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Closing the gap? The effect of private philanthropy on the provision of African-American schooling in the U.S. south ☆

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ABSTRACT

Long-run labor market inequities are frequently attributed to disparities in primary and secondary school quality, and philanthropists often resort to targeted and tightly conditioned gifts to address these quality disparities. We match data on the Rosenwald Schools Program, an early 20th century initiative aimed at the Southern black–white school quality gap, to newly assembled data on local school districts and measure the impact of Rosenwald gifts on African-American public school resources. Although these gifts increased contemporaneous expenditures on African-American schools, results show that they yielded no lasting change in multiple school quality proxies. Further, because Rosenwald funds were diverted or implicitly matched to favor white schools, we find no evidence that the Fund reduced the black–white gap in superficial school quality. We demonstrate, however, that overall black public education expenditures in this era had a steeper marginal effect on black attendance and literacy measures than white public expenditures had on white outcome measures, which helps to explain why the Rosenwald program led to meaningful human capital gains for black, but not white, individuals despite its failure to impact relative school quality.

1. Introduction

Education quality is a common target of modern philanthropic efforts. Relying on evidence that long-run labor market inequalities can be attributed in large part to disparities in education provision, some of the country's best known foundations, including the Bill & Melinda Gates Foundation and the William & Flora Hewlett Foundation, include the advancement of educational achievement within disadvantaged communities in their mission statements. These groups (and others) have frequently sought to ameliorate gaps in wages, employment rates, and cognitive ability measures by giving conditional grants or donations to underprivileged schools and districts.[1]

We measure the impact of such targeted grants on education provision in disadvantaged communities using an historical case study. In the early years of the 20th century, the quality of education available to African-Americans in the U.S. South lagged far behind that available to whites.[2] A number of private individuals sought to address this

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[1] Estimates of the degree of inequality in black and white wages, employment rates, and test scores vary, but the literature universally reports a sizable and statistically significant gap. See Smith and Welch (1989), Altonji and Blank (1999), and others for labor market inequalities. See Fryer and Levitt (2004), Neal (2006), Card and Rothstein (2007), and others for differences in test scores. Card and Krueger (1992), Dobbie and Fryer (2011) and Curto et al. (2011) all find that differences in school quality are significant factors in the achievement and labor market wage gaps for black students, both today and historically. Curto et al. (2011) further argue that changing the quality of schools is *all* that is required to close the modern gap in academic achievement (although not necessarily labor market outcomes). Comparing initiatives that address school quality (e.g., charter schools) with those addressing the community or home environment, the authors find that the entirety of the gap can be addressed through quality schooling. A related line of literature argues that labor market wage gaps are largely explained by pre-market factors, including cognitive ability and labor market skills. See Neal and Johnson (1996).

[2] This is true for measures of spending per student, student/teacher ratios, term lengths and teacher salaries. See Margo (1990) and Donohue et al. (2002).

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disparity, and the largest source of private monies was the Julius Rosenwald Fund. Between 1912 and 1933, Rosenwald bankrolled the construction of nearly 5000 schools for black students across 15 Southern states. The Fund left a very large footprint in the South — the 2012 value of all Rosenwald contributions to Southern school enhancement is $23.1 million. By 1933 Rosenwald schools represented 21% of all black schools and housed 24% of black teachers in the five states we examine.

Our study is motivated by seemingly contradictory observations about Rosenwald-funded schools. First, recent evidence indicates that Rosenwald's investments succeeded in changing the trajectory of black achievement, both in an absolute sense and relative to whites. Aaronson and Mazumder (2011) find that the Fund significantly increased educational attainment, school attendance, literacy, and cognitive test scores for African-American students while imparting no comparable advantage to whites. At the same time, however, Donohue et al. (2002) observe that there is little evidence of a narrowing of Georgia's black–white school quality gap in the Rosenwald era. Our study of five states, including Georgia, echoes this conclusion. Although African-American student–teacher ratios, term lengths, expenditures per pupil, and average teacher salaries all improved over this period, these measures improved for whites as well, so much so that the ratio of black to white quality measures showed little change. So how could Rosenwald's targeted, large-scale philanthropic spending have failed to affect the gap in black–white school quality while significantly narrowing subsequent gaps in cognitive achievement?

Using a newly-constructed panel data set of county-level education and Rosenwald Fund data, we find that Rosenwald monies indeed effected no long-term change in the relative quality of black and white schools in five Southern states. Private Rosenwald gifts both crowded-out public funds for African-American education and were diverted to white schools. For example, we find that one dollar of private funds donated for the purpose of building a Rosenwald school resulted in $0.88 additional, immediate funds for black schools, including capital expenditures, but no significant gain even one year later. This influx was not enough, moreover, to narrow the funding gap between black and white schools; white schools gained $1.34 per Rosenwald dollar in the year of construction. We find no evidence that the grants served to close the gap in other measures of schooling provision including the number of teachers, term lengths, or school buildings.

We provide two potential explanations from the economics of education literature for Rosenwald's surprising success in affecting the black–white achievement gap despite his inability to improve the *relative* position of African-American students in the receipt of nominal education inputs. First, the absolute level of expenditures on both black and white schools rose with Rosenwald intervention. If the educational production function is characterized by sufficiently steep diminishing marginal returns to education inputs, then lifting the resources for black and white schools simultaneously could have had a larger impact on black individuals, whose schools lagged far behind those of whites at this point in history. We use microdata from the U.S. Census to show that this was the case in the U.S. South prior to 1940. In addition, we emphasize that Rosenwald's impact on individual well-being via unobserved factors like the quality of physical space and community engagement may have been substantial. Cellini et al. (2010) show that homeowners value school facility investments in the modern era, and since Rosenwald schools showed vast improvements over existing black schoolhouses, it is certainly conceivable that these buildings yielded long-term human capital gains in spite of crowding out.

The Rosenwald Fund's historic episode offers lessons on the use of private funds as vehicles for change in public education today. Despite strict conditions on the use of Rosenwald funds, we find that these donations did not completely "stick where they hit" in terms of increases in total spending, teachers, or facilities, and that they failed to narrow the gap between white and black school quality. Other research indicates that NAACP litigation and court-ordered desegregation were more effective in raising the relative quality of black schooling (Donohue et al., 2002; Reber, 2010, 2011), and even so, many parts of the South remained effectively segregated several years after the *Brown v. Board of Education* ruling (Ashenfelter et al., 2006). Modern philanthropists will be wise to expect crowd-out and diversion in response to donations, and monitoring and perfect enforcement of grant stipulations may be prohibitively costly in a number of contexts. Nevertheless, our findings and those of Aaronson and Mazumder (2011) are suggestive of the potential power of conditional gifts. If philanthropists are able to identify and affect areas with steeper marginal returns, they may achieve short-term and long-term objectives despite the fungibility of recipient resources.

2. Historic and economic context

The Rosenwald school-building program began in 1913 as a philanthropic partnership between Booker T. Washington, president of the Tuskegee Institute in Alabama, and Julius Rosenwald, president of Sears, Roebuck and Co. Following the successful construction of six schools in rural farming communities surrounding Tuskegee, Rosenwald agreed to expand the building campaign to other areas of Alabama, and later, other states. The objectives of the nascent program were to build better schoolhouses for rural black students and encourage longer term lengths and higher teacher salaries (Leavell, 1930). Management of the program shifted from Tuskegee to the Rosenwald Fund itself in 1920. At that point the volumes of application, approval, and construction grew tremendously and the design of schools became standardized by a freely-distributed series of bulletins entitled *Community School Plans* (Hoffschwelle, 2006, chap. 3).

"Rosenwald schools," as they quickly came to be known, numbered close to 5000 by 1933. The Fund also contributed to the construction of 179 workshops and 217 teachers' homes. In total, these buildings cost $28.4 million nominally (more than twice the value of all black rural school property in 1913), with Rosenwald and his eponymous Fund contributing $4.4 million (Embree, 1936, p. 14).[3]

Communities or school districts typically applied to the Rosenwald Fund, which in turn negotiated terms with recipient districts and facilitated fundraising campaigns for donations from citizens. The broad terms of Rosenwald's involvement in new construction projects were threefold. First, Rosenwald and Fund administrators insisted that each new school become part of the public school system.[4] Second, the Fund withheld its gift to recipient districts until agreed-upon matching funds had been raised by citizens and committed by districts.[5] Costs borne by the Fund averaged 20.9% but varied from school to school and generally shrank in later years. And finally, districts had to commit to at least a five-month term length,[6] meaningfully longer than the status quo. Eight-month terms were incentivized with additional funds for teachers' homes.

[3] In addition to $4.4 million in construction expenditures, fund expenditure tables indicate that $0.4 million was spent on administration with smaller sums going to libraries, "Initiating Bus Transportation to Consolidated Schools," rehabilitating existing schools, shop equipment, and various smaller causes (Embree, 1936, p. 22).

[4] Ascoli (2006): "The local county government had to not only approve but become actively involved. Implicit in this arrangement, as it had been from the beginning, was the concept that every Rosenwald school was a public school (p. 231)."

[5] An early agreement between the Fund and local governments states that Rosenwald challenge grants were intended "to encourage public school officers and the people in the community … by *supplementing* what the public school officers or the people themselves may do." Emphasis added. Quoted in Ascoli (2006), p. 141.

[6] Leavell (1930), p. 116; Embree and Waxman (1949), p. 41; Donohue et al. (2002), p. 239; Hoffschwelle (2006), p. 87. Additional items in the Fund's "Plan for the Distribution of Aid from the Julius Rosenwald Fund for Building Rural Schoolhouses in the South" dictated appropriate acreage, building plans, and speed of construction(Leavell, 1930, pp. 114-116).

It is not clear exactly how many applications were refused, nor does there appear to have been a formal method of monitoring district compliance after the schools were built.[7] The sheer volume and frequency of Rosenwald schools within counties likely resulted in indirect monitoring and encouraged at least short-term compliance in return for future support. On average, counties built new Rosenwald schools in five of the Fund's fourteen active years following 1920.[8]

Prior to Rosenwald's involvement, facilities for black students were often found in "churches, abandoned huts, lodge halls, or rented homes," and lacked indoor heating, textbooks, blackboards, and basic furniture.[9] By contrast, Rosenwald schools were professionally-designed structures with natural lighting, ventilation, and sanitary privies. In many cases Rosenwald schools were superior to nearby schools for white students[10] and influenced the design of new white schools.[11] The Fund and its beneficiaries were concerned about jealousy and retaliation from whites; they took steps to frame the school building program as a rising tide that lifted all races by improving the stock of skilled black labor. The process of fundraising for a Rosenwald school and subsequent construction of a school had positive spillovers throughout black communities. Ascoli (2006) notes that Rosenwald schools had a "galvanizing effect," so much so that the number of newly whitewashed and painted homes tended to increase with proximity to a new Rosenwald school.[12]

The Fund's school-building campaign had a large footprint throughout the Southeast, but economic theory generates an ambiguous prediction for the net impact of grants like those given by Rosenwald. On one hand, the public goods literature provides scenarios under which we would anticipate no sizable impact of private donations on the provision of black education (Heutel, 2009). If resource-constrained governments view private and public contributions as substitutes in the production of education, an increase in private support for education may yield less than a one-for-one increase in total funding.[13] Further, if local governments perceive higher utility returns to investments in *white* schools, the optimal response to receipt of a Rosenwald grant was to not only let the windfall crowd out counterfactual public education spending but also to re-allocate some of the designated gift to white schools. In that scenario, grants could have failed to generate any additional resources for Southern black schools and may have resulted in net gains for white schools. The racial and political climate of the U.S. South, including the severe disenfranchisement of Southern blacks and their absence from policy-making positions within school districts, may have affected the value that predominantly white district administrators placed on African-American education, adding additional motivation for fund diversion. Further, governments may have felt pressure to compensate white schools for any gains made by their African-American counterparts during the Rosenwald era.[14]

On the other hand, Rosenwald grants were designed to prevent this type of fund diversion. Matching gifts can increase public spending if a welfare-maximizing government sees fit to sacrifice consumption and increase the provision of public goods by taking advantage of the match. In practice, seed monies are often used to motivate donors and have been shown to induce greater giving overall (List and Lucking-Reiley, 2002). Further, as Donohue et al. (2002) note, tying contributions to capital assets like school buildings reinforced the permanence of Rosenwald gifts and reduced the feasibility of explicit diversion. These conditions could have amplified the fiscal impact of Rosenwald grants and weakened – or even reversed – the crowd-out of public funds.

As an additional consideration to underscore the ambiguous expectations about public spending's response to Rosenwald contributions, we note that African-Americans were migrating in large numbers to northern cities during the Rosenwald era. Given the importance of black labor to agricultural economies, Margo (1991) argues that public services like education were one lever that Southern governments used to stem black out-migration. Rosenwald schools may have crowded-in public spending in districts that used the Fund to spur additional resources for black schools.[15]

Thus the theoretical impact of Rosenwald grants on black school resources is ambiguous. Moreover, Rosenwald schools thrived against a complicated backdrop of progressive education policies as well as strained black–white relations. Compulsory schooling laws that required students to attend school through their mid-to-late teens were passed throughout the Southeast in the early 20th century. Although these laws had little impact on the duration of schooling in the United States, they yielded meaningful returns in the form of higher adult wages for black and white individuals (Oreopoulos, 2006). And though the 1910–1940 period this study examines coincides with the "nadir" of egregious discrimination, disenfranchisement, and violence against blacks (Loewen, 2005, chap. 2), Southern black schools saw modest improvements in the 1930s as governments preempted foreseeable challenges to segregation Ashenfelter et al., 2006. Estimates of the impact of Rosenwald schools on the provision of black education may be biased if these external factors coincided with or influenced Rosenwald involvement in local schools.

We empirically estimate the impact of Rosenwald funds on education resources for black and white schools, relying on a variety of selection controls to distinguish the influence of philanthropic influxes from underlying trends. County-by-race public education statistics, covering expenditures and other school quality proxies for five Southern states between 1910 and 1940, are matched to funding data from Rosenwald records. Counties differed in the size, number, and timing of Rosenwald involvement, and this sample variation is used to test for changes in public resources for African-American schools in the wake of Rosenwald receipts. As a baseline, a difference-in-difference approach is utilized to account for heterogeneous growth in expenditure and school quality measures unrelated to the Rosenwald initiative, but we also explicitly control for selection in other ways. Our approach to measuring the de facto impact of Rosenwald contributions on both black and white education resources is described in more detail in Section 5.1.

[7] Applications for Fund support were not guaranteed to meet with success, however. Donohue et al. (2002) note that nearly half of Alabama's potential Rosenwald gifts in 1928 were withheld for lack of district matching funds. The Fund placed great confidence in the fidelity of recipients. Embree (1936): "In giving away money a foundation need only assure itself of the general soundness and effectiveness of the recipient institutions; responsibility for all action and operation is left to the agencies which accept the gifts (p. 10)."

[8] By way of example, consider Carroll School in Rock Hill, South Carolina, built in the 1929–1930 budget year as a three-teacher school. Carroll school was one of the last of twenty-two Rosenwald schools built in York county. The school cost $4520 in total, with black citizens contributing $1745, tax funds contributing $2075, and the Fund contributing the remaining $700. The five-month (10 day) term length condition was likely not binding, as black schools in York County already averaged 114 days in session in 1929.

[9] Embree (1936), p.38.

[10] Embree and Waxman (1949), p. 43; Ascoli (2006), p. 150.

[11] Hoffschwelle (2006), pp. 111–115.

[12] p. 144.

[13] While a broad body of research has theorized and estimated the effect of governmental provision of public goods on private giving (see, among others, Kingma (1989), Ribar and Wilhelm (2002), and Gruber and Hungerman (2007)) the impact of private giving on governmental provision in the spirit of heutel09 has been relatively unexamined. See Gordon (2004) and Cascio et al. (2011) for evidence that federal grants can crowd out local school spending.

[14] As one commenter noted, "Maybe the [black–white] gap *was* the objective function."

[15] The idea that Rosenwald schools were a lever used to stem black outmigration has anecdotal support (Embree and Waxman, 1949, p. 46). Further, households who valued education (and by extension, were willing to pay more in local taxes to fund better schools) may have gravitated to areas with more Rosenwald involvement. This form of Tiebout (1956) sorting – over a number of years – could have increased public expenditures for black schools.

3. Data

We utilize newly transcribed data on county-level black and white public schooling statistics for the time period 1910–1940 for Alabama, Georgia, Louisiana, North Carolina, and South Carolina. Public school data are drawn from annual reports issued by each state's Department of Education or equivalent office. Throughout the late 19th century and much of the 20th century, state education agencies surveyed school districts annually or biennially regarding enrollment, attendance, teachers, expenditures, revenues, and teacher certification, among other measures. These data and the data collection process are described in more detail in the online appendix. Transcribed data are assembled into a county-by-race panel describing the students, teachers, and finances of each county in these states.[16,17]

The Rosenwald school-building program had two distinct phases. The Tuskegee Institute administered the first phase, beginning with six experimental schools in 1913 and concluding with the transition of administrative responsibilities to the Julius Rosenwald Fund in 1920. The Rosenwald Fund oversaw a vast expansion of the campaign. Under the Fund's control, school buildings were standardized in such a way that we can confidently identify the number of teachers who were meant to work at each campus. Following the 1920 managerial transition, the Fund also strengthened its insistence that local school districts take ownership of each new African-American school, a condition that allows us to more sensibly test for the effect of Rosenwald schools relying on counties' reported schooling statistics. We therefore focus on the post-1920 era and merge the transcribed education panel with information on every recorded Rosenwald school built after that date.[18] The Rosenwald schools data includes the name and location (county, state) of each school, the building's "plan" (number of classrooms), year of construction, total cost, and the division of that total cost between funding sources.[19] Rosenwald grants were challenge grants, requiring buy-in from recipient school districts and also from private citizens. Table 1 contains a summary of the schools' establishment dates, size, and funding in all fifteen states where the Rosenwald Fund was active, as well as in the five states we focus on here. Most of the schools were built to house two or more classrooms, and construction costs averaged $5653 per school (in 1925 dollars). On average, just over half of that cost was contributed by local school districts. The Fund itself contributed 20.9%, black citizens accounted for 22.1%, and white citizens contributed 4.1% on average. Summary statistics are very similar for the five-state subsample we focus on in the empirical analyses to follow.

Rosenwald and Fund administrators insisted that each new school become part of the public school system, and Rosenwald disbursements were made directly to the public sector in charge of school finances (Leavell, 1930, p. 115). Importantly, all sources of funds, both public and private, are contained in state revenue tables, making this an ideal setting to test whether private giving can crowd out the public provision of education.[20]

Table 1
Characteristics of Rosenwald schools.

	(1) All States with Rosenwald schools	(2) Five-state sample
Year established	Number of schools (percent)	Number of schools (percent)
1921–1925	2209 (47.6)	1000 (51.0)
1926–1930	2104 (45.4)	835 (42.6)
1931–1933	323 (7.0)	124 (6.3)
School type	Number of schools (percent)	Number of schools (percent)
One-Teacher	757 (16.3)	233 (11.9)
Two-Teacher	1685 (36.3)	698 (35.6)
Three-Teacher or Larger	1745 (37.6)	864 (44.1)
Missing school type	450 (9.7)	164 (8.4)
Total funding and sources	Mean (st. dev.)	Mean (st. dev.)
Total funding	5652.60 (8430.05)	5734.52 (8082.12)
Percent public	52.9 (25.1)	50.4 (22.3)
Percent black	22.1 (18.2)	24.0 (16.4)
Percent white	4.1 (10.1)	4.1 (10.6)
Percent Rosenwald	20.9 (8.2)	21.5 (7.9)

Source: Authors' calculations and the Fisk University Rosenwald Fund Database.

To control for other observable characteristics of counties, we match education and Rosenwald data with socio-demographic data from decennial population and agricultural censuses taken in 1910, 1920, 1930, and 1940. We interpolate between census years to form annual measures of population and agricultural variables. Relevant county-level statistics from these census reports include total population, black population as a percent of total population, crop value per capita, and agricultural land share.

4. Location of Rosenwald schools

In order to identify the effect of Rosenwald private grants on local education funding, we must explicitly address the site selection process for these schools. We begin by listing the determinants of Rosenwald school locations. Table 2 lists summary statistics as of 1919–1920 for four types of counties: counties where no Rosenwald schools were built by 1933, counties with 1–3 Rosenwald schools (the median), counties with 4–6 schools (the 75th percentile), and counties with more than 6 Rosenwald schools. There is more of a Rosenwald presence in larger counties, counties with higher black population shares, more urban or densely populated counties, counties with higher per-pupil spending overall, and counties where black enrollment encompasses a larger share of school-aged black youth. Together, these statistics suggest that Rosenwald was more active in relatively more advanced black school districts.

To measure the importance of county attributes in determining site selection, we explicitly estimate a model of Rosenwald siting between 1921 and 1933.[21] The estimating equation is

$$r_{cs} = \mathbf{Z}_c\beta + \theta_s + \varepsilon_c, \quad (1)$$

[16] Despite the fact that communities and school districts were the decision-makers in applying for and implementing Rosenwald funds, these statistics are measured at the county level in both the Rosenwald data and the annual reports of state departments of education. The reliance on county-level data should serve to bias our results toward zero, provided Rosenwald activity in one school district did not have large spillover effects within the same county.

[17] The transcription process is subject to human error, and although these errors should be white noise in our estimates, we strengthen the accuracy of data in two ways. First, we perform a targeted, non-random 0.5% audit of raw transcribed figures. Second, although this is, to our knowledge, the first attempt to transcribe and use these county-level data for multiple Southern states, a number of other studies have used subsets of these data or aggregated state-level data to measure public schooling resources. As a check of the validity of the data and the transcription process, we replicate summary statistics from earlier work and find broadly similar results. Audit and replication details are provided in the online appendix.

[18] Data on Rosenwald schools were taken from the online catalog at Fisk University: http://rosenwald.fisk.edu/.

[19] Rosenwald "plan" types were indexed by the number of teachers the building was intended to house, generally a number between one and ten. A "one-teacher" type corresponded not to a one-room schoolhouse, but to a school with a single classroom, a kitchen, a library, and a number of cloakrooms. Similarly, "four-teacher" schools included four classrooms, but a substantial amount of auxiliary space as well. See http://www.durhamcountylibrary.org/ncc/jeanes/schools/plans.php for examples on Rosenwald school plans.

[20] Some state Department of Education reports include a "Rosenwald Fund" column in revenue tables, but this recording practice is not consistent across time and states. Other likely accounts for Rosenwald monies include "Donations" or simply "Other Revenues."

[21] Note that pre-program county features are not adequate instruments for our purposes, since they likely affected post-Rosenwald schooling outcomes as well as the dosage of Rosenwald involvement.

Table 2
Pre-program characteristics of counties, by degree of Rosenwald presence.

	(1)	(2)	(3)	(4)
	Number of Rosenwald schools			
	None	1–3	4–6	7 or more
1919 or 1920 county characteristic				
Total population (000 s)	13.46	27.12	29.73	42.14
	(7.67)	(39.45)	(19.57)	(31.62)
Percent black population	33.47	37.37	46.11	48.25
	(25.40)	(21.33)	(18.86)	(17.11)
Percent urban population	0.04	0.13	0.11	0.16
	(0.08)	(0.21)	(0.13)	(0.17)
Population density	36.73	66.92	43.27	68.09
	(14.81)	(175.03)	(18.92)	(116.44)
Crop value per capita	214.06	180.04	183.38	219.49
	(88.25)	(101.91)	(92.07)	(118.89)
Black farmholder tenancy share	71.00	68.05	70.36	73.91
	(26.70)	(23.66)	(23.67)	(19.37)
Black literacy rate	0.60	0.62	0.60	0.63
	(0.15)	(0.11)	(0.09)	(0.07)
Total per-pupil spending (black and white)	10.91	18.13	17.46	19.68
	(6.77)	(14.20)	(10.62)	(12.30)
Black enrollment share	0.53	0.46	0.47	0.72
	(0.18)	(0.14)	(0.14)	(1.94)
Black term length	109.71	108.93	106.26	98.93
	(17.24)	(30.54)	(28.46)	(23.27)
Black student–teacher ratio	51.15	59.14	61.09	60.61
	(19.27)	(31.13)	(26.90)	(16.87)
n (counties)	69	158	66	97

Notes: The table summarizes pre-Rosenwald characteristics of counties, measured in 1919 or 1920, by the number of Rosenwald schools that were ultimately built between 1921 and 1933. Black enrollment shares and pupil–teacher ratios are from 1919 (when all five states reported those statistics), and other variables are from 1920. Standard deviations are listed in parentheses below each mean.
Source: Authors' calculations, numerous annual reports of states' Department of Education or equivalent office, and 1920 U.S. population and agricultural censuses.

Table 3
Eq. (1) results: Pre-program determinants of Rosenwald school location and timing.

	(1)	(2)	(3)	(4)
	Number of Rosenwald schools		Years until first Rosenwald school	
1919 or 1920 county characteristic				
Total population (000 s)	0.091***	0.082***	−0.017**	−0.012
	(0.015)	(0.015)	(0.008)	(0.008)
Percent black population	0.081***	0.073***	−0.027***	−0.031***
	(0.017)	(0.016)	(0.010)	(0.010)
Percent urban population	5.021**	2.235	0.818	1.448
	(2.454)	(2.329)	(1.274)	(1.320)
Population density	−0.018***	−0.016***	0.003	0.002
	(0.004)	(0.003)	(0.002)	(0.002)
Crop value per capita	0.012***	9.1E−05	−0.003	−0.001
	(0.004)	(0.004)	(0.002)	(0.002)
Black farmholder tenancy share	−0.019	0.053***	0.017*	−0.001
	(0.017)	(0.017)	(0.009)	(0.010)
Black literacy rate	9.208**	11.276***	−1.275	−3.244
	(3.685)	(3.701)	(2.048)	(2.200)
Total per-pupil spending (black and white)	0.081***	−0.02	−0.015	0.018
	(0.027)	(0.032)	(0.014)	(0.018)
Black enrollment share	0.625**	0.299	−0.245*	−0.182
	(0.301)	(0.273)	(0.148)	(0.145)
Black term length	−0.061***	−0.023	0.016**	−0.004
	(0.013)	(0.016)	(0.007)	(0.009)
Black student–teacher ratio	−0.013	−0.021*	4.8E−04	0.003
	(0.013)	(0.012)	(0.007)	(0.007)
State fixed effects	No	Yes	No	Yes
n (counties)	383	383	321	321
Adjusted R^2	0.24	0.38	0.05	0.10

Notes: The estimating equation is

$$r_{cs} = \mathbf{Z}_c\beta + \theta_s + \varepsilon_c$$

where Z_c includes pre-Rosenwald summary statistics from Table 2 and θ_s is a state fixed effect. In Columns 1–2, the dependent variable is the number of Rosenwald schools built over 1921–1933. In Columns 3–4, the dependent variable is the number of years between 1920 and the year a county received its first Rosenwald school (limited to counties with any Rosenwald schools). Black enrollment shares and pupil–teacher ratios are from 1919 (when all five states reported those statistics), and other variables are from 1920. Heteroscedasticity-robust standard errors are in parentheses below each coefficient.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

where r_{cs} is either the number of Rosenwald schools built between 1921 and 1933 or the timing of participating counties' first Rosenwald school (measured as the number of years between 1920 and the county's first Rosenwald school) for county c in state s. $\mathbf{Z}_c$ includes pre-Rosenwald characteristics summarized in Table 2 and θ_s is a state fixed effect. Results of this estimation are located in Table 3. Columns 1 and 2 show that more populous counties had more Rosenwald schools, even within states (Column 2 with state fixed effects). The Rosenwald schools program targeted rural areas, but it was certainly scalable and Tables 2 and 3 show that larger counties built more schools. Within states, counties with higher black population shares, lower population density, higher shares of black farmholders operating under tenancy, higher black literacy, and somewhat lower black pupil–teacher ratios built more Rosenwald schools. More populous counties and counties with higher black population shares also waited shorter periods to receive Rosenwald support.

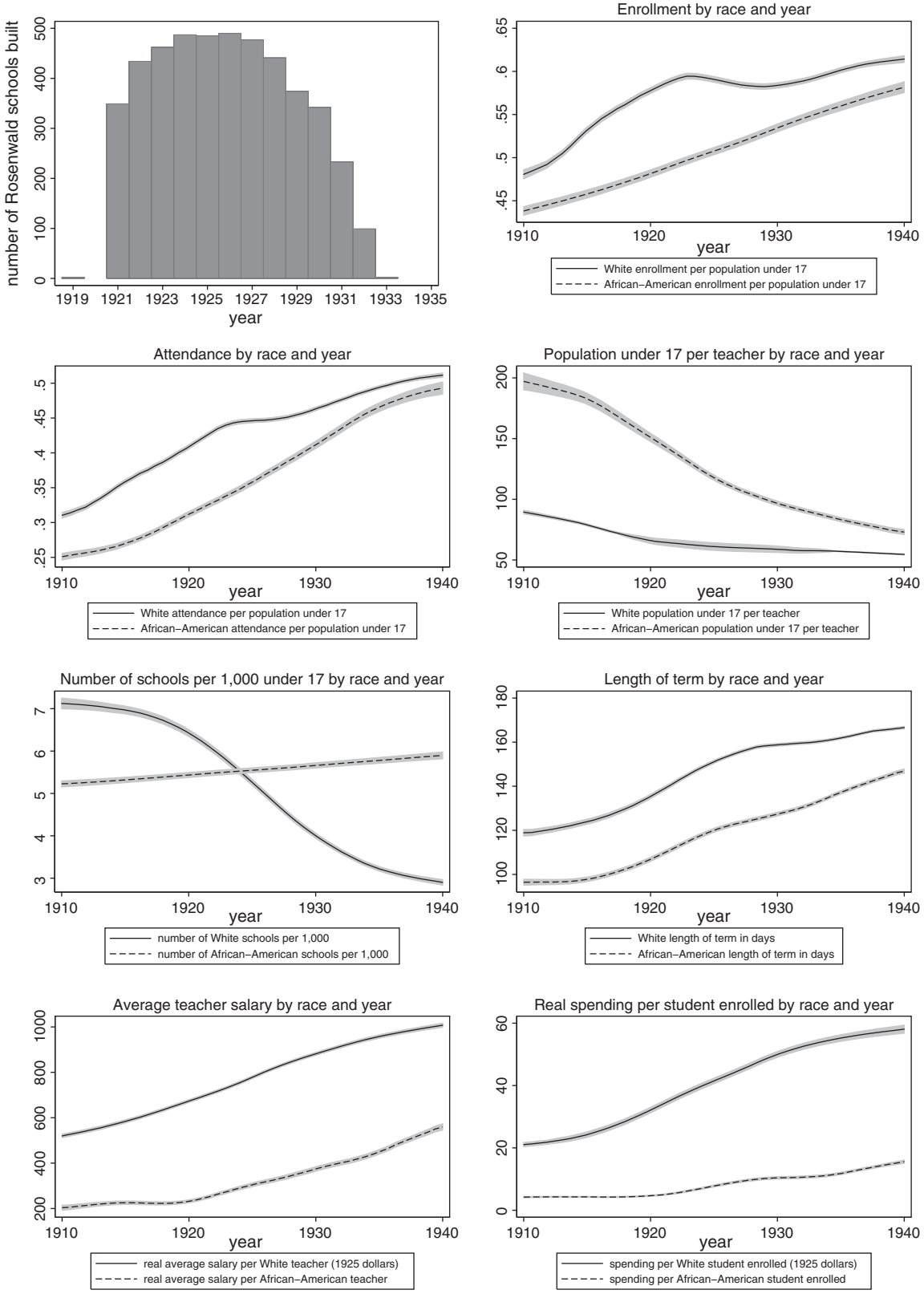
Yet, despite the large number of potential predictors of Rosenwald school establishment contained in $\mathbf{Z}_c$, the models listed in Columns 1–4 have very little predictive power. The *R*-squared value is 0.38 in the Column 2 model, indicating that these observable pre-program characteristics were good predictors of the dosage of Rosenwald treatment within states, but that there was also considerable unexplained variation.[22] We control for county observables and utilize the remaining idiosyncracy in Rosenwald funding for identification. Robustness checks described in Section 7 control explicitly for pre-program correlates of selection into the program.

5. The impact of Rosenwald gifts on education expenditures

As Fig. 1 indicates, schooling resources were generally on an upward trend throughout 1910–1940. A key challenge in identifying the effect of Rosenwald interventions on these resources is the fact that school siting may have been correlated with underlying trends. Our primary strategy to circumvent biases from endogenous trends is to identify the effect of Rosenwald interventions on the *growth* in school spending and other resources, conditional on heterogeneous county-level growth rates. We look for evidence of crowd-out and diversion in school spending by estimating the impact of newly donated private funds on changes in contemporaneous total county expenditures (by race) between years $t-1$ and t, controlling for changes in private contributions in years $t-1$ through $t-5$ and for a county-specific growth trend (a county fixed effect in the difference model). Estimates of crowd-out rely on the change in total black expenditures as the dependent variable while estimates of diversion rely on changes in total white expenditures.

We utilize a difference-in-difference approach to estimate the impact of Rosenwald interventions on school expenditures, teachers, schools and other resources. This approach allows us to condition on

[22] This is consistent with the conclusion of Aaronson and Mazumder (2011): "We show that black school attendance rates in 1910 and the change in those rates between 1900 and 1910 were similar between counties that received a school and those that did not and that the prevalence of Rosenwald schools across counties was not systematically related to observable measures of black socioeconomic conditions prior to the Rosenwald Fund's creation."



Source: Authors' calculations, numerous annual reports of states' Department of Education or equivalent office, and, for the upper-left figure, the Fisk University Rosenwald Fund Database.The last seven figures illustrate third-degree local polynomials of county-level statistics by year and race, with data weighted by the county-level population of white or black individuals under the age of 17.

Fig. 1. Rosenwald schools and education statistics in five southeastern states, 1910–1940.

county-by-race trends in each outcome variable of interest. We employ the following estimating equation:

$$\Delta Y_{ct}^{k} = \alpha^{k}\Delta R_{ct} + \sum_{s=1}^{5}\alpha_{-s}^{k}\Delta R_{c,t-s} + \Delta \mathbf{X}_{ct}\beta^{k} + \theta_{c}^{k} + \theta_{t}^{k} + \varepsilon_{ct}^{k}, \quad (2)$$

where ΔY_{ct}^{k} is the change in primary and secondary school spending for African-American or white schools ($k = a,w$) in county c and year t. ΔR_{ct} represents the change in private Rosenwald contributions between years t and $t-1$ (i.e., the sum of new or additional contributions from Rosenwald and private citizens), $\Delta \mathbf{X}_{ct}$ contains lagged changes in black or white enrollment, lagged changes in school districts' property tax revenues as reported in the schools data, as well as changes in interpolated population and agriculture data from decennial Censuses (total population, percent black, crop value per capita, and percent of land devoted to agriculture). The parameter θ_{c}^{k} is a county fixed effect and θ_{t}^{k} is a year fixed effect. ΔY_{ct}^{k} measures changes in all county school expenditures, including capital costs and spending from private contributions flowing through county budgets. In this and all estimating equations, robust standard errors are clustered to account for serially correlated errors within counties. Year fixed effects control for sample-wide shocks to educational spending growth. County fixed effects control for any unobserved heterogeneity in the linear growth of black or white spending that might be aligned with Rosenwald activity.

Unweighted estimates of α^{k} would represent the impact of private contributions on total expenditures averaged across counties. But we wish our estimates to measure the impact of the *average private dollar contributed to the Rosenwald initiative*, so our preferred specifications weight estimates by the total, cumulative value of private contributions to Rosenwald schools in each county as of 1933.[23]

5.1. Identification

Identifying variation in philanthropic interventions stems from the intensity of time-varying Rosenwald activities within counties. The estimated impact of higher Rosenwald contributions in county c, year t is identified relative to other years where county c benefitted from fewer or smaller Rosenwald schools.[24] Important to this empirical strategy is the fact that 62% of counties participated in the program multiple times over the 1920–1933 period.

The identifying assumption is that Rosenwald sites are exogenous interventions affecting school spending and other resources, conditional on observed, time-varying county characteristics and any unobserved heterogeneity in linear growth of school resources. The first-difference specification eliminates all unobservable, linear trends in school resources that may have been correlated with Rosenwald location as sources of bias. But a remaining concern inherent in this approach is the potential for differential non-linear trends in the outcome variables between Rosenwald and non-Rosenwald locations, which would then generate biased estimates of the impact of Rosenwald's intervention. If communities with non-linearly declining (rising) levels of school resources, for instance, were more likely to ask for Rosenwald assistance, and if these underlying trends continued after each school was built, then our estimates of the impact of each Rosenwald dollar, teacher, or classroom will be biased downward (upward). We explore these threats to causal inference with multiple robustness checks, including pre-treatment falsification texts and specifications that allow for differential trends in counties with a higher ex ante predicted Rosenwald presence. The issue of endogenous trends is explored more fully in Sections 5.3 and 7.

5.2. Interpreting coefficients

The coefficient of interest is α^{k}, the marginal effect of a $1 within-county increase in private Rosenwald contributions on the trajectory of total educational spending by race.[25] ΔR_{ct} includes both contributions from the Rosenwald Fund as well as private citizens' contributions that were a standard condition of Rosenwald involvement.[26] If districts simply allow all such contributions to flow to black schools without affecting other budget areas, and in the absence of district matching funds, we will see one-for-one gains in total spending on black schools ($\hat{\alpha}^{a} = 1$).

Because Rosenwald grants were conditioned on public matching funds in addition to private monies, identifying crowd-out of public spending in this framework requires some thought on the nature of the public match. Consider a district experiencing no growth in black education spending and suppose the district spent $20,000 in real terms on black schools in year $t-1$. In year t, suppose a Rosenwald school worth $6000 is built with a $3000 combined contribution from the Rosenwald Fund and private citizens as well as a $3000 contribution from the local school district. (According to Rosenwald records, districts contributed half of the necessary funds for school construction on average. See Table 1.) Perfect crowd out would manifest as $20,000 spent on black schools in year t: i.e., both the private and public contributions to the new school substitute perfectly for funds that *would have been spent* on black schooling in the absence of Rosenwald intervention. Our coefficient α^{a}, which measures the impact of private funds on changes in total spending for black schools, would be zero.[27] Observing $22,000, by contrast, would be indicative of incomplete crowd out ($\hat{\alpha}^{a} = 0.67$) as 2/3 of private contributions flowed through to spending on black schools but matching public funds did not materialize. An increase to $23,000 in black educational spending would be evidence that private gifts increased total expenditures one-for-one (and accordingly, $\hat{\alpha}^{a} = 1$), but would still represent crowd-out in public expenditures as the $3000 public match displaced funds that would have been spent anyway. Indeed any estimate of $\alpha^{a} < 2$ indicates that some portion of the earmarked Rosenwald monies were diverted to other uses. This is our baseline definition of crowd-out.[28] Complete compliance with Rosenwald's stipulations should result in $\hat{\alpha}^{a} \approx 2$ in the spending equation for black schools.

Since spending on Rosenwald schools was predominantly capital outlay, we might expect the grants to have had at least a "lumpy" effect on overall school spending, exhibiting a significant, positive impact on

[23] Results for the effect of Rosenwald interventions on spending and other educational resources are robust to several modifications of this empirical strategy, each available on request. Unweighted findings are qualitatively similar but show an even larger diversion to white schools. Adding controls for county-specific linear time trends to Eq. (2) (to control for quadratic underlying trends that might be correlated with site selection) yields estimates that are similar in sign and significance to those discussed below. Estimating the effect of Rosenwald schools on resource *levels* rather than differences yields qualitatively similar conclusions for outcomes that are less characterized by potentially endogenous trends (e.g., spending) but level specifications tend to overstate the impact on strongly trending outcomes (e.g., teacher salaries).

[24] 82% of counties in the five-state sample built at least one Rosenwald school. Counties with no Rosenwald presence do not contribute to the identification of Rosenwald treatment effects but are retained in the estimating sample to identify coefficients on control variables.

[25] Expenditures are not disaggregated by race in North Carolina, so Eq. (2) applies only to Alabama, Georgia, Louisiana, and South Carolina. See the online appendix for more information.

[26] Private donations to school districts, including all non-governmental contributions to the Rosenwald project, are generally included in revenues reported to state departments of education.

[27] This conclusion does not depend on *how* educational funds are reallocated with Rosenwald involvement. Crowd out substitution may be made within educational capital outlay expenditures (i.e., the $3000 gift crowds out other, counterfactual capital projects), or across major educational expenditure categories and even other functions of local government.

[28] Note that income effects will increase spending, increase α^{k}, and therefore, understate the true extent of crowding out.

Table 4
Eq. (2) results: Impact of private Rosenwald contributions changes on local tax revenues and total spending in African-American and white public schools.

	(1)	(2)	(3)
	African-American School spending	White School spending	Local Tax revenues
$\hat{\alpha}^k$ Current Rosenwald contributions	0.877***	1.335**	−1.1E−04
	(0.159)	(0.518)	(0.001)
	[0.565, 1.189]	[0.319, 2.351]	[−0.002, 0.002]
$\hat{\alpha}^k_{-1}$ Rosenwald contributions, one year lag	0.023	1.656	1.6E−04
	(0.163)	(1.284)	(0.001)
	[−0.296, 0.343]	[−0.862, 4.174]	[−0.002, 0.002]
$\hat{\alpha}^k_{-2}$ Rosenwald contributions, two year lag	−0.306*	0.940	−0.001
	(0.176)	(0.980)	(0.001)
	[−0.561, 0.040]	[−0.981, 2.861]	[−0.003, 0.002]
$\hat{\alpha}_{-3^k}$ Rosenwald contributions, three year lag	−0.242	0.122	0.002
	(0.152)	(0.585)	(0.002)
	[−0.541, 0.056]	[−1.026, 1.269]	[−0.001, 0.006]
$\hat{\alpha}^k_{-4}$ Rosenwald contributions, four year lag	−0.188	−0.113	0.002*
	(0.120)	(0.650)	(0.001)
	[−0.422, 0.046]	[−1.387, 1.162]	[−4.8E−04, 0.005]
$\hat{\alpha}^k_{-5}$ Rosenwald contributions, five year lag	−0.097	−0.829	9.0E−05
	(0.126)	(0.592)	(0.001)
	[−0.344, 0.149]	[−1.990, 0.332]	[−0.001, 0.001]
n (county-years, 1916–1940)	3444	3444	3529
Adjusted R^2	0.09	0.09	0.10

Notes: The estimating equation is

$$\Delta Y^k_{ct} = \alpha^k \Delta R_{ct} + \sum_{s=1}^{5} \alpha^k_{-s} \Delta R_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}$$

where Y_{ct} represents total school expenditures, by race, and R_{ct} measures total private expenditures on Rosenwald initiatives. θ^k_c is a county fixed effect and θ^k_t is a year fixed effect. Unreported control variables include changes in enrollment (by race and lagged one year), and changes in Census variables (interpolated between decennial years: total population, black population share, crop value per capita, and percent of land devoted to agriculture). Spending regressions also control for changes in revenues from local taxes (lagged one year). Regressions are weighted by total, county-wide Rosenwald contributions from 1921–1933 and estimated with heteroscedasticity-robust standard errors clustered within counties. Standard errors are in parentheses below each expenditure coefficient and 95% confidence intervals are in brackets.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

contemporaneous expenditures and perhaps small impacts on subsequent spending driven by ongoing term length mandates.[29]

Our study is unique in the philanthropy and public finance literature in that we can identify crowding out per se as well as one potential avenue for diversion of funds: spending on white schools. Although fund administrators placed tight constraints on each grant, districts may have nevertheless allocated counterfactual African-American educational spending to white schools in response to Rosenwald grants. If private contributions had a perfect flypaper effect such that monies were not diverted to white schools, we should observe no significant impact of private Rosenwald funds on white expenditures. Thus, $\hat{\alpha}^w = 0$ is the point estimate in the equation for ΔY^w_{ct} corresponding to no diversion.

5.3. Expenditure results

Table 4 contains the results from estimating Eq. (2) for white and black spending in the four states (Alabama, Louisiana, Georgia, and South Carolina) where spending was reported by race. Column 1 displays strong evidence of crowd-out in the funding of African-American schools in response to private Rosenwald donations. For each $1 increase in Rosenwald contributions, we find that total spending on black schools increased by $0.877. The 95% confidence interval for this point estimate includes $1, so we do not reject the hypothesis that private Rosenwald monies increased contemporaneous spending one-for-one. But we solidly reject the null hypothesis that $\alpha^a = 2$. Crowd-out is even greater when the expenditure effects of Rosenwald contributions are allowed to lag the initial gift year. Summing over the first three years of the grant (i.e., summing α^a coefficients in Column 1), the cumulative spending impact of a $1 private Rosenwald gift is insignificantly different from zero,[30] implying that Rosenwald-induced giving had no lasting impact on growth in black schooling expenditures.

Estimating $\hat{\alpha}^w$ for white expenditures, we find evidence that Rosenwald monies were diverted or matched in favor of white schools. White school spending increased with each new Rosenwald school, gaining $1.335 for every $1 of private Rosenwald contributions. Looking at lagged impacts in subsequent years, the three-year impact on white expenditures is weakly significant but potentially even larger.[31]

In spite of rapid fadeout and diversion, Rosenwald contributions had a relatively large impact on African-American spending growth in the year of school construction. If each dollar of private contributions "stuck" to the ledgers of African-American school systems at a rate of $0.877, then increasing countywide private contributions to Rosenwald schools by one standard deviation ($2342 during the active building years of 1921–1933) would have led to a one-time gain equal to 9% of a standard deviation in spending growth ($22,746 over 1921–1933). This short-lived impact may have had long-lasting benefits for the individual students affected by Rosenwald schools. By comparison, the effect of Rosenwald-induced funds for white schools, scaled by standard deviations, was only 3%.

In order to confirm that the results noted in Table 4 are not driven by differential long-run trends in expenditures in Rosenwald and non-Rosenwald counties, we implement a pre-treatment falsification test. We measure the "impact" of future Rosenwald interventions on current levels of school quality measures. Specifically, we estimate Eq. (2), substituting controls for *leading* Rosenwald spending (i.e., identifying the false "effect" of $R_{c(t+s)}$ on year t outcomes for $s = 1,2,3,4,5$).

[29] Although the historical record indicates rapid construction once citizen and public funds were accounted for, if school construction took place over a number of years, we should see $\hat{\alpha}_{-k} > 0$ and should sum α parameters over a short time-frame to get a true measure of the program's impact.

[30] The 95% confidence interval for $\hat{\alpha}^a + \hat{\alpha}^a_{-1} + \hat{\alpha}^a_{-2}$ is [−0.255, 1.445].

[31] The 95% confidence interval for $\hat{\alpha}^w + \hat{\alpha}^w_{-1} + \hat{\alpha}^w_{-2}$ includes zero, but the 90% confidence interval is weakly above zero: [0.006, 7.885].

Table 5
Pre-Rosenwald trends in school expenditures and other resources.

	(1)	(2)	(3)	(4)	(5)
Rosenwald intervention measure	Contributions	Teachers	Teachers	Schools	Schools
School resource	Expenditures	Teachers	Salaries	Schools	Term length
African-American school population					
$\hat{\alpha}^k_{+5}$ Intervention in 5 years	−0.144*	0.015	−0.064	0.361**	−0.185
	(0.086)	(0.079)	(0.360)	(0.141)	(0.246)
$\hat{\alpha}^k_{+4}$ Intervention in 4 years	0.089	0.062	−0.178	0.204**	−0.154
	(0.150)	(0.053)	(0.243)	(0.102)	(0.273)
$\hat{\alpha}^k_{+3}$ Intervention in 3 Years	−0.157	−0.086	0.098	0.150	0.479
	(0.123)	(0.082)	(0.285)	(0.101)	(0.293)
$\hat{\alpha}^k_{+2}$ Intervention in 2 years	0.033	0.025	0.770***	0.124	0.012
	(0.090)	(0.071)	(0.263)	(0.109)	(0.346)
$\hat{\alpha}^k_{+1}$ Intervention in 1 year	0.247	0.125	0.646*	−0.038	0.857***
	(0.171)	(0.080)	(0.369)	(0.114)	(0.305)
$\hat{\alpha}^k$ Current-year intervention	1.040***	0.306***	0.844***	0.150	0.491**
	(0.113)	(0.055)	(0.276)	(0.133)	(0.246)
White school population					
$\hat{\alpha}^k_{+5}$ Intervention in 5 years	−1.396**	0.115	−0.279	0.012	−0.086
	(0.633)	(0.075)	(0.793)	(0.124)	(0.168)
$\hat{\alpha}^k_{+4}$ Intervention in 4 years	0.764	0.211*	1.227**	−0.519***	−0.143
	(1.013)	(0.118)	(0.482)	(0.151)	(0.209)
$\hat{\alpha}^k_{+3}$ Intervention in 3 years	−0.260	0.235**	1.229	−0.231*	−0.214
	(0.565)	(0.108)	(0.774)	(0.127)	(0.198)
$\hat{\alpha}^k_{+2}$ Intervention in 2 years	−0.118	0.403***	0.965**	−0.184	−0.212
	(0.755)	(0.150)	(0.465)	(0.118)	(0.166)
$\hat{\alpha}^k_{+1}$ Intervention in 1 year	0.082	0.459***	2.107***	−0.062	0.008
	(0.696)	(0.154)	(0.717)	(0.130)	(0.205)
$\hat{\alpha}^k$ Current-year intervention	1.336**	0.733***	1.409***	−0.292*	0.318**
	(0.671)	(0.160)	(0.483)	(0.177)	(0.138)

Notes: The table lists results from estimates of Eqs. (2)–(4), regressing current changes in expenditures, teachers, salaries, schools, and term length on past, present, and *future* Rosenwald interventions (spending, teachers, or schools). Unlisted control variables include county fixed effects, year fixed effects, changes in enrollment (by race and lagged one year), changes in revenues from local taxes (lagged one year), and changes in Census variables (interpolated between decennial years: total population, black population share, crop value per capita, and percent of land devoted to agriculture). Regressions are weighted by cumulative 1921–1933 county-wide Rosenwald contributions, (Column 1), classrooms (Columns 2–4), or schools (Columns 5–6) and estimated with heteroscedasticity-robust standard errors clustered within counties. Standard errors are in parentheses below each coefficient. The relevant data years are 1916–1935.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

Estimates for pre-Rosenwald trends in black and white expenditures are located in Table 5, Column 1. We observe no remarkable trend in black school expenditures (top panel of Table 5) or white expenditures (bottom panel) to qualify the results relating to crowd-out and diversion of public expenditures. Pre-treatment trends are evident for other resources, which has bearing on Section 6 results to come.

Taken together, point estimates for black and white spending indicate that contemporaneous local government spending on all schools increased by *more* than the cost of building a typical Rosenwald school. $(0.877 + 1.335 > 2)$ This sort of crowding in is exceedingly rare in the public finance literature, where any degree of "flypaper effect" is considered an anomaly with behavioral, rather than economic, foundations (Hines and Thaler, 1995). Indeed, we would expect exogenous private giving to result in lower taxes (the source of public funding) if Rosenwald monies were viewed as close substitutes to other sources of educational finance. Yet the third column of Table 4 indicates that school districts' revenues from local taxes did not change in response to private giving.

One possible explanation for crowding-in and diversion to white schools is that white citizens' utility was dependent in part on the quality of white schooling resources *relative to* black schooling resources. Governments, in turn, would compensate white citizens, who perceive their own schools as losing ground, for the welfare-reducing effects of additional spending on black schools. Further, local governments may have been emboldened in diversionary endeavors by the exclusion of blacks from the political process. If so, a priori, we expect that counties with more disenfranchised black populations diverted and/or matched a greater share of Rosenwald resources to benefit white schools. We also expect that counties where black schooling levels were already relatively high diverted more resources to white schools and had a greater rate of crowd-out in an attempt to maintain the overall quality gap. To test these hypotheses, we bifurcate the sample within states into counties with above-median and below-median 1860 black population shares (a measure of the strength of the slave economy in the 19th century and the historical level of disenfranchisement),[32] 1920 black literacy rates (a measure of contemporaneous political enfranchisement), or 1915 black enrollment shares (a measure of the density of black schooling immediately prior to the Rosenwald campaigns), and re-estimate Eq. (2). Results (not shown, available in the online appendix) indicate that the gap between black and white spending gains attributable to Rosenwald involvement was largest (1) where race relations were poorest, as measured by 1860 population shares, (2) where blacks were more politically disenfranchised in the early 20th century according to literacy, and (3) where a larger share of blacks were already enrolled in school.

6. The impact of Rosenwald gifts on other school quality measures

The richness of county-level schooling data also allows us to test for crowd-out and diversion in other measures of school resources beyond expenditures. We measure the impact of the Rosenwald initiative on the number of employed teachers, average teacher salaries, the number of schools, and average term lengths for both African-American and white schools, again evaluating for both crowd-out and diversion.

[32] Black population shares were higher in "Black Belt" Southern counties with disproportionate emphasis on plantation crops. Post-emancipation, these counties experienced poorer race relations and more black disenfranchisement. See Aaronson and Mazumder (2011).

Table 6
Eq. (3) results: Impact of Rosenwald teachers on number of teachers and teacher salaries.

	(1)	(2)	(3)	(4)
School resource	Number of teachers		Annual teacher salaries	
School population	African-American	White	African-American	White
$\hat{\gamma}^k$ Current Rosenwald classrooms	0.215***	0.394***	0.378	0.001
	(0.048)	(0.095)	(0.263)	(0.557)
$\hat{\gamma}^k_{-1}$ Rosenwald classrooms, one year lag	0.346***	0.199	0.483	1.087**
	(0.080)	(0.151)	(0.304)	(0.506)
$\hat{\gamma}^k_{-2}$ Rosenwald classrooms, two-year lag	0.211***	0.235**	0.052	0.116
	(0.069)	(0.117)	(0.269)	(0.524)
$\hat{\gamma}^k_{-3}$ Rosenwald classrooms, three-year lag	0.052	0.258	0.525*	0.570
	(0.077)	(0.158)	(0.297)	(0.522)
$\hat{\gamma}^k_{-4}$ Rosenwald classrooms, four-year lag	−0.027	0.069	0.625**	0.290
	(0.052)	(0.104)	(0.254)	(0.791)
$\hat{\gamma}^k_{-5}$ Rosenwald classrooms, five-year lag	−0.007	−0.077	0.370	−0.315
	(0.048)	(0.097)	(0.299)	(0.683)
n (county-years, 1916–1940)	6175	6175	5952	5952
Adjusted R^2	0.08	0.15	0.16	0.16

Notes: The estimating equation is

$$\Delta T^k_{ct} = \gamma^k \Delta RT_{ct} + \sum_{s=1}^{5} \gamma^k_{-s} \Delta RT_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}$$

where T^k_{ct} represents either the total number of teachers or average teacher salaries, by race. RT_{ct} is the number of Rosenwald-funded classrooms built in year t. θ^k_c is a county fixed effect and θ^k_t is a year fixed effect. Unlisted control variables include changes in enrollment (by race and lagged one year), changes in revenues from local taxes (lagged one year), and changes in Census variables (interpolated between decennial years: total population, black population share, crop value per capita, and percent of land devoted to agriculture). Regressions are weighted by total number of Rosenwald-funded classrooms built between 1921 and 1933 and estimated with heteroscedasticity-robust standard errors clustered within counties. Standard errors are in parentheses below each coefficient.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

6.1. Interpreting coefficients

For each school funded by the grant, Rosenwald archives indicate the number of classrooms designed to operate therein. After 1920, building blueprints were rigorously standardized across locales, and we expect each school's planned number of classrooms to correspond strongly with the number of teachers who worked there. As such, we assume that each classroom represented one teacher and test for in-kind crowd-out in teachers. Let ΔT^k_{ct} represent the change in the number of African-American or white teachers in county c, year t. Let the (implied) number of new teachers working in Rosenwald schools be ΔRT_{ct}. We estimate the following:

$$\Delta T^k_{ct} = \gamma^k \Delta RT_{ct} + \sum_{s=1}^{5} \gamma^k_{-s} \Delta RT_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}. \quad (3)$$

Rosenwald-funded classroom measures are inclusive of local matching resources so in the absence of crowding out, the expected impact of these classrooms on the total number of teachers is unity. Thus, $\hat{\gamma}^a$<1 is taken as evidence of crowd-out in kind.[33] If $\hat{\gamma}^w > 0$, this is evidence of in-kind diversion to white schools through augmented teacher counts.

We also use Eq. (3) to test for changes in average teacher salaries, which should also have varied in proportion to the number of Rosenwald classrooms and teachers.[34] Teachers were often paid a daily rate, so to the extent that the Fund's conditions on term lengths were binding, average annual black teacher salaries should have risen as well. We weight all estimates of Eq. (3) by the total number of Rosenwald classrooms built in a county before 1933 so that coefficients measure the average impact of a Rosenwald-funded classroom on these outcome measures.[35]

Of course, the Rosenwald initiative was primarily a school construction program. To determine if Rosenwald schools had a net effect on the capital stock of black schools, we estimate the effect of the number of new Rosenwald-built schools in year t (ΔRS_{ct}) on the change in the total number of schools between years $t-1$ and t (ΔS_{ct}).

$$\Delta S^k_{ct} = \delta^k \Delta RS_{ct} + \sum_{s=1}^{5} \delta^k_{-s} \Delta RS_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}. \quad (4)$$

Analogous to Eq. (3), $\hat{\delta}$<1 in the African-American school regressions may indicate crowd-out in school buildings, while $\hat{\delta} > 0$ in the white school regressions may indicate diversion. However, while black schools were slowly increasing in number at this time in U.S. history, white community schools were rapidly consolidating into a smaller number of larger schools, as indicated in Fig. 1. This consolidation was costly to school districts in the short-term, and a diversion of Rosenwald resources may have actually led to *fewer* white school buildings. Accordingly, we interpret Eq. (4) results carefully with respect to white school buildings.[36] We weight these regressions by the total number of Rosenwald schools constructed in a county by 1933 so that coefficients represent the average impact of a Rosenwald school on the total number of schools.[37]

We also use Eq. (4) to measure the impact of Rosenwald funding on term length, reported as the average number of days schools were in session county-wide and therefore potentially proportional

[33] Another possibility is that planned classrooms could not be filled with an equivalent number of teachers, but this outcome is akin to crowd-out. Population and enrollment statistics indicate that black schools and teachers were burdened with far more students than white schools and teachers. There were 117.2 (63.0) black (white) individuals under 17 per teacher throughout 1910–1940, and there were 50.7 (34.6) black (white) enrolled students per teacher. If a planned classroom could not be filled with a teacher, excess capacity was unlikely to be the cause.

[34] Average annual teacher salaries are measured county-wide and any Rosenwald-induced increase in this average should be proportional to the number of Rosenwald classrooms funded by the grants.

[35] Unreported, unweighted coefficients are similar.

[36] Blackschools experienced some consolidation over this time period as well, but as illustrated in Fig. 1, there was a net growth in black schools per 1000 individuals under 17.

[37] Again, unweighted results are not shown but contain no remarkable difference from weighted results.

Table 7
Eq. (4) results: Impact of Rosenwald schools on total schools and term length.

	(1)	(2)	(3)	(4)
School resource	Number of schools		Average term length	
School population	African-American	White	African-American	White
$\hat{\delta}^k$ Current Rosenwald schools	0.095	−0.091	−0.019	0.362***
	(0.167)	(0.143)	(0.297)	(0.098)
$\hat{\delta}^k_{-1}$ Rosenwald schools, one year lag	−0.016	0.002	0.184	0.244*
	(0.156)	(0.141)	(0.239)	(0.128)
$\hat{\delta}^k_{-2}$ Rosenwald schools, two-year lag	0.014	−0.031	−0.186	0.093
	(0.141)	(0.110)	(0.187)	(0.095)
$\hat{\delta}^k_{-3}$ Rosenwald schools, three-year lag	−0.042	−0.029	−0.975	−0.044
	(0.268)	(0.105)	(0.902)	(0.093)
$\hat{\delta}^k_{-4}$ Rosenwald schools, four-year lag	−0.025	0.021	0.268	−0.042
	(0.201)	(0.083)	(0.257)	(0.094)
$\hat{\delta}^k_{-5}$ Rosenwald schools, five-year lag	0.068	−0.141	0.052	−0.032
	(0.184)	(0.140)	(0.228)	(0.131)
n (county-years, 1916–1940)	5204	5204	6038	6038
Adjusted R^2	0.06	0.08	0.07	0.11

Notes: The estimating equation is

$$\Delta S^k_{ct} = \delta^k \Delta RS_{ct} + \sum_{s=1}^{5} \delta^k_{-s} \Delta RS_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}$$

where S^k_{ct} represents either the total number of schools or average county term length, by race. RS_{ct} is the number of new Rosenwald schools constructed in year t and θ^k_c is a county fixed effect and θ^k_t is a year fixed effect. Unlisted control variables include changes in enrollment (by race and lagged one year), changes in revenues from local taxes (lagged one year), and changes in Census variables (interpolated between decennial years: total population, black population share, crop value per capita, and percent of land devoted to agriculture). Regressions are weighted by total, county-wide Rosenwald schools built between 1921 and 1933 and estimated with heteroscedasticity-robust standard errors clustered within counties. Standard errors are in parentheses below each coefficient.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

to the number of Rosenwald schools. As five-month terms were a condition of each grant, results will indicate whether districts were affected by an unfunded, long-term mandate.[38] The impact of Rosenwald schools on white term lengths is another measure of diversion, since diverted Rosenwald funds could have been applied toward operating expenses that were necessary to keep white schools open longer.

6.2. Other school quality results

Table 6 reports coefficient estimates for Eq. (3), where the number of new Rosenwald-funded classrooms is the intervention measure.

There is evidence of both crowd-out in the provision of black teachers (Column 1) and in-kind diversion to white schools (Column 2) when number of teachers is the dependent variable. An additional Rosenwald classroom resulted in just 0.215 additional black teachers contemporaneously, and 0.346 and 0.211 additional teachers in the two years following construction. Much like the impact of contributions on African-American spending, the contemporaneous effect of Rosenwald classrooms on the number of employed African-American teachers is fairly large despite a high rate of crowding out. Between 1921 and 1933, increasing the number of new Rosenwald classrooms by one standard deviation (2.7) increased the growth in African-American faculties by 8% of a standard deviation (7.0) in the year of construction, by 13% one year later, and 8% two years later before fading out in years 3 and higher. White teacher counts increased contemporaneously by 0.394 for every Rosenwald classroom and by a significant amount two years later. However, as shown in Table 5, Column 2, measured impacts on white teachers may have been continuations of pre-existing, nonlinear trends in Rosenwald-funded locations.

Table 6 also reports the impact of new Rosenwald-funded classrooms on average teacher salaries, which exhibit sporadically positive gains for both races in the wake of Rosenwald interventions. Again, however, we cannot attribute these gains entirely to Rosenwald involvement. Teacher salaries were increasing non-linearly *before* Rosenwald schools were built (Table 5, Column 3). Nevertheless, teacher salaries are the only black schooling input where we observe *persistent* gains coincident with Rosenwald resources.[39] The campaign is associated with higher countywide average black salaries, by $0.624 (5% of the average $12.55 growth in black teacher salaries each year) a full four years after construction. Still, given the underlying trajectory of black salaries, this impact is at best an upper bound.

Table 7 reports the estimated impact of Rosenwald schools on the total number of schools in each county, as well as the average term length across schools. The results indicate that each Rosenwald school resulted in *no* significant change in black schools in the year of the grant, and no significant change in black school counts in subsequent years. Considered alone, this is not necessarily indicative of crowd-out in school buildings. If small, older schoolhouses were replaced with larger and more modern Rosenwald schools (the historic record provides several anecdotes like this), the actual capacity and quality of black schools may have increased by more than the number of buildings. We show no significant impact of Rosenwald schools on the stock of white buildings or on contemporaneous black term length. White term lengths exhibited a small, positive response to new Rosenwald schools, increasing countywide by an average 0.362 days per Rosenwald school.

Overall, empirical results imply that the increase in resources provided or stipulated by the Rosenwald Fund did not come to fruition as persistent gains in black expenditures, teachers, teacher salaries, schools, or term lengths. In light of this, we estimate the impact of the Rosenwald initiative on the *ratio* of black to white resources – the

[38] Term lengths were dictated for each Rosenwald school, but would not necessarily have impacted other schools currently operating. The effectiveness of these mandates will be diluted if there were no spillovers in the form of longer school years throughout counties with a Rosenwald presence.

[39] Modern lines of research point to effective teachers as one of the most important components of education production. See, among others, Rockoff (2004) and Jennings and DiPrete (2010).

Table 8
Impact of Rosenwald contributions, teachers, and schools on black–white ratios.

	(1)	(2)	(3)
Rosenwald intervention measure	Private contributions (000 s)	Teachers	Schools
Ratio	Black/white per-pupil spending (×100)	Black/white pupils per teacher (×100)	Black/white term Length (×100)
(Mean)	(22.2)	(158.5)	(82.0)
$\hat{\eta}^k$ Current Rosenwald interventions	0.237***	−0.247	−0.213
	(0.054)	(0.322)	(0.208)
$\hat{\eta}^k_{-1}$ Rosenwald interventions, one year lag	0.006	−0.536*	−0.029
	(0.031)	(0.312)	(0.129)
$\hat{\eta}^k_{-2}$ Rosenwald interventions, two-year lag	−0.051*	−0.482	−0.202
	(0.031)	(0.302)	(0.124)
$\hat{\eta}^k_{-3}$ Rosenwald interventions, three-year lag	0.004	−0.220	−0.651
	(0.042)	(0.158)	(0.595)
$\hat{\eta}^k_{-4}$ Rosenwald interventions four-year lag	0.008	−0.036	0.190
	(0.030)	(0.104)	(0.154)
$\hat{\eta}^k_{-5}$ Rosenwald interventions, five-year lag	0.037	0.028	0.042
	(0.031)	(0.093)	(0.122)
n (county-years, 1916–1940)	3444	6175	6038
Adjusted R^2	0.03	0.03	0.02

Notes: The table lists estimates of the effect of Rosenwald contributions, teachers, and schools (respectively) on changes in black/white per-pupil spending ratios, black/white per-pupil teacher ratios, and black/white term lengths. Unlisted control variables include county fixed effects, year fixed effects, changes in enrollment (by race and lagged one year), changes in revenues from local taxes (lagged one year), and changes in Census variables (interpolated between decennial years: total population, black population share, crop value per capita, and percent of land devoted to agriculture). Regressions are weighted by total, county-wide Rosenwald contributions from 1921 to 1933 (Column 1); total, county-wide Rosenwald-funded classrooms built 1921–1933 (Column 2); total, county-wide Rosenwald-funded schools built 1921–1933 (Column 3) and estimated with heteroscedasticity-robust standard errors clustered within counties. Standard errors are in parentheses below each coefficient.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

education "gap" itself – and report results in Table 8. The first column illustrates the impact of Rosenwald monies on the ratio of black to white expenditures per pupil, where we find a small, positive, and significant effect, but only in the first year of the grant. We also measure the impact of Rosenwald interventions on the ratio of black to white pupils per teacher and on the ratio of term lengths, and find no persistent change in either resource gap. Beyond the first year of Rosenwald interventions, crowd-out of local funding and the diversion of resources towards white schools appears to have prohibited the Rosenwald initiative from closing the gap in measurable school inputs. We emphasize, however, that unobserved aspects of school quality may well have been substantially impacted by the Rosenwald intervention.

7. Assessing selection bias

The largest threat to the internal validity of our findings is the possibility that Rosenwald schools were systematically located in a manner correlated with our outcomes of interest. Rosenwald's broad aim was to transform some of the worst African-American school systems, and if the Fund targeted counties with especially weak and eroding black schools, estimates of the program's effect on black schooling resources may be understated. Conversely, more organized citizens in areas with ascending black schools may have been more successful in obtaining Rosenwald assistance, therein introducing an upward bias to our results.

In this section we present the results of two additional specifications that explicitly take into account the site selection process. We assess the nature of Rosenwald site selection using observable county characteristics from *before* the school building campaign and then test whether controlling for the ex ante expected dosage of Rosenwald activity affects our results.[40]

In the first specification, we interact each pre-Rosenwald variable listed in Tables 2 and 3 with a linear trend, and we incorporate these interactions into Eq. (2).[41] Specifically, we estimate the following:

$$\Delta Y^k_{ct} = \alpha^k \Delta R_{ct} + \sum_{s=1}^{5} \alpha^k_{-s} \Delta R_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \psi^k_1 f(t) + \psi^k_2 (f(t) * \mathbf{Z}_c) + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}, \quad (5)$$

where Y^k_{ct} represents total spending for race $k = a,w$, the vector $\mathbf{Z}_c$ contains pre-Rosenwald county features listed in Tables 2 and 3, $f(t)$ is a linear trend, R_{ct} represents private contributions, θ^k_c is a county fixed effect, and θ^k_t is a year fixed effect. Eq. (5) is structured just like the model from our main analysis, Eq. (2), with the addition of $f(t)$ and pre-Rosenwald interactions. These additions allow the estimated impact of Rosenwald gifts to be conditioned on pre-existing factors that may have driven distinct *trends* in expenditures.

In a related test, we estimate the predicted number of Rosenwald schools using Eq. (1) with state fixed effects (Table 3, Column 2).[42] We then interact this predicted value ($\hat{r}_{cs}$) with a linear trend and include the interaction and trend in the following model:

$$\Delta Y^k_{ct} = \alpha^k \Delta R_{ct} + \sum_{s=1}^{5} \alpha^k_{-s} \Delta R_{c,t-s} + \Delta \mathbf{X}_{ct} \beta^k + \psi^k_1 f(t) + \psi^k_2 (f(t) * \hat{r}_{cs}) + \theta^k_c + \theta^k_t + \varepsilon^k_{ct}. \quad (6)$$

In doing so, we allow school resource trends to vary with the ex ante expected dosage of Rosenwald activity.

Table 9 lists our baseline estimates of the immediate impact of Rosenwald interventions on black and white schooling inputs, respectively, in Columns 1 and 4 which are taken from Tables 4, 6, and 7. We compare these to α^k coefficient estimates from Eqs. (5) and (6). Despite the significance of several pre-Rosenwald county characteristics in

[40] We have also evaluated the sensitivity of our results to adding county-specific time trends to the difference model (to control for differing quadratic growth trajectories) and to testing for magnitude bias by comparing impacts in above- and below-median school enrollment counties. We note no remarkable change to the estimates, and these results are available upon request.

[41] See Hoynes and Schanzenbach (2009) and Acemoglu et al. (2004) for recent applications of this method to other non-experimental settings.

[42] Of all of the pre-program models presented in Table 3, Eq. (1) for the number of cumulative Rosenwald schools built, with state fixed effects, has the strongest predictive power.

Table 9
Eqs. (2)–(4) results: Comparing baseline results to specifications with additional controls for endogenous site selection.

	(1)	(2)	(3)	(4)	(5)	(6)
School population	African-American	African-American	African-American	White	White	White
Specification	Main	Eq. (5), with pre-Rosenwald observables interacted with linear trends	Eq. (6), with predicted Rosenwald schools interacted with linear trend	Main	Eq. (5), with pre-Rosenwald observables interacted with linear trends	Eq. (6), with predicted Rosenwald schools interacted with linear trend
Effect of current Rosenwald contributions on						
Total Public Spending	0.877***	0.885***	0.876***	1.335**	1.382***	1.347***
	(0.159)	(0.160)	(0.159)	(0.518)	(0.515)	(0.519)
Effect of current Rosenwald teachers on						
Total Public Teachers	0.215***	0.216***	0.215***	0.394***	0.391***	0.383***
	(0.048)	(0.049)	(0.049)	(0.095)	(0.095)	(0.095)
Average Teacher Salaries	0.378	0.358	0.333	0.001	-0.033	-0.044
	(0.263)	(0.263)	(0.263)	(0.557)	(0.558)	(0.557)
Effect of current Rosenwald schools on						
Total Public Schools	0.095	0.094	0.091	-0.091	-0.080	-0.075
	(0.167)	(0.169)	(0.169)	(0.143)	(0.142)	(0.142)
Average Term Length	-0.019	-0.018	-0.025	0.362***	0.364***	0.362***
	(0.297)	(0.296)	(0.298)	(0.098)	(0.098)	(0.098)

Notes: The table compares results of Eqs. (2)–(4) (coefficients from these models are also reported in Tables 4, 6, and 7) with those of Eqs. (5)–(6), which supplements preferred specifications with linear trends that are allowed to vary by pre-program county characteristics (Columns 2 and 5) or the predicted number of Rosenwald schools built over 1921–1933 (Columns 3 and 6, estimated by Eq. (1)). Heteroscedasticity-robust standard errors are in parentheses below each coefficient.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

predicting the number of schools Rosenwald that were ultimately built, there is surprisingly little difference between our baseline results and those that additionally control for trends dependent on pre-program county features.

Even after controlling for selection, then, black school expenditures are estimated to have increased as a result of Rosenwald Fund activity, but at less than the two-to-one rate indicated by the terms of the typical Rosenwald-district partnership. At the same time, funding was diverted towards white schools at an even greater rate. We also find evidence that Rosenwald contributions had a short-lived but significantly positive impact on black teacher counts and white term lengths, even after the inclusion of pre-treatment trends.

8. Rosenwald's impact on the black–white achievement gap

Table 8 highlights the inability of private funds related to Rosenwald school construction to close the Southern black–white gap in observable aspects of school quality: spending per pupil, student–teacher ratios, and term–length ratios. Nonetheless, Aaronson and Mazumder (2011) show that black literacy, school attendance, cognitive test scores, and northern migration were all positively impacted by the intensity of Rosenwald activity within a county with no measurable impact on the same outcomes for white individuals. Thus, Rosenwald narrowed the black–white attainment and achievement gaps in the South without having affected the gap in measurable educational inputs.

There are a number of ways to reconcile these disparate findings. First, Rosenwald schools may have failed to affect black–white gaps in superficial school quality proxies but nevertheless effected meaningful gains in a deeper sense of school quality, one that is difficult to observe from published education reports. This was certainly true for the quality of black school buildings. Research on modern schools has indicated that school facility upgrades improve student achievement (Jones and Zimmer, 2001). Rosenwald's state-of-the-art, standardized school plans may not have increased the absolute number of black school buildings in service, but by supplanting dilapidated structures, Rosenwald schools substantially improved the quality of the black schooling infrastructure, which in turn may have improved the quality of black human capital.

In addition, positive spillovers in the form of parental and community engagement offer another explanation for large individual gains in black human capital but little change in school input gaps in the near term. Indeed, by involving the entire community in funding and building Rosenwald schools, the program foreshadowed successful multi-pronged interventions like Mexico's Progresa-Oportunidades, the Perry Pre-School Project, and the Harlem Children's Zone (see, respectively, Behrman et al. (2005), Heckman et al. (2010), and Dobbie and Fryer (2011)).

Finally, if there were sufficiently steep diminishing marginal returns to investments in school quality, or if there were large differential returns to school quality across races, Rosenwald's unintended effect on white schooling resources may have had no long-term effect on white human capital while the smaller impact on black schooling resources may have generated substantial human capital returns. That is, a dollar given to a black school may have had a much larger impact on individual outcomes than a dollar diverted or matched in white schools. Along these lines, recall from Section 5.3 the finding that private contributions had a larger practical effect (in relative terms) on African-American spending than they did on white spending, even though white schools received a higher nominal share of each private dollar.

We test this latter proposition directly by estimating the race-specific relationship between county-level public educational spending, culled from our panel data, and school attendance, taken from the public-use one percent samples of the 1910–1930 U.S. Census returns. (Ruggles et al., 2010) Following Aaronson and Mazumder (2011), we focus our analysis of school attendance on individuals aged 7 to 17 over these years. We match each respondent's county to published data on current and lagged school expenditures from our panel of transcribed school data.[43]

We estimate the following linear probability model:

$$Y_{ict}^{k} = \alpha^{k} + S_{ct}^{k}\psi^{k} + \mathbf{X}_{i}^{k}\beta^{k} + \theta_{c}^{k} + \theta_{t}^{k} + \varepsilon_{ict}^{k} \quad (7)$$

[43] Accordingly, this analysis is limited to the four states – Alabama, Georgia, Louisiana, and South Carolina – which reported expenditures separately by race.

Table 10
Eq. (7) results: school spending, school attendance, and literacy.

Outcome population	Attending or enrolled in school, 1910–1930	
	African-American	White
Per-pupil real expenditures	0.001*	−5.20E−6
(current)	(0.001)	(−2.4E−4)
n (census respondents)	23,135	29,102
Adjusted R-squared	0.09	0.11
Outcome population	Can read and write, 1930	
	African-American	White
Per-pupil real expenditures	0.001*	1.3E−4*
(average over age 7–13)	(5.5E−4)	(7.5E−5)
n (census respondents)	6266	9347
Adjusted R-squared	0.10	0.06

Notes: The estimating equation is

$$Y_{ict}^{k} = \alpha^{k} + S_{ct}^{k}\psi^{k} + \mathbf{X}_{i}^{k}\beta^{k} + \theta_{c}^{k} + \theta_{t}^{k} + \varepsilon_{ict}^{k}$$

where Y_{ict}^{k} is school attendance for age 7–17 respondents in the 1910–1930 U.S. Census, by race (top panel) or reading and writing literacy for age 15–23 respondents in 1930 (bottom panel). S_{ct} is per-pupil spending in a respondent's county of residence. X_i is a vector of individual characteristics, θ_c^k is a county fixed effect and θ_t^k is a year fixed effect. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

where Y_{ict}^{k} is school attendance of respondent i, race k, residing in county c, observed in Census year t. We estimate Eq. (7) separately for white and black individuals. S_{ct} is per-pupil spending in a respondent's county of residence. $\mathbf{X}_i$ is a vector of individual characteristics.[44] County and year fixed effects are given by θ_c^k and θ_t^k. Identifying variation comes from changes in educational spending within counties over time.

Table 10 lists coefficient estimates from Eq. (7). In the top panel, we find that for black individuals aged 7–17 years of age, a one standard deviation ($9.61) increase in per-pupil spending in black schools between 1910 and 1930 is associated with a 1.0 percentage point increase in school attendance. The corresponding figure for white youths is insignificantly different from zero.

In the bottom panel of Table 10, we estimate the impact of school spending when an individual was 7–13 years old on literacy as reported in the 1930 Census for individuals aged 15–23 in that year. Again, we find evidence of diminishing marginal returns as a $10 increase in per capita school expenditures raises black literacy by a statistically significant 1%, ten times the effect of spending on white literacy rates.[45]

Relatively high returns to black school spending validates the idea that black individuals could have disproportionately benefitted from Rosenwald interventions, despite diversion and matching in white schools. This may have allowed Rosenwald to achieve his end goal, despite the fact that private donations failed to accrue solely to black students.

9. Conclusion

The Julius Rosenwald Fund invested $4 million (in 1925 dollars) and raised another $22.4 million from districts and citizens over the years 1921–1933 in an effort to improve the quality of education for Southern blacks.[46] We examine the impact of the Rosenwald Fund's initiatives on the quality of local schooling provision between 1910 and 1940 using newly assembled data on white and black schooling inputs. We measure the impact of Rosenwald funds, schools, and teachers on overall funding, schools, teachers, teacher salaries, and term lengths for both black and white schools in five Southern states. We find evidence that the level of contemporaneous black schooling resources significantly increased with Rosenwald attention, with 8–9% impacts in terms of standard deviations of growth in African-American spending and faculties. But we reject the hypothesis that private Rosenwald contributions did not crowd-out public expenditures on African-American schools, and we detect *no* long-term gains in spending in response to Rosenwald contributions. We also identify significant crowd out in kind. Each new classroom in a Rosenwald school was associated with just 0.215 additional teachers, an effect which faded to zero three years after schools were built.

Further, measures of white schooling resources indicate that monies earmarked for Rosenwald schools were diverted from their intended beneficiaries to white schools and even supplemented. Each dollar of private donations for the construction of a Rosenwald school was associated with $2.12 in additional public spending across white and black schools in accordance with the Fund's matching stipulation. But white schools received $1.34, or 63%, of total spending gains. This is consistent with the idea that whites valued their standing relative to blacks, which has support both anecdotally and empirically. Thus, while the Rosenwald initiative increased the *absolute* level of some black schooling inputs in the South in the short-run, we find no evidence that it narrowed the stark resource gap between black and white schools in either the short or long run.

Two important caveats bear emphasis alongside these findings. First, whether crowd-out and diversion are welfare-reducing will depend entirely on the underlying social welfare function. For example, in light of exogenous but conditional influxes of private monies, benevolent governments may increase welfare overall by letting some portion of private funds displace what the government would have spent toward meeting those conditions anyway. Second, although the Rosenwald campaign failed to equalize measurable education inputs in the U.S. South, black Rosenwald beneficiaries nevertheless realized human capital gains that outpaced their white counterparts. We reconcile this verdict on the Rosenwald Fund's success with ours by linking public school spending measures to individual education outcomes. We show that school expenditures had a relatively larger impact on black children and young adults, meaning that "separate but equal" investments in segregated schools – or even unequal investments favoring white schools – could have nonetheless narrowed the gap in individual human capital.

Despite the Rosenwald Fund's large footprint in the rural Southeast, its counterfactual is somewhat unclear. Our conclusions indicate that in the absence of the program, the quality of physical facilities for black students would certainly have remained substandard for some time and the combined level of spending on black and white schools throughout the Rosenwald period would have been lower. The total value of private contributions to Rosenwald schools between 1920 and 1933 in five Southern states represents a remarkable 5.8% of all spending on black schools over the same window of time. And even though a substantial proportion of new funds "stuck" in the ledger of black spending, over and above underlying growth, an even greater

[44] X_i includes gender, age, indicators for the absence of parents in the household (which is more relevant for older respondents), mother's literacy (if present in the household, parental literacy includes both reading and writing literacy), father's literacy, father's occupational score, and a dummy variable for rural locations. In the IPUMS data, "occupational score" is the median income of all persons with a given occupation. We use this as a proxy of labor force participation and expected household earnings.

[45] In the literacy equation, S_{ct} is defined as average per-pupil spending in a respondent's county of residence over the years when he or she was aged 7 to 13. When estimating this specification, we suppress county fixed effects so that identifying variation in prior school expenditures is across counties and birth cohorts.

[46] In 2012 dollars, the Fund's direct investments amounted to $51.9 million, and total contributions from school districts and citizens were worth $289.4 million. Our five-state sample accounted for 45 and 43% of these funds, respectively.

amount was added to resources for white schools. We therefore conclude that the gap in schooling inputs would have been no wider in the absence of Rosenwald schools, but because of diminishing marginal returns to education and the probable effect of Rosenwald schools on unobserved dimensions of education, the gap in individual outcomes narrowed as a result of this program. The historical episode of Rosenwald schools suggests that funding interventions can successfully narrow outcome disparities even in situations where crowd-out of public provision or diversion to untargeted groups seem likely, particularly when targeted groups exhibit higher marginal returns to funds.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2013.02.003.

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