mechanisms[3] and human capital spillovers, as well as by the many Rosenwald schools that remained open into the 1950s.

The next section provides background on the Rosenwald schools, including descriptions of the program's history, initiatives, financing, and school location selection. Section III describes the data. Section IV outlines our empirical strategy. Section V presents our early Census results on school attendance and literacy and section VI addresses selection concerns using the earliest Alabama schools and state of birth-specific Rosenwald exposure rates. Section VII discusses our results on adult outcomes using the World War II enlistment data and the 1940-50 Censuses. Section VIII describes how these results differ across individual and pre-Rosenwald community characteristics. Brief conclusions are offered in section IX.

II. The Rosenwald Rural Schools Initiative

A. Historical Background

The Rosenwald schools program arose from the collaboration of two exceptional individuals. Booker T. Washington was born into slavery and became a leading Black educator and the first principal of the Tuskegee Institute. Julius Rosenwald, a Chicago area businessman, made his fortune as part-owner of Sears, Roebuck and Company and was also a noted philanthropist. Washington recruited Rosenwald to serve on the board of the Tuskegee Institute. The Rosenwald Rural Schools Initiative originated from a request by Washington in 1912 to

[3] 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).

use a small fraction of Rosenwald's donation to the Tuskegee Institute to fund six nearby experimental Black primary schools. Frustration with the disbursement of funds by local county education boards and the general inadequacy of Black schools[4] led Washington to seek out Northern philanthropists, including but not limited to Rosenwald. Other philanthropic efforts at the time, notably the Slater and Jeannes Funds, were limited, and it was becoming clear that major investments in Black schools would require further outside intervention (Donohue, Heckman, and Todd 2002). Washington's unusual ability to cultivate contacts with wealthy philanthropists, combined with his vision for Black education, made him a unique figure to launch a comprehensive school construction program from scratch.

After the opening of the initial schools, completed by the spring of 1914, Rosenwald agreed to partly fund up to 100 additional rural schools, primarily in Alabama. The program spread quickly. By the end of the decade, 716 rural schools, covering 11 states, were open. Figure 2 displays a map of the coverage of Rosenwald schools as of the 1919-20 school year. Each county is shaded based on our estimate of the fraction of school age Black rural children that could be accommodated by Rosenwald schools.[5] School coverage was high in Alabama but the schools were also clustered in Louisiana, Tennessee, Kentucky, North Carolina and Virginia. Three final states – Florida, Oklahoma, and Texas – were approved for funding in 1920.

Construction activities escalated in the 1920s, when the number of schools grew by 18 percent per year. Towards the end of the 1920s, the Rosenwald Fund moved from constructing small primary schools towards larger schools, often with the capability of offering high school level instruction.[6] Southern Black public high school instruction was highly unusual prior to the Rosenwald program. Even in the mid-1920s, with the high school movement well underway in the North (Goldin and Katz 1999), Alabama and South Carolina contained no four-year accredited Black public high schools, and Florida, rather remarkably, funded only two Black public

[4] For accounts of early 20th century Black schooling, see Jones (1917), Bond (1934), McCormick (1934), Myrdal (1944), Margo (1990), and Hoffschwelle (2006). In an early letter to Rosenwald (Fisk archives, 6/12/1912), Washington described many of Alabama's Black schools as being "as bad as stables," a sentiment generalized to much of the South in Jones (1917). Racial funding inequities are described bluntly in Washington's letters to Rosenwald and catalogued more systematically in Johnson (1941) and Margo (1990). For example, Johnson (1941) notes that in 1930 the average county spent 39 cents on Black school salaries per capita for every dollar on White school salaries per capita.

[5] These calculations are based on county-specific counts of Rosenwald teacher capacity, full counts of the Black rural population aged 7 to 17 obtained though ancestry.com, and an assumed student-teacher ratio of 45. The latter is consistent with surveys of rural Black Southern schools in state and county education board reports at the time.

[6] The increase in school size is reflected in the rising number of teachers per school. For example, the average school size in 1930 and 1931 was 4.9 teachers, compared to 2.1 teachers per school in the 716 schools built during the 1910s.

high schools, regardless of accreditation or length.[7] However, by 1932, roughly 10 percent of existing Rosenwald schools offered at least two years of high school instruction (Donohue, Heckman, and Todd 2002).

When the initiative closed in 1932, [8] a Rosenwald school existed in 85 percent of counties with Black children, nearly achieving the Fund's vision of having a school in every Southern county. Nevertheless, most counties contained far too few schools to serve all rural Black children. Figure 3 displays the student coverage of the schools as of 1931-32. At that point, official documents report 4,977 schools in 883 counties with the capacity to serve 663,615 students. We estimate that roughly 36 percent of the Southern rural Black school-age population, and 25 percent of all Southern Black school-age children, attended Rosenwald schools by 1930.

The program's varied timing and geographic development provides the basis for our main identification strategy. Figure 4 plots the interquartile range (25^{th} to 75^{th} percentile), along with the mean, of the share of school-age Black rural students served by Rosenwald schools across counties over the 1919 to 1930 period, highlighting the substantial amount of cross-county variation. The coverage of Rosenwald schools varied substantially over time *within* counties as well. For simplicity we show the time pattern of coverage for each state in Figure 5. 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 was the site of the first Rosenwald schools, by 1930 its rural Rosenwald coverage was among the lowest.

B. Activities of the Rosenwald Fund

The strategies pursued by the Rosenwald Fund reflected the experiences and visions of both Booker T. Washington and Julius Rosenwald. Washington felt strongly that in the wake of slavery, the Black community could only develop through self-reliance. He received strong financial support from both the Black and White communities in building and expanding the Tuskegee Institute. Rosenwald meanwhile was a strong proponent of using matching grants to foster community support and "buy-in" for his previous philanthropic efforts.[9]

[7] These numbers are from an internal 1925 Rosenwald report based on state education reports.
[8] 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 asset value prior to the market crash (McCormick 1934).
[9] For example, Rosenwald used matching grants for Jewish charities, Black YMCAs and to promote agricultural extension agents.

Given these views, the Rosenwald Fund was deliberately designed to provide only a small share of the funding and required a strong commitment from the local community. Local Blacks and state and county governments provided the majority of the funding, particularly after construction was complete. 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 in the last five years of the program.[10]

The program emphasized school construction, a strategy justified by legitimate concern that Black school donations would otherwise be expropriated.[11] Other initiatives of the Fund, however, went well beyond financial support and included teacher salary standards, teacher home construction, school building standards and designs, school terms extensions, and access to high school curriculums. Improvements in teacher salaries and term length required local governments to channel extra expenditures to Black schools.[12]

Extending the length of the school term was a particularly important goal of the program. The average term in Southern Black schools was 4.7 months just prior to the Rosenwald program (McCormick 1934). Initially, the Fund established a 5 month minimum, boosted it to 6 months in the mid- to late-1920s, and to 8 months for larger schools in 1930. Financial incentives, including paying for half of the construction cost of teacher homes, were offered to schools that further lengthened terms. Overall, we estimate that the program could account for approximately 1½ months of the increase in term lengths in Black rural schools.[13]

Finally, the Fund played a critical role in building local coalitions. Explicitly, this included paying canvassers to explain available opportunities and guide local Black leaders through the fundraising process (Hoffschwelle 2006). More importantly perhaps, the Fund and the Tuskegee Institute supplied much needed

[10] In total, 64 percent of funding came from local government sources, another 20 percent from private sources, and only 15 percent from the Rosenwald Fund (Table A1). These figures do 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.

[11] Previous philanthropic funding for education provided by Rockefeller and others under the General Education Board was largely appropriated by White school districts (Ascoli 2006).

[12] In county level regressions not reported here, we find that exposure to Rosenwald schools raised Black school salary expenditures per pupil by $0.34 (0.06), or just over 3 percent of average annual Black school salaries in 1930 with no corresponding effect for Whites ($0.02 (0.09)). The Black school expenditure effects are twice as large, but not precisely estimated, for the initial schools built in Alabama (see section VI). These effects are likely to underestimate the true effect since our data combined urban and rural schools. Further details are available from the authors.

[13] We used state level data from an internal Rosenwald Fund report (titled "Negro Public Schools in the South, 1925-26") that contained term length for the 1925-26 school year and found that a 10 percentage point increase in Rosenwald coverage was associated with a 0.48 month increase in term length for rural Black schools. Therefore, going from no Rosenwald coverage to 30 percent coverage would increase terms by roughly 1½ months. Since these are rough estimates (unconditional on covariates), we do not necessarily interpret them as causal effects.

prestige and credibility in the Black community during this process.[14] But coalition building was critical among Whites as well. 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). 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. Moreover, Washington (and Rosenwald) viewed working with the White public as a critical component of any school program's success, a principle that appears to have been adopted by the Rosenwald Fund in the program's early years (e.g. see Appendix A).

C. Matching Grants and School Location Selection

The Fund was unambiguous in its requirement that it only provide funding conditional on local support: "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). Matching grants were a mechanism for ensuring this self-reliance.[15] Therefore, the prospect for school funding was likely linked to a county's social, political, and economic environment. This suggests that 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 provide several pieces of evidence regarding the possibility of selective school location. In Figure 6, we show if anything, 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 that never received a school.[16] Thus, with respect to our key outcome, we find no evidence of positive selection for the earliest schools. This also motivates our analysis in section VI of the early Alabama schools,

[14] To take one example, a new school required a donation of at least two acres of land to the state, a rather perverse requirement given Southern history. This led to initial concerns that the fundraising might be a hoax (McCormick 1934). It seems likely the reputations of Tuskegee and Washington were partly responsible for alleviating this initial apprehension.
[15] Letters available at the Fund archives at Fisk University provide a few examples of communities that struggled to obtain the required funds and were denied funding.
[16] 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. Data for 1890 is unavailable.

which we believe were chosen idiosyncratically. Appendix A more formally investigates 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 have limited ability to predict the timing or intensity of initial Rosenwald school locations in a statistically or economically significant way, suggesting limited scope for reverse causality (Table A4).[17] However, we do find clear 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. 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 were given digital versions of the index cards used to keep track of 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 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. Appendix Table A1 provides basic statistics about all Rosenwald school

[17] Black occupational status has a marginally statistically significant effect in some specifications but the size of the effect is not qualitatively large.

construction projects. Our analysis will use a database that includes 4,932 schools with the capacity to hold 14,438 teachers in 880 counties.[18]

B. Census (1910-1930)

We create samples drawn from the 1910, 1920 and 1930 decennial Censuses using the IPUMS (Ruggles et al. 2010) and link them to the Rosenwald schools by county of residence and birth year.[19] Importantly, we can distinguish between those living in rural or urban areas within a county.[20] We concentrate on school attendance and literacy, the two relevant outcomes that are available prior to 1940. For school attendance, we construct a pooled sample of over 604,000 Black and White children between the ages of 7 and 17. For literacy, our sample totals over 398,000 persons between the age of 15 and 22.[21]

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 race 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 these factors in our regressions.

C. World War II Enlistment Records

We also use records of US Army World War II enlistees, available from the National Archives and Records Administration, to extract a sample of roughly 2.1 million Black and White men aged 17 to 45 from

[18] This deletes 37 "schools." In 35 cases, the card does not contain key information (in particular, cost or teacher counts). In 25 of these 35 cases, the project seems to involve only a teacher home or shop built as part of an existing school. We also do not include the lone two Missouri projects (another Missouri project is among the 35 with missing school data). 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, particularly the latter, ultimately are.

[19] We use the 1.4 percent sample for 1910, the 1 percent sample for 1920 and an early version of the 1930 5 percent sample. Since the 1910 data oversamples certain groups, we utilize sample weights in our main estimates.

[20] 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.

[21] Collins and Margo (2006) argue that literacy acts as a proxy for completing 1 to 3 years of schooling and show that it rises as a cohort ages from 10-19 to 20-29. To abstract from literacy effects due to schooling at young ages, we chose a lower age cutoff of 15. The upper age range is restricted to avoid spurious correlation arising from the possibility that adults with high literacy moved into Rosenwald counties for their children's schooling. We actually 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. We address migration more directly below.

Rosenwald states who enlisted between 1941 and 1945.[22] To ascertain potential Rosenwald status, we use age and county of residence at enlistment. Unlike the Census, however, we do not have precise information on geography within the county to infer rural status. Instead, we use a blunter approach, classifying a county as rural if the average rural share was greater than 50 percent over the 1910 to 1930 period.

The key advantage of this data, relative to the 1910 to 1930 Censuses, is that it provides measures of human capital during adulthood. This includes completed years of schooling beyond grammar school[23] and scores from the Army General Classification Test (AGCT) used to determine military occupation. Since these test scores were thought lost to history, we describe these measures in Appendix B. In addition, the data contain height and weight, which we use as a validity check. Access to schools after age 5 is unlikely to affect height (Martorell et al 1995; Behrman and Hoddinott 2005) and might only affect weight indirectly through improved economic status. The summary statistics are presented in Appendix Table A3. The Black-White gap in years of schooling is about 2.2 years while the racial difference in AGCT scores is about 1.1 standard deviations.

One concern is that there may be selection into who was inducted into the Army and therefore required to take the AGCT. In 1940, prior to entering the war, the US enacted a draft. At that time, the manpower requirements were relatively low, so screening standards on physical and mental characteristics were higher. After the US entered the war in 1942 and manpower needs became critical, selection standards were lowered considerably (Lew 1944). We discuss how we address potential selection bias in the next section.

For a smaller subsample of about 17,000 WWII enlistees we also know county of birth. This sample was constructed by matching individuals by name, state of birth and year of birth to the Social Security Death Master File (DMF). For a subsample of these individuals 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.[24]

D. Census (1940-1950)

[22] Records are available for 1938 to 1946, but few individuals enlisted prior to 1941 and after 1945. See Feyrer, Politi, and Weil (2008) for more description of the data.

[23] 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.

[24] There are many sources of possible selection with this sample: 1) Individuals must have died by 1965 or later; 2) they must have entered the Social Security System; 3) they must be uniquely identified; and 4) they must have provided a clearly recognized county or city name that could be matched to the Rosenwald database.

Finally, we use IPUMS samples from 1940 and 1950 to assess the effects of the program on completed schooling, log annual earnings, log hourly wages, and migration. For education we use a sample of approximately 200,000 individuals aged 18 to 40. For a subset of this sample (around 70,000) who worked as employees, we have information on annual earnings and hours worked, from which we can construct wages. For economic outcomes we limit our sample to those who worked at least 100 hours during the year. For those who did not work as employees, we use an indicator for having at least $50 in non-wage income during the year as an additional outcome and as a robustness check concerning selection into wage employment.

Unlike the earlier IPUMS, county of residence for the 1940-50 sample is not publicly available yet. Instead, we link individuals to their exposure to Rosenwald schools by state economic area (SEA), aggregations of contiguous counties with similar economic characteristics developed by the Census Bureau. On the one hand, this could lead to greater classification error, since our measures will be blunter than before. On the other hand, the larger aggregation retains individuals who moved across counties (e.g. rural to urban migration) within the same SEA. We further check the robustness of the results by only including individuals who remained in their state of birth ("stayers").

The 1940 Census is the first to ask about location five years prior, which allows us to measure migration patterns based on SEA residence in 1935 and 1940. Our migration sample includes all residents of Rosenwald states in 1935. We define a South-to-North migrant as someone who did not live in a Rosenwald state in 1940. Likewise, a within-South migrant is a person living in a different Southern SEA in 1940. The sample is broken down by birth cohort in order to estimate the impact of Rosenwald exposure on children that are likely in school in 1940, young adults that would have left school between 1935 and 1940, and those who are into adulthood.

IV. Empirical Strategy

A. Regression Model

A typical specification for our analysis is of the following form:

$$(1)\ y_{ict} = \alpha + female + black + rural + blackrural + \gamma_0 ROSE + \gamma_1(black \times ROSE) + \gamma_2(rural \times ROSE) + \gamma_3(blackrural \times ROSE) + \beta X_{ict} + \theta_{st} age_{it} + county_c + \varepsilon_{ict}$$

where y_{ict} is an outcome for individual *i* living in county *c* at time *t, female*, *black*, *rural* and *blackrural* are indicators of being in one of those demographic categories, X_{ict} 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 and the year of the Census, *county* represents county fixed effects, and ε_{ict} is an error term.

For our analysis of the 1910-1930 Census samples, we use two measures of the Rosenwald treatment, ROSE. The first measure, $R_{ct,}$ is an indicator of whether a Rosenwald school was built in an individual's county *c* as of Census year *t*. This measure is limited in at least two ways: it fails to distinguish differences in Rosenwald exposure between birth cohorts within a county[25] and it does not adjust for the breadth of Rosenwald coverage within a county. Despite these drawbacks, we start with R_{ct} since it provides a straightforward approach that is easy to interpret.

Our more comprehensive measure of Rosenwald exposure, E_{bc} estimates the average Rosenwald coverage each student experienced over the ages 7 to 13[26] based on their birth year *b* and county or SEA, *c*. Specifically, $E_{bc} = \frac{1}{7}\sum_{t=b+7}^{t=b+13} T_{ct}$, where T_{ct} is the share of the total school-age rural Black population in county *c* in year *t* that can be accommodated by Rosenwald classrooms.[27] Since, the measure takes on values between 0 and 1, we interpret the coefficient on the measure as the effect of going from no Rosenwald exposure in one's county or SEA to complete exposure. For our WWII samples and for the 1940-1950 Census samples, we are limited to only using E_{bc} as our measure of Rosenwald exposure. Standard errors are clustered either at the county or SEA level.[28]

As we show in Figure 4, overall Rosenwald coverage was about 4 percent in 1919 and grew to 36 percent by 1930. Therefore, depending on the cohorts used in each exercise, the average level of E_{bc} may vary

[25] For example, a 13 year old in 1930 living in a county that had first opened a Rosenwald school in 1928 who had only two years of exposure, is treated the same as a 13 year old living in a county that had built a Rosenwald school in 1924 and had 6 years of exposure.

[26] 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. Our results are robust to defining exposure over the ages of 7 to 17 (Table A6).

[27] Specifically, T_{ct} is the ratio of the number of Rosenwald teachers in county *c* in year *t* times an assumed class size of 45 (see footnote 5), to the estimated number of rural Blacks between the ages of 7 and 17 in the county in each year. We use the digitized Census manuscript files available through Ancestry.com to retrieve full counts of the school aged population by county and for urban areas in 1920 and 1930. We use this to calculate rural counts by county for the Census years and then interpolate the population for 1919, and 1921 through 1929. Since in some cases, the exposure measure exceeds 1, we topcode values of the exposure measure at 1. This affects 6 counties in 1919, 49 counties in 1925 and 112 counties in 1930.

[28] Clustering at the state level (as in Bester, Conley, and Hansen 2009) has minimal impact on our inferences, especially when we introduce a full set of controls.

across samples. For example, in our 1910-30 Census school attendance sample, mean Rosenwald exposure was 0.28 (Table A2) so we use that figure for calculating the effects of exposure on the narrowing of racial gaps in school attendance observed within the 1910 to 1930 samples. For other exercises where we consider the implications only for more recent cohorts who entered school in the 1930s, we use the 0.36 figure.

These Rosenwald measures are interacted 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 serve as controls. The γs, which are the coefficients on the Rosenwald measure and its interactions with the race-rural groups, enable us to construct the main "differenced" estimates. For example, to calculate the effect of complete exposure versus no exposure on only Black rural children we would sum γ_0, γ_1, γ_2, and γ_3. Similarly, to estimate the Black-White difference of the Rosenwald effect in rural areas we would sum γ_1 and γ_3. Finally, γ_3 taken alone, provides an estimate of the "triple difference", that is the effect of Rosenwald exposure on the Black-White gap in rural school attendance relative to the Black-White gap in urban school attendance. To construct other differenced estimates (e.g. White rural-urban), we re-normalize the regressions by changing the omitted categories and replacing the relevant dummy variables for race and rural status so as to return the intended parameter of interest along with its standard error.

By differencing across groups, we remove any factors that are correlated with Rosenwald school exposure that affect the groups similarly. For example, if the rural economy happened to be improving more in Rosenwald counties, this would presumably benefit both rural Whites and rural Blacks. Indeed, the "undifferenced" estimates for our control groups are also potentially revealing the extent of selection. For example, a "zero" coefficient on exposure for rural Whites would suggest that there was no systematic form of selection that favored rural communities. Similarly, there may be race-specific factors that happened to be coincident with the construction of Rosenwald schools that would affect urban Blacks in Rosenwald counties. Finally, we can difference out both race and rural status.

Access to repeated cross-sections across Census years allows us to exploit the variation over time in Rosenwald school coverage *within-county*. We can therefore control for unobserved characteristics of the county by using county fixed effects. With the exposure measure, E_{bc}, we can also specify separate county fixed effects *for each Census year* to address any long-term (e.g. 10 year) time trends that are county-specific (i.e. add

county*year$_{ct}$ to equation 1). 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. This framework also 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).[29]

To address any remaining concerns related to unobservable family characteristics, 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[30], 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 WWII Data

To address any potential bias from nonrandom induction into the military, we use two strategies. First, we include indicator variables for each calendar quarter of enlistment interacted with race. This allows us to control for differential patterns of selection over time by racial groups in a very flexible way.[31] To address potential selection within these cells, we weight the regression models by the inverse of an estimate of the probability that different men within the cell enlisted in the military at different rates – also known as Inverse Probability Weighting (IPW).[32] Define p to be the true likelihood that a given individual will enlist, and $\hat{p}$ to be

[29] 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).
[30] We can only use family fixed effects for studying effects of school attendance with the 1910-30 Census samples.
[31] This strategy is not useful for test scores since they are only observed for a short period of time.
[32] Unlike studies which use a selection equation (e.g. propensity score) with a sample to estimate the probability of selection, we have the universe of World War II enlistees and thus the full set of data for calculating the numerator of the fraction of the true probability of selection. In order to construct the denominators, we use the digitized records of the complete 1930 Census manuscript files to produce counts of the universe of the population. Specifically we define cells by

an estimate of that likelihood. Then, weighting the regression equations by $w_i = \frac{1}{\hat{p}}$ removes any remaining 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).

V. Census Results on School Attendance and Literacy

A. School Attendance[33]

Table 1 provides results for school attendance using the indicator of county Rosenwald presence, R_{ct}. To provide intuition for interpreting the results in the remaining tables, we first describe school attendance rates, broken down by Rosenwald presence, race and urban status in the first four rows and first two columns. For example, conditioning just on Census year, the average school attendance rate among rural blacks aged 7 to 17 was 73.2 percent in a county with a Rosenwald school and 65.7 percent in a county without a Rosenwald school. The third column, titled "Rose-no Rose difference," subtracts the non-Rosenwald rates from the Rosenwald rates. In the case of rural Blacks, this very simple estimate of the effect of Rosenwald schools is 7.6 percentage points (with a standard error of 0.7 percentage points).

In this simple specification, there is no "Rosenwald" effect on White rural students but small and statistically significant effects on urban Blacks and, to a lesser extent, urban Whites. The second panel, labeled "difference-in-difference," reports estimates that account for these Rosenwald trends among the various groups. That is, the row labeled "Black, rural-urban" differences out the common Black effect encompassed among urban Blacks. Likewise "B-W Rural" does the analogous calculation for common rural effects. The Rosenwald effect estimated from rural-urban Black is 4.9 (1.1) percentage points and the Black-White rural gap is 7.5 (0.7) percentage points. Finally, when we difference the rural Black-White and urban Black-White estimates, labeled "triple difference," we find that a Rosenwald school is estimated to raise school attendance by 6.2 (1.2) percentage points.

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

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race, county of enlistment and year of birth (for those born between 1910 and 1926) and use counts in the enlistment records for the numerators. We construct analogous counts from the 1930 Census for the denominators.

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

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. Concentrating on the most complete specification in column (4), which includes controls and county fixed effects, we find that the rural Black result remains economically and statistically important; Rosenwald boosted school attendance among potentially eligible children by 6.0 (0.8) percentage points. Including county fixed effects eliminates the effect on urban Whites but still leaves a small positive effect for urban Blacks. The finding of precisely estimated "zero effects" for Whites when including county fixed effects suggests that this approach is in fact, removing any selection that might arise at the county level. The various difference-in-difference estimates such as the Black-White rural difference, the rural-urban difference among Blacks, or the triple difference, range narrowly between 4 and 6 percentage points.

The magnitudes of these estimates are economically important. Over the period 1910 to 1930, Black rural attendance in the Rosenwald states rose by 14.4 percentage points, so our Black rural estimate of 6.0 percentage points suggests that Rosenwald schools account for about 42 percent of the gain. Similarly, the Black-White rural difference in school attendance fell by about 11.5 percentage points from 1910 to 1930. Our estimates in Table 1 suggest that Rosenwald schools can account for about 53 percent of this decline.

Table 2 reports results using the more refined Rosenwald exposure measure, E_{bc}. To save space, we report only the difference-in-difference outcomes. Moving across the table shows how results change as we add controls. Column (1) begins with a sparse specification without county fixed effects, and shows large and significant effects of complete exposure on the key estimates: the Black rural-urban gap, the Black-White rural gap and the triple difference. Columns (2) to (4) add controls for county fixed effects and county-by-year fixed effects. This removes a negative effect on the White rural-urban gap, though a small positive effect remains for the Black-White urban gap. Some of the magnitudes of the point estimates are altered but all of the key effects remain statistically and economically significant. We show further robustness checks in the Appendix. We find that our results are similar if we include county-specific age (cohort) trends or if we further interact our state and Census year specific age trends with race or rural status (Table A5). Our results are also not altered by our choice of IPUMS samples, weighting methods or using both urban and rural Blacks in constructing our exposure measure (Table A6).

Moving from the simple but blunt Rosenwald treatment measure shown in Table 1 to the more nuanced measure in Table 2 slightly lowers the implied effects of the program. For example, using our preferred specification in column (4), which includes the baseline controls along with county-by-year fixed effects and age interactions by state and Census year, the estimates suggest going from no exposure to Rosenwald schools (E_{bc}=0) to the mean level of Rosenwald exposure in 1930 (E_{bc}=0.28 or 28 percent) would raise school attendance of rural Blacks relative to rural Whites by about 3.2 percentage points. That estimate accounts for close to 30 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). While the standard errors rise sharply since we have much less power, the point estimates are remarkably similar to what we find in other specifications. We also note that with family fixed effects, we now find a quantitatively small effect on the Black-White urban gap.

B. Literacy

Table 3 reports results for literacy using our sample of 15 to 22 year olds. Using the Rosenwald dummy variable with controls and county fixed effects (column 2) shows that the presence of a Rosenwald school in the county raises Black rural literacy rates by 6.5 (0.6) percentage points. As with school attendance, we estimate no effect for rural Whites and a small positive effect on urban Blacks. This leads to difference-in-difference estimates of 6.7 (0.6) percentage points for the Black-White rural estimate and 5.0 (0.9) percentage points for the rural-urban Black difference. The triple difference is somewhat lower at 3.9 (0.1) percentage points. However, some of this is due to an estimated decline in literacy rates among urban Whites in Rosenwald counties. This may be driven by sampling error rather than a true decline in literacy since literacy rates were already close to 100 percent among urban Whites by 1910.

Again, the implied impact of Rosenwald school presence is large. The Black rural literacy rate rose by about 16.6 percentage points over the 1910 to 1930 period. Our Black rural estimate suggests that Rosenwald schools account for 39 percent of the gain. We also estimate that Rosenwald schools accounted for 53 percent of the 12.7 percentage point narrowing of rural Black-White literacy gap. These results are virtually identical to the analogous calculations for school attendance based on Table 1.

We also find large and statistically significant effects of Rosenwald exposure. Focusing on our preferred specification in column (4), we find that complete exposure to Rosenwald schools would improve Black literacy relative to Whites in rural areas by about 19 percentage points. Similarly the effect on literacy for rural Blacks is nearly 14 percentage points larger than the effect on urban Blacks. 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 nearly 13 percentage points. These results imply that going from no exposure to the mean 1930 exposure can account for about 20 percent of the closing of the Black-White rural gap in literacy and about 34 percent of the closing of the differences in the Black-White literacy gaps between rural and urban areas.

VI. Alternative Tests to Address Selection

Thus far we have relied on fixed effects and differences between treatment and control groups to address any potential selection driven by local demand for education. This section presents alternative approaches to bolster these findings.

A. Earliest Schools Built in Alabama

Archival records suggest that the initial Rosenwald schools, which were heavily clustered in specific geographic areas (see Figure 2), arose for idiosyncratic reasons that were largely unrelated to economic or educational circumstances.[34] We focus on the initial schools in Alabama where this claim is most transparently verified by Booker T. Washington's initial plans for the Rural Schools Initiative:

[34] The cluster of schools in Eastern Virginia and North Carolina could be explained by their proximity to Hampton Institute in Virginia 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. The presence of the state agents, who were White men, was a recent development that was due to the General Education Board, a philanthropic effort started by John D. Rockefeller and Frederick T. Gates. Washington specifically considered exploiting such friendly contacts:

> "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 only Louisiana, North Carolina, and Tennessee. Appendix A also highlights that the initial school sites were not strongly related to observable characteristics of the counties.

> "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." [35]

That Washington started the program in Alabama, as opposed to Mississippi or Georgia, is very plausibly a matter of happenstance. Washington was born into slavery in southwest Virginia and made his way to the Hampton Institute in Virginia where he became trained as a teacher. The principal of the Hampton Institute recommended Washington as a suitable choice to head the school that was to become the Tuskegee Institute, and this was the basis of his move. Moreover, we know from future construction activity (e.g. Figure 5), school expenditure data, as well as anecdotes[36] that there is little to suggest that Alabama's underlying demand for Black schools was high. Therefore, we take advantage of Washington's location to estimate the effects of the initial Rosenwald schools that were built in Alabama by 1919.

We do this in two ways. First, we compare the effects on students who lived in counties in Alabama relative to a control group of students who lived in contiguous counties just on the other side of the border in states where Rosenwald had not yet expanded at all, or had extremely limited presence.[37] The exercise is based only on the 1910 and 1920 Censuses. Because there are few large urban areas in our comparisons and sample sizes are limited, we do not break out the analysis by rural status and use only the indicator of any Rosenwald presence in one's county by 1920. Panel A of Table 4 reports the results. We find that when we combine all four borders, the presence of a Rosenwald school increases the likelihood of Black school attendance relative to Whites by between 8 and 9 percentage points when we use OLS (column 5) and between 9 and 11 percentage points when we include county fixed effects (column 10).

In a second exercise, we experiment with the distance of counties from the state capital in response to a stated strategy in the original plans:

> "It is thought best at present to concentrate upon supplying schoolhouses for the following three counties: Montgomery, Lowndes, and Lee in Alabama. One of these counties contains the capital of the State. It is thought wise for advertising and for the purpose of creating public sentiment to put the county containing the capital of the state and near-by counties in good shape first; by concentrating

[35] Source: "Plan for Erection of Rural Schoolhouses," Date and author unknown, Fisk archives.
[36] 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.
[37] See Figure A1 for a map showing Rosenwald school presence in the Alabama area. 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.

upon a few counties may serve the further purpose of bringing about a rivalry between the communities that will prove of value."[38]

For this exercise, we again examine the effects of a Rosenwald school in one's county on Black-White differences in school attendance. We draw several samples using only Alabama counties within a specified distance of Montgomery using only the 1910 and 1920 Censuses. The results are shown in Panel B of Table 4. We find that using a 20 mile radius, a Rosenwald school in the county increases Black school attendance by 20.3 (6.1) percentage points relative to Whites, a result that is impervious to the inclusion of county fixed effects. This effect falls to a still large 8 percentage points for counties within 75 miles of Montgomery.

Interestingly, the point estimates in Table 4 are uniformly larger than the analogous estimates using the full Rosenwald state sample in Table 1 which were on the order of 4 to 7 percentage points. However, they are not statistically different in most cases. Still, they raise the possibility that the program may have been less successful as time went on or that the later schools may have been more negatively selected. Overall, while we are not dismissing the role of endogenous selection, we believe there is compelling evidence that the main results are not significantly swayed by this selection process.

B. Exposure based on birthplace

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.[39] A possible implication of such selective migration would be to overstate the effects of the program since outcomes for these children might have been higher even in the absence of the program. It is difficult to directly assess the potential magnitude of the effects since the rate of migration across geographic areas is generally unavailable prior to the 1940 Census (see section VII.F).

Our evidence on the pre-1919 Alabama schools largely address this concern, since the rapid building of the pilot schools preceded the large-scale rollout of the program throughout the South. It is unlikely that migration would have responded quickly to the pilot program. In Panel A of Table 4, we find even larger effects

[38] "Plan for Erection of Rural Schoolhouses," Date and author unknown, Fisk archives.

[39] Emmett Scott of the Tuskegee Institute in 1918 noted that the presence of the schools appeared to 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).

when we restrict our Alabama sample to cohorts born in 1906 or earlier, most of whom would have already been enrolled in schools before the Rosenwald schools were built and therefore would have been less likely to move.

However, to further address concerns about selective migration, we rerun our 1910-1930 Census analysis on the full set of Rosenwald states using a source of exposure that is determined prior to the time of school attendance, namely exposure based on one's state of birth. Here we utilize state fixed effects to absorb any time invariant effects and use Whites as a control group. We can also further refine the analysis to cohorts born prior to 1918, that is, before the large-scale rollout of the schools throughout the South. For migration to be a concern for these cohorts, one must argue that Black (and not White) parents selectively moved across states prior to their child's birth, and correctly forecast which states would have Rosenwald schools.

The results of this analysis are shown in Panel A of Table 5. In column (1), we estimate the effect on school enrollment using all birth cohorts. We find that the effect of complete exposure for Blacks is to raise school attendance by 17 percentage points with no corresponding effect for Whites. In column (2), we restrict the sample to the older cohorts and find the effects are even larger at 20 percentage points for Blacks and again find no effect on Whites. We find similarly large effects on literacy in column (3). We take these estimates as general confirmation that our estimated effects are, if anything, larger when we account for selective migration. Although the state of birth estimates are quite large and may be of independent interest, we are cautious in our interpretation of them and prefer estimates that use county level exposure where we are better able to control for unobservables. We discuss the remaining panels of Table 5 in the next section.

VII. Results on Adult Outcomes from World War II Enlistment Records and the 1940-50 Census

A. Years of Schooling

Table 6 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) is used to correct sample selection. The top panel of the table begins by ignoring any distinction between rural and urban status. In column (1), we find that going from no exposure to full exposure would raise Black educational attainment by 0.26 years (0.17) and lower attainment for Whites by about 0.16 years (0.04). When we use IPW in column (2), the estimate for Blacks rises to 0.39 (0.13) and

becomes highly significant while the coefficient for Whites is smaller in absolute value but still significant. Both specifications suggest that Blacks with exposure to Rosenwald schools would gain about 0.4 to 0.5 years relative to Whites.

The second panel estimates the same specifications but only uses counties where the majority of the population is classified as rural based on the 1910 to 1930 IPUMS. This restriction considerably increases the effect on Blacks and on the Black-White gap. Both specifications suggest that complete exposure raised Black schooling levels by 0.7 years of schooling and narrowed the Black-White gap by 0.8 years . By contrast, the third panel of the table shows that in urban counties, Rosenwald exposure was associated with a slight worsening in completed schooling for both races and consequently little to no impact on the Black-White gap. Finally, the fourth panel estimates the triple difference and our IPW estimate suggests that complete Rosenwald exposure is associated with narrowing the difference in the Black-White schooling gaps between rural and urban counties by about 0.9 years (hereafter, we only show the IPW results).[40] Based on these estimates, Rosenwald exposure increased Black schooling by about 12 percent relative to the mean and appears to have accounted for about 30 percent of the narrowing of the Black-White gap.[41]

B. High School Completion

In column (3), we next focus on the program's effects on high school completion. These results are particularly striking. Focusing on rural counties in the second panel, the estimates suggest that full exposure to Rosenwald schools is associated with more than a 9 percentage point increase in the probability of high school completion amongst Blacks and a relative gain of 10 percentage points compared to rural Whites. Further, we estimate very precise effects of close to 0 for urban Whites and a small but statistically insignificant decline of about 2 percentage points for urban Blacks. The triple difference is estimated to be close to 13 percentage points and is highly significant. Since the mean high school completion rate among Blacks is about 32 percent,

[40] We note that since we cannot distinguish between rural and urban areas within a county, our estimates may be attenuated. To gauge the degree of this bias, we re-estimated the effect on adult literacy using the Census data and imposed the same county level rural classification scheme. We found that our key estimates were only slightly lower using this approach.
[41] The Black-White gap for Southern born men is about 3 years for the pre-Rosenwald cohorts born between 1905 and1909 (see Figure 1) and closes by about 1.2 years for the 1924 birth cohort who would have had the maximal exposure to the program. If we use 1 year as the effect of full Rosenwald exposure (we show effects of about 1.5 in section VII.E) and mean Rosenwald exposure rises from 0 to 0.36 over this time, the effect at the mean is 1*0.36 = 0.36. Therefore, Rosenwald explains about 30 percent (0.36/1.2) of the closing of the gap.

this suggests that a county that went from no exposure to complete exposure could improve high school completion among Blacks by about 40 percent.

Our finding of larger effects on high school completion than on total years of schooling could be due to the fact that one key aspect of the Rosenwald treatment was the opportunity to go to high school at all (see section II.A). It could also be that the World War II records measure schooling at too young an age for many individuals who may have gone on to post-secondary schooling after the War.

C. AGCT Scores

Column (4) presents the results on WWII test scores. We once again find quantitatively large and statistically significant effects on Blacks from rural counties but small effects for Blacks from urban counties and Whites from rural or urban counties. Using the triple difference estimate, complete exposure to Rosenwald schools would improve Black test scores by about 5.9 points or about 0.24 standard deviations. In the aggregate, moving from no exposure to the mean exposure of 0.36 (for cohorts who entered school by the end of the Rosenwald program in 1931) narrowed the Black-White test score gap by about 8 percent.

Since the publication of Herrnstein and Murray (1994), several studies have shown that the Black-White test score gap can be influenced by environmental factors (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, had a sizable effect on test scores. Further confirming that schooling influences these scores, we also find that the Rosenwald test score effect is eliminated when we include educational attainment as a covariate in the regressions (column 5).

D. Height and Weight

A potential validity check for our results would be to measure the effects on outcomes for which we expect the Rosenwald program to have had less, and perhaps no, influence. A good candidate is height, since effective interventions are believed to be confined mostly to the early life period, well before children enter school (Martorell et al 1995, Behrman and Hoddinott 2005). Since Rosenwald targeted children beyond the

early life period and was not designed to treat childhood nutrition or health,[42] we expect Rosenwald exposure to have no impact on height. That is indeed what we report in column (6). We also find little impact on weight (column 7). These results do not suggest any obvious confounding factor.

E. Longer-term Outcomes

Table 7 displays estimates of the effect of the program on completed years of schooling and economic outcomes of individuals living in Rosenwald states in the 1940 and 1950 Census samples. Recall that for this sample, we do not know rural classification or county of residence, so we estimate overall Black-White differences based on exposure measured at the SEA level. For each outcome, we show estimates for our overall sample and for a subsample of those who were residing in their state of birth ("stayers"). Sample means for blacks are shown in the bottom row of each panel of Table 7.

In column (1), we find that complete exposure raises Black years of schooling by 1.1 years and reduces White schooling by about 0.4 years, resulting in a relative increase of about 1.5 years. We find slightly higher effects when we confine the sample to stayers. In columns (3) and (4), we show that complete exposure is associated with a 20 to 25 percent increase in annual earnings for Blacks and a decline of about 20 percent for Whites leading to a differenced estimate of greater than 40 percent. In columns (5) and (6), the wage gains for Blacks with complete exposure are about 21 percent while the losses for Whites are about 16 percent leading to relative wage gains for Blacks of about 37 percent. Finally, in columns (7) and (8), we show that the probability of having non-employee income of $50 or more in a year increases by between 7 and 9 percentage points with complete Rosenwald exposure an effect of about 40 to 50 percent evaluated at the mean. This finding also suggests that our earnings results are not driven by selection into being a salaried employee.

A Wald estimate of the returns to schooling on wages is the ratio of the estimates in columns (1) and (5), yielding a return of 17 percent (0.197/1.131).[43] This estimate however, does not remove the common effect of exposure shared by both racial groups. If we construct a Wald estimate based on Black-White differences,

[42] 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).

[43] We also estimated the returns to schooling for Blacks directly. An OLS estimate of the private returns to education is about 0.05 while the IV estimate using Rosenwald exposure as an instrument is 0.176, virtually identical to the Wald estimate.

the implied returns to Rosenwald are an even larger 24 percent. These estimates are notably larger than the 8 to 14 percent returns typically estimated using contemporary data sources and modern era schooling interventions (e.g. Card 1999) or Duflo's (2001) analysis of an Indonesian school construction program in the 1970s.

We can think of several plausible explanations for our outsized estimate. Rosenwald support for salaries, homes, and improved school environments could have facilitated a higher quality teacher workforce and thus boosted the return to a year of school.[44] Additionally, Rosenwald's promotion of longer term length increased the definition of a school year and therefore mechanically boosted the return to a "year" of schooling. A third possibility is that the returns to schooling for those who were most impacted by the Rosenwald program were just extremely large. International evidence suggests that there may be larger returns to education in developing countries, particularly at the primary school level (Psacharapoulos and Patrinos 2004) and in very rural settings (Duflo 2001). Finally, the marginal returns to education for the groups affected by Rosenwald may be larger than the average return. We turn to such evidence in the next section.

F. South-to-North Migration

Lastly, another channel by which improved education could raise earnings is through greater opportunity to relocate to superior labor markets, in this case chiefly to Northern cities (e.g. Bowles 1970, Margo 1990, Card and Krueger 1992). We use the 1940 Census to study Rosenwald's impact on migration by comparing current residential SEA to residential SEA five years prior. The sample includes all residents of Rosenwald states in 1935. Results are stratified by age cohorts and presented separately for South-to-North and South-to-South migration. Conditioning variables include gender, gender by race, age, state fixed effects, and state-age-race trends.

Results are presented in panel B of Table 7. Strikingly, only one of the six Black-White estimates, reported in column 2, is statistically or economically significant -- migration out of the South among cohorts that likely finished school between 1935 and 1940 (that is, are ages 17 to 21 in 1940). At a Rosenwald exposure rate

[44] Potentially limiting these positive mechanisms are concerns expressed by Rosenwald himself in letters, about the ability to hire enough qualified teachers to staff new schools. Trepidation was particularly prominent during and right after WWI and thus may have been a temporary situation related to the War. Ultimately, we do not have data to measure the extent to which a shortage of labor was an issue beyond the War. But we would emphasize that large school expansions can have a negative impact on teacher quality, an issue Jepsen and Rivkin (2009) study in a modern setting.

of 36 percent, typical for these cohorts, we find that Black migration to the North among recent school finishers increased by over 60 percent (from 1.4 to 2.3 percent). By contrast, we find no effect on within-South migration for these same cohorts (column 5). We also find no effect on Blacks who were still of school-age in 1940, nor older cohorts (between the ages of 22 and 30 in 1940) that were finished with school and who had remained in the South as of 1935. Finally, we find no effect of Rosenwald exposure on migration of Whites in any of these age groups. The only group of Southerners that responded with their feet to higher Rosenwald exposure, at least in the late 1930s, was Blacks finishing school, and they increased their propensity to move North.

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 education on Black Northern migration at this critical time. Additionally, if Rosenwald schools did in fact induce more moves to potentially better labor markets, the rate of return to the Rosenwald program may have been even larger than what we show in Table 6.[45]

G. Exposure Based on State or County of Birth

In section VIB, we showed that by using exposure based on state of birth rather than county of residence, our results on schooling and literacy with the early Censuses were robust to the possibility of selective migration to Rosenwald counties. The same conclusion holds with respect to long-term outcomes. In panels B through D of Table 5, we show that the effects on education, test scores, and income that use state of birth exposure are consistently larger than but statistically indistinguishable from those based on current county of residence exposure. Those results hold when we restrict the sample to cohorts born before the full scale rollout of the schools throughout the South (cohorts born before 1918). Of particular note, Panel C shows that the education results are similarly larger but statistically indistinguishable from the full sample when we use a select subsample of WWII enlistees for whom we know county of birth (columns 3 and 6).

VIII. Heterogeneous Effects

A. By initial school conditions

[45] This point is potentially offset by a negative labor supply effect on Blacks that were already in the North (Boustan 2009). See also Duflo (2004) for a similar result following the Indonesian school construction program.

Card (1999) builds on the Becker (1967) model to show that heterogeneous rates of return to education may arise due to differing costs of education, preferences, or marginal returns to the production function relating schooling to earnings. Card suggests that one possible explanation for the tendency for many IV estimates of the returns to schooling to exceed OLS estimates is that in the presence of heterogeneous returns, the marginal returns to education for the groups affected by the instrument may be larger than the average return. This could arise if marginal returns are higher for those with low levels of schooling and the instrument (e.g. school reform, school proximity) mainly affects this segment of the population by lowering the costs of schooling.[46] This could also explain Duflo's (2001) finding of larger effects of school construction in more sparsely populated regions.

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 the effects on our various outcomes using samples that are stratified by county rates of Black school attendance in the period prior to their school entrance.

In Panel A of Table 8, we report the 1910-1930 IPUMS school attendance results separately by quartiles of 1910 county level Black school attendance rates.[47] For those in the lowest quartile, the effect of Rosenwald exposure on rural Black children is 0.27 higher (relative to White rural children). The effects decline monotonically, with small and insignificant point estimates computed for the higher half of pre-Rosenwald Black attendance counties. Notably, there is little evidence that such a pattern shows up across pre-Rosenwald White attendance rates (panel B). These results are consistent with school attendance increasing primarily where relevant schooling was lacking, possibly due to high costs, and where there was substantial room for progress. We find similar patterns in the Black rural minus Black urban gap (not shown).

These patterns appear in other outcomes as well. Panels E thru J show the results for the three main World War II sample outcomes – education, high school completion, and AGCT scores – followed by education

[46] Cameron and Taber (2004) find no support for this conjecture when they compare IV estimates of the returns to schooling that use direct costs of schooling as opposed to costs based on measures of foregone earnings. They caution that their results apply only to the existing institutional arrangements for higher education in the modern US setting.

[47] For this exercise, we use our baseline specification from column (4) of Table 2, where the full sample effect of Rosenwald exposure on the Black rural-urban gap is 0.054 and the effect on the Black-White rural gap is 0.11.

and wages from the 1940-50 Censuses.[48] Perhaps most strikingly, in Panel G, we find a 14 point effect on AGCT test scores at the bottom quartile, equal to about 0.6 standard deviations (0.8 standard deviations for blacks). In contrast, the effects are only about 3 to 4 points, or about 0.12 to 0.17 standard deviations, in counties in the top three quartiles.

B. By demographics

Finally, panels C and D show the school attendance results when we stratify by age and gender. The top row suggests that rural Blacks improved relative to urban Blacks up to age 13. However, when the control group is rural Whites, the improvements are largest for 14 to 17 year olds. This may reflect that school attendance actually fell in rural areas during the 1920s, and fell for this age group in particular, due to poor conditions in the farm economy. It appears that despite this environment, rural Blacks continued to make relative progress compared to rural Whites but not when compared to urban Blacks.[49] Panel D shows that the program's effects were statistically similar for males and females.

Overall, the evidence in Table 8 strongly suggests that students schooled in areas with the lowest initial schooling levels benefited the most from access to the Rosenwald schools.

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 certain 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 explains this rapid convergence, we show that the Rosenwald Rural Schools Initiative was a primary contributor, explaining at least 30 percent and perhaps up to half of the narrowing of the racial education gap among the cohorts that we study. Incomes rose in line with educational improvements, with private rates of

[48] We used 1920 instead of 1910 as the basis for stratification for the WWII data since over 80 percent of the sample entered school in 1920 or later.

[49] The triple difference (B-W rural – B-W urban), which is not shown, does not vary as much by age with an effect of 0.08 for 7 to 10 year olds, 0.06 for 11 to 13 year olds and 0.09 for 14 to 17 year olds.

return of as high as 20 percent for those who remained in the South. 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 improvement to school inputs are introduced to relatively deprived environments. This conclusion is highlighted by the especially large gains measured in communities that were contending with the worst pre-Rosenwald educational conditions. The enormous gains made by Southern Blacks who went through the Rosenwald schools may have been obscured by other institutional factors that generally impeded Black economic progress. Nevertheless, Rosenwald schools undoubtedly increased the supply of human capital among Blacks reared in the South.

More generally, the gains in human capital acquisition due to the availability of schooling likely had implications for economic development in the 20th century South, as well as the US economy in general. While beyond the scope of the current paper, we view understanding 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.

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

	(1)			(2)	(3)	(4)
	Baseline, no contols			**Controls**	**County, F.E. No Controls**	**County, F.E. Controls**
	No Rosenwald	*Rosenwald*	Rose - No Rose Difference	Rose - No Rose Difference	Rose - No Rose Difference	Rose - No Rose Difference
Black Rural	0.657	0.732	0.076	0.058	0.073	0.060
	[0.006]***	[0.005]***	[0.007]***	[0.007]***	[0.008]***	[0.008]***
White Rural	0.834	0.834	0.001	-0.001	-0.004	-0.002
	[0.004]***	[0.002]***	[0.004]	[0.004]	[0.006]	[0.006]
Black Urban	0.764	0.791	0.027	0.024	0.017	0.018
	[0.008]***	[0.005]***	[0.009]***	[0.008]***	[0.010]	[0.009]*
White Urban	0.849	0.863	0.014	0.012	-0.010	-0.002
	[0.007]***	[0.004]***	[0.008]*	[0.006]*	[0.007]	[0.007]
Difference in Difference						
Black, Rural-Urban			0.049	0.035	0.056	0.042
			[0.011]***	[0.010]***	[0.010]***	[0.010]***
White, Rural-Urban			-0.013	-0.014	0.006	0.000
			[0.008]	[0.006]**	[0.006]	[0.005]
B-W Rural			0.075	0.06	0.077	0.061
			[0.007]***	[0.006]***	[0.006]***	[0.006]***
B-W Urban			0.013	0.011	0.026	0.020
			[0.011]	[0.009]	[0.010]***	[0.009]**
Triple Difference						
B-W Rural - B-W Urban			0.062	0.048	0.051	0.041
			[0.012]***	[0.011]***	[0.011]***	[0.011]***
N			589198	580565	589198	580565

Notes: Samples include children between the ages of 7 and 17 in the 1910, 1920 and 1930 IPUMs. Dependent variable is school attendance. "Rosenwald" indicates the presence of a Rosenwald school in one's county as of the Census year. 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 2: School Attendance Effects of Rosenwald Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
Difference in Difference						
Black, Rural-Urban	0.061	0.056	0.066	0.054	0.074	0.051
	[0.018]***	[0.020]***	[0.021]***	[0.015]***	[0.081]	[0.080]
White, Rural-Urban	-0.023	-0.013	-0.005	-0.009	0.002	-0.004
	[0.010]**	[0.010]	[0.011]	[0.008]	[0.033]	[0.033]
B-W Rural	0.121	0.12	0.12	0.113	0.09	0.075
	[0.012]***	[0.012]***	[0.011]***	[0.011]***	[0.045]**	[0.045]*
B-W Urban	0.037	0.050	0.048	0.051	0.018	0.020
	[0.013]***	[0.013]***	[0.013]***	[0.013]***	[0.075]	[0.074]
Triple Difference						
B-W Rural - B-W Urban	0.084	0.07	0.072	0.063	0.072	0.055
	[0.016]***	[0.017]***	[0.017]***	[0.016]***	[0.087]	[0.086]
Baseline Controls	**Y**	**Y**	**Y**	**Y**		
Age-St.-Yr			**Y**	**Y**		
County F.E		**Y**	**Y**	**Y**		
County by Year F.E.				**Y**		
Family F.E					**Y**	
Family F.E, Birth Order						**Y**
N	576400	576400	576400	576400	437320	437320

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

	Effects of School Presence in County				Effects of Rosenwald Exposure	
	(1)			(2)	(3)	(4)
	Baseline, no contols			Cty F.E., controls	Cty F.E., controls	Cty-yr F.E., controls
	No Rosenwald	*Rosenwald*	Rose - No Rose Difference	Rose - No Rose Difference	Effect of Exposure	Effect of Exposure
Black Rural	0.764	0.835	0.071	0.065	0.174	0.159
	[0.006]***	[0.004]***	[0.007]***	[0.006]***	[0.015]***	[0.014]***
White Rural	0.980	0.974	-0.006	-0.002	-0.032	-0.032
	[0.003]***	[0.001]***	[0.003]*	[0.003]	[0.006]***	[0.008]***
Black Urban	0.924	0.937	0.013	0.015	0.024	0.023
	[0.008]***	[0.005]***	[0.009]	[0.008]*	[0.012]**	[0.011]**
White Urban	1.015	0.997	-0.018	-0.013	-0.046	-0.041
	[0.004]***	[0.001]***	[0.004]***	[0.004]***	[0.009]***	[0.010]***
Difference in Difference						
Black, Rural-Urban			0.058	0.050	0.150	0.136
			[0.010]***	[0.009]***	[0.019]***	[0.017]***
White, Rural-Urban			0.012	0.012	0.014	0.010
			[0.005]***	[0.003]***	[0.007]**	[0.007]
B-W Rural			0.077	0.067	0.206	0.191
			[0.006]***	[0.006]***	[0.016]***	[0.015]***
B-W Urban			0.031	0.028	0.070	0.065
			[0.009]***	[0.008]***	[0.015]***	[0.014]***
Triple Difference						
B-W Rural - B-W Urban			0.046	0.039	0.136	0.126
			[0.011]***	[0.010]***	[0.020]***	[0.019]***
N						

Notes: Samples includes individuals between the ages of 15 and 22 in the 1910, 1920 and 1930 IPUMs. Dependent variable is literacy. "Rosenwald" indicates 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 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 Presence in County Using Initial Location of Schools in Alabama

Panel A: Effects of County Rosenwald Presence (in Alabama) on Black-White School Attendance
Using Counties along both sides of the Alabama border

	OLS					County Fixed Effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	AL/GA Border	AL/FL Border	AL/MS Border	AL/TN Border	**All Borders**	AL/GA Border	AL/FL Border	AL/MS Border	AL/TN Border	**All Borders**
cohorts	0.110	-0.030	0.015	0.230	**0.078**	0.139	0.006	0.034	0.218	**0.094**
<=1908	[0.054]*	[0.157]	[0.071]	[0.091]**	**[0.042]***	[0.052]**	[0.167]	[0.070]	[0.104]*	**[0.041]****
N	2678	859	2279	1394	**6907**	2678	859	2279	1394	**6907**
cohorts	0.100	-0.097	0.019	0.426	**0.091**	0.135	-0.063	0.036	0.388	**0.109**
<=1906	[0.054]*	[0.136]	[0.090]	[0.062]***	**[0.055]**	[0.056]**	[0.147]	[0.089]	[0.077]***	**[0.052]****
N	2389	761	2001	1266	**6148**	2389	761	2001	1266	**6148**

Panel B: Effects of County Rosenwald Presence on Black-White School Attendance
for counties within X many miles of Montgomery

	OLS				County Fixed Effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	20 miles	40 miles	60 miles	75 miles	20 miles	40 miles	60 miles	75 miles
effect on	0.203	0.109	0.094	0.080	0.203	0.096	0.097	0.078
B-W gap	[0.061]	[0.073]	[0.039]**	[0.038]**	[0.061]	[0.089]	[0.043]**	[0.040]*
N	411	866	1920	3367	411	866	1920	3367

Notes: Estimates show the effect of the presence of a Rosenwald schools in one's county in Alabama by 1919 on black school attendance relative to whites. Panel A uses counties contiguous to Alabama's borders on both sides of Alabama. The controls include a dummy for 1910, age dummies, female dummy, father's and mother's literacy, white adult literacy in the county in 1910 (cols 1-4), father's occupational score and father's home ownership and state dummies (cols 1-4). Regressions also control for the presence of a Rosenwald school in non-Alabama counties interacted with being black. Panel B shows the effect only using counties within a specified distance from Montgomery Alabama where we think school location was idiosycratic. 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 Using Rosenwald Exposure Based on State or County of Birth

Panel A: 1910-1930 Census outcomes using state of birth

	(1)	(2)	(3)
	Schooling		Literacy
Blacks	0.167	0.202	0.313
	[0.057]**	[0.102]*	[0.111]**
Whites	-0.015	-0.022	-0.098
	[0.052]	[0.067]	[0.043]**
B-W Difference	0.183	0.224	0.411
	[0.034]***	[0.053]***	[0.092]***
Birth Year <1918	N	Y	Y
N	592472	352230	403058

Panel B: WWII data outcomes using st. of birth

	(1)	(2)	(3)
	Education	Comp. H.S.	AGCT Score
Blacks	1.017	0.192	13.204
	[0.481]*	[0.068]**	[3.486]***
Whites	-0.358	-0.027	-4.56
	[0.150]**	[0.023]	[3.022]
B-W Difference	1.375	0.219	17.765
	[0.598]**	[0.084]**	[4.781]***
Birth Year <1918	N	N	N
N	2294340	2617232	139784

Panel C: WWII data outcomes, county of residence vs. county of birth

	(1)	(2)	(3)	(4)	(5)	(6)
	Education			Completed HS		
Blacks	0.301	0.282	0.108	0.05	0.057	0.023
	[0.163]*	[0.146]*	[0.173]	[0.021]**	[0.027]**	[0.027]
Whites	-0.121	-0.507	-0.541	-0.003	-0.064	-0.061
	[0.043]***	[0.190]***	[0.203]***	[0.006]	[0.033]*	[0.034]*
B-W Difference	0.422	0.788	0.649	0.052	0.121	0.084
	[0.187]**	[0.252]***	[0.223]***	[0.023]**	[0.039]***	[0.033]**
Cty. Exp. Measure	Residence	Residence	**Birth**	Residence	Residence	**Birth**
Sample type	Full	Restricted	Restricted	Full	Restricted	Restricted
N	2042743	17286	17146	2081422	17693	17549

Panel D: Outcomes from 1940-50 Census using state of birth

	(1)	(2)	(3)	(4)	(5)	(6)
	Education		Log Income		Log Wages	
Blacks	1.411	1.703	0.415	0.217	0.414	0.396
	[0.383]***	[0.665]**	[0.062]***	[0.139]	[0.105]***	[0.090]***
Whites	-0.663	-0.172	-0.189	-0.324	-0.117	-0.184
	[0.162]***	[0.172]	[0.074]**	[0.072]***	[0.064]*	[0.067]**
B-W Difference	2.074	1.875	0.605	0.541	0.531	0.580
	[0.343]***	[0.558]***	[0.085]***	[0.136]***	[0.085]***	[0.107]***
Birth Year <1918	N	Y	N	Y	N	Y
N	216782	139823	103744	68044	78601	53306

Notes: Sample for Panel A, columns 1 and 2 uses 7 to 17 year olds in the 1910 to 1930 IPUMS who were born in a Rosenwald state. Panel A column 3 uses 15 to 22 year olds in the 1910 to 1930 IPUMS who were born in a Rosenwald state. Panel B uses the World War II enlistment records and includes men between the ages of 17 and 45 who were born in a Rosenwlad state. Panel C uses the World War II enlistment records and includes men between the ages of 17 and 45 but uses only a subset of men who were residing in Rosenwald counties who could be matched to SSA death records, and who provided SSA a county or city of birth that is easily matched. Panel D includes individuals between the ages of 18 and 40 in the 1940 and 1950 IPUMs samples who were born in a Rosenwald state. All regressions include state fixed effects.

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

Table 6: Effects of Rosenwald Exposure on Human Capital, Height and Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Education		Completed High School	AGCT Scores	AGCT cond'l on ed.	Height	Weight
	No IPW	IPW	IPW	IPW	IPW	IPW	IPW
All Counties							
Black	0.260	0.388	0.053	3.497	-1.020	0.032	-1.107
	[0.166]	[0.129]***	[0.017]***	[2.203]	[2.249]	[0.084]	[0.617]*
White	-0.159	-0.085	-0.003	-0.286	-0.153	0.070	-0.202
	[0.040]***	[0.039]**	[0.006]	[1.298]	[1.203]	[0.071]	[0.395]
Black - White	0.419	0.474	0.057	3.783	-0.866	-0.038	-0.905
	[0.193]**	[0.147]***	[0.019]***	[1.644]**	[1.882]	[0.074]	[0.562]
N	1739680	1739680	1762789	89230	88143	867665	867665
Rural Counties							
Black	0.640	0.689	0.092	6.635	-0.245	0.021	-0.618
	[0.127]***	[0.114]***	[0.015]***	[1.976]***	[1.700]	[0.097]	[0.604]
White	-0.168	-0.113	-0.010	0.271	0.955	0.116	-0.561
	[0.031]***	[0.035]***	[0.005]**	[1.111]	[0.833]	[0.079]	[0.465]
Black - White	0.808	0.802	0.102	6.364	-1.200	-0.094	-0.057
	[0.143]***	[0.130]***	[0.017]***	[1.785]***	[1.526]	[0.079]	[0.493]
N	1264087	1264087	1280344	62551	61766	634009	634009
Urban Counties							
Black	-0.208	-0.223	-0.023	-0.604	-1.722	-0.050	-1.017
	[0.242]	[0.204]	[0.028]	[3.645]	[3.604]	[0.151]	[1.002]
White	-0.192	-0.109	0.001	-1.092	-1.983	-0.105	0.697
	[0.086]**	[0.106]	[0.015]	[2.847]	[2.729]	[0.142]	[0.682]
Black - White	-0.016	-0.115	-0.025	0.488	0.261	0.055	-1.714
	[0.300]	[0.247]	[0.032]	[2.312]	[2.607]	[0.104]	[0.826]**
N	475593	475593	482445	26679	26377	233656	233656
B-W Rur - B-W Urb	0.824	0.917	0.127	5.875	-1.467	-0.149	1.657
	[0.331]**	[0.278]***	[0.036]***	[2.918]**	[3.027]	[0.130]	[0.958]*
N	1739680	1739680	1762789	89230	88143	867665	867665

Notes: Sample is drawn from World War II enlistment records and includes men between the ages of 17 and 45. 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, age dummies interacted with race, and county fixed effects. Columns 2 through 7 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 7: Effects of Rosenwald Exposure on Long-Term Outcomes (1940-50 Census)

Panel A: Rosenwald Exposure on Education, Earnings and Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Education		Log Earnings		Log Wage		Pr of Non Wage Inc	
	All	Stayers	All	Stayers	All	Stayers	All	Stayers
Black	1.133	1.230	0.220	0.259	0.196	0.223	0.066	0.088
	[0.192]***	[0.196]***	[0.054]***	[0.055]***	[0.056]***	[0.059]***	[0.020]***	[0.028]***
White	-0.418	-0.349	-0.228	-0.171	-0.186	-0.134	0.002	0.003
	[0.103]***	[0.118]***	[0.042]***	[0.044]***	[0.033]***	[0.036]***	[0.017]	[0.018]
Black - White	1.551	1.580	0.448	0.431	0.382	0.357	0.065	0.085
	[0.207]***	[0.200]***	[0.058]***	[0.056]***	[0.060]***	[0.058]***	[0.017]***	[0.024]***
N	183331	143764	69131	52117	64156	48341	82679	65390
Mean for Blacks	6.017	5.943	5.620	5.563	-1.625	-1.684	0.166	0.170

Panel B: Rosenwald Exposure on Migration

	(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
Black	0.009	0.025	-0.004	0.021	0.014	0.014
	[0.005]	[0.010]**	[0.010]	[0.014]	[0.017]	[0.024]
White	0.005	0.000	0.009	0.013	0.014	0.002
	[0.006]	[0.007]	[0.010]	[0.013]	[0.015]	[0.011]
Black - White	0.003	0.025	-0.013	0.008	0.001	0.011
	[0.007]	[0.012]**	[0.013]	[0.017]	[0.019]	[0.024]
N	68044	35750	54521	68044	35750	54521
Mean for Blacks	0.008	0.014	0.022	0.035	0.052	0.070

Notes: Sample for Panel A includes individuals between the ages of 18 and 40 in the 1940 and 1950 IPUMs samples living in a Rosenwald state. 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 in Panel A include a female dummy, female*black dummy, age interacted with state interacted with year, and state economic area by year fixed effects. The 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 or are shown in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Results Based on Stratified Samples

Census Results on School Attendance Using Stratified Samples

	A. Attendance by 1910 Black Attendance				*B. Attendance by 1910 White Attendance*			
	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
B-W	0.272	0.158	0.062	-0.031	0.087	0.098	0.123	0.138
Rural	[0.029]***	[0.022]***	[0.016]***	[0.020]	[0.023]***	[0.020]***	[0.020]***	[0.024]***
	108038	131112	153969	199813	144712	169304	162597	116319

	C. Attendance by Age			*D. Attendance by Sex*	
	7 to 10	11 to 13	14 to 17	Male	Female
Black	0.093	0.087	0.03	0.056	0.048
Rural-Urban	[0.016]***	[0.017]***	[0.027]	[0.020]***	[0.018]***
	226811	155256	202599	294601	290044
B-W	0.105	0.1	0.15	0.116	0.11
Rural	[0.013]***	[0.014]***	[0.019]***	[0.013]***	[0.013]***
	226811	155256	202599	294601	290044

World War II Enlistment Records Results Using Stratified Samples

	E. Education by 1920 Black Attendance				*F. Complete H.S. by 1920 Black Attendance*			
	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
B-W	1.093	0.686	0.65	0.613	0.104	0.086	0.089	0.094
Rural	[0.355]***	[0.264]**	[0.229]***	[0.224]***	[0.046]**	[0.035]**	[0.028]***	[0.027]***
	250160	254840	311053	448034	253539	257682	317613	451510

	G. AGCT Scores by 1920 Black Attendance			
	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
B-W	14.214	3.743	3.897	2.951
Rural	[4.302]***	[3.772]	[3.026]	[3.742]
	12400	12195	16190	21766

1940-50 Census Results Using Stratified Samples

	I. Education by 1920 Black Attendance				*J. Log Wage by 1920 Black Attendance*			
	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
B-W	1.915	1.365	1.359	1.383	0.657	0.344	0.357	0.293
	[0.360]***	[0.417]***	[0.306]***	[0.338]***	[0.111]***	[0.113]***	[0.109]***	[0.100]***
	42550	55542	41170	44069	12646	20910	14321	16279

Notes: Panels A through D are drawn from 1910 to 1930 IPUMS and use the specification shown in column (4) of Table 2. Panels E through G use World War II enlistment records and use IPW (see Table 6). Panels I through J use the baseline specification from Table 7 Panel A. Standard errors clustered by county (or SEA) are shown in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

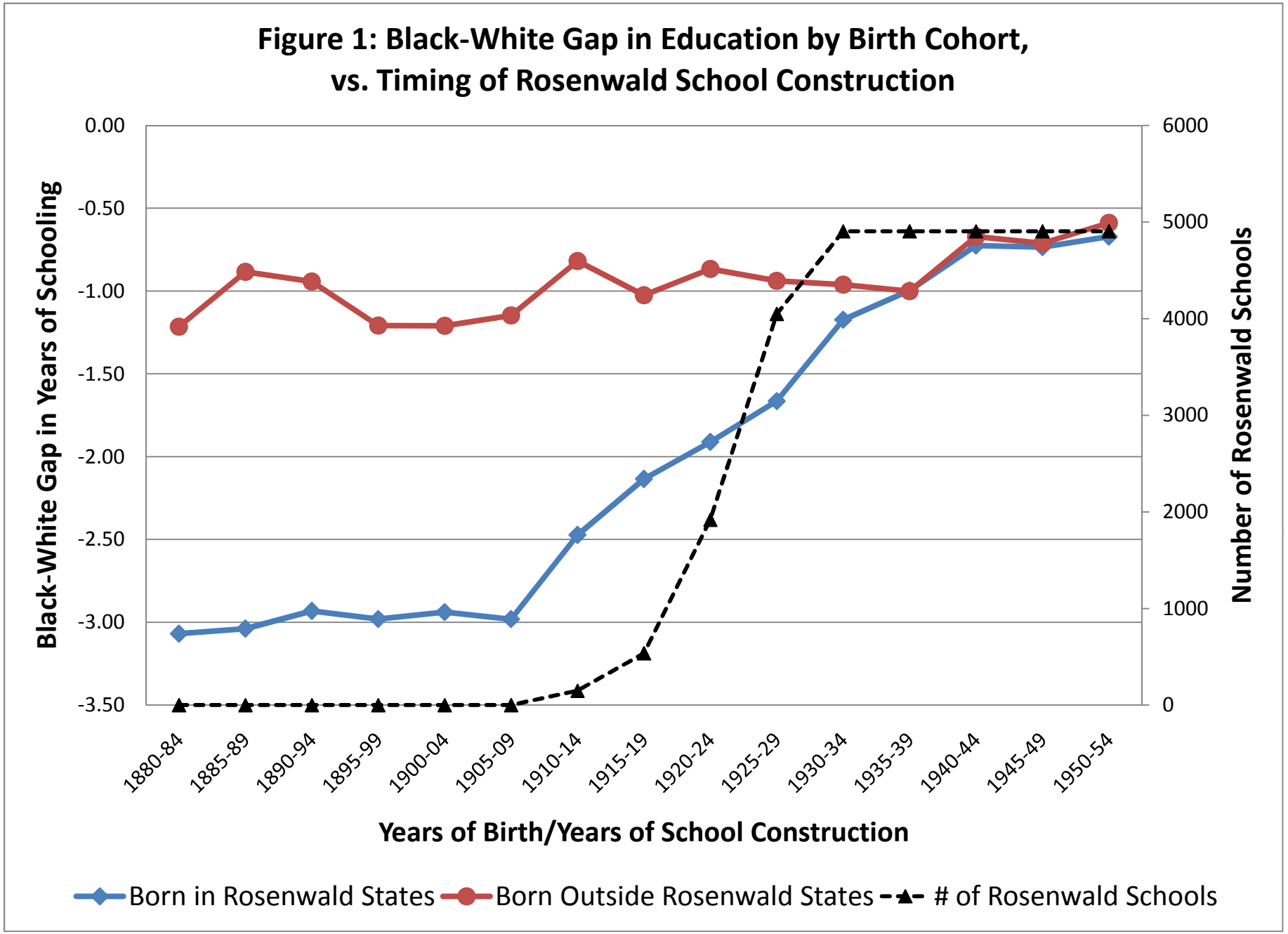


Figure 1: Black-White Gap in Education by Birth Cohort, vs. Timing of Rosenwald School Construction
Black-White Gap in Years of Schooling
0.00
-0.50
-1.00
-1.50
-2.00
-2.50
-3.00
-3.50
Number of Rosenwald Schools
6000
5000
4000
3000
2000
1000
0
1880-84
1885-89
1890-94
1895-99
1900-04
1905-09
1910-14
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 2: Coverage of Rosenwald Schools by County as of 1919

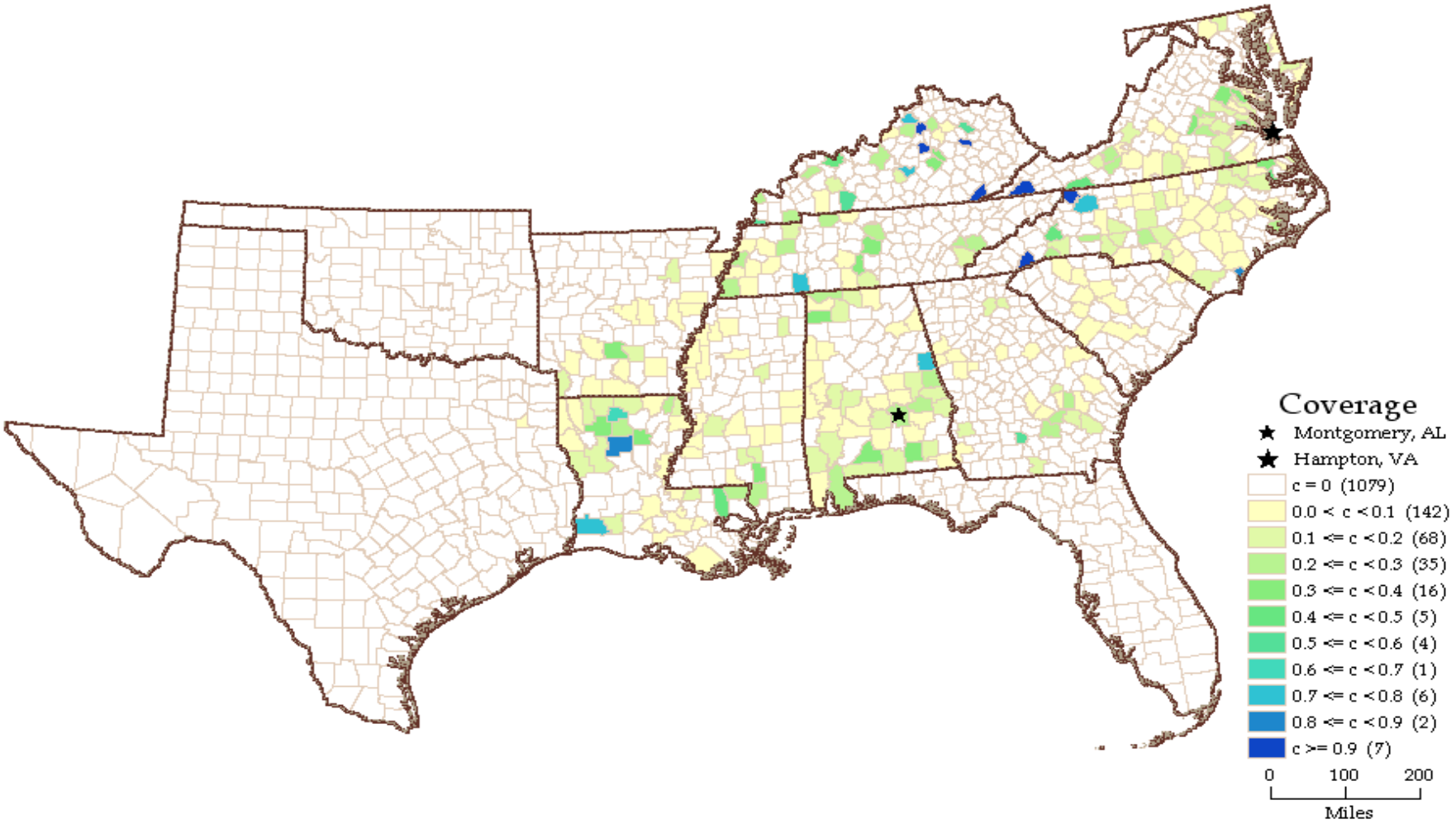


Figure 3: Coverage of Rosenwald Schools by County as of 1931

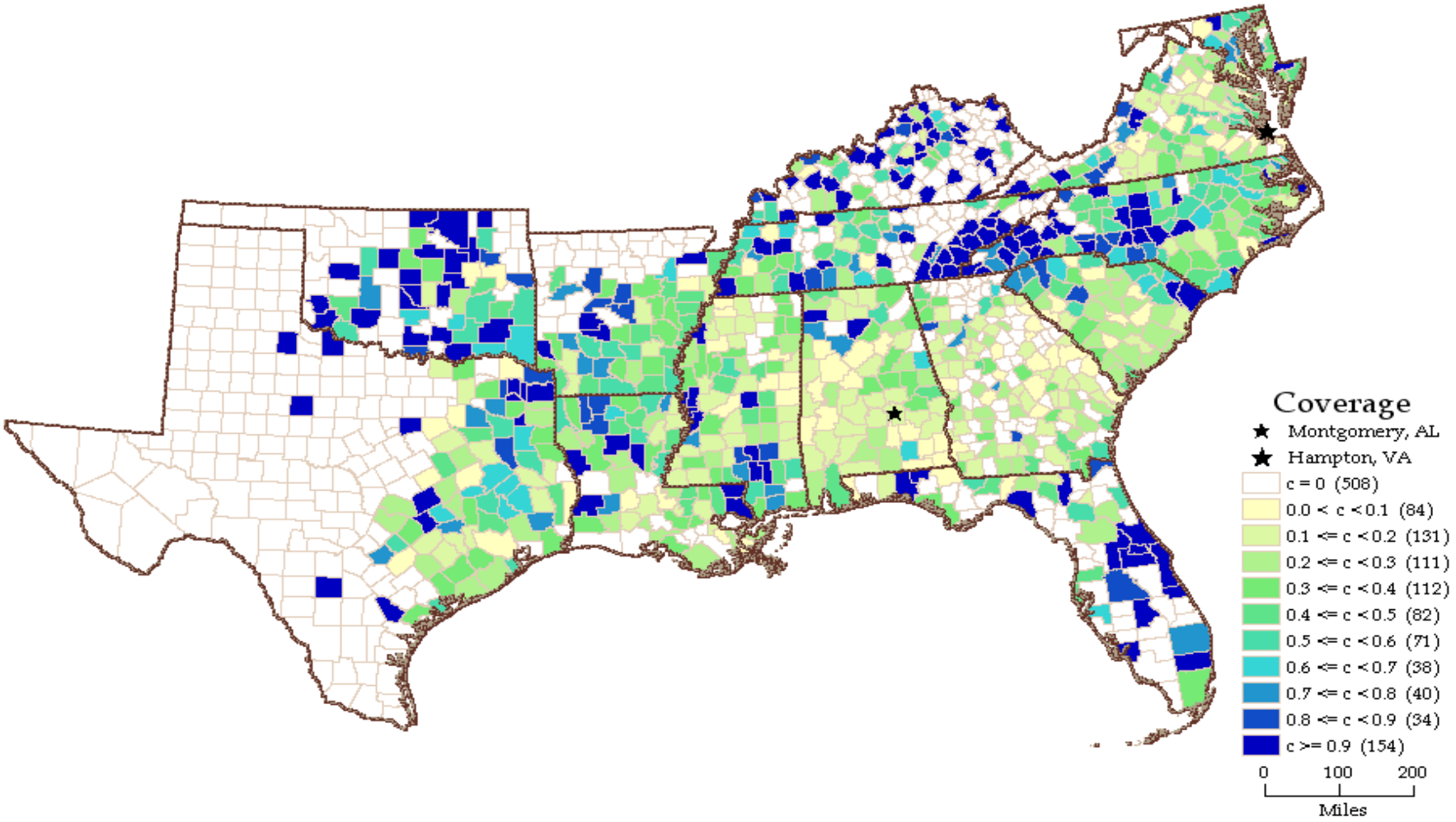


Figure 4: Distribution of Rosenwald Share of Rural Black School Age Children Across Counties

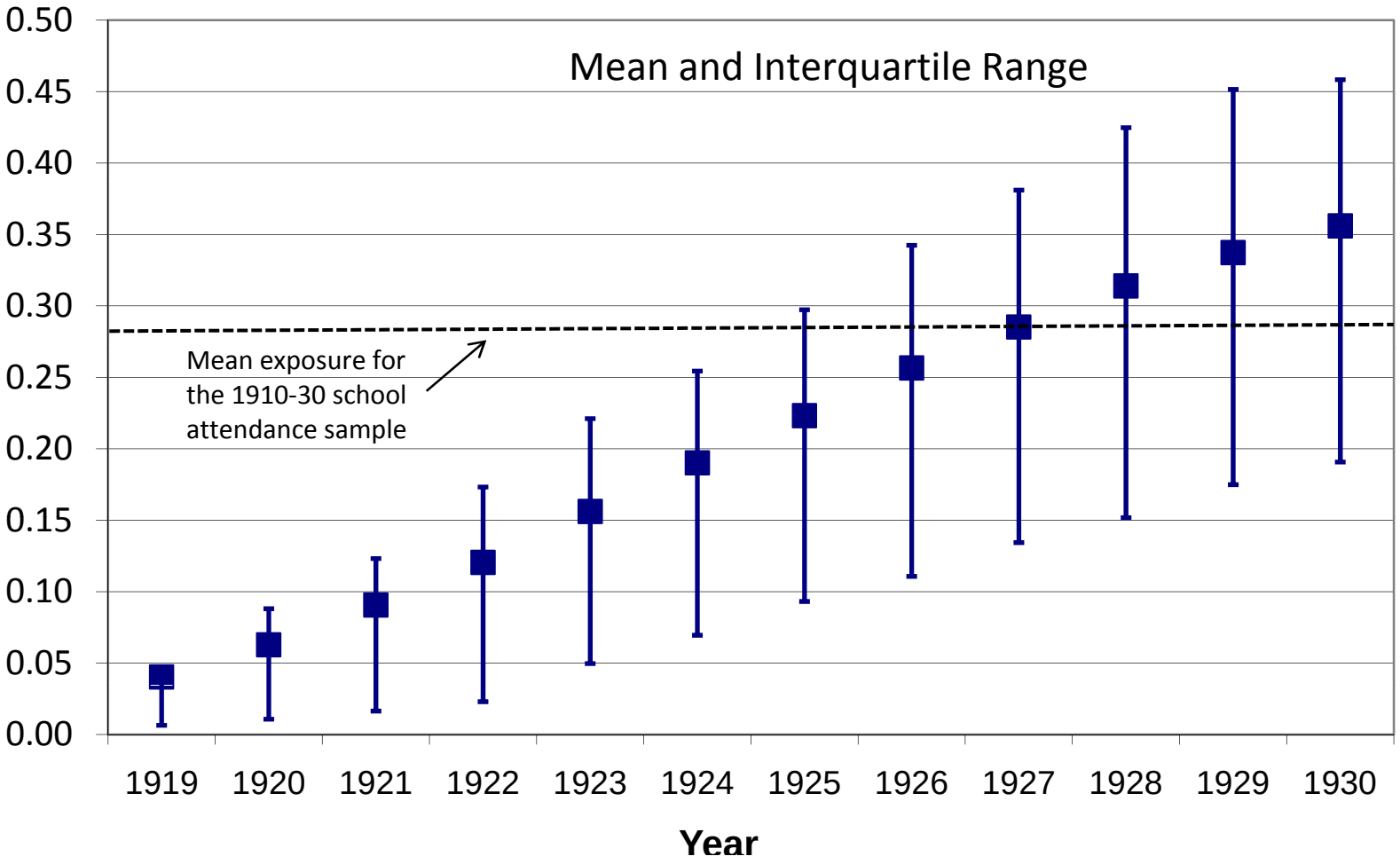
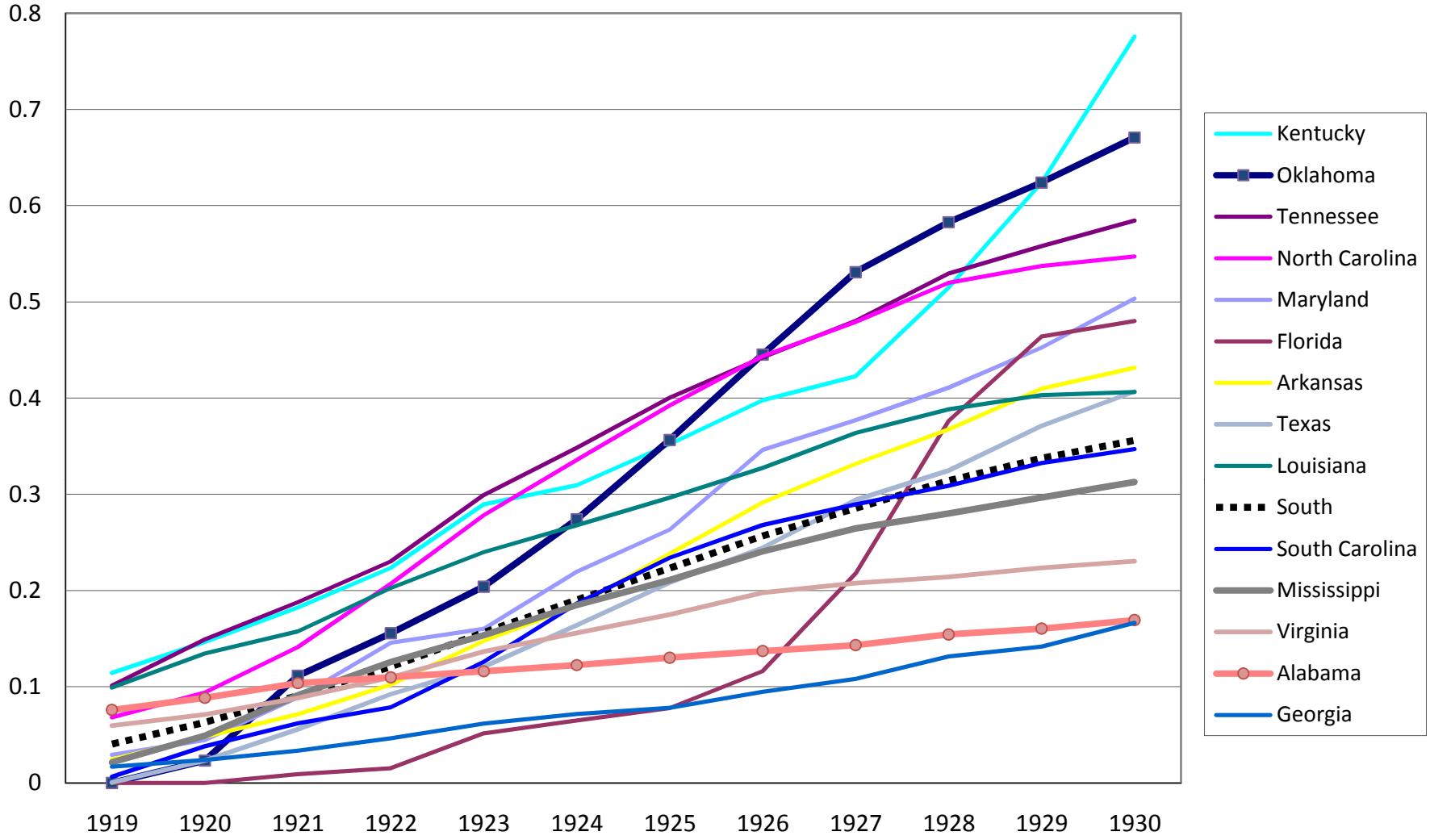


Figure 5: Estimated Share of Black Rural School Age Children in Rosenwald Schools by State and Year



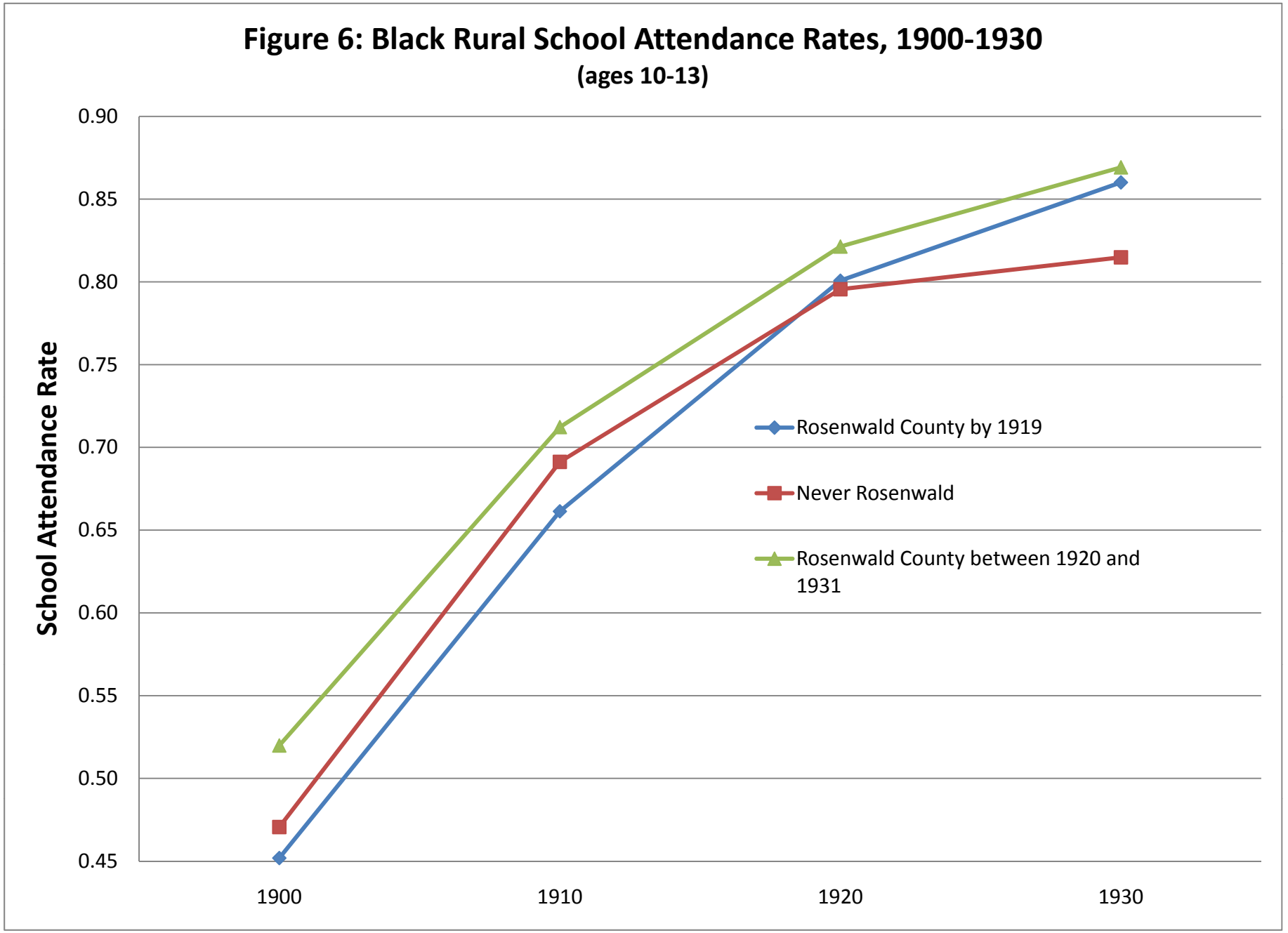


Figure 6: Black Rural School Attendance Rates, 1900-1930
(ages 10-13)
School Attendance Rate
0.90
0.85
0.80
0.75
0.70
0.65
0.60
0.55
0.50
0.45
1900
1910
1920
1930
Rosenwald County by 1919
Never Rosenwald
Rosenwald County between 1920 and 1931

Appendix A: School Location Selection

This appendix describes a set of regressions that show the association of pre-Rosenwald county characteristics with school location decisions. Pre-Rosenwald county characteristics primarily come from the 1910 Census and include race-specific measures of educational and economic status such as school enrollment, literacy, and occupational status.[1] The regressions also incorporate state fixed effects and industry share controls. Some specifications are augmented to include non-Census measures reported in a Southern almanac (Johnson, 1941) that contains county-level information on various demographic and economic measures, including lynching incidents between 1900 and 1931 per capita, ownership status of farms by race, and a list of top agricultural crops.[2] These measures tend to be from around 1930, and therefore could be better thought of as potential outcomes of Rosenwald but we include them as simple tests of model robustness.

Table A4 shows the results. In the first three columns, labeled "First school built by 1919", the dependent variable is the presence of a Rosenwald school in a county by 1919. We find very little evidence, on average, that 1910 Black socio-economic characteristics have a statistically significant or economically large impact on eventual school location. Moreover, in other specifications (not shown), we also look at whether *changes* between 1900 and 1910 in the key characteristics of Whites and Blacks predict the location of Rosenwald schools. Those results likewise show that initial schools were not more likely to be built in counties where Blacks had made socio-economic progress prior to the Rosenwald period. This provides some comfort that some of our key outcomes when measured prior to the Rosenwald period, do not appear to be significantly correlated with the location of where schools were built in the 1910s, suggesting limited scope for the inotion that our results could be due to reverse causality. The results are generally similar whether we include the 1930 Southern Almanac measures of

[1] The occupational status measures are provided by IPUMS and are based on 1950 levels of income and education by occupation.
[2] This data excludes Oklahoma.

lynchings, farm ownership, and cotton farming intensity, or pre-Rosenwald political participation measures (not shown) generously supplied by Kenneth Chay and Kaivan Munshi.[3]

That said, one intriguing finding is that a 10 percentage point increase in a county's 1910 White literacy rate is associated with a 5 to 6 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, the industrial composition of the White workforce has no 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 suggestive evidence that schools were concentrated in areas with better socio-economic characteristics. This is clear from a comparison of columns (4) to (6) versus (7) to (9). Columns (4) to (6) reports regressions of the Rosenwald exposure 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 (columns 7 to 9), there is some marginally statistically significant evidence that counties with higher Black occupational status and literacy in 1910 had greater Rosenwald exposure.

Although this finding could present a threat to the identification of causal effects, our econometric strategy (use of county fixed effects and county by year fixed effects) is robust to this source of bias. 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. Moreover, the results in Table A4 provide further support for using the location of pre-1919 Alabama schools (see section VI) to identify causal Rosenwald effects and, perhaps, more generally exploiting pre-1919 schools across the South.

[3] We have included these both separately and jointly.

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 for the electronic records 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. For example, Figure A2 plots the mean and standard deviation of the data contained in the "weight" field for a 40 week period in 1943 for all enlistees in New York City. 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 A3 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.

Table A1: Descriptive Statistics about Rosenwald School Projects by School

		Mean	Std dev	Min	Max
Panel A: School Building Details					
Rosenwald school		0.993			
Rosenwald teacher homes		0.044			
Rosenwald shops		0.035			
Country Training Schools		0.045			
Fraction of schools with additions		0.062			
Fraction of schools rebuilt		0.006			
Fraction of schools burned		0.013			
N		4968			
Panel B: Cost of Rosenwald Schools in 1925 dollars					
Real Cost of original schools	$	5,374	8083	583	169761
borne by local Blacks	$	882	1021	0	16528
borne by local Whites	$	221	1226	0	39375
borne by local government	$	3,454	7425	0	163473
borne by Rosenwald Fund	$	816	575	58	7859
Fraction of schools with positive contributions from:					
local Blacks		0.92			
local Whites		0.29			
local government		0.97			
Rosenwald Fund		1.00			
Real cost, including changes to schools (additions/rebuilds)	$	5,606	8,346	583	169,761
N		4932			

Notes: Samples are of Rosenwald school projects that could involve multiple buildings including school buildings, teacher homes, shops for industrial education and county training schools. Therefore, the rows in Panel A do not sum to 1. Samples of Rosenwald school buildings refer to those with a known number of teachers and date of construction.

Table A2: Summary statistics of 1910-1930 IPUMS samples

	1910		1920		1930	
	Blacks	Whites	Blacks	Whites	Blacks	Whites
School Enrollment Sample of 7 to 17 year olds (N= 589258)						
School Enrollment						
All Ages	0.60	0.80	0.74	0.85	0.75	0.84
Age 7 to 10	0.61	0.82	0.78	0.91	0.81	0.90
Age 11 to 14	0.69	0.87	0.81	0.92	0.85	0.93
Age 15 to 17	0.45	0.66	0.55	0.65	0.52	0.63
Male	0.57	0.80	0.72	0.85	0.73	0.83
Female	0.64	0.81	0.76	0.86	0.77	0.84
Rural	0.59	0.80	0.72	0.85	0.74	0.83
Urban	0.68	0.81	0.82	0.87	0.79	0.86
Family Characteristics						
Father literate	0.53	0.87	0.63	0.90	0.73	0.93
Mother literate	0.49	0.87	0.66	0.91	0.80	0.95
Father Occ. Score	15.01	19.81	15.47	20.34	15.70	20.63
Father Owned home	0.27	0.57	0.27	0.53	0.27	0.47
Rosenwald Measures						
Presence in County	0.00	0.00	0.49	0.29	0.91	0.73
Exposure (ages 7 to 13)	0.00	0.00	0.02	0.02	0.28	0.29
Geography						
Rural	0.86	0.82	0.82	0.77	0.78	0.74
City Population	9963	20733	17021	30530	26542	35335
Alabama	0.10	0.06	0.11	0.07	0.10	0.07
Arkansas	0.05	0.06	0.06	0.06	0.05	0.06
Florida	0.03	0.02	0.03	0.03	0.04	0.04
Georgia	0.15	0.08	0.14	0.08	0.13	0.07
Kentucky	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
Maryland	0.03	0.05	0.02	0.04	0.02	0.05
Mississippi	0.12	0.04	0.11	0.04	0.11	0.04
North Carolina	0.09	0.08	0.10	0.08	0.11	0.10
Oklahoma	0.01	0.08	0.01	0.09	0.02	0.08
South Carolina	0.12	0.04	0.12	0.04	0.10	0.04
Tennessee	0.05	0.09	0.04	0.09	0.05	0.08
Texas	0.09	0.17	0.08	0.17	0.09	0.16
Virginia	0.07	0.07	0.07	0.07	0.08	0.07
Number of observations	28399	71409	17680	52188	115146	304436
Literacy Sample of 15 to 22 year olds (N = 398388)						
Literacy						
All ages	0.71	0.94	0.80	0.96	0.88	0.98
Age 15 to 17	0.72	0.94	0.82	0.96	0.89	0.98
Age 18 to 22	0.71	0.93	0.79	0.96	0.86	0.98
Rural	0.68	0.92	0.77	0.95	0.85	0.97
Urban	0.85	0.98	0.90	0.98	0.94	0.99
Rosenwald Measures						
Presence in County	0.00	0.00	0.46	0.29	0.91	0.73
Exposure (ages7 to 13)	0.00	0.00	0.00	0.00	0.13	0.13
Number of observations	19439	51355	11998	33623	78477	200029

Table A3: Summary statistics of WWII enlisted men sample

	Pooled			Whites			Blacks		
	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N
Outcomes									
Years of Schooling	9.3	3.0	2091279	9.7	2.9	1653908	7.5	2.8	437371
Completed H.S.	0.55	0.50	2137274	0.62	0.49	1675310	0.32	0.47	461712
AGCT Score	87.7	24.2	97896	91.2	23.2	84353	65.6	17.3	13543
Height	68.4	4.3	1048232	68.5	4.5	834227	68.0	3.3	214005
Weight	148.4	28.1	1048232	148.3	29.3	834227	149.0	22.8	214005
Demographics									
Age	24.40	5.76	2137022	24.39	5.80	1675310	24.46	5.59	461712
Enlisted, 1940	0.05	0.21	2137022	0.06	0.23	1675310	0.01	0.08	461712
Enlisted, 1941	0.10	0.31	2137022	0.11	0.31	1675310	0.08	0.27	461712
Enlisted, 1942	0.39	0.49	2137022	0.39	0.49	1675310	0.38	0.49	461712
Enlisted, 1943	0.20	0.40	2137022	0.19	0.39	1675310	0.26	0.44	461712
Enlisted, 1944	0.11	0.31	2137022	0.11	0.31	1675310	0.10	0.30	461712
Enlisted, 1945	0.11	0.31	2137022	0.11	0.31	1675310	0.12	0.32	461712
Enlisted, 1946	0.05	0.21	2137022	0.04	0.20	1675310	0.06	0.23	461712
Enlistment Probability	0.22	0.19	1800111	0.23	0.18	1409121	0.21	0.20	390990
Rosenwald Measures									
Presence in County	0.79	0.41	2137022	0.75	0.43	1675310	0.91	0.29	461712
Exposure (ages 7 to 17)	0.27	0.33	2099089	0.27	0.33	1647962	0.27	0.30	451127
Geography									
% Rural in County	0.70	0.29	2128618	0.71	0.29	1668473	0.70	0.29	460145
Alabama	0.08	0.27	2137022	0.08	0.27	1675310	0.10	0.30	461712
Arkansas	0.03	0.18	2137022	0.03	0.18	1675310	0.03	0.16	461712
Florida	0.06	0.23	2137022	0.05	0.22	1675310	0.08	0.26	461712
Georgia	0.09	0.29	2137022	0.09	0.28	1675310	0.10	0.30	461712
Kentucky	0.07	0.25	2137022	0.08	0.27	1675310	0.02	0.15	461712
Louisiana	0.04	0.20	2137022	0.03	0.18	1675310	0.07	0.26	461712
Maryland	0.04	0.19	2137022	0.04	0.20	1675310	0.03	0.17	461712
Mississippi	0.07	0.25	2137022	0.05	0.21	1675310	0.13	0.34	461712
North Carolina	0.10	0.30	2137022	0.10	0.30	1675310	0.11	0.31	461712
Oklahoma	0.06	0.23	2137022	0.07	0.25	1675310	0.02	0.14	461712
South Carolina	0.05	0.22	2137022	0.04	0.21	1675310	0.08	0.26	461712
Tennessee	0.09	0.29	2137022	0.10	0.30	1675310	0.07	0.25	461712
Texas	0.16	0.37	2137022	0.18	0.38	1675310	0.11	0.31	461712
Virginia	0.06	0.23	2137022	0.05	0.23	1675310	0.06	0.24	461712

Table A4: Determinants of Location of Rosenwald Schools Using 1910 County Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Counties with Rural Blacks								
	First School Built by 1919			Exposure by 1919			Exposure by 1931		
Rural Black Population (1910)	0.016	0.015	0.014	0.000	0.000	0.000	-0.005	-0.006	-0.005
	[0.003]***	[0.003]***	[0.004]***	[0.001]	[0.001]	[0.001]	[0.002]**	[0.002]**	[0.003]*
Black School Enrollment (1910)	0.001	0.005	-0.008	-0.004	-0.001	-0.002	0.030	0.035	0.041
	[0.057]	[0.063]	[0.062]	[0.018]	[0.019]	[0.020]	[0.045]	[0.046]	[0.046]
Black Literacy (1910)	-0.002	-0.001	-0.004	0.012	0.018	0.019	0.030	0.087	0.087
	[0.072]	[0.083]	[0.082]	[0.022]	[0.026]	[0.026]	[0.056]	[0.061]	[0.061]
Black Occupational Status (1910)	0.006	0.008	0.010	0.002	0.002	0.003	0.008	0.007	0.007
	[0.004]	[0.005]	[0.005]*	[0.001]	[0.002]	[0.002]	[0.004]**	[0.004]*	[0.004]*
Black Occupation Ed. Score (1910)	0.001	0.002	-0.002	0.000	0.000	0.000	-0.002	-0.006	-0.006
	[0.005]	[0.007]	[0.007]	[0.001]	[0.002]	[0.002]	[0.004]	[0.005]	[0.005]
White School Enrollment (1910)	-0.045	-0.020	-0.030	0.017	0.025	0.022	0.057	0.020	0.029
	[0.105]	[0.114]	[0.114]	[0.033]	[0.035]	[0.036]	[0.082]	[0.083]	[0.085]
White Literacy (1910)	0.554	0.568	0.502	0.114	0.127	0.126	0.482	0.532	0.541
	[0.188]***	[0.204]***	[0.207]**	[0.059]*	[0.063]**	[0.065]*	[0.147]***	[0.149]***	[0.154]***
White Occupational Status (1910)	-0.014	-0.015	-0.012	-0.004	-0.004	-0.004	0.000	0.001	0.001
	[0.007]**	[0.007]**	[0.007]*	[0.002]**	[0.002]**	[0.002]*	[0.005]	[0.005]	[0.005]
White Occupational Ed. Score (1910)	0.011	0.012	0.010	0.003	0.003	0.003	0.006	0.005	0.005
	[0.006]*	[0.006]*	[0.006]	[0.002]*	[0.002]	[0.002]	[0.005]	[0.005]	[0.005]
% Teachers (1910)	1.246	1.397	1.467	0.072	0.020	0.035	0.829	0.521	0.537
	[1.306]	[1.485]	[1.472]	[0.410]	[0.461]	[0.463]	[1.027]	[1.090]	[1.094]
Cotton Share>25%			0.128			0.018			0.019
			[0.068]*			[0.022]			[0.051]
Lynching			-0.024			-0.008			-0.005
			[0.030]			[0.009]			[0.022]
Cotton Share>25% * Black Rural			-0.217			-0.005			0.004
			[0.140]			[0.044]			[0.104]
Black Owned Farms			-0.003			0.000			0.002
			[0.001]**			[0.000]			[0.001]*
White Owned Farms			0.001			0.000			-0.001
			[0.001]			[0.000]			[0.001]
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry Share Controls (1910)	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1045	964	964	1038	961	961	1040	961	961

Standard errors in brackets

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

Table A5: School Attendance Effects of Rosenwald Exposure Using Other Specifications

	(1)	(2)	(3)	(4)
Difference in Difference				
Black, Rural-Urban	0.105	0.037	0.087	0.062
	[0.017]***	[0.016]**	[0.019]***	[0.01]***
White, Rural-Urban	0.040	-0.006	0.045	0.001
	[0.011]***	[0.009]	[0.011]***	[0.006]
B-W Rural	0.114	0.077	0.076	0.113
	[0.011]***	[0.013]***	[0.013]***	[0.007]***
B-W Urban	0.049	0.034	0.035	0.052
	[0.013]***	[0.014]**	[0.014]**	[0.009]***
Triple Difference				
B-W Rural - B-W Urban	0.065	0.043	0.041	0.061
	[0.016]***	[0.017]**	[0.018]**	[0.011]***
Baseline Controls	Y	Y	Y	Y
Age-St.-Yr	Y	Y	Y	Y
Age-St.-Rural-Yr	Y		Y	
Age-St.-Race-Yr		Y	Y	
Age-St.-Race-Rural-Yr			Y	
County F.E	Y	Y	Y	Y
County by Year F.E.	Y	Y	Y	Y
Cnty by Year F.E, Age-Cnty-Yr				Y

Notes: Samples include approximately 580,000 children between the ages of 7 and 17 in the 1910, 1920 and 1930 IPUMs samples. 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, father's occupational score and father's home ownership. Estimates use Census sampling weights. Standard errors, clustered on county are shown in brackets except for column (4) which do not use clustering.

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

Table A6: Robustness Checks on Census Results Using Rosenwald Exposure

	School Enrollment				*Literacy*			
Difference in Difference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black, Rur-Urb	0.019	0.012	0.007	0.014	0.049	0.039	0.048	0.035
	0.054	0.034	0.020	0.054	0.136	0.108	0.134	0.136
	[0.015]***	[0.014]**	[0.021]	[0.015]***	[0.017]***	[0.014]***	[0.025]***	[0.017]***
White, Rur-Urb	-0.003	-0.005	-0.001	-0.002	0.004	0.004	0.003	0.003
	-0.009	-0.015	-0.004	-0.009	0.01	0.012	0.007	0.01
	[0.008]	[0.010]	[0.012]	[0.008]	[0.007]	[0.007]*	[0.010]	[0.007]
B-W Rural	0.041	0.037	0.035	0.029	0.069	0.050	0.072	0.050
	0.113	0.104	0.098	0.113	0.191	0.14	0.2	0.191
	[0.011]***	[0.010]***	[0.017]***	[0.011]***	[0.015]***	[0.013]***	[0.021]***	[0.015]***
B-W Urban	0.018	0.020	0.027	0.013	0.023	0.016	0.026	0.017
	0.051	0.055	0.075	0.051	0.065	0.045	0.073	0.065
	[0.013]***	[0.013]***	[0.015]***	[0.013]***	[0.014]***	[0.009]***	[0.019]***	[0.014]***
Triple Difference								
B-W Rur - B-W Urb	0.023	0.018	0.008	0.016	0.045	0.035	0.046	0.033
	0.063	0.05	0.023	0.063	0.126	0.096	0.127	0.126
	[0.016]***	[0.015]***	[0.021]	[0.016]***	[0.019]***	[0.014]***	[0.028]***	[0.019]***
Unweighted		Y				Y		
Use 1% 1930 sample			Y				Y	
Alternative Exposure				Y				Y

Notes: Controls include county by year fixed effects , age interacted with state, female dummy, father's and mother's literacy, father's occupational score and father's home ownership. Bold estimates are the effect size of going from no exposure to the mean level of exposure in 1930. This allows for a meaningful comparison of columns 1-3 with column (4) which uses all blacks (not rural blacks) in the denominator. Standard errors, clustered on county are shown in brackets.

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

Figure A1: Rosenwald Schools in the Alabama Area by 1919

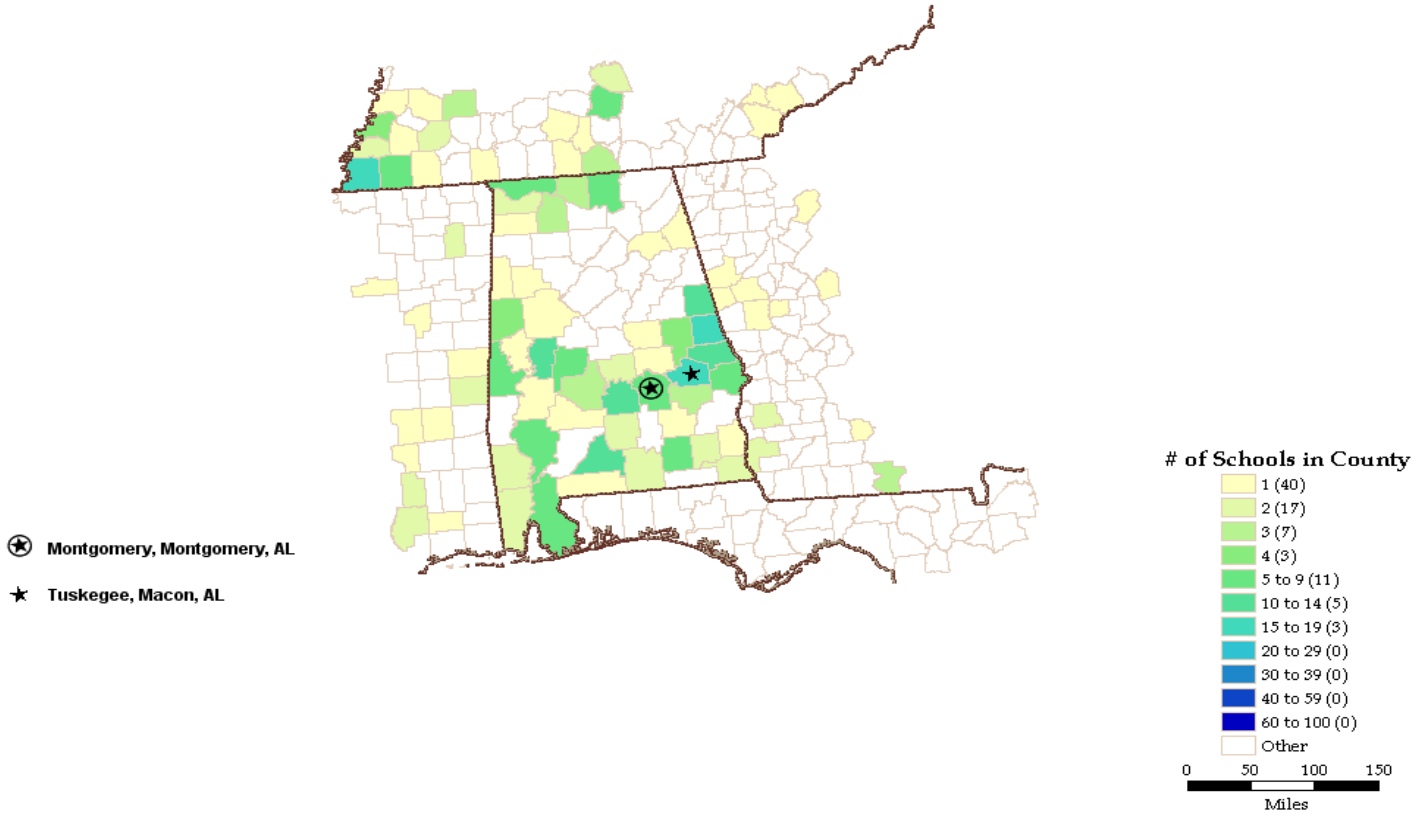


Figure A2: "Weight" Filed for Enlistees in New York City Around March 1943

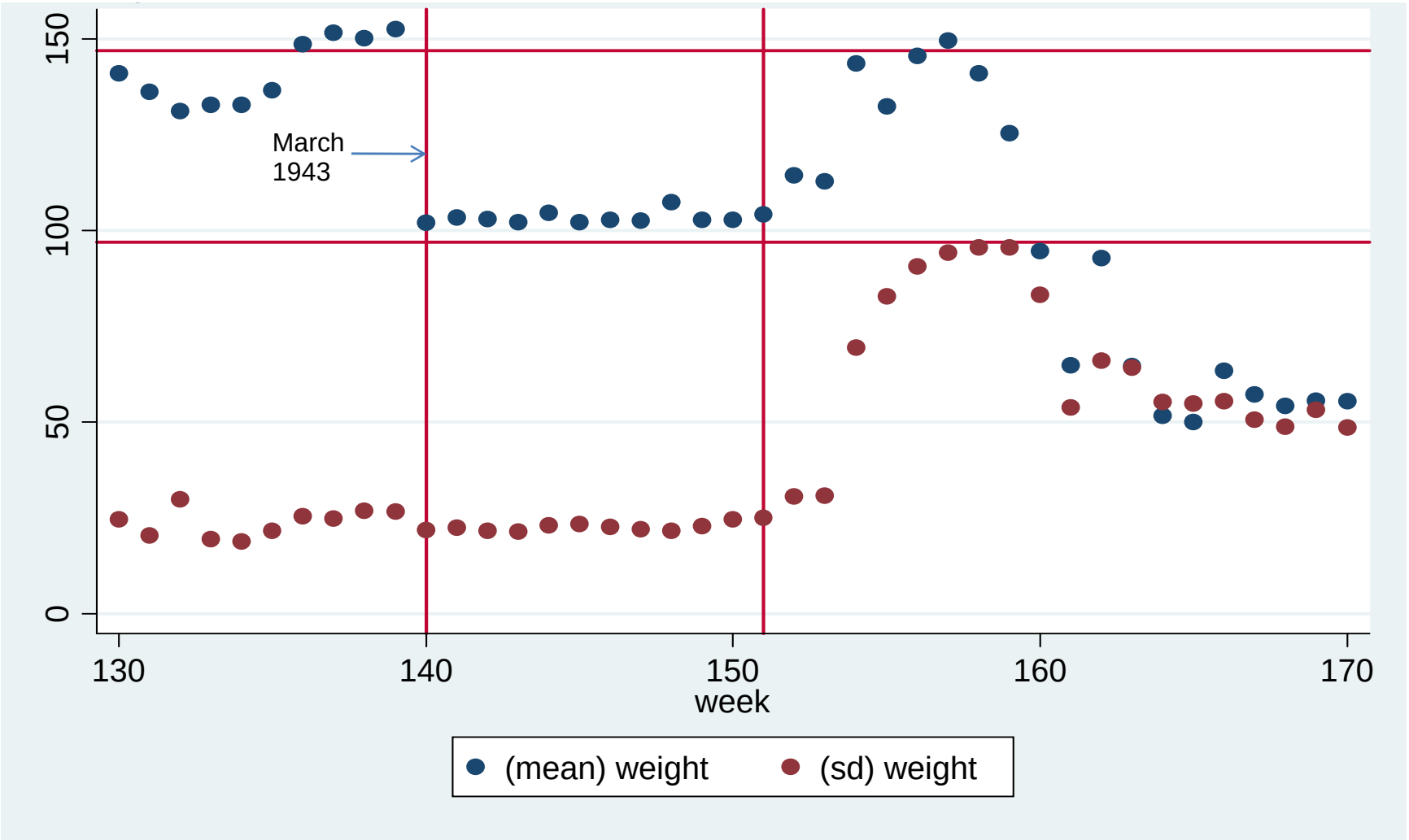
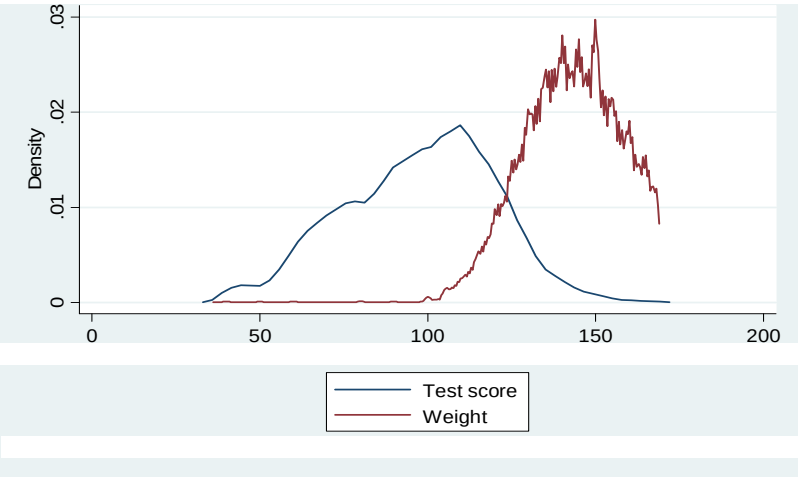
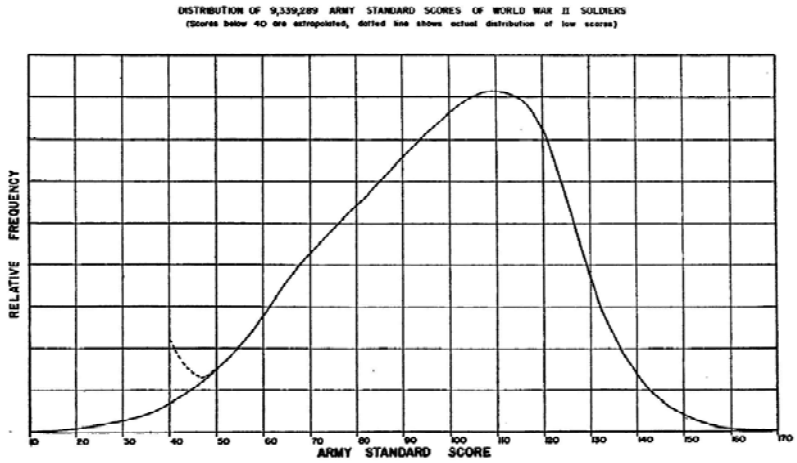


Figure A3: Comparison of AGCT and Weight Data to Historical Sources

Panel A: 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)