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ABSTRACT

Cities in the United States dramatically expanded spending on public education in the years following World War I, with the average urban school district increasing per pupil expenditures by over 70 percent between 1916 and 1924. We provide the first evaluation of these historically unprecedented investments in public education by compiling a new dataset that links individuals to both the quality of the city school district they lived in as a child and their adult outcomes. Using plausibly exogenous growth in school spending generated by anti-German sentiment during and after World War I, we find that school resources significantly increased educational attainment and wages later in life, particularly for the children of low socioeconomic status households. Increases in expenditures can explain between 26 and 36 percent of the sizable increase in educational attainment of cohorts born between 1895 and 1915.

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"Every great war in which the United States has played a part has been followed by educational developments of supreme national importance…Although the United States was engaged in the World War less than two years, the effects upon education resulting from this brief period of warfare will perhaps prove to be as far-reaching and as important as those growing out of any previous war…Undoubtedly the World War was the most important factor in awakening the American public to the inadequacy of its educational provisions and in arousing the States to vigorous efforts to improve educational conditions."

– Fletcher Harper Swift, *Biennial Survey of Education 1920-1922, Volume 1*, pp. 1-2.

1. Introduction

The question of how much to invest in education – and the returns to those investments – has attracted a great deal of attention in economics, particularly in light of the substantial increases in spending occurring nationwide since the 1960s (Coleman et al., 1966; Card and Krueger, 1992; Betts, 1996; Hanushek, 1986, 1996). Publicly funded education has long been viewed as the most important policy tool for improving the future labor market outcomes of children, particularly for youths from disadvantaged backgrounds. Accordingly, concerns about disparities in access to educational resources have motivated a complex and evolving system of transfers from the federal and state governments to local districts.[1] However, for much of American history, local governments assumed the bulk of the responsibility for financing their own school systems (Stoddard, 2009).

As this paper's epigraph indicates, most wars the United States has been involved in have brought about educational reforms.[2] We study the impact of the unprecedented investments in public education made by city school districts in the aftermath of World War I and provide the first nationwide, district-level analysis of spending on education in early twentieth-century

[1] For instance, the state share of public elementary and secondary school revenues nationally grew from 30 percent to over 50 percent between 1940 and 1990 ("Revenues and Expenditures for Public Elementary and Secondary School Districts" (Fiscal Year 2010), National Center for Education Statistics: https://nces.ed.gov/pubs2013/2013307.pdf). Federal outlays increased significantly beginning in the 1960s.

[2] The G.I. Bill is, perhaps, the best-known example of these reforms.

America. We digitized reports of education published biennially for city school districts – the finest geographic unit for which there are comprehensive surviving records from this period – from 1900 to 1930 for major cities in the United States. Panel A of Figure 1 displays the trend in real expenditures per pupil in our sample of cities separately by census region. The growth in educational spending by cities after the United States entered World War I marked a significant departure from nineteenth century levels. On the eve of the United States entering World War I (1916), real expenditures per pupil were about $82 (in 1930 dollars). However, between 1916 and 1924, expenditures ballooned to $142 per pupil, a 73 percent increase. Such a rapid increase in real expenditures per pupil would not occur again until the 1960s.

This paper leverages several newly digitized data sources to examine the impacts of these investments. We constructed measures of student exposure to higher spending on education using the *Report of the Commissioner of Education* (1900-1916) and the *Biennial Survey of Education* (1918-1930). To obtain adult outcomes for students educated in urban schools during these decades, we matched school-age individuals from the 1900, 1910, 1920, and 1930 complete count censuses to the 1940 complete count census. Because the 1940 census only contains information on state of birth, linking individuals is essential to match adults in 1940 to the city-level school resources they experienced as children. An advantage of our approach is that we can investigate the potentially heterogeneous returns to educational resources based on childhood socioeconomic status, a task that is generally not possible with retrospective analyses.

The trends apparent in Figure 1 suggests that World War I played a critical role in this early major investment in public education in American cities. This war-driven expansion of school resources has received relatively little attention in economics.[3] However, the expansion of public

[3] While the relationship between urban school spending and student outcomes have been largely unexplored for the early twentieth century, a large literature has investigated the impacts of educational investments made in the ensuing

education over the course of the early twentieth century has been well documented (see Goldin and Katz, 1997; Goldin, 1998, 2001). Included in the story of American educational expansion is that Progressive Era reformers were motivated by the need to prepare youths for the American labor market, and voters largely supported investments in education (Goldin, 2001). Progressive Era education crusaders particularly supported higher school spending in "foreign and congested" neighborhoods as well as improved instruction in matters of citizenship, character formation, and vocational education that emphasized the hierarchical nature of early twentieth century work (Amsterdam, 2016).

Our empirical strategy relies on the observation that post-World War I expansions in school resources were in part a response to immigrants from enemy nations who had already settled in the United States. World War I abruptly downgraded the status of ethnic Germans living in the Unites States, with lasting effects throughout the 1920s (Moser, 2012; Ferrara and Fishback, 2022). Anti-German hysteria exploded across many facets of American life, from outright violence to politically divisive Americanization laws, some of which outright forbid instruction in the German language (Lleras-Muney and Shertzer, 2015; Fouka, 2020). City governments reacted with panic to large populations of German descent and undertook efforts to assimilate the children of enemy aliens through public schooling, along with expanded city school budgets (Ross, 1994). We argue that anti-German panic can be used to construct an instrument for school resources in the immediate aftermath of World War I.

decades. A significant number of papers, particularly those using test scores as outcomes and a difference-in-differences approach, echo the findings of the Coleman Report and find little evidence of a relationship between school inputs and student outcomes. On the other hand, a literature using state-level aggregated education metrics has largely found positive returns to mid-twentieth century school expenditures (Morgan and Siragelding, 1968; Akin and Garfinkel, 1977; Card and Krueger, 1992).

The intuition for our approach borrows from the literature on the impact of post-1960 increases in public school resources, which has found that estimates depend crucially on whether expenditures are exogenously determined (Lavy, 2015; Jackson et al., 2016; Lafortune et al., 2018) The narrative history suggests that city school system administrators during the Progressive Era responded to deteriorating student outcomes by increasing spending. A naïve panel estimation of the returns to such endogenously determined school resources might be biased towards zero. We develop an instrument for increases in school spending that is instead related to anti-German sentiment as proxied by exposure to post-World War I school spending in cities that had differing levels of German settlement prior to the conflict. We show this instrument is both predictive of spending on education after World War I and robust to a range of tests for violations of the exclusion restriction. Importantly for our identification strategy, it is not the case that educational attainment or wages were trending differentially across cohorts in cities with different German shares prior to World War I.

We find limited evidence of a positive return to educational spending associated with endogenous increases in resources for either attainment or wages, consistent with reactionary increases in school resources by early twentieth century city school boards. However, utilizing variation in spending arising from pre-WWI German settlement yields economically significant estimates. A 10 percent increase in educational expenditures per pupil across all eight mandatory years of education led to an increase in educational attainment of about one month. We also find that a 10 percent increase in expenditures per pupil increased the probability of completing eight and twelve years of schooling by about 2 percentage points and increased wages in adulthood by about 1.6 percent.

We also find varying results by childhood socioeconomic status. In particular, the effects for completing eight years of schooling and wages are driven by the children of blue-collar workers. The eight years of schooling effect is four times larger for these children than the children of white-collar workers. Increased educational resources related to anti-German sentiment appear to have primarily benefited the children of lower socioeconomic status families, regardless of nativity. Overall, higher spending on public education can explain between 26 and 36 percent of the sizable increase in attainment of cohorts born between 1895 and 1915.

Our findings shed new light on the long-running debate on returns to school resources in the United States. We find robust evidence that investments in public schools led to higher educational attainment and adult wages for less-advantaged children, providing an urban companion to papers examining the impacts of historical school spending in rural areas in the Midwest (Parman, 2012) and in the South (Carruthers and Wanamaker, 2017; Mauer, 2019; Cascio and Lewis, 2024). Investments in public education are also relevant to the increases in the return to education around the middle of the twentieth century studied in recent papers (Card et al. 2018; Feigenbaum and Tan, 2020).

World War I was a watershed in the provision of public education in the United States, yet we find little evidence that even large investments that primarily benefited less-advantaged children were effective in closing the urban educational attainment gap that existed between the children of high and low-skilled fathers, which remained constant at about one year throughout the early twentieth century. However, it is possible that educational investments made by cities allowed the "Great Compression" of wage inequality to occur later in the twentieth century by helping the children of unskilled workers at least keep up with their more advantaged peers (Goldin and Margo, 1992; Collins and Niemesh, 2019). Our findings thus also relate to the history of

transfers intended to equalize access to school resources demonstrating that the gains in our context were generated by cities themselves – however indirectly – rather than through financing schemes undertaken by the state or federal governments (Cascio et al., 2013).

2. Background and Historical Context

2.a. Public education around World War I

The early twentieth century saw rapid population growth in cities, fueled largely by immigration from Europe. Foreign-born workers were seen as resistant to assimilation into American society, and, troubling for city leaders, susceptible to organized labor movements.[4] The concerns about unassimilated immigrants heightened as the United States entered World War I, and reformers called for investments in public education to help immigrant youth adopt American values for the sake of national solidarity. A quote from an introduction to one edition of the *Biennial Survey of Education* illustrates why the conflict generated pressure to improve education across the county:

> "It was not until American Army officers found it necessary to have their orders shouted to American privates in three, four—yes, and even five—languages that America awoke, awoke to the fact that in a country whose laws, whose very ideals were written in English, thousands upon thousands of adult citizens could not read a single word of the language of their adopted country."[5]

The German population in U.S. cities was substantial and a source of concern for elected leaders. On April 6, 1917, President Woodrow Wilson signed the declaration of war against Germany and issued proclamation 1364 in which he warned at length of the dangers of enemy aliens, which he defined to be male immigrants from Germany over the age of thirteen.[6]

[4] Annual Report of the Detroit Public Schools, 1920.
[5] *Biennial Survey of Education 1920-1922*, p. 2.
[6] Wilson spent 19 of the 25 paragraphs of this proclamation speaking about enemy aliens and he warned them to "preserve the peace towards the United States and to refrain from crime against public safety." He even set limits on enemy aliens' proximity to government buildings: "An alien enemy shall not approach or be found within one-half of

Meanwhile, the Justice Department attempted to compile a list of all male and female German immigrants and arrested over 4,000 of them on charges of espionage (Yockelson, 1998). Anti-German sentiment reached its peak in April of 1918 when Robert Prager, a German immigrant, was lynched by a mob in Collinsville, Illinois.[7]

Education was viewed as the foremost policy tool for controlling the Teutonic threat by inculcating a sense of loyalty to America in individuals of German descent. City leaders hoped that children would introduce their parents to the English language and American values they learned about in school (Schlossman, 1983; Ross, 1994). Accordingly, school curricula were reformed to include matters of citizenship and civic duty and to eliminate the teaching of German (Land, 2002). However, the literature has generally found that Americanization education policies did not improve assimilation-related outcomes for foreign-born youth (Lleras-Muney and Shertzer, 2015; Fouka, 2020). We thus take the view that investments in public education after World War I improved the school environment for children in general without having much of a direct assimilation effect on immigrant children. Our results are consistent with this historical interpretation; effects of school spending on immigrants and the native born are generally similar, and our results are not driven by German youths.

Even when not explicitly stated, much of the push to assimilate immigrants was directed towards Germans. For example, the above quote about Army officers having to shout orders in multiple languages does not specifically mention Germans. However, William Ross fills in the context of this quote: "Of the ten million registrants for the draft during the war, some 700,000

a mile of any Federal or State fort, camp, arsenal, aircraft station, Government or naval vessel, navy yard, factory, or workshop for the manufacture of munitions of war."

[7] See Hickey (1969) for a detailed explanation of this event. Although extreme, this lynching was far from the only instance of mob violence toward German immigrants during World War I. There were numerous other instances of mob violence in Kansas and Illinois and a plaque in Cincinnati still commemorates the "Anti-German Hysteria" that swept the city in 1917 and 1918 (Juhnke, 1975).

could not sign their own name, and many others were literate only in a foreign language, usually German" (Ross, 1994, pp. 59-60).[8] Our finding that cities with larger German populations increased spending on public education by greater amounts is not surprising when viewed in this broader historical context. Education reformers found the German threat narrative to be a useful tool for increasing public support for the ballooning school budgets that were proposed in cities across the country in the aftermath of the conflict. In the city of Chicago, a former member of the Board of Education proposed the enactment of a *criminal statute* compelling school attendance for any American between the ages of 16 and 45 who could not read and write in the summer of 1918, just a few months before the Armistice was signed (Ross, 1994). Such thinly veiled attempts to demonize unassimilated immigrants, at the moment of peak anti-German sentiment, likely assisted with the passage of new education budgets even if few such proposals became law. With these increased resources, city school reformers could tackle goals such as increasing basic literacy by improving the quality of schools and the duration of time poor pupils spent attending them. "Intermediate" or middle schools, an innovation intended to help keep older children from dropping out became common, and high schools with multiple tracks were also introduced (Goldin and Katz, 2008b). We provide empirical support for the relationship between German immigration and increases in school resources in Section 4.b., but we note here that our quantitative documentation of this relationship is, to our knowledge, a new historical fact.

2.b. The role of the state in the provision of public education

[8] As this quote demonstrates, World War I also revealed the extent of illiteracy among draft-aged men; a 1921 government report suggested that perhaps a quarter of men in wartime Army camps could not read or write in English. This report, edited by Robert Yerkes, is entitled *Psychological Examining in the United States Army*. Table 279 of this report shows that about 25 percent of men were administered the "beta" intelligence tests, which "was developed primarily for men who could not read and write English and was used for these men in place of the alpha examination, which presupposes English literacy" (p. 743; Yerkes, 1921).

As mentioned in the introduction, local governments assumed the bulk of the responsibility for financing public schools. Nevertheless, is it useful to understand the role that various levels of government played in the provision of public education. During the early twentieth century state governments limited their involvement in public education to two areas. First, state legislatures passed compulsory schooling laws (CSLs) and child labor laws intended to keep children in school through eighth grade (or longer if they were not in the labor force). Studies investigating the impacts of these laws have found mixed results but generally agree that state legislation was not the primary driver of the increase in educational attainment in the early twentieth century.[9]

The second source of state involvement was in providing transfers to municipalities to support education. Beginning in the middle of the nineteenth century, state governments began to recognize that some municipalities and counties were too poor to provide a quality public education to children living within their borders. The typical policy response was to pass a law requiring all localities to provide at least universal primary school access. The states would then provide a "flat grant", or a lump sum of money, to each locality to help finance the operation of those primary schools. Flat grants were distributed to rich and poor districts alike. As the cost of education rose in the early twentieth century, states switched their funding formulas to a per classroom, per teacher, or even per school-aged pupil flat grant (Odden and Picus, 2004).

It was not until the Strayer and Haig (1923) report, *Financing Education in the State of New York*, that states began to switch from flat grant financing schemes to "foundation" programs. These programs set a minimum foundation level of revenue per pupil that a district should collect

[9] Landes and Solmon (1972) find no effect of compulsory schooling laws (CSLs) while Eisenberg (1988) finds modest effects on school attendance. Margo and Finegan (1996) find that CSLs significantly increased attendance in states that coupled a CSL with comprehensive child labor laws. Lleras-Muney (2002) finds that legally requiring children to attend one more year of school increased educational attainment by 5 percent. Clay et al. (2021) use CSLs to demonstrate that the returns to schooling were highest for the lowest quantiles of the 1940 wage distribution.

in taxes. If poorer districts could not meet this minimum, then the state made up the difference. Such equalization schemes gained traction during the Great Depression and were widespread by 1940. Importantly, these foundation programs were not in effect during our study period. Figure 2 shows the percent of city school revenues that came from the state government in 385 major cities during our study period. In 1930, city governments were contributing about 85 percent of the revenue for schools, while states were contributing just over 10 percent.[10]

The impact of early grant programs on local school finances has gone largely unexplored in economics. Of particular interest is the question of whether policy changes that increased funds from the state can serve as an instrumental variable for school resources in the spirit of the court-ordered school finance reforms used by Jackson et al. (2016). We obtained information on which states passed laws mandating major increases in grants from the state to local districts immediately following World War I from the *Biennial Survey of Education*.[11] In some cases, state aid was doubled. Figure 3 illustrates the impacts of these policy changes. Panel A shows the change in state aid per pupil in cities located in states that modified their grant law compared to those that did not change their grant law. The cities in states with changes to their grant law saw large relative increases in state aid per pupil after World War I. However, revenues raised by the city appear to have dropped by an equivalent amount, as Panel B shows that expenditures per pupil were virtually the same in the two sets of cities after the laws came into force.

These figures suggest that early grant programs crowded out local spending on education. We confirm these findings by running a simple two-way fixed effects difference-in-differences

[10] Some states, such as Missouri, depended on counties as the primary unit of organization to support education.
[11] The 1920-1922 *Biennial Survey of Education* reports that "Among the States which since the close of the World War provided for greatly increased school revenue to be furnished by the State are Arizona, California, Georgia, Iowa, Louisiana, Massachusetts, New York, North Carolina, Pennsylvania, South Carolina, Texas, Utah, Washington, and West Virginia" (p. 16).

regression on our baseline sample of cities where we include dummies for the post-WWI period and the passing of a state law increasing grant aid, as well as the interaction of these factors. We report the results in Table 1. In states that passed a grant law after World War I, city school districts saw a 64 percent increase in per pupil aid from the state government (column 1). However, the revenues a school received from the city decreased by about 18 percent (column 2). This left expenditures per student unchanged (column 3). It thus appears that increased state aid to schools after World War I crowded out local investments in education.[12] We therefore develop a novel instrument for changes in educational resources using anti-German sentiment, which is discussed in Section 4.

3. Data

3.a. City school resource data

We used the *Report of the Commissioner of Education* (1900-1916) and the *Biennial Survey of Education* (1918-1930) to construct a new city-level dataset on public school resources during the early twentieth century. We collected the available data on school resources for every other academic year, beginning with the 1899-1900 academic year.[13] The reports contain information on expenditures on teachers and supervisors, expenditures on capital, other expenditures, average daily attendance in public schools, the number of public-school teachers, and the revenues that city school districts received from city, county, and state governments. These data allow us to compute

[12] We have, also, run these regressions using levels rather than logs. We find that state aid per pupil increased (significantly) by $4.53 for cities in states that passed a grant law after WWI, but receipts from the city decreased (significantly) by $4.82. Thus, the crowd out of local funding due to increased state aid was almost one for one.

[13] We have data for academic years beginning with an odd number from 1899-1900 through 1929-1930 with the exception of 1915-1916. We could not locate a report for the 1915-1916 academic year, so we collected data for the 1914-1915 academic year instead.

total expenditures per pupil, which we define as the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a school. For our analysis, we form a panel of 385 of the largest cities in the United States during the early twentieth century.[14]

To provide a more complete picture of the evolution of school resources in the early twentieth century we graph a time series of real expenditures per pupil and the pupil-teacher ratio by census region in Figure 1. Panel A of Figure 1 shows real expenditures per pupil, which were fairly flat from 1900 to 1920. It is only after 1920 that large real increases are evident. Average real expenditures per pupil increased from $78 in 1920 to $142 in 1924, an 82 percent increase. Panel B graphs the pupil-teacher ratio, which decreased steadily from 1900 to about 1920 before levelling off. Figure 1 suggests that increased expenditures after WWI were not simply a matter of smaller class sizes.

To explore this idea further, Figure 4 breaks the time series of real expenditures per pupil into three main categories of expenditures: expenditures on teachers and supervisors, operations, and capital. The overall trend shows decreasing percentages spent on teachers and supervisors and increasing percentages spent on capital and operations. Expenditures on teachers and supervisors made up about 60 percent of total expenditures in 1900, but this category dropped to around 50 percent by 1930. Expenditures on capital and operations each made up less than 20 percent in 1900, but had each increased to around 25 percent by 1930, reflecting a burst of new school construction and administration. Panel A of Table 2 displays summary statistics for the 385 city

[14] For academic years where data is missing for one of our cities it is interpolated by using the two adjacent academic years. The population of cities in the sample exhibits a long right tail, with a few cities having very large populations. New York City is an extreme outlier with a population of 3,437,202 in 1900, which is over twice the size of the next largest city (Chicago). The strength of our first-stage estimates are slightly sensitive to the inclusion of New York City, and accordingly we chose to drop New York City from our analysis even though our second-stage results are similar when including it (i.e. our first-stage F-statistic drops from 11.39 to 8.55). The cities in our sample are shown in Appendix Figure A.I.

school systems in our sample for four academic years: 1899-1900, 1909-1910, 1919-1920, and 1929-1930.

3.b. Linked Census Samples

To measure if student outcomes were impacted by the large expansion of school investments associated with World War I, we construct a dataset of individuals linked from the 1900, 1910, 1920, and 1930 complete count censuses to the 1940 complete count census (Ruggles et al., 2021). Linking individuals is necessary to match adults in 1940 to the local level of school resources they experienced as children. We begin our linking procedure by restricting the 1900, 1910, 1920, and 1930 censuses to males, who were 6 to 15 years of age when the census occurred and were living in one of the 385 cities for which we have school resource data.

We employ the ABE linking algorithm, which was developed by Ran Abramitzky, Leah Boustan, and Katherine Eriksson and used in Abramitzky et al. (2012), Abramitzky et al. (2014), Abramitzky et al. (2021b), and many other papers. We begin by adjusting first names for common nicknames and then standardize each first and surname using the NYSIIS algorithm, which transforms a word into a phonetic code. We then restrict our sample to individuals who are unique by NYSIIS first name, NYSIIS surname, birthplace, and birth year. For each individual in the 1900, 1910, 1920, and 1930 censuses we search for records in the 1940 census that match exactly on NYSIIS first name, NYSIIS surname, birthplace, and birth year. If we find a unique match, then we declare this observation to be a match. If we find multiple matches, then the observation is discarded. If we do not find a unique match then we continue to search for individuals who match exactly on NYSIIS first name, NYSIIS surname, and birthplace, but we now allow birth year to differ by up to one year (e.g. if an individual in the 1910 census reports a birth year of 1902 we

search for individuals in the 1940 census with a birth year of 1901 and 1903). If no unique match is found we continue to search for individuals who match exactly on NYSIIS first name, NYSIIS surname, and birthplace, but we now allow birth year to differ by up to two years. The ABE algorithm is one of many algorithms currently used to link individuals across censuses. For other algorithms see Abramitzky et al. (2020), Bailey et al. (2020), and Feigenbaum (2016). Importantly, Abramitzky et al. (2021a) show that automated approaches, including the ABE algorithm, result in low false positive rates and similar coefficient estimates compared to a hand linked sample.

The results from this linking procedure are displayed in Appendix Table A.I. From the 1900 complete count census we searched for 1,948,639 individuals and were able to find 585,386 of them in the 1940 census (a 30 percent link rate). Our link rates for 1910, 1920, and 1930 are 33, 35, and 39 percent, respectively, which are higher than the standard in the literature. For example, we were able to successfully link 33 percent of individuals from the 1910 to the 1940 census, whereas Abramitzky et al. (2021b) were able to successfully link 29.5 percent of individuals between the same censuses. This discrepancy in link rates is likely due to the fact that Abramitzky et al. (2021b) link individuals both forward in time (from the 1910 to 1940) and backwards in time (from the 1940 to 1910) and take the intersection of individuals that were linked both ways. We only link forward since we need information on the city an individual was living in as a child, which is not available in the 1940. Despite the slightly higher link rates, we show the robustness of our main results to alternative and more conservative matching procedures in Section 5.b.

Panel B of Table 2 displays summary statistics for our sample of linked men. All of the summary statistics are calculated using the number of observations reported at the bottom of the table except for weekly wages. We construct weekly wages by dividing annual income by the number of weeks worked, both reported for the year 1939. We then follow Acemoglu et al. (2004)

by censoring weekly wages at the 98th percentile and assigning values above the 98th percentile with 1.5 times the 98th percentile wage.[15] For weekly wages, we only include individuals that were wage workers, worked more than 30 weeks in the year, and were not employed in a work relief program such as the CCC or WPA.

Appendix Table A.I shows that our linked sample appears to be representative of the overall population. For example, the average age of individuals in 1900 that we were able to link was 10.01 years, while it was 10.06 years in the entire sample that we attempted to link. We do find that children from families that likely had higher socioeconomic statuses were more likely to be linked. For example, individuals in our linked sample are usually about 4 percentage points more likely to live in a dwelling that is owned, as opposed to rented. In addition, the parents of individuals in our linked sample have a slightly higher literacy rate and the fathers have slightly higher occupational income scores (OCC scores). We address these differences by splitting the sample based on whether the father is high or low socioeconomic status in some specifications.

4. Empirical strategy

4.a. Panel Estimation using OLS

The objective of our empirical work is to identify the causal effect of the large increases in school resources that occurred during the early twentieth century on adult outcomes. We begin with a naïve estimation of the effect in a panel framework using the following specification:

$$[outcome]_{iec} = \boldsymbol{X'_{iec}}\boldsymbol{\delta} + \boldsymbol{Y'_{ec}}\boldsymbol{\beta} + \gamma_c + \gamma_e + \varphi[\ln expenditures\ per\ pupil]_{ec} + \tau_{iec} \quad (1)$$

[15] In our sample, the 98th percentile weekly wage is \$90.05 so we replace weekly wages above this amount with $1.5 \times \$90.05$ or \$135.08.

In equation (1), i indexes individuals, e indexes city-of-education, and c indexes cohorts. $[outcome]_{iec}$ is one of four adult outcomes: (1) educational attainment, (2) the probability of completing eight years of schooling, (3) the probability of completing 12 years of schooling, and (4) weekly wages. We restrict the sample to white men born between 1894 and 1916 because our school resource data covers the 1900 to 1930 period and we, therefore, can only compute an average of expenditures per pupil for all mandatory school-age years for these cohorts.[16] As mentioned above, when weekly wages are the dependent variable we restrict the sample to wage workers that worked more than 30 weeks in the year and that were not employed in a work relief program.

The vector $\boldsymbol{X'_{iec}}$ contains individual-level characteristics including: mother's literacy (three dummy variables: mother literate, mother illiterate, and mother not present), father's literacy (three dummy variables: father literate, father illiterate, and father not present), mother's occupation (dummies), and father's occupation (dummies). The vector $\boldsymbol{Y'_{ec}}$ are city-of-education by cohort level controls. This vector contains a series of variables for the average percentage of each county's working population employed in various professions during each cohort's mandatory school age years.[17] In particular, we control for the percent of a county's working population employed as a professional, craftsmen, operator, service worker, laborer, or farmer (non-occupational responses are the omitted category).[18] $\boldsymbol{Y'_{ec}}$ also controls for the average number

[16] In addition, we face the issue that we are assigning school resources based on year of birth, but we allowed year of birth to differ by up to two years when performing the linking. We resolve any discrepancies by assigning school quality based on the birth year that is reported when the individual was a child (i.e. birth year reported in the 1900, 1910, 1920, or 1930 censuses). We also test the robustness of our main results using just individuals that match exactly on birth year and find little difference.

[17] We use the county, not the city, to construct this measure because IPUMS data do not identify all of the cities in our sample. We map the cities in our sample to the county they were located in during the 1910 census.

[18] To calculate these variables, we divided the number of individual's reporting a particular profession in a county by the total number of individuals reporting any profession in that county using the 1900-1930 complete count censuses from the Integrated Public Use Microdata Series (IPUMS; Ruggles et al., 2021). These percentages are then linearly interpolated between census years.

of mandatory school years, which are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a).

γ_c are cohort fixed effects and γ_e are city-of-education fixed effects. For our main treatment variable, we construct a measure of a student's exposure to school resources, $[\ln expenditures\ per\ pupil]_{ec}$, which is the log of average expenditures per pupil (in real 1930 dollars) during expected school-age years (ages 6-14 during our time period) for individuals in cohort c who were educated in city e. Finally, τ_{iec} is a stochastic error term and we cluster standard errors at the city-of-education level.

For equation (1) to estimate a causal effect it must be the case the expenditures per pupil were randomly assigned to city school districts. This assumption, undoubtedly, does not hold. Thus, our primary concern in equation (1) is that the OLS panel estimates are biased. For example, cities with booming labor markets could increase school expenditures (perhaps from increases in the tax base) and student outcomes could improve through mechanism unrelated to school expenditures biasing our results away from zero. Alternatively, if cities made dynamic investment decisions and increased spending by more when schooling outcomes were deteriorating, estimated impacts of school resources would likely be biased towards zero. The narrative history certainly suggests that reformers lobbied for larger school budgets in response to the poor performance of "foreign elements" in the school system during a period of generally high immigration before 1924. To deal with the endogeneity of expenditures per pupil, we next discuss our instrumental variables approach, which aims to isolate variation in school resources that are uncorrelated with trends in student outcomes.

4.b. Instrumental variables approach using anti-German sentiment

Our instrumental variable approach exploits variation in educational spending that arose as a result of anti-German sentiment as opposed to concerns about schooling outcomes. Specifically, we construct a measure of exposure to years of education after World War I interacted with the share of the population in a city that was of German descent in 1910, prior to World War I. Our approach shares some similarities to Acemoglu et al. (2004) who use county German share to obtain variation in World War II mobilization rates uncorrelated with economic conditions. In this section we show that our instrument is both predictive of future increases in educational spending and that German share, not general immigrant levels, is driving increases in investments. Finally, we provide a set of checks of the exclusion restriction, including showing that cities with different German shares were not on different trajectories of wages or educational attainment prior to WWI.

We visualize the basic variation underlying our instrument in Figure 5. In this figure we subdivide our sample of cities by median German population share and show trends in spending per pupil.[19] Prior to World War I, cities with higher German shares spent more on education than cities with lower German shares. The level differences arise largely as a function of geography, with German immigrants having settled predominantly in the large, industrial cities of the Midwest and Northeast and having largely avoided the South (see Panel A of Table 3). Importantly, cities with high German shares do not appear to have been on a different trend in expenditures per pupil for at least 10 years prior to World War I. In 1908, cities with high German shares had expenditures per pupil that were 18 percent higher than cities with low German shares ($85.01 vs. $71.86). Ten years later, in 1918, the difference was 17.5 percent ($75.62 vs. $64.38). However, after 1918 the

[19] For our instrument we do not use a dummy variable for above median German share, but rather use a continuous measure of the German share in a city.

gap between expenditures per pupil in high and low German share cities began to widen. By 1924, the gap had reached 34 percent, double the level in 1918.[20] All cities increased expenditures in the aftermath of World War I (as is obvious from Figure 5), but cities with large German populations differentially increased their expenditures.

Having introduced our instrument, we can estimate the following system of equations using two-stage least squares (2SLS):

$$
\begin{aligned}
&[\ln \widehat{expenditures}\ per\ pupil]_{iec} \\
&\quad = \boldsymbol{X'_{iec}}\boldsymbol{\delta} + \boldsymbol{Y'_{ec}}\boldsymbol{\beta} + \gamma_c + \gamma_e \\
&\quad + \varphi[exposure]_c \times [German\ share_{1910}]_e + \tau_{iec}
\end{aligned} \tag{2}
$$

$$
\begin{aligned}
[outcome]_{iec} = {} & \boldsymbol{X'_{iec}}\boldsymbol{\tau} + \boldsymbol{Y'_{ec}}\boldsymbol{\rho} + \theta_c + \theta_e + \sigma[\ln \widehat{expenditures}\ per\ pupil]_{iec} \\
& + \varepsilon_{iec}
\end{aligned} \tag{3}
$$

In equations (2) and (3), γ_c and θ_c are cohort fixed effects, γ_e and θ_e are city-of-education fixed effects, $\boldsymbol{X'_{iec}}$ are individuals level control variables, and $\boldsymbol{Y'_{ec}}$ are city-of-education by cohort level controls. These controls are the same as those from equation (1). $[exposure]_c$ is cohort *c's* exposure to years of schooling after the United States entered World War I in 1917. Therefore, $[exposure]_c$ is zero for individuals born before 1905, one for individuals born in 1905, two for individuals born in 1906, and takes a maximum value of eight for individuals born after 1911, since all eight years of mandatory schooling would have occurred after the United States entered World War I. Finally, $[German\ share_{1910}]_e$ is the German share of the population in 1910 for city-of-education *e*.

[20] In 1924, expenditures per pupil were $154.29 in high German share cities and $115.29 in low German share cities.

In order for the German share of the population in 1910 interacted with exposure to years of education after World War I to be a suitable instrument for educational expenditures it must satisfy two criteria. First, it must be the case that the presence of Germans specifically prompted increased spending on education around the time of the war, when fears of enemy aliens peaked. Second, it must satisfy the exclusion restriction, which is that the German share of the population in 1910 impacted student outcomes only through its effect on expenditures per pupil.

We explore the first criteria in Panel A of Table 4. All columns in this table control for city and year fixed effects. Column (1) of Table 4 uses the German share of a city's population in 1910, which has been standardized to have a mean of zero and a standard deviation of one. A one standard deviation increase in the German share of a city's population resulted in a 4 percent increase in expenditures per pupil after World War I. Column (2) shows that cities with above median German shares of the population in 1910 increased expenditures per pupil after World War I by 4 percent relative to cities with low German shares. Finally, columns (3) and (4) of Table 4 show that having a high German population, not a high foreign-born population more generally, is what led to the divergence in expenditures per pupil using both dichotomous and continuous measures of non-English-speaking, non-German immigrants.

We now turn to the exclusion restriction. One concern regarding the validity of our instrument is whether cities with high German shares of the population were on systematically different trajectories from cities with lower German shares of the population. For example, if German immigrants had a preference to settle in cities with growing tax bases and increasing expenditures on public schools this would invalidate our argument that city schools responded to the German share of the population by increasing expenditures after World War I. We provide evidence that German immigrants were not simply clustered in cities with better tax bases in Panel

B of Table 4. Specifically, we run a series of regressions on 100 cities for which we obtained non-educational public expenditure data on fire, police, and sewer services.[21] These regressions, shown in Panel B of Table 4, demonstrate that the German share of a city's population does not appear to have significantly impacted non-educational public expenditures after World War I. If anything, such cities spent less on these other public goods. These results strongly suggest that German share is not simply a proxy for a growing post-World War I tax base.

A related concern (and violation of the exclusion restriction) is that cities or regions with more Germans were on different trends on unobservable dimensions that would somehow have led to differing evolutions of educational attainment or wages in the absence of increased educational spending. For instance, it could be the case that areas with more Germans were shifting out of manufacturing and into white-collar work more rapidly. To explore this idea, Panels B through D of Table 3 examine demographic and economic characteristics of cities based on the German share of the population. Panel B shows that cities with high German shares had larger populations and a lower percent black, which is consistent with few Germans settling in the South. Panel C shows that while there are some significant differences in the industrial composition of high and low German cities, these differences are generally small in magnitude (less than three percentage points). Importantly for our identification, Panel D shows that there were no differential trends in the growth of occupations in high versus low German cities between 1910 and 1920. We nevertheless include controls for occupational structure in our baseline specifications. We also show our main results are robust to the inclusion of region time-trends and to dropping the "German Triangle" in Section 5.

[21] These data come from the *Statistics of Cities* and were provided by Elyce Rotella and Louis Cain.

A recent literature has highlighted the importance of the assumptions made about the trends in outcomes absent treatment and the stability in treatment underlying empirical approaches such as ours (for instance, see De Chaisemartin and D'HaultfŒuille, 2017 and Goodman-Bacon, 2018). In our context, we require that cities with high German shares of their population did not have differential trends in our outcome variables prior to the beginning of World War I. To test for these differential trends, we estimate the following equation:

$$[outcome]_{iec} = \boldsymbol{X'_{iec}}\boldsymbol{\delta} + \boldsymbol{Y'_{ec}}\boldsymbol{\beta} + \gamma_c + \gamma_e + \varphi_c \sum_{c=1895}^{1916} \gamma_c \times [High\ German\ share = 1]_e + \tau_{iec} \quad (4)$$

This equation is similar to equation (1), but we replace expenditures per pupil with a series of cohort dummy variables (γ_c; omitting 1894) interacted with a dummy variable if the individual was educated in a high-German-share city. We wish to demonstrate that individuals who were living in high-German-share cities and were completely educated prior to World War I had similar outcomes to individuals living in low-German-share cities.

We plot the coefficients φ_c in Figure 6. Panel A shows the coefficients when educational attainment is the dependent variable. There is no significant difference in educational attainment for individuals living in high German cities who were completely educated prior to World War I (the 1895-1899 birth cohorts). We begin to see an upward trend in educational attainment for individuals educated in high-German cities starting with the 1900 birth cohort. The 1900-1904 birth cohorts could have been in high school during and shortly after World War I, so individuals from high-German-share cities would have experienced some of the rapid, war-induced increase

in expenditures. Finally, because compulsory schooling laws mandated most children to stay in school until at least the age of 14, the 1905 birth cohort and all later cohorts were definitely exposed to some post-World War I education. Accordingly, the upward trend that started in 1900 is more pronounced for these cohorts. Panel B of Figure 6 plots the coefficient estimates when log of weekly wage is the dependent variable. Again, we see no upward trend prior to the 1900 birth cohort.

We also illustrate the trend in school spending by German share in Appendix Figure A.II. Estimated differences in average expenditures per student are essentially flat for cohorts born between 1897 and 1905, after which high German-share cities begin to spend more. We believe that the evidence presented in Table 3 panels B through D, Table 4 panel B, and Figure 6 are consistent with the exclusion restriction holding.

5. Results

5.a. Main results

We begin our analysis by estimating equations (1)-(3) for our four outcomes of interest. Panel A of the Table 5 presents the baseline panel regression estimates. Columns (1), (3), and (5) use the entire sample of individuals that we have years of schooling for, while columns (2), (4), (6), and (7) use the sample of individuals that were wage workers. Consistent with the expectation that the naïve OLS estimation would be biased downwards, nearly all of the panel estimates are small, even when they are significantly different from zero. For example, in column (1) of Panel A, a 10 percent increase in expenditures per pupil during mandatory school-age would result in a 0.0129-year increase in years of schooling, or about 2 additional days of school for a typical 180-day school year.

Panel B of Table 5 reports on the first-stage from our 2SLS regressions. The F-statistic on the first-stage is around 11 in all cases, indicating a sufficiently strong first-stage. Panel C reports the coefficients from the second-stage of our 2SLS approach. Column (1) shows that a 10 percent increase in expenditures per pupil during mandatory school-age years increased educational attainment by 0.18 school years. For a typical 180-day school year, this effect translates into approximately 32 additional days of schooling. In columns (3)-(6) we find that a 10 percent increase in expenditures per pupil increased both the probability of completing eight and twelve years of schooling by about 2 percentage points. Finally, column (7) shows that a 10 percent increase in expenditures per pupil led to a 1.6 percent increase in adult wages.

We next explore the robustness of these results to the inclusion of regional time trends to address the concern that areas with more Germans were evolving differently along unobservable dimensions in terms of wages or educational attainment. Appendix Table A.II reports the results of the baseline specification with the addition of four census region time trends, which for instance allow the "German Triangle" in the Midwest to have a different trend relative to the Northeast. Our estimates for attainment, completing eight years of schooling, and wages are similar but slightly attenuated in the case of attainment. The estimated effect on twelve years of schooling drops by more than half and loses significance. We also rerun the baseline specification on the sample of cities outside the "German Triangle" in Appendix Table A.III and find results similar to the regional time trend specification for educational attainment but a wage effect roughly double the magnitude of the baseline specification. We conclude that the German share instrument may be picking up some regional trends in high school completion. However, it appears unlikely that trends in attainment at lower rungs of the educational ladder or in wages are being driven by general trends across region that are correlated with German share.

To put these results into context, the average student in our sample saw expenditures per pupil increase by 41 percent over his eight mandatory years of schooling. This increase would translate to 0.52 to 0.72 years of additional schooling, depending on the inclusion of regional trends, using the estimates from Tables 5 and Appendix Table A.II. During our study period educational attainment increased by two years from the 1895 cohort to the 1915 cohort, meaning that increased expenditures per pupil can account for between 26 and 36 percent of the increase in educational attainment over our sample period. The same average increase in expenditures translated into wage increases of 6 to 6.5 percent.

We explore whether large or small cities are driving our results in Appendix Table A.IV by dropping individuals educated in cities whose populations were over 250,000 in 1900. The 2SLS estimates are displayed in Panel B and our first-stage is even stronger for this subsample, with F-statistics over 30. The coefficient estimates remain similar to Table 5, with the exception of the coefficient estimate for weekly wages, which is smaller. Only 17 cities in our sample have populations over the 250,000, indicating that our effects are not simply being driven by large cities. In fact, it could be the case that our instrument has more power in this subsample if German immigrant populations were more noticeable in smaller cities.

Individuals born after 1910 would be under the age of 30 in 1940 and may not have achieved their full earnings potential. We explore the robustness of our results to an age restriction in Appendix Table A.V by dropping individuals born after 1910. We find that our results are generally robust to this restriction and our first stage remains sufficiently strong.

It is useful to compare our main results to modern estimates of the impact of school resources on long-run outcomes. One such set of estimates comes from Jackson et al. (2016). They look at children born between 1955 and 1980 and find that a 10 percent increase in expenditures

per pupil resulted in 0.31 more years of completed schooling and 7 percent higher wages. Recall that for a similar 10 percent increase, we find that years of schooling would increase by 0.18 years and weekly wages by about 1.6 percent. Thus, our results are generally smaller in magnitude than Jackson et al. (2016). There could be several reasons for these differences. First, it is possible that the return to school resources has changed over time. Perhaps schooling is more valuable in the modern era due to changes in the occupational and industrial structure of the United States. A second potential source of difference between estimates is that Jackson et al. (2016) instrument for expenditures per pupil with court-ordered school finance reforms, whereas we use the share of the German population in a city prior to World War I.

5.b. Alternative linking methods

Our main results use ABE linking algorithm. However, Bailey et al. (2020) show that automated linking algorithms that use phonetically cleaned names can result in a large number of false positive matches (i.e. linking a child to the wrong adult). The rate of false positives in our sample would have to be systematically related to both the German share of a city's population and the years in which a child was educated for our results to be confounded by false positives (e.g. many false positives for children educated in all places prior to World War I, but few false positives for children educated in high German share cities after World War I). While we believe this is unlikely, we, nevertheless, follow Abramitzky et al. (2021b) and use three more conservative linking techniques to demonstrate the robustness of our results.

First, we restrict our sample to individuals that match *exactly* on first name (not phonetically cleaned), last name (not phonetically cleaned), birthplace, and birth year. We re-estimate our main empirical results (Table 5) with this sample and display the results in Appendix

Table A.VI. The results using only individuals that match exactly on name and birth year are almost identical to our main results. The second method restricts to individuals whose NYSIIS cleaned first and last names are *unique* within a five-year age band. We, again, re-estimate Table 5 using the unique names and display the results in Appendix Table A.VII. The results are, again, almost identical to our main results. Finally, we restrict to individuals that match exactly on name and birth year and that are unique within a five-year age band. The results using this method remain similar and are displayed in Appendix Table A.VIII. It therefore appears unlikely that our results are sensitive to the inclusion of false positives, since more conservative linking approaches – which should result in fewer false positive matches – yield similar results.

5.c. Heterogeneous effects by socioeconomic status and nativity

The early twentieth century was a time of significant inequality. An advantage of our approach is that we can assess the returns to school resources for children from different economic backgrounds. Table 6 shows our results for children whose father had a blue-collar job, which we define as being a craftsman, operative, service worker, or laborer. Table 7 shows the results for children whose father had a white-collar job, which were professionals, managers, proprietors, clerks, or salesmen.

We find that expenditures per pupil had large, positive, and significant effects on all outcomes for the children of low socioeconomic status, blue-collar fathers. In particular, we find that a 10 percent increase in expenditures per pupil would have increased educational attainment by 0.2 school years (approximately 36 days), the probability of completing eight years of schooling by 2.2 percentage points, and weekly wages by 1.8 percent. In comparison, we find that expenditures per pupil had much smaller effects for weekly wages and completing eight years of

schooling for the children of high socioeconomic status, white-collar fathers. Children of both low and high socioeconomic status fathers saw increases in the probability of completing twelve years of schooling, but we caution against interpreting this effect as causal in light of our findings related to regional trends in Section 5. Despite the results, generally, being larger among the children of low socioeconomic status fathers, we do not find evidence that the gap in educational attainment between low and high socioeconomic students closed for the cohorts used in our sample. Figure 7 shows that the gap in educational attainment remained relatively constant at over one year for all cohorts in our sample.

Why are the effects of increased school resources concentrated among the children of lower-skilled workers? One explanation is that the children of professionals were frequently enrolled in private schools and academies in the early twentieth century and, with high parental incomes, would have completed eighth grade regardless of the quality of public schooling in their city. We cannot test for the role of private schools directly; nonetheless, we believe that improved public-school quality would have had a larger scope to impact children who could not afford private education. The difference in estimated effects across children of different socioeconomic statuses also suggests that Progressive Era reformers followed through on their intentions to use increases in public money to improve educational outcomes for working class youths.

Many of the lower-status workers and their children were foreign born. We, therefore, also consider differential impacts by nativity. This question is of particular interest since our instrument uses variation in school resources related to anti-German sentiment. We subdivide our sample by nativity and rerun our analysis in Appendix Table A.IX. Panel A shows the 2SLS results for the native-born population, while Panel B shows the results for the foreign-born population. We find that the effect of increased expenditures on educational attainment were larger for native-born

individuals, while the effect on adult wages were larger for the foreign-born population. Immigration significantly declined during World War I and after the Emergency Quota Act was passed in 1921, so only a relatively small share of our sample (about 3 percent) was foreign born and school aged in the 1920s. Thus, although increases in school resources resulted from concerns about immigrant assimilation, the native-born (including second-generation immigrants) saw broad benefits.

5.d. Mechanisms

We conclude our empirical analysis by considering mechanisms through which expenditures per pupil improved student outcomes. To do this we decompose expenditures into three components: expenditures per pupil on teachers and supervisors, expenditures per pupil on operation expenditures (catch-all category that includes expenditures on clerical work, fuel, heating, textbooks, water, etc.), and expenditures per pupil on capital projects. This decomposition allows us to examine how schools were spending money and, therefore, what elements of expenditures led to improved outcomes.

Table 8 regresses each of these three components on total expenditures per pupil, city fixed effects, and year fixed effects. Each column in this table can be interpreted as the amount each category of spending would increase by from a one dollar increase in per pupil spending (i.e. the coefficients across categories of expenditures should add up to 1). Accordingly, for a one dollar increase in expenditures per pupil, about 15 cents went to teachers, 11 cents went to operations, and 74 cents went to capital projects. The implication of most money going towards capital projects is that city schools' districts were likely constructing new middle and high schools, which might have encouraged students to stay in school beyond sixth grade.

6. Conclusion

This paper documents that World War I was a pivotal moment in the history of educational spending in the United States. In the decade following the conflict, the level of financial support received by urban school districts permanently shifted upward. We provide the first quantitative analysis of the returns to these resources, highlighting several key facts about this historical event. First, overall increases in per pupil spending were generated by cities themselves, not transfers from state governments. Second, while all cities increased spending, urban areas with a larger share of enemy aliens saw proportionally larger growth in school resources. We argue this divergence was related to the assimilation prerogative of cities after the outbreak of World War I and use German share as an instrument for changes in school resources.

As in the present day, using endogenous increases in educational spending leads to estimated returns to school resources that are close to zero. However, using variation arising from the distribution of the German population prior to World War I leads to estimated returns that are statistically significant and economically meaningful. Our results suggest that war-driven increases in spending were an important part of the overall increase in educational attainment and wages across cohorts born at the end of the nineteenth and start of the twentieth century. These benefits were particularly pronounced in children from lower socioeconomic backgrounds. Thus, the public education response to World War I may have played an important role in the midcentury decline in inequality in the United States.

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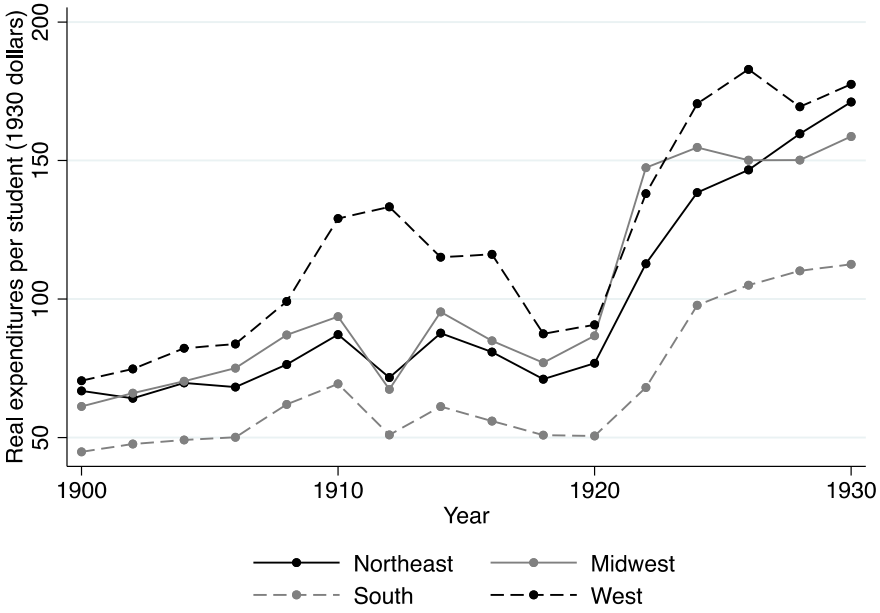
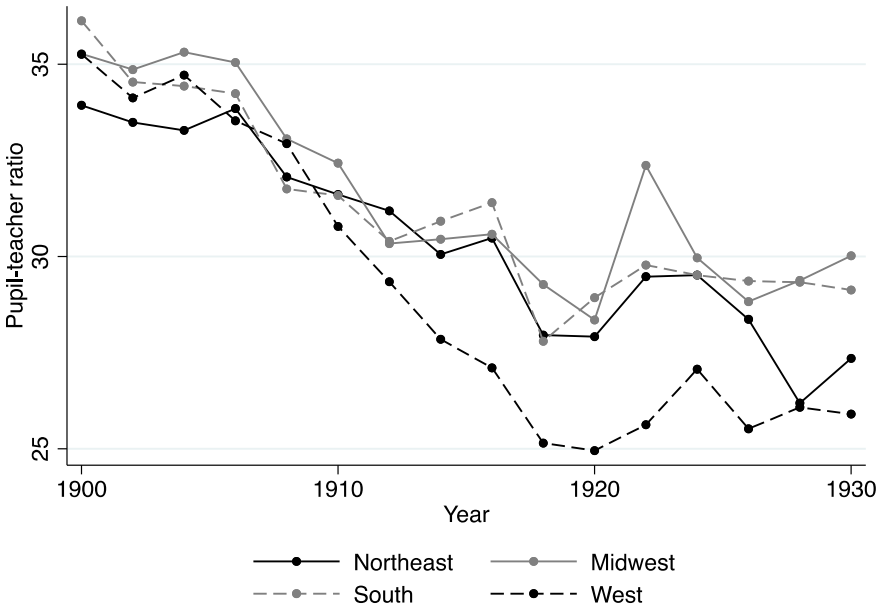
Figure 1: Trend in resources per pupil for sample cities (1900-1930)

Panel A. Real expenditures per pupil

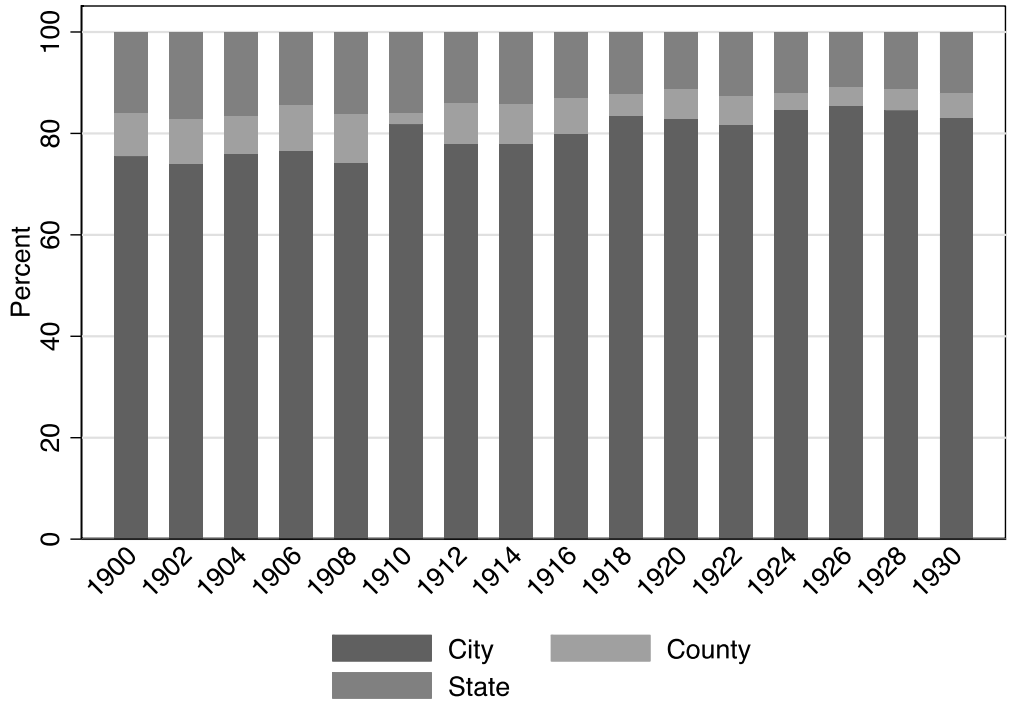


Panel B. Pupil teacher ratio



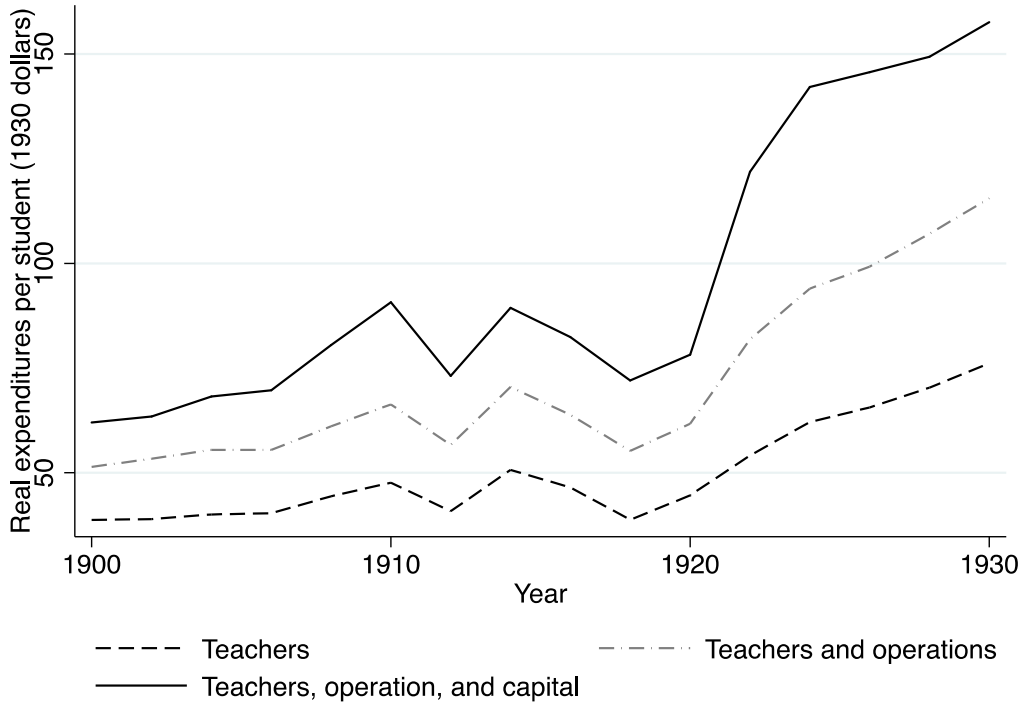
Notes: Data are weighted averages in each census region for our sample of 385 cities. In panel A the data are weighted by the number of pupils in average daily attendance in each city in every academic year. In panel B the data are weighted by the number of teachers in each city in every academic year. Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures divided by the average daily attendance in a city. Real expenditures per student are adjusted using the CPI from Officer and Williamson (2021). The year of each data point corresponds to the calendar year in which the academic year ended (e.g. expenditures per pupil for the 1905-1906 academic year are plotted in 1906). The 1914-1915 academic year is plotted in 1916, since we could not find data for the 1915-1916 academic year.

Figure 2: Sources of revenues for public schools for sample cities (1900-1930) APPENDIX



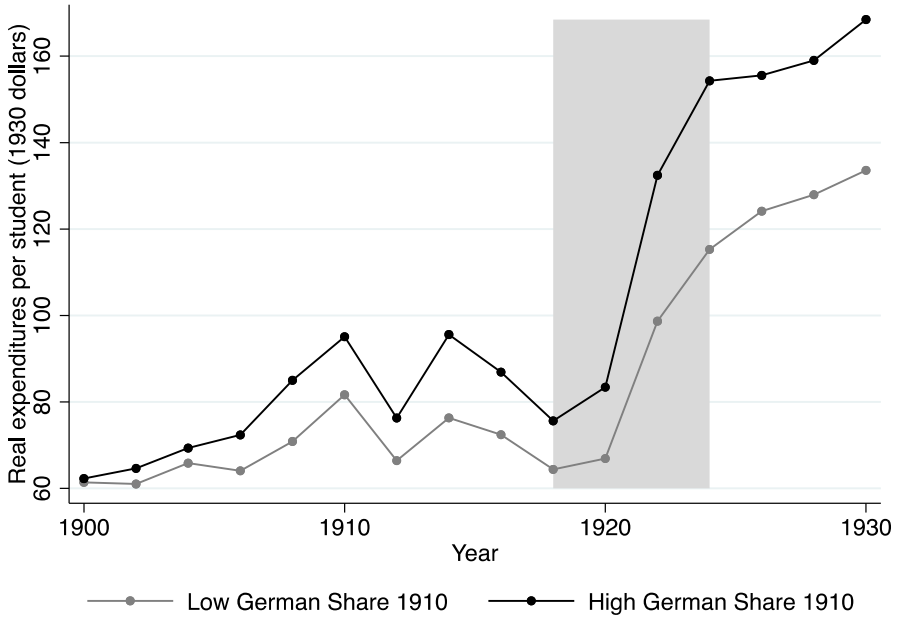
Notes: This graph shows the percentage of city school receipts that come from various levels of government. The year of each data point corresponds to the calendar year in which the academic year ended (e.g. expenditures per pupil for the 1905-1906 academic year are plotted in 1906). The 1914-1915 academic year is plotted in 1916, since we could not find data for the 1915-1916 academic year.

Figure 4. Growth in educational spending by category



Notes: Data are weighted averages for our sample of 385 cities. The data are weighted by the number of pupils in average daily attendance in each city in every academic year. Real expenditures per student are adjusted using the CPI from Officer and Williamson (2021). The year of each data point corresponds to the calendar year in which the academic year ended (e.g. expenditures per pupil for the 1905-1906 academic year are plotted in 1906). The 1914-1915 academic year is plotted in 1916, since we could not find data for the 1915-1916 academic year.

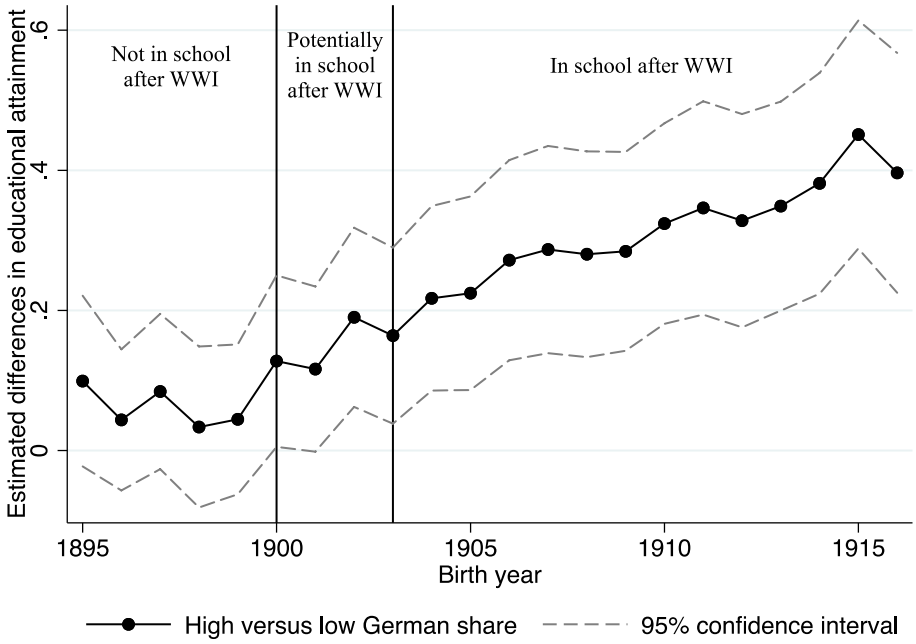
Figure 5: Growth in expenditures per pupil by German share



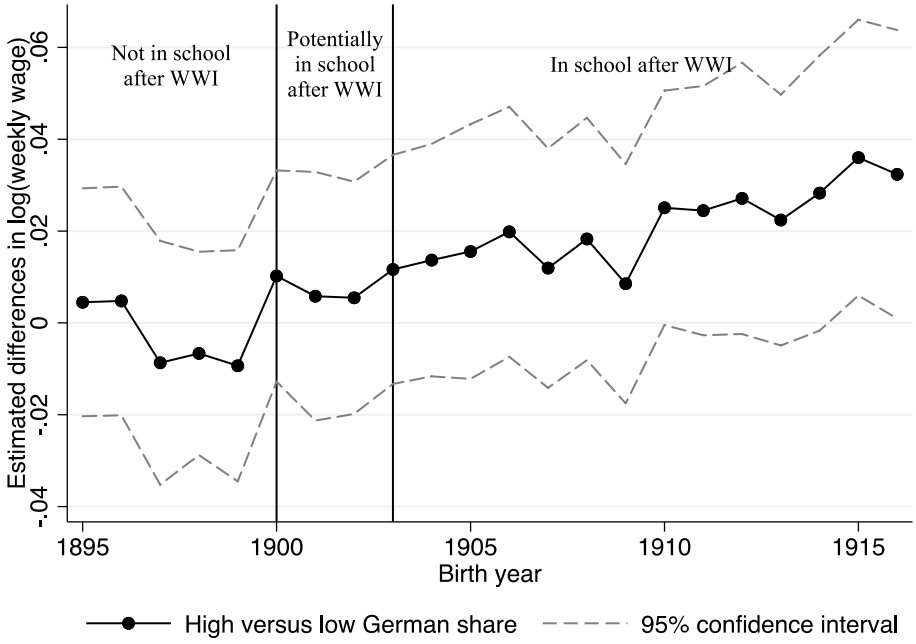
Notes: Data are weighted averages for cities in each group. The data are weighted by the number of pupils in average daily attendance in each city in every academic year. "High" and "low" German share are defined as cities above and below the median German share, which is 2.16 percent of the population.

Figure 6. Estimated differences in outcomes by German share of the city population

Panel A. Educational attainment

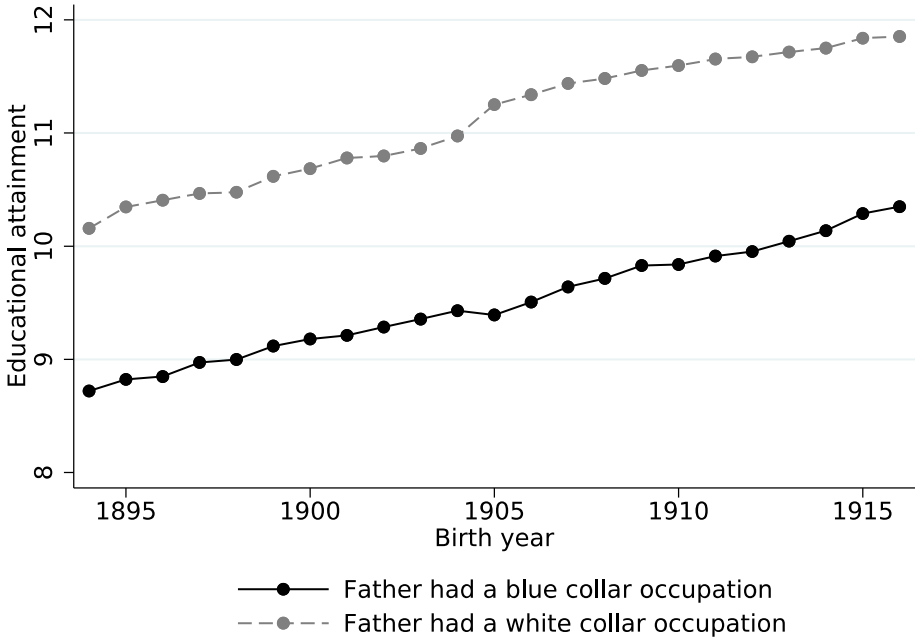


Panel B. Weekly wage



Notes: The unit of observation is an individual. The figure graphs the coefficient estimates from equation (4) in the text. The points are the difference in outcomes between high and low-German-share cities relative to 1894 (the omitted year).

Figure 7. Gap in education attainment in 1940 by father's SES



Notes: The plotted data is the average educational attainment of individuals in our sample whose father had a blue- or white-collar occupation when we observe the child (in the 1900, 1910, 1920, or 1930 census). Blue color occupations are defined as: craftsmen, operators, service workers, or laborers. White collar occupations are defined as: professionals, managers, proprietors, clerks, or salesmen.

Table 1. Impacts of state educational funding laws

	Log(state aid per pupil)	Log(city receipts per pupil)	Log(expenditures per pupil)
	(1)	(2)	(3)
Post WWI * State Law	0.641***	-0.178***	-0.0130
	(0.0657)	(0.0486)	(0.0227)
N	6145	6145	6145
Cities	385	385	385

Notes: The unit of observation is a city-year. The "Post WWI" variable is an indicator variable that takes a value of 1 for the years 1917-1930, and a 0 for the years 1900-1916. "State Law" is an indicator that takes a value of 1 if a city is located in a state that passed a law increasing state aid to schools after World War I. The states that passed these laws are: Arizona, California, Georgia, Iowa, Louisiana, Massachusetts, New York, North Carolina, Pennsylvania, South Carolina, Texas, Utah, Washington, and West Virginia. State aid per pupil, city receipts per pupil, and expenditures per pupil are interpolated between two adjacent academic years when it is not reported for a city. We do not extrapolate state aid per pupil in cities for which it was not available in the 1899-1900 academic year because the extrapolation sometimes results in negative values. Accordingly, we have 6,145 observations rather than a completely balanced panel which would have 6,160 observations (385 cities across 16 academic years). All regressions control for city and year fixed effects. Standard errors are clustered at the city-level and reported in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 2. Summary statistics

Panel A: City-level summary statistics				
Academic Year:	1899-1900	1909-1910	1919-1920	1929-1930
Teacher and supervisor expenditures per pupil (1930)	33.63	42.78	39.22	66.52
Capital and debt expenditures per pupil (1930)	9.73	19.69	15	36.68
Other expenditures per pupil (1930)	12.18	17.57	16.52	36.72
Total expenditures per pupil (1930)	55.54	80.05	70.73	139.92
Pupil-teacher ratio	34.53	30.86	27.86	28.05
School revenues from city per pupil (1930)	36.92	50.7	50.63	95.4
School revenues from state per pupil (1930)	8.97	11.76	7.02	14.66
School revenues from county per pupil (1930)	5.38	2.14	3.39	4.48
Observations	385	385	385	385
Panel B: Individual-level summary statistics				
Census:	1900	1910	1920	1930
Educational attainment	9.15	9.54	10.34	10.73
8 years of educational attainment	0.78	0.81	0.88	0.91
12 years of educational attainment	0.28	0.32	0.42	0.5
Weekly wage (1940) †	$41.42	$39.11	$30.92	$23.74
Real per pupil spending (average ages 6-14)	$68.79	$80.96	$100.33	$144.13
Years of post-WWI schooling	0	0	5.35	8
Age (1940)	45.82	40.35	30.44	24.51
Mother present?	0.97	0.95	0.96	0.96
Mother literate if present?	0.92	0.94	0.93	0.95
Father present?	0.94	0.9	0.91	0.91
Father literate if present?	0.95	0.96	0.95	0.96
High SES HH	0.18	0.25	0.19	0.15
Low SES HH	0.55	0.59	0.46	0.41
Could not determine SES	0.27	0.16	0.35	0.44
Observations	26,560	716,696	958,603	242,957

Notes: Data in Panel A are from the *Report of the Commissioner of Education* (1900-1916) and the *Biennial Survey of Education* (1918-1930). Monetary values in Panel A are in 1930 dollars, which were adjusted using the CPI from Officer and Williamson (2021). Data in Panel B are from the linked census sample.

†: There are a different number of observations for weekly wages. The number of observations for each census year are as follows: 14,752 (1900); 431,495 (1910); 625,878 (1920); 147,288 (1930).

Table 3. Difference in characteristics of cities by German share

	Below median German share		Above median German share		Equality of means
	Mean	Std dev.	Mean	Std dev.	p-value
Panel A: Region cities are located in					
In Northeast	111		70		
In Midwest	33		94		
In South	45		9		
In West	3		20		
Total	192		193		
Panel B: Demographics in 1910					
Share German	0.0099	(0.0061)	0.0505	(0.0309)	0.000***
Share Irish	0.0259	(0.0285)	0.0211	(0.0220)	0.067*
Share Italian	0.0155	(0.0225)	0.0193	(0.0236)	0.103
Share Russian	0.0171	(0.0330)	0.0209	(0.0237)	0.195
Share Foreign Born	0.1752	(0.1310)	0.2181	(0.0986)	0.003***
Share Black	0.0909	(0.1422)	0.0249	(0.0380)	0.000***
Average population in 1910	40,208	(65,276)	91,210	(215,815)	0.002***
Panel C: Share of county population in 1910 employed as:					
White collar	0.1948	(0.0510)	0.2125	(0.0522)	0.001***
Farmer	0.1017	(0.0825)	0.0956	(0.0717)	0.445
Craftsman	0.1409	(0.0396)	0.1576	(0.0394)	0.000***
Operatives	0.1993	(0.1239)	0.1685	(0.0989)	0.007***
Service	0.1134	(0.0513)	0.0978	(0.0274)	0.002***
Laborers	0.2061	(0.0731)	0.2174	(0.0626)	0.105
Panel D: Change in share of county population (1910-1920) employed as:					
White collar	0.0429	(0.0240)	0.0438	(0.0233)	0.719
Farmer	-0.0039	(0.0211)	0.0000	(0.0240)	0.085*
Craftsman	0.0181	(0.0244)	0.0194	(0.0247)	0.595
Operatives	0.0096	(0.0252)	0.0103	(0.0259)	0.794
Service	-0.0239	(0.0166)	-0.0217	(0.0117)	0.143
Laborers	-0.0258	(0.0328)	-0.0301	(0.0290)	0.176
Observations	192		193		

Notes: This table shows average characteristics in 1910 for the 385 cities in our sample. There are 192 cities in the "Below median German share" group and 193 cities in the "Above median German share" group. White collar workers are defined as professional, managers, officials, proprietors, clerks, and sales workers. The final column provides the p-value from a test for the equality of means between low and high German share cities. * p<0.1, ** p<0.05, *** p<0.01.

Table 4. Validity of German share instrument

Panel A: Educational expenditures	Log(real expenditures per pupil, 1930 dollars)			
	(1)	(2)	(3)	(4)
Post WWI*German share (1910)	0.0422***			
	(0.0109)			
Post WWI*High German share (1910)		0.0422*		
		(0.0227)		
Post WWI*Non-English speaking, non-German share (1910)			0.00122	
			(0.0120)	
Post WWI*High non-English speaking, non-German share (1910)				0.00968
				(0.0228)
N	6160	6160	6160	6160
Cities	385	385	385	385

Panel B: Non-educational public expenditures						
Dependent variable:	Log(exp. on fire)		Log(exp. on police)		Log(exp. on sewer)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post WWI*German share (1910)	-0.0322		0.00698		0.0659	
	(0.0313)		(0.0245)		(0.0512)	
Post WWI*High German share (1910)		-0.0831		-0.0161		-0.00393
		(0.0832)		(0.0794)		(0.127)
N	1600	1600	1600	1600	1600	1600
Cities	100	100	100	100	100	100

Notes: The unit of observation is a city-year. The "Post WWI" variable is an indicator that takes a value of 1 for the years 1917-1930, and a 0 for the years 1900-1916. "German share (1910)" is the share of a city's population that was an immigrant from Germany in 1910. "Non-English speaking, non-German share (1910)" is the share of a city's population that were immigrants and not from Canada, England, Germany, Ireland, Northern Ireland, Scotland, and Wales. Both measures are standardized to have a mean of zero and standard deviation of one. "High German share (1910)" is an indicator if a city had above median German share of the population in 1910. "High non-English speaking, non-German share (1910)" is an indicator if a city had an above median non-English speaking, non-German share of the population in 1910. Finally, Expenditures per student are interpolated between two adjacent academic years when it is not reported for a city. All regressions control for city fixed effects and year fixed effects. Spending on fire, police, and sewer services were provided by Elyce Rotella and Louis Cain. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 5. OLS and 2SLS estimates of expenditures per pupil on adult outcomes

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Panel estimates							
Log(expenditures per pupil)	0.129**	0.168***	0.0181***	0.0205***	-0.000520	0.00350	0.0171*
	(0.0509)	(0.0526)	(0.00572)	(0.00611)	(0.00880)	(0.00994)	(0.00971)
Panel B: First-stage estimates; dependent variable is Log(expenditures per pupil)							
Post WWI exposure*German share (1910)	0.24***	0.24***	0.24***	0.24***	0.24***	0.24***	0.24***
	(0.071)	(0.073)	(0.071)	(0.073)	(0.071)	(0.073)	(0.073)
First stage F-statistic	11.39	10.77	11.39	10.77	11.39	10.77	10.77
Panel C: 2SLS estimates							
Log(expenditures per pupil)	1.761***	1.812***	0.188***	0.171**	0.182**	0.201**	0.161**
	(0.516)	(0.516)	(0.0689)	(0.0664)	(0.0729)	(0.0798)	(0.0733)
Wage-workers		X		X		X	X
N	1944816	1219413	1944816	1219413	1944816	1219413	1219413
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1), Panel B provides estimates of equation (2), and Panel C provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 6. Effect of school resources on adult outcomes for sons of low SES fathers

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS estimates for sons of fathers that had a blue-collar occupation (craftsman, operator, service worker, or laborer)							
Log(expenditures per pupil)	0.169***	0.172***	0.0223***	0.0226***	0.000171	0.000735	0.0149
	(0.0553)	(0.0558)	(0.00667)	(0.00726)	(0.00996)	(0.0109)	(0.0112)
Panel B: 2SLS estimates for sons of fathers that had a blue-collar occupation (craftsman, operator, service worker, or laborer)							
Log(expenditures per pupil)	1.956***	2.047***	0.221***	0.208***	0.180**	0.198**	0.184**
	(0.530)	(0.526)	(0.0788)	(0.0770)	(0.0726)	(0.0798)	(0.0830)
Wage-workers		X		X		X	X
First stage F statistic	12.17	11.43	12.17	11.43	12.17	11.43	11.43
N	977422	619460	977422	619460	977422	619460	619460
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 7. Effect of school resources on adult outcomes for sons of high SES fathers

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS estimates for sons of fathers that had a white-collar occupation (professional, manager, proprietor, clerk, or salesman)							
Log(expenditures per pupil)	0.0897	0.116*	0.00901*	0.00667	0.00601	0.0110	0.00516
	(0.0591)	(0.0627)	(0.00502)	(0.00626)	(0.00949)	(0.0115)	(0.0123)
Panel B: 2SLS estimates for sons of fathers that had a white-collar occupation (professional, manager, proprietor, clerk, or salesman)							
Log(expenditures per pupil)	1.802***	1.799***	0.0805**	0.0542*	0.253***	0.287***	0.0742
	(0.668)	(0.653)	(0.0338)	(0.0289)	(0.0947)	(0.0992)	(0.0635)
Wage-workers		X		X		X	X
First stage F statistic	11.01	10.79	11.01	10.79	11.01	10.79	10.79
N	401240	244933	401240	244933	401240	244933	244933
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 8. Decomposition of expenditures per pupil

Dependent variable:	Teacher and supervisor expenditures per pupil	Operation expenditures per pupil	Capital expenditures per pupil
	(1)	(2)	(3)
Expenditures per pupil	0.149***	0.111***	0.733***
	(0.0181)	(0.0103)	(0.0205)
N	6160	6160	6160
Cities	385	385	385

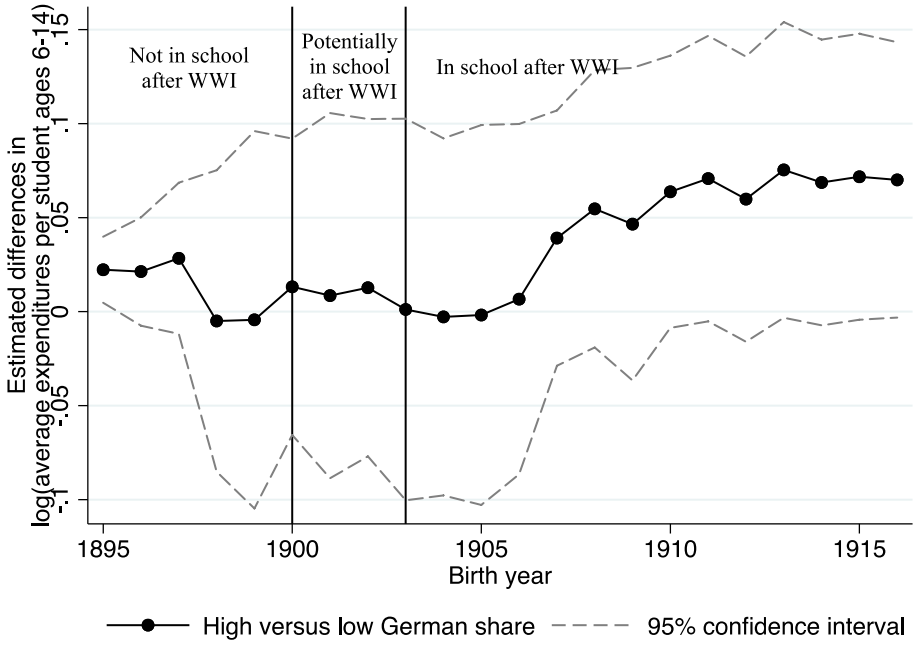
Notes: The unit of observation is a city-year. Expenditures per pupil is total expenditures per pupil. We decompose this aggregate measure into three constitute parts: teachers and supervisors expenditures per pupil, operation expenditures per pupil (catch-all category that includes expenditures on clerical work, fuel, heating, textbooks, water, etc.), and capital expenditures per pupil. All regressions control for city fixed effects and year fixed effects. Standard errors are clustered at the city-level and reported in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix

Figure A.I. Geographical Distribution of Sample Cities

Notes: This figure shows the location of the 385 cities in our sample.

Figure A.II. Estimated differences in school spending by city German share



Notes: The unit of observation is an individual. The figure graphs the coefficient estimates from equation (4) in the text and uses log(expenditures per pupil) as the dependent variable. The points are the difference in outcomes between high and low-German-share cities relative to 1894 (the omitted year).

Table A.I. Matching Representativeness

Census Year:	1900		1910		1920		1930	
	Linked Sample	Attempted to Link	Linked Sample	Attempted to Link	Linked Sample	Attempted to Link	Linked Sample	Attempted to Link
Personal characteristics:								
Mean age	10.01	10.06	10.42	10.44	10.32	10.3	10.37	10.43
Median age	10	10	10	10	10	10	10	10
Literate	0.95	0.95	0.99	0.99	0.99	0.99	0.99	0.99
In school	0.9	0.89	0.92	0.91	0.9	0.88	0.93	0.92
Household and family characteristics:								
In urban area	0.97	0.97	0.98	0.98	0.98	0.98	0.98	0.98
Home owned	0.34	0.31	0.37	0.33	0.4	0.36	0.36	0.34
Mother present	0.96	0.95	0.95	0.94	0.96	0.95	0.97	0.97
Father present	0.9	0.88	0.89	0.88	0.9	0.9	0.93	0.93
Mother literate if present	0.92	0.89	0.92	0.89	0.92	0.88	0.95	0.94
Father literate if present	0.94	0.92	0.95	0.93	0.94	0.92	0.97	0.96
Father OCC score if present	21.93	21.76	27.75	27.25	20.77	20.45	24.76	24.61
Observations	585,406	1,948,639	850,943	2,554,211	1,131,162	3,207,363	1,521,739	3,917,714

Notes: This table reports averages for a number of characteristics for individuals that we linked and that we attempted to link. We successfully linked more individuals than we use in our empirical specifications. There are multiple reasons for this discrepancy. First, we linked individuals from the 1885-1894 birth cohorts from the 1900 to the 1940 census. However, of these linked individuals we only use the ones from the 1894 birth cohort in the empirical specifications because they are the only ones for which we have a measure of expenditures per pupil during all mandatory school-age years. Similarly, we linked individuals from the 1915-1924 birth cohorts from the 1930 to the 1940 census. However, of these linked individuals we only use the ones from the 1915 and 1916 birth cohorts in the empirical specifications because they are the only ones for which we have a measure of expenditures per pupil during all mandatory school-age years. Finally, some individuals do not report an educational attainment and we do not include individuals that were educated in New York City in our empirical analysis. The census question on literacy only applied to persons 10+ years of age. Father's occupational score is included if the father is present, and an occupational score is given.

Table A.II. Estimates of expenditures per pupil on adult outcomes with region-specific time trends

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS estimates with region-specific time trends							
Log(expenditures per pupil)	0.151***	0.192***	0.0137***	0.0159***	0.00837	0.0128	0.0210**
	(0.0383)	(0.0448)	(0.00503)	(0.00538)	(0.00688)	(0.00847)	(0.00890)
Panel B: 2SLS estimates with region-specific time trends							
Log(expenditures per pupil)	1.260**	1.243**	0.166**	0.160**	0.0785	0.0802	0.147**
	(0.502)	(0.497)	(0.0736)	(0.0740)	(0.0581)	(0.0624)	(0.0678)
Wage-workers		X		X		X	X
First stage F statistic	9.67	9.11	9.67	9.11	9.67	9.11	9.11
N	1944816	1219413	1944816	1219413	1944816	1219413	1219413
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, the average percent of a city's population that works in various occupations, and region-specific time trends. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.III. Effect of school resources on adult outcomes – dropping "German Triangle" cities

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(expenditures per pupil)	1.240*	1.371*	0.137*	0.164**	0.0535	0.0472	0.323***
	(0.703)	(0.778)	(0.0768)	(0.0748)	(0.0675)	(0.0788)	(0.0852)
First stage F-statistics	46.75	46.21	46.75	46.21	46.75	46.21	46.21
Wage workers		X		X		X	X
N	52227	31045	52227	31045	52227	31045	31045
Cities	370	357	370	357	370	357	357

Notes: The unit of observation is an individual. Panels A and B provide estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.IV. Estimates of expenditures per pupil on adult outcomes for cities with population below 250,000 in 1900

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS estimates for cities under 250,000 in 1900							
Log(expenditures per pupil)	0.0736	0.0971*	0.0127***	0.0149***	-0.000403	0.00222	0.0129*
	(0.0529)	(0.0584)	(0.00416)	(0.00454)	(0.00987)	(0.0111)	(0.00753)
Panel B: 2SLS estimates for cities under 250,000 in 1900							
Log(expenditures per pupil)	1.540***	1.576***	0.127***	0.106***	0.254***	0.277***	0.0719*
	(0.410)	(0.447)	(0.0342)	(0.0293)	(0.0736)	(0.0843)	(0.0401)
Wage-workers		X		X		X	X
First stage F statistic	30.17	30.26	30.17	30.26	30.17	30.26	30.26
N	1191590	741549	1191590	741549	1191590	741549	741549
Cities	368	368	368	368	368	368	368

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.V. Estimates of expenditures per pupil on adult outcomes dropping younger birth cohorts

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS estimates for 1894-1910 birth cohorts							
Log(expenditures per pupil)	0.0940**	0.118**	0.00385	0.00357	0.00867	0.0124	0.0160*
	(0.0474)	(0.0541)	(0.00451)	(0.00509)	(0.00753)	(0.00899)	(0.00862)
Panel B: 2SLS estimates for 1894-1910 birth cohorts							
Log(expenditures per pupil)	1.439***	1.366***	0.193***	0.184**	0.131**	0.130**	0.168**
	(0.445)	(0.448)	(0.0706)	(0.0720)	(0.0526)	(0.0558)	(0.0657)
Wage-workers		X		X		X	X
First stage F statistic	12.24	11.71	12.24	11.71	12.24	11.71	11.71
N	1296166	807353	1296166	807353	1296166	807353	807353
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.VI. Estimates of expenditures per pupil on adult outcomes using exact name and age matching

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Panel estimates							
Log(expenditures per pupil)	0.148**	0.179***	0.0162**	0.0162**	0.00498	0.00604	0.0148
	(0.0604)	(0.0597)	(0.00713)	(0.00713)	(0.0100)	(0.0113)	(0.0101)
Panel B: 2SLS estimates							
Log(expenditures per pupil)	1.869***	1.993***	0.181**	0.168**	0.191**	0.224**	0.176**
	(0.609)	(0.639)	(0.0765)	(0.0760)	(0.0798)	(0.0887)	(0.0746)
Wage-workers		X		X		X	X
First stage F statistic	10.79	10.25	10.79	10.25	10.79	10.25	10.25
N	840821	545519	840821	840821	840821	545519	545519
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.VII. Estimates of expenditures per pupil on adult outcomes using unique names

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Panel estimates							
Log(expenditures per pupil)	0.144***	0.191***	0.0214***	0.0214***	0.00101	0.00529	0.0190*
	(0.0510)	(0.0548)	(0.00554)	(0.00554)	(0.00907)	(0.0106)	(0.0101)
Panel B: 2SLS estimates							
Log(expenditures per pupil)	1.664***	1.820***	0.197**	0.177**	0.178**	0.212**	0.159*
	(0.575)	(0.603)	(0.0828)	(0.0784)	(0.0819)	(0.0951)	(0.0826)
Wage-workers		X		X		X	X
First stage F statistic	11.34	10.54	11.34	10.54	11.34	10.54	10.54
N	855557	541713	855557	541713	855557	541713	541713
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.VIII. Estimates of expenditures per pupil on adult outcomes using exact and unique names and ages

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Panel estimates							
Log(expenditures per pupil)	0.161**	0.194***	0.0190**	0.0190**	0.00310	0.00185	0.0190
	(0.0637)	(0.0686)	(0.00736)	(0.00736)	(0.0105)	(0.0121)	(0.0124)
Panel B: 2SLS estimates							
Log(expenditures per pupil)	1.932***	2.186***	0.208**	0.191**	0.198**	0.247**	0.174*
	(0.742)	(0.778)	(0.0981)	(0.0949)	(0.0942)	(0.105)	(0.0913)
Wage-workers		X		X		X	X
First stage F statistic	10.67	10	10.67	10	10.67	10	10
N	376316	245355	376316	245355	376316	245355	245355
Cities	385	385	385	385	385	385	385

Notes: The unit of observation is an individual. Panel A provides estimates of equation (1) and Panel B provides estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.IX. Effect of school resources on adult outcomes by nativity

Dependent variable:	Educational attainment		Pr(8 years attainment = 1)		Pr(12 years attainment = 1)		Log(weekly wage)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 2SLS estimates for native population							
Log(expenditures per pupil)	1.790***	1.853***	0.185***	0.168**	0.192**	0.212**	0.153**
	(0.539)	(0.535)	(0.0713)	(0.0690)	(0.0762)	(0.0827)	(0.0747)
First stage F-statistics	11.16	10.56	11.16	10.56	11.16	10.56	10.56
Wage workers		X		X		X	X
N	1880276	1180403	1880276	1180403	1880276	1180403	1180403
Cities	385	385	385	385	385	385	385
Panel B: 2SLS estimates for immigrant population							
Log(expenditures per pupil)	1.106*	1.180	0.157**	0.183***	0.0417	0.0410	0.232***
	(0.620)	(0.716)	(0.0632)	(0.0661)	(0.0663)	(0.0750)	(0.0751)
Wage workers		X		X		X	X
First stage F-statistics	22.26	20.18	22.26	20.18	22.26	20.18	20.18
N	64482	38946	64482	38946	64482	38946	38946
Cities	373	364	373	364	373	364	364

Notes: The unit of observation is an individual. Panels A and B provide estimates of equation (3). The treatment variable Log(expenditures per pupil) is average per pupil spending (in real 1930 dollars) during school-age years (ages 6-14). Expenditures per pupil is the sum of expenditures on teachers, supervisors, capital, and other expenditures all divided by the average daily attendance in a city-year cell. All regressions control for: city of education fixed effects, birth year fixed effects, mother's literacy (dummy variables for mother literate, mother illiterate, and mother not present), father's literacy (dummy variables for father literate, father illiterate, and father not present), mother's occupation (dummies), father's occupation (dummies), the average number of mandatory school years, and the average percent of a city's population that works in various occupations. The average number of mandatory school years, which we control for, are defined for each birth cohort based on state compulsory schooling laws. In particular, we subtract the work permit age from the entry age for states that had work permit laws. For states without work permit laws, we subtract the exit age from the entry age. Data on entry, exit, and work permit age are taken from Goldin and Katz (2008a). We control for the average percent of a city's population working in the following occupations: white collar (professional, manager, clerk, or salesmen), craftsman, operator, service worker, laborer, or farmer. Non-occupational responses are the omitted category. These percentages were constructed from the 1900-1930 IPUMS (Ruggles et al., 2021) complete count data and are interpolated in-between census years. Standard errors are clustered at the city-level and reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.