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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 from 1880 to 1910. We examine a large scale school construction project, the Rosenwald Rural Schools Initiative, which was designed to dramatically 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. 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 Blacks during the 1910s and 1920s. We also use data from the Army General Classification Test (AGCT), a precursor to the AFQT, and find that access to Rosenwald schools increased average Black scores by about 0.25 standard deviations adding to the existing literature showing that interventions can reduce the racial gap in cognitive skill. In the longer run, exposure to the schools raised the wages of blacks that remained in the South relative to Southern whites by about 35 percent. For blacks the private rate of return to a year of additional schooling induced by Rosenwald was about 18 percent. Moreover, Rosenwald significantly increased Northern migration of young adult Blacks, with no corresponding impact on school-age Blacks or young adult Whites, 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 at the bottom of the skill distribution.

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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, as exists in some developing countries today, where the most rudimentary school inputs may be 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 had been made in the preceding thirty years. Racial schooling gaps remained persistently high relative to the North, and funding inequities appeared to be growing (Margo 1990).

But over a relatively short period between the World Wars, the Southern racial education gap improved dramatically (see figure 1).[1] While Black cohorts born between 1880 and 1910 obtained three years less schooling that their White contemporaries, a generation later the Southern 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 the World Wars. Significant convergence happened along other school quantity (term length) and quality measures over the same time (e.g. Card and Krueger 1992, Donohue, Heckman, and Todd 2002).

In this paper, we evaluate 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 Julius Rosenwald supplied matching grants for the construction of almost 5,000 schoolhouses for Southern Black rural children beginning 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 employed over

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

14,000 teachers, providing classrooms for over a third of the rural South's Black school-age population (and therefore roughly a quarter of all Southern Black school-age children).

The dearth of research on this program has been driven by lack of systematic information about not just who went to the schools but 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 append this data to several large historical samples -- 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.

Our research design is similar to natural experiments, such as Duflo (2001) and Chin (2005), which exploit well-observed variation in birth cohort access to a program. Since the Rosenwald schools were constructed over 18 years, there are considerable differences in school exposure across birth cohorts within counties even in the same Census year, allowing us to control for unobserved fixed and time-varying county characteristics. Variation in exposure to schools over time, combined with the data structure of the IPUMS, further allows us to control for unobservable background by exploiting sibling differences through family fixed effects.

Importantly, we can also take advantage of the explicit targeting of the Rosenwald program to one well-identified demographic group, rural Blacks. This allows us to use other demographic groups, particularly rural Whites and urban Blacks, to remove any confounding effects that might have been correlated with Rosenwald school exposure.

We find economically and statistically significant effects of the program on Black rural school attendance, literacy, school attainment, military test scores, earnings, hourly wages, and South-to-North migration but no impact on rural or urban White students and mixed, and always smaller, effects on Black urban students. Rosenwald can explain about 30 percent of the Black-White education convergence for Southern born men born between 1910 and 1924, and even larger portions of other education outcomes.

[2] Donohue, Heckman, and Todd (2002) provide an important discussion of 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).

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 schooling induced by the program are around 18 percent. Moreover, these returns do not account for potential benefits of migration to better labor opportunities in the North. We estimate that Southern Blacks who were between the ages of 12 and 16 in 1935 were about 65 percent more likely to migrate to the North by 1940 if they were exposed to Rosenwald schools. No migration effects are found for Black children or young adults nor Whites of any age. Finally, we find no impact on height or weight, consistent with nutritional research in developing countries (e.g. Martorell et al 1995, Behrman and Hoddinott 2005) that suggest such influences are over well before children begin primary school.

The effects on these human capital and economic outcomes tend to be largest among students that resided in counties with lower levels of Black school attendance suggesting that schooling interventions targeted at the bottom of the skill distribution may have especially large returns. This may be consistent with a growing view in the development literature (Glewwe and Kremer 2006), that introducing schools in rural areas with few alternatives disproportionately benefited students facing the highest cost of attending school.

Of particular note is the considerable effect that the program had on Army General Classification Test (AGCT) scores, a precursor to the modern Armed Forces Qualifying Test (AFQT). We find that full Rosenwald exposure increases Black test scores by about 6 points, or 0.25 standard deviations and would close the Black-White test score gap by about the same amount. Again, the test score effects are particularly large, about 0.6 standard deviations, among individuals residing in counties with the lowest pre-Rosenwald Black school attendance rates. Yet, when we control for educational attainment, the Rosenwald test score effect disappears, providing further compelling evidence that racial gaps in test scores are directly linked to available resources and are not immutable (e.g. Neal and Johnson 1996, Hansen, Heckman, and Mullen 2004, Cascio and Lewis 2006, Chay, Guryan, and Mazumder 2009).

The detailed econometric specification that we use is designed to overcome threats to identification from two related sources of selection. First, school location decisions were not random. This is formulized in the way the grants were designed. Local citizens were required to donate substantial funds for construction, sometimes including land and labor. Therefore, funds were plausibly disbursed to those counties with higher demand for educational resources. Second, it is plausible that families with the highest demand for educational resources selectively migrated to counties with high exposure to Rosenwald schools. Keeping young families, and presumably the best young families, in the area was a clear goal of Rosenwald leaders.[3]

As confirmation that our results are not driven by these selection mechanisms, we perform several robustness checks. To further account for endogenous program placement, we use the idiosyncratic way that locations were chosen in the program's earliest years, in particular in Alabama and in the counties surrounding Tuskegee and the state capital of Montgomery, to show that our estimated Rosenwald effects are not due to endogenous program placement. We also establish that Black school attendance rates and 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 were not significantly related to observable measures of Black socioeconomic conditions prior to the Rosenwald Fund's creation. To account for selective migration, we recalculate Rosenwald exposure rates based on state of birth,[4] rather than county of residence. By construction, these exposure rates are unrelated to family migration decisions driven by Rosenwald, especially when we restrict the sample to cohorts born prior to the upsurge in building. Again, we find similar results across a variety of outcomes.

While we argue that selection does not drive our inferences, it is nevertheless likely that racial convergence with the North was inevitable given the high rates of return to basic education. Indeed, by *Brown vs. Board of Education* in 1954, common measures of Black-White educational resources gaps had been mostly closed (e.g. Card and Krueger 1992, Donahue, 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

[3] Emmett Scott of the Tuskegee Institute argued as much in 1918: "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).

[4] Unfortunately, county of birth is unavailable.

discrimination, as well as the important role of liquidity constraints, to argue that major investments in Black schools required outside intervention at the time. The racial convergence that occurs in relatively short-order after the introduction of the Rosenwald program seems to validate these views. At a minimum, our econometric results suggest that the education and consequently economic outcomes of cohorts born in the 1910s and 1920s were significantly altered by the Rosenwald schools. Perhaps more speculatively, later cohorts could have been impacted as well through intergenerational mechanisms[5] 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 on these outcomes using the earliest Alabama schools and by calculating Rosenwald exposure based on state of birth. 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. History

The Rosenwald Rural Schools Initiative originated from a request by Booker T. Washington to Chicago philanthropist and businessman Julius Rosenwald in 1912. Washington hoped to use a small fraction of Rosenwald's donation to Alabama's Tuskegee Institute, where Washington was Principal, 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[6] led Washington to seek out Northern

[5] 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). If some fraction of this parameter reflects causal effects, this would imply a nontrivial effect on the outcomes of children, and even grandchildren, of Rosenwald students.

[6] For accounts of early 20th century Black schooling, see Jones (1917), Bond (1934), McCormick (1934), Margo (1990), and Hoffschwelle (2006). In an early letter to Rosenwald (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

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 school construction projects as of the 1919-20 school year.[7] Schools still remained heavily in Alabama but a good number were clustered in three other areas: Western Louisiana, Western Tennessee, and Eastern North Carolina and Virginia. Three final states – Florida, Oklahoma, and Texas – were approved for funding in 1920.[8]

Construction activities escalated in the 1920s, as shown in figure 3. The number of schools and teachers expanded by 17.5 and 20.8 percent per year, respectively, between 1920 and 1931. With the number of teachers growing faster than the number of schools, school size, as measured by teachers per school, grew by 2.9 percent per year. 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.

Much of this school size growth occurred toward the end of the 1920s, when the Rosenwald Fund moved from constructing small primary schools towards larger schools, often with the capability of offering high school level instruction.[9] Southern Black public high school instruction was highly unusual prior to the Rosenwald program. Even in the mid-1920s, when the high school movement was well underway (Goldin and Katz 1999), Alabama and South Carolina contained no four year accredited Black public high schools,

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inequities are described bluntly in Washington's letters to Rosenwald and catalogued more systematically in Johnson (1941) and Margo (1990). For example, Johnson reports 1930 county-level Black and White salary expenditures per pupil based on state education reports at the time. Unweighted, the average county spent 39 cents on Black school salaries per capita for every dollar on White school salaries per capita. The source of all Rosenwald correspondence and primary documents cited in this paper is the Rosenwald Fund archives at Fisk University in Nashville, Tennessee.

[7] Although schools were built between 1914 and 1919, the Rosenwald records do not distinguish year of construction prior to the 1919-1920 school year.

[8] Three Missouri schools were built after 1929. Because there were so few, we exclude Missouri in our computations.

[9] Fischel (2009) describes a similar consolidation trend in the North beginning in the early 20th century that he argues was driven by the closure of small rural ungraded schools in response to the demand for high schools.

and Florida, rather remarkably, funded only two Black public high schools, regardless of accreditation or length.[10] However, by 1932, Rosenwald Fund pamphlets claimed roughly 10 percent of existing Rosenwald schools offered at least two years of high school instruction (Donohue, Heckman, and Todd 2002).

Toward the end of the decade, Rosenwald's geographic reach was wide, nearly completing the Fund's hope to have a school in every Southern county. Ultimately, 85 percent of counties with Black children under the age of 19 had a Rosenwald school when the program closed in 1932.[11] Figure 4 displays the Rosenwald map as of 1931-32. At that point, official documents report 4,977 schools in 883 counties with the capacity to serve 663,615 students, opened at a cost of \$28.4 million dollars. Even though county coverage was nearly ubiquitous, most counties contained far too few schools to serve all rural Black children. 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. [12]

For identifying causal effects, the program's varied timing and geographic development is critical. Figure 5 plots the distribution 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. Figure 6 shows that the coverage of Rosenwald schools varied substantially over time within states as well. For example, although Oklahoma was among the last states to be funded by Rosenwald, by 1930 it had the second highest share of rural Black coverage. In contrast, although Alabama was the site of the first Rosenwald schools, by 1930 its rural Rosenwald coverage was among the lowest. Since these time patterns are also evident at the county level, we exploit the fact that Black children born in later years in a particular county will have greater access to Rosenwald schools than earlier cohorts born in the same county.

[10] These numbers are from an internal 1925 Rosenwald report based on state education reports.
[11] The Fund voluntarily closed within a year of Rosenwald's death, swifter than Rosenwald's requirement that the endowment be dispersed within 25 years of his passing. The Fund's closure was sped up 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).
[12] According to the 1930 IPUMS, there were approximately 1.8 million school-age (7 to 17 year old) rural Black children in the fourteen Rosenwald states. At an assumed student-to-teacher ratio of 45, student capacity would have been roughly 650,000 in 1930. This student-teacher ratio, which is in-line with Rosenwald Fund assumptions, is supported by a 1925-26 internal Rosenwald survey of state and county education board reports showing an average student-teacher ratio of 46 to 1 in rural Black Southern schools. The ratios vary from 29 (Texas) to 62 (Louisiana) but most are between 40 and 50. Margo's (1990, table 2.7) numbers are a bit smaller, varying from 33 to 49 with a simple state average of 39. However, Margo does not distinguish rural and urban populations.

B. Rosenwald Financial and Nonfinancial Contributions

The Rosenwald Fund's investments were, from the beginning, heavily tilted towards school construction. One important reason was justifiable concern that donations would be expropriated from Black schools. Similarly, other important Rosenwald initiatives tended to avoid fungible outlays. For example, the contemporary facilities required new designs and standards for size, equipment, lighting, ventilation, and bathrooms. Such improvements likely encouraged the hiring of higher quality teachers and principals, aided by Rosenwald's espousal of new minimum standards for salaries, some direct support for teacher salaries, and funding for teacher home construction.[13] Rosenwald's emphasis on minimum standards and financial incentives to lengthen school terms were equally difficult to confiscate and, moreover, mechanically required local governments to channel salary expenditures to Black schools, associations that we find in the cross-section.[14]

However, the initial plan drawn up by Rosenwald and Washington emphasized that the Fund would only be a facilitator and minority partner in these expenditures. Local Blacks and governments would provide the majority of the funding, particularly after construction was complete. Over time, public and local fund matching became even more critical, with the Rosenwald share of contributions falling from around 25

[13] At the time, there were concerns, expressed among others by Rosenwald himself in 1919 letters, about the ability to hire enough qualified teachers to supply new schools. The concerns were 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 negative impacts on teacher quality, an issue studied in a modern setting by Jepsen and Rivkin (2009).

[14] The average term in Southern Black schools was 4.7 months just prior to the Rosenwald program (McCormick 1934), and varied by state from 50 to 90 percent of White term length (Margo 1990). Initially, the Rosenwald 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. Using an undated internal Rosenwald Fund survey of State reports on term length for the 1925-26 school year (titled "Negro Public Schools in the South, 1925-26"), we estimate that a 10 percentage point increase in Rosenwald coverage increased term length among all rural Black schools by 0.48 months. Assuming average Black rural student coverage of 30 percent, this implies that Black rural term lengths increased by 1 ½ months. Of course, more (and potentially better) teachers and longer school terms ultimately involves higher salary expenditures. To detect such an association, we use county-level instructional salary expenditures from Johnson (1941), which are compiled from state education reports, measures of rural Black student exposure to Rosenwald schools, and the 1930 Census. Specifically, we regress county instructional salary expenditure per capita by race on county average Black rural student Rosenwald coverage, while controlling for race-specific school enrollment rates, literacy, occupational structure, mean home values, employment, and state fixed effects. Note that the data forces us to aggregate rural and urban populations and therefore leads to attenuation in our Rosenwald estimates. Nevertheless, we still find that average Black student 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. There was no impact (0.02 (0.09)) on White school salary expenditures. The Black school effects are twice as large, but not precisely estimated, for the initial schools built in Alabama (see section VI).

percent for the earliest schools to the 10 to 15 percent range in the last five years of the program. In total during the program's existence, 64 percent of funding came from local government sources, another 20 percent from private sources, and only 15 percent from the Rosenwald Fund (see bottom of table A1).[15]

Therefore, it is important to emphasize that the Fund's contributions to the initiation and continuation of the rural schools program went beyond merely fulfilling a share of construction costs. Support for teacher standards, school terms extensions, and access to high school curriculum are a big part of the Fund's initiative. But of additional note is the role that the Fund played in building local coalitions. Explicitly, this included paying canvassers to explain available opportunities and guide local Black leaders through the fundraising process (Hoffschwelle 2006). But perhaps ultimately more important is the hard-to-measure credibility that the Fund and the Tuskegee Institute added to the process. To take one example, to build a new school, at least two acres of land had to be donated to the state. Often, this necessitated new purchases by local Blacks, a rather perverse requirement given recent Southern history. Having the backing of Washington, Tuskegee, and the Rosenwald Fund likely added a level of trust that would have been very difficult to accomplish otherwise.

Coalition building was critical among Whites as well, and Rosenwald money likely helped buy 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 needed to be addressed first, a strategy that led to deep conflicts with other activists, notably W.E.B Dubois and the NAACP. Moreover, Washington 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).

[15] With equal school weights, Rosenwald contributed 21 percent, private sources 28 percent, and local governments the remaining half. 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 suggest that they were particularly important for the earliest schools.

Over time, the Fund also played a role in dispersing information and setting standards for buildings, curriculum, and (anecdotally) teacher training. The former, in particular, was likely quite valuable to poor, rural communities that otherwise might have had limited access to architectural plans and school designs.[16]

C. Selection of Rosenwald School Locations

The matching grant program explicitly linked funding prospects to a county's social, political, and economic environment. The philosophy of the Fund was unambiguous on this point: "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.[17] But the grants also highlight the potential importance of controlling for confounding factors in our econometric approach since communities that were particularly open to improving Black schools, and thus were able to convince the Fund to invest in their area, might have experienced better outcomes even in the absence of the Rosenwald program.

Briefly, we provide several pieces of evidence about selection. In Figure 7, we find that the initial counties selected for Rosenwald schools, the "Tuskegee" schools built by 1919, actually had similar levels and trends in Black rural school attendance in the decades preceding the program to those that never received a school. Thus, there is no evidence of positive selection on one of our key outcomes. This also motivates our analysis of the earliest schools clustered in Alabama, which we believe were chosen idiosyncratically. We discuss this claim and how we use it to identify a Rosenwald effect in more detail in section VI.

Related, appendix A investigates the extent to which pre-Rosenwald county characteristics affected school location decisions. We again find that pre-existing Black socio-economic conditions (e.g. school attendance, literacy, occupation) and trends in these conditions are unable to predict the timing or intensity of

[16] Tuskegee developed the first school plans in 1915 and the Fund updated them repeatedly starting in 1920. Applicants were not required to use them but needed other approved plans, typically from state Education Boards, to move forward.
[17] This financing structure was controversial at the time. The President of the Slater Fund, for example, took exception, preferring that "effort should be made to get more and more from the public funds." Letter from Slater Fund President James Dillard to Rosenwald on March 11, 1919. Many local communities struggled to obtain the required funds, and even when they reached minimum requirements, it may not have been enough to warrant Rosenwald intervention. That is clear in the following letter from Rosenwald to Major Moto of the Tuskegee Institute (December 5, 1917):

> "Mr. Rosenwald was somewhat disappointed in noticing that for the Longley School in Pulaski County, Arkansas, which cost $900, only $50 of land was contributed by the residents and all of that small donation came from colored people…it did not evidence a spirit of sacrifice or even of deep interest on the part of the residents who relied chiefly on public funds and on his contribution for the school."

initial Rosenwald school locations in a statistically or economically significant way, suggesting limited scope for reverse causality. However, we do find suggestive 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 adapted by the Fund after his untimely death in 1915, of avoiding areas that might lead to White backlash. Like figure 7, we also find that school location selection during the 1920s was associated to some degree with observable pre-Rosenwald Black and White conditions. That result is a primary reason why a research design that takes advantage of considerable variation in Rosenwald school exposure across birth cohorts within counties even in the same year is important for identification.

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. Each card contains a description of a Rosenwald school, teacher home, or industrial shop, or some combination thereof. Information on the cards is limited to location (state and county), year of construction, school name, number of teachers (or home/shop rooms), number of acres of land, insurance valuation, and construction cost. Cost is broken down by four possible funding sources: the Rosenwald Fund, local Black individuals, local White individuals, and local public governments. Room additions, as well as complete destructions due to fire or weather, are recorded in handwriting ex-post although it is difficult to know how complete these adjustments, particularly the latter, ultimately are. This is the only known systematic information about individual Rosenwald schools. Appendix Table A1 provides basic statistics about all Rosenwald school construction projects. Our analysis will use a database that includes 4,935 schools with 14,438 teachers in 880 counties.[18]

We append this information to several large datasets on potential students.

[18] This deletes 37 "schools." In 35 cases, the card does not contain key information about a school opening (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 two Missouri projects (another Missouri project is among the 35 that are missing school data). The results are not sensitive to including these other cases.

B. IPUMS (1910-1930)

First, we use samples drawn from 1910 through 1930 decennial Censuses (IPUMS) to build a panel of school-age children and a separate panel of young adults.[19] These samples are matched to potential access to Rosenwald schools through county of residence, birth year, race, and rural status.[20] We concentrate on the two key outcomes, school attendance and literacy, available prior to 1940. School attendance is an outcome of obvious interest given the purpose of the Rosenwald program. For this variable, we construct a pooled sample of over 589,000 children between the ages of 7 and 17. For literacy, which Collins and Margo (2006) suggest acts as a proxy for completing 1 to 3 years of schooling, our sample totals over 398,000 persons between the age of 15 and 22.[21]

Table A2 presents descriptive statistics of our 1910 to 1930 IPUMS 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 detailed individual level data we are able to control for these factors in our regressions.

C. World War II Enlistment Records

Our second source is a database of US Army World War II enlistees, available from the National Archives and Records Administration. We extract a sample of roughly 2.1 million men aged 17 to 45 from Rosenwald states who enlisted between 1941 and 1945.[22] The data contain several key pieces of demographic information including age, race and county of residence at enlistment that allow us to ascertain

[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. In internal documents, the Fund often used the Census definition for data organization and evaluation.
[21] Collins and Margo (2006) show literacy rising as a cohort ages from 10-19 to 20-29. To abstract from literacy effects due to schooling, 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 find similar results when we look directly at migration in section VII.F.
[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 detail on the data.

potential Rosenwald status. Unlike the Census, however, we do not have precise information on geography within the county to infer rural status. Therefore we use a slightly more blunt approach with the World War II data, classifying rural status by the share of a county's population living in rural areas according to the 1910 to 1930 Census.[23]

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[24] and scores from the Army General Classification Test (AGCT) that was given to enlistees to determine military occupation. Since these test scores were thought to be lost to history, we describe our method for obtaining this data in Appendix B. In addition, the enlistment records contain information on height and weight, which could be used as a validity check if access to schools after age 5 is less likely to affect these characteristics (e.g. Martorell et al 1995, Behrman and Hoddinott 2005). The summary statistics are presented in 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 address potential selection bias first by using a rich set of fixed effects including quarter of enlistment interacted by race and second by using inverse probability weighting (IPW), which we discuss below.

D. IPUMS (1940-1950)

Finally, we use Census samples from 1940 and 1950 to assess the effects of the program on completed schooling, log annual earnings, log hourly wages, and migration. For the education and earnings

[23] Specifically, we classify a county as rural if the average rural share was greater than 50 percent over the 1910 to 1930 period.

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

measures, we use a sample of approximately 200,000 individuals aged 18 to 40, of which 70,000 contain information on earnings and wages. The main drawback is that, unlike the earlier Censuses, county of residence is not publicly available yet. Instead, we link individuals to their exposure to Rosenwald schools at the state economic area (SEA), aggregations of counties with similar characteristics developed by the Census Bureau. On the one hand, this can lead to greater classification error, since our measures will be much blunter than before. But the larger aggregation retains individuals who moved across counties (e.g. rural to urban migration) within the same SEA. We can 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.[25] The 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 well into adulthood.

IV. Empirical Strategy

A. 1910-1930 IPUMS Statistical Model

A typical specification for our IPUMS 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

[25] We have also constructed migration based on state of birth and state of residence, as is commonly done in the literature (e.g. Margo 1990). Results based on this measure are fairly similar, albeit less precise, than the SEA-based estimates described below. Our view is that the SEA measures are superior for measuring Rosenwald exposure. Alternatively, to get earlier county-to-county migration, we are experimenting with matching the full 1920 and 1930 Census manuscript obtained from ancestry.com by name, age, race, and birthplace, and subsets. Thus far, we have done this match using the 1 and 5 percent 1920 and 1930 IPUMS but the state-to-state "migration rates" were too high relative to estimates derived from Census comparisons of state of residence and birth. That indicates to us that there are too many false positive matches, presumably due to common names of rural Southern Blacks at the time.

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.

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.[26] This measure is limited in at least two ways: it fails to distinguish differences in Rosenwald exposure between birth cohorts within a county[27] 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 for each student from ages 7 to 13 based on their birth year b and county 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.[28] In principle, the measure should only take on values between 0 and 1. Therefore, we can interpret the coefficient on the measure as the effect of going from no Rosenwald exposure in one's county to complete exposure.

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.[29] By differencing across groups, we remove

[26] Table A2 shows that 49 percent of Blacks had R_{ct} =1 by 1920 compared to 30 percent of Whites. By 1930, this increased to 91 percent for Blacks and 73 percent for Whites.

[27] 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.

[28] 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 10), 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.

[29] For example, to calculate the effect of complete exposure versus no exposure on only Black rural children we would sum $\gamma 0$, $\gamma 1$, $\gamma 2$, and $\gamma 3$. Similarly, to estimate the Black-White difference of the Rosenwald effect in rural areas we would sum $\gamma 1$ and $\gamma 3$. Finally, $\gamma 3$ 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. In order to calculate standard errors for each parameter of interest, we recast the regressions by changing the dummy variables so as to return the intended parameter of interest along with its standard error.

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.[30] 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).[31]

A lingering concern could be that there are more abrupt trends or policy interventions affecting school age children even within a county and in a particular Census year that are coincident with the timing of Rosenwald school construction. To address this possibility, we include a wide set of interactions of age by state by Census year by race and by rural status.[32] In addition, we run specifications allowing for separate age patterns for each county in each Census year along with county-by-year fixed effects.[33]

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[30] Alternatively, negative shocks to rural counties with Rosenwald schools could result in small observed effects for rural Blacks despite having large effects on the Black-White difference.
[31] 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).
[32] Interactions of age by year effectively control for birth cohort trends.
[33] Since our identification of Rosenwald treatment is based on county by race by rural status by birth year, we obviously cannot include this level of controls.

Another possible concern relates to unobservable family background, and its potential impact on initial nonrandom residential location decisions of households. Although the rich set of controls we use likely removes much of this concern, we also take advantage of the household structure of the IPUMS to estimate family fixed effects models.

B. WWII Enrollee Statistical Model

The basic regression specification used with the WWII data is similar to equation (1) but data limitations require a few alterations:

$$(2)\ y_{ict} = \alpha + black + \gamma_0 E_{bc} + \gamma_1(black \times E_{bc}) + X_{ict}\beta + \theta_{st}age_{it} + county_c + \varepsilon_{ict}.$$

Equation (2) accounts for the draft's restriction to men, the exclusive use of the exposure measure E_{bc} since the vast majority of WWII enrollees originate from counties with a Rosenwald school, and the lack of individual rural status. On the latter issue, we analyze the overall Black-White difference, γ_1, as well as stratify the sample by the share of the enrollee's county that is rural in the 1930 IPUMS.

Finally, nonrandom induction into World War II, and consequently the sample of who takes the AGCT test, potentially adds another form of selection bias. We first address this problem by using 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.[34] To address potential selection across men 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).[35] Define p to be the true likelihood that a given individual will enlist, and $\hat{p}$ to be an estimate of that likelihood. Then, weighting the regression equations by $w_i = \frac{1}{\hat{p}}$ removes any remaining

[34] This strategy is not useful for test scores since they are only observed for a short period of time.
[35] 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 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.

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).[36]

V. Census Results on School Attendance and Literacy

A. School Attendance

Table 1 provides results for school attendance using the indicator of county Rosenwald presence, R_{ct}. To fix intuition for the remaining tables, we first show school attendance rates, broken down in columns by Rosenwald presence and in the first four rows by race and urban status. For example, the average school attendance rate, conditioning just on Census year, was 73.2 percent among rural Black 7 to 17 year olds living in a county with a Rosenwald school and 65.8 percent if not. The third column, titled "Rose-no Rose difference," subtracts the Rosenwald rates by the non-Rosenwald rates. In the case of rural Blacks, this very simple estimate of the effect of Rosenwald schools is 7.4 percentage points (with a standard error of 0.7 percentage points).[37]

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 5.0 (1.1) percentage points while the Black-White rural gap is 7.4 (0.7) percentage points. Finally, when we difference the rural Black-White and urban Black-White estimates, labeled "triple difference," a Rosenwald school is estimated to raise school attendance by 6.3 (1.2) percentage points.

Of course, there are other important factors like family background characteristics and other local conditions that are likely to affect school attendance that we have not yet accounted for. In Column (2), we include gender, age, parents' literacy, father's occupation score, father's homeownership, state fixed effects,

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[36] See Chay, Guryan, and Mazumder (2009) for a similar application of IPW to military selection.
[37] Standard errors are clustered at the county level. Generally, clustering at the state level (as in Bester, Conley, and Hansen 2009) has minimal impact on our inferences, especially when we introduce the controls in table 2.

and the White literacy rate in the county in 1910. We find that all of these controls have significant effects and are in the expected direction. Column (3) adds county fixed effects (without controls), and column (4) includes both the controls and the fixed effects.

Concentrating on the most complete specification in column (4), we find that the rural Black result remains economically and statistically important; Rosenwald boosted school attendance among potentially eligible children by 5.9 (0.8) percentage points. Including county fixed effects eliminates the effect on urban Whites but still leaves a small positive effect for urban Blacks.[38] 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.

Over the period from 1910 to 1930, Black rural attendance in the Rosenwald states rose by 14.4 percentage points, so our Black rural estimate of 5.9 percentage points suggests that Rosenwald schools account for about 41 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 52 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, notably without any 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. However, smaller significant effects are also found on the White rural-urban gap and the Black-White urban gap which were not targets of the project. Columns (2) to (7) add controls for county fixed effects, county-by-year fixed effects, and age profiles that are specific to states, races, and rural status. Some of the magnitudes of the point estimates are altered but all of the key

[38] That the program's impact is primarily limited to rural Blacks could also mitigate negative general equilibrium effects that arise when higher educational attainment lowers the return to education and thus cause a reduction in future educational attainment (e.g. Angrist 1995). In a similar context, Duflo (2001) argues that general equilibrium effects are unlikely to be an important part of Indonesia's school construction program in the 1970s.

effects remain statistically significant.[39] In column (8), we allow each county in each Census year to have a separate age profile and find even this has little impact on our inferences.

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}=0.3 or 30 percent) would raise school attendance of rural Blacks relative to rural Whites by about 3.3 percentage points. That estimate accounts for about 30 percent of the 11 percentage point reduction in the gap between 1910 and 1930.

Finally, in columns (9) and (10), 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

Tables 3 and 4 repeat these analyses for literacy using our sample of 15 to 22 year olds. Starting with table 3, our preferred specification in column (4) 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 percentage points for the Black-White rural estimate and 5.0 percentage points for the rural-urban Black difference. The triple difference is somewhat lower at 3.9 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.

[39] The latter specifications, especially columns that add group-specific age profiles, considerably reduce the variation in Rosenwald exposures since identification now rests only on cross-cohort differences in exposure within more narrowly defined demographic groups.

The Black rural literacy rate rose by about 16.6 percentage points over the 1910 to 1930 period. Therefore, our Black rural estimate suggests that Rosenwald schools account for about 40 percent of the gain. The rural Black-White literacy gap narrowed by about 12.7 percentage points. We estimate that Rosenwald schools accounted for about 53 percent of this gain. These results are virtually identical to the analogous calculations for school attendance based on Table 1.

In table 4, we again find large and statistically significant effects of Rosenwald exposure that are robust to a wide range of controls.[40] 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 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 level of exposure in 1930 (30 percent) 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. Robustness Checks to Address Selection

A. Earliest Schools Built in Alabama

Our results thus far attempt to statistically adjust for school location selection driven by local demand for education. Alternatively, our reading of the archival records suggest that the first Rosenwald schools, which were heavily clustered in four geographic areas (see figure 2), arose for idiosyncratic reasons that were largely unrelated to economic or educational circumstances. While there is evidence in appendix A and figure 7 that this observation can be applied more generally to schools built before 1919, we concentrate on the initial buildings in Alabama where this claim is most transparently verified by Booker T. Washington's initial plans for the Rural Schools Initiative:

[40] Since our sample includes many adults who have left home we cannot use family fixed effects.

> "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." [41]

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 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 from Virginia to Alabama.[42] Moreover, we know from future construction activity (e.g. figure 6), school expenditure data, as well as anecdotes[43] 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 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 upon a few counties may serve the further purpose of bringing about a rivalry between the communities that will prove of value."[44]

The state capital happened to be very close to Tuskegee, another reason why the initial schools might have been located in that region. Second, we compare the effects on students who lived in counties in Alabama

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[41] Source: "Plan for Erection of Rural Schoolhouses," Date and author unknown, Rosenwald Fund archives.

[42] Hampton's location could explain the cluster of Rosenwald schools in Eastern Virginia and North Carolina. The clusters in Louisiana and Tennessee are less obviously traceable. They could be an endogenous response to high demand for the schools from local citizens. Alternatively, these states may have had officials, perhaps randomly assigned, with the interest and capacity to support Rosenwald. It is, of course, impossible to disentangle these two explanations with available data. But we do know that Washington believed that the latter was critical:

> "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…"

And, in fact, during 1918 and 1919, when the Fund was beginning to divert more resources out of Alabama, the Rosenwald Fund received strong letters of support and interest from the Boards of Education from only three states: Louisiana, North Carolina, and Tennessee. That said, the reasons for these clusters are speculative and therefore we concentrate solely on the experiences of the early Alabama schools.

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

[44] "Plan for Erection of Rural Schoolhouses," Date and author unknown, Rosenwald Fund archives.

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.

Table 5 reports the results from these exercises. For this analysis, we use only data from the 1910 and 1920 Censuses, do not break out the analysis by rural status and use only the measure of any Rosenwald presence in one's county by 1920.[45] In the top row of Panel A, we find that the schools that are close to the state capital have an effect that is, if anything, larger than the rest of the South. Using a 20 and 60 mile radius, a Rosenwald school in the county increases Black school attendance by 20.3 (6.1) and 9.4 (3.9) percentage points relative to Whites, a result that is impervious to the inclusion of county fixed effects.

In the second row of Panel A, we enlarge the sample to include all of Alabama. In addition to the main Rosenwald dummy and its interaction with being Black, we add an additional set of interactions related to being within radii from Montgomery. The entries in the second row of Panel A show the estimates from these interactions. The results again imply that the effects in these counties were considerably larger than the effects in the rest of the state. We hesitate to draw strong conclusions from the results of this second exercise because of the small samples involved and also because we do not know the precise timing of the openings of the schools built prior to 1919. It could be that many of the schools outside of the tested radii were built much later and hence contain relatively little signal in a blunt measure of whether a school was built by 1919. However, it seems consistent with the comparison to the full Rosenwald state sample reported in table 1. Although those estimates are smaller, on the order of 4 to 7 percentage points, they are not statistically different than the Alabama-only estimates in most cases.

Panel B uses the happenstance of Booker T. Washington's location to estimate the effects of the initial Rosenwald schools along the Alabama border.[46] Again, we find Rosenwald effects that are of a similar order of magnitude to the Montgomery experiment, roughly 7 to 11 percentage points with standard errors of 4 to 5 percentage points when we combine all borders. Therefore, while we are not dismissing the

[45] There are few large urban areas in our comparisons. The choice of Rosenwald measure is driven by limited power with smaller samples.
[46] In general, the Alabama counties had lower levels of schooling and poorer socioeconomic outcomes than the adjacent counties outside of Alabama. However, county fixed effects should sweep out these differences.

role of endogenous selection, we believe there is compelling evidence that the main results are not significantly swayed by this selection process and, if anything, selection could lead to some attenuation.

B. Exposure based on state of birth

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 had a strong preference for their children's schooling to migrate to these counties. 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 counties during this time period is unavailable from historical data.[47] We first argue that our evidence from Alabama likely addresses this concern, since the rapid building of the pilot schools beginning in 1914 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 Table 5 we find even larger effects 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 any concerns we rerun our analysis on the full set of Rosenwald states using a more exogenous source of exposure that is determined prior to the time of school attendance, namely exposure based on one's state of birth. Since we do not know county or rural status at birth, our specifications are more limited. Nevertheless, we can still 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. For migration to be a concern, one must argue that only black parents selectively moved across states prior to their child's birth, and correctly forecast which states would have Rosenwald schools.

The results are shown in Panel A of Table 6. 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

[47] The first migration question is asked in the 1940 Census. In section VII.F, we show that Rosenwald exposure had no impact on within South migration of Black or White children and their families between 1935 and 1940.

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 examine the effects on literacy in column (3) and find even larger effects for blacks of over 30 percentage points and now a negative effect of nearly 10 percentage points on whites. Our literacy sample uses 15 to 22 year olds so it is already restricted to older cohorts. 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 these results and prefer our estimates that use county level exposure and are better able to control for unobservables. We discuss the remaining panels of Table 6 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 7 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 0.27 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 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 about 0.8 years. 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. 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.[48]

It is important to note that since this data cannot distinguish between rural and urban areas within a county, our estimates are likely to be attenuated. In order to gauge the degree of this bias, we re-estimated the effect on 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.[49]

B. High School Completion

We next focus on the effects of the program on high school completion in columns (3) and (4). Here, the results are particularly striking. Focusing on rural counties in Panel B, 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 small (but statistically insignificant) declines of about 1 to 2 percentage points for urban Blacks. The triple difference is estimated to be close to 13 percentage points when we use IPW. Since the mean high school completion rate among Blacks is about 32 percent, 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 also could reflect heterogeneous treatment effects, which we describe in more detail in

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[48] 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.

[49] For example, using a specification analogous to that shown in column (4) of Table 4, we obtained an estimate of 0.182 on the Black-White rural gap compared to 0.191 using the within-county variation in rural status. Similarly, our triple difference estimate was 0.109 instead of 0.128.

Section VIII. Finally, it could 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

Columns (5) and (6) present 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.[50] Using the triple difference estimate from the IPW specification, complete exposure to Rosenwald schools would improve Black test scores by about 5.9 points or about 0.25 standard deviations. In the aggregate, moving from no exposure to the mean exposure of 0.35 (for cohorts who entered school by the end of the Rosenwald program) would narrow 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 strong effect on test scores. As further confirming evidence 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 (columns 7 and 8).

D. Height and Weight

A potential validity check is to measure the effects on outcomes for which we expect the Rosenwald program to have less, and perhaps no, influence. Generally, health interventions are thought to be most critical in the early life period, well before children enter school (e.g. Cunha and Heckman 2007). Our only health measure is height and weight, of which there is experimental evidence in developing countries (Martorell et al 1995, Behrman and Hoddinott 2005) that these particular measures do not respond to nutritional improvements after age 3 or 4. Since Rosenwald targeted children beyond the early life period

[50] Compared to the other outcomes, the test score results are more affected by the use of IPW. For example, for our rural county sample, we find that the effect on Blacks falls from about 8 points to 6.8 points when using IPW. That is likely because test scores are only available for a short period of time. Consequently, we are unable to control for selection into the military using fixed effects for quarter of enlistment by race and must rely on the IPW.

and was not designed to treat childhood health anyway,[51] we might expect little change in height and weight. In columns (9) and (10), we show no quantitatively or statistically meaningful impact on height. For weight (columns 11 and 12), we find no effect in rural counties but Black weight *declines* relative to White weight by 1.7 pounds among individuals living in urban counties who were fully exposed to Rosenwald schools. This effect, though statistically significant, does not suggest any obvious confounding factor.

E. Longer-term Outcomes

Table 8 displays estimates of the effects of the program on completed schooling, log annual earnings, and log hourly wages using 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").

In column (1), we find that complete exposure raises black years of schooling by 1.1 years, 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 greater than 20 percent increase in annual earnings for blacks and 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 20 percent while the losses for whites are about 15 percent leading to relative wage gains for blacks of about 35 percent.

A Wald estimate of the returns to schooling on wages is simply the ratio of the estimates in columns (1) and (5), yielding a return of 17 percent (0.197/1.131). We also calculated the returns to schooling for Blacks directly in Panel B. The 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. These estimates however, do not remove the common effect of exposure shared by both racial groups. If we construct a Wald estimate based on Black-White differences, the implied returns to Rosenwald are an even

[51] While there is no historical documentation of health initiatives in the primary or secondary Rosenwald schools, Julius Rosenwald clearly 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).

larger 0.24. These estimates are notably more than the 8 to 14 percent returns typical estimated using contemporary data sources with modern era schooling interventions (e.g. Card 1999), as well as Duflo's (2001) analysis of an Indonesian school construction program in the 1970s.

We can think of several potential explanations for our outsized estimate. One possibility is that the Rosenwald program had effects on the aggregate local economy, perhaps through externalities, in addition to directly affecting the productivity of individuals. To the extent that this should also have affected white workers living in the same SEA, there is little evidence in support of this argument. Similar arguments that posit a correlation between Rosenwald exposure at the SEA level and the error term of the wage equation must rely on race-specific effects that occur within county and Census year. That said, we cannot dismiss the possibility of spillovers to non-treated Blacks and consequently that part of our estimated rate of return may capture external returns.

A second possibility is that the Rosenwald schools directly affected the return to schooling by improving school quality (e.g. through better teachers). Card and Krueger (1992) use data at the state by cohort level to show that improvement in the quality of Southern Black schools increased the returns to schooling to individuals working in labor markets in the North who were born in these states. This would seem to be a reasonable explanation for at least some part of our estimated effects.

Third, any increase in school term length caused by Rosenwald interventions, which we estimate could have been sizable in the mid-1920s (see footnote 12), should mechanically increase the rate of return to a year of education except under the extreme assumption that this extra time has no impact on learning.

Finally, it could simply be the case that the returns to schooling for those who were most impacted by the Rosenwald program, presumably those with little or no access to schooling, were just extremely large. International evidence pointing to larger returns to education in developing countries, particularly at the primary school level (Psacharapoulos and Patrinos 2004), is consistent with such an explanation. We discuss this possibility in greater detail 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. Conditioning variables include gender, gender by race, age, state fixed effects, and state-age-race trends.

These results are in panel C of table 8. In column (10), we find that complete exposure to Rosenwald increases Black migration out of the South by 2.5 percentage points for cohorts that likely finished school between 1935 and 1940 (that is, are ages 17 to 21 in 1940). For Blacks in this age group that experienced average (30 percent) Rosenwald exposure, migration propensity to the North increased from 1.4 percent to 2.3 percent. By contrast, we find no effect on within-South migration for this same cohort (column 13). We also find no effect on Blacks who were of school age in both 1935 and 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 to higher Rosenwald exposure by increasing migration to the North was Blacks finishing school.

We are certainly not the first to link better education to the Great Migration. But 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 did in fact induce more moves to potentially better labor market opportunities, the rate of return to the Rosenwald program may have been even larger than what we show in table 7.[52]

G. Exposure Based on State 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

[52] Potentially offsetting this point is any negative supply effect for Blacks that were already in the North, as in Boustan (2009).

selective migration to Rosenwald counties. We also show that the same conclusion holds with respect to long-term outcomes. Panels B and C of Table 6, show even stronger effects of state of birth exposure on the relative outcomes for blacks. In the World War II data these effects are also substantially stronger for cohorts who were born in 1917 or earlier prior to the large-scale rollout of the program in the 1920s. In the 1940-50 Census data, which is more representative, the effects across the two cohort groups are more similar.

VIII. Heterogeneous Effects

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.[53]

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 this section, we also explore whether there were different effects by age or sex.

In Panel A of Table 9, we report the 1910-1930 IPUMS school attendance results separately by quartiles of 1910 county level black school attendance rates.[54] For those in the lowest quartile, the effect of

[53] 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.

[54] 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 0.053 and the effect on the Black-White rural gap is 0.11.

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).

Panel C divides the sample by age. 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 actually largest for 14 to 17 year olds. This likely reflects 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.[55] Panel D shows that the effects were statistically similar by sex.

The next three panels, labeled E, F, and G, consider the effects on the Black-White rural gap for the three main outcomes in the World War II data – education, completing high school, and AGCT scores -- using samples stratified by Black school attendance in 1920.[56] In panel E, we find that the effect on educational attainment was over 1 year in counties in the bottom quartile and about 0.6 to 0.7 years for the other three quartiles. Similarly, the largest effects on high school completion (Panel F) are also found in the bottom quartile, although the effects are also quite large in the top quintile. Finally, in Panel G, we find a 14 point effect on AGCT test scores at the bottom quartile, equal to about 0.6 standard deviations. 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.

Finally, panels I and J re-examine outcomes from the 1940-50 Census using samples stratified by Black school attendance at the SEA level. For education, we again find a similar pattern as above; the largest impact appears at the bottom quartile. For wages, we find an extremely large estimate of 66 percent for those

[55] 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.

[56] We did not use 1910 as the basis for stratification for this data since over 80 percent of the sample entered school beginning in 1920.

in SEAs that were in the bottom quartile of black school attendance, implying a private rate of return to an additional year of education of roughly 34 percent.

Overall, we view the evidence as suggesting that nearly all treated student groups benefited from Rosenwald exposure. Nevertheless, there appears to be strongly suggestive evidence over all of the education and wage outcomes, that students schooled in areas with the poorest initial conditions benefited the most from Rosenwald exposure.

IX. Conclusions

After stubbornly stagnating for three decades, Southern racial education gaps declined markedly starting with cohorts born around 1910. While no one explanation could possibly capture all of the rapid convergence with Northern racial gaps that ensued in the next thirty years, we show that the Rosenwald Rural Schools Initiative was a primary contributor, explaining at least 30 percent and perhaps up to half of educational gains in the cohorts that we study. Incomes rose in line with educational improvements, with private rates of return of close to 20 percent for those who remained in the South. Moreover, the program stimulated greater migration to better labor market opportunities in the North. Finally, we provide evidence that the educational and economic gains were especially large in communities that were contending with the worst pre-Rosenwald educational conditions.

The Rosenwald-caused gains in human capital acquisition 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. Given the striking dearth of analytical research on this historically important program, we believe there is much to learn about other channels in which Rosenwald had an impact. Three such areas that we are currently pursuing are longer-term benefits of the program on permanent income, health, and Southern race relations.

Appendix A: School Location Selection

This appendix describes a set of regressions that show the association of pre-Rosenwald county characteristics on school location decisions. Pre-Rosenwald county characteristics primarily come from the 1910 Census and include race-specific measures of educational and economic status available at the time, such as school enrollment, literacy, and occupational status.[57] The regressions also incorporate state fixed effects and industry share controls. Some specifications are augmented to include non-Census measures reported in Johnson (1941), a Southern almanac 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. These measures tend to be from around 1930, and therefore could be best thought of as potential outcomes of Rosenwald but we include them as simple tests of model robustness. Because Oklahoma is not part of the Johnson data, those counties are excluded when Johnson variables are covariates.

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. These results also 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 that our results could be due to reverse causality. The results are generally similar whether we include the 1930 Southern Almanac measures of lynchings, farm ownership, and cotton farming intensity, as well as the inclusion of pre-Rosenwald political participation measures (not shown) generously supplied by Kenneth Chay and Kaivan Munshi.

[57] The occupational status measures are provided by IPUMS and are based on 1950 levels of income and education by occupation.

That said, an intriguing finding is that a ten 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 historical source, we do know that Washington and his colleagues at Tuskegee 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, 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, including interestingly White literacy which only seems to influence the construction of the first school in a county. But by 1931 (columns 7 to 9), there is some evidence, economically significant albeit statistically only marginally so, that counties with higher Black (and White) schooling rates in 1910 had greater Rosenwald exposure.

This finding could present a threat to the identification of causal effects. This would be especially true if we relied solely on cross-county differences. However, our econometric strategy is designed to be robust to a variety of ways to control for time-invariant and time-varying levels of White and Black background. Moreover, the results in table A4 provide further evidence 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.

Appendix B: AGCT test scores

Although the enlistment records database does not appear to contain test scores, Joe Ferrie discovered that a May 1943 Army training manual instructed punch card operators to input AGCT scores into the weight field (see Staff, Personnel Research Section 1947 and Ferrie, Rolf, and Troesken 2009). 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 A1 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 (Karpinos 1958). 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.

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

	(1)			(2)	(3)	(4)
					County, F.E.	**County, F.E.**
		Baseline, no contols		**Controls**	**No Controls**	**Controls**
	No Rosenwald	*Rosenwald*	Rose - No Rose Difference	Rose - No Rose Difference	Rose - No Rose Difference	Rose - No Rose Difference
Black Rural	0.658	0.732	0.074	0.058	0.072	0.059
	[0.006]***	[0.005]***	[0.007]***	[0.007]***	[0.008]***	[0.008]***
White Rural	0.834	0.834	0.000	-0.002	-0.004	-0.001
	[0.004]***	[0.002]***	[0.004]	[0.004]	[0.006]	[0.005]
Black Urban	0.766	0.79	0.024	0.022	0.016	0.018
	[0.008]***	[0.005]***	[0.009]***	[0.008]***	[0.010]	[0.009]*
White Urban	0.850	0.863	0.013	0.012	-0.010	-0.001
	[0.007]***	[0.004]***	[0.008]*	[0.006]*	[0.007]	[0.007]
Difference in Difference						
Black, Rur-Urb			0.050	0.036	0.057	0.041
			[0.011]***	[0.010]***	[0.010]***	[0.010]***
White, Rur-Urb			-0.013	-0.013	0.005	0.000
			[0.008]	[0.006]**	[0.006]	[0.005]
B-W Rural			0.074	0.059	0.076	0.061
			[0.007]***	[0.006]***	[0.006]***	[0.006]***
B-W Urban			0.011	0.010	0.025	0.019
			[0.011]	[0.009]	[0.009]***	[0.009]**
Triple Difference						
B-W Rur - B-W Urb			0.063	0.049	0.051	0.042
			[0.012]***	[0.011]***	[0.011]***	[0.010]***

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. "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)	(7)	(8)	(9)	(10)
Difference in Difference										
Black, Rur-Urb	0.061	0.056	0.066	0.054	0.106	0.037	0.085	0.062	0.076	0.053
	[0.018]***	[0.020]***	[0.021]***	[0.015]***	[0.017]***	[0.016]**	[0.019]***	[0.010]***	[0.081]	[0.080]
White, Rur-Urb	-0.023	-0.013	-0.005	-0.008	0.042	-0.006	0.047	0.001	0.004	-0.002
	[0.010]**	[0.010]	[0.011]	[0.009]	[0.011]***	[0.009]	[0.011]***	[0.006]	[0.033]	[0.033]
B-W Rural	0.12	0.119	0.119	0.113	0.113	0.076	0.074	0.113	0.087	0.072
	[0.012]***	[0.012]***	[0.011]***	[0.011]***	[0.011]***	[0.013]***	[0.013]***	[0.006]***	[0.045]*	[0.045]
B-W Urban	0.036	0.050	0.048	0.050	0.049	0.033	0.036	0.052	0.015	0.016
	[0.013]***	[0.013]***	[0.013]***	[0.013]***	[0.013]***	[0.014]**	[0.014]**	[0.009]***	[0.075]	[0.073]
Triple Difference										
B-W Rur - B-W Urb	0.084	0.069	0.071	0.062	0.064	0.043	0.038	0.061	0.072	0.056
	[0.016]***	[0.017]***	[0.017]***	[0.016]***	[0.016]***	[0.017]**	[0.018]**	[0.011]***	[0.087]	[0.086]
Baseline Controls	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**		
Age-St.-Yr			**Y**	**Y**	**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**	**Y**	**Y**	**Y**		
County by Year F.E.				**Y**	**Y**	**Y**	**Y**	**Y**		
Cnty by Year F.E, Age-Cnty-Yr								**Y**		
Family F.E									**Y**	
Family F.E, Birth Order										**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, 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 column (8) which do not use clustering.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Literacy Effects of Rosenwald School Presence in County

	(1)			(2)	(3)	(4)
					County, F.E.	**County, F.E.**
	Baseline, no contols			**Controls**	**No Controls**	**Controls**
	No Rosenwald	*Rosenwald*	Rose - No Rose Difference	Rose - No Rose Difference	Rose - No Rose Difference	Rose - No Rose Difference
Black Rural	0.764	0.835	0.071	0.062	0.079	0.065
	[0.006]***	[0.004]***	[0.007]***	[0.006]***	[0.006]***	[0.006]***
White Rural	0.980	0.974	-0.006	0.000	-0.006	-0.002
	[0.003]***	[0.001]***	[0.003]*	[0.002]	[0.003]	[0.003]
Black Urban	0.923	0.937	0.014	0.015	0.019	0.015
	[0.009]***	[0.005]***	[0.009]	[0.008]*	[0.008]**	[0.008]**
White Urban	1.015	0.998	-0.018	-0.010	-0.021	-0.013
	[0.004]***	[0.001]***	[0.004]***	[0.004]**	[0.005]***	[0.004]***
Difference in Difference						
Black, Rur-Urb			0.057	0.047	0.060	0.050
			[0.010]***	[0.009]***	[0.009]***	[0.009]***
White, Rur-Urb			0.011	0.010	0.015	0.011
			[0.005]**	[0.003]***	[0.004]***	[0.003]***
B-W Rural			0.077	0.062	0.084	0.067
			[0.006]***	[0.006]***	[0.006]***	[0.006]***
B-W Urban			0.032	0.025	0.040	0.028
			[0.009]***	[0.008]***	[0.009]***	[0.008]***
Triple Difference						
B-W Rur - B-W Urb			0.045	0.037	0.045	0.039
			[0.011]***	[0.010]***	[0.010]***	[0.010]***

Notes: Samples include approximately 390,000 individuals between the ages of 15 and 22 in the 1910, 1920 and 1930 IPUMs samples. Dependent variable is literacy. "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 4: Literacy Effects of Rosenwald Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Difference in Difference								
Black, Rur-Urb	0.156	0.150	0.152	0.135	0.120	0.092	0.069	0.137
	[0.019]***	[0.019]***	[0.021]***	[0.017]***	[0.017]***	[0.017]***	[0.017]***	[0.014]***
White, Rur-Urb	0.011	0.014	0.033	0.010	0.000	0.018	0.011	0.012
	[0.009]	[0.007]**	[0.008]***	[0.007]	[0.007]	[0.006]***	[0.006]*	[0.009]
B-W Rural	0.216	0.206	0.181	0.191	0.189	0.048	0.04	0.188
	[0.017]***	[0.016]***	[0.015]***	[0.015]***	[0.015]***	[0.014]***	[0.014]***	[0.010]***
B-W Urban	0.071	0.070	0.062	0.065	0.069	-0.025	-0.017	0.067
	[0.015]***	[0.015]***	[0.014]***	[0.014]***	[0.014]***	[0.015]*	[0.014]	[0.011]***
Triple Difference								
B-W Rur - B-W Urb	0.145	0.135	0.119	0.125	0.119	0.074	0.057	0.125
	[0.021]***	[0.020]***	[0.019]***	[0.019]***	[0.018]***	[0.017]***	[0.018]***	[0.015]***
Baseline Controls	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**	**Y**
Age-St.-Yr			**Y**	**Y**	**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**	**Y**	**Y**	**Y**
County by Year F.E.				**Y**	**Y**	**Y**	**Y**	**Y**
Cnty by Year F.E, Age-Cnty-Yr								**Y**

Notes: Sample includes approximately 390,000 individuals between the ages of 15 and 22 in the 1910, 1920 and 1930 IPUMs samples. Dependent variable is literacy. 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 and state dummies (column 1 only). Estimates use Census sampling weights. Standard errors, clustered on county are shown in brackets except for column (8) which does not use clustering.

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

Table 5: School Attendance Effects of Rosenwald Presence Using Initial Location of Schools in Alabama

Panel A: Effects of County Rosenwald Presence on Black-White School Attendance
for counties within X many miles of Montgomery, and effect relative to the rest of Alabama

	OLS				County Fixed Effects			
	20 miles	40 miles	60 miles	75 miles	20 miles	40 miles	60 miles	75 miles
effect on	0.203	0.109	0.094	0.08	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]*
	411	866	1920	3367	411	866	1920	3367
effect	0.205	0.138	0.116	0.104	0.221	0.109	0.094	0.105
relative to	[0.034]***	[0.067]**	[0.059]*	[0.061]*	[0.040]***	[0.087]	[0.065]	[0.067]
rest of AL	9164	9164	9164	9164	9164	9164	9164	9164

Panel B: 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				
	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	0.11	-0.03	0.015	0.23	**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]****
	2678	859	2279	1394	**6907**	2678	859	2279	1394	**6907**
cohorts	0.1	-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]****
	2389	761	2001	1266	**6148**	2389	761	2001	1266	**6148**

Notes: Estimates show the effect of the presence of a Rosenwald schools in Alabama by 1919 on black school attendance relative to whites. The first row of Panel A shows the effect only using counties within a specified distance from Montgomery Alabama where we think school location was idiosycratic. The second row estimates use data for the whole state and shows the estimated effect for blacks in the Montgomery area over and above the effect of Rosenwald schools elsewhere in the state. Panel B uses counties ocontiguous 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, father's occupational score and father's home ownership and state dummies. Regressions also control for the presence of a Rosenwald school in non-Alabama counties interacted with black. Estimates use Census sampling weights. Standard errors, clustered on county are shown in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

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

Panel A: Outcomes from 1910-1930 Census

	(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 <=1917	N	Y	Y
N	592472	352230	403058

Panel B: Outcomes from World War II Data

	(4)	(5)	(6)	(7)	(8)	(9)
	Education		Completed H.S.		AGCT Score	
Blacks	1.017	2.42	0.192	0.343	13.209	25.185
	[0.482]*	[0.830]**	[0.068]**	[0.127]**	[3.490]***	[7.568]***
Whites	-0.358	-0.456	-0.027	-0.06	-4.57	-2.578
	[0.150]**	[0.608]	[0.023]	[0.088]	[3.019]	[4.162]
B-W Difference	1.375	2.876	0.219	0.403	17.779	27.763
	[0.598]**	[1.243]**	[0.084]**	[0.168]**	[4.782]***	[8.492]***
Birth Year <=1917	N	Y	N	Y	N	Y
N	2294340	822539	2617232	990266	139784	36123

Panel C: Outcomes from 1940-50 Census

	(10)	(11)	(12)	(13)	(14)	(15)
	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 <=1917	N	Y	N	Y	N	Y
N	216782	139823	103744	68044	78601	53306

Notes: (To be completed) Samples for (1) and (2) pools the 1910-1930 IPUMS and includes individuals aged 7 to 17 born in Rosenwald states. All regressions include state fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Effects of Rosenwald Exposure on Human Capital, Height and Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Education		Completed H.S.		AGCT Scores		AGCT cond'l on Ed.		Height		Weight	
	No IPW	IPW	No IPW	IPW	No IPW	IPW	No IPW	IPW	No IPW	IPW	No IPW	IPW
All Counties												
Black	0.265	0.386	0.043	0.053	4.856	3.606	-0.539	-0.907	0.078	0.025	-0.669	-1.162
	[0.166]	[0.129]***	[0.021]**	[0.017]***	[1.919]**	[2.210]	[2.197]	[2.253]	[0.071]	[0.083]	[0.524]	[0.619]*
White	-0.159	-0.087	-0.009	-0.004	0.724	-0.166	0.756	-0.082	0.061	0.054	0.109	-0.198
	[0.040]***	[0.039]**	[0.006]	[0.006]	[0.981]	[1.306]	[0.799]	[1.205]	[0.063]	[0.070]	[0.355]	[0.396]
Black - White	0.424	0.473	0.053	0.057	4.132	3.772	-1.295	-0.825	0.018	-0.029	-0.779	-0.964
	[0.194]**	[0.147]***	[0.023]**	[0.019]***	[1.917]**	[1.649]**	[2.134]	[1.882]	[0.061]	[0.073]	[0.442]*	[0.562]*
N	1737720	1737720	1760781	1760781	89156	89156	88069	88069	866343	866343	866343	866343
Rural Counties												
Black	0.653	0.689	0.092	0.092	7.871	6.803	0.076	-0.082	0.055	0.011	-0.806	-0.702
	[0.128]***	[0.114]***	[0.017]***	[0.015]***	[1.791]***	[1.986]***	[1.429]	[1.705]	[0.081]	[0.095]	[0.521]	[0.607]
White	-0.167	-0.115	-0.017	-0.010	0.916	0.453	1.349	1.057	0.086	0.095	-0.294	-0.557
	[0.031]***	[0.035]***	[0.005]***	[0.005]**	[0.965]	[1.126]	[0.716]*	[0.837]	[0.056]	[0.078]	[0.371]	[0.467]
Black - White	0.820	0.803	0.109	0.102	6.956	6.350	-1.273	-1.139	-0.032	-0.084	-0.512	-0.144
	[0.143]***	[0.130]***	[0.018]***	[0.017]***	[1.656]***	[1.792]***	[1.292]	[1.528]	[0.066]	[0.077]	[0.451]	[0.496]
N	1262127	1262127	1278336	1278336	62477	62477	61692	61692	632687	632687	632687	632687
Urban Counties												
Black	-0.208	-0.223	-0.016	-0.023	2.609	-0.604	-0.158	-1.722	0.100	-0.050	0.053	-1.017
	[0.242]	[0.204]	[0.032]	[0.028]	[2.454]	[3.645]	[2.255]	[3.604]	[0.125]	[0.151]	[0.858]	[1.002]
White	-0.192	-0.109	-0.002	0.001	0.623	-1.092	0.182	-1.983	0.013	-0.105	0.925	0.697
	[0.086]**	[0.106]	[0.012]	[0.015]	[1.845]	[2.847]	[1.529]	[2.729]	[0.130]	[0.142]	[0.650]	[0.682]
Black - White	-0.016	-0.115	-0.013	-0.025	1.985	0.488	-0.340	0.261	0.087	0.055	-0.872	-1.714
	[0.300]	[0.247]	[0.037]	[0.032]	[2.374]	[2.312]	[1.983]	[2.607]	[0.098]	[0.104]	[0.676]	[0.826]**
N	475593	475593	482445	482445	26679	26679	26377	26377	233656	233656	233656	233656
B-W Rur - B-W Urb	0.836	0.918	0.122	0.127	4.973	5.860	-0.895	-1.406	-0.118	-0.139	0.360	1.570
	[0.331]**	[0.278]***	[0.041]***	[0.036]***	[2.893]*	[2.922]**	[2.400]	[3.028]	[0.118]	[0.129]	[0.810]	[0.960]
	1737720	1737720	1760781	1760781	89156	89156	88069	88069	866343	866343	866343	866343

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. Even numbered columns 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 8: 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)
	Education		Log Earnings		Log Wage	
	All	Stayers	All	Stayers	All	Stayers
Black	1.131	1.229	0.221	0.259	0.197	0.224
	[0.192]***	[0.196]***	[0.054]***	[0.055]***	[0.056]***	[0.059]***
White	-0.420	-0.351	-0.228	-0.173	-0.187	-0.135
	[0.103]***	[0.118]***	[0.042]***	[0.045]***	[0.033]***	[0.036]***
Black - White	1.551	1.580	0.449	0.432	0.384	0.359
	[0.207]***	[0.201]***	[0.058]***	[0.056]***	[0.060]***	[0.058]***
N	183331	143764	69131	52117	64156	48341
Mean for Blacks						

Panel B: Returns to Black Education, OLS and IV using Rosenwald Exposure

	(7)	(8)
	Log Wages, Blacks	
	OLS	IV
Education	0.049	0.176
	[0.003]***	[0.041]***

Panel C: Rosenwald Exposure on Migration

	(9)	(10)	(11)	(12)	(13)	(14)
	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]	[0.025]
White	0.005	0.000	0.009	0.013	0.014	0.003
	[0.006]	[0.007]	[0.010]	[0.013]	[0.015]	[0.011]
Black - White	0.003	0.025	-0.013	0.008	0.000	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. 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 summy, 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, and age interacted with state interacted with race interacted with year. Standard errors, clustered on state economic area or are shown in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: 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	0.270	0.158	0.061	-0.031	0.084	0.098	0.123	0.138
Rural	[0.029]***	[0.022]***	[0.016]***	[0.020]	[0.023]***	[0.020]***	[0.020]***	[0.024]***
	107407	131091	154349	198321	144081	169304	161485	116298

	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	0.046
Rural-Urban	[0.016]***	[0.017]***	[0.027]	[0.020]***	[0.018]***
	226198	154796	201908	293708	289173
B-W	0.104	0.099	0.15	0.113	0.107
Rural	[0.013]***	[0.014]***	[0.019]***	[0.013]***	[0.013]***
	226198	154796	201908	293708	289173

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	1.08	0.69	0.648	0.631	0.104	0.087	0.088	0.096
Rural	[0.358]***	[0.265]***	[0.229]***	[0.223]***	[0.047]**	[0.035]**	[0.028]***	[0.027]***
	249096	254463	311053	447515	252475	257295	317613	450953

	G. AGCT Scores by 1920 Black Attendance			
	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
B-W	14.279	3.654	3.897	2.987
Rural	[4.322]***	[3.804]	[3.026]	[3.744]
	12374	12181	16190	21732

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	1.914	1.365	1.359	1.385	0.656	0.344	0.357	0.299
	[0.360]***	[0.417]***	[0.306]***	[0.340]***	[0.111]***	[0.113]***	[0.109]***	[0.099]***
	42550	55542	41170	44069	12646	20910	14321	16279

Notes: Panels A through D are drawn from 1910 to 1930 Census 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

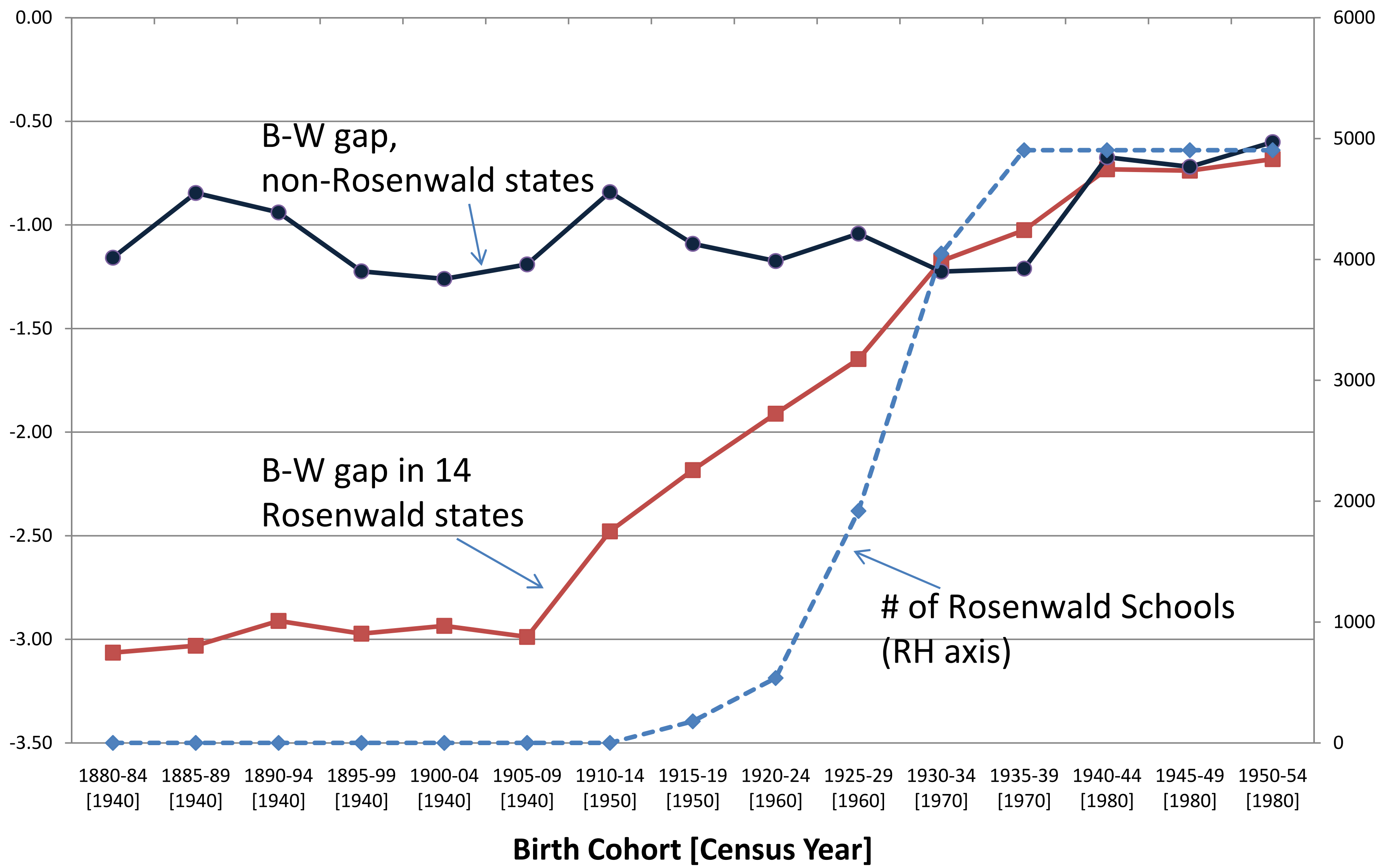


Figure 2: Counts of Rosenwald Schools by County as of 1919

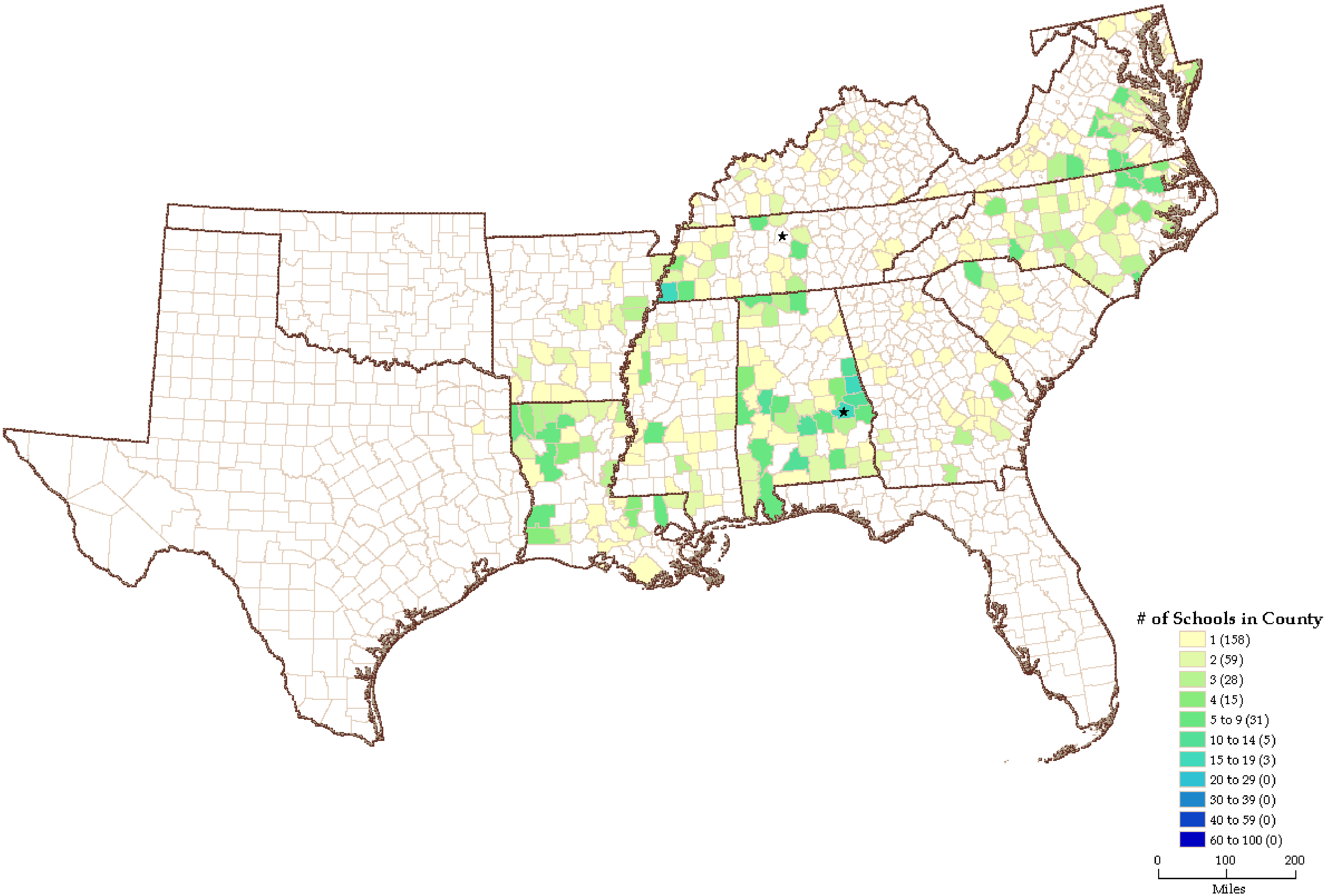
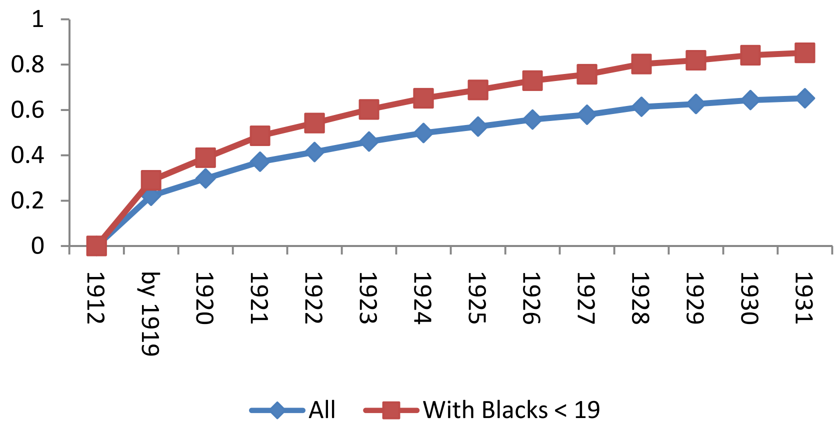
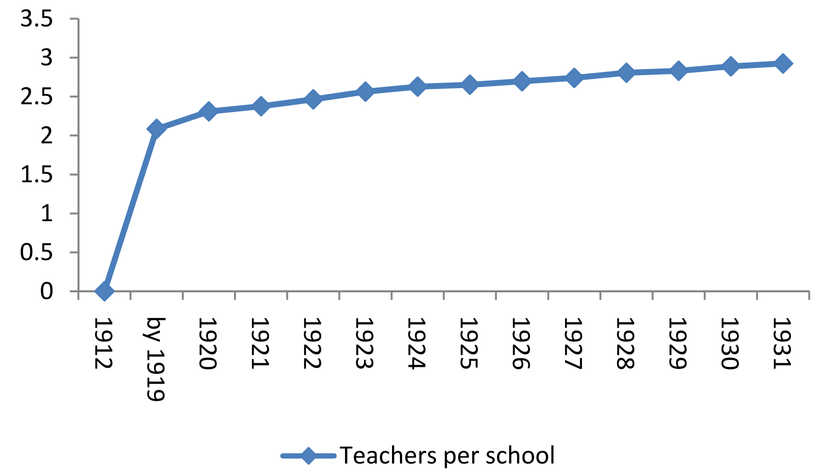


Figure 3: Trends in Rosenwald Project Activity

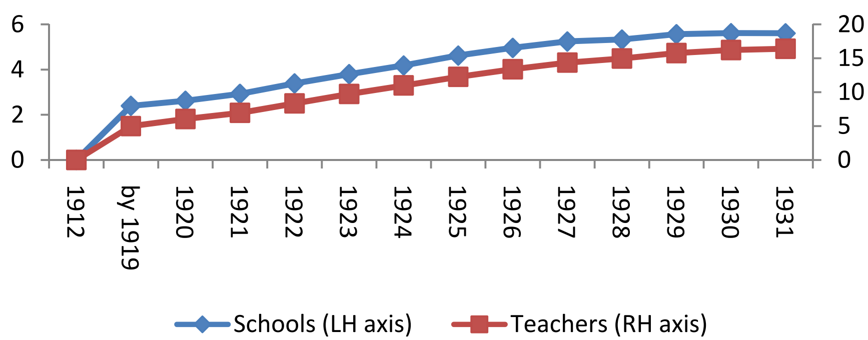
Across-county coverage: Share of counties w/ R schools



School size: Teachers per R school



Within-county coverage: Average number of R schools and teachers per R county



Cumulative Growth, 1919-1931	
Across-county coverage	194%
Within county coverage: Schools/ R county	134%
Within county coverage: Teachers / R county	229%
School size: Teachers per Rosenwald school	40%

Figure 4: Counts of Rosenwald Schools by County as of 1931

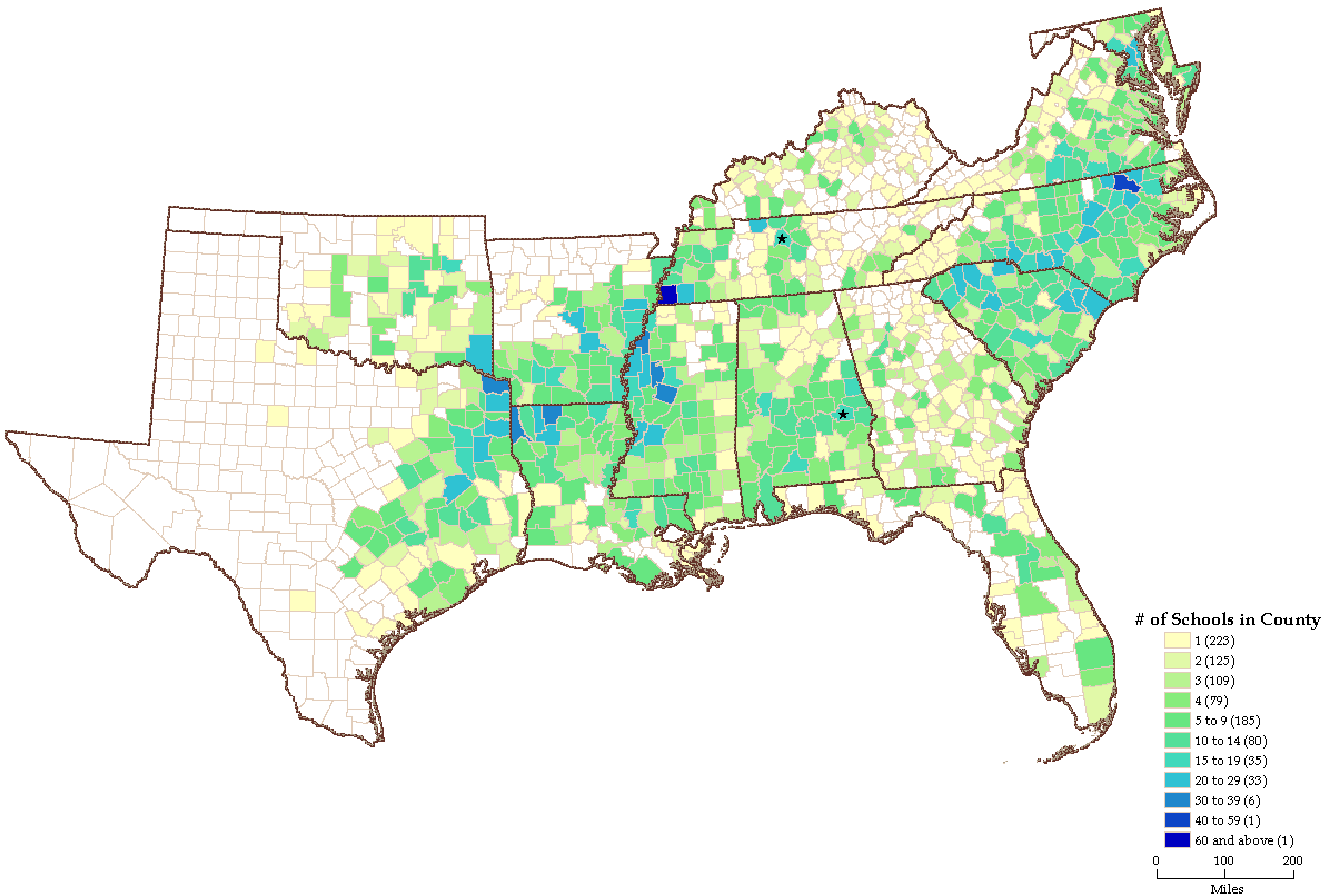


Figure 5: Distribution of Rosenwald Share of Rural Black School Age Children Across Counties

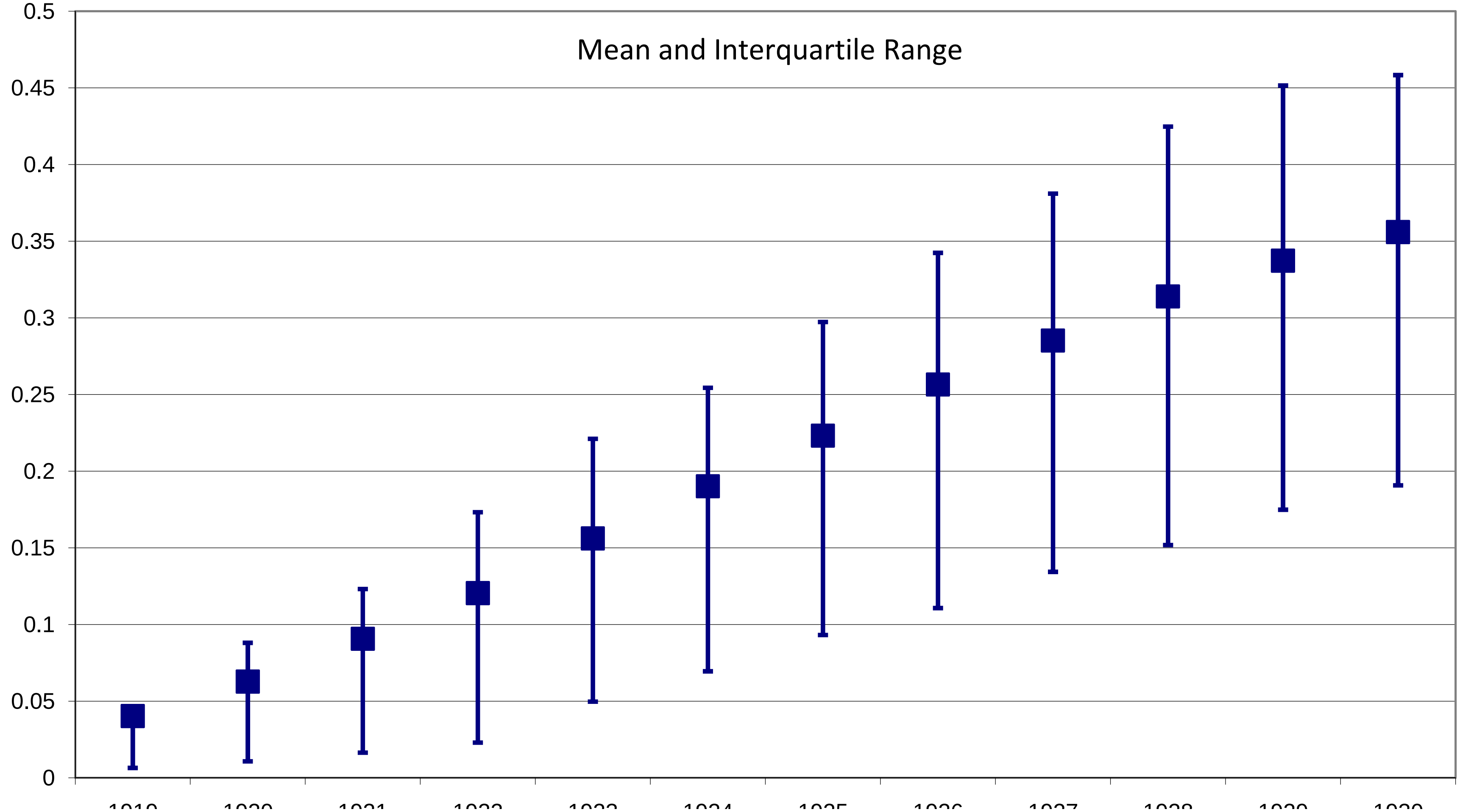
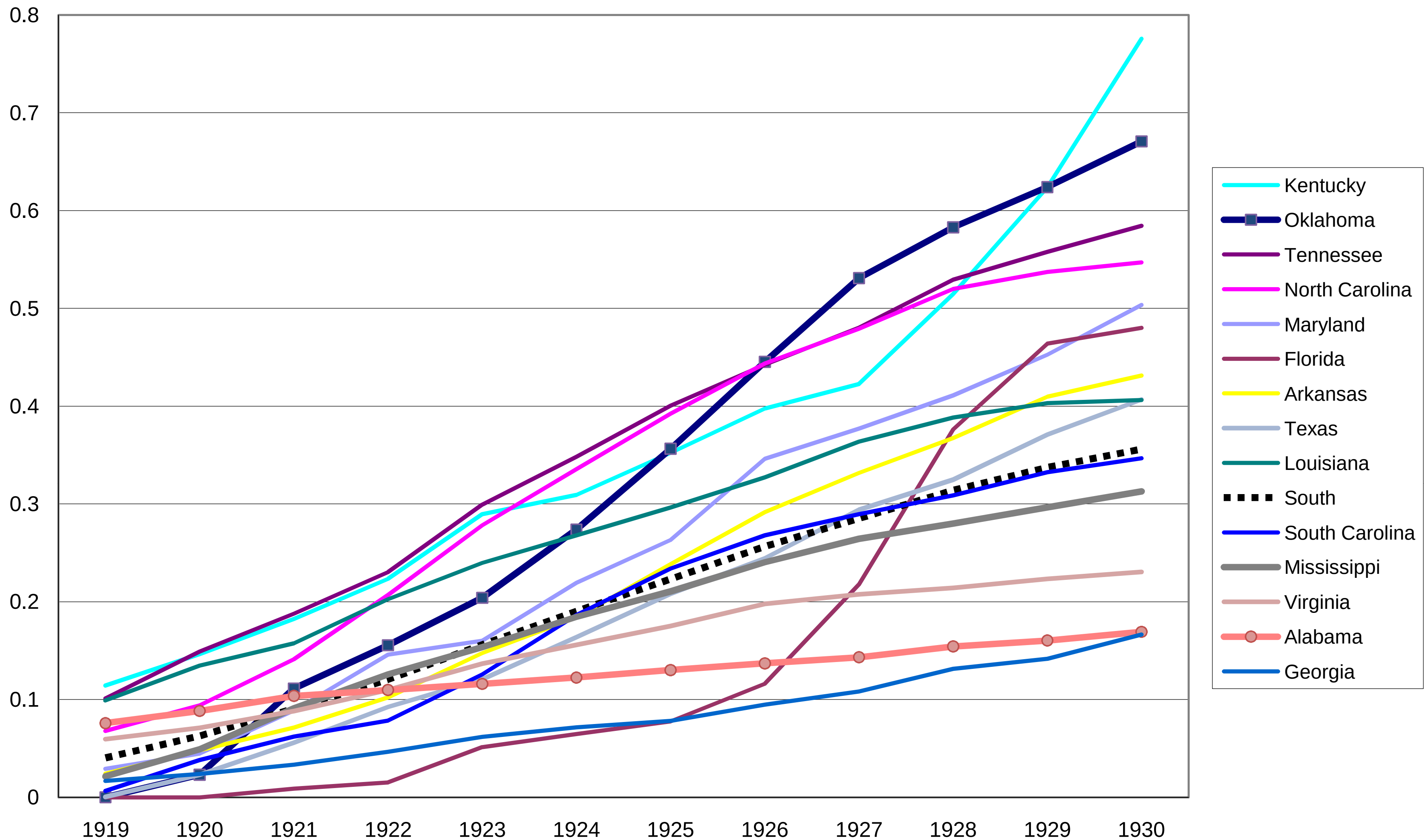


Figure 6: Estimated Share of Black Rural School Age Children in Rosenwald Schools by State and Year



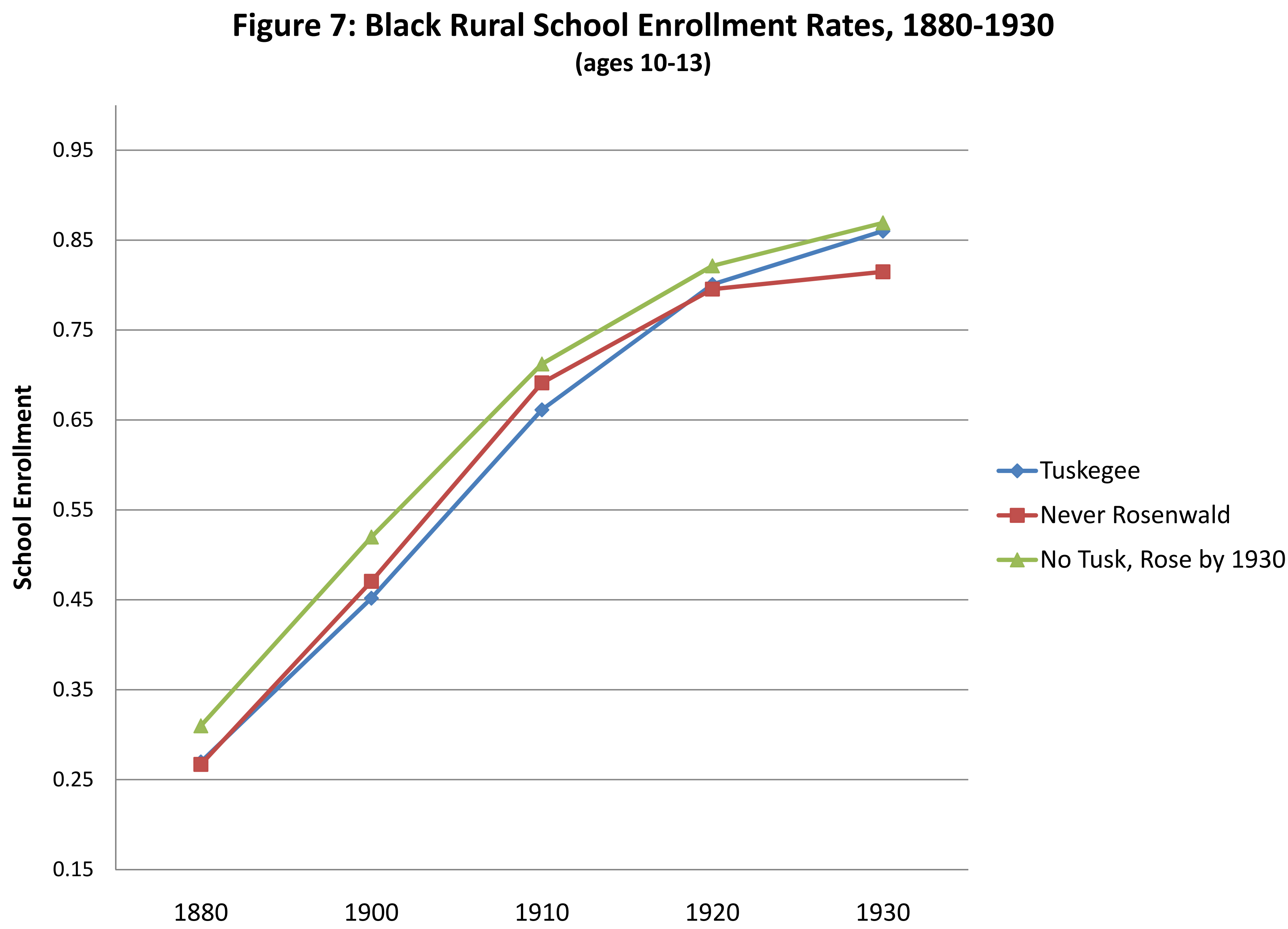


Figure 7: Black Rural School Enrollment Rates, 1880-1930
(ages 10-13)
School Enrollment
0.95
0.85
0.75
0.65
0.55
0.45
0.35
0.25
0.15
1880
1900
1910
1920
1930
Tuskegee
Never Rosenwald
No Tusk, Rose by 1930

Table A1: Descriptive Statistics about Rosenwald School Projects by School

	Mean	Std dev	Min	Max
School Building Details (n=4,968)				
Rosenwald school	0.993			
Rosenwald teacher homes	0.044			
Rosenwald shops	0.035			
Country Training Schools	0.045			
Fraction of schools with additions	0.062			
Fraction of schools rebuilt	0.006			
Fraction of schools burned	0.013			
Cost of Rosenwald Schools (n=4,932)				
Real Cost of original schools	5,374	8,083	583	169,761
borne by local Blacks	882	1,021	0	16,528
borne by local Whites	221	1,226	0	39,375
borne by local government	3,454	7,425	0	163,473
borne by Rosenwald Fund	816	575	58	7,859
Fraction of schools with positive contributions from:				
local Blacks	0.92			
local Whites	0.29			
local government	0.97			
Rosenwald Fund	1			
Real cost, including changes to schools (additions/rebuilds)	5,606	8,346	583	169,761

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

Table A2: Summary statistics of IPUMS samples

School Enrollment Sample of 7 to 17 year olds (N= 589258)

	1910		1920		1930	
	Blacks	Whites	Blacks	Whites	Blacks	Whites
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	0.54	0.88	0.64	0.90	0.73	0.93
Mother literate	0.51	0.88	0.67	0.92	0.80	0.95
Father Occ. Score	14.90	19.54	15.34	20.02	15.55	20.29
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	0.30	0.91	0.73
Exposure (ages 7 to 13)	0.00	0.00	0.02	0.02	0.29	0.31
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)

	1910		1920		1930	
	Blacks	Whites	Blacks	Whites	Blacks	Whites
Literacy						
All ages	0.71	0.94	0.80	0.96	0.87	0.97
Age 15 to 17	0.72	0.94	0.82	0.96	0.89	0.97
Age 18 to 22	0.70	0.93	0.79	0.96	0.86	0.97
Rural	0.68	0.93	0.77	0.95	0.84	0.96
Urban	0.85	0.98	0.90	0.98	0.94	0.99
Rosenwald Measures						
Presence in County (1920, 1930)	0.00	0.00	0.47	0.30	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	12109	33687	79848	201950

Table A3: Summary statistics of WWII enlisted men sample

	Pooled			Whites			Blacks		
	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N
<u>Outcomes</u>									
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
<u>Demographics</u>									
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
<u>Rosenwald Measures</u>									
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	2097087	0.27	0.33	1646535	0.27	0.30	450552
<u>Geography</u>									
% 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	-0.001	-0.001	-0.001	-0.014	-0.015	-0.014
	[0.003]***	[0.003]***	[0.004]***	[0.001]	[0.001]	[0.002]	[0.006]**	[0.007]**	[0.007]*
Black School Enrollment (1910)	0.001	0.005	-0.008	0.007	0.012	0.011	0.158	0.179	0.206
	[0.057]	[0.063]	[0.062]	[0.025]	[0.028]	[0.028]	[0.115]	[0.122]	[0.122]*
Black Literacy (1910)	-0.002	-0.001	-0.004	0.013	0.018	0.019	-0.065	0.020	0.036
	[0.072]	[0.083]	[0.082]	[0.032]	[0.036]	[0.037]	[0.144]	[0.162]	[0.161]
Black Occupational Status (1910)	0.006	0.008	0.010	0.003	0.004	0.004	0.014	0.015	0.012
	[0.004]	[0.005]	[0.005]*	[0.002]	[0.002]*	[0.002]*	[0.009]	[0.010]	[0.010]
Black Occupation Ed. Score (1910)	0.001	0.002	-0.002	-0.001	-0.002	-0.002	-0.004	-0.011	-0.003
	[0.005]	[0.007]	[0.007]	[0.002]	[0.003]	[0.003]	[0.009]	[0.013]	[0.013]
White School Enrollment (1910)	-0.045	-0.020	-0.030	0.026	0.034	0.032	0.402	0.357	0.367
	[0.105]	[0.114]	[0.114]	[0.046]	[0.050]	[0.051]	[0.210]*	[0.222]	[0.223]
White Literacy (1910)	0.554	0.568	0.502	0.042	0.052	0.062	0.252	0.293	0.465
	[0.188]***	[0.204]***	[0.207]**	[0.083]	[0.090]	[0.093]	[0.380]	[0.400]	[0.407]
White Occupational Status (1910)	-0.014	-0.015	-0.012	-0.004	-0.004	-0.004	0.001	0.006	0.001
	[0.007]**	[0.007]**	[0.007]*	[0.003]	[0.003]	[0.003]	[0.013]	[0.014]	[0.014]
White Occupational Ed. Score (1910)	0.011	0.012	0.010	0.004	0.003	0.003	0.008	0.003	0.006
	[0.006]*	[0.006]*	[0.006]	[0.003]	[0.003]	[0.003]	[0.012]	[0.012]	[0.012]
% Teachers (1910)	1.246	1.397	1.467	-0.013	-0.073	-0.038	3.413	2.365	2.457
	[1.306]	[1.485]	[1.472]	[0.579]	[0.651]	[0.654]	[2.639]	[2.899]	[2.880]
Cotton Share>25%			0.128			0.013			-0.152
			[0.068]*			[0.031]			[0.134]
Lynching			-0.024			-0.012			0.026
			[0.030]			[0.013]			[0.059]
Cotton Share>25% * Black Rural			-0.217			0.022			0.420
			[0.140]			[0.063]			[0.275]
Black Owned Farms			-0.003			0.000			0.004
			[0.001]**			[0.001]			[0.003]*
White Owned Farms			0.001			0.000			-0.004
			[0.001]			[0.001]			[0.003]
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	1033	956	956	1037	958	958

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

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