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Returns to school resources in the Jim Crow South

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ABSTRACT

We estimate returns to school resources in the Jim Crow era, as measured by young males' 1940 wage earnings, occupational status, and cognitive aptitude scores. Results point to a 16 cent annual return on each $1 invested in public schools. To the question of whether some school inputs mattered more than others, we find comparable 25–32 cent returns per dollar invested in extended school years, teacher salaries, and smaller classes. School spending and inputs had much more bearing on labor market outcomes than aptitude scores. We document diminishing returns to school expenditures, which, in combination with segregated schools, resulted in higher returns to expenditures in black schools relative to white.

1. Introduction

Two hallmarks of U.S. public schools in the early part of the 20th century are racial segregation and the decentralization of taxation and budgeting. A consequence of these features was substantial variation in resources available to students in different locations and across segregated schools in the same district. Districts differed widely on overall school funding as well as the allocation of funds across school inputs, choosing varying investments in teacher pay, length of the school year, class size, and so on. In this note, we rely on variation in the level and allocation of school resources to examine labor market returns to public school expenditures in the Jim Crow South, calculating the relative return to particular school inputs and the comparative returns by race. In doing so, we present estimates of returns to local school resources for the broadest and earliest sample of young men examined in the literature to date.

We draw on county-level school statistics for four segregated Southern states – Alabama, Georgia, Louisiana, and South Carolina – covering school years 1921–1922 through 1939–1940. We estimate flexible Mincer equations that allow for non-linear returns to school inputs alongside interactive effects of inputs and educational attainment. Outcomes of interest include labor market wages reported in the 1940 U.S. Census, occupational status in the 1940 Census, and 1943 aptitude scores from the Army General Classification Test (AGCT). Results point to a 16 cent gain in 1940 earnings for each additional dollar of per-pupil annual expenditures, corresponding to a payback period of 6–7 working years under the assumption that the human capital premium persists. Longer school terms, smaller class sizes, and higher teacher salaries each yielded comparable income returns of 25–32 cents per dollar equivalent.

Cross-sectional identifying variation in school inputs leaves results vulnerable to biases from omitted variables that determine individual labor market success as well as public school inputs. Nevertheless, findings for labor market outcomes are very similar when we restrict the analysis to men who were educated somewhere other than their 1940 county of residence.

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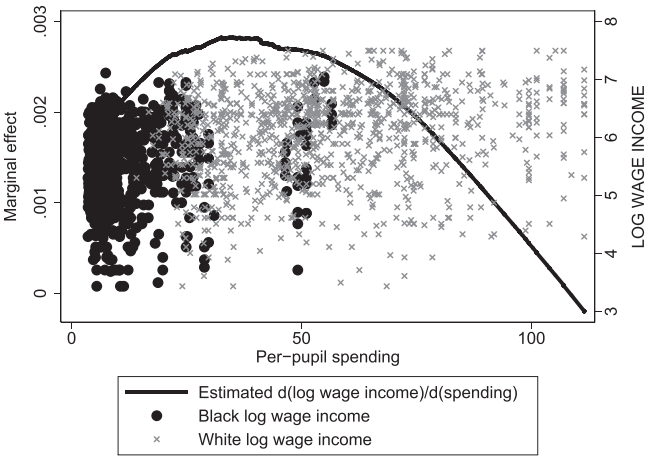

Fig. 1. Marginal effects of per-pupil expenditure, by race. *Notes*: The figure depicts marginal effects of per-pupil expenditure on 1940 log income. Estimates are derived from kernel-based regularized least squares and a random one percent subsample of young men in the 1940 Census.

As a preview of findings, Fig. 1 documents the relationship between a summary measure of school resources – per-pupil spending – and the log of wage income in the 1940 Census. Circles represent log earnings for black men and *x* markers represent the same for white men. The smoothed, solid line traces marginal effects of per pupil spending on wage income. In the figure, pointwise partial derivatives are estimated by kernel-based regularized least squares (KRLS).[1] Foremost, the KRLS analysis shows that school expenditures played a meaningful role in labor market wages. The average of pointwise partial derivatives was 0.0024 with a standard error of 0.0005 (corresponding to a 10% one-year rate of return). Second, we note a range of increasing returns to school investments, followed by a noticeable taper at higher (predominantly, but not exclusively, white) levels of expenditure. This motivates a non-linear characterization of school inputs, which we describe in Section 4. Although it is beyond the scope of this paper to fully explore the source of these nonlinearities, increasing marginal returns at low levels of education inputs are not inconsistent with informed local decision-making. Rather, they are consistent with extraordinarily low support for black public schools in the South at this time. Black constituents were effectively barred from voting, and thus unable to hold policymakers accountable for foregone labor market returns to education.

2. Literature

We are not the first to measure the labor market effects of school expenditures in this era. Previous work, however, has been limited to state-level aggregate education metrics, blind to wide variation in school quality within states.[2,3] Morgan and Siragelditn (1968) detect a relationship between state-level school expenditures from 1930 to 1950 and hourly earnings observed in the 1960s, and Akin and Garfinkel (1977) find similarly positive results for income reported in the early waves of the Panel Study of Income Dynamics. The effect of school resources may operate primarily through their effect on attainment (Morgenstern, 1973) or, more specifically, their effect on *returns to* attainment (Johnson and Stafford, 1973; Carruthers and Wanamaker, 2017). At the same time, a number of studies, notably Ribich and Murphy (1975), fail to find a relationship between school resources and labor market earnings in this period.

This note adds to the existing literature in three ways. First, we estimate returns to school quality at the county, rather than state, level. Second, in addition to the empirical import of local school resources overall, the literature has yet to confidently identify *which* historic school resources mattered for labor market success. We assess the return to dollar equivalents of three specific education inputs: teacher salaries, term lengths, and teachers per pupil.[4] Finally, we document how segregated schools, and the associated racial differences in resource levels, produced different returns to some school inputs by race.

[1] KRLS is a machine learning technique that relaxes functional form assumptions, draws inferences from more observably similar individuals, and permits much more flexible specifications while penalizing excessive model complexity. See Hainmueller and Hazlett (2014) for statistical properties and Ferwerda et al. (2015) for implementation details. KRLS demands extraordinary computing power, and accordingly, we do not rely on this method for our main results to follow. Fig. 1 depicts KRLS marginal effects for a random one-percent sample of young men in the 1940 Census. In addition to per-pupil spending, controls include categorical attainment, age, race, and county covariates listed in Section 3.

[2] Wachtel (1975) provides one exception. Using disaggregated high school expenditure data collected in the 1940s–1960s for a sample of males likely limited to white students, he finds a positive effect on labor market earnings as well as on academic achievement.

[3] If there are nonlinear labor market returns to school quality for Southern workers, the direction of bias from state-level aggregations is uncertain. Reviews by Hanushek et al. (1996) and Betts (2010) suggest that estimated returns to school resources *rise* with the level of aggregation, which is more consistent with omitted variable bias than classical measurement error. Carruthers and Wanamaker (2017) find that state-level aggregated data are inferior to county-level data in explaining overall racial wage inequality in the 1940 Census.

[4] To our knowledge, Raymond's (1968) study of West Virginia is the only statistical "horse race" pitting various school resources against each other to determine the historic importance of each. He finds that teacher salaries matter for standardized test scores and college freshman grade point averages, whereas class sizes, library book availability, and non-instructional spending were insignificantly related to these outcomes.

3. Data

We measure the impact of school resources on three distinct outcomes: 1940 annual wage income, 1940 occupational score, and 1943 AGCT scores. Annual income and occupation data come from the 100% count of the 1940 Census enumeration (Ruggles et al., 2010), the first census to report wage earnings. We restrict the Census sample to black and white young men ages 18–25 in 1940 whose 1935 county of residence was in one of the four states for which we have the most complete data on school inputs (described below). Enumerators were instructed to record where respondents lived in 1935, and we assume that these locations were where respondents were educated.[5] Those whose earnings were primarily from self-employment are omitted from the analysis to ensure that we have an accurate count of all earnings.[6]

Census enumerators also recorded occupations, and we calculate an occupational score for each individual in the sample, including salaried workers and the self-employed where possible. Occupational scores are defined here as the average wage of all white Southerners in that occupation in the 1% public use sample of the 1950 Census (Ruggles et al., 2010). We restrict the 1950 average to whites since a race-specific score would also encompass wage discrimination. Occupational score results will therefore highlight the role of school resources in facilitating movement up the occupational ladder.[7]

We gather AGCT scores for a selection of World War II enlistees from the National Archives.[8] AGCT data are available for a few months in 1943, as described in Ferrie et al. (2012). Men with observed AGCT scores are not necessarily representative of their cohort, and the net selection bias is unclear.[9] Another shortcoming of the enlistee data relative to the 1940 Census is that there are no data on migration or prior places of residence, so we must assume that each enlistee's county of residence is where he was educated.[10]

We match each individual in the census and enlistment samples to measures of local school resources in his assumed county of education. Public school data were published by state departments of education annually or biennially throughout the period of interest. Broadly available school quality measures – expenditures per pupil, average term length (days), average teacher salaries (daily rate), and teachers per enrolled pupil (henceforth, “inverse class size”) – are averaged across the years when each individual would have been age 7–17. We focus on Alabama, Georgia, Louisiana, and South Carolina because these states consistently reported expenditure and input data for both black and white school systems over the relevant years.

Summary statistics for income, occupational scores, AGCT scores, and school resources are located in Table 1. School input averages are similar across the two Census samples, whereas it is clear that enlisted men tended to come from better resourced systems, in part because enlisted men were more likely to be white.

4. Methodology

Our starting point is a simple Mincer equation regressing log income, occupation score, or AGCT score on measures of school resources, educational attainment, age, race, and county measures of economic, social, and political activity:

$$\ln(Y_{ia}) = \alpha + \gamma Z_{iac}^{1935} + \beta X_{iac}^{1940} + \epsilon_i, \quad (1)$$

where Y_{ia} is the census or AGCT outcome for person i of age a. The vector Z_{iac}^{1935} represents an individual's race and human capital, including an indicator for black men, categorical indicators for age, a function of highest grade completed, and a function of school resources in i's 1935 county of residence, c^{1935}. The school resource component of Z_{iac}^{1935} is specified in one of the two ways: as average per-pupil spending during ages 7–17 (in constant 1925 dollars), or as three separate inputs summarized in Table 1 that collectively describe 75% of variation in per-pupil spending.

The vector X_{iac}^{1940} describes individual i's 1940 residence, including an indicator for residing in an urban area and state fixed effects. To account for geographic differences in productivity, X_{iac}^{1940} also includes county-level measures of manufacturing value per capita, retail sales per capita, and crop value per capita. Additional socio-political controls include each county's total

[5] It is necessary that we restrict our focus to respondents ages 18–25 to more confidently link these men to their likely county of schooling, although we acknowledge that doing so precludes any examination of the evolution of returns to school inputs over the lifecycle. Carneiro et al. (2005) find that black–white wage gaps widen with age, and for our purposes this may mean that comparatively high returns to black school inputs tended to wane over time.

[6] Individuals with missing earnings for other reasons, including unemployment and labor force non-participation, are also excluded from the analysis. We observe wages for 50% of whites and 54% of blacks among men ages 18–25 in 1940 (or 76% and 75%, respectively, when we exclude men who were self-employed or out of the labor force). In an unreported analysis, we find that school resources are positively associated with selection into the wage and occupation score analytical samples. If extra-marginal wage earners tend to be poorer, on average, then we may understate the impact of school quality on latent earnings by excluding non-wage earners whose school resources were typically sparser and, according to Fig. 1, who realized higher marginal returns.

[7] The 1940s were a decade of substantial wage compression and returns to human capital may be understated by using 1950 as a benchmark (Goldin and Margo, 1992). Still, using 1950 rather than 1940 occupational scores allows us to broaden the analysis to include some self-employed and farm workers.

[8] National Archives and Records Administration (2002). The U.S. Government's official position was that the AGCT measured “general learning ability” (Adjutant General's Office; Personal Research Section, 1945) and was intended to follow a normal distribution. Practically, the test measured aptitude in arithmetic computation, arithmetic reasoning, reading and vocabulary, and spatial relations. As a result, the test measured both innate learning capacity and past educational experiences.

[9] On the left tail of the aptitude distribution, we do not observe men who could not enlist because of literacy requirements. And because test scores were used in assigning enlistees to tasks for which they were suited, higher-aptitude men with predetermined assignments did not take the test.

[10] If intercounty migration of men in this cohort is orthogonal to local school quality, this introduces attenuation bias in estimates of the effect of school quality on aptitude. On the other hand, if higher-ability men tended to move to places that were richer (poorer) in public school resources, estimates will be biased upwards (downwards).

Table 1
Summary statistics.

Variable	(1) Wage sample	(2) Occupation sample	(3) Enlistee sample
1940 wage income	469.5		
	(463.7)		
1940 occupational score		1,816.1	
		(742.6)	
1943 AGCT score			80.9
			(25.9)
Black (%)	38.6	38.1	31.6
Migrants (%)	15.1	15.0	
Per-pupil expenditures (1925$)	39.6	39.5	41.2
	(35.8)	(35.4)	(35.8)
Term length (days)	149.5	149.3	154.0
	(26.0)	(25.9)	(22.6)
Teacher salary, daily rate (1925$)	4.9	4.9	5.2
	(2.9)	(2.9)	(2.9)
Inverse class size (×100)	2.7	2.8	2.8
	(7.5)	(7.4)	(7.4)
N	371,744	411,043	29,103

Notes: Income statistics in Column (1) are for all wage-reporting men ages 18–25 in the 1940 U.S. Census whose 1935 residence was in Alabama, Georgia, Louisiana, or South Carolina. Occupation score results include men with a reported occupation but without wage income. AGCT averages are for individuals in the World War II enlistment records who enlisted March through June 1943 with the same state of residence restrictions.

lynchings from 1900 to 1930, Democratic vote share in the 1940 presidential election, voter turnout in 1940, and indicators for missing controls.[11]

There are a few challenges with specifying such a model. First, returns to human capital may be non-linear, as plainly depicted by Fig. 1. To account for this, we specify Z_{iac}^{1935} to include quadratic functions of continuous school inputs and categorical indicators for discrete attainment. Second, school resources can plausibly affect human capital independently and through attainment, by extending one's time in school and/or by raising the returns to each additional year of school. With this in mind, the Z_{iac}^{1935} vector includes interactions between school resource quadratics and attainment categories. We then evaluate the marginal effect of school spending and other resources at their mean values, using delta-method standard errors to assess the statistical precision of estimates. Note that under this specification, we abstract away from any direct effect of school quality on educational attainment, potentially understating the total effect of school resources.[12]

A third challenge relates to the potential endogeneity of school resource investments. The fundamental threat to interpreting $\hat{\gamma}$ as the return to school inputs is the notion that school resources were endogenously determined alongside other factors also correlated with Y_{ia}. For example, school districts may have been inherently more advantaged in areas with more robust labor markets; intertemporal dependence of labor market vitality would lead us to overstate the effect of lagged school resources on 1940 wages and occupations.

To probe the importance of omitted variables, we proceed by first presenting baseline results with no accommodation for endogeneity of school resource measures. For Census outcomes, we then provide a second set of estimates limited to intercounty migrants.[13] These men were observed in one county by 1940 enumerators but lived somewhere else in 1935, when they would have been ages 13–20. Since they worked in a location separate from their likely county of schooling, they are less susceptible to endogeneity between school resources and local labor market conditions. Inferences based on migrant subsamples continue to show statistically significant, positive returns to school resources.[14]

[11] We omit county fixed effects as they would soak up the local variation in spending we seek to capture and would limit identifying variation to migrants or within-county changes in the moving average of school quality.

[12] In alternative specifications, not shown here, we find attenuated but typically significant and positive returns to school spending and inputs when we restrict school resources to a linear functional form, limit the sample to men who were educated in their state of birth, or omit educational attainment measures from Eq. (1) under the strong assumption that attainment is entirely explained by school resources.

[13] For enlistees, migration is only observable based on state of birth compared to state of enlistment. As a result, assigning counties for the purpose of school resource assumptions is likely to result in higher levels of attenuation bias in the enlistment data. Even so, and consistent with results to follow, unreported analyses of men whose states of birth and enlistment differ suggest that AGCT returns per dollar of school investments are positive and much smaller than labor market returns.

[14] Another way to approach the potential extent of endogeneity bias is a simple test, motivated by Altonji et al. (2005), comparing baseline results with and without observable characteristics of counties. If the addition of county-level controls greatly reduces estimated returns to school resources, unobserved factors may explain our main findings if we assume that such factors matter at least as much as county features we can measure. But in Eq. (1) specifications where we drop all county-level controls (not shown), the returns to school resources are in most cases slightly *smaller* than those found by models with county controls. Omitted variable bias would have to greatly exceed and run counter to the effect of county features in X_{iac}^{1940} in order to explain away the estimated effect of school resources.

Table 2
Marginal effects and returns to school resources.

Variable	(1) Ln(Annual income, 1940)	(2)	(3) Ln(1950-based occupation score)	(4)	(5) Ln(AGCT score, 1943)
	All Men	Migrants	All Men	Migrants	All Men
Panel A					
Expenditures per pupil	0.0038***	0.0029***	0.0023***	0.0012***	0.0008***
	(0.0006)	(0.0003)	(0.0003)	(0.0002)	(0.0001)
Implied return per $1	[0.160]	[0.152]	[0.373]	[0.228]	[0.006]
Panel B					
Term length	0.0010**	0.0012***	0.0006***	0.0090***	2.4E–05
	(0.0005)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Implied return per $1	[0.308]	[0.436]	[0.771]	[1.148]	[0.001]
Average daily teacher salary	0.0239***	0.0234***	0.0182***	0.0046***	0.0085***
	(0.0067)	(0.0037)	(0.0033)	(0.0017)	(0.0015)
Implied return per $1	[0.248]	[0.278]	[0.731]	[0.194]	[0.014]
Inverse class size	5.5356***	2.7314**	2.2850***	0.5303	0.8895*
	(1.7322)	(1.2125)	(0.8061)	(0.6067)	(0.5128)
Implied return per $1	[0.322]	[0.195]	[0.515]	[0.135]	[0.008]
Observations	371,744	56,022	411,043	61,487	29,103

Notes: Marginal effect estimates and rate of return inferences from Eq. (1). Delta method standard errors are reported in parentheses below each marginal effect estimate. Implied returns per dollar are in brackets below standard errors. See Section 4 for methods and Section 5 for discussion.

* Significance at the 10% level.

** Significance at the 5% level.

*** Significance at the 1% level.

5. Results and discussion

5.1. Full sample and migrant results

We first estimate Eq. (1) using expenditures per pupil as a summary measure of school resources. Results are reported in Panel A of Table 2. For each dollar invested over the 11 years students are of school age, 1940 wages increase by 0.38% for the full sample. Significant and sizable returns for intercounty migrants (0.29%), where endogenous resources are less likely, are apparent in Column 2. Returns measured in occupational scores are slightly smaller: 0.23% for the full sample and 0.12% for migrants. The estimated impact of expenditures on AGCT aptitude is even smaller, measuring 0.08%.

It is straightforward to convert the semi-elasticity estimates in Panel A to rates' of return per dollar spent. A one unit increase in average expenditures per pupil corresponds to an increase of $11 per pupil, in real 1925 dollars, over 11 years, resulting in a return of $0.16 per dollar. Under the untestable assumption that this one-year premium persists in step with inflation, public school expenditures would be “paid back” in the form of higher earnings in 6.2 working years.

In Panel B, we replace per-pupil expenditure quadratics with quadratic functions of term length, teacher salary per day, and inverse class size.[15] To facilitate comparisons across these three resources, each of which represent a different dollar cost per unit, we again report returns per dollar investment per student.[16]

Three salient conclusions emerge from Panel B. First, implied wage returns per dollar point to similar yields across marginal investments in term lengths, teacher salaries, and inverse class sizes (Column 1–2). In terms of wage earnings, each input returns 25–32 cents per dollar invested in the full sample and 20–44 cents per dollar among migrants.[17]

Second, these same inputs are significant determinants of better occupational standing (Columns 3–4), with some caveats. All three inputs entail significant and positive marginal returns with respect to the full sample's occupational score, but the marginal effect of teacher salaries is considerably smaller among migrants (19 versus 73 cents), and there is an insignificant gradient between inverse class size and migrants' occupation score. These observations, combined with those from Columns 1–2, suggest that omitted variables may have been more of a factor in occupational sorting than overall wages. Carruthers and Wanamaker (2017) note a

[15] In unreported regressions where we control for these three inputs alongside expenditures, returns to spending are generally smaller and less precisely estimated. This is to be expected since term length, teacher salary, and inverse class size explain three quarters of the variation in per-pupil spending.

[16] To calculate the per-student cost of a one-day longer term, we assume that the marginal cost of an additional day in session is entirely instructional and therefore equal to the average daily teacher salary multiplied by the average value of inverse class size each year for 11 years. The per-pupil cost of a one-dollar increase in daily teacher salaries is computed as the product of the $1 daily raise, term length, and inverse class size, again multiplied by 11. Finally, we calculate the dollar cost of a unit increase in the inverse class size as the average daily teacher salary multiplied by term length, again multiplied by 11 years.

[17] We thank an anonymous referee for pointing out that average teacher salaries and class sizes may have been measured with more error than the easily calculable term length. If this represents classical measurement error, estimated returns to these inputs may be attenuated toward zero.

Table 3
Marginal effects and returns to school resources by race.

Variable	(1) Ln(Annual income, 1940)	(2)	(3) Ln(1950-based occupation score)	(4)	(5) Ln(AGCT Score, 1943)	(6)
	Black men	White men	Black men	White men	Black men	White men
Panel A						
Expenditures per pupil	0.0124***	0.0038***	0.0088***	0.0018***	0.0008	0.0008***
	(0.0028)	(0.0006)	(0.0014)	(0.0003)	(0.0007)	(0.0001)
Implied return per $1	[0.335]	[0.200]	[1.130]	[0.337]	[0.004]	[0.007]
Panel B						
Term length	0.0015***	0.0027***	0.0007***	0.0007	0.0003	0.0005
	(0.0005)	(0.0009)	(0.0002)	(0.0004)	(0.0003)	(0.0004)
Implied return per $1	[0.682]	[0.716]	[1.486]	[0.674]	[0.025]	[0.023]
Average daily teacher salary	0.0145	0.0300***	0.0207***	0.0208***	0.0068	0.0066***
	(0.0144)	(0.0050)	(0.0067)	(0.0025)	(0.0052)	(0.0017)
Implied return per $1	[0.146]	[0.306]	[1.000]	[0.758]	[0.013]	[0.011]
Inverse class size	5.4357*	−0.5470	2.9156*	−0.5254	1.0435	−0.0306
	(3.1791)	(2.4905)	(1.5750)	(0.9479)	(1.0822)	(0.8144)
Implied Return per $1	[0.399]	[−0.029]	[1.025]	[−0.098]	[0.014]	[−2.5E−04]
Observations	143,592	228,151	156,789	254,253	9196	19,907

Notes: Marginal effect estimates and rate of return inferences from Eq. (1). Delta method standard errors are reported in parentheses below each marginal effect estimate. Implied returns per dollar are in brackets below standard errors. See Section 4 for methods and Section 5 for discussion. ** Significance at the 5% level.
* Significance at the 10% level.
*** Significance at the 1% level.

similar phenomenon with respect to 1940 black–white wage gaps.

Third, the effect of any input on AGCT aptitude, as measured in percentage terms by the reported marginal effects, is a small fraction of corresponding effects on income or occupational status. We only observe a portion of inputs into the human capital development process, and it is clear from these results that while inputs related to schooling were strongly predictive of labor market outcomes, the same cannot be said for cognitive aptitude among enlistees.

5.2. Results by race

Given nonlinear returns to school resources highlighted in Fig. 1 and the starkly different resources allocated to black and white schools, it would not be surprising to find that returns to school resources differ by race. Table 3 contains marginal effect estimates for each school resource and for overall expenditures per pupil, calculated at the mean value for black and white men separately. Because black schools were at the lower end of the resource distribution, these results frequently, although not universally, indicate higher returns in black schools than white.[18]

Columns 1 and 2 of Table 3 again contain returns to school resources for logged values of 1940 income. Black men earned higher labor market returns to overall school expenditures and to reduced class sizes, whereas white men realized somewhat higher returns to both teacher pay and term length. In terms of log occupational score (Columns 3 and 4), black men earned higher returns to overall expenditures and to class sizes. Although there is little racial difference in the marginal effects of teacher pay and term length on occupational scores, the estimated return per dollar invested favors black students because of much lower mean values for these inputs in black schools. Black–white differences in the return to school resources as measured by AGCT scores are unremarkable, although a larger sample size for white enlistees appears to permit more precise estimates for AGCT returns to overall spending and teacher salaries.

6. Conclusion

Overall, findings point to an important role for school resources in determining labor market outcomes for Southern men in the pre-War years; cross-sectional variation in per-pupil spending was likely recouped very quickly with additional wage earnings. We also make advances toward unpacking the historic effect of specific school resources. We find strong evidence of non-linear and diminishing marginal returns to additional spending. Looking past spending *per se*, we document very similar marginal returns to three particular school inputs, suggesting that, on average, schools were not overlooking inputs with higher returns than others. We do find, however, that districts under-invested in black schools; the labor market return to expenditures in black schools exceed

[18] Using Oaxaca decompositions of the 1940 wage gap across ten Southern states (including the four states we focus on here), Carruthers and Wanamaker (2017) find no significant racial difference in returns to an index measure of school quality when evaluated at a common value.

those for white schools. Lastly, returns to school quality may have operated through human capital channels other than aptitude, since we find relatively modest AGCT effects from enhanced school inputs for both black and white students.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found in the online version at 10.1016/j.eeh.2017.02.004.

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