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André Martínez Fritscher
Banco de México

Aldo Musacchio
Harvard Business School

Martina Viarengo
London School of Economics

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The Great Leap Forward: The Political Economy of Education in Brazil, 1889-1930*

André Martínez Fritscher†
Banco de México

Aldo Musacchio‡
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Martina Viarengo§
London School of Economics

Abstract: Recent research links the inequality across countries and regions to colonial institutions. This paper argues that trade shocks could alter the development path of a country or subnational units, in spite of its colonial institutions. This hypothesis is analyzed using state-level data for Brazil, a country with high regional heterogeneity in endowments. We find that positive trade shocks, or improvements in export tax revenues, increased expenditures on education per capita and education outcomes in the period 1889 to 1930. In fact, trade shocks ended up altering the inequality in education levels across states in a permanent way. The paper ends by explaining why politicians spent windfall tax revenues to invest on education.
Keywords: Institutions; Fiscal Federalism; Education; Long Run Development.
JEL Classification: I20; H41; H75; N26; N36; N46; N96.

Resumen: Investigación reciente relaciona la desigualdad actual entre países y regiones con las instituciones coloniales. Este artículo argumenta que choques comerciales pudieron alterar la trayectoria de desarrollo de los países o de unidades subnacionales, a pesar de sus instituciones coloniales. Esta hipótesis es analizada utilizando datos a nivel estatal para Brasil, un país con gran heterogeneidad regional en dotaciones inciales de factores. Nosotros encontramos que choques comerciales positivos, o aumentos en los ingresos por concepto de impuestos a la exportación, incrementaron los gastos y resultados en educación en los estados brasileños entre 1889 y 1930. De hecho, dichos choques alteraron la desigualdad educativa entre estados de una manera permanente. El artículo finaliza explicando porque los políticos utilizaron estos ingresos fiscales para invertir en educación.
Palabras Clave: Instituciones; Federalismo Fiscal; Educación; Desarrollo de Largo Plazo.

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† Dirección General de Investigación Económica. Email: amartinez@banxico.org.mx.
‡ Harvard Business School. Email: amusacchio@hbs.edu.
§ London School of Economics. Email: m.g.viarengo@lse.ac.uk.

I. Introduction

Recent research links the inequality we observe today across former colonies, and even within regions in former colonies, to colonial institutions (Acemoglu, Johnson, and Robinson, 2001; Engerman and Sokoloff, 1997; 2002; Bruhn and Gallego, 2007). According to this literature endowments and the conditions at the time of colonization determined a set of political institutions that ended up perpetuating an unequal distribution of land, wealth, and political power. In fact, the variation in colonial institutions has been identified as a cause of heterogeneity in expenditures on public goods per capita, such as education, both across countries (Engerman, Mariscal, and Sokoloff, 2009; Gallego, 2010) or within countries (Banerjee and Iyer, 2005; Iyer, 2010; Wegenast, 2009).

Yet much of the literature on colonial institutions has focused on finding persistent effects using reduced form estimates and very little research has been done to study how some countries or subnational units broke away from their colonial past and changed their development trajectories. For instance, we know that in the nineteenth century former colonies in what is now Latin America experienced a radical reversal of fortune for the worse (Acemoglu, Johnson, and Robinson, 2002). There is also evidence that trade shocks in the nineteenth century increased inequality within countries in the Americas (Williamson, 2009).

Still, we do not know much of how trade shocks can actually improve institutions in former colonies. Moreover, as the development literature acknowledges, economic growth and development outcomes are a complex set of interactions between policies and institutions (Rodrik and Rosenzweig, 2010). Thus, we look at the variation over time in the provision of public elementary education, holding constant colonial institutions, either through fixed effects or by holding constant variables common to all states (e.g., identity of the colonizer, religion, or legal origin). By focusing on variation over time, rather than just on path dependence since colonial times, our findings contrast with the growing literature on colonial institutions in Brazil (Naritomi, Soares and Assunção, 2007; Wegenast, 2009; Summerhill, 2010; de Carvalho and Colistete, 2010).

We do not think that looking at path-dependence in our case gives us much mileage because if education outcomes were a consequence of colonial institutions more than any of the dynamics we show in this paper, we would expect to find that original distribution of human

capital across states should not change that much over time. For instance, we would expect to find that measures of literacy in 1872 (the year of the first census) were highly correlated with measures of literacy in the twentieth century and we would not expect to find radical reversals of fortune during our period of study (1889-1930). The evidence we have, however, documents a reversal of fortune among states in our period. Literacy rates across states in 1872 are not correlated strongly with literacy rates in the second half of the twentieth century, while literacy rates after 1900 are highly correlated with literacy rates in 1991 or 2007 (see Table 1). Our evidence, therefore, suggests that something altered the relative inequality among states between 1890 and 1930 that then had persistent effects in the second half of the twentieth century.

We show that one of the main drivers of change in relative human capital accumulation across states between 1891 and 1930 was the effect of the commodity boom of the late nineteenth century and the fact that Brazil adopted an extremely decentralized fiscal system in the Constitution of 1891. The Constitution of 1891 divided Brazil into 20 states with very autonomous spending powers and with the sole right to collect export taxes. The fact that states governments could tax commodity exports allowed the governments of provinces with positive shocks in the terms of trade to collect higher revenues per capita and spend more on education. In contrast, those states that had negative shocks in their terms of trade collected lower revenues and lagged behind in terms of expenditures in things like education, infrastructure, and police.

We look at this as a quasi-experiment because Brazil is a country large enough to have significant heterogeneity in endowments and colonial institutions across provinces (e.g., variation in climate, soil types, or the extent to which plantation agriculture was used by settlers). Moreover, between 1891 and 1930, The Constitution of 1891 gave states almost total autonomy when it came to tax collection, even giving them the right to tax exports. We, therefore, feel we can treat states as independent observations given that during this period the federal government did not do a significant effort to redistribute among states and immigration within the country was minimal.

Using both OLS and IV techniques (and controlling for a series of macro variables, fixed effects, and time dummies) we find that both changes in export tax revenues or simply the

change in the terms of trade correlated positively with education expenditures per capita. We also run regressions in which we interact variables that measure colonial institutions (time-invariant) with our variable of interest, export tax revenue per capita, and find no significant correlation with expenditures on education.

We show that the boom in certain commodities allowed Brazilian states to increase their revenues and, in turn, their expenditures on public goods, such as education. Between 1889 and 1930, and despite bad colonial institutions, Brazil as a whole had the largest increase in literacy rates in Latin America, going from 19.8% in 1890 to 40% in 1940 (for the population over 4 years of age). This improvement, however, was uneven, with some states such as São Paulo improving their literacy rate from 18.8% to 52%, while others like Maranhão, Mato Grosso, and Bahia kept their rate flat at 20% during the same period. In that sense, Brazil may have represented a second-best environment for education reform and policy implementation (Rodrik, 2008).

We devote the last section of the paper to study the political economy of education expenditures. We follow Lindert (2003, 2004) and show that political voice (the percentage of the population who could vote) is correlated with expenditures on education. Yet, instead of looking at a one-way causality, from voters to expenditures, we think that there is possible reverse causality. As politicians spent on education, literacy rates increased, and consequently, as only literate male adults could vote, these increases in literacy led to an increase in the number of voters. We attribute the improvements in the supply of education to the fact that during the period 1889 to 1930, the electoral law of Brazil provided incentives for politicians to use windfall export tax profits to spend on education, more than on any other "normal" public good (e.g., healthcare). Since there was a national literacy requirement to vote and state politicians had incentives to maximize the number of voters they could mobilize in federal elections, increasing literacy rates was necessary to increase the number of voters.

We provide econometric evidence that exhibits a positive correlation between expenditures on education and an increase in the number of voters over time. That means that states with positive trade shocks were able to spend more on education and increased the number of literate males who could vote. This finding is at odds with the idea that in countries with literacy requirements political elites have no incentives to increase the supply of public

education that can benefit the masses (Engerman, Mariscal and Sokoloff, 2009; Lindert, 2003, 2004). Yet we find that the expansion of public education benefitted disproportionately white Brazilians.

The paper is organized as follows. In Section II we describe the changes in education in Brazil between independence and 1930. Section III shows how commodity prices determined the changes in expenditures on education and in education indicators. Section IV discusses the incentives of political elites to spend on education. Section V concludes by discussing some of the long-term implications of education policy between 1889 and 1930.

II. The Evolution of Education in Brazil from Independence to 1930

A newly independent Brazil adopted, in 1821, a constitutional monarchy with a clear division of power and centralized taxation. During the imperial period (1821-1889), executive power rested with the emperor and council of ministers and an elected parliament was responsible for legislative tasks. Parliamentarians (senators and deputies) were elected by state electoral colleges. Electoral participation was restricted by an income requirement, which was a year's income for most skilled professions.[1] Provincial governments were weak and had little control over fiscal revenues under this political arrangement, and most of the revenues collected by the central government were spent in the capital.

Despite the centralization of taxation and expenditures, the members of congress that drafted the Constitution of 1824 chose to decentralize the provision of education. Therefore, from 1824 on, the imperial government focused mostly on providing education in the capital of the country and subsidizing a couple of universities around the country, while the provincial governments were in charge of elementary and secondary education in their own territories (Hilsdorf, 2003).

[1] The process was, in fact, even more complex because Brazil had a system of indirect elections. That is, voters in parishes (known as *eleitores*) would vote to elect an electoral college similar to that of the United States. The members of this electoral college were known as *votantes* (voters). The Constitution of 1824 included income requirements for both, eleitores and votantes. For the former it was 100$ per year (or approximately US $60), while the latter needed to prove an income of $200. There were exceptions to this requirement, mostly for members of the army. See Porto (2002), pp. 44-45. Law 3029 of January 9, 1881 increased the income requirement to vote to 200$ for eleitores.

The centralization of fiscal resources paired with the decentralization of education yielded poor results. For instance, by the end of the imperial period, in 1889, Brazil was the largest country in South America and had one of the lowest literacy rates (16.6%). In some Brazilian provinces literacy rates were closer to 10%, with enrollment rates below 10% in most states. Finally, there were two schools for every 1,000 school-age children in the country and in some states, such as Bahia and Ceará, there was only one school per 1,000 children (see Table 2). This confirms the findings of Pritchett and Woolcock (2004), who argue that other critical elements of effective service delivery are information, accountability, and improved delivery mechanisms.

In 1879, Leôncio de Carvalho, Minster for Internal Affairs, sent a bill to reform the education system of the country to Congress that introduced secular education and mandated the creation of schools of education to train teachers. Education outcomes improved gradually in most states after these reforms, but significant changes in school infrastructure, number of teachers, and the curriculum did not take place until after the Republican parties took over state governments in the 1890s.

Education During The Republic (1889–1930): Increases in Literacy in 1-2-3

In 1889, a Republican movement that overthrew the emperor in a peaceful revolution established a provisional government in charge of drafting a new constitution. Through the change in the legal framework and the rise of a new dominant ideology (positivism), the Republican government brought about a major reform in the way schooling was financed and organized.

Among the most important issues the new Constitution of 1891 brought about was the decentralization of public finances in Brazil.[2] State governments were allowed to tax exports and keep all the revenue. This boosted state coffers in states that exported commodities in high

[2] In the Constitutional Congress of 1890-91, a coalition of exporter states that included São Paulo, Minas Gerais, Rio de Janeiro, Bahia, Pará, and Amazonas defeated a more disorganized coalition that included sugar exporting states in the northeast and the cattle-exporting state of Rio Grande do Sul. In fact, the bargaining power of the winning coalition stemmed to a large extent from the fact that the commodities those states exported, such as coffee and rubber, had significant booms at the end of the nineteenth century. Martinez Fritscher (2009) argues that the economic power of the local elites made the threat of leaving the federation credible enough to allow them to push for a decentralized constitution.

demand (e.g., rubber and coffee) and eroded the public finances of states that exported commodities with negative price shocks (e.g., sugar, tobacco, or cotton). Table 3 shows that, from the Empire to the Republic, there was an increase in real expenditures on education per capita of almost 80% on average, but also show the decline in many states exporting sugar, tobacco, and cotton.

Table 2 shows that the states that had higher average expenditures on education per capita between 1889 and 1930, were those that exported rubber, coffee, and cattle. States that exported coffee and rubber, for instance, spent more than 2.5 times what sugar-exporting states spent per capita (and over 3.5 times what cotton exporters spent). The same differences across states is clear when we look at the number of schools per thousand children in Table 2, a figure closely correlated with the level of export tax revenues per capita.

The education system in Brazil underwent a gradual transformation throughout the Republican period. First, ministers of the interior or of education in the states gradually changed the way schools worked. From the Lancaster method in which in one room students from all ages studied together and helped each other learn with the guidance of one teacher, Republican governments in the states started to modernize schools, introducing the idea of having one teacher per subject and one subject at a time in the schedule. These changes required changes in the buildings as well. Schools could no longer consist of one large room. They required specialization of certain spaces, a separation of students by grades, and the creation of spaces like labs, gyms, and libraries. Obviously not all the states could provide all of these facilities in all of their schools, but gradually schools in large cities started to converge to the new school layout and the new schedule (de Souza, 1998).

The results of an increase in the fiscal capacity of states to spend in schools and the ideological drive to change the schooling system led to significant improvements in school enrollments, teacher-pupil ratios, and the number of schools per children enrolled. Enrollment rates in elementary school, defined as the number of students enrolled over the population of children from 5 to 14 years old, went from 7% in 1889 to 23% in 1933 (Table 2).

The most important increases in enrollment rates took place as a consequence of the expansion of public education at the state level. The elementary school system during the republic was divided into four: private, state, municipal, and federal schools. Since

independence in 1821 most of the elites attended private schools; in most towns and cities private schools were perhaps the best providers of education. Yet, most of the increase in enrollment between 1907 and 1933 took place in schools sponsored by their state governments, gaining market share over private schools

The increase in the number of teachers is perhaps a better indicator of the speed at which state governments invested in education. Table 4 shows the pupil-teacher ratios at the state level decreased, as state governments hired enough new teachers to outpace the rapid increase in enrollment rates. In contrast, the pupil-teacher ratio in private, municipal, and federal schools increased over the same period.

III. Data and Methodology

In order to document the drivers of expenditures on education and of education outcomes, we created a panel with data on expenditures on education, export tax revenues per state, population density, and imports per capita between 1890 and 1930. The Appendix explains the sources and methodology by which the key variables used in the present analysis were estimated. Below, we explain how we construct our main dependent variables and the empirical strategy used to estimate the determinants of public goods expenditures for Brazilian states.

We start by running a simple OLS regression using panel data. Our baseline specification for examining the determinants of expenditures on education per capita by state is of the following form:

$$ee_{it} = \beta\, s_{it} + \delta X_{it} + \zeta_i + \varphi_t + \varepsilon_{it},$$

where ee_{it} is the log of expenditures on education per capita in state i in year t, s_{it} is the log of export tax revenue per capita for each state i and year t. We also include a vector of state characteristics, X, which includes imports per capita, and population or population density. Most specifications include fixed effects (ζ_i) to control for state unobservable characteristics and year dummies (φ_t) to account for time varying trends common to all states (in some specifications we include state trends as well).

The main coefficient β should be interpreted as an (export) income elasticity for state governments that tells us, in percentage points, how much expenditures on education would increase given a 1% increase in export tax revenue. We use the natural logarithm of the

variables to minimize the effect of outliers. Working with natural logs we know most variables follow a normal distribution**.**

We believe it is important to control for imports per capita because it allow us to control for factors that may have determined the demand for education, such as the increase in GDP per capita at the state level. This is because imports had a high elasticity of income in Brazil during this period. Also, as the average family got richer it was easier to send their kids to school. Thus, imports per capita may also help us to control for factors driving the demand for education, such as the level of income or industrialization in the state**.** It may not be the best variable to capture all of these effects, but given the data limitations, especially to build a panel, we think this variable is the best we can do to control for some of those factors on a year-by-year basis.

We understand that even if the type of commodities states could export and the prices of those commodities were determined exogenously for each of the states, the amount of state tax revenues devoted to education may depend on initial conditions at the state level. For instance, politicians may spend less on education per capita in states with higher initial levels of education or in states in which the there was more inequality in the distribution of assets (e.g., land) (Engerman, Mariscal, and Sokoloff, 2009). Moreover, perhaps in states in which there were more slaves before emancipation (1888), elites would want to restrict education for blacks, a phenomenon that took place in the south of the United States for decades after the Civil War (Margo, 1990).

We, therefore, have three ways to deal with the initial heterogeneity across states. First, we run our OLS regressions using fixed effects. Second, we run the baseline specification adding an interaction terms of our variable of interest s_{it} (the log export tax revenue per capita) with different variables that proxy for "colonial institutions" or at least for inequality in the distribution of land and wealth that it could come from colonial times. The variables that proxy for colonial institutions include the percentage of slaves to total population in 1872, population per state before the arrival of the Portuguese, measures of the concentration of land ownership, and dummies that capture if the main commodity produced during colonial times in a state relied on plantation agriculture and/or slave labor (for precise definitions see Panel C of the Appendix). In a way this may be redundant information because export tax revenues per capita

were a product of the crop mix of each state, which in turn were a product of endowments, previous availability of slaves, and other initial conditions. Still we use these specifications as robustness checks to confirm if education expenditures were driven by the institutions commonly used in the literature.

Third, it is important to check if we are confounding the effects of positive trade shocks with possible state-specific trends that may come from before our period. Moreover, it could also be the case that there are state-specific trends that may be correlated with trade shocks, but that were not necessarily a consequence of them. Therefore, we run our baseline OLS specifications using only the averages of our variables. We also run the full panel with OLS including state-specific trends. The results we get doing these two specifications are very similar and have statistical significance of either 1% or 5%.

Instrumental Variables Approach

Beyond using simple OLS estimations, we run a series of estimations using instrumental variables for three reasons. First, we want to ensure that variation in export tax revenues is attributable to exogenous conditions in commodity markets or coming from the fact that natural endowments limit the kind of commodities a state can produce and export. Second, we want to isolate the exogenous variation in prices from possible changes in the tax rates at the state level that could drive the variation in export tax revenues per capita. By making sure we are not including the variation in taxes, we make sure that our results are not a product of political economy factors driving export tax rates, which could be endogenous to either endowments, colonial institutions, or the type of commodities a state exports. In fact, from the scant data on export taxes we have we know that most states had similar tax rates for the same commodity (the differentials were minimum according to costs of transportation). Third, we think there is a possibility of serial correlation in our estimates, since it is likely that export tax revenue at period *t-1* is correlated with the error term at period *t*. For example, a permanent change in conditions (e.g., in preferences or competitiveness) in the international market for the main commodity export of state *i* could increase export tax revenue and, consequently, expenditures on public goods in *t-1*, which could persist through the error term in *t*, thereby driving up expenditures on public goods in period *t*.

Seeing how taxes on commodity exports account for much of state revenues, we wanted to find an exogenous factor that determined the export and revenue collection capacity of each state (without affecting expenditures on public goods directly). Initially we thought of geographical or climate-related variables that explained the supply of exports across states (i.e., why some states specialized in some and not other commodities).

Yet we ran into two obstacles. First we did not have panel data for weather variables. In fact, weather and temperature varied widely within states. Second, creating a panel with climatic variables (such as rainfall, temperatures, and barometric pressure), geographical variables (such as altitude and distance to the equator), and other geological variables (such as soil types, which determine which crops can be produced) would have enabled us to control for conditions that affected the supply of, but not demand for, commodities.

Because the shock we want to capture has an important demand component, and weather data was largely unavailable for the period 1891-1930, we devise an alternative approach. First we rely on the fact that the geographic and weather data that we do have shows a strong correlation with the export or crop mix of each state (i.e., the export mix of each state reflects the specific geographic and weather conditions of the state). Therefore, we use the export mix in 1901 (the first year for which we have complete fiscal data for all states) to create export price indices per state. Having the export mix of each state we then proceed to use the annual variation in the prices of the largest exports to capture shot-term fluctuations in demand and supply and create simulated export price indices for every state (leaving the weights fixed according to the export mix in 1901). We use fixed weights because we want the export mix to be as exogenous as possible to expenditures on education (in any case the results do not change much if we use the export basket in each year to weight prices).

We combine the information on commodity exports at the state level in the initial year with the variation in prices and create export price indices for every state. We take the eight most important commodity exports and use their shares in 1901 to weight the price index.[3] We

[3] The first year for which there are data for commodity exports at the state level is 1901. There being no evidence of compositional changes in the state exports during the 1890s, we believe that 1901 should be representative of the state of commodity exports in 1890.

use world market prices for commodities, either from Global Financial Data or from the database of Jacks, O'Rourke, and Williamson (2009).

We then use a price index for each state as an instrument for state public revenue per capita in the first stage, the idea being that our price indices per state will reflect how much states can extract in ad valorem taxes on exports. In the second stage, we use our estimated state public revenues per capita as independent variable to estimate the expenditures on education per capita.

Using price indices of commodity exports, however, assumes that states did not influence the growth rate of prices in international markets, which is not necessarily true. This is problematic because São Paulo, Minas Gerais, and Rio de Janeiro, as price setters in the international coffee market, largely determined the growth rate of national coffee exports (especially in 1906-1914, and in some years in the 1920s). Also, Amazonas and Pará were the principal suppliers in the international rubber market, but there was no coordination or any explicit effort to control prices; rubber exporters were price takers. To deal with the potential endogeneity in coffee prices, we construct alternative price indexes that ignore the price fluctuations for coffee and we then do the same excluding rubber prices. The results do not change too much when we exclude coffee or rubber from the price indices or when we remove from the sample the states that obtained most of their revenue per capita by exporting coffee (e.g. São Paulo) and rubber (Amazonas).

IV. Findings

Our OLS estimates show that increases in export tax revenues are significant to explain the increases in expenditures on education at the state level (see Table 5) and that the effect of an increase of 1% in export tax revenues is an increase in education expenditures of 0.12%-0.27% once we control for imports, population density and fixed effects. That means that large jumps in export tax revenues per capita over time, for instance jumps of 100% in states that exported rubber or coffee, education expenditures per capita could be increased almost 20%. Even when we control for the composition of the export basket we find that the coefficient for export revenues per capita is still significant and of similar magnitude. That means that it was not changes in the composition of exports that determined the increase in revenues and expenditures, but either the price ramp up or the capacity to export more volume.

Robustness checks

In specifications 7 through 12 of Table 5 we run OLS specifications that include state-specific time trends, in addition to the fixed effects and the time dummies for all states. We then find that export tax revenue is still significant in some of the specifications and explain increases in education expenditures, even if only at 10% significance. In specification 9 we have to take out the data for the state of Minas Gerais because we do not have data on its imports and in specification 5 and 6 (as well as in 11 and 12) we take out states that exported coffee (Rio de Janeiro and São Paulo) and rubber (Amazonas and Pará), respectively. Across the board our coefficient for the logarithm of export tax revenue is weakened, with the elasticity going to 0.12. That means that the true effect may be at the lower bound of the OLS estimate without state-specific trends.

Another way to approach the same concern is to run a simple OLS using the average of the variables of interest. Interestingly, the coefficient of export tax revenue per capita is of similar magnitude to those we found using panel estimates with time trends.

Instrumental Variables

In order to show that the variation in export tax revenues is exogenous to the political economy of the state (e.g., to changes in tax rates), and to correct for possible serial correlation, we run the same estimates using our export price indices for each state as instrumental variables (IVs). The results of our IV estimates are in Table 6. The variation in export prices at the state level seems to explain the variation in expenditures on education over time quite strongly. Again even after controlling for the composition of the portfolio (the average) we find strong coefficients in the first and second stages. This perhaps implies that what mattered the most to increase revenues and expenditures were the price ramp ups. In this table we also run estimates that exclude the price of coffee and rubber and show that the results are not driven by Brazil's market power in these two products as the coefficients do not change radically.

The coefficients for the variable of interest (export tax revenues) in the second stage are larger than our OLS panel coefficient, but close to one standard error larger so we believe there is no significant bias or measurement error driving our IV results. One could think that the

coefficients could be biased upwards because the prices of commodities affect expenditures through other channels than just export tax revenues (e.g., commodity prices could have pushed land prices up and thus increased the collection of land taxes and expenditures on education), that is, there could be a possible violation of the exclusion restriction. However, in Table 6 we have controlled for the other tax revenues, which include land taxes, a tax on industries and professions, and other stamp taxes in order to study the pure effect of export tax revenues on education expenditures. Even after controlling for these alternative channels we still find a strong effect of our simulated price indices on education expenditures. Moreover, when we control for the crop mix of the state the alternative tax revenue channels have no significant effects, while our instrumented export tax revenues is still significant. Thus, we think the evidence shows that the effect of commodity prices on expenditures through other revenues is not a major problem and that there is no violation of the exclusion restriction.

Explaining Education Indicators Using a Reduced Form

Going beyond just expenditures on education, what we really care about is whether the increase in export tax revenue per capita or the price of exports can help us explain the improvements in education indicators over time. In order to check this we take two approaches. First, we average out all of our variables and run a simple cross-sectional regression (with limited sample size of 20) and check if average expenditures on schooling per capita are correlated with the change in literacy rates (1890-1940), the number of schools (1890-1940), and the number of students (1890-1940). We find significant correlations across the board, except for the change in the number of students, which is only significant when we control for state characteristics (See Table 7A). We then run similar regressions using panel data (Table 7B) and using our simulated export price indices at the state level as independent variable, rather than using export tax revenue per capita. We get consistent significant coefficients except for the specification in which we control for population.

In sum, our empirical strategy shows that state governments collected more tax revenue when they had increases in the prices of their commodities. Those states that had higher export tax revenues ended up spending more on education and having better outcomes such as higher literacy and enrollment rates or more schools. Yet, we have not explained why the political

elites who controlled the government in the different states of Brazil would have incentives to use the "windfall" profits of exports to pay for education for all. In the next section we examine the incentives of these elites.

Colonial institutions and education expenditures between 1889 and 1930

In order to explore whether initial conditions may be determining why states spend on education when they receive an additional dollar in revenue we run the same OLS regressions (with panel data) we presented in the previous section, but this time we add interaction terms that multiply export tax revenue per capita by each of our variables that are proxies colonial institutions (see Table 8). The interactive variables we use are the percentage of slaves to total population by state in 1872 (Engerman and Sokoloff, 1997), the native population before colonization (Acemoglu, Johnson and Robinson, 2001; Bruhn and Gallego, 2007) the average size of a farm in 1920, as a proxy for land concentration (Engerman and Sokoloff, 2002; Engerman, Mariscal and Sokoloff, 2009), and a dummy for good (coded as 1) and bad (0) colonial institutions depending on whether the main commodity the state produced during colonial times either relied on plantation agriculture or on some form of coerced labor (we follow the classification of commodities of Bruhn and Gallego, 2007, see Panel C of the Appendix).

For simplicity, we call the set of all of these variables "colonial institutions," even if not all these initial conditions come from colonial times (e.g., our data on land concentration). This is because the argument of the literature on colonial institutions is that inequality in the distribution of economic assets and political power was broadly determined during colonial times and then persisted over time (Acemoglu et al, 2001; Engerman and Sokoloff, 2002).[4]

[4] We actually think that for some variables there is relative persistence. For instance, the correlation of the number of slaves by state in 1864, the first year for which we have data and 1887, the last year before emancipation, is 0.8, even though there was significant migration from the sugar regions in the northeast to the coffee areas of the southeast of Brazil. Yet, we are not sure about the persistence in land holding patterns. Wegenast (2009) assumes that land concentration was stable since colonial times and even uses the Gini coefficient for land concentration in 1950 as "exogenous" source of variation to explain expenditures on education in the twentieth century. In contrast, Engerman and Sokoloff (forthcoming) explain that land laws and land ownership had more changes over time than other institutions.

Our econometric estimates show that these proxies for colonial institutions are not significant when interacted with export tax revenues per capita. This is probably because export tax revenues are, as we mentioned earlier, already determined by endowments. The only coefficient that deserves a separate explanation is that of the good/bad commodity dummy interaction in specification 3. This coefficient is positive and significant and our variable of interest (export tax revenues) alone loses significance. This is because the states that ended up producing the profitable commodities (e.g., rubber and coffee) during our period, where, coincidentally, states that did not have plantation agriculture during colonial times. In fact, they were provinces with low population densities. In contrast, the states that produced sugar, tobacco, and cotton during our period were states that had a large slave population and produced sugar and tobacco in large plantations.

Thus, one may think that a large part of the effect of colonial institutions or how entrenched imperial elites were in each state may be captured by the fixed effect of the OLS regressions. In fact, most of the fixed effects were negative and in some states, such as Bahia and Pernambuco, they were also large. We believe these large negative fixed effects may be related to how entrenched imperial elites were in those states. A good example is the state of Pernambuco, with one of the largest negative fixed effects, where "ex-monarchists dominated state politics," and where "not a single historical Republican was elected governor" (Love, 1980, p. 112). In fact, Pernambuco started with one of the highest literacy rates within Brazil (in 1889) and then fell to the bottom of the rankings by 1930 because of lack of investment in education (see Table 1). On average Pernambuco devoted 7.1% of expenditures to education during the Republic, making it the state with the second lowest share of expenditure going to education. Pernambuco also had one of the lowest per capita expenditures on education, far below the mean for Brazil (see Table 3).

V. Demand vs. Supply in the Provision of Education

In this section we examine the motivation of state politicians and state political parties to spend money on education. Understanding the incentives that politicians had to spend on education in Brazil between 1889 and 1930 is particularly important because their behavior is puzzling when seen under the light of the literature that studies political institutions and education expenditures. In a country with such steep inequality and in which the Constitution included a

literacy requirement to vote we would expect elites to limit the provision of education to the elites (Engerman, Mariscal and Sokoloff, 2009; Lindert, 2004). In fact, before 1889 most of the expenditures on education went to a limited number of schools and there were subsidies for certain private schools that educated mostly the children of the imperial elites.

Following, Lindert (2003, 2004) one would expect that in states that had a larger number of voters to total population—his measure of political voice—there should be higher expenditures on education per children. A simple scatter plot showing average expenditures per capita and the change in the number of voters from 1875 to 1930 across Brazilian states shows that the dynamic that Lindert suggested may have been at work in Brazil since the change in the number of voters is highly correlated with the level of expenditures. We also find that there is a significant and positive correlation between the increase in the number of voters to total population and education expenditures per capita at the state level (Table 9).

There are many reasons why we would expect to find an increase in the demand for education over time, in particular as the number of voters increased. For instance, as Brazil industrialized, industrialists could have pressured governments to provide more education. Alternatively, families themselves could have demanded more education as skill premia increased (i.e., the difference in salary between skilled and unskilled workers), or simply as a product of the fact that families were richer and could afford to send their kids to school. Finally, the rapid increase in European immigration after 1890 could have been another cause of the increase in demand, either because planters in Brazil pushed local governments to offer better public education to attract migrants or simply because as the migrants arrived they demanded public schools.

We test for some of these hypotheses to see if there is clear demand push for education. We, however, find no consistent evidence that industrialization, or immigration drove the increase in education expenditures at the state level. Since there is not panel data for industrialization or immigration by state, we use data from the population census (1890, 1920, 1940) and industrial census (1907, 1920, and 1940) and interact the data with our variable of interest, export tax revenue per capita, in order to use the full potential of our panel. We find significant coefficients but with negative signs when we interact the latter variable with either growth in industrial production between 1907 and 1940, the number of industrial firms or the

value of industrial production in 1907, 1920, and 1940. The same happens when we interact export tax revenue per capita with the number of immigrants in 1890 or 1920 (Table 10)

There are two reasons why we feel confident about our puzzling results that immigration and industrialization are not correlated with increased in expenditures on education at the state level. First, a great majority of the European immigrants to Brazil came from countries where governments did not spend much on education, such as Italy, Portugal and Spain (Lindert, 2004), so there is no reason to expect them to demand education in Brazil. Second, the industrialization of Brazil was not with technology that had skill-complementarity. For instance, following Goldin and Katz (1998), we divide the industries for which we have data on technology imports between those that are the product of the first industrial revolution (i.e., textile and machinery for woodwork), which require low levels of education, and a second generation of technology, product of the second industrial revolution (i.e., machinery for energy and electric equipment) that relies on a more skilled labor force. We find that the largest increase in machinery imports took place in sectors linked to the first industrial revolution, which were labor-intensive and required less skilled workers.

Still, even if the link between industrialization or immigration and education expenditures is weak, we cannot falsify the hypothesis that changes in income or societal preferences increased the demand for education. Nevertheless, what we can do is documenting some of the dynamics in the supply side just to show that there is stronger statistical evidence to back some of the supply-side dynamics.

In our view, the correlation between voters and expenditures on education in Table 9 has an endogeneity problem. Since there was a literacy restriction to vote, the number of voters is endogenous to expenditures on education. In states where expenditures on education were used to teach children (and adults) how to read and write, there was an increase in the number of voters. This problem is particularly clear in our case because we are working with education data that comes from census years that were spaced far apart, thus blurring the causality line between the increase in voters and improvements in education. It is hard to get away from this problem of reverse causality as it is hard to think about an instrument that could explain the increase in the number of voters that does not affect directly expenditures per capita or is not

highly correlated with expenditures or education outcomes. Thus, in the case of Brazil it is hard to defend the causality from voters to expenditures only (Lindert, 2003,2004).

Instead, we think that our simulated price indices can be used as an instrument for expenditures on education per capita (a reduced form of our IV regressions) and that if we find they are correlated with the number of voters, there could be evidence that the causality runs from expenditures to voters. This is because most of the expenditures of state governments came from export tax revenues, thus expenditures on education per capita can be instrumented using the exogenous variation in exports per state given by price movements. States with better terms of trade could have attracted immigrants who could be potential voters, say because of their higher literacy rates. This dynamic, however, was not that strong as there was minimal internal migration in Brazil as transportation costs were too expensive and because about half of the European immigrants who went to Brazil were illiterate and not all of the literate immigrants naturalized to become voters.

In our previous estimations, we find that the variations in price movements are highly correlated with expenditures on education (Table 6), with the change in education outcomes (Table 7), and education expenditures are correlated with the increase in the number of voters per state (Table 9). That is, it is easy to defend statistically the supply story than the demand story of voters demanding more expenditure.

The last issue is just to provide an explanation of why state politicians would want to invest in education if there was a literacy requirement preventing the masses from demanding such public services. In our view, it is precisely *because* there was a literacy requirement to vote that state politicians had incentives to provide basic elementary education not only to meet the demands of voters for public goods, but as a way to increase their capacity to mobilize voters for national elections. In order to increase the number of voters the state dominant parties could mobilize, politicians needed to increase the number of literate adult males. This had to be done by teaching the "desired" group of voters the basics of how to read and write.

These incentives for politicians came into place when the Republican movement overthrew the imperial government in 1889. Since 1881, adult males who wished to become registered voters had to be able to write their name and the date when they registered. This law also kept the income requirement that prevailed in Brazil since the early nineteenth century,

increasing the minimum annual income required to vote from 100 mil reis (about US$43) to 200 mil reis (US$85), the equivalent of an annual salary for most blue collar jobs.[5] Then, between 1889 and 1890, two Republican decrees eliminated the income requirement to vote and changed the electoral system, from one with electoral colleges at the state level, to a system with direct elections for president and federal congressmen. Thus, the government made every vote in any part of the country be worth the same in national elections. In order to compete for political power, either to win the presidency of the country, or to win favors from the ruling coalition, state political parties had to compete against one another by increasing the number of votes they could mobilize in their states in national elections.

In general, state parties had to bargain with the ruling coalition at the federal level, integrated by the Republican parties of the states of São Paulo and Minas Gerais.[6] That was the case in the 1890s and by 1902 President Manuel Ferraz de Campos Sales forged an agreement with governors and state parties through which, in exchange for support for the ruling coalition in national congress and for votes in the presidential elections, state politicians got favors. The kind of favors a state politician asked for in such a decentralized federation ranged from no military intervention from the federal government, the deployment of less federal soldiers in their states, and subsidies to build railways or ports, to congressional support to block state opposition parties.

According to this agreement between the ruling coalition at the federal level and Republican (or pro-Republican) parties at the state level , the latter could appeal to the president and its ruling coalition in Congress for help if an opposition party at the state level threatened their hold of power. This is because contested elections for governors or federal senators and congressmen had to be scrutinized by national congress. Therefore, the dominant block in Congress could help a state party to annul the election of an opposition candidate on some technical ground. This practice was commonly referred to as "beheading"[7].

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[5] See the so-called Saraiva Law, Decree 3029 of 1881.

[6] Except for the 1910 to 1914 presidential period, when the ruling coalition had the Republican Party of Minas Gerais that of Rio Grande do Sul, leaving the Republican Party of São Paulo outside of the circle of power. For a basic overview of power relations among states see Fausto (1999: 265-267).

[7] See Porto (2002), p. 196 and Fausto (1999) pp. 258-259 or the vote count in the *Diario do Congresso* on June 27, 1902.

It is hard to think that state politicians had a long enough horizon to invest in educating children so that they could vote in future elections. Yet, dominant political families ruled for 10 or 15 years in power in some states, while in others the dominant parties ruled for decades (de Souza, 1984). Also, most states had a dominant state republican party that had the incentive to invest in increasing the number of voters it could mobilize in the future, both in order to keep or increase its bargaining power vis a vis the dominant parties controlling the presidency or as a way of hedging against the rise of an opposition party in their own state.

Now, the objective function of politicians at the state level was not just to maximize the number of voters, otherwise one could argue that they could have simply done away with the literacy requirement, ignoring the writing test at the time of registering voters. But political elites did not want to increase the number of voters in a way that threatened their tenure in office. Thus, we think that the literacy test was a way to "filter" who could vote and the policy variable used to increase the franchise was the increase in literacy, either in elementary education or in night schools. Doing away with electoral institutions, such as the process to register voters, was not an option. The political system was oligarchic, but had some checks and balances in operation. Massive electoral fraud or manipulation of the registration process was monitored and punished by parties in national congress. Electoral conflicts and anomalies led to significant conflict in congress, military tensions between state governments and the federal government, and even a civil war in 1930.

As a way to minimize political opposition at the state level parties and politicians made investments to improve education only at the margin; only enough to make people pass the literacy test to vote, but not enough to increase the franchise and education in a way that would risk their control of the state. This is because the potential risk of enfranchising too many people or marginalized sectors of the population could end up leading to an overthrow of the dominant party and of the status quo in the state. For instance, the Brazilian ruling white elite may have wanted to keep former slaves (emancipated in 1888) at bay as much as possible.

Therefore, we should not expect to find that education expenditures before 1930 increased dramatically the educational attainment of the population and, especially not for black Brazilians. One way to examine these two hypotheses is to look at the education accomplishments of two cohorts, those who were 6-10 years old in 1920 and those who were of

the same age in 1930, using Brazilian census data compiled by IPUMS. In Table 11 we show that there were significant improvements in literacy in this cohort compared to the initial level of literacy in our period, going from a literacy rate of less than 20% of the population in 1890 to over 50% for these cohorts. Yet, this improvement in basic skills to read and write did not translate into a radical improvement in academic attainment for all. For instance, there is a significant difference in the educational attainment of blacks and mixed race Brazilians compared to whites, with literacy rates of around 30 percent or less for the former and around 60 percent for the latter. The percentage of people who never attended school is closer to 80% in the black and mixed race group, versus 50% for whites.

VI. Conclusion: Implications in the Long Run

In this paper we have shown that there was some progress in the provision of elementary education in Brazil between 1889 and 1930 and that it was to a large extent a consequence of the fact that some states got export tax revenues to spend on public education. We are cautious, however, because for the period we examined we could not infer anything on the quality of education. We acknowledge the fact that increases in the quantity of education do not necessarily translate into increases in the accumulation of human capital. Still, given the starting level of educational attainment in Brazil, the expansion in the supply of education in our period was significant.

We think that our findings are original and surprising for a broad literature that studies the political economy of education for three reasons. First, the fact that there can be trade shocks that alter the development trajectory of a state in a significant way, despite the legacy of colonial institutions, is important. Few of the works that defend the persistent effect of colonial institutions discuss in depth the kind of shocks that actually can change the development trajectory of a country or in this case, a state. We argue that initial conditions (or the so-called colonial institutions) were strong constraints to increase education expenditures after states received windfall profits from taxing exports, but at the end of the day our econometric work shows that windfall tax revenues had a net positive effect on education expenditures.

Moreover, we show that shocks to the terms of trade can have long-lasting consequences on the distribution of wealth and human capital across states. For instance, the ranking of Brazilian states according to literacy rates has not changed much since 1930, but is very

different from that of the late nineteenth century (e.g., 1872). This is partly because after 1930 both industrialization and internal migration patterns perpetuated the relative inequality across states and even accentuated it as capital and labor flowed to the states that were more educated at the turn of the century. Therefore, our paper suggests one explanation of the origin of high regional inequality in Brazil.

Second, the advances that we describe in the provision of public education happened despite the fact that there was a literacy requirement to vote. This may be puzzling when compared to the findings of Engerman and Sokoloff (1997, 2002) or Lindert (2004), who find that in countries with literacy requirements the ruling elite spends less on education that in countries without such restrictions. Naidu (2010) also finds similar results at the county level for the Post-Bellum South in the United States. Yet we show that competition in national elections in Brazil (to mobilize more voters for presidential election) and the literacy requirement may have provided the right incentives for state political parties and state politicians to spend on education. We think some of the divergent results are due to the fact that the cross-country literature has an implicit model with one elite, with coherent and unified preferences, which controls politics and rations the supply of education. In the case of Brazil (1889-1930) we find that there were a multitude of state and federal elites competing and bargaining with each other. Dominant elites at the state level were rationing education, but not to prevent as many people as possible from voting, but as a way to maximize their hold of power. This sometimes implied increasing the provision of education. Recent research on Brazil, Russia, India, and China finds that in large countries with relatively autonomous provinces the rationing of education varies according to the heterogeneity of elite interests across subnational units (Chaudhary, Musacchio, Nafziger, and Yan, 2010).

Third, the fact that the expansion in the provision of education was financed by taxing commodity exports is surprising because there is a long discussion among social scientists on whether there is a so-called "resource curse" (Sachs and Warner, 1995; Lederman and Maloney, 2007). A broad definition of the resource curse, beyond the fact that countries with abundant natural resources tend to have slower growth, would argue that countries that have abundant natural resources develop renter mentalities that can prevent them from investing in productive capacity in the long run (e.g., leading them to have low investment in education). Our findings

support the idea that there is no resource curse, but that positive trade shocks can be converted into long-term development if there is electoral competition and economic assets are not concentrated in a few hands.

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The Great Leap Forward: The Political Economy of Education in Brazil, 1889-1930
Appendix. Data Sources

Panel A. Sources for Education Indicators, 1872–1940

Variable	1872	1890	1900	1907	1920	1933	1940	Public/Private	Source
Literacy Rate	X	X	X		X		X		1872, 1890, 1900 and 1920 from Brazil (1923); 1940 from Brazil (1950)
Population, total, age brackets and national/foreigners	X	X	X		X		X		1872, 1890, 1900 and 1920 from Brazil (1923); 1940 from Brazil (1950)
Number of Primary Schools	X			X	X	X	X	Both	For 1872, from Brazil (1917a); 1907 from (1917b); 1920 from Brazil (1923); 1933 from Brazil (1936) and 1940 from Brazil (1946)
Enrollment in Primary Schools (TOTAL)	X			X	X	X	X	Both	For 1872 from Brazil (1940); 1907 from (1917b); 1920 from Brazil (1923); 1933 from Brazil (1936) and 1940 from Brazil (1946)
Primary Schools Teachers				X		X	X	Both	1907 from (1917b); 1920 from Brazil (1923); 1933 from Brazil (1936) and 1940 from Brazil (1946)
Teachers who attended a school of education ("Normal" Teachers)						X	X		Brazil (1946)
Graduation ("Conclusao")				X		X	X	Both	1907 from (1917b); 1920 from Brazil (1923); 1933 from Brazil (1936) and 1940 from Brazil (1946)

Panel B. Fiscal and Trade Data

Variable	Source:
Education Expenditure and Export Tax Revenue[8]	Willeman (1909) and Brazil (1926), data for the 1880s from Brazil (1887)
State Public Revenue[9]	For data before 1897, we use Brazil (1914). For data from 1897 to 1939, see AEB V (1939/40).
Commodity prices	Global Financial Data and Jacks et al (2009).
Stock of Debt	Wileman (1909) has unbalanced data until 1908. For 1912 we take the information from Brazil (1917a). For 1922, we take the information from Brazil (1926) and finally for 1930 the source is Bouças (1932). We have also added data compiled for São Paulo from Love (1980). We extrapolated between these data

[8] We only have state expenditures in schooling for the periods: 1901-1907, 1914-1916, 1919-1921 and 1924-1926. Expenditures come from the state budgets and may differ from the actual amounts spent.

[9] The data is the budgeted and not the "actual" amounts spent. The data sources we have reported budgets for either 6 or 18 months, thus we had to annualize the amounts multiplying by 2 or 2/3 respectively. Finally, we completed some missing data using simple linear interpolation between the closest data points available.

	points in a way that allowed us to run a panel.
Exports and Imports	Data from 1902 (imports) and 1901 and 1902 (exports) from Brazil (1904); 1908-1912 comes from Brazil (1917a); Data from 1913-1927 and 1935-40 comes from Commerico Exterior do Brasil, several years.; Information from 1928-1934 is from Brazil (1938); Data for 1887, 1892 to 1897 and 1903-1907 is from Brazil (1908). Except for Minas Gerais[10] and the Federal District (Distrito Federal).[11] Data for Minas Gerais from Minas Gerais (1929)

10 We have information only for states that had customs offices and a port (or a navigable river that connected it to the ocean). For this reason, we originally had no data for Góias (GO) and Minas Gerais (MG). Yet for Minas Gerais we have some reports of total exports, but not from which port they were shipped. Since we know that most of the exports were shipped from Rio de Janeiro (RJ), Santos (in São Paulo, SP), and in the 1920s through Espírito Santo (ES). For simplicity we assume that the exports of MG were exported through RJ and SP in equal proportions. Thus we subtract the exports from MG from those two other states. For the MG export data for 1927-1931, we assume that the MG average export share between 1923 and 1927 will prevail for the rest of the studied period and we proceed with the same methodology as explained above. In order to show that results of the estimations do not change, we also use the exports as reported by the federal publications (excluding MG). Unfortunately, data for imports for MG are not available. Therefore, all the estimations that include imports as a control exclude the observations from MG.

11 The city of Rio de Janeiro was the capital of Brazil, known as Federal District (Distrito Federal or DF). Rio de Janeiro City is in the middle of what was Rio de Janeiro State, now Guanabara. Both the city and the state collected their own tax revenue, yet export taxes collected in the port of Rio de Janeiro accrued mostly to the State of Rio, while import taxes accrued to the Federal Government, as in other parts of the country. Moreover, the port of Rio de Janeiro, in the Federal District, served the states of Rio de Janeiro and Minas Gerais. Rio de Janeiro state had no other port until the 1920s (i.e. Angra dos Reis). Therefore, we cannot distinguish the exports made from the capital itself and Rio de Janeiro State (or Minas Gerais, see note above). We are confident, however, that most of the exports shipped from the Rio de Janeiro port were commodities produced in the state of Rio de Janeiro and not in the Federal District. Furthermore, we consider that the state of Rio de Janeiro benefited from the exports and economic activity of the port of the city of Rio de Janeiro and vice versa and for this reason we use the same level of international trade activity for both state and city.

Panel C. Data sources for variables that measure institutions, industrialization, and electoral participation

Variable	Definition	Source:
Capital invested	Total social capital in industrial companies	1920 Census
Dummy Good Commodity	If the state grew a "good" commodity is 1; otherwise 0. Good commodities include cacao, cattle, and cotton; bad commodities include the trade of enslaved Indians, mining, and sugar. We use Bruhn and Gallego's coding, but add Ceará as cotton and Piauí as sugar. Thus we code states as follows: AL=Sugar, AM=Cacao; BA=Sugar; CE=Cotton; ES=Sugar; GO=Mining; MA=Cotton; MG=Mining; MT=Cattle; PA=Cacao; PB=Sugar; PE=Sugar; PI=Sugar; PR=Mining; RJ=Sugar; RN=Cattle; RS=Cattle; SC=Cattle; SE=Sugar; SP=Indians.	Bruhn and Gallego (2007)
Industrial Production and Number of Industrial Establishments; and Wage Premium	Industrial production in 1920 milreís and number of industrial establishments. Skill premium for 1940 is defined as the ratio of the average administrative wage to the average worker wage in 1940. Skill premium for 1920 is defined as the average wage of the food industry to the average wage of textile industry, as the former has more administrative workers than the latter.	1907, 1920 and 1940 Industrial Census
Mortality Rates	We use three different measures. The first one is an overall measure of mortality per 1,000 people from the population census of 1920 and 1940 (Brazil, 1923, 1950). The second is a measure of mortality from tropical diseases, which include yellow fever, "intermittent fever," Malaria or paludism, and Typhoid fever. The third measure also includes all sorts of gastrointestinal diseases, especially Cholera and Dysentheria. The latter two mortality rates are estimated over 1000 inhabitants and are for 1910.	Brazil (1913)
Population Density	Population/km^2	For population see Panel A; for state areas, see Wileman (1909)
Pre-colonial Native Population	Population per squared km at the time of colonization	Bruhn and Gallego (2007)
Size of Rural Establishments in 1920	Average number of hectares per rural establishment in 1920.	1920 Industrial Census
Slave Share in 1872	Percentage of the population that was slave in 1872	1872 Population Census
Voters in 1875, 1910 and 1934	Before 1891 the number of voters represents the number of registered voters, between 1891 and 1934 we have the data for the number of registered voters (*eleitores)* and we only have the number of actual votes for the 1910 election.	Brazil (1913) and ipeadata.com

Table 1. Ranking of States by Literacy Rates In the Long Run

Panel A. Ranking of States by Literacy Rates

	1872		1890		1940		2007	
	Literacy Rate	Ranking	Literacy Rate	Ranking	Literacy Rate	Ranking	Literacy Rate	Ranking
States that moved up the ranking over time								
SP	18.8	**10**	16.6	**10**	52.1	**2**	95.4	**3**
SC	16.5	**11**	23.3	**3**	49.1	**3**	95.6	**2**
GO	16.2	**12**	12.6	**16**	22.8	**16**	91.2	**8**
AM	14.1	**15**	19.0	**6**	36.6	**9**	92.0	**6**
ES	13.1	**17**	16.0	**13**	39.8	**8**	91.5	**7**
MG	11.2	**20**	12.2	**17**	33.0	**10**	91.1	**9**
RJ	19.1	**9**	17.8	**8**	42.5	**5**	95.7	**1**
States that did not move significantly from their ranking in 1872[a]								
PR	28.9	**1**	22.5	**4**	42.9	**4**	93.4	**5**
RS	22.5	**3**	30.3	**1**	54.4	**1**	95.0	**4**
SE	13.4	**16**	11.6	**19**	27.2	**11**	83.2	**12**
CE	13.0	**18**	16.3	**11**	26.2	**13**	80.8	**15**
PB	12.9	**19**	14.9	**15**	20.8	**18**	76.5	**18**
States that moved down the ranking over time								
PA	26.7	**2**	26.0	**2**	41.1	**6**	88.3	**11**
MA	22.1	**4**	15.4	**14**	21.2	**17**	78.5	**17**
MT	20.5	**5**	19.4	**5**	40.5	**7**	89.9	**10**
BA	20.3	**6**	10.1	**20**	23.7	**15**	81.5	**13**
PE	19.6	**7**	16.8	**9**	25.1	**14**	81.5	**14**
RN	19.1	**8**	18.3	**7**	27.1	**12**	80.4	**16**
PI	15.0	**13**	11.8	**18**	19.0	**20**	76.5	**19**
AL	14.3	**14**	16.2	**12**	19.5	**19**	74.8	**20**

Panel B Correlation of Literacy Rates by State[b]

	1872	1890	1900	1920	1940	1950	1970	1980	1991
1890	0.8215*	1							
1900	0.6735*	0.8666*	1						
1920	0.7432*	0.9107*	0.9256*	1					
1940	0.6555*	0.8372*	0.8631*	0.9731*	1				
1950	0.6070*	0.7888*	0.8055*	0.9427*	0.9895*	1			
1970	0.3969	0.5539*	0.6529*	0.7840*	0.8719*	0.9127*	1		
1980	0.3914	0.5381	0.6447*	0.7718*	0.8592*	0.8984*	0.9922*	1	
1991	0.3545	0.4844	0.6069*	0.7382*	0.8301*	0.8732*	0.9792*	0.9925*	1
2007	0.3295	0.4735	0.6504*	0.7384*	0.8218*	0.8550*	0.9684*	0.9801*	0.9839*

Notes:a) This group shows states that did not move more than five places in the overall ranking between 1872 and 2007. b) These correlations include all states except the Federal District. Stars (*) denote 1% significance.

Table 2. Expenditures in Schooling, Literacy Rate , Enrollment and Schools

	Main Commodity	Expenditure in schooling per cap (avg. 1901-1926)	Primary schools in 1889	Primary schools in 1933	Students in 1889	Students in 1933	Enrollment Rate in Primary School 1889	Enrollment Rate in Primary School 1933	Schools per 1000´s children 1889	Schools per 1000´s children 1933
Alagoas	Sugar	0.5	209	560	6,928	32,913	5.4	13.2	1.62	2.25
Amazonas	Rubber	3.2	122	926	3,546	24,100	10.0	23.2	3.43	8.90
Bahia	Tobacco	0.4	671	1,624	22,131	86,876	4.4	9.2	1.34	1.72
Ceará	Cattle	0.7	237	861	9,497	62,035	4.2	13.0	1.04	1.80
Espírito Santo	Coffee	1.0	280	801	18,698	166,644	7.2	25.2	2.93	4.40
Distrito Federal			105	784	2,582	44,783	19.0	56.1	2.84	2.70
Goiás		0.2	95	391	2,708	22,956	4.4	12.1	1.56	2.06
Maranhão	Cotton	0.5	170	636	6,545	34,117	5.7	12.2	1.49	2.28
Minas Gerais	Coffee	0.8	1,757	3,628	46,997	396,769	5.7	23.4	2.15	2.14
Mato Grosso	Rubber	1.8	51	302	1,830	20,888	7.9	22.8	2.20	3.30
Pará	Rubber	2.0	336	999	11,904	65,745	13.5	27.9	3.80	4.23
Paraíba	Cotton	0.5	92	710	2,531	51,317	2.0	16.0	0.74	2.22
Pernambuco	Sugar	0.5	747	1,902	19,742	98,204	7.5	15.7	2.85	3.04
Piauí	Cotton	0.2	84	181	2,129	15,999	2.9	8.0	1.14	0.91
Paraná	Mate	1.4	213	1,037	6,968	69,140	10.2	25.2	3.11	3.78
Rio de Janeiro	Coffee	1.0	852	1,531	31,091	129,543	14.4	29.1	3.95	3.44
Rio Grande do Norte	Cotton	0.5	159	430	5,443	34,847	7.7	20.6	2.26	2.55
Rio Grande do Sul	Cattle	1.5	499	4,313	24,287	249,895	9.8	33.2	2.01	5.73
Santa Catarina	Mate	0.8	174	1,733	7,508	100,861	10.0	37.3	2.31	6.41
Sergipe	Sugar	0.9	206	448	3,750	22,291	4.9	17.4	2.69	3.49
São Paulo	Coffee	3.6	1,098	4,910	21,989	488,646	6.3	31.6	3.15	3.18
Brazil		**1.2**	**8,157**	**28,707**	**258,804**	**2,218,569**	**7.0**	**23.3**	**2.2**	**3.0**

Table 3. State Expenditures on Education Per Capita Before and During the Republic, 1875-1925

		1875-1884 (average)		1901-1925 (average)		
	Main commodity exported	Expenditure on education per capita (1913 milreis)	Expenditures on education /total expenditure	Expenditure on education per capita (1913 milreis)	Expenditures on education /total expenditure	Growth in real expenditures per capita
Alagoas	Sugar	0.5	19%	0.5	13%	-3%
Amazonas	Rubber	1.8	12%	3.2	9%	80%
Bahia	Tobacco	0.5	15%	0.4	6%	-15%
Ceará	Cattle	0.4	23%	0.7	19%	76%
Espírito Santo	Coffee	1.2	22%	1.0	9%	-14%
Goiás		0.4	21%	0.2	8%	-37%
Maranhão	Cotton	0.9	32%	0.5	10%	-46%
Mato Grosso	Rubber	0.9	23%	1.7	12%	76%
Minas Gerais	Coffee	0.4	28%	2.4	15%	448%
Pará	Rubber	2.4	25%	2.1	11%	-13%
Paraíba	Cotton	0.4	18%	0.5	12%	31%
Paraná	Mate	0.9	20%	1.4	14%	54%
Pernambuco	Sugar	1.0	20%	0.5	7%	-46%
Piauí	Cotton	0.3	16%	0.2	9%	-14%
Rio de Janeiro	Coffee	1.6	19%	1.2	11%	-24%
Rio Grande do Norte	Cotton	0.5	27%	0.5	9%	-2%
Rio Grande do Sul	Cattle	1.1	19%	1.8	15%	67%
Santa Catarina	Mate	0.6	27%	0.8	13%	30%
São Paulo	Coffee	0.7	14%	3.6	16%	441%
Sergipe	Sugar	0.7	19%	0.9	14%	24%
Brazil		**0.7**	**19%**	**1.2**	**11%**	**79%**

Table 4. Pupils by teacher and type of primary schools (% of enrollment) , 1907-1940

	Pupils by teacher			Pupils by teacher in state schools			Schools 1940			
	1907	1933	1940	1907	1933	1940	State	Local	Private	Federal
Acre			29.3		35.3	29.2	34.4	58.3	7.3	0.0
Alagoas	36.7	44.4	41.7	42.1	49.4	41.2	52.0	21.9	26.1	0.0
Amazonas	16.2	20.2	36.8	16.3	19.2	38.3	74.3	10.7	15.0	0.0
Bahia	30.1	32.9	51.6	44.8	37.7	58.3	82.8	4.9	12.1	0.1
Ceará	30.6	42.1	35.7	38.7	46.2	37.0	66.8	22.1	11.0	0.0
Distrito Federal	22.5	33.6	37.0				0.0	57.9	41.2	0.8
Espírito Santo	30.7	41.2	41.6	34.6	43.3	43.1	84.7	9.5	5.8	0.0
Goiás	28.3	36.7	35.3	27.9	41.6	46.6	49.4	31.5	18.3	0.8
Maranhão	38.2	37.0	42.2	44.7	39.5	44.8	32.5	46.7	20.8	0.0
Minas Gerais	39.0	40.1	37.1	59.1	40.6	35.7	55.1	37.6	7.3	0.0
Mato Grosso	27.3	33.6	39.2	42.9	34.1	38.6	64.2	10.7	25.1	0.0
Pará	27.4	42.1	52.6	34.4	45.1	52.6	81.4	0.0	18.6	0.0
Paraíba	32.7	51.1	47.3	45.6	57.8	52.5	70.0	0.0	30.0	0.0
Pernambuco	30.8	40.2	39.0	44.7	42.4	40.6	25.6	39.8	34.6	0.0
Piauí	32.3	40.9	48.8	45.1	45.8	53.5	75.7	9.2	15.2	0.0
Paraná	33.3	35.9	33.6	40.5	37.0	33.3	82.4	6.6	11.0	0.0
Rio de Janeiro	31.0	45.8	48.3	47.1	45.0	47.2	60.0	28.2	11.8	0.0
Rio Grande do Norte	40.1	54.2	49.3	48.7	56.7	51.2	66.5	8.5	24.8	0.2
Rio Grande do Sul	36.8	38.1	37.7	49.6	41.0	34.4	31.5	23.2	44.0	1.3
Santa Catarina	33.2	42.9	44.5	42.6	49.7	45.5	59.8	32.5	7.7	0.0
Sergipe	27.1	38.8	39.4	32.5	41.0	43.2	76.5	9.1	14.4	0.0
São Paulo	27.4	37.3	42.9	31.7	39.2	44.0	57.4	17.8	24.8	0.0
TOTAL	31.0	38.6	40.8	42.2	40.9	42.3	**57.1**	**22.7**	**20.0**	**0.2**

Table 5. Expenditure on education per capita at State Level. 1901-1926. The dependent variable is the logarithm of the state governments expenditure per capita in education. Regressions test the hypothesis that revenues per capita derived by exports explain the capacity of the states to provide education. A positive coefficient on export tax revenue per capita support our hypothesis that states with endowments that yielded higher export revenues were able to spend more on education. Specifications 7 through 12 include state-specific trends, and 13 and 14 regional specific trends. Variables are in logarithms, so the coefficient is an elasticity. Robust state cluster standard errors shown in parenthesis. Coefficients marked with: *** indicates significant at 1%, ** at 5% and * at 10%

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Fixed effects and year dummies						Fixed effects, year dummies, and state-specific trends						Region-specific	
	L(Education)						L(Education)						L(Education)	
					No coffee	No rubber					No coffee	No rubber		
L(Exports Revenue)	0.637***	0.345***	0.271***	0.270***	0.274***	0.151**	0.434***	0.182*	0.142*	0.131*	0.155*	0.119*	0.154**	0.422***
	(0.10)	(0.10)	(0.08)	(0.07)	(0.07)	(0.06)	(0.07)	(0.09)	(0.07)	(0.07)	(0.08)	(0.07)	(0.057)	(0.115)
L(Import)			0.239***	0.144*	0.180*	0.057			0.121***	0.075	0.125	0.053	0.070	0.084
			(0.08)	(0.07)	(0.09)	(0.06)			(0.03)	(0.07)	(0.08)	(0.07)	(0.054)	(0.093)
L(Population Density)			0.166	0.256	0.285	0.043							0.662	0.048
			(0.10)	(0.27)	(0.29)	(0.29)							(0.715)	(0.104)
Sugar Share				-0.321**	-0.372***	-0.255*				-0.219	-0.222	-0.198	-0.369***	-0.307
				(0.11)	(0.11)	(0.12)				(0.17)	(0.17)	(0.17)	(0.118)	(0.187)
Coffee Share				-0.32	-0.774	-0.308				0.223	1.403	0.208	-0.297	0.328
				(0.29)	(1.10)	(0.31)				(0.19)	(1.44)	(0.19)	(0.320)	(0.872)
Cotton Share				0.084	-0.001	0.165				0.034	-0.007	0.059	0.042	-0.367*
				(0.17)	(0.18)	(0.17)				(0.13)	(0.13)	(0.13)	(0.136)	(0.199)
Rubber Share				0.839***	0.770**	0.820**				-0.071	0.126	0.762	0.624*	-0.543
				(0.27)	(0.34)	(0.30)				(0.59)	(0.63)	(0.66)	(0.341)	(0.477)
Cocoa Share				-3.600***	-3.253***	-4.450***				-2.684*	-2.235**	-3.404***	-3.335***	-2.772***
				(0.86)	(0.73)	(0.92)				(1.30)	(0.95)	(0.99)	(0.907)	(0.477)
Tobacco Share				0.376	0.115	0.278				-0.047	-0.136	-0.071	0.204	0.210
				(0.86)	(1.09)	(0.76)				(0.77)	(0.88)	(0.70)	(0.799)	(0.510)
Mate Share				0.271	0.221	0.2				0.1	-0.026	0.176	-0.071	-0.262
				(0.53)	(0.60)	(0.55)				(0.23)	(0.26)	(0.21)	(0.533)	(0.284)
Constant	-3.424***	-4.770***	-5.834***	-5.870***	-5.696***	-6.085***	-4.462***	-6.015***	-6.112***	-6.130***	-6.022***	-6.205***	-4.664**	-4.510***
	(0.56)	(0.63)	(0.53)	(0.73)	(0.79)	(0.78)	(0.446)	(0.588)	(0.403)	(0.446)	(0.609)	(0.412)	(2.079)	(1.005)
Export commodity mix	N	N	N	Y	Y	Y	N	N	N	Y	Y	Y	Y	Y
State fixed effects	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	Y	N
Year dummies	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	Y	Y
State-specific trends	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y	N	N
Region-specific trends	N	N	N	N	N	N	N	N	N	N	N	N	Y	Y
Observations	287	287	257	257	215	230	287	287	257	257	215	230	257	215
R-squared	0.535	0.875	0.885	0.899	0.893	0.898	0.841	0.922	0.930	0.934	0.931	0.930	0.899	0.893
R-squared adjsuted	0.533	0.857	0.867	0.880	0.868	0.876	0.830	0.904	0.913	0.915	0.908	0.908	0.880	0.868

Table 6. Regressions for Expenditure on Education. Panel A reports the second stage estimates with expenditures on education at state level, and Panel B the first stage using commodity international prices index as instrument. Panel C reports OLS estimates already reported in Table 5. Variables are in logarithms, so the coefficient is an elasticity. Robust state cluster standard errors shown in parenthesis. Coefficients marked with: *** indicates significant at 1%, ** at 5% and * at 10%

	1	2	3	4	5	6
	L(Education pc)					
					No Coffee Prices	No Rubber Prices
	Panel A: 2 SLS. L(Education Expenditure)					
L(Export Tax Revenue)	0.735***	0.537***	0.453***	0.354***	0.529***	0.313**
	(0.119)	(0.121)	(0.114)	(0.120)	(0.167)	(0.099)
L(SPRpc - ETRpc)	0.312***	0.188**	0.150**	0.12	0.146**	0.115*
	(0.103)	(0.064)	(0.062)	(0.062)	(0.069)	(0.062)
Observations	272	272	257	257	257	257
R2 Adjusted	0.59	0.87	0.88	0.90	0.88	0.90
	Panel B: First Stage for Export Tax Revenue per capita					
L(Commodity Prices)	-0.428***	0.610**	0.609**	0.559**	0.544**	0.597**
	(0.119)	(0.277)	(0.238)	(0.251)	(0.208)	(0.237)
R2 Adjusted	0.224	0.73	0.789	0.83	0.83	0.84
F statistic	7.8	183.2	133.6	9.7	26.4	24.8
Kleibergen-Papp Stat.	9.8	4.8	6.5	5.0	6.8	6.3
	Panel C: OLS					
L(Export Tax Revenue)	0.637***	0.345***	0.271***	0.270***	0.270***	0.270***
	(0.097)	(0.100)	(0.079)	(0.074)	(0.074)	(0.074)
	Panel D: OLS with Instrument (Simulated Prices)					
L(State Public Revenue)	0.604***	0.315***	0.247***	0.259***	0.259***	0.259***
	(0.095)	(0.095)	(0.080)	(0.079)	(0.079)	(0.079)
L(Commodity Prices)	0.036	0.165**	0.134*	0.068	0.068	0.068
	(0.058)	(0.073)	(0.071)	(0.075)	(0.075)	(0.075)
State and Year Dummies	N	Y	Y	Y	Y	Y
Pop. Density & Imports	N	N	Y	Y	Y	Y
Commodity Share	N	N	N	Y	Y	Y

Table 7.A. Correlations between Expenditures and Education Outcomes (Cross Section). Robust errors in parenthesis. Coefficients marked with: *** indicates significant at 1%, ** at 5% and * at 10%

				Controls			
	Change in Literacy Rate 1890-1940	**% change in primary schools 1890-1940**	**Change in Enrollment 1940/1907**	**none**	**Initial Conditions**	**Change in private enrollment 1940/1907**	**Pop density, imports pc**
Coefficient Avg. expenditure pc education	6.412***	0.815	0.041*	Y			
Coefficient Avg. expenditure pc education	6.608***	1.011***	0.032		Y		
Coefficient Avg. expenditure pc education	6.627***	1.011***	0.032		Y	Y	
Coefficient Avg. expenditure pc education	7.179***	1.540***	0.029		Y	Y	Y

Table 7.B. Reduced Form Estimate. Effects of Commodity Prices on Education Outcomes. Dependent variables are education outcomes. The independent variable of interest is logarithm of our state price indices for three periods. Panel data using three education census years: 1890, 1900, 1920. In this reduced form We test the hypothesis that favorable fluctuations in the international price of commodities increased the expenditure on schooling, which was reflected in higher education outcomes . The expected sign of the coefficient is positive. Coefficients marked with: *** indicates significant at 1%, ** at 5% and * at 10. Robust standard errors in parenthesis. Errors clustered at the state level.

				Controls			
	L(Literacy Rate)	**L(schools)**	**L(Enrollment Rate)**	**none**	**FE**	**FE, macro**	**FE, macro, year dummies**
Coefficient of L(simulated Prices)	0.207**	0.647***	0.329***	Y			
Coefficient of L(simulated Prices)	0.340***	0.576***	0.340***		Y		
Coefficient of L(simulated Prices)	0.278***	0.214**	0.272***		Y	Y	
Coefficient of L(simulated Prices)	-0.068	-0.104	-0.073			Y	Y

Table 8. Public Goods Expenditures per capita at State Level and Colonial Institutions. 1901-1926. In this table we replicate our OLS estimates and add an interaction term of export tax revenue with the share of slaves in 1872, population density at the time of colonization, the average size of rural establishments in 1920, and a dummy for good commodities that follows Bruhn and Gallego (2007). These interactions try to measure how important were colonial institutions as initial conditions to explain the pattern in public expenditure. Robust cluster standard errors shown in parenthesis. Coefficients marked with: *** indicates significant at 1%, ** at 5% and * at 10%. Standard errors clustered at the state level.

	(1) Education pc	(2) Education pc	(3) Education pc	(4) Education pc
Export Tax Revenue pc	0.04970**	0.06449***	0.01672	0.07033
	(0.02062)	(0.01135)	(0.01722)	(0.16387)
Exports Revenue pc*Slave share in 1872	0.24157			
	(0.38649)			
Exports Revenue pc*Native population pre colonial		-0.00164		
		(0.01015)		
Export Revenue pc*Dummy Good Commodity			0.04745**	
			(0.01987)	
Exports Revenue pc*Average Size of Rural Establishment in 1920				-0.00699
				(0.16574)
State fixed effects	Y	Y	Y	Y
Imports pc, pop. density	Y	Y	Y	Y
Observations	257	257	257	257
R^2	0.918	0.915	0.916	0.915
R^2 Adj.	0.901	0.898	0.899	0.898

Note. The average and standard deviation (in parenthesis) for each institutional variable is as follows: slave share in 1872=0.05(0.07); precolonial native population (inhabitants per km2)= 3.1 (2.6); dummy for "good commodity" during colonial times= 0.45(0.51), and average size of rural establishment in 1920(has)= 630(1246)

Table 9. Voters growth and Education Expenditure

Dependent variable:	(1) Log (voters 1930- voters 1875)	(2) Log (voters 1930- voters 1894)
L(Education pc)	0.411*	0.409**
	(0.193)	(0.152)
Voters 1875/ literate male 1872	-1.373***	
	(0.320)	
Voters 1894/ literate male 1890		-0.948*
		(0.484)
Constant	Y	Y
Macro controls	Y	Y
Dummy for outlier (Amazonas)	Y	Y
Literacy Rate 1890	Y	Y
Observations	20	17
r2_a	0.697	0.550
R-squared	0.761	0.663
F	11.95	5.889
p-value Test F	0.000145	0.00735

Table 10. Education Expenditures per capita at State Level. 1901-1926. The dependent variable is the state expenditure per capita in schooling. Regressions look at the effects of interaction terms between export tax revenue per capita and immigration and industrialization indicators from different census. All the interacted variables were normalized with mean 0 and standard deviation 1. Monetary variables are in 1913 reis. Robust standard errors shown in parenthesis (clustered at the state level). Coefficients marked with: *** indicates significant at 1%, ** at 5% and * at 10%

	Dependent Variable: Expenditure on Education per capita									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables interacted with Export Tax Revenue:	**% of Foreigners 1890**	**% of Foreigners 1920**	**# of Industries in 1907**	**# of Industries in 1920**	**# of Industries in 1940**	**Industrial Production in 1907**	**Industrial Production in 1920**	**Industrial Production in 1940**	**Production growth 1920/1907**	**Production growth 1940/1907**
ETR pc	0.106***	0.069***	0.033**	0.054***	0.053***	0.052***	0.056***	0.054***	0.057***	0.060***
ETR pc interacted with (see columns):	-0.070**	-0.047	-0.111**	-0.020***	-0.019**	-0.039*	-0.021***	-0.020**	-0.021***	-0.024***
Pop. density & imports	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State and year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Constant	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	257	262	262	262	262	262	262	262	262	262
r2_a	0.923	0.927	0.931	0.923	0.923	0.922	0.924	0.923	0.923	0.924

Table 11. Actual Education Outcomes Using Census Data from 1960

	Age of cohort in 1910 (1960)	Age of cohort in 1920 (1960)	Age of cohort in 1930 (1960)
	6-10 (56-60)	6-10 (46-50)	6-10 (36-40)
Literacy rate (%)	**44.8**	**51.5**	**56.2**
Whites	55.3	62.0	67.0
Blacks	21.7	27.6	33.1
Mixed race	26.8	33.5	37.7
Completed elementary education (% of cohort)	**2.5**	**3.2**	**3.5**
Whites	3.6	4.5	5.1
Blacks	0.2	0.4	0.5
Mixed race	0.5	0.6	0.9
Completed up to fourth grade	**10.3**	**11.8**	**13.2**
Whites	14.1	15.7	17.3
Blacks	3.3	4.2	5.8
Mixed race	3.3	4.6	5.5
Never attended school (% cohort)	**59.9**	**53.1**	**48.1**
Whites	49.8	42.6	37.3
Blacks	81.4	75.7	70.2
Mixed race	77.2	71.1	66.5