School Resources and Labor Market Outcomes: Evidence from Early Twentieth-Century Georgia

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Abstract

The relationship between school resources and students' labor market outcomes has been a topic of debate among economists for the last half-century. The relatively recent public release of the 1940 US Census, the first to ask questions regarding income, allows for a closer examination of this relationship. I link children residing in Georgia in 1910 to their responses as adults to the 1940 census and to county-level school revenues collected from the reports of the Georgia Department of Education. Georgia is attractive as a case study since the State School Fund allocation rules provide a plausibly exogenous source of variation in school district revenues. A preliminary analysis, using a sample of three to seven year olds in 1910, suggests that a one standard deviation increase in school revenues received from the state per school-age child during the first three years of schooling reduced weekly wage earnings in 1940 by 1.85 percent for whites.

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1 Introduction

Whether school resources affect students' labor market outcomes later in life has been much debated in the economics of education literature over the last half-century (Hanushek 2006). While the evidence of a positive impact is mounting, Card and Krueger (1998, 49) suggest that "it may be useful to consider the evidence generated by natural experiments". Thus, I consider a historical case study of the affect of school resources in Georgia during the 1910s on students' wage earnings in 1940, exploiting plausibly exogenous variation in school district budgets generated by the rules governing the allocation of the State School Fund.

Between 1877 and 1925 the State of Georgia appropriated funds to school districts in proportion to their school-age population as determined by the most recent school census. Since the school census was taken quinquennially, large changes in school-age population were often observed between school censuses. Assuming that the population of 6 to 18 year olds changed in a smooth and continuous way, a district showing an increase in official school-age population would have been "under funded" relative to its actual school-age population in the years prior to the census update, as illustrated in Figure 1. Similarly, a district experiencing a decrease in school-age population between censuses would have been "over funded". The degree of budget distress, or surplus, generated by this rule was non-trivial, with the change in appropriations due to the 1913 census update ranging between -20.8 percent and +36.3 percent. Thus, the infrequent census updates combined with the state's budget allocation rule produced a plausibly exogenous source of variation in school resources at the district level.

To examine the impact of these periods of budget distress, or surplus, on long-run educational attainment and later-life labor market outcomes, I match children in the 1910 US Census living in Georgia to their responses as adults in the 1940 Census, creating a linked sample of individuals. The 1940 census was the first to ask all respondents about their earnings and educational attainment, the dependent variables of interest. Linking individuals to their childhood households in the 1910 census provides the county in which they likely attended school . The county in which individuals attended school allows for the data to be matched with school revenue information collected from annual reports of the Georgia Department of Education.

There is a related literature in economic history seeking to understand the determinants of education and labor market outcomes, often with particular attention to racial differences (see, for example, Margo 1990). However, up to the release of the 1940 census in 2012, it was not pos-

sible to link an individual's childhood schooling environment to wage earnings and total years of schooling as an adult. In a recent addition to this literature, Carruthers and Wanamaker (2015) utilize responses to questions in the 1940 Census regarding place of residence in 1935 to discern the probable schooling location for 18 to 25 year old men in 1940. This allows them to examine the impact of various school quality metrics on the racial differential in the earnings of young men. Whereas Carruthers and Wanamaker focus on the contribution of the racial gap in school quality to the racial gap in earnings, the proposed work seeks to estimate the impact of school resources on earnings for this period in general while acknowledging differential effects by race. This project also has the advantage of observing all individuals in their childhood home.

2 Data

The 1940 U.S. Census was the first to collect educational attainment and individual earnings data. Thus, the complete-count 1940 census data (Ruggles et al. 2010) provide a rich source of information on human capital investments and labor market outcomes. The 1940 census, however, contains only state of birth information for individuals and did not inquire as to their county of birth. While state-level analyses have been informative in some contexts, they ignore the substantial within state variation in school quality and resources present in the early twentieth century. To study the relationship between childhood schooling conditions and later life outcomes it is useful to link individuals across censuses to recover their childhood county of residence. Thus, I have linked a sample of males between the ages of 33 and 37 in 1940 to the 1910 census manuscripts, when they were between three and seven years of age.

Published reports of the Georgia Department of Education provide the school-age population by county for the school census years of 1908, 1913, and 1918. The population recorded by each school census served as the basis for the apportionment of state educational funds for the subsequent five years. Between census years I impute the school-age population. These estimates are then used to approximate the relative budget distress or surplus experienced by each county-year due to infrequent enumeration. A county with population growth would be in budget distress, while a county with declining population would have a surplus of funds, relative to the target per-child amount disbursed by the state. Relative per capita receipts from the state in county c and year t

can be calculated as follows:

$$PerCapitaReceipts_{ct} = \frac{Census\ Population_{ct}}{Estimated\ Population_{ct}}. \quad (1)$$

I assign to each individual the average receipts per capita for the first three years of schooling in their county of residence in 1910. In doing so I make two assumptions: First, children begin school at age six and attend continuously, at least through age nine. Second, those aged three to seven in 1910 reside in the county in which they attended school between the ages of six and nine.

The 1910 census information on city of residence is used to identify individuals residing in urban areas in their youth. County-level controls for urban share of the population, share of land in farms, and literacy rate are calculated from published census figures (Haines and ICPSR 2010).

3 Empirical Strategy and Results

Table 1 provides summary statistics. Columns (1) and (2) provide means for the full sample, while columns (3) and (4) show averages for those reporting earned income. Since the latter excludes many farmers and tenants, human capital and urban status measures are slightly higher for this sample.

The empirical approach is to regress later-life outcomes of interest on the average of relative school receipts per child during the first three years of schooling and controls. The estimating equation follows:

$$y_{ica} = \alpha + \theta_a + \gamma Receipts_{ac} + \beta X_{ica} + \epsilon_{ica} \quad (2)$$

where y_{ica} is the outcome of interest for individual i of age a in 1910 who attended school in county c. Using this framework I consider each of 4 outcomes: years of schooling, grammar school completion, log weekly wages, and log annual wages. The control variables included in X_{ica} are the share of land in farms, race-specific literacy rate for voting-age males, and urban share of the population for the 1910 county of residence. Additionally, I include an indicator for urban residence in 1910 and its interaction with urban share.

3.1 Results for Whites

I first consider the experience of whites. Table 2 presents the results of specifications where years of schooling is the outcome of interest. The estimate of γ is approximately 0.024, statistically significant, and robust. Thus, a 1 percentage point increase in per capita school funds relative to the state target increases human capital by one-fiftieth of a year. A one standard deviation (or 4.74 percentage point) increase yields an increase in human capital by more than one-tenth of a year, an economically significant amount. Considering that the target per capita expenditure in Georgia was $3.44 in 1913 (Georgia Department of Education 1914), the per child cost of a one standard deviation increase in expenditures for three years would have been $0.49 ($0.69 in 1940 dollars), or less than 0.1 percent of the mean annual income in 1940.

The effect of school funds on the probability of completing the eighth grade is shown in Table 3. A 1 percentage point increase in the relative receipts increased the probability of graduating from grammar school by 0.3 percentage points. This suggests higher quality schooling early on had long run benefits for the development of human capital.

Tables 4 and 5 consider the reduced-form effect of school resources on log annual and weekly wage earnings, respectively. A 1 percentage point increase in relative funding increases annual wages by 0.31 percent and weekly wages by 0.39 percent. At the 1940 mean that is equivalent to an additional $2.65 per year and $0.08 per week worked. A one standard deviation increase in school receipts led to an additional $12.56 per year ($214.10 in 2015 dollars). Thus the investment seems well worth the cost from the social planner's perspective.

3.2 African Americans

I have thus far only considered the effect of these budget shocks on whites, but African-Americans may have been similarly affected. Table 6 presents results for the sample of African Americans alone. I fail to find a significant effect of relative funding on years of schooling, grammar school completion, or wage earnings for African Americans, even though the standard errors are quite small. I caution against the interpretation that African-American children did not benefit from increased expenditures on their schools, as Aaronson and Mazumder (2011) show otherwise, rather African-American schools were likely insulated from budget shocks. This is consistent with prior evidence that budget shocks were absorbed by expenditures on white schools during the 1910s with expenditures on African-American schools remaining largely unchanged (Baker 2014).

4 Placebo Test

While the budget rules generating these periods of relative distress and surplus are arbitrary and exogenously set, the underlying population flows may be correlated with omitted variables which affect human capital and labor market outcomes, resulting in biased estimates. To examine the potential for bias, I use data on the county-level school-age population of the neighboring state of Florida and impose on it Georgia's school budget allocation rules to construct a placebo for relative funding. Since Florida apportioned state school funds on the basis of average attendance in the prior year, rather than school-age population, the constructed measure reflects population trends but is uncorrelated with school budgets.

Results for whites analogous to those presented for Georgia are shown in Table 7. The coefficients on the constructed measure are statistically significant, but they are opposite in sign, suggesting that higher rates of out-migration are associated with reduced educational attainment for the sedentary population. This result could reflect the movement of families to higher quality school districts or urban areas. These results suggest that the coefficients on relative funding presented above are downward biased and thus understate the true impact of money on human capital and labor market outcomes.

5 Conclusion

The preliminary results presented here suggest that children benefited significantly in terms of human capital and labor market outcomes from increased school expenditures. And these benefits appear to be well worth the cost.

Further work will include controls for additional family background characteristics from the 1910 census manuscripts (e.g. father's occupation and literacy of parents) to alleviate concern that omitted variables are correlated with school quality. I will also explore the degree to which the increase in wage earnings can be accounted for by years of schooling. Furthermore, data from other states with similar school fund apportionment rules will be added to verify the representative nature of these results.

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Table 1: Summary Statistics

	Full Sample		Income Sample	
	White (1)	Black (2)	White (3)	Black (4)
Individual in 1940				
Highest grade completed	8.6139	4.9561	8.8587	5.1113
	[3.6813]	[3.1406]	[3.6513]	[3.1921]
Grammar school completion	0.5745	0.1950	0.6089	0.2150
	[0.4944]	[0.3962]	[0.4880]	[0.4109]
High school completion	0.2551	0.0446	0.2786	0.0484
	[0.4359]	[0.2064]	[0.4483]	[0.2146]
Log annual wage income	6.7519	6.0607	6.7519	6.0607
	[0.9063]	[0.7925]	[0.9063]	[0.7925]
Log weekly wage income	2.9677	2.3486	2.9677	2.3486
	[0.7923]	[0.7206]	[0.7923]	[0.7206]
Weeks worked	43.9595	40.1740	45.9849	43.4165
	[14.6368]	[16.7802]	[10.4008]	[11.7599]
Labor force participation	0.9654	0.9413	0.9887	0.9854
	[0.1829]	[0.2351]	[0.1056]	[0.1198]
Employment rate	0.9678	0.9336	0.9659	0.9409
	[0.1764]	[0.2490]	[0.1816]	[0.2358]
Individual in 1910				
Relative receipts	-4.2892	-3.3456	-4.2331	-3.2988
	[4.7352]	[4.7442]	[4.7103]	[4.7220]
Urban residence	0.2016	0.1487	0.2258	0.1525
	[0.4012]	[0.3558]	[0.4181]	[0.3595]
County in 1910				
Urban share	0.1890	0.1692	0.2086	0.1748
	[0.2827]	[0.2551]	[0.2954]	[0.2598]
Literacy rate, whites	0.9056	0.9299	0.9084	0.9304
	[0.0508]	[0.0387]	[0.0510]	[0.0387]
Literacy rate, blacks	0.6040	0.5637	0.6083	0.5662
	[0.1045]	[0.1005]	[0.1038]	[0.1015]
Farm share	0.7504	0.7468	0.7458	0.7452
	[0.1557]	[0.1663]	[0.1609]	[0.1690]
Observations	24,831	10,402	17,209	7,832

Notes: Standard deviations in brackets. The full samples include all Georgia-born males between the ages of 33 and 37 in 1940 with reported years of schooling who could be matched to the 1910 census manuscripts and were residents of Georgia in 1910. However, those in counties that experienced border changes between 1908 and 1918 are excluded. Only individuals reporting earnings are included in columns (3) and (4).

Sources: Author's calculations from the complete-count 1940 IPUMS data (Ruggles et al. 2010), reports of the Georgia Department of Education, published 1910 census data (Haines and ICPSR 2010), and 1910 census index from FamilySearch.

Table 2: Effect of Budget Shocks on Years of Schooling, White

	(1)	(2)	(3)	(4)
Relative receipts	0.0243***	0.0250***	0.0248***	0.0240***
	(0.0050)	(0.0049)	(0.0049)	(0.0049)
Urban residence		2.1382***	1.6120***	1.3017***
		(0.0566)	(0.0780)	(0.1194)
Literacy rate, whites			13.1004***	13.1907***
			(0.5603)	(0.5608)
Farm share			0.4554***	0.6176***
			(0.1685)	(0.1749)
Urban share			-0.2279*	-0.5164***
			(0.1342)	(0.1583)
Urban res. X urban share				0.8038***
				(0.2343)
Constant	8.9580***	8.4891***	-3.5773***	-3.7595***
	(0.0587)	(0.0584)	(0.5305)	(0.5331)
Observations	24831	24831	24831	24831
R-squared	0.0023	0.0565	0.0804	0.0808

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.
Notes: Standard errors in parentheses. All specifications include age fixed effects.

Table 3: Effect of Budget Shocks on Grammar School Completion, White

	(1)	(2)	(3)	(4)
Relative receipts	0.0029***	0.0030***	0.0030***	0.0029***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Urban residence		0.2127***	0.1428***	0.1168***
		(0.0077)	(0.0106)	(0.0163)
Literacy rate, whites			1.7145***	1.7221***
			(0.0763)	(0.0764)
Farm share			0.0598***	0.0734***
			(0.0230)	(0.0238)
Urban share			-0.0278	-0.0520**
			(0.0183)	(0.0216)
Urban res. X urban share				0.0674**
				(0.0319)
Constant	0.6095***	0.5628***	-1.0167***	-1.0320***
	(0.0079)	(0.0079)	(0.0723)	(0.0726)
Observations	24831	24831	24831	24831
R-squared	0.0014	0.0312	0.0540	0.0541

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.
Notes: Standard errors in parentheses. All specifications include age fixed effects.

Table 4: Effect of Budget Shocks on Log Annual Wage Earnings, White

	(1)	(2)	(3)	(4)
Relative receipts	0.0029*	0.0032**	0.0031**	0.0031**
	(0.0015)	(0.0015)	(0.0014)	(0.0015)
Urban residence		0.4811***	0.2929***	0.2971***
		(0.0160)	(0.0221)	(0.0341)
Literacy rate, whites			2.2405***	2.2393***
			(0.1682)	(0.1684)
Farm share			0.1650***	0.1628***
			(0.0497)	(0.0515)
Urban share			0.1874***	0.1913***
			(0.0389)	(0.0455)
Urban res. X urban share				-0.0108
				(0.0660)
Constant	6.7500***	6.6343***	4.4759***	4.4783***
	(0.0171)	(0.0171)	(0.1592)	(0.1599)
Observations	17418	17418	17418	17418
R-squared	0.0004	0.0497	0.0673	0.0673

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.
Notes: Standard errors in parentheses. All specifications include age fixed effects.

Table 5: Effect of Budget Shocks on Log Weekly Wage Earnings, White

	(1)	(2)	(3)	(4)
Relative receipts	0.0037***	0.0040***	0.0039***	0.0039***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Urban residence		0.4307***	0.2565***	0.2593***
		(0.0140)	(0.0194)	(0.0298)
Literacy rate, whites			1.7037***	1.7030***
			(0.1471)	(0.1472)
Farm share			0.1231***	0.1217***
			(0.0434)	(0.0450)
Urban share			0.2052***	0.2078***
			(0.0340)	(0.0398)
Urban res. X urban share				-0.0071
				(0.0577)
Constant	2.9665***	2.8629***	1.2167***	1.2183***
	(0.0149)	(0.0149)	(0.1391)	(0.1397)
Observations	17418	17418	17418	17418
R-squared	0.0010	0.0526	0.0688	0.0688

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.
Notes: Standard errors in parentheses. All specifications include age fixed effects.

Table 6: Effect of Budget Shocks on the Outcomes of African Americans

	(1)	(2)	(3)	(4)
Relative receipts	-0.0009	0.0004	0.0009	0.0013
	(0.0065)	(0.0008)	(0.0019)	(0.0017)
Urban residence	0.3392*	0.0480**	-0.0261	-0.0133
	(0.1751)	(0.0221)	(0.0508)	(0.0460)
Literacy rate, blacks	1.9200***	0.1824***	0.0023	0.0891
	(0.3985)	(0.0504)	(0.1173)	(0.1064)
Farm share	0.4020*	0.0355	0.0365	0.0364
	(0.2093)	(0.0265)	(0.0604)	(0.0548)
Urban share	0.6266***	0.0925***	0.1364**	0.1313**
	(0.1995)	(0.0252)	(0.0572)	(0.0519)
Urban res. X urban share	1.0539***	0.1073**	0.2660***	0.2389***
	(0.3372)	(0.0426)	(0.0964)	(0.0874)
Constant	3.4111***	0.0398	5.9765***	2.2342***
	(0.2994)	(0.0378)	(0.0879)	(0.0797)
Observations	10402	10402	7922	7922
R-squared	0.0348	0.0311	0.0105	0.0143

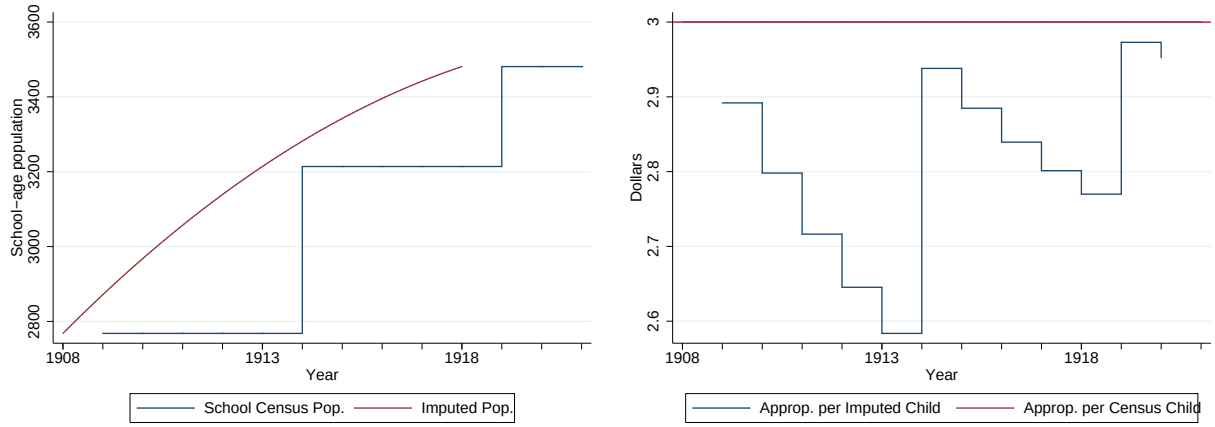
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.
Notes: Standard errors in parentheses. All specifications include age fixed effects.

Table 7: Placebo Test

	(1)	(2)	(3)	(4)
Placebo receipts	-0.0115***	-0.0013**	-0.0021*	-0.0276**
	(0.0039)	(0.0006)	(0.0012)	(0.0137)
Urban residence	2.3042***	0.1762***	0.5914***	6.3395***
	(0.1843)	(0.0265)	(0.0573)	(0.6400)
Literacy rate, whites	14.8737***	1.8084***	2.5197***	18.2308***
	(0.9037)	(0.1297)	(0.2874)	(3.1545)
Farm share	0.3376*	0.0394	0.2689***	-0.0010
	(0.1987)	(0.0285)	(0.0631)	(0.6907)
Urban share	-0.6629***	-0.0873***	0.3493***	2.5830***
	(0.1898)	(0.0272)	(0.0581)	(0.6587)
Urban res. X urban share	-1.3094***	-0.1080**	-0.5675***	-4.3518***
	(0.3334)	(0.0479)	(0.1026)	(1.1585)
Constant	-5.1534***	-1.0279***	4.1190***	6.3723**
	(0.8606)	(0.1235)	(0.2733)	(3.0040)
Observations	9673	9673	7215	9527
R-squared	0.0815	0.0395	0.0727	0.0488

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.
Notes: Standard errors in parentheses. All specifications include age fixed effects.

Figure 1: How Georgia's School Fund Allocation Rule Generated Periods of Budget Distress



(a) Official versus Imputed School-Age Population (b) State School Funds per School-Age Child

Note: Panel (a) compares the imputed school-age population with the official school-age population of a school district, as given by the latest school census at the time State School Fund appropriation decisions were made. School censuses were conducted in the spring of 1908, 1913, and 1918. Since appropriations for the next school year were set in January each year, there was approximately a one year delay in the utilization of new school census figures (hence the discrete jump in official school-age population in 1914 rather than 1913). The actual population of 6 to 18 year olds is assumed to have changed in a smooth and continuous fashion between school censuses. If the actual school-age population is increasing between census years (as shown), then the official school-age population will be less than the actual school-age population when funding decisions are made. Panel (b) shows how this results in periods of underfunding. It assumes that the State School Fund is apportioned in each year such that districts are given three dollars for every child of school age, according to the latest school census. Due to the infrequent census updates and the delay in utilization, the amount of funds received per school-age child is less than the target of $3 and the degree of the shortfall is increasing in the number of years since the last census.