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Historical Origins of Schooling: The Role of Democracy
and Political Decentralization

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1 INTRODUCTION

Educational attainment varies widely across countries. Among the former colonies, the adult population in Ethiopia and Mali had on average one year of schooling between 1985 and 1995, whereas the adult population in the Neo-Europes (Australia, Canada, New Zealand, the US) had more than 10 years of schooling in this period. Median years of schooling in former colonies was slightly more than 4 years in the same time period. Even within narrower groups of countries differences are significant. For instance, among former British colonies the Neo-Europes coexist with countries such as Bangladesh and Sierra Leone that have two years of schooling on average. While the adult population of Sri Lanka had on average 6 years of schooling in 1985-1995, in neighboring India average years of schooling were only 4. Moreover, differences in schooling often predate the present. In 1900 the primary enrollment rates in the Neo-Europes were above 85% and in countries such as Haiti, Morocco, and Vietnam were less than 2%. While India's enrollment rate was 4% in 1900, Sri-Lanka's was 22%.

Why does schooling vary widely across countries? Why are differences in schooling highly persistent? In this paper, I study the connection between historical variables, political institutions, and educational outcomes in former colonies. Theoretically, I argue that historical variables determine the distribution of political power among different agents and affect the political institutions established in the past. These institutions present a high degree of inertia and affect educational institutions and outcomes. I argue that two important political institutions that affect schooling are democracy and *local* democracy (decentralization of political power).

To test these hypotheses empirically, I use settler mortality (Acemoglu et al., 2001), population density in 1500 (Acemoglu et al., 2002), and pre-existing factor endowments (Engerman and Sokoloff (1997, 2002) as proxies for the historical factors that affect political institutions. In addition, I use the number of native cultures before colonization as a source of exogenous variation for political decentralization. The number of native cultures before colonization affected the colonization strategy in each former colony. Colonizers tended to establish (or take up) centralized states in colonies with one (or no) strong ethnic group(s) and relatively decentralized governments in colonies with several ethnic groups. Current political structures resemble at least partially these initial structures. Thus, I expect areas where colonizers established more centralized states to have more centralized states in the present. By using the number of native cultures before colonization as an additional instrument for political decentralization, I am able to disentangle the effects of two political institutions on schooling.

My results suggest that conditions faced by colonizers and pre-existing factor endowments affected the characteristics of educational systems established in the past. Cross-country differences in schooling levels persist to the present because colonial factors influence some determinants of schooling such as the extent of democracy and decentralization of political power and the general level of development of a country. My empirical estimates, using two sources of exogenous variation to identify the effects of different political institutions on schooling, imply that the degree of democratization positively affects the development of primary education, whereas the decentralization of political power is the most important explanation for differences in higher levels of schooling, such as secondary and higher education. In turn, results suggest that political decentralization is correlated with the decentralization of school management at the local level, and that this variable has a positive impact on current levels of schooling. These results confirm my hypothesis that while democratization should be more relevant for variables related to the quantity of education (such as primary enrollment), decentralization of political power should be more related with variables capturing differences in the quality of education (such as years of schooling or secondary and higher enrollment). In addition, my results also identify a positive causal effect of per-capita GDP on schooling capturing income and other effects on schooling related to the level of development of a country.

These results for the effect of local democracy on schooling give support to the theories that emphasize the importance of decentralization in the provision of goods such as education (See Oates, 1972 and Inman and Rubinfield, 1997). In addition, by using evidence from a comprehensive sample of former colonies and instrumental variables, I give broader support to historical papers underlining the role of decentralization in the expansion of primary schooling in the 19th and 20th centuries (See Lindert, 1999 for Europe and the US, Engerman et al., 1997 for the Americas, and Goldin and Katz 2003 for the US). In addition, my results about the effects of political institutions on schooling are related to the literature on the determinants of the quality and quantity of education. The literature suggests that the link between resources spent on education and *quantity* of education is stronger than the link between resources and *quality* of education. While a salient feature of democracies is the ability to increase public expenditure in areas such as education, political decentralization tends to raise local pressures to increase the efficiency of educational systems. This argument implies that democratic countries may have better access to education and that decentralized democracies may have a stronger effect on the efficiency of the educational system. My results support this view.

It is worth mentioning the main limitations of my analysis and the way I deal with them. By using two different sources of exogenous variation, I assume that I can disentangle two specific dimensions of political institutions: democracy and local democracy. I use settler mortality and population density as instruments for democracy, but they were meant to work not simply through this particular political dimension, but through a broad cluster of institutions (which may also include political decentralization). If so, my actual estimate of democracy may be actually overestimated (underestimated) if the other dimensions of institutions affect positively (negatively) schooling, or it may be underestimated if the instruments are not powerful enough to disentangle between democracy and local democracy. Similarly, it may be the case that my instruments work through other channels different than democracy or local democracy. In order to deal with these concerns (i) I present over-identification tests for all regressions, and (ii) I implement a number of robustness exercises to deal with potential biases in the estimates by controlling for variables that capture other potential channels through which history may affect schooling. In general, my estimates of the effects of political institutions tend to be quite stable and, for instance, my estimate of the lower bound of the effect of political decentralization on average years of schooling is about 70% of my basic estimates and the upper bound of the estimate of democracy on average years of schooling is about 50% of the estimated effect of local decentralization on average years of schooling (and only marginally significant, with a p-value of 0.22).

The paper is organized as follows. Section 2 briefly presents theoretical and historical background about the determinants of schooling. Section 3 describes the empirical strategy I implement in this paper. Section 4 discusses the data used in this paper. Section 5 analyzes the relationship between historical factors and schooling. Section 6 tries to disentangle the role of political decentralization and democracy on schooling. Section 7 presents some robustness checks and section 8 briefly concludes.

2 THEORETICAL AND HISTORICAL BACKGROUND

A series of theories relates human capital accumulation to government action and institutions. A first group of arguments emphasizes the role of public policies in overcoming such market failures as credit constraints in financing education. A second group of arguments emphasizes that the implementation of educational policies depends on political institutions. The higher the level of enfranchisement, the greater public expenditures on education. A third line of research stresses collective action problems in the provision of schooling. For instance, inequality and fractionalization lower the level of educational

expenditure (Engerman et al 1997, Galor et al., 2008). In addition, several authors highlight the idea that less centralized governments tend to provide better education (Oates, 1972; Inman and Rubinfeld, 1997; Engerman et al., 1997; Lindert, 1999 and 2002). In contrast, some papers argue that decentralization can create inefficient provision of education (See Haggard, 1999, Bardhan, 2002, Kremer et al., 2002, and Gennaioli and Rainer, 2004). For instance, decentralization in the absence of local checks and balances could allow local elites to capture the local government and block the provision of public goods or to channel expenditures towards their members.¹

Other papers studying the effects of different educational systems on school outcomes using microeconomic evidence find that giving more management autonomy to schools produces positive effects on educational results; while at the same time, centralized allocation of resources have a positive impact on school quality (Fuchs and Woessman, 2007). Thus, this discussion suggests that while democratization should be more able to increase the resources used in education and expand the quantity of education, decentralization of local power and of the management of schools may be more related to the quality of education.²

These explanations propose a number of patterns affecting the level of schooling of a country that are related to institutional factors having historical roots. This connection suggests a link between theories explaining a country's social organization using colonial factors and theories explaining the development of educational institutions. Several papers have related colonial or historical factors to schooling and educational institutions.³ First, Acemoglu et al. (2001) mention that educational policies are part of the cluster of institutions established by colonizers that persist to the present and that human capital accumulation is a consequence of the development of democratic and neo-European

¹The contrast between Sierra Leone and Botswana is an interesting case that illustrates the relevance of decentralization. Reno (1995) shows that in Sierra Leone after independence, some state initiatives to expand and decentralize social services were ineffective because corrupt and autocratic chiefs controlled local governments. In 1985-1995, the adult population of Sierra Leone had an average of 2.1 years of schooling. In contrast in Botswana from colonial times, there were democratic chiefs bound by local checks and balances. Not surprisingly, Botswana had an average of 5 years of schooling among the adult population in 1985-1995 and from the beginning of independence has invested significant resources in education, health and other social services (Acemoglu et al, 2003).

²Some authors show a positive correlation between measures of school quantity and school quality (eg., Barro and Lee, 2001a) and other papers present evidence of a causal impact of school quality on school quantity (eg., Hanushek et al., 2006). Therefore, variation in advanced levels of schooling is probably closely related to variation in school quality.

³There are other social characteristics affecting schooling. First, some authors suggest the consolidation of mass schooling during the 20th century is related to the consolidation of national identity of several countries (Meyer, et al., 1992; Ramirez and Boli, 1987). Second, other factors such as religion and cultural heritage can affect schooling because various civilizations and faiths put different emphases on formal instruction (Engerman et al., 1997; Lindert, 2002).

social structures. Engerman et al. (1997) and Engerman and Sokoloff (2002) present empirical evidence showing that suffrage institutions in the early 1900s are associated with schooling. Easterly (2002) and Galor et al. (2008) provide evidence that income and land inequality have a negative impact on human capital accumulation, respectively.⁴ Gallego and Woodberry (2008) argue and present evidence that policies and regulations established in the past that increased competition between Protestant and Catholic missionaries increased schooling in former colonies.

I follow a subset of these papers in assuming that the education system was one of the dimensions affected by European colonization, which is, in turn, assumed to be an exogenous shock (Acemoglu et al., 2001, 2002, and 2006, and Engerman and Sokoloff, 1997 and 2002). I depart from these papers in three dimensions. First, I study explicitly the relationship between history, political and educational institutions, and schooling. Second, I propose a new specific mechanism through which the conditions faced by colonizers affected the type of institutions established in the past, namely the fact that colonizers tended to establish decentralized institutions in areas with more than one native culture. Third, this additional source of variation in institutional design allows me to disentangle between the effects of two dimensions of *political institutions* on schooling.⁵

The main rationale of my theory implies that European colonizers settling in an area were more willing to spend resources in instruction for their children and for the native population. In contrast, extractive colonizers are not interested in investing in an activity that has low and uncertain returns. Extractive colonizers settle in high mortality areas with profitable opportunities in producing crops or minerals with large economies of scale in native (and illiterate) populations. Then, educational investments have low returns for the powerful elite that makes most of the public policy decisions.

Colonizers also established political institutions that were consistent with the distribution of political power that they faced. Colonizers established centralized administrations in areas with one (or no) indigenous group(s) but tended to establish decentralized institutions in areas with more than one ethnic group with power. This is an important extension with respect to the current empirical literature on the effects of colonization on political institutions (Acemoglu et al., 2001, 2002).⁶

⁴As I discuss below in my sample of former colonies I do not find a significant relationship between historical variables and country endowments and different measures of inequality and, therefore, I discard income inequality as a mechanism explaining the effect of history on schooling. Bruhn and Gallego (2008) and Nunn (2008) find similar results using different datasets.

⁵The seminal paper by Acemoglu and Johnson (2005) tries to unbundle the effects of two different *economic* institutions – “property right institutions” and “contracting institutions” – on development.

⁶The conventional wisdom is that the British tended to establish more decentralized structures of

Political institutions established by colonizers also affect educational institutions because inclusive institutions are more democratic and locally decentralized and give the masses the political power to demand and receive education. Finally, political institutions also affect individuals' willingness to invest in human capital.⁷ Institutional settings assuring the respect for property and civil rights provide an incentive to accumulate human capital (i) directly, because there is less (political) income uncertainty, in the sense that expropriation is less likely, and (ii) indirectly, because these institutions provide more incentives to the accumulation of other forms of capital that are complementary with human capital.⁸

Lindert (2002) emphasizes differences in the extent of the franchise among India, Pakistan, and Sri Lanka as sources of divergent educational development. While Britain gave Sri Lanka universal adult suffrage in 1931 (including provincial elections in 1931 and 1936), India received only a very limited franchise in 1919. Lindert (2002) relates these disparities in political power to educational results in the three countries. Whereas Sri Lanka had a primary enrollment rate of more than 50% in 1935-40, India had an enrollment rate of less than 15% in the same period. However, these developments do not necessarily reflect a causal connection between electoral rights and schooling because in 1900 (before the formal extension of the franchise) schooling differences were also significant. While Sri Lanka had a primary enrollment rate of more than 20%, India had an enrollment rate of less than 5%.⁹

colonial government than the French. However, the evidence suggests that there is some variation within this general practice. For instance, Ollowu and Wunsch (2004) describe decentralization in several cities controlled by the French in the presence of various ethnic groups starting with the municipal law of 1884; Brown and Roger-Luois (1999) and Herbst (2000) argue that the British applied the indirect rule system in areas in which local groups were more powerful.

⁷Nugent and Robinson (2001) compare differences in the paths of development of two groups of coffee producers: Colombia-Costa Rica (CRC) and El Salvador-Guatemala (ESG) and shed light on the incentives to establish educational institutions in different contexts. While CRC developed smallholder economies, ESG developed plantation economies. Nugent and Robinson (2001) develop a model in which the divergence in development outcomes between these groups of countries is a result of the lack of incentives to accumulate human capital in the plantation economies, where peasants are held up as a consequence of the monopsony power of landowners. This equilibrium is supported in ESG because of the lack of political competition. In contrast, CRC had elites that were more polarized and competitive than ESG and were more oriented toward mercantile activities.

⁸Krusell et al. (2000) argue that there is a high degree of complementarity between human and equipment capital. Clemens and Williamson (2000) present historical evidence showing that the fraction of population enrolled in schools in the early 1900s had a significant effect on British capital inflows going to different countries. Galor and Moav (2006) construct an argument and present supportive evidence in which the complementarity between human and physical capital created an incentive for the capitalists to support the development of mass education in Britain over the 19th century.

⁹The dissimilarity likely arises from the Colebrooke-Cameron Reforms instituted in Sri Lanka during the first half of the 1800s. These reforms unified the country and gave power and political participation to local citizens. The reforms can be interpreted as an exogenous shock to the country's institutions

Disentangling between two dimensions of *political institutions* of a country is important because even though decentralization and democracy are correlated, the correlation is far from being perfect. For instance, Lindert (1999) stresses the similarities and differences between Prussia and the United States in the early 1900s and strongly stresses the role played by school decentralization in schooling outcomes before 1914. Both countries had very different political regimes, while Prussia was a central autocracy, the United States was a democracy. But Lindert argues that both countries left its schooling more to local forces and this explains that both countries led in mass education.¹⁰

A simple way to illustrate the relationship between current educational institutions and outcomes and historical factors is to show that early and current educational institutions and outcomes are related, i.e., there is inertia in schooling. Many authors have emphasized the existence of inertia in institutional performance (e.g., Acemoglu et al., 2001), but delineating this persistence has been difficult. There are several reasons why institutional persistence is plausible in the case of education. Firstly, as pointed out by Acemoglu et al. 2001, setting up institutions is costly, the gains of the extractive strategy are shared among the small elite, and there are irreversible complementary investments. This means that educational policies, as part of a *long lasting* and *multidimensional* cluster of institutions, are persistent. Secondly, intergenerational inertia creates persistence in educational levels among members of several cohorts. Thirdly, the accumulation of human capital is endogenous. Increases in the supply of education make investment in human capital-related technologies more profitable which, in turn, encourages schooling (Acemoglu, 2002). Finally, peer effects can explain low levels of education over several generations even though there are policies aiming to expand schooling. I present evidence that cross-country differences in schooling are highly persistent in Section 5. Clemens (2004) also presents evidence of a high degree of inertia of schooling in different countries.

3 EMPIRICAL FRAMEWORK

Using the theoretical and historical background described above, I develop in this section an empirical investigation of the effects of historical factors on schooling. My main

that produced a number of differences in the extent of the franchise, schooling, and other institutions compared to India. This historical event probably explains the success of Sri Lanka in expanding the franchise vis-à-vis India and Pakistan.

¹⁰Several authors argue that the impressive performance in terms of schooling of former colonies like Australia, New Zealand, and the US around 1900 is related to school autonomy and decentralization. See Engerman et al. (1997) and Goldin and Katz (2003) for Canada and the United States, and Shaw (1967) for Australia.

hypotheses are the following:

- First, educational outcomes and institutions are persistent and, therefore, differences among countries in levels of schooling can have historical origins.
- Second, certain characteristics of the countries that are historically given affect the political institutions, such as democracy and local democracy, established by these countries in the past.
- Third, among these institutional characteristics, democracy may be more correlated with measures of quantity of education (e.g., primary enrollment rates) and decentralization of political power with measures of quality of education (e.g., average years of education, secondary and higher education enrollment rates).

In this section I describe the empirical methodology I use to study these hypotheses. I use two basic approaches to analyze the effects of historical factors on cross-country differences in schooling. First, I study whether historical factors are correlated with current and past levels of schooling. To do so, I first run reduced form equations of the following form:

$$S_i = \mathbf{Z}'_i\alpha + \mathbf{X}'_i\beta + e_i, \quad (1)$$

where i refers to country, S is a schooling indicator, \mathbf{Z} is a vector of historical variables (settler mortality, population density in 1500, factor endowments less and more favorable to development, and presence of various natives cultures before colonization), \mathbf{X} is a vector of control variables (religion shares, the national identity of the colonizer and, in most regressions, GDP per capita), and e is an error term. Equation (1) correlates schooling with historical factors, without considering the specific mechanisms that could explain the association.

Next, I study how political institutions mediate the effects of colonial factors on schooling. To do so, I estimate the following system of equations using two-stage least squares:

$$S_i = \gamma_1 Y_{1i} + \gamma_2 Y_{2i} + \mathbf{X}'_i\delta + u_i, \quad (2)$$

$$Y_{1i} = \mathbf{Z}'_{1i}\eta_{11} + \eta_{12}Z_{2i} + \mathbf{X}'_i\theta_1 + \epsilon_{1i}, \quad (3)$$

$$Y_{2i} = \mathbf{Z}'_{1i}\eta_{21} + \eta_{22}Z_{2i} + \mathbf{X}'_i\theta_2 + \epsilon_{2i}, \quad (4)$$

where Y_1 is a measure of democracy, Y_2 is a measure of political decentralization, \mathbf{Z}_1 is a vector of instrumental variables for Y_1 and Y_2 (settler mortality, population density in

1500, and factor endowments less and more favorable to development) that allows me to identify an exogenous source of variation for Y_1 and Z_2 is an instrumental variable for Y_2 (the number of indigenous cultures before colonization started) that allows me to identify an exogenous source of variation for Y_2 . In particular, as I show later, η_{12} is equal to zero in all regressions. \mathbf{Z} will be a valid instrument for Y as long as it is uncorrelated with u . Put it differently, the key exclusion restriction is that in the population $Cov(u_i; Z_{1ji}) = Cov(u_i; Z_{2i}) = 0$ for $j = 1, \dots, 4$ (i.e., the variables included in $\mathbf{Z}'_1 = [Z_1 \ \dots \ Z_4]$). An over-identification test is a useful approximation to check this set of conditions.¹¹

The motivation for this strategy is that I use two different sources of exogenous variation that allow me to unbundle the contribution of two different political institutions on schooling. By doing so, I am able to identify specific mechanisms for the effect of political institutions on different educational outcomes.¹²

In addition, the level of development of a country may also be correlated with schooling outcomes. Thus, I control for GDP per capita to capture the effect of income and other omitted variables related to the effects of development on schooling.¹³ In addition, in standard production functions, increases in GDP per-capita are related to increases in capital per capita and therefore this term may capture any causal effect of physical capital on schooling due to complementarities, for instance. In this sense, the inclusion of income should take into account explanations such as the ones proposed by Galor and Moav (2006).

In order to deal with a potential endogeneity problem because schooling also may affect per-capita GDP and the instruments are also related to this variable, I use terms of trade shocks as an instrument for the level of income in 1995.¹⁴ As a robustness check I also include per-capita GDP as an exogenous variable in a regression. I interpret this estimate as an upper bound of the effect of democracy on schooling because if measurement error is present in both income and democracy, and both variables are

¹¹The over-identification test is a Lagrange multiplier test statistic that, under the null hypothesis, is distributed χ^2_Q , where Q equals the number of excluded exogenous variables minus the number of endogenous variables included as regressors in (1). Hall and Jones (1999), Acemoglu et al. (2001,2002), Easterly and Levine (2003), and Persson (2005) among others also use an over-identification test to study the validity of using historical variables to explain current institutions.

¹²Acemoglu (2005) discusses the idea of unbundling institutions in the general context of comparative political economy and, as previously discussed, Acemoglu and Johnson (2005) apply this idea to distinguish the effects of different economic institutions on cross-country differences in economic development.

¹³See Banerjee (2004) for a theoretical rationale for the effects of income on schooling that go beyond the traditional argument emphasizing the existence of liquidity or borrowing constraints

¹⁴Specifically, I compute terms of trade shocks as the ratio of the average level of terms of trade in 1985-1995 with respect to 1960-1984.

related to the instruments, when I treat income as exogenous I expect the coefficient on democracy to increase and the coefficient on income to decrease *with respect to a situation in which both variables are instrumented*.

There are a number of concerns related to my identification assumptions that I deal with in the empirical implementation of this strategy. For instance, I assume that the instruments are only related to my institutional variables, conditional on the vector of controls. The over-identification test is a statistical way of testing this. However, these tests have in general low power and therefore I present a number of robustness exercises: (i) I present regressions controlling for other historical variables that may be related to the effect of historical variables on schooling, such as ethnic fractionalization, land inequality, and the presence of Christian missionaries in the past; (ii) I use two alternative indicators of institutions: the Gastil index—an alternative measure of democracy—and expropriation risk—a measure of institutions including more dimensions. The main qualitative results of the paper do not change when implementing these exercises.

4 DATA

I use a dataset including more than 50 former colonies. Table 1 presents descriptive statistics and data sources.

My indicators of current educational attainment are the average years of schooling of the population above 15 years of age (from Barro and Lee, 2001b and Cohen and Soto, 2007) and primary, secondary, and higher education enrollment rates from the Global Development Network Growth database in 1985-1995.¹⁵ To measure schooling in 1900, I use data from Benavot and Riddle (1988) on gross primary enrollment rates for a sample of countries.

My main indicator of democracy is institutionalized democracy in 1900 and 1985-1995 from the Polity IV data set.¹⁶ Political decentralization is an indicator of the extent of local democracy and local political power. This variable is constructed using information from Beck et al. (2000) and the Polity IV data set. Using Beck et al. (2000), I construct a proxy for decentralization that takes a value of 0 if neither the local executive nor the local legislature is directly elected by the local population, 1 if either is directly elected and the other is indirectly elected or appointed, and 2 if both are directly and locally

¹⁵In general I construct contemporary variables using the average for 1985-1995 to make this paper comparable to others using similar datasets, as Acemoglu et al., (2001 and 2002). However, results are robust to using information in 1985 or 1995.

¹⁶In a regression I use the Gastil index of civil rights from Freedom House for 1985-1995 as an alternative measure of democracy.

elected. The decentralization variable in the Polity dataset takes three values: 1 refers to a centralized state (unitary state: no more than moderate decision-making authority is vested in local or regional governments), 2 to an intermediate category, and 3 to decentralized states (federal state: local and/or regional governments have substantial decision-making authority). For 1985-1995, I use the average of the Beck et al. (2000) and Polity normalized indices and the Polity index for 1900.¹⁷

I also construct two measures of the degree of decentralization of education systems using data from the UNESCO World Data on Education databank. The first measure is a dummy that takes a value of 1 if the administration of schools is decentralized to the provincial or municipal level, and 0 otherwise. This measure aims to capture local autonomy to manage schools. The second measure of decentralization aims to capture the degree of financial decentralization of schools. The variable takes a value of 0 if provincial/municipal levels have no financial autonomy to manage and raise school resources, 1 if the local level has some autonomy to manage and raise education funds, and 2 if local levels have complete or almost complete autonomy to raise and manage education funds.

I use two historical colonial variables from Acemoglu et al. (2001, 2002): settler mortality risk and population density in 1500. Settler mortality represents the potential mortality risk faced by colonizers (see Acemoglu et al 2001). Population density in 1500 is a measure of the density of the native population and, therefore, adds information about the colony's labor supply and the opportunities of taking over the pre-colonial tax system and establishing extractive institutions (Acemoglu et al 2002; Engerman and Sokoloff 2002).

I classify a country's agricultural and mineral endowments as "good" for development or as "bad" for development. Easterly (2002) and Easterly and Levine (2003) use a group of 11 dummies to indicate whether a country produced any of a given set of leading commodities (crops and minerals) in 1998-1999. Following the rationale of Engerman and Sokoloff (2002) the commodities less favorable to development are bananas, coffee, copper, rice, rubber, silver, and sugarcane. The commodities more favorable to development are maize, millet, and wheat.

¹⁷Notice that my decentralization index is different from other measures used in the literature. While decentralization is typically measured as the subnational share of total government spending (e.g., Fisman and Gatti, 2002), my indicator is related more to a measure of local democracy. This distinction is important because the lack of local checks and balances is one of the factors that explain why some theories predict a potentially negative effect of decentralization on education and other social outcomes (Bardhan and Mokerjee, 2000; Bardhan, 2002; Gennaioli and Rainer, 2004). My measure combines both centralization of government and local democracy.

I use data from Murdock and White (1969) on the number of indigenous cultures as a measure of the number of ethnic groups living in a country when colonizers arrived. I use a dummy that takes a value of 1 if there was more than one ethnic group and 0 otherwise. Because colonizers established a state that, at least partially, resembled the preexistent distribution of power, societies having only one ethnic culture tended to develop more centralized states.

I proxy for religious and cultural heritage with the share of the population that is Roman Catholic, Muslim, or another non-Protestant religion and with the national identity of the colonizer (British, French, and Spanish). The shares of a religious denomination are from Barrett (1982) for 1900 and La-Porta et al. (1999) for 1985-1995. The identity of the colonizer is from CIA (2002).

5 THE EFFECTS OF HISTORICAL FACTORS ON SCHOOLING

In this section, I study the relationship between schooling and historical factors. I first show that cross-country differences in schooling are persistent. Next, I show that schooling is related to historical factors such as settler mortality, population density, factor endowments, and the number of native cultures before colonization.

Before estimating the reduced form equation (1) I evaluate the persistence of the cross-country variability of schooling. Figure 1 presents the results of regressing education attainment in 1985-1995 on primary enrollment in 1900. Results suggest that there is a high degree of persistence in cross-country differences (even if I do not incorporate the countries with higher levels of enrollment). The regression for the complete sample explains 63% of the cross-country variation in current levels of schooling. The Spearman rank correlation is 0.69 (a test rejects the null hypothesis that schooling variables in the past and today are independent). This evidence suggests that schooling is highly persistent and that its early and current levels are closely related.¹⁸

Table 2 presents reduced form estimates for my main measures of schooling in 1900 (primary enrollment rate) and 1985-1995 (average years of schooling of the adult population, primary, secondary, and higher education enrollment rates), based on equation (1). Odd numbered columns present results without including covariates and even numbered columns present results including covariates (the identity of the main colonizer, and religion variables). Most historical variables are statistically significant considering conventional significance levels and explain a relevant share of cross-country variability

¹⁸As a comparison, Acemoglu et al. (2001) conclude that the degree of persistence of institutions is high when their measures of early institutions explain about 20% of the variability of current institutions (and the rank correlation between both variables is 0.20).

(more than 50%).

Regarding the estimated effects of historical factors, settler mortality, population density in 1500 and the dummy for good factor endowments present the expected signs (i.e., higher settler mortality and population density decrease schooling, and having good endowments increases schooling). Results for the dummy for bad endowments in general present the expected sign but are statistically significant in only about half of the specifications.

Results for the variable measuring the number of cultures before colonization are interesting. This variable has a positive and significant effect only for regressions measuring average years of schooling and higher education enrollment. In contrast, the number of native cultures before colonization is not statistically significant in the regressions for primary enrollment levels both in 1900 and 1985-1995. This evidence brings indirect support to my discussion in Section 2. I argue that the number of native cultures before colonization captures an exogenous source of variation of decentralization of political power. If decentralization is more important for advanced levels of schooling, which, as I argue, are more related to quality of education, I would expect the number of native cultures to be significant only in regressions measuring advanced levels of schooling. The evidence in Table 2 supports this view.

The inclusion of controls for the religious denomination of the population and the national identity of the colonizer does not affect the significance of the effects of historical variables on schooling. Results confirm previous findings that former British colonies and Protestant countries tended to develop more extensive educational systems circa 1900 and that these variables are not correlated with educational outcomes when using modern data (Benavot and Riddle, 1988).¹⁹

As a whole, Table 2 shows that there is a robust and significant relationship between historical factors and schooling variables. For example, after controlling for religion variables, a country having settler mortality in the lower 25% of the distribution has a population with 1.5 additional years of education than a country located in the upper 25% of the distribution of the same variable (column 4). Analogously, a country situated in the lower 25% of the distribution of the population density in 1500 has 1.4 more average years of education than a country in the upper 25% of the distribution. Results for endowments are similar. A country having good endowments has a population with an average of 2.2 more years of education than countries not having good endowments. Countries having “bad” endowments have 1.9 fewer years of education than countries

¹⁹In the following tables I control for both religion and colonizer variables but, in order to save space, I only report p-values for tests for the joint significance of these variables.

not having bad endowments. Finally, countries with more than one native culture at the time of colonization have about 0.8 more years of education.

Overall, evidence in this section reports a strong correlation between historical factors and educational outcomes in 1900 and 1985-1995. The effect of these historical factors may operate through some specific institutional factors such as democratic institutions and political decentralization. The next section studies empirically this hypothesis.

6 POLITICAL INSTITUTIONS AND SCHOOLING: DEMOCRACY OR POLITICAL DECENTRALIZATION?

In this section, I present IV estimates of the relationship between political institutions and educational outcomes. To do so, I estimate the system of equations composed by (2), (3), and, (4) to determine whether (i) there is a significant relationship between democratization and decentralization of political power and the historical variables, (ii) the proposed institutional variable has a significant effect on schooling, and (iii) the effects of the historical factors on schooling do not go beyond their effects on the proposed institutional variable, i.e., the over-identification test confirms that the instruments are valid. In the final part of this section, I study the relationship between political decentralization and measures of decentralization of school management, as an additional test of my theoretical argument.

Table 3 presents IV estimates for educational outcomes in 1900 and 1985-1995 in Panel A and all the relevant first stages in Panel B. I start discussing regressions for 1900. The first-stage results for democracy in 1900 suggest that settler mortality, population density in 1500, and the proxies for factor endowments present the expected signs, but only settler mortality and good endowments are statistically significant. The dummy for the presence of several native cultures before colonization is only marginally related with democracy in 1900. In contrast, the number of native cultures before colonization has a positive and significant impact on my measure of political decentralization in 1900. This evidence validates my theoretical assumption that the number of native cultures captures a different source of exogenous variation than the historical variables stressed by Acemoglu et al. (2001, 2002) and Engerman and Sokoloff (1997, 2002).²⁰ Therefore, this variable allows me to pin down a potentially valid source of exogenous variation that is different from the other historical variables I use as instruments.

Second-stage results in Panel A of Table 3 for 1900 presents estimates of the “horse-

²⁰Moreover, the pairwise correlation between the number of native cultures and the other historical variables is not statistically significant.

race” between the two institutional dimensions, democracy and political decentralization.²¹ Results suggest that what matters more for primary enrollment in 1900 is democracy, which has a positive and significant effect on primary enrollment. At the same time, the political decentralization index is positive but statistically insignificant.

I do not have a measure of per capita GDP to control for in this sample—Maddison (2003) presents data for just about 22 countries included in my sample for c. 1900. In order to control for differences in levels of development I also run regressions including urbanization rates in 1700 from Acemoglu et al. (2002) as a control variable—which includes information for 39 countries—, results are qualitatively very similar to the ones reported in Table 3: the coefficient on democracy in 1900 is 3.08 (and significant at 5%) and the coefficient on decentralization is equal to 5.09 (and not significant).²²

In terms of the economic significance of the impacts, an increase of democracy of one standard deviation implies an increase in private enrollment of about 8.5 p.p. (equivalent to 36% of a standard deviation of primary enrollment in 1900). Thus, the impact of democracy is not only statistically significant but also economically relevant.

Table 3 also presents results of IV estimates using data for average schooling in 1985-1995. First-stage results in Panel B confirm my theoretical assumptions. The presence of native cultures is related only with decentralization (and uncorrelated with democracy and GDP per capita) and terms of trade shocks are correlated with GDP (and marginally related to democracy). Settler mortality, population density and good factor endowments are correlated with democracy.

Second-stage results in Panel A of Table 3 present a pattern that suggests that decentralization wins the race with democracy when analyzing average years of schooling. In addition, per capita GDP has a positive and significant effect on schooling, as expected. In terms of the impact of these variables, IV estimates imply that an increase of one standard deviation of decentralization increases average years of schooling by about 0.8 years (equivalent to 0.32 standard deviations). In turn, a one-standard deviation increase of per capita GDP—equivalent to an increase of about 140 log points—increases

²¹In previous versions of this paper I also included the share of income that goes to the middle class in 1900 and 1985-1995 as a potential variable related to historical factors and schooling. However, historical variables were only marginally significant in the first stage regression and can only account for 10% of the variability of the middle class share. These results suggest that middle class share is not a good candidate as a channel for explaining the effects of colonial factors on schooling in my sample of former colonies. Results are similar if I use the Gini coefficient as the dependent variable. Moreover, if I control for either measure of inequality in the regressions the main conclusions of the paper are unchanged. Results available upon request.

²²Another robustness exercise I run was to assume urbanization rate was equal to 0 for all the missing countries in Acemoglu et al. (2002), again results are quantitatively very similar to results reported in Table 3 (results available upon request).

schooling by 1.5 years (equivalent to about 0.57 standard deviations). Thus, both effects are not only significant, but also economically relevant. Democracy has a much smaller, and non significant, effect: a one standard deviation of democracy increases average years of schooling by just 0.2 years.

As discussed in section 3, there are some potential concerns about my identification strategy. I now discuss some of them and the results of a number of robustness checks. First, there may be the case that historical variables affect schooling through other variables. In this respect, the over-identification tests suggest that there is no correlation between the instruments and estimated residuals, thus giving support to my assumption. Second, I study an alternative definition of democracy: the Gastil civil rights index. Results are reported in column (1) in Table 4. As before, democracy is not significant and the impacts of decentralization and income on schooling remain almost unchanged.

Next, I estimate the model omitting each political variable and study how results change. Columns (2) and (3) of Table 4 present the results. In column (2) I exclude decentralization, in this case both democracy and income increase their impact on schooling and actually democracy becomes statistically significant. This result implies that the positive effect of democracy on schooling found in other papers is due to an omitted variables problem because local democracy is not included in the regressions. In a sense, this result can be interpreted as the effect of political institutions broadly defined. Next, I exclude democracy. In this case while the impact of decentralization increases, the impact of GDP remains basically unchanged. This result, jointly with column (3), is useful to answer the following potential concern: given that the instruments were meant initially not to work simply through democracy, but through a broad cluster of institutions that, in turn, affect income, there may be the case that the non-significant effect of democracy on schooling is due to a colinearity problem between democracy and GDP. Results in the last two columns suggest that this is not the case and that the zero effect of democracy on schooling is more related to a horse-race between democracy and decentralization.

To further analyze this potential bias, in column (4), I exclude the democracy index and include a broader measure of institutions –expropriation risk from Acemoglu et al. (2001, 2002). The effect of decentralization is very similar to my main estimate in Table 3 but now income becomes non-significant. Something similar happens regarding expropriation risk. This is not surprising given that both variables are affected by the same instruments. In fact, a test for the joint significance of both variables strongly rejects the null hypothesis that both variables have a zero effect on income ($p\text{-value} < 0.01$). This

result suggests that it is hard to disentangle between the causal effect of income and the causal effect of institutions on schooling.²³

Column (5) presents another exercise in which I treat income per capita as an exogenous variable. As discussed in section 3, this should decrease the coefficient on income and increase the coefficient on democracy. Results support this prediction. Democracy, even though still non significant (p-value=0.22), increases its standardized impact on schooling from about 0.07 to about 0.16 (and GDP decreases slightly from 0.57 to 0.53). I see this as an upper bound of the effect of democracy on schooling, controlling for decentralization.

Next, columns (6) to (8) of Table 4 include three additional controls of schooling to study whether my results are not driven by alternative explanations. In column (6) I include an index of ethnic fractionalization that may be correlated with decentralization and democracy. Results remain qualitatively unchanged and if anything the impact of decentralization on schooling increases. Next, I include a measure of land inequality—a Gini index of land inequality—to take into account the theoretical insights suggested by Galor et al. (2008).²⁴ Results again are similar. Finally, I include a measure of the penetration of Christian missionaries in the early 20th century to control for theory suggested by Gallego and Woodberry (2008). As before, the main conclusions do not change, even though in this case the impact of both decentralization and income decrease in size, but remain highly significant. One possible explanation for the decrease in the impact of decentralization is that countries with many cultures were more open to the entry of missionaries so there may be a bias by omitting missionaries in the regressions.²⁵ In any case, still the impact of decentralization is positive and implies a standardized effect of about 0.27. I see this as the lower bound of the impact of decentralization on schooling. In sum, these exercises imply that my main results are mainly robust to a set of alternative explanations.

Why do these results are different from what I found in 1900 in that in this case decentralization wins the race against democracy? My main hypothesis is that, as dis-

²³A very similar result is found by Acemoglu and Johnson (2005) when studying the effect of institutions and income on investment rates.

²⁴It is worth mentioning that in my sample a regression of the land Gini index on my historical variables yield significant effects of historical variables but with the opposite signs to what is predicted by most theories: higher population density before colonization and settler mortality are correlated with *lower* land inequality and the presence of good economic activities is correlated with a more *unequal* land distribution.

²⁵At the same time, missionaries is a potentially endogenous variable and therefore, as discussed in Acemoglu et al. (2001), including it in the regressions as an exogenous variable may bias downwards the estimated impact of the instrumented variables in the regression.

cussed in the theory section, while democracy should affect more strongly development of initial instructional institutions and the quantity of education (related to the expansion of primary education), political decentralization should be more important to explain the development of more advanced levels of education (related to secondary and higher education and the quality of education). I study this hypothesis in the remaining columns of Table 4. In column (9) I run regressions using primary enrollment rate in 1985-1995 as the dependent variable. None of the political variables are significant and the magnitude of their effects is small: the standardized impact of both democracy and decentralization are about 0.10 standard deviations of primary enrollment (while the standardized impact of income is about 0.55 standard deviations of primary enrollment). This contrasts with results in columns (9) and (10). In both cases decentralization has a much bigger, and significant, standardized impact on secondary and higher enrollment rates, equivalent to 0.54 and 0.41 standard deviations of the dependent variable, respectively. These impacts are actually of the same order of magnitude than the impact of income on secondary and higher enrollment rates (equivalent to 0.52 and 0.40 standard deviations, respectively). This evidence gives support to my prediction that decentralization should affect more strongly advanced levels of schooling and democracy more strongly initial levels of schooling.²⁶

The theoretical argument I stress for the potential positive effects of political decentralization on advanced levels of schooling relies on the ability of decentralized governments to give more autonomy to schools. This is an important test for my argument. Decentralization of political power may affect different dimensions of the management of the educational system by increasing the proximity between schools and users. This proximity may refer to the ability of educational systems to produce both a better match between curriculum and student needs and structures more accountable and reactive to the voice of users. An alternative interpretation is that more decentralized systems are able to raise more resources from the local level.

Figure 2 presents initial evidence of the partial correlation between my two measures of school decentralization (decentralization of the management of schools and financial

²⁶The positive and significant effect of decentralization contrasts with those of Gennaioli and Rainer (2004), who present evidence that pre-colonial decentralization has a negative effect on the provision of education. Two factors could explain the differences. First, while my measure of decentralization already incorporates the relevance of local checks and balances, their measure captures only the degree of centralization of government (see footnote 17). Second, the interpretation of their results is not straight-forward because their measure of pre-colonial decentralization is difficult to disentangle from a proxy for state/stateless societies. In particular, the data suggest that their measure of pre-colonial decentralization *is not* correlated with current level of decentralization and *is negatively* correlated with measures of current and past democracy and current measures of governance.

decentralization) and schooling, after controlling for democracy and the level of income. Results suggest that only decentralization of the management of schools is correlated with attainment.

I study formally which dimension of decentralization seems to be more related to school outcomes in Table 5. In the first two columns I present IV regressions of both indices of school decentralization on democracy, political decentralization, income, and the other control variables. Results suggest that decentralization of political power has a positive and significant effect on decentralization of school management, but an insignificant effect on decentralization of education finance. Thus, this evidence gives support to the theoretical mechanism I propose. To further study this point, I study whether decentralization of school management affects especially advanced levels of schooling. Columns (3) to (10) in Table 5 suggest that indeed decentralization of the management of schools has a positive and significant impact on educational attainment, secondary and higher education enrollment today. In contrast, estimates for primary enrollment show no effect of decentralization on school management. Consistent with my previous evidence. Estimates for the effects of financial decentralization suggest that this variable has no significant effect on current educational outcomes.

Overall, these results suggest that decentralization of management of schools at the local level is the variable that seems to explain the effect of political decentralization on current levels of schooling. In addition, as previously discussed, these results are consistent with Fuchs and Woessman (2007), who stress the positive effects of decentralization of school management on educational outcomes and find negative or no effects of decentralization of school finance.

7 ROBUSTNESS CHECKS

In this section I present a number of robustness checks and additional exercises. First, I explicitly assume that the causality goes from political institutions to schooling. An implication of this result is that changes in democracy should precede changes in primary enrollment. Panel A of Table 6 presents results along those lines. Taking a sample of former colonies with data for primary enrolment and democracy from 1870 to 1940, I find that changes in democracy precede changes in primary enrollment and that changes in primary enrollment do not precede changes in democracy. Thus, these results provide an indirect test of my assumption supporting the idea that causality comes from political

institutions to schooling and not vice versa.²⁷

Second, Panel B of Table 6 discusses an additional concern about my previous results. Glaeser et al. (2004) argue that differences in human capital that colonizers carried to the colonies is what explains current differences of development across countries. I test this claim by regressing primary enrollment in 1900 and schooling in 1995 on a proxy of human capital carried by colonizers (primary enrollment in 1870 of the colonial power that controlled the country), my group of historical variables and controls for religion variables. Results in panel B do not bring support to this interpretation: my measure of the colonizers' human capital is not significantly related to schooling levels neither in 1900 nor in 1995.²⁰ All in all, this panel rules out the alternative explanation that colonization affects schooling through the colonizers' human capital.

Third, a recent alternative hypothesis offered by Rajan and Zingales (2006) suggests that institutions have no direct effect on measures of development, after controlling for the share of the European population in 1900. They interpret the share of the European population as an indicator of education homogeneity in 1900. Certainly, many alternative interpretations for the same variable are also plausible. For instance, Acemoglu et al. (2001) interpret the same variable as an indicator of the existence of the share of the population wanting to establish a broad set of good institutions. More importantly, Rajan and Zingales (2006) support their claims by including measures of democracy, the share of the European population, and primary enrollment in 1900 as regressors in a regression of educational outcomes in the present. This regression is hard to interpret because at least some of these variables may be jointly determined and affect each other and probably have a lot of measurement error.²⁸ Therefore, an OLS regression on these variables is not identified. One needs to find potentially valid sources of exogenous variation for each dependent variable to identify the causal effect of these historical variables on schooling. In Panel C of Table 6 I implement such an exercise using three plausible instruments for the three endogenous variables: settler mortality

²⁷Glaeser et al. (2004) find that education precedes democracy and not vice versa, using data for 1960-2000. In contrast, Acemoglu et al. (2004) find that education does not foster democracy, using data from 1965 to 2000 when including time fixed effects. I do not have a comparable dataset of my primary enrollment measure after 1940 so I cannot extend the analysis forward. More importantly, my exercises capture a period in which both education and democracy were developed basically from a country perspective. In subsequent periods both variables have been pushed by global and external policies and, therefore, may not accurately capture country-specific variation (even though the time dummies should take care of this, they may not do so perfectly). Clemens (2004) presents a detailed review of international policies aimed to increase education levels in poor countries.

²⁸For instance, measuring the share of the European population is much easier than measuring the number of children that go to school, which is much easier than measuring a subjective concept like democracy.

as an instrument for democracy, population density in 1500 for the share of the European population, and the number of Christian missionaries per capita in c. 1900 for primary enrollment (from Gallego and Woodberry, 2008). Results suggest that, using these three instruments, conclusions change dramatically. Only democracy and primary enrollment have positive effects on years of schooling in the present (effects are in general marginally significant). Moreover, the effects are economically relevant: a one standard deviation increase of democracy in 1900 increases years of schooling by about 2.4 years in the regression including controls (about 3.3 years in the regression without including controls). In contrast, a one standard deviation increase of European population in 1900 increases education today by about 1.3 years (about 1 year in the regression without including controls).²⁹ Overall, evidence in the three panels of Table 6 provide evidence that suggests that institutions established in the past affect educational outcomes today and confirms previous results in this paper.³⁰

8 CONCLUSIONS

My paper shows how history affects schooling differences across countries today. I argue that differences in conditions faced by colonizers had a significant influence on educational policies in the past that persist to the present. Factors such as potential settler mortality, density of native population, the characteristics of factor endowments, and the numbers of native cultures before colonization have a considerable influence on schooling levels observed in 1900 and 1985-1995.

I also present evidence that key channels behind the influence of historical factors on schooling are political institutions (the extent of democracy and political decentralization) and the level of development. By trying to disentangle between the effects of two particular forms of political institutions I confirm some theoretical and empirical findings presented in the literature, but also qualify the traditionally emphasized effect of franchisement and democracy on schooling. In particular, I find that while democracy is a significant determinant of primary or elementary schooling, the degree of decentralization of political power is much more relevant to explain more advanced levels of

²⁹In the first stages (unreported in the text but available from the author), as expected, the instruments I postulate are statistically significant (settler mortality for democracy, population density for the share of the European population, and Christian missionaries for primary enrollment) and the other variables are not statistically significant in the regressions without controls.

³⁰I also run a series of additional robustness checks. Regressions excluding the so-called Neo-Europes and excluding countries with imputed schooling data present similar results to those reported in the main text. I also run regressions using schooling and political data for 1995, instead of using the average for 1985-1995, results are similar and are available from the author.

instruction, such as secondary and higher education. Moreover, I argue that the potential effect of political decentralization on schooling is related to school decentralization. This is an important check of my theory and I find that, consistent with my theoretical arguments, decentralization of school management (i) is affected by political decentralization and (ii) has a significant impact on advanced levels of schooling above primary schooling.

My estimates are not only statistically significant, but also economically relevant. For instance, my estimates imply that higher decentralization of school management in Latin America explains about 40% of the difference in schooling levels with Africa (equivalent to about 3 years of schooling). Similarly, my estimates for the effect of democracy on primary enrollment in 1900 can also account for about 40% of the differences in primary enrollment between African and Latin American countries in the same year (equivalent to 14 percentage points of enrollment).

Does the evidence in this paper imply that history is destiny and schooling levels can not be changed? No. The evidence in this paper suggests that educational institutions are endogenous and related to history. Thus, changing educational institutions is possible, but costly because it implies changing the distribution of political power in a society. The example of Malaysia is an interesting case study. While primary enrollment was close to the median enrollment in 1900, schooling in 1985-1995 was in the upper 25% of the distribution. Malaysia started having elections at the local level in the late 1960s and years of schooling increased sharply in the late 1970s. This example suggests an interesting line of future research: understanding the role of the expansion of local democracy in explaining the experiences of countries having “bad” historical factors, very low levels of educational systems in the past and high levels of human capital in the present. Putting it more broadly, can local power foster schooling even in countries with bad historical conditions?

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Table 1: Descriptive Statistics

	Observations	Average	Median	Standard Deviation	Percentile 5	Percentile 95	Source
Schooling Variables							
Primary Enrolment in 1900	76	17.6	7.9	23.8	0.2	87.3	Benavot and Riddle (1988)
Average years of education in 1985-1995	69	4.6	4.2	2.7	0.8	11.0	Barro and Lee (2001b), Cohen and Soto (2007)
Primary enrollment in 1985-1995	58	66.6	66.9	23.9	2.4	98	GDN Growth Dataset
Secondary enrollment in 1985-1995	58	29.7	26.9	19.7	3.2	77.7	GDN Growth Dataset
Higher education enrollment in 1985-1995	58	6.7	3.5	9.3	0.4	34.9	GDN Growth Dataset
Historical Variables							
Log of Settler Mortality	72	4.6	4.4	1.2	2.7	6.5	Acemoglu et al. (2001, 2002)
Log of Population Density in 1500	72	0.53	0.43	1.71	-2.41	3.17	Acemoglu et al. (2002)
Good Endowments	72	0.69	1.00	0.46	0	1	Easterly and Levine (2004)
Bad Endowments	72	0.81	1.00	0.40	0	1	Easterly and Levine (2004)
Several Indigenous Ethnic groups	75	0.33	0	0.47	0	1	Murdock (1967)
Catholic, 1985	72	40.1	27.3	36.2	0.2	92.4	La Porta et al. (1999)
Muslim, 1985	72	22.3	3.9	33.0	0	99.1	La Porta et al. (1999)
Other (Non-Protestant) Religion, , 1985	72	25.9	20.4	24.5	0.5	84.1	La Porta et al. (1999)
Catholic, 1900	72	28.8	3.2	39.7	0	98.0	Barrett (1982)
Muslim, 1900	72	16.0	0.5	28.5	0	86.6	Barrett (1982)
Other (Non-Protestant) Religion, 1900	72	42.8	34.7	39.3	0.5	99.4	Barrett (1982)
British	76	0.37	0	0.49	0	1	CIA
French	76	0.28	0	0.45	0	1	CIA
Spanish	76	0.24	0	0.42	0	1	CIA
Institutional and Economic Variables							
Democracy in 1900	76	1.1	0	2.46	0	9	Polity IV
Democracy, 1985-1995	59	4.4	4.0	3.49	0	10	Polity IV
Gastil Index, 1985-1995	59	0.53	0.53	0.26	0.18	1	Freedom House
Political Decentralization, 1985-1995	69	0.03	-0.16	0.86	-0.85	1.97	Beck et al. (2000) and Polity IV
Political Decentralization, 1900	76	1.15	1	0.49	1	3	Polity IV
Decentralization of school management	65	0.32	0	0.47	0	1	UNESCO World Data on Education
Decentralization of school finance	65	0.43	0	0.68	0	2	UNESCO World Data on Education
Ethnic Fractionalization	76	0.54	0.59	0.25	0.06	0.88	Alesina et al. (2003)
Gini Land	61	0.64	0.65	0.17	0.34	0.87	Deininger and Olinto (1999), Frankema (2006), Alesina and Rodrik (1994), and Muller and Seligson (1987)
Log Per-Capita GDP 1995	76	7.18	6.99	1.42	5.26	10	GDN Growth Dataset
Terms of Trade Growth (ratio)	63	1.14	1.10	0.26	0.77	1.71	GDN Growth Dataset

Table 2: Reduced-Form Estimates: Educational Outcomes in 1900 and 1985-1995

Dependent Variable:	Primary Enrollment in 1900		Average Years of Schooling in 1985-1995		Primary education enrollment in 1985-1995		Secondary education enrollment in 1985-1995		Higher education enrollment in 1985-1995	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log of settler mortality	-7.88*** (2.00)	-1.75 (1.70)	-1.21*** (0.17)	-0.98*** (0.17)	-13.07*** (2.20)	-9.27*** (1.89)	-10.10*** (1.86)	-8.35*** (2.23)	-2.95*** (0.85)	-2.15** (0.98)
Log of population density in 1500	-6.17*** (1.49)	-4.05*** (1.07)	-0.54*** (0.10)	-0.39*** (0.11)	-4.65*** (1.16)	-2.67** (1.22)	-3.27*** (0.99)	-2.80** (1.09)	-2.11*** (0.56)	-1.79*** (0.57)
"Good endowments"	67.36*** (12.97)	35.57*** (9.71)	2.09* (1.23)	2.15* (1.20)	12.28*** (4.37)	13.36 (10.78)	15.16* (8.16)	15.02 (10.10)	22.95*** (5.80)	18.27*** (6.06)
"Bad endowments"	-21.23 (14.26)	-14.16* (7.32)	-1.56* (0.89)	-1.96*** (0.71)	9.29** (4.60)	-3.62 (6.67)	-11.44 (6.82)	-11.73 (6.98)	-14.68** (6.48)	-14.98** (6.27)
Several native cultures	-3.84 (4.30)	0.95 (3.31)	0.78** (0.37)	0.81** (0.36)	0.65 (4.50)	0.21 (3.80)	2.84 (3.64)	4.74 (3.98)	3.69** (1.70)	4.19** (1.90)
British colony		7.02 (5.10)		1.02 (0.70)		-3.26 (6.66)		10.04 (6.76)		3.86 (3.34)
French colony		-2.29 (5.08)		0.25 (0.70)		-13.74* (8.05)		6.01 (6.59)		0.36 (3.35)
Spanish colony		4.54 (8.65)		1.22 (0.81)		8.71 (6.89)		14.65** (6.98)		2.97 (4.27)
Catholic population		-0.51*** (0.13)		-0.03 (0.02)		-0.22 (0.31)		-0.33 (0.22)		-0.11 (0.11)
Muslim population		-0.51*** (0.10)		-0.03* (0.02)		-0.32 (0.26)		-0.29 (0.18)		-0.12 (0.09)
Other religion (Non-protestant) population		-0.57*** (0.10)		-0.02 (0.02)		-0.17 (0.33)		-0.21 (0.23)		-0.17 (0.12)
Observations	68	68	60	60	51	51	51	51	51	51
R-squared	0.56	0.85	0.75	0.82	0.64	0.77	0.67	0.73	0.70	0.74
F-test (p-value): Identity of the Main Colonizer		0.00		0.14		0.10		0.15		0.39
F-test (p-value): Religion Variables		0.04		0.07		0.22		0.30		0.39

Note: This panel presents OLS regressions for the cross-section of countries for equation (1). Identity of the main colonizer F-test present the p-value for the global significance of British, French and Spanish colonies variables, and religion variables F-test present the p-value for the global significance of Catholic, Muslim and other religion population variables. White-Huber robust standard errors are shown in parentheses. Constants are not reported. Significance level: *** p<0.01, ** p<0.05, * p<0.1

Table 3: IV Estimates: Educational Outcomes

Panel A: Second Stage Estimates					
Dependent Variable:	Primary Enrollment in 1900		Average Years of Schooling, 1985-1995		
	(1)		(2)		
Democracy	3.45***		0.05		
	(0.98)		(0.14)		
Political Decentralization	6.55		0.92**		
	(5.21)		(0.45)		
Log GDP per-capita			1.06***		
			(0.15)		
Observations	68		48		
Overidentification test (p-value)	0.16		0.63		
F-test (p-value): Identity of the Main Colonizer	0.00		0.01		
F-test (p-value): Religion Variables	0.09		0.08		

Panel B: First Stage Estimates					
Dependent Variable:	Democracy in 1900	Political Decentralization, 1900	Democracy, 1985-1995	Political Decentralization, 1985-1995	Log GDP per-capita, 1995
	(1)	(2)	(3)	(4)	(5)
Log of settler mortality	-0.85***	-0.08	-0.95***	-0.20***	-0.60***
	(0.29)	(0.06)	(0.30)	(0.07)	(0.07)
Log of population density in 1500	-0.34	-0.10*	-0.66***	-0.22**	-0.39***
	(0.25)	(0.06)	(0.19)	(0.08)	(0.06)
"Good endowments"	5.57***	0.50	7.03***	2.78***	-0.23
	(1.84)	(0.43)	(1.34)	(0.57)	(0.52)
"Bad endowments"	-2.94	-0.04	0.61	-0.17	-0.56*
	(1.82)	(0.39)	(0.88)	(0.34)	(0.33)
Several native cultures	1.03*	0.36**	1.13	0.60**	0.12
	(0.61)	(0.17)	(0.71)	(0.26)	(0.24)
Term of Trade shock			-2.33*	0.54	1.07**
			(1.37)	(0.49)	(0.46)
Observations	68	68	48	48	48
R-squared	0.58	0.44	0.74	0.59	0.84
F-test (p-value): Identity of the Main Colonizer	0.30	0.21	0.02	0.01	0.71
F-test (p-value): Religion Variables	0.42	0.44	0.51	0.09	0.83

Note: These tables present first and second stages of instrumental variables regressions for the cross-section of countries for the system of equations (2), (3), and (4) Identity of the main colonizer F-test present the p-value for the global significance of British, French and Spanish colonies variables, and religion variables F-test present the p-value for the global significance of Catholic, Muslim and other religion population variables. The overidentification test presents the p-value for a test that the instruments are orthogonal to the second-stage residuals. White-Huber robust standard errors are shown in parentheses. Constants and religion and colonizer variables are not reported. Significance level: *** p<0.01, ** p<0.05, * p<0.1

Table 4: Additional IV Estimates, Robustness Checks and Extensions

Dependent Variable:	Average Years of Schooling, 1985-1995							Primary education enrollment, 1985-1995	Secondary education enrollment 1985-1995	Higher education enrollment 1985-1995	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Democracy		0.29* (0.16)			0.11 (0.10)	0.01 (0.15)	0.07 (0.17)	-0.04 (0.11)	0.56 (1.45)	-1.08 (2.03)	0.52 (0.86)
Political Decentralization	0.99* (0.51)		1.15*** (0.31)	0.86** (0.42)	0.78** (0.34)	1.10** (0.47)	0.98* (0.52)	0.67** (0.28)	3.11 (4.74)	11.79* (5.99)	4.23* (2.53)
Log GDP per capita	1.05*** (0.18)	1.16*** (0.25)	1.06*** (0.13)	0.47 (0.97)	0.99*** (-0.11)	0.97*** (0.16)	1.08*** (0.13)	0.75*** (0.20)	9.55*** (2.16)	7.40*** (1.25)	2.82** (0.62)
Gastil Index	0.44 (2.37)										
Expropriation risk				0.79 (1.26)							
Ethnic Fractionalization						-1.10 (0.74)					
Land Gini Index							1.89 (1.92)				
Christian Missionaries per 1000 people								4.46** (1.93)			
Observations	47	53	50	46	48	48	44	48	43	43	43
Over-identification test (p-value)	0.64	0.12	0.85	0.92	0.56	0.84	0.98	0.50	0.24	0.98	0.23
F-test (p-value): Identity of the Main Colonizer	0.01	0.01	0.02	0.33	0.01	0.02	0.00	0.05	0.26	0.04	0.63
F-test (p-value): Religion Variables	0.15	0.08	0.08	0.21	0.07	0.02	0.17	0.72	0.03	0.30	0.01
GDP treated as endogenous variable?	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: White-Huber robust standard errors in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1. (see notes from table 3).

Table 5: Education Decentralization Regressions

Dependent Variable	Decentralization	Decentralization	Average Years of Schooling,		Primary education enrollment		Secondary education enrollment		Higher education enrollment	
	of School Management	of School Finance	1985-1995		1985-1995		1985-1995		1985-1995	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy	0.03 (0.05)	0.07 (0.06)	0.06 (0.20)	0.06 (0.52)	1.36 (1.28)	-0.81 (3.72)	0.05 (1.76)	-3.39 (6.49)	0.13 (1.19)	-0.50 (2.75)
Political Decentralization	0.28* (0.16)	0.09 (0.19)								
Log GDP 1995	-0.00 (0.03)	0.06 (0.05)	1.17*** (0.18)	1.11*** (0.26)	8.31*** (2.18)	7.57*** (2.67)	9.05*** (1.76)	7.33* (4.00)	4.16*** (1.01)	3.45** (1.40)
Decentralization of School Management			2.24* (1.20)		0.03 (9.47)		18.65* (10.86)		16.35* (8.48)	
Decentralization of School Finance				1.80 (3.69)		18.18 (29.31)		43.66 (47.31)		18.32 (20.20)
Observations	45	45	49	49	44	44	44	44	44	44
Overidentification test (p-value)	0.92	0.98	0.51	0.26	0.67	0.89	0.74	0.99	0.94	0.73
F-test (p-value): Identity of the Main Colonizer	0.78	0.52	0.62	0.18	0.11	0.24	0.54	0.48	0.85	0.91
F-test (p-value): Religious	0.64	0.79	0.40	0.35	0.11	0.10	0.56	0.85	0.11	0.37

See notes on previous tables.

Table 6: Robustness Exercises

Panel A: Panel Data Estimates: Primary Enrollment and Democracy, 1870-1940						
Dependent Variable:	Primary Enrollment			Democracy		
	(1)	(2)	(3)	(4)	(5)	(6)
Primary Enrollment (-10)	0.62*** (0.06)			-0.00 (0.01)		
Democracy (-10)	0.21 (0.27)			0.33*** (0.13)		
Primary Enrollment (-20)		0.38*** (0.08)			-0.01 (0.02)	
Democracy (-20)		0.61* (0.32)			0.06 (0.13)	
Primary Enrollment (-30)			0.09 (0.08)			0.00 (0.01)
Democracy (-30)			0.75* (0.38)			-0.15 (0.14)
Observations	415	348	282	415	348	282
Countries	67	66	62	67	66	62
R-squared	0.98	0.98	0.98	0.90	0.90	0.91

Notes: This panel presents panel data regressions for primary enrollment or democracy in country i and year t on lags democracy and primary enrollment and country and time fixed-effects. White-Huber robust standard errors are reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel B: Schooling in Colonies and Colonizers

	Primary Enrollment in 1900	Average years of schooling, 1985-1995
	(1)	(2)
Colonizer Primary Enrollment, 1870	-0.02 (0.08)	-0.01 (0.02)
Observations	75	62
R-squared	0.79	0.73
Controls	No	Yes

Notes: This panel presents cross-section regressions for primary enrollment in 1900 and average years of schooling in 1985-1995 on colonizers primary enrollment, my vector of historical variables and religion variables. White-Huber robust standard errors in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel C: Cross-section Estimates: Schooling today and Institutions in 1900

Dependent Variable:	Average Years of Schooling			
	(1)	(2)	(3)	(4)
Democracy in 1900	-0.26** (0.11)	-0.23* (0.12)	1.20 (0.93)	1.47 (0.95)
European Population in 1900	5.60*** (1.30)	5.87*** (1.44)	-0.63 (5.44)	6.08 (6.03)
Primary Enrollment in 1900	0.06*** (0.01)	0.04*** (0.01)	0.16*** (0.05)	0.10 (0.08)
Observations	69	69	59	59
R-squared	0.69	0.76		
Estimation Technique	OLS	OLS	IV	IV
Controls	No	Yes	No	Yes

Notes: This panel presents cross-section regressions for average years of schooling today on democracy in 1900, the share of European population in 1900, and primary enrollment in 1900. The vector of controls includes religion variables and the identity of the main colonizer. In IV regressions, instruments are settler mortality, population density in 1500, and Protestant missionaries in 1900, respectively. White-Huber robust standard errors are shown in parentheses. Constants and religion and colonizer variables are not reported. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

11 TABLES AND FIGURES

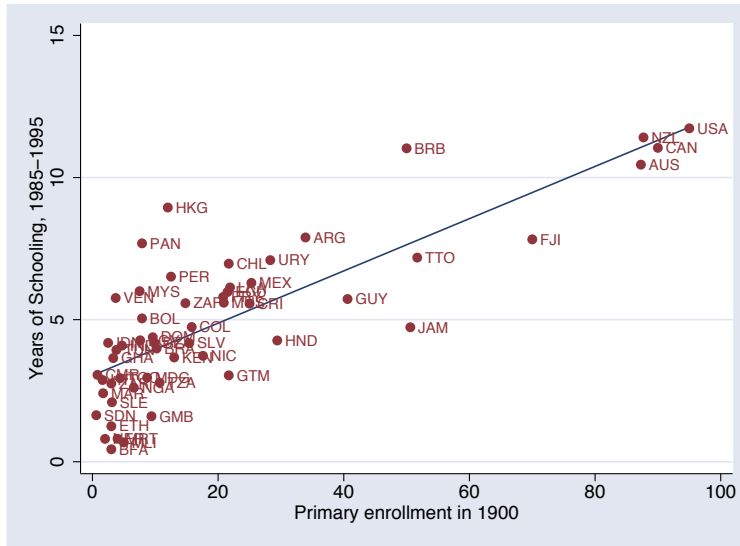


Figure 1, Panel A: Persistence of Schooling, Complete Sample

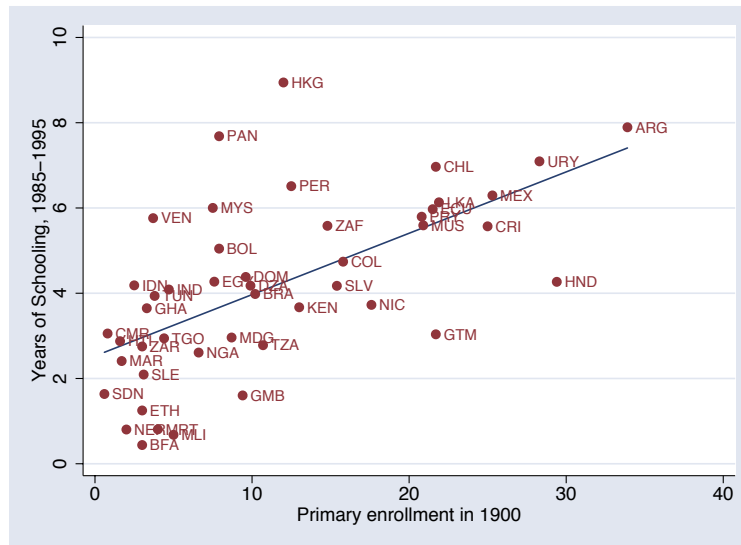


Figure 1, Panel B: Persistence of Schooling, Sample Excluding Primary Enrollment above 40%

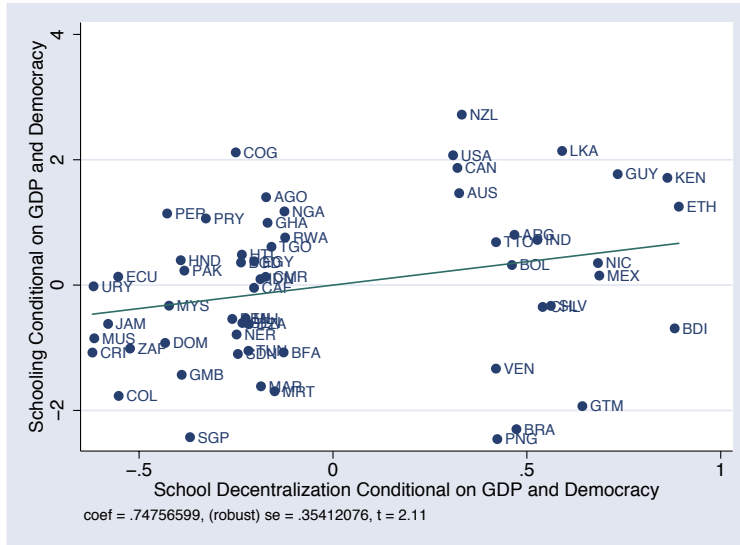


Figure 2, Panel A: Schooling and Decentralization of School Management

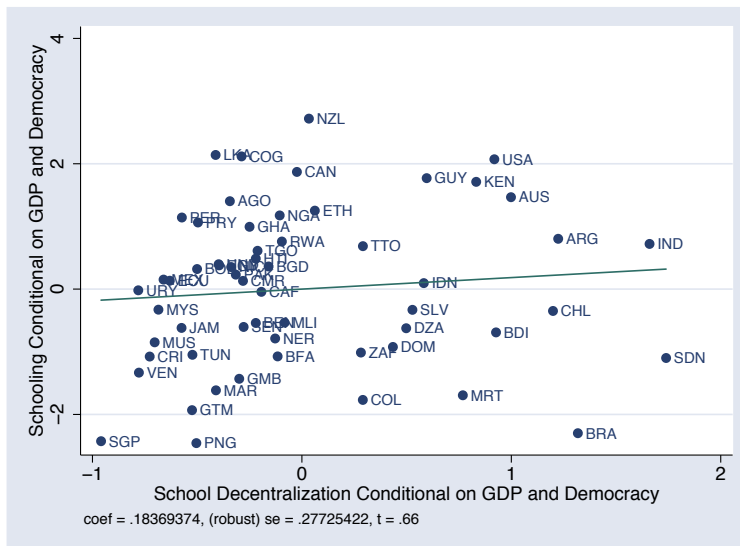


Figure 2, Panel B: Schooling and Decentralization of School Finance

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