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JEL Classification: N37, O15

Keywords: Human Capital, Africa, Colonialism, Numeracy, Education Policy

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The Evolution of Human Capital in Africa, 1730 – 1970: A Colonial Legacy?

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Abstract

How did colonialism interact with the development of human capital in Africa? We create an innovative panel dataset on numeracy across African countries before, during and after the Scramble for Africa (1730 – 1970) by drawing on new sources and by carefully assessing potential selection bias. The econometric evidence that we provide, based on OLS, 2SLS and Propensity Score Matching, shows that colonialism had very diverse effects on human capital depending on the education policy of the colonizer. Although the average marginal impact of colonialism on the growth of numeracy was positive, the premium that we find was driven by the British educational system. Especially after 1900, the strategies chosen by the British were associated with faster human-capital accumulation, while other colonies were characterized by a negative premium on the growth of education. We connect this finding to the reliance of British education policy on mission schools, which used local languages and the human capital of local teachers to expand schooling in the colonies. We also show that this, in turn, had long-lasting effects on economic growth, which persist to the present day.

Introduction

The colonial period was clearly a dark time for colonized countries, as they lacked political freedom and experienced inhuman treatment perpetrated by their colonizers; however, there has been much debate about the impact of colonialism on the long-term economic development of colonized countries. In particular, research has focused on whether colonial rule left a legacy that affects current economic performance and human development in developing countries. Ferguson (2011) argues that colonialism brought about modernization. In Ferguson's provocative view, the alternatives to colonialism might have been even more detrimental for long-term economic development than European rule. For example, African rulers were already engaged in the slave trade prior to European settlements and were unlikely to spark sustained economic growth. Instead, colonized territories benefitted from what Ferguson calls Western "killer apps", i.e., competition, science, democracy, medicine, consumerism and a work ethic. The idea that African rulers could have adopted these innovations autonomously is, in Ferguson's view, naive.

By contrast, Huillery (2014) supports a more negative view of colonialism. She estimates the economic cost of the French colonization of West Africa; contrary to the positive hypothesis proposed by Ferguson, she finds that colonial rule represented a substantial burden on development for African taxpayers. Indeed, Huillery challenges the argument that French African colonies benefited from imperial public investments. She finds that, above all, a large share of resources was destined to French officials. This represented a remarkable financial burden for the colonies (but not for France) that limited the capability to supply public services. Huillery also discusses how colonization hindered access to markets and changed the distribution of rights within the colonies (see Austin 2016 for an excellent overview; see also Ferro 2003, Liauzu 2004, Bancel, Blanchard and Lemaire 2005, Manceron 2005).

We contribute to this ongoing debate by identifying the effect of colonial rule on the growth of basic human capital. We focus on numeracy, which is one of its core components. We approximate it with the widely used age-heaping indicator (see section below).Is it possible to identify a common and persistent effect from colonial rule on human capital across African countries prior to independence? Did this effect change over time and across metropoles and if so, how? By exploring the evolution of numeracy skills through the 18th and 19th centuries and up to independence (1730 – 1970), we answer these research questions and avoid a simple “compression of history” (Austin 2008, see also Jerven 2015) – thanks to our data, we can explore how the strength of the “colonial effect” on human capital changed over time. We focus on numeracy as an output measure of human capital because –as Hanushek and Woessmann (2012) have recently argued – math and science abilities are the most crucial determinants of economic growth. Indeed, these two authors employed a wide range of country-level and migration data to assess the causality of this relationship; they find that numerical skills explain much more of the variation in growth rates across countries than domeasures focused on the quantity of schooling provided. While their research focuses on the 1960 – 2006 period, Baten and Juif (2014) show that numeracy in approximately 1820 – measured using the share of those who were able to report their exact age, the measure we use in this study – is strongly correlated with later math and science skills. They argue that some societies were able to start down a human-capital-intensive development path and that only few economies left this path thereafter. Taken together, these two contributions suggest that basic numeracy during the early-modern period can provide precious insights into patterns of long-term development. Given the evidence discussed thus far, it is crucial to reconstruct the path of long-term human capital accumulation to elucidate the development of Africa: our estimates of numeracy allow research on the growth of human capital in the continent to be conducted, for the first time, for the period from the early 18th century to the eve of the 20th century.

The empirical strategy of this study is based on identifying a common effect of colonial rule on human capital accumulation in Africa prior to independence and on exploring whether this changed over time and across metropoles using the largest human capital data set ever assembled for pre-, post-, and colonial Africa: we mobilize 284 country-decade observations for the period until the 1960s. We propose that colonialism positively influenced the development of the basic quantitative skills of Africans. This hypothesis stems from qualitative literature on the effort to provide education during the colonial era as well as from evidence of a significant impact of colonial education policy on African schooling in the 20th century, which is discussed in the literature review. We also explore the possibility that this effect might have been heterogeneous and connected to the identity of the European colonizer and its type of educational policy.

We address potential selectivity issues in our estimates of numeracy, and we find that selection bias was probably not very substantial in the historical samples used to reconstruct long-term human capital trends in Africa. Our regression analyses take into account other factors that might have influenced the development of numeracy over time, such as quantity-quality trade-off behavior, urbanization and the frequency of conflicts. Furthermore, we provide two different robustness tests to combat potential bias due to reverse-causality and other causes of endogeneity. Finally, we investigate the mechanism behind the positive impact of colonialism on human capital by assessing the role of slavery, direct colonial rule and missionary activity.

Although we find that the impact of colonialism on basic numeracy was positive, the very heterogeneous effects that existed across African countries suggest that colonial rule was often detrimental to African development; indeed, the positive impact of European rule on Africa's human capital in the period under study appears to have been driven by British rule *only*. Interestingly, as other contributions have recently suggested, the primacy of British school policy in Africa might stem from its reliance on local languages and on the human capital of African teachers, which was enhanced by missionaries in the British colonies (Frankema 2012, Gallego and Woodberry 2010). In the case of long-term human-capital development, the outcome depended on foreign rule as much as it was connected to local conditions.

Finally, we find a strong correlation between historical numeracy and current GDP per capita across African countries, even after controlling for GDP per capita in 1950. Numeracy explains approximately one third of the variation in economic performance observed across African countries in 2000; because we find that colonialism had a large impact on human capital accumulation on the continent, we argue that the interaction of colonial institutions and local endowments created a legacy affecting African development.

Literature review: colonialism and Africa's development

Does colonialism affect economic outcomes in former colonies today, and if so, how? Acemoglu, Johnson and Robinson (2001) have shown that the nature and persistence of institutions established by settlers explain much of the disparities in economic performance across former colonies today. By the same token, Glaeser, La Porta, Lopez-de-Silanes et al. (2004) suggest that colonialism has an important impact on current development; yet, they argue that colonial rule primarily influenced the human capital of Africans and that positive institutional developments followed human capital accumulation – a hypothesis that has been explored and confirmed by Baten and Prayon (2013).

According to Nunn (2008), the impact of slavery stemming mainly from the precolonial period also played a central role across African countries, as the number of slaves exported before the mid-19th century is a robust predictor of today's economic performance. Gallego and Woodberry (2010) explore the impact of missionary activity on the development of today's schooling in 180 African provinces. They find that the presence of Protestant missions is highly correlated with education in Africa today. However, they stress that the strength of this relationship depends on the protection that Catholic missions received from the colonizers: the effect of Protestant missions on schooling is larger in areas where Catholic missions were less protected.

Although important, much of this evidence is based on post-colonial data and patterns of growth; yet, a number of scholars have recently provided new, quantitative evidence that allows investigation of the effect of colonialism beyond the post-independence period. Frankema and van Waijenburg (2012) have reconstructed real wages for nine colonies that belonged to British Africa between 1880 and 1965. They show that Africa's living standards did not lag behind those of Asia and that the growth of real wages was rather sustained over the late-nineteenth and early-twentieth centuries. Anthropometric evidence (i.e., evidence on human stature) also provides insights into the effect of colonialism on development:[1] Moradi (2009) looked at heights in Kenya during and after colonialism and found that height increased, especially towards the end of the colonial era, thus suggesting a positive trend in well-being under colonial rule. In sum, the literature on colonial and postcolonial Africa is rapidly growing, and this illustrates research interest in the economic history of this continent; yet, the effect of colonialism on development continues to be fiercely debated.

Human capital in Africa: a long-term perspective?

The analysis of trends concerning different dimensions of development is important per se to trace the evolution of well-being during the transition from the pre-colonial time to colonial rule and independence. However, some dimensions of human development represent important growth factors in the long run. Among them, human capital – and numeracy in particular – is known to play a decisive role as a determinant of cross-country economic performance (see Galor 2005, Hanushek and Woessmann 2010). Even institutions may be

[1]Baten and Blum (2012) have shown that height is a good predictor of real income as far as poor countries in the nineteenth and twentieth centuries are concerned. Furthermore, trends in heights can be reconstructed for the very long run, including the pre-colonial, colonial and post-colonial phases.

endogenous to education, so that human capital may exert a positive effect on economic performance via formal and informal norms (see Glaeser, La Porta, Lopez-de-Silanes et al. 2004 for a general formulation of the issue and Bolt and Bezemer 2009 for empirical evidence based on Africa). Human capital depends not only on schooling but also on basic skill formation in the family. There is also a positive interaction between school and learning in the family. Although there appears to be a consensus that colonial rule sought to strengthen the widespread diffusion of schools (Clignet and Foster 1964; see also Grier 1999, p. 326), the magnitude of the effect of colonial education appears to be conditional on the policy adopted by the ruling European powers. To be sure, quasi-natural experiments based on the Togo-Ghana border (Cogneau and Moradi 2014) and Cameroon (Dupraz 2013) have explored the different impact of British and French colonial rule on different indices of schooling. When the two are compared through a quasi-experimental design, British rule is associated with a positive premium on education. This result is confirmed by the cross-country analysis provided by Frankema (2012) for 1938 and 1960. Despite this remarkable research effort, however, the evidence has long remained limited to the late-colonial period, while a similar quantitative assessment of colonial education policies in the 19th century remains an uncharted territory. It is also worth noting that results from quasi-natural experiments, though internally highly valid, cannot be easily generalized to other contexts and countries.

We know that British colonial education policy was highly decentralized, as it encouraged the activities of missionaries by granting them full administrative freedom and providing them with occasional grants-in-aid. Later (starting in the 1920s), the government stepped in more decidedly; yet, even then, a central role was played by the close cooperation between the central administration and the missions, the adaptation of education to local needs and demands, the use of local languages in class and a specific focus on gender disparities. According to Frankema (2012), the reliance on African agency and local conditions are central in explaining the success of Britain’s colonial school system. By way of contrast, the French top-down education policy of *assimilation* allegedly failed to promote the diffusion of schools in the continent. Most of the 70 schools established in French West Africa by 1900 were run by the colonial administration, and by the 1930s, the French government was exerting an even stronger control on colonial schooling. In this context, classes were given in French and the education of the masses was not given priority. Often, enrolment was limited to avoid a growing number of unemployed graduates (for more details on British vis-à-vis French education policies in the colonies, see White 1996).

Despite these recent research efforts, quantitative research on the effect of colonial rule on education remains scant. More evidence is needed on human-capital trends before independence: standard measures of schooling are minimally available for pre-colonial and colonial Africa and recent studies are mainly based on a single-country perspective. We shed new light on the issue thanks to a new dataset on numeracy (measured by the ABCC index) across African countries during the pre-colonial and colonial period, from the 1730s to the 1960s.

Data and sources

We estimate trends in basic human capital (1730 – 1970) by using the ABCC index, a proxy for basic quantitative skills. A recent line of research has shown that the capability to count and conduct basic calculations is negatively correlated with the extent of age-heaping, which is defined as people’s tendency to report their own age on rounded numbers (such as 0 and 5). This negative correlation has been shown to hold across samples, countries and historical periods and therefore provides an innovative tool to explore human capital trends in the far past, typically when (and where) literacy and schooling data are not available (see A'Hearn, Baten and Crayen 2009). The extent of age-heaping in a sample of individuals can be summarized by using the Whipple Index (Equation 1), while the ABCC index is obtained through a linear transformation (Equation 2), which allows the index to range between 0 and 100.

$$(1) \qquad W = \frac{\sum (n_{25} + n_{30} + ... + n_{55} + n_{70})}{(1/5)\sum (n_{23} + n_{24} + ... + n_{70} + n_{71} + n_{72})} \times 100$$

$$(2) \qquad ABCC = \left(1 - \frac{(WI - 100)}{400}\right) \times 100$$

This methodology has prompted new research on the human capital of different world regions and countries since the Middle Age and, sometimes, earlier eras such as the Roman Empire (see Baten and Priwitzer 2015). A large number of potential biasing factors have been studied in the age-heaping literature. For example, the question whether a stronger bureaucratic tradition influenced age-heaping patterns could be relevant for our study on colonialism as well. On this question, Crayen and Baten (2010) used a variety of different administrative proxies (such as the number of previous censuses in which age statements were demanded, government age etc.). They found that these bureaucracy indicators were not correlated with age-heaping, whereas educational indicator explained a large part of the variation in multiple regressions.

Our dataset relies mostly on two sources, early census evidence and slave censuses. The census data have already been evaluated by economic history studies and found to be quite reliable (Crayen and Baten 2010). We also estimate average numeracy in Senegal in the long run based on new census data provided by Cappelli and Baten (2015); data for South Africa are obtained from Baten and Fourie (2015), who studied the evolution of numeracy in the 17th and 18th century. We describe here a new, complementary source for the early period: slave census data. Midlo-Hall collected numerous sources for slave censuses. These were taken after slaves were deported to Louisiana (US) from Africa by investigating statements about their origin; we use this information together with a companion dataset (the Maranhão Inventories Slave Database or MISD) to reconstruct numeracy in the 18th century for Angola, Benin, Congo, Ghana, Guinea, Guinea-Bissau, Mali, Mozambique, Nigeria, Senegal, Sudan and Togo after assessing potential selectivity (see Midlo-Hall 2000 and Hawthorne 2010).

The use of data collected in different contexts requires a distinction between samples obtained from different sources, to avoid bias due to selectivity. Indeed, one hypothetical objection to the use of slave concerns the selectivity of individuals, whose declared age we use in order to calculate age-heaping and, in turn, numeracy. This is especially so given that we rely on different primary sources in an effort to expand the evidence on census data from Crayen and Baten (2010).

Were the slaves brought to the Americas a selective group? For example, one could imagine that they would be younger, healthier, and more often male. However, the extensive literature about the selectivity of slaves suggests that selectivity was not substantial: Eltis has argued strongly that the bias between freed slaves and the populations from which they came was small or even negligible (see Eltis 1982, 455–6, on this and the following; see also Austin et al. 2012).[2]The logic of this argument also applies to slaves held and traded within Africa: slave raiders and victorious armies had an interest in capturing everyone who could move.

How can we assess the selectivity of slave samples? A large number of tests have been made and are documented in an Appendix[3];we provide some examples here. We test the presence of bias by comparing the regional distribution of numeracy in Western Sudan (today's Senegal) across two distinct datasets to assess whether our estimates of numeracy suffer from specific selectivity (Figure 1): We find that in particular, numeracy calculated from local census lists is highly consistent with figures obtained from slave records.

Furthermore, we compared numeracy levels obtained from the dataset on slaves deported to the US with the numeracy of the natives in the Cape Colony around the same time

[2]Physically strong (and tall) Africans were also in demand in African plantations and farms, and the demand for slaves within West African economies was high in this period. Finally, evidence about slaves in Brazil and the Caribbean suggests that slaves born in Africa were much shorter than those born in the New World—whereas if slaves in Africa had been chosen by health or height, we would have expected them to be taller, especially if the deadly voyage over the Atlantic implied additional selectivity in favor of the taller and healthier individuals.
[3]www.xxx.xx (anonymous for refereeing purposes)

obtained from Baten and Fourie (2015):one issue could be whether slaves self-reported ages or slave owners reported their age. This is a reliable way to check the plausibility of our estimates based on the large sample of slaves collected by Midlo-Hall and Hawthorne because the court registers used to reconstruct numeracy in the Cape explicitly state that people were declaring their age themselves, and therefore there cannot be bias from non-self-reporting. Furthermore, Baten and Fourie (2015) carefully checked the bias of their sample, and their published evidence (adjusted for different selectivities of different crime types) confirms that the sample is quite representative of the underlying population. The average numeracy in the Midlo-Hall-Hawthorne slave data is 29.5 in the period 1750 – 1800; the numeracy of natives in South Africa ranged from c. 25 (Khoesan), to c. 30 (slaves), and up to c. 35 (South African blacks, mostly Xhosa; see Figure 4 in Baten and Fourie 2015, p. 648). It is remarkable that before the surge of racism during the late 19th century, African slaves were actually asked for their age (racism actually increased over time so that it was less strong in the 18th century than it would be later on, see Gould 1977).This might also be the case because the numeracy gap between the African and the European population during the 18th and early 19th century was not as large as it was during the late 19th and early 20th century, when racism reached its nadir (Baten and Fourie 2016).

[Figure 1 here]

To complement census and slave census evidence, a small number of additional observations throughout the 19th century are obtained from contemporary anthropological reports drafted by scholars who travelled throughout the African continent. Finally, we also use marriage registers to reconstruct numeracy in Uganda from the mid-19th century to 1970 (see data Appendix on sources).

What type of selectivity may occur with marriage registers? In most historical Christian societies, the people who married were a positive selection of the underlying population (especially before 1900). In addition, the observed numeracy of 40 is upwardly biased because most marriages take place in the early 20s (hence there are many more aged 23 than 30). A first adjustment of estimates of numeracy based on marriage data was performed by Plötz (2013). She explored the evolution of numeracy over time of those countries for which we have both census data (usually relatively unbiased) and marriage registers (upwardly biased). By comparing the two sources, she provided specific adjustment factors to be applied to the original data from the marriage registers. The adjustment factor (negative) is represented by the following equation (Equation 3):

$$AdjF = [\![(ABCC]\!]_{mr}\ x\ 0.264) - \mathbf{5.51} \tag{3}$$

where $ABCC_{mr}$ represents numeracy obtained from the marriage registers. For example, an observed marriage-register numeracy of 40 would be adjusted downward by $(40\ x\ 0.264) - 5.51 = 10.56 - 5.51 = \ \mathbf{5.05}$. The estimate of unbiased numeracy is then $40 - 5.05 = 34.95$. To be sure, we show (Figure 2) that numeracy calculated through marriage registers (Uganda), and adjusted according to the methodology proposed by Plötz using a regression model, does not present any particular bias when compared to numeracy as elaborated from national censuses: the difference is negligible for the years shown, and on average, the two series present the same levels and, more importantly, the same trend over time. Observations of numeracy drawn from census data (including IPUMS) represent approximately 70 percent of our sample; 16 percent comes from slave data, while the remaining 14 percent comes from marriage lists and, to a lesser extent, other sources.

[Figure 2 here]

Cultural differences might also invalidate the use of age-heaping for calculating numeracy: indeed, this technique is based on the assumption that basic ways of counting are broadly similar across different countries as well as cultures. Needless to say, we do not want to dismiss the argument that different populations and societies may value counting differently or count in different ways, with biases toward certain digits that are different from multiples of 5. However, in earlier studies, cultural preferences for certain numbers were found to play only a very modest role on rounding behavior. The fact that most human beings start counting with their hands implies a strong tendency to round on multiples of 5:studies on cultures as remote as the Inca-Indio culture and East Asia have shown this (Baten and Juif 2013, see also Baten, Ma, Morgen and Wang 2010). It is also worth noting that the strong correlations that we have found between numeracy in c. 1900 and inputs into schooling in c. 1950 (see Figure 3 below) suggest that the ABCC index is actually a reasonable measure of educational outcomes in Africa. Interestingly, figures concerning numeracy in Uganda (estimated through marriage registers) fit the pattern outlined by Figure 3.

[Figure 3 here]

These efforts to reconstruct numeracy have culminated in an unbalanced panel of 44 African countries over the decades of the 1730s – 1960s (ten-year intervals). Despite the use of additional sources, numeracy could not be estimated at any point in time in the case of Comoros, Djibouti, Equatorial Guinea, Sao Tome & Principe, Seychelles and Sierra Leone. As Table 1 shows, we substantially broaden the availability of human-capital figures by peeking backward into the late-19th century for a large number of countries and, for a number of them, even back into the 18th century.

[Table 1 here]

Figure 4, based on a lowess smoothing function, shows the average trend of numeracy growth across African countries between 1730 and 1970.[4] Slow growth throughout the 18th century is followed by sharp acceleration in 1800 – clearly before the Scramble for Africa. The rate of growth of numeracy accelerates slightly by the end of the 19th century and slows down thereafter – plausibly as a result of numeracy reaching the upper-bound of 100 percent. Next, we outline an econometric model aimed to explore the determinants of such trend and

[4] Using a standard unweighted or weighted mean would lead to a spurious trend given the imbalance of our panel.

such remarkable regional disparities with a particular focus on the effects of colonial rule on the growth of numeracy over time.

[Figure 4 here]

Methodology and baseline results

We use a Weighted Least Squares (WLS)[5] panel-data model to regress the annual growth of ABCC (numeracy) within each ten-year interval (1730 – 1970) on the initial level of numeracy and on a dummy aimed to capture the presence of colonial rule: the latter takes a value equal to one if a country was a colony in a given year and is coded according to Olsson (2009). This variable captures the variability of colonialism across space and over time: according to Olson's code, only 8 African countries were colonized in 1730, while the number of colonies grew slowly during the first half of the nineteenth century and more quickly during and after the Scramble for Africa: in the 1910 – 1919 decade, a small minority is coded as independent. The baseline model is shown in Equation 3 below:

$$ngr_{it} = \beta_0 + \beta_1 n_{it} + \beta_2 colony_{it} + \alpha X'_{it} + \delta_i + \gamma_t + \varepsilon_{it} \quad (3)$$

where ngr is the average annual rate of growth of numeracy for country i and the ten-year interval t (e.g., 1900 – 1910), n is the level of numeracy at the beginning of each period (1900 in the previous example) and $colony$ is the dummy capturing colonialism as described above. X is a vector of control variables, which include the average annual rate of population growth in the ten-year interval concerned, the urbanization rate, the intensity of international and domestic conflict in a given period (measured as share of years characterized by a major

[5]Constant weights are set equal to the population in 1950.

conflict out of ten), cattle per capita and cropland per capita (see Table A3 – Data appendix for more information on the explanatory variables). We also include country fixed effects and time dummies to capture the trend in the data.

In our first regression (Table 2), the coefficient of the "colony effect" is positive: being a colony is associated, on average, with a 1.22 percent increase in the annual growth rate of numeracy, a substantial impact. However, the p-value is equal to 0.19, indicating that the effect should be more precisely estimated. The coefficient of the initial level of numeracy at the beginning of each ten-year interval is positive but small and not statistically significant (column 1).

[Table 2 here]

In a second regression (column 2 in Table 2), we include a number of control variables that may have an impact on the growth of numeracy. Population growth is used to control for the quality-quantity trade-off. More children are normally associated with less (or less effective) education because parents have to divide their investment in human capital among a larger group, although this does not hold for all societies (Clark 2007).The urbanization rate is included to control for the role played by urban areas in raising the demand for skills and because commuting distance to schools is normally lower in urban areas. We also include two variables measuring the intensity of both domestic and international conflict within each ten-year interval (number of years of conflict within a total of ten years). Finally, we include cattle and cropland per capita to control for the availability of food and proteins, which may positively affect numeracy via the nexus connecting malnutrition and lower numeracy skills (Baten, Crayen and Voth 2014). Although the quality of the estimates is expected to improve because of reduced potential omitted variable bias, data to construct the control variables are available only for the period 1800 – 1970, and therefore this regression is limited to the 19^{th} and 20^{th} centuries. The coefficient of the initial level of numeracy is now negative but still not statistically significant. Instead, the "colony effect" becomes significant at the 5-percent level, while the magnitude basically doubles (2.53 percent increase in annual numeracy growth). All of the control variables are significant except for the intensity of domestic conflict, cattle per head and cropland per capita. The significant explanatory variables have all of the expected signs: in particular, a one-percent increase in population growth is associated with a 0.30-percent decrease in numeracy. Finally, we differentiate between the impact of colonial rule before and after the Scramble for Africa by including a dummy for years before the 1880s and by interacting it with the dummy that captures colonial rule (columns 3 and 4 of Table 2). We do so because African native populations experienced contact with Europeans well before 1890, but colonialism became more prominent only on the eve of the twentieth century; additionally, slavery still played some role in early-nineteenth century Africa, a role that we expect to be negative for human capital accumulation. This particular specification shows that the rate of growth in numeracy before the 1880s was generally lower than after the Scramble for Africa (note that we include time dummies to control for a general trend in numeracy) and that colonization only marginally improved the pace of human capital accumulation (the marginal effect before 1880 is equal to +0.69 percent per annum in column 4) – a finding compatible with a small positive colonial effect *mitigated* by the negative impact of slavery, which we will tackle more directly in the last section of the paper. After 1890, the impact of colonization on the growth of human capital within African countries was positive and large: with respect to the previous specification, the marginal effect of colonial rule on the growth of numeracy within countries between 1800 and 1970 doubles to 4.71-percent (average per annum).

Was this positive effect homogeneous across all African countries? Does the positive impact of colonial rule on numeracy depend on the identity of the colonizers and their specific policies? For example, British education policy and its reliance on missionary schools connected to local teachers and language might have had a very different impact on numeracy with respect to the schooling provided by other colonizers. To explore this question, we code different dummies for different metropoles. In particular, we distinguish the UK and France from the reference category (which includes Belgium, Germany, Italy and Portugal) given the debate surrounding the performance of British and French colonial education systems. These are simply interacted with the "colony effect" dummy to obtain metropole-specific coefficients for colonial rule (i.e., heterogeneous effects). As Table 3 shows, the positive effect of colonialism appears to be largely driven by the performance of the British colonial education system (column 1). The coefficient of "colony x UK" has a positive sign, and it is larger than the overall coefficient of "colony effect" found in the previous estimates. Being a colony under British rule is estimated to have increased the growth rate of numeracy by c. 2.85 percent per annum, a result that is obtained by summing the "colony x UK" coefficient with that of "colony effect" (it is worth noting that the latter is not significantly different from zero). The coefficient of "colony x France" is not statistically significant and, if anything, the effect appears to have been negative throughout 1800 – 1970 (-0.028 percent per annum). This result is strengthened when the period of most intense colonization is taken into account, i.e., 1900 – 1970 (column 2), providing even more compelling evidence that colonial rule made a difference.

[Table 3 here]

The net effect of colonialism under British rule is estimated to have been +2.1 percent per annum (sum of coefficients of "colony effect" and "colony x UK" as before), but the statistical significance of the results is now higher. The effect of France is negative but statistically equal to zero and, by way of comparison, colonization under the rule of other countries in the reference category also had a negative impact on the growth of numeracy (the coefficient of being a colony is now negative and significant, with a magnitude of -2.1 percent per annum). The other results, concerning the coefficients of the control variables, remain basically unchanged. However, the coefficient of the level of numeracy at the beginning of each period is now positive and highly significant, albeit small (a 1-percent increase in initial numeracy is associated to a 0.1 higher growth rate): thus, the results suggest that human-capital divergence took place in the 20th century.

2SLS and Propensity Score Matching estimates

The propensity to occupy a country in Africa in the late 19th century might have depended on the country's features, so we might observe a faster growth of numeracy in colonized countries *just* because they were positively selected during the Scramble for Africa or before. If reverse causality was at work, then our estimates of the impact of colonial rule on numeracy may be spurious.

A way to circumvent endogeneity issues is to use 2SLS regressions. However, the quest for a good instrument is particularly challenging for panel-data models (and for pre-colonial Africa even more). A valid instrumental variable should (1) be correlated with the potentially endogenous variable conditional on other covariates, (2) not be affected by the growth of numeracy after 1800 (our dependent variable) and (3) affect the growth of numeracy post-1800 only through colonization. Obviously, finding a suitable instrument is challenging in a model with country and year fixed effects, which will absorb the impact of both country and time-invariant factors, like geography and any other aspect that changes only over time but not across country.

In order to tackle this problem, we use a mixed strategy. First, we drop the fixed effects from our panel-data regression to allow the use of time-invariant instruments. The country-level fixed effects are replaced by macro-regional fixed effects: we code and include dummies for 5 major African regions (as defined by the UN Statistical Division). It is worth noting that re-running the OLS regressions with these macro-regional dummies instead of the country fixed effects does not affect the results.

The first set of instruments is time-invariant. We aim to capture the fact that places that played a role in the rise of the African slave trades experienced early European occupation and therefore were more likely to be colonized in the 19^{th} and 20^{th} century. However, slavery itself might be endogenous to early colonization, for example, if we consider the occupation of coastal areas of Senegambia by the French in the early 19^{th} century. Therefore, we use an instrument that has been shown to capture exactly this mechanism: arguing that the number of slaves deported from each African country is influenced by demand basins (i.e., markets for slaves) more than any other factor, Nunn (2008) uses distances from markets connected to different slave trades as an instrument for the number of slaves deported normalized to land (in logs).

In a similar way, we use the distance from the markets concerning the Atlantic Slave Trade and the distance from markets connected to the Saharan Slave Trade to instrument our "colony effect" dummy. As far as the Atlantic Slave Trade is concerned, we expect a significant and negative impact on the likelihood of being colonized: more distance means less likelihood of developing a slave trade, which in turn should negatively influence the chances of European occupation, according to our hypothesis that early European contact to exploit slavery increased the chance of colonization in the future. For the Saharan Slave Trade, the expected sign can hardly be predicted: although a similar mechanism might hold, the proximity to routes that were connected to the spread of Islam and fights against European occupation (see, e.g., Lydon 2009 on the Trans-Saharan Trade), and therefore more distance from these slave markets, might have actually increased the possibility of colonization.

In a second step, we also interact these slavery-related instruments with European fiscal capacity to explore the possibility that the marginal impact of the distance from slave-trade markets might have changed according to the capability of European countries to occupy and administer African territories. For example, if the distance from the Atlantic-slave-trade markets is negatively associated with colonialism as we expect, then the interaction of fiscal capacity with the distance from Atlantic-slave-trade markets should be positive: the effort required by European colonizers to settle down in a country that had not previously experienced extensive contact with Europeans should be greater.

The rationale for using the fiscal capacity of European colonizers as an instrument for the likelihood of colonization in Africa is simple: the occupation and settlement of African countries required augmented fiscal capacity to sustain new military efforts overseas and to consolidate European rule by means of the colonial administrative system. Our value of average European fiscal capacity is obtained from national series of fiscal capacity between 1800 and 1970, which concern Great Britain, France, Germany, Belgium, Italy and Portugal. For each country, fiscal capacity is measured as tax revenues (central government) in per capita grams of gold: part of the series (1800 – 1913) is obtained directly from Dincecco (2009), while revenues in the second period (1913 – 1970) have been converted in per-capita grams of gold from the series kindly provided by Sabaté Domingo (2016) using the exchange rates from Officer (2015). We take the non-weighted average of these national series to come up with a country-invariant value of European fiscal capacity between 1800 and 1970.

We can safely assume that the rate of growth of numeracy (our dependent variable) in Africa would not reverse-cause fiscal capacity in Europe. Additionally, we assume that fiscal capacity in Europe cannot have a direct effect on the growth of numeracy in Africa other than via colonization. Indeed, fiscal capacity across European countries might have mattered for the provision of education in the colonies but, naturally, not before a country was colonized. In the absence of a direct link between European and African countries, we can posit that there is no direct effect. However, the exclusion restriction may not hold even if the instrument is correlated with other (unobserved) factors that influenced the rate of growth of numeracy over time. However, once again, it is difficult to imagine a correlation between European fiscal capacity and correlates of numeracy (pertaining to Africa) in the absence of colonial rule. All of this suggests that the instrument is a valid one.

Since the variable capturing European fiscal capacity is only time-variant, the marginal impact of this factor alone is absorbed by the time dummies. Therefore, our final set of instruments includes four variables, i.e. the two time-invariant instruments and their interaction with average European fiscal capacity. Because we use macro-regional dummies instead of the country fixed effects and instruments that might be correlated with the dependent variables via the error term through their relationship with geographic factors, we include a set of geographic variables in both the first and second-stage regressions, not as instruments but rather as further controls: these include absolute latitude, min. of monthly average rainfall (mm), min. of average monthly low temperature (C) and the log of the ratio between coastline areas and total surface (also obtained from Nunn 2008). Our slave-trade-distance instruments might be correlated with geographic features such as latitude, climate and similar variables, and thus omitting them might invalidate the IV strategy because of the correlation of the instruments with the error term.

The first-stage estimates for our entire sample (including British and non-British colonies) confirm the tight relationship between our instruments and colonialism as well as the relevance of their interaction with European fiscal capacity: the distances from the slave trade markets, as well as the interaction between these and European fiscal capacity, are all significantly different from zero (first-stage estimates in Table 4).

[Table 4 here]

The second stage confirms the positive impact of colonialism on numeracy (column 1 of Table 5), with a marginal impact of the colony-effect dummy of +2.81 percent per annum. The post-estimation statistics reported in Table 5 show that the instruments are actually correlated with the potentially endogenous regressors, that this correlation is not weak and that the combined set of instruments is exogenous (under the usual assumption that at least one instrument is valid).

We also want to check whether the 2SLS estimates confirm that the impact of colonialism on numeracy was driven by British institutions. Given the challenge of instrumenting the colony dummy, the dummy that identifies the UK as a colonizer and their interaction at the same time (see Angrist and Pischke 2009), we split the sample into British versus non-British colonies (columns 2 and 3 in Table 5). Although it is worth noting that the instruments appear to be weak in the last regression concerning non-British colonies, the second-stage results largely confirm our previous findings based on the OLS regressions: the marginal impact of colonialism on human capital accumulation in Africa between 1800 and 1970 is due to the effect of British colonial institutions. Given the weak instruments for the non-British case, we further study the endogeneity issue below using propensity score matching.

[Table 5 here]

Another way to tackle this problem is to see colonization as a treatment in a non-randomized experiment. We may be able to rule out endogeneity by obtaining a sample where countries are selected into "colonial rule" *as if* they were randomized. Such a procedure can be implemented by using Propensity Score Matching (PSM). In a nutshell, PSM allows us to estimate the probability that a country would be colonized after 1850 based on a set of observable features: we use our control variables, given the paucity of data for early-19th century Africa. Once the propensity scores are calculated, we are able to obtain a sample of countries where colonial rule is distributed *as if* randomized. The idea behind this procedure is to select countries that share similar features but differ in the "treatment" assigned.

We start from a *global* database of former colonies. We regress the propensity (probability) of being a colony post-1850 on the potential determinants of colonial occupation, such as population growth, urbanization rates, international and domestic conflicts, and cattle and cropland per capita. These aspects are measured in 1800, i.e., before the Scramble for Africa took place, and as such they are not influenced by colonial rule itself

(Figure 5 shows propensity scores for colonies and non-colonies).[6] This first-step model (see Figure A1 in the Appendix) serves to estimate the probability of being colonized after 1850 based on observable features in 1800 and allows us to store the predicted probabilities (ranging from 0 to 1) as propensity scores. It is impossible to test that we include all of the variables that are relevant to randomize our sample (assumption of unconfoundedness), especially when few indices are available as in the case of nineteenth-century Africa. However, our data should capture enough variation to greatly reduce the potential bias between our treatment and control groups. Furthermore, it is worth nothing that adding too many variables would lead to increased explanatory power of the probit model, which in turn might lead to the reduction of the common support (the overlapping of the propensity score shown in Figure 7), which is not desirable in the context of PSM (for a reference on the PSM methodology see Rosenbaum and Rubin 1983).

There is some overlapping in the propensity scores, although not to a large extent: there are countries with a low probability of being colonized after 1850 that were eventually occupied and countries with a higher propensity to be colonized that were not. Following Crump et al. 2009, we use a simplified procedure to select our PSM sample with respect to the standard matching algorithm by including only those countries that present a propensity score in the 0.1 – 0.9 range (a list is shown in the notes below Table 6; see also Semrad 2015 for a recent application in the context of human-capital development). Within this band, the treatment cannot be explained by the explanatory factors we take into account because the distribution of the treatment does not strictly follow the distribution of propensity scores based on observables.

[Figure 5 here]

We then run the same regressions as in the baseline OLS panel-data model, testing the general effect of colonialism and then separately testing that for the British colonies (Table 6).

[6] See Leuven, E. and B. Sianesi (2014). PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing.

[Table 6 here]

As the table shows (column 1), the effect of colonial rule remains virtually zero, being not significant. This result does not change if one takes into account all the control variables (column 3). In both columns 2 and 4, estimating the effect of colonial rule separately for British and non-British colonies shows that the colonial effect was heterogeneous and that, again, British rule was associated with a premium on numeracy growth over time. Even if one takes into account the coefficient of the "colony effect" (which is still not significant), the marginal effect of colonial rule on ABCC growth in British colonies given by column 4 is equal to +3.2% annual growth: this estimate is large compared to the magnitude found in our previous estimates that were not quasi-randomized using propensity score matching.

Robustness checks: excluding non-census sources

We also run a fixed effects panel regression with heterogeneous effects of colonizers ("metropole x colony" interaction terms) where we exclude the ABCC observations that are not based on censuses (i.e., we exclude slave censuses, marriage, prison and anthropological samples and sources) in order to indirectly test for the presence of greater selectivity in specific historical sources relative to census data (Baten and Prayon 2013). As Table 7 shows, the baseline results do not change. Once again, the overall effect of being a colony, once British rule is excluded, is negative, while being a British colony is strongly positive – especially in post-1900 years.

[Table 7 here]

What mechanisms were at work?

In this short section, we test empirically whether the positive effect of colonialism that we find in British African territories is driven by mechanisms other than direct colonial rule and colonial institutions. In particular, we take into account the impact of missionary activity, which has recently drawn renewed attention in the field (Fourie and Swanepoel 2015, Fourie, Ross and Viljoen 2014, Bolt and Frankema 2015, Woodberry 2012). Several studies on schooling in early-20th century Africa have proposed that the positive impact of British colonial education policy was due to its reliance on mission schools and their decentralized organization and funding as well as their use of local languages and African teachers.

Given the lack of data on missions for African countries in the long term (1800 – 1970), we change temporal focus and analyze the impact of missionary activity and colonialism(the latter as a residual mechanism) using a cross-section WLS regression focused on the years 1900 – 1970. This leaves us with 35 countries for which we can calculate the average rate of growth of numeracy for at least three decades between 1900 and 1970 and measure (or reasonably estimate through linear interpolation) numeracy in 1900. We capture the impact of colonial rule using the same method applied to the panel-data model, i.e., through a dummy variable. It is worth noting that with this model, we capture different information compared to that from the previous panel-data specification, where we explored how being a colony increased the pace of human capital accumulation *within* African countries.

[Table 8 here]

Column 1of Table 8 shows the results based on the hypothesis that the premium on human-capital growth observed for British colonies in the panel-data model was due to the impact of missions, which are seen as the main driver of schooling expansion in twentieth-century Africa prior to WWII (see e.g., Nunn 2010, Frankema 2012 and Woodberry 2012).To explore this hypothesis, we include missionaries per 10,000 inhabitants (Woodberry 2012).[7]According to this model, the number of missionaries has a positive and large impact on the growth of numeracy between 1900 and 1970 (see beta coefficients in parentheses). British education policy in the colonies has a positive impact on the growth of numeracy between 1900 and 1970 even when we take into account missionary activity. In a second

[7]Because Gallego and Woodberry (2010) have shown that the interaction of Catholic and Protestant missions was important for the development of schooling in colonial times, we use an index that captures both.

model, we explore whether the *interaction* of British colonialism and missionary activity was also important for the development of numeracy in Africa. In this specification (column 2), all of the variables previously used retain their sign and significance; interestingly, the interaction of colonial rule and missionary activity is generally negative; it becomes positive only in the case of British colonial rule, which indicates that this education policy interacted positively with missions. This provides additional evidence supporting the hypothesis that the British were successful in fostering education in their colonies thanks to their reliance on local forces. However, the significance and the sign of the interaction "Colony x UK x Missionaries/10,000 inhabitants 1923" suggests that colonial institutions did contribute to this pattern beyond a mere laissez faire attitude towards missions. This interaction should be further explored by future research. Finally, it is worth noting that the effect of colonialism on numeracy in British colonies does not depend on the fact that British sorted themselves into countries with better human capital accumulation because the dummy that identifies countries that had been (or would later be) British territories shows a negative sign.

[Table 8 here]

Conclusions

We investigate the effect of colonial rule on the growth of numeracy in a panel of African colonies from 1730 to 1970 thanks to new estimates of human capital accumulation. Such a dataset allows an original reconstruction of trends in education on the continent before, during and after the intense period of colonization experienced during the late-nineteenth and early-twentieth century. Our results show that colonial rule – in spite of its otherwise negative effects – was associated with a significant premium on human capital accumulation; alternative estimates based on 2SLS regressions and Propensity Score Matching show that endogeneity is probably not a major issue. Our findings are consistent with earlier evidence on human capital development during the colonial period, as well as with previous evidence that even a detrimental event, such as the foreign occupation of a country and colonialism, can have positive effects on long-run growth via the development of institutions (for the importance of market-related institutions, see Acemoglu, Cantoni, Johnson and Robinson 2011; for a history of the impact of Soviet occupation on human capital development in Central Asia, see Ghanem and Baten 2016).

However, by disentangling the effect of colonialism across European occupying powers, we find that the premium on the growth of numeracy associated with colonial occupation was driven by the effect of British colonial education policy, while it appears to have been neutral, or even negative, for other countries. This, in turn, depended on the interaction between British colonial education policy and missionary activity: the openness of the British colonial administration towards mission schools and the use of native languages (especially in rural areas) brought about a sustained increase in the numeracy of Africans, which remained unparalleled compared to other regions of the continent. Therefore, the presence of "killer apps" brought by Europeans to the continent, such as well-organized school systems, was possibly a necessary condition to prompt sustained development – but definitely not a sufficient one. Our analysis thus mediates between the positive and negative views of colonialism. Although the British system favored the growth of education thanks to its reliance on local structures and human capital, occupation by other European rulers proved to be detrimental for the continent's human capital accumulation.

The link between colonialism and human capital accumulation has crucial implications for today's development in Africa: the levels of numeracy of the early twentieth century, which were influenced by colonial education policy, are highly correlated with GDP per capita (2000) across African countries (although this effect is non-linear, see Figure A1 in the Appendix). Their inclusion in a simple cross-section regression explains c. 30 percent of the variation in economic performance today, a result that is robust to the inclusion of per capita GDP in 1950 (see Table A2 in the Appendix). This confirms the view that human capital development, and math and quantitative skills in particular, play an important role as determinants of a country's economic fortune. Furthermore, the persistent effect of past numeracy on today's development shows that education policy and reforms should not be based on short-term targets but rather on long-term objectives.

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Tables

Table 1 – (continues on the next page): observations (empty cells: missing values) for numeracy across African countries, 1730 – 1970

Country	1730	1740	1750	1760	1770	1780	1790	1800	1810	1820	1830	1840	1850	1860	1870	1880	1890	1900	1910	1920	1930	1940	1950	1960
Algeria											o	o	o	o	o	o	o	o	o	o	o			
Angola		o	o	o	o	o	o																	
Benin			o		o													o	o	o	o	o	o	
Botswana																o	o	o	o	o	o	o	o	o
Burkina Faso																		o	o	o	o	o	o	
Burundi																			o	o	o	o	o	o
Cameroon			o			o									o		o	o	o	o	o	o		
Cape Verde																o			o	o	o	o	o	o
Central African Rep.																		o	o	o	o	o		
Chad																						o	o	o
Congo	o	o	o	o	o	o	o							o										
Cote d'Ivoire																			o	o	o	o	o	o
DR of Congo																		o	o	o	o	o	o	
Egypt								o	o	o	o	o	o	o	o	o	o	o	o					
Ethiopia																						o	o	o
Gabon																							o	o
Gambia																	o	o	o	o	o	o		
Ghana			o									o	o	o			o	o	o	o	o	o		
Guinea	o	o	o	o	o	o													o	o	o	o	o	
Guinea-Bissau		o	o	o	o	o									o	o	o	o	o	o				
Kenya											o	o				o	o	o	o	o	o	o	o	o
Lesotho													o	o	o	o	o	o	o	o	o	o	o	

Country (cont'd)	1730	1740	1750	1760	1770	1780	1790	1800	1810	1820	1830	1840	1850	1860	1870	1880	1890	1900	1910	1920	1930	1940	1950	1960
Liberia																	o	o	o	o	o	o		
Libya																	o	o	o	o	o	o		
Madagascar																	o	o	o	o	o	o		
Malawi																			o	o	o	o	o	
Mali		o	o	o	o	o											o	o	o	o	o	o		
Mauritania																			o	o	o	o	o	
Mauritius																	o	o	o	o	o	o		
Morocco																o	o	o	o	o	o			
Mozambique				o	o	o														o	o	o	o	o
Namibia													o	o	o									
Niger																						o	o	o
Nigeria		o	o	o	o	o										o	o	o	o	o	o			
Rwanda																				o	o	o	o	o
Senegal	o	o	o	o	o	o		o	o	o	o	o	o	o	o	o	o			o	o	o	o	o
Somalia																o	o							
South Africa								o	o	o	o	o	o	o	o	o	o	o	o	o	o	o	o	
Sudan			o	o	o									o	o					o	o	o	o	o
Swaziland																o	o	o	o	o	o	o	o	
Togo	o	o	o	o	o	o								o		o	o	o	o	o	o	o		
Tunisia																o	o	o	o	o	o			
Uganda														o	o	o	o	o	o	o	o	o	o	
UR of Tanzania														o		o	o	o	o	o	o	o	o	
Zambia																	o	o	o	o	o	o	o	o
Zimbabwe													o	o	o	o	o	o	o	o				

Table 2 – FE panel-data model, 1730 – 1970 (column 1 & 3) and 1800 – 1970 (column 2 & 4)

	(1)	(2)	(3)	(4)
Dep. Var.: Av. Annual Growth of Numeracy	1730 - 1970	1800 - 1970	1730 - 1970	1800 - 1970
Initial numeracy	0.0001	-0.0001	0.0002	0.0001
	(0.740)	(0.815)	(0.555)	(0.770)
Colony effect	0.0122	0.0253**	0.0288	0.0471***
	(0.189)	(0.026)	(0.103)	(0.009)
Before 1880 (dummy)			-0.0260**	-0.0106
			(0.029)	(0.677)
Colony effect x Before 1880			-0.0293	-0.0402**
			(0.126)	(0.034)
Annual growth population		-0.2966***		-0.3190***
		(0.000)		(0.000)
Urbanization rate		0.1657***		0.1121**
		(0.008)		(0.032)
Intensity of int. conflicts		-0.0318***		-0.0296***
		(0.001)		(0.001)
Intensity of dom. conflicts		-0.0160		-0.0198
		(0.387)		(0.209)
Cattle p.c.		-0.0021		-0.0079
		(0.793)		(0.321)
Cropland (sq. km) p.c.		0.0060		0.0014
		(0.672)		(0.937)
Constant	-0.0135	-0.0325	0.0170	-0.0008
	(0.607)	(0.377)	(0.474)	(0.987)
Observations	284	238	284	238
Number of countries	44	41	44	41
Time dummies	Y	Y	Y	Y
Adjusted R-squared	0.289	0.297	0.303	0.327

Robust p-values in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3 – FE panel-data model with heterogeneous effects of colonial rule according to colonizer, 1800 – 1970 (column 1) and 1900 – 1970 (column 2)

	(1)	(2)
Dep. Var.: Av. Annual Growth of Numeracy	1800 - 1970	1900 - 1970
Initial numeracy	0.0000	0.0011***
	(0.963)	(0.000)
Colony effect	0.0001	-0.0208***
	(0.995)	(0.000)
Colony effect x UK	0.0285*	0.0421***
	(0.074)	(0.001)
Colony effect x France	-0.0028	-0.0055
	(0.837)	(0.460)
Annual growth population	-0.3020***	-0.2284
	(0.000)	(0.239)
Urbanization rate	0.1919***	0.0428
	(0.007)	(0.253)
Intensity of int. conflicts	-0.0281***	0.0002
	(0.008)	(0.985)
Intensity of dom. conflicts	-0.0221	-0.0052
	(0.177)	(0.569)
Cattle p.c.	-0.0039	-0.0058
	(0.606)	(0.201)
Cropland (sq. km) p.c.	0.0086	0.0041
	(0.582)	(0.742)
Constant	-0.0362	-0.0841***
	(0.409)	(0.005)
Observations	238	182
Number of countries	41	40
Time dummies		
Adjusted R-squared	0.302	0.406

Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the “base” effect of British or French rule were dropped by the regression software as result of their strong collinearity with the country Fixed Effects.

Table 4 – First stage for the 2SLS estimates

2SLS models: first-stage regressions Dependent variable: colony effect (dummy)	(1) All	(2) British	(3) Non-British
Minimum Saharan distance (000s of kms)	0.1188*	0.1488	-0.1494**
	(0.058)	(0.354)	(0.025)
Minimum Atlantic distance (000s of kms)	-0.0810***	-0.2526**	0.0557
	(0.000)	(0.030)	(0.351)
Eur. fiscal capacity x Minimum Saharan distance (000s of kms)	-0.0651***	-0.0787***	0.0051
	(0.000)	(0.002)	(0.751)
Eur. fiscal capacity x Minimum Atlantic distance (000s of kms)	0.0507***	0.0555***	-0.0099
	(0.000)	(0.000)	(0.364)
Control variables	Y	Y	Y
Macro-regional dummies	Y	Y	Y
Time dummies	Y	Y	Y
Weak-identification test (Kleibergen-Paaprk Wald F stat)	19.3	20.01	2.13
Stock-Yogo weak ID test critical values (5% max IV bias)	16.85	16.85	16.85
Observations	238	110	128

Robust p-values in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5 – 2SLS panel-data model, second-stage estimates

2SLS models. Dep. variable: av. annual growth of numeracy Countries included	(1) All	(2) British	(3) Non-British
Colony effect	0.0281**	0.0397***	0.0024
	(0.017)	(0.007)	(0.870)
Control variables	Y	Y	Y
Macro-regional dummies	Y	Y	Y
Time dummies	Y	Y	Y
Observations	238	110	128
R-Squared	0.361	0.499	0.148
Underidentification test (p-value)	0.0000	0.0031	0.0683
Weak-identification test (Kleibergen-Paaprk Wald F stat)	19.3	20.01	2.13
Stock-Yogo weak ID test critical values (5% max IV bias)	16.85	16.85	16.85
Hansen J stat (p-value)	0.0716	0.3153	0.1367

Robust p-values in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6 – FE panel-data model results based on the Propensity Score Matching (PSM) sample, 1800 – 1970. Column 1 reports results assuming a homogeneous effect of colonial rule across countries; column 2 introduces heterogeneous effects; columns 3 and 4 estimate the same equations but introduce control variables based on ClioInfra data.

Dep. Var.: Av. Annual Growth of Numeracy	(1)	(2)	(3)	(4)
Initial numeracy	0.0001	0.0001	0.0002	0.0003
	(0.669)	(0.316)	(0.303)	(0.131)
Colony effect	0.0123	-0.0138	0.0164	-0.0122
	(0.228)	(0.258)	(0.235)	(0.392)
Colony x UK		0.0415***		0.0440***
		(0.000)		(0.000)
Observations	273	273	258	258
Number of countries	39	39	39	39
Time dummies	Y	Y	Y	Y
Adjusted R-squared	0.441	0.473	0.490	0.522

Robust p-values in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: the "base" effect of British or French rule were dropped by the regression software as result of their strong collinearity with the country Fixed Effects. The countries included in the PSM regressions are Algeria, Belize, Botswana, Brazil, Central African Republic, Chad, Chile, Costa Rica, Cuba, Ecuador, Egypt, El Salvador, Eritrea, Guadeloupe, Guatemala, Haiti, Honduras, India, Lesotho, Libya, Mexico, Morocco, Myanmar, Namibia, Nicaragua, Niger, Pakistan, Panama, Peru, Puerto Rico, Qatar, Senegal, Somalia, South Africa, Sri Lanka, Suriname, Togo, Trinidad and Tobago, Tunisia, Vanuatu and Zambia

Table 7 – FE panel-data model, 1800 – 1970 (column 1) and 1900 – 1970 (column 2). Observations based on census sources.

	(1)	(2)
Dep. Var.: Av. Annual Growth of Numeracy	1800 - 1970	1900 - 1970
Initial numeracy	0.0010***	0.0012***
	(0.002)	(0.003)
Colony effect	-0.0188**	-0.0219***
	(0.018)	(0.001)
Colony effect x UK	0.0256***	0.0377***
	(0.000)	(0.001)
Colony effect x France	-0.0002	-0.0140**
	(0.974)	(0.047)
Control variables	Y	Y
Number of countries	31	31
Time dummies	Y	Y
Observations	154	134
Adjusted R-squared	0.919	0.429

Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The “base” effect of British or French rule were dropped by the regression software, probably a result of their strong collinearity with the country Fixed Effects (both types of dummies are time-invariant).

Table 8 – Cross-section model, 1900 – 1970: what mechanism behind colonial rule?

Dependent variable: average annual growth of numeracy	(1)	(2)
Initial numeracy	-0.0002***	-0.0002***
	(-0.8215)	(-0.6597)
Colony effect	-0.0035**	0.0031
	(-0.2996)	(0.2605)
Colony effect x UK	0.0069**	0.0651*
	(0.6924)	(6.5225)
UK	-0.0116***	-0.0726**
	(-1.1848)	(-7.4080)
Missionaries/10,000 inh. 1923	**0.0064*** **	**0.0387** **
	(0.8003)	**(4.8644)**
Colony effect x Missionaries/10,000 inh. 1923		-0.0377*
		(-4.4686)
Colony effect x UK x Missionaries/10,000 inh. 1923		**0.0074** **
		(0.6833)
Constant	0.0197***	0.0127***
	(.)	(.)
Control variables	N	N
Observations	35	35
Adjusted R-squared	0.657	0.741

Robust normalized beta coefficients in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Missionary activity per 10,000 inhabitants in 1923 is the average between the number of foreign Catholic priests and the one of Protestant missionaries. The length of missionary activity is obtained as an average between the Catholic priests and Protestant missionaries.

Figures

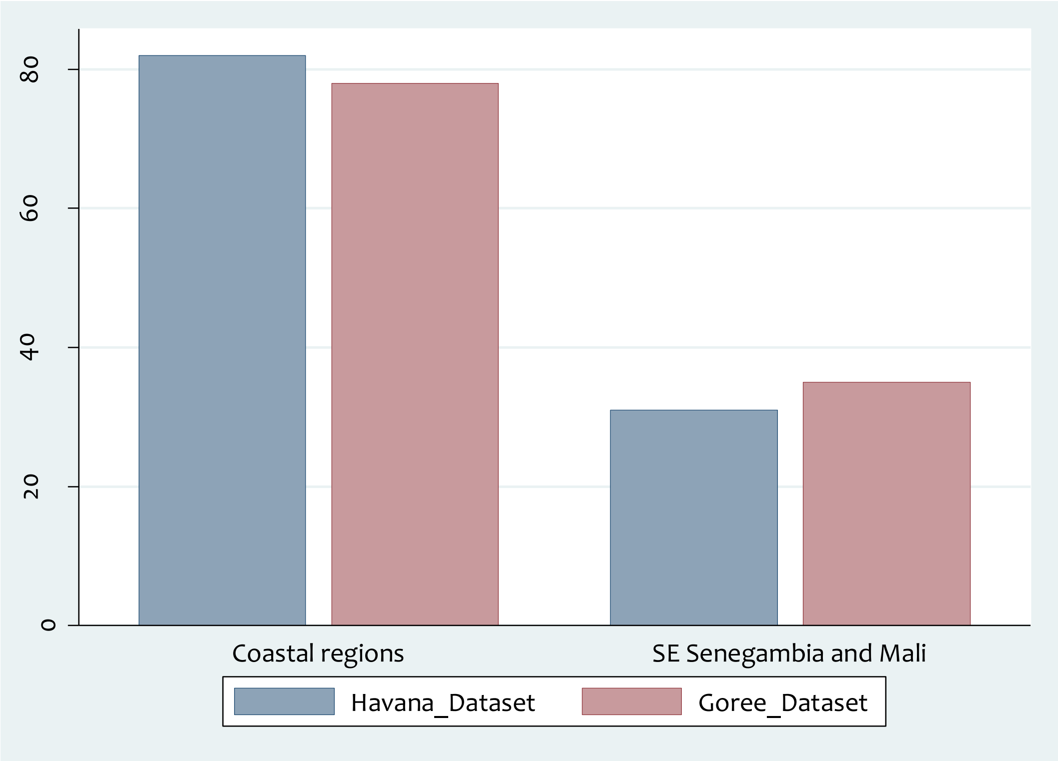


Figure 1: An assessment of potential selectivity through estimates of the numeracy of Africans (in %) from two regions and two different sources; Havana (a slave census at the port) and Gorée (a population census).

Notes: We measure the numeracy of people living in the same areas using different sources to show that the numeracy figures that we obtain are not subject to selection bias.

Sources: "Havana_Dataset" refers to a dataset on the shipment of African slaves to the port of Havana in the early nineteenth century (1825 – 1840), elaborated from the Transatlantic Slave Trade Database. Two main categories of individuals are taken into account: "coast" refers to slaves shipped from the port of Bissau (Portuguese Guinea) while "interior" represents slaves belonging to Mandingo tribes, who inhabited an area that stretched from today's southwest Mali to southeast Senegal. The "Goree_Dataset" refers to a different dataset on numeracy across regions of Western Sudan (see Cappelli and Baten 2015). Here, "coast" refers to Muslim individuals born in Senegal, in the coastal cities called the Four Communes, while "interior" refers to a sample of Muslims born in French Sudan, today's Kayes in Mali.

Figure 2: Numeracy in Uganda (in %) by birth decade and source—marriage registers (adjusted) versus census data (census of 1969). For a description of the adjustment factors used, see text.

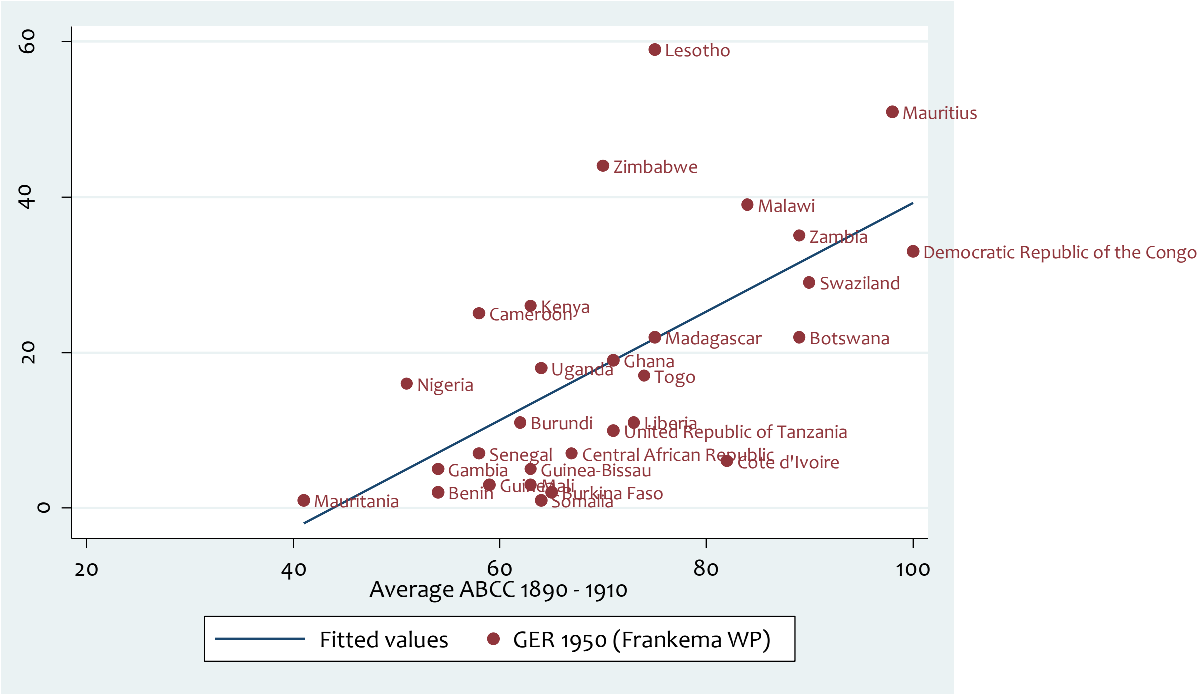


Figure 3: Gross Enrolment Ratios (in %) in 1950 plotted against average numeracy (in %) in 1890 – 1910 across African countries. Source: own elaboration from primary sources and from Frankema (2011).

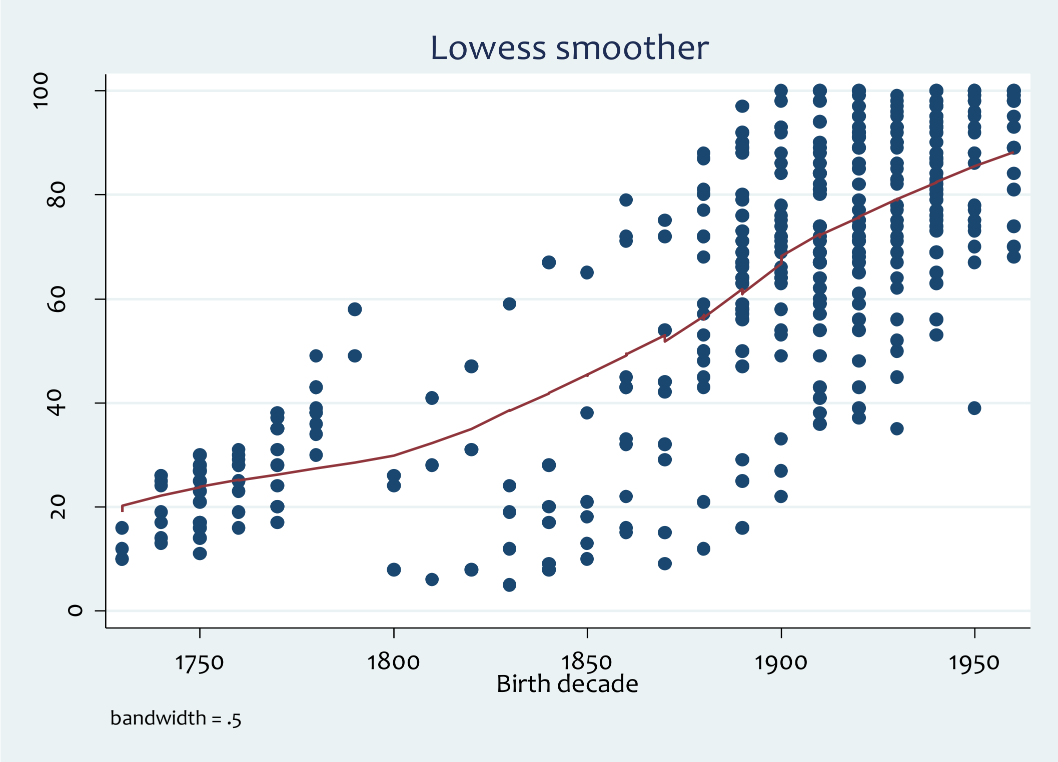
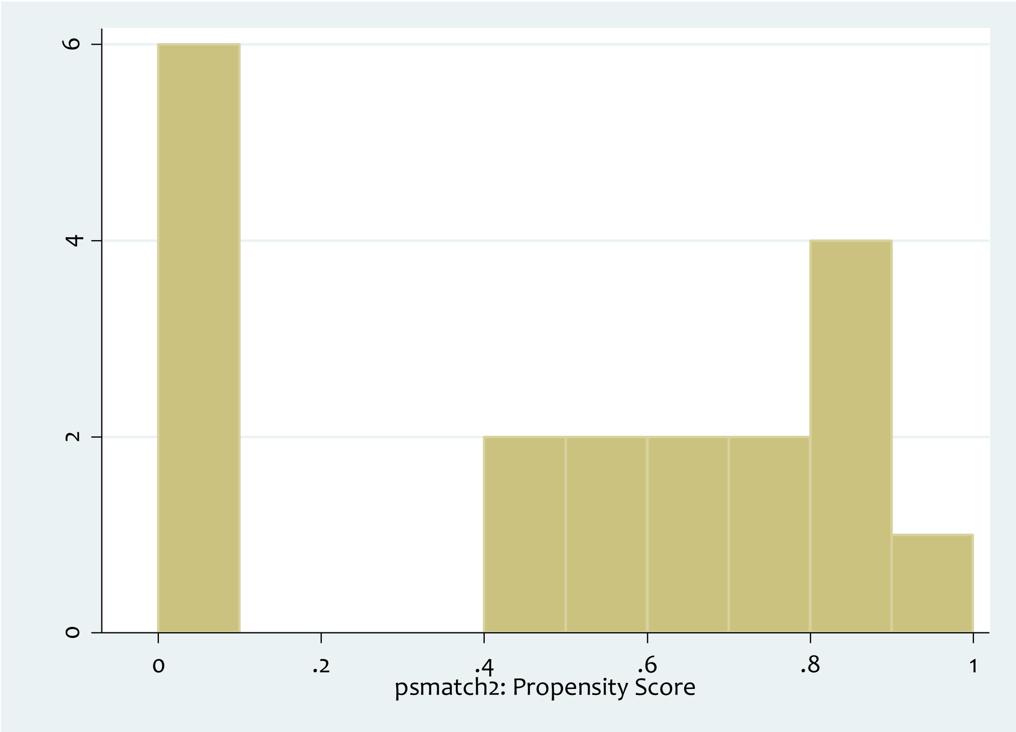
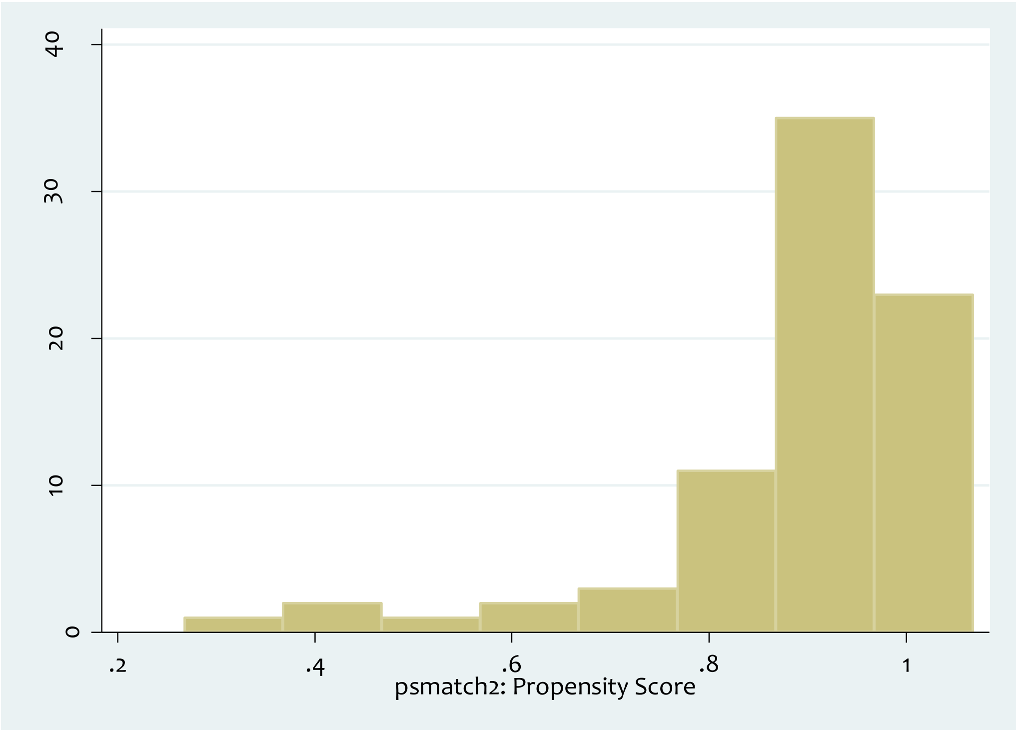


Figure 4: Average trend of numeracy (in %) across African countries, 1730 – 1970 (lowess function with bandwidth equal to 0.5). Notes: the lowess function was used to avoid bias due to the standard mean being calculated through an unbalanced panel.



Colonized after 1850 = 0



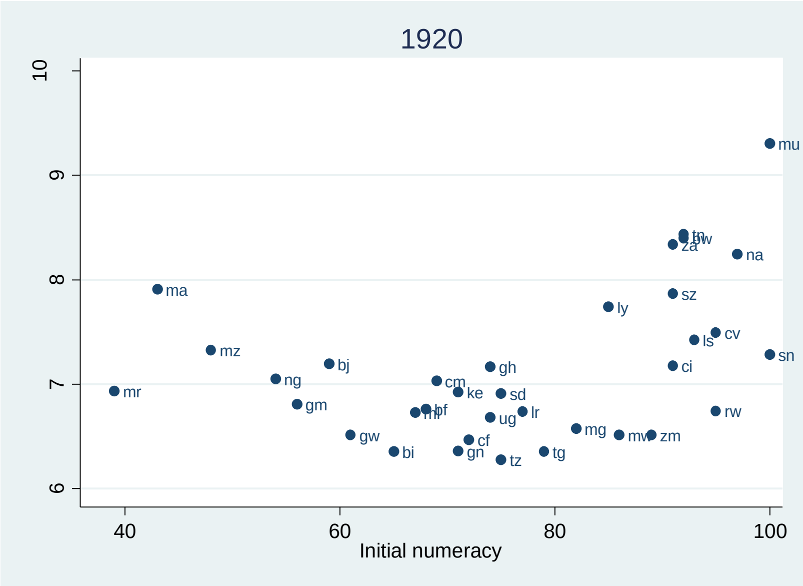
Colonized after 1850 = 1

Figure 5: Propensity scores for "being colonized after 1850" displayed for non-colonies (first graph) and colonies (second graph). Propensity scores are based on population growth, urbanization rates, intensity of international and domestic conflicts, cattle per capita and cropland per capita in 1800.

Appendix

Figures

Figure A1 –Early numeracy (1920) and today's economic development across African countries.



Tables

Table A1 – Probit model to estimate propensity scores for the PSM procedure.

	(1)
Probit model: mfx reported	Ever a colony before 1850?
Annual growth population	-17.4205
	(0.314)
Urbanization rate	-5.8666**
	(0.029)
Intensity of int. conflicts	-0.7393
	(0.320)
Intensity of dom. conflicts	-1.3985
	(0.129)
Cattle p.c.	-1.3046***
	(0.002)
Cropland (sq. km) p.c.	-1.1710***
	(0.001)
Constant	2.7958***
	(0.000)
Pseudo-R2	0.39
Observations	97

Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2 – Early numeracy (1920) and today's economic development across African countries

Log of p.c. GDP 2000	(1)	(2)	(3)
Early numeracy (1920)	-0.1356***	-0.1828***	
	(0.003)	(0.003)	
Early numeracy (1920) squared	0.1028***	0.1379***	
	(0.002)	(0.001)	
Log of p.c. GDP 1950	0.5389***		0.7233***
	(0.005)		(0.000)
Constant	7.6763***	12.7102***	2.3115*
	(0.001)	(0.000)	(0.075)
Observations	35	35	46
Adjusted R-squared	0.564	0.420	0.244

Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: according to the model in column 1, the overall marginal effect of numeracy is positive for values of numeracy in 1920 equal to – and above – c. 66 percent, which is true for the majority of the countries included in the cross-country sample (see also Figure A1 in this Appendix).

Table A3 – Variable definitions and sources

Variable	Definition	Source
Initial numeracy & average annual growth of numeracy	Numeracy is an index of basic cognitive and quantitative skills based on age-heaping. We calculated the growth of numeracy (%) between t and t-1 for each ten-year interval and divided it by ten to obtain the average annual growth	The primary sources used to calculate numeracy and its growth rate are discussed in the text
Colony effect	A dummy variable that is equal to one if a country was a colony in a given period	Own elaboration from Olsson (2009)
Annual growth population	The population growth rate between t and t-1 divided by ten to obtain the average annual value	ClioInfra dataset: www.clio-infra.eu
Urbanization rate	Ratio between urban population and total population	ClioInfra dataset
Intensity of int. conflicts	For each country, the index measures the number of years within a ten-year period that were characterized by international conflicts (the index ranges between 0 and 1)	ClioInfra dataset
Intensity of dom. conflicts	For each country, the index measures the number of years within a ten-year period that were characterized by internal conflicts (the index ranges between 0 and 1)	ClioInfra dataset
Cattle p.c.	Number of cattle per head	ClioInfra dataset
Cropland (sq. km) p.c.	Cropland per head, measured in squared kilometers	ClioInfra dataset
Missionaries per 10,000 inh. (cross-section regression only)	Average between the number of Protestant missionaries per 10,000 inhabitants and the number of foreign Catholic priests per 10,000 inhabitants, both measured in 1923	Own elaboration from Woodberry (2012)