Institutions, Human Capital, and Development*

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Abstract

In this article, we revisit the relationship among institutions, human capital, and development. We argue that empirical models that treat institutions and human capital as exogenous are misspecified, both because of the usual omitted variable bias problems and because of differential measurement error in these variables, and that this misspecification is at the root of the very large returns of human capital, about four to five times greater than that implied by micro (Mincerian) estimates, found in the previous literature. Using cross-country and cross-regional regressions, we show that when we focus on historically determined differences in human capital and control for the effect of institutions, the impact of institutions on long-run development is robust, whereas the estimates of the effect of human capital are much diminished and become consistent with micro estimates. Using historical and cross-country regression evidence, we also show that there is no support for the view that differences in the human capital endowments of early European colonists have been a major factor in the subsequent institutional development of former colonies.

1. INTRODUCTION

1.1. Background

> The factors we have listed (innovation, economies of scale, education, capital accumulation, etc.) are not causes of growth; they *are* growth.
>
> North & Thomas (1973, p. 2)

In laying out their explanation for the "rise of the Western world," North & Thomas (1973) make a distinction between what they argue are the "proximate" and the "fundamental" determinants of economic growth. Their quotation above lists some of the proximate factors: innovation, education, capital accumulation—roughly corresponding to the factors of production embodied in the aggregate production function. The thrust of their argument is that although rich countries clearly have greater levels of total factor productivity (TFP); more educated workers (human capital); and more machines, tools, and factories (physical capital); this is not an explanation of the sources of differences in prosperity. Rather, it just redescribes what it means to be prosperous. The interesting intellectual questions, from their point of view, are, Why is it that some countries are so much more innovative than others, why do they invest much more resources into the educational system, and why do people save and invest to accumulate physical capital?

North & Thomas's (1973) theoretical approach can be captured in a simple diagram:

$$\text{fundamental determinants} \Rightarrow \text{proximate determinants} \Rightarrow \text{economic development.}$$

More specifically, they argue for the following causal chain:

$$\text{institutions} \Rightarrow \left.\begin{array}{c}\text{TFP}\\ \text{human capital}\\ \text{physical capital}\end{array}\right\} \Rightarrow \begin{array}{c}\text{economic}\\ \text{development,}\end{array}$$

which can also be applied when the key fundamental determinant is not institutions but involves other factors, such as culture or geography.

Institutions are by no means absent in standard economic theory, but they are often left implicit. For example, Arrow and Debreu's approach to general equilibrium (see Debreu 1959) presumes a set of very specific institutions that specify the initial ownership of assets in society, enforce private property rights over factors of production and shares that individuals hold in firms in the economy, uphold contracts, and prevent the monopolization of markets. What was missing in economic analyses until recently was systematic evidence on whether and how institutions influence economic development, as well as theoretical insights on why institutions differ across countries and how they evolve.

This empirical challenge is difficult because institutions are endogenous and develop in tandem with other potential determinants of long-run economic performance. So any attempt to ascertain the importance of institutions for economic development by simply looking at their correlation with various measures of economic development, or equivalently throwing them into on the right-hand side of an ordinary least squares (OLS) regression, is unlikely to provide convincing evidence.

The recent literature has therefore focused on various strategies to isolate differences in institutions across countries that are plausibly exogenous to other determinants of long-run economic performance. Acemoglu et al. (2001), following in the footsteps of initial research by Knack & Keefer (1995) and Hall & Jones (1999), adopt just such an approach. They exploit a historically determined, plausibly exogenous source of variation in a broad measure of economic institutions.

In particular, Acemoglu et al. (2001) argue that, in the modern world, Europeans used various types of colonization policies, which created different sets of institutions. At one extreme, European powers set up extractive institutions to transfer resources from a colony to themselves, and this led to the creation of economic institutions supporting such extraction, particularly forms of labor coercion such as slavery, monopolies, legal discrimination, and rules that made the property rights of the indigenous masses insecure. At the other extreme, Europeans settled and tried to replicate, or in fact improve over, European institutions. This led to inclusive institutions, which were much better for economic growth. The colonization strategy adopted by the Europeans was naturally influenced by the feasibility of settlements. Specifically, in places where the European mortality rate from disease was relatively high, the odds were against the creation of settler colonies with inclusive institutions, and the formation of extractive institutions was more likely. Finally, these colonial institutions, once set up, have tended to persist. Based on this reasoning, Acemoglu et al. suggest that the potential mortality rates expected by early European settlers in the colonies could be an instrument for current institutions in these countries. The basic idea of their theory can be summarized as follows:

$$\text{potential mortality of European settlers} \Rightarrow \text{settlements} \Rightarrow \text{past institutions} \Rightarrow \text{current institutions.}$$

As a practical matter, Acemoglu et al. (2001) estimate a simple two-stage least squares (2SLS) regression with log GDP per capita today (in their case, in 1995) as the dependent variable and a measure of economic institutions, proxied by protection against the risk of expropriation, as the key explanatory variable. This variable was instrumented with the logarithm of potential settler mortality. The use of the log transform was motivated by the argument that the relationship between the potential mortality of settlers and settlements is likely to be concave (e.g., few would attempt settlements beyond a certain level of mortality) and that some of the very high mortality estimates resulted from epidemics, unusual idiosyncratic conditions, or small sample variation, and thus were potentially unrepresentative of mortality rates that would ordinarily have been expected by settlers. Acemoglu et al. (2012b) go one step further and use an alternative formulation of the instrument, capping potential settler mortality estimates at 250 per 1,000.[1]

With the original formulation of the settler mortality instrument or its capped version, the results are similar and show a large effect of institutions on long-run development. The results appear robust to controlling for various measures of geography that could be correlated with economic development; continent dummies; whether a country was colonized by the British, French, or other European powers; and various measures of current health. They are also robust, even if somewhat less precise, when the neo-Europes (the United States, Canada, Australia, and

[1]Acemoglu et al. (2012b) follow Curtin (1989, 1998) and the nineteenth-century literature by reporting mortality per 1,000 mean strength (also referred to as "with replacement"), meaning that the mortality rate refers to the number of soldiers who would have died in a year if a force of 1,000 had been maintained in place for the entire year. The 250 per 1,000 estimate was suggested by A.M. Tulloch, the leading expert on soldier mortality in the nineteenth century, as the maximum mortality in the most unhealthy part of the world for Europeans (see Curtin 1990, p. 67; Tulloch 1840, p. 7). Recall that if a variable is a valid instrument, meaning that it is orthogonal to the error term in the second stage, then any monotone transformation thereof is also orthogonal to the same error term and is thus a valid instrument too.

New Zealand) are dropped.[2] Quantitatively, the results are first order, for example, accounting for as much as 75% of the gaps between high- and low-institution countries.[3]

In their empirical strategy, Acemoglu et al. (2001) follow the paradigmatic structure outlined by North & Thomas (1973). They treat physical capital, human capital, and TFP as proximate causes, determined by, and acting as channels of influence for, institutions and thus do not control for these separately. According to this approach, it would be both incorrect and interpretationally confusing to control for, say, TFP differences across countries in trying to explain differences in income per capita with institutions. If the bulk of the effect of institutions on income per capita worked through TFP differences, then such a regression would lead to a zero coefficient on institutions, but this would of course not mean that institutions are not a fundamental determinant of income per capita. Equally worryingly, even if TFP were not a major channel through which differences in institutions impact prosperity, differential measurement error in TFP and institutions would lead to estimates indicating a major role for TFP and no or little role for institutions. To see why, note that TFP is naturally correlated with institutions (which, according to Acemoglu et al., are the fundamental cause working in part through TFP). But if TFP were subject to less measurement error than are institutions, the effect of institutions would be attenuated and would load onto the TFP variable. In summary, under the paradigmatic approach of North & Thomas, controlling for the proximate determinants in trying to estimate the effects of fundamental causes would be what Angrist & Pischke (2008, pp. 64–68) refer to as "bad control."

One can of course challenge this entire conceptual framework. For example, the modernization hypothesis outlined by Lipset (1959) suggests that economic growth and the processes that go along with it—such as expanding education, urbanization, and the development of a middle class—create institutional change. Lipset, in particular, emphasizes the role of these factors in laying the foundations for democracy. According to this view, institutions are likely to be a sideshow or at the very least largely shaped by, or adapted to, the differences in education or urbanization in society. Lipset does not himself propose a theory of why a country did or did not experience modernization, but his emphasis is echoed in much recent research. Easterlin (1981), for instance, puts differential paths of human capital development at the heart of the Great Divergence in economic development that has taken place in the modern world. He conceptualizes the divergence in human capital across countries as idiosyncratic, related, for instance, to religious conversion and Protestantism's emphasis on individuals' ability to read the Bible.

A more recent version of this approach is articulated by Glaeser et al. (2004), who criticize Acemoglu et al. (2001) for putting the institutional cart before the human capital horse. They suggest that the main thing Europeans brought to their colonies was human capital and that they did so differentially across them. In places where they brought more human capital, the economy flourished and society came to be organized differently (and this may or may not have contributed to the flourishing of the economy). Places where they brought no or less human capital

[2]The neo-Europes are the best-case illustration of Acemoglu et al.'s (2001) hypothesis. Dropping them is useful to see whether a similar pattern applies when these four exemplars are excluded.

[3]Acemoglu et al. (2002) take a different but complementary approach and show how the density of indigenous population before Europeans arrived affected the returns from setting up extractive institutions for Europeans (as labor was a key resource enabling Europeans to run extractive colonies). The authors show that the effect of population density around 1500 accounts for why there has been a reversal of fortunes within the former colonies, whereby the areas that were previously more prosperous (and thus more densely populated) ended up relatively poorer today. Engerman & Sokoloff's famous work is also related (e.g., Engerman & Sokoloff 1997, 2011). They emphasize how the diverging development paths of the Americas over the past 500 years are related to initial conditions that led to different institutions in different parts of the Americas.

faltered. According to this perspective, Acemoglu et al.'s (2001) empirical strategy is capturing the significant impact of human capital on long-run development, particularly because it is presumed that Europeans brought more human capital to settler colonies such as the United States. Glaeser et al. (2004) provide several types of evidence to bolster their case, which we discuss in the next section.

1.2. This Article

In this article, we critically assess the roles of human capital and institutions in long-run economic development. The article has three main contributions. First, in Section 3, we provide a brief historical survey of what is known about the human capital that Europeans brought to their colonies in the Americas. The main point of this review is to show that, contrary to what Glaeser et al. (2004) presume, Europeans appear to have brought more human capital per person to their extractive colonies than their settler colonies, using Acemoglu et al.'s (2001) terminology, with inclusive institutions. If the United States is more educated today than Peru or Mexico, this is not because original colonizers there had higher human capital. Rather, it is because the United States established institutions that supported mass schooling, whereas Peru and Mexico did not.

Our second main set of results, presented in Section 5, is based on a new cross-country investigation of the effects of institutions and human capital on GDP per capita today. Here, in addition to exploiting the same sources of variation in institutions as in Acemoglu et al. (2001, 2002, 2012b), we follow Gallego & Woodberry (2009, 2010) and Woodberry (2011) in using variation in Protestant missionary activity as a determinant of long-run differences in human capital in the former colonies. The argument here is that, conditional on the continent, the identity of the colonizer, and the quality of institutions, much of the variation in Protestant missionary activity was determined by idiosyncratic factors and need not be correlated with the potential for future economic development.[4] Because Protestant missionaries played an important role in setting up schools, partly motivated by their desire to encourage reading of the Scriptures, this may have had a durable impact on schooling (Woodberry 2004, 2012; Becker & Woessmann 2009).[5]

We find that when human capital, proxied by average years of schooling, is treated as exogenous by itself or instrumented by Protestant missionary activity early in the twentieth century, it has returns in the range of 25–35% (in terms of the contribution of one more year of average schooling to GDP per capita today). These numbers are very similar to those implied by the regressions reported in Glaeser et al. (2004).[6] They can be directly compared to the contribution of one more year of individual schooling on individual earnings, which are typically estimated to be in the range of 6–10% (see Card 1999 for a survey). But in theory and reality, these two numbers

[4]Consistent with this, **Table 3** shows that Protestant missionary activity in the early twentieth century is uncorrelated with prior levels of schooling.

[5]The conditioning here is important. Protestant missionaries certainly went to some places ahead of others. First, they went to places with a lot of native people because their main objective was conversions. Second, they may have had an easier time penetrating into the interior of countries, which already had better institutions (indeed, Protestant missionary activity is correlated with our historical instruments for institutions). Third, missionary activity differed systematically among continents and among British, French, and other colonies.

[6]Glaeser et al. (2004)—somewhat unusually given the well-established micro and macro literatures on this topic (e.g., Card 1999, Acemoglu & Angrist 2001, Krueger & Lindahl 2001, Acemoglu 2009)—use the logarithm of average years of schooling on the right-hand side, so the exact magnitude of the returns to human capital, especially in comparison to micro estimates, cannot be easily seen from their regressions.

should be more tightly linked.[7] With an elastic supply of capital, no externalities, and no omitted variable biases, the two numbers should be the same (see Acemoglu 2009, chapter 3; Krueger & Lindahl 2001; Caselli 2005). If the supply of capital is inelastic, the aggregate estimate should be even smaller. One way in which the cross-country estimate could be larger is if there are very large human capital externalities. But existing evidence does not support human capital externalities of any significant magnitude, certainly not as large as the own effect (e.g., Acemoglu & Angrist 2001, Rauch 2003, Duflo 2004, Caselli 2005, Ciccone & Peri 2006; but see also Moretti 2004). So there is a prima facie case for a severe omitted variable bias in these regressions that include human capital. Either human capital is proxying for something else or it is capturing some of the effects of institutions.

Our results in Section 5 support the second interpretation. Once we control for the historical determinants of institutions and human capital, or simultaneously treat both variables as endogenous, the estimates of the effect of human capital on long-run development decline significantly and are often in the range of 6–10%, consistent with the micro (Mincerian) evidence—although they are not always significantly different from zero. In contrast, the impact of institutions on long-run development remains qualitatively and quantitatively robust to whether human capital is included in the regression (and treated endogenously) or historical determinants of education are directly controlled for. This evidence provides support for the view that institutions are the fundamental cause of long-run development, working not only through physical capital and TFP, but also through human capital.

For our third main set of results, we turn to cross-regional data (defined mostly at the first-level administrative division, such as US states, Colombian departments, and Argentine provinces). Exploiting variation in Protestant missionary activity across 684 regions across former European colonies, we investigate the role of human capital in long-run regional development. As documented in Acemoglu & Dell (2010) for Latin America and in Gennaioli et al. (2013) more broadly, there is huge regional inequality within countries, and this is correlated with the average educational attainment of the inhabitants of the regions. Gennaioli et al. (2013) interpret this OLS correlation as the causal effect of education on regional prosperity. We show that at the regional level too (once we control for country fixed effects, thus focusing purely on within-country variation), there is a strong correlation between human capital and GDP per capita today, and the coefficient is comparable in size to the returns, in the range of 25–35%, one sees in the cross-country data. However, when differences in average years of schooling are treated as endogenous and instrumented with Protestant missionary activity in the early twentieth century, the coefficient on human capital once again declines significantly and is often far from being statistically different from zero and from the traditional Mincerian (micro) estimates in the 6–10% range.

We interpret our cross-country and cross-regional results not as evidence that human capital is unimportant for long-run economic development. Rather, once the fundamental cause of cross-country economic development, institutional differences, is controlled for directly or through its historical proxies, the effects of human capital are cut down to the plausible range implied by micro evidence. And because these institutional differences are also at the root of the differences in

[7]Gennaioli et al. (2013) provide a model of the spatial distribution of income per capita and human capital with externalities and suggest that the larger impact of schooling on incomes at the macro than the individual level results from the contribution of entrepreneurial inputs (related to average levels of schooling in a region/country). Although this is a theoretical possibility, it is not straightforward to reconcile with existing evidence. For example, Rauch (1993), Acemoglu & Angrist (2001), Duflo (2004), and Ciccone & Peri (2006) exploit variation in average schooling in a local labor market, and this local variation should also capture differences in the average human capital of entrepreneurs. The limited externalities that these papers estimate suggest that the external effects of human capital working through entrepreneurial inputs are also likely to be limited.

human capital, institutions and human capital are positively correlated, and estimates of the latter's effect become somewhat imprecise. But the bottom line appears to be clear: The evidence is quite robust that institutional differences, once instrumented by their historical determinants as in Acemoglu et al. (2001, 2002, 2012b), are the major cause of current differences in prosperity, and it is also fairly consistent with North & Thomas's (1973) overall distinction between fundamental and proximate determinants of long-run economic development.

1.3. Outline

The article proceeds as follows. Section 2 discusses Glaeser et al.'s (2004) and Gennaioli et al.'s (2013) previous attempts to distinguish between human capital and institutions in long-run economic development. Section 3 surveys the historical evidence on the human capital levels of early European colonists in the Americas, documenting that they tended to be more educated in the very extractive Spanish colonies of Latin America and less so in the settler colony that became the United States. Section 4 introduces the cross-country and cross-regional data we utilize in the rest of the article. Section 5 provides our main cross-country results, showing that once we properly control for institutions or their historical determinants, the effect of human capital is estimated to be in a range consistent with micro evidence, and is much smaller than what is sometimes presumed or assumed, whereas controlling for human capital has little qualitative or quantitative impact on the estimates of the effect of institutions on long-run development. Section 6 turns to within-country, cross-regional variation and shows that controlling for various historical and geographic characteristics correlated with the path of development of different regions also reduces the estimated effect of human capital on long-run development to the more plausible range consistent with micro estimates. Section 7 concludes. Additional details on data construction and regression results omitted from the article are contained in the **Supplemental Appendix** (follow the Supplemental Material link from the Annual Reviews home page at **http://www.annualreviews.org**).

Supplemental Material

2. COMMENTS ON THE PREVIOUS LITERATURE

As discussed in Section 1, the most prominent previous contribution attempting to distinguish institutions and human capital as determinants of long-run development is Glaeser et al.'s (2004). After criticizing Acemoglu et al. (2001) for ignoring the role of human capital, Glaeser et al. pursue several strategies to show that human capital is the real driver of differences in long-run economic performance. Here we briefly summarize their contribution and the related contribution by Gennaioli et al. (2013), which makes the same argument using cross-regional-level data.

First, Glaeser et al. (2004) estimate cross-sectional OLS regressions with the growth rate of income per capita between 1960 and 2000 as the dependent variable. In these models (e.g., their table 4), they control for human capital, measured by the logarithm of average years of schooling and various measures of institutions. They find both institutions and human capital to be significant and positively correlated with growth. Unsurprisingly, in view of our explanation above for why human capital thus included would be a bad control, and the fact that these regressions include many endogenous variables (e.g., the initial level of GDP per capita in 1960, in addition to measures of institutions), we believe that the estimated coefficients tell us little about the causal effect of either human capital or institutions.

Glaeser et al.'s (2004) second strategy is to estimate a series of models to show that initial levels of human capital are a better predictor of economic growth over various 10-year periods between 1960 and 2000 than are initial political institutions measured by constraints on the executive. Although the notion that increased constraints on the executive should be correlated with improvements in economic institutions is important (e.g., North & Thomas 1973, North &

Weingast 1989, Acemoglu et al. 2005), the basis of Acemoglu et al.'s (2001) approach was to connect the exogenous component of (economic) institutions to incentives and opportunities underpinning economic development. These regressions speak little to this issue, even if we set aside the usual omitted variable biases. In addition, they are particularly likely to be plagued by differential measurement error. In particular, constraints on the executive at the beginning of a 10-year period are likely to be a highly imperfect measure of the true economic and political institutions of a nation [a point stressed by Acemoglu et al. (2001) in arguing how OLS regressions are likely to underestimate the true effect of institutions on long-run development because of measurement error]. Moreover, as noted above, to the extent that measurement error is less severe in human capital, OLS regressions will tend to find human capital to be significant and institutions not.

Glaeser et al.'s (2004) third strategy is to estimate instrumental variables models similar to Acemoglu et al. (2001) (although again focusing on constraints on the executive rather than Acemoglu et al.'s measures of economic institutions, protection against expropriation). They then instrument human capital and constraints on the executive by a dummy variable for French legal origin in conjunction with either settler mortality or population density in 1500 (see Glaeser et al. 2004, table 11). But the identification strategy implicit in this approach is not clearly discussed.[8] Indeed, it is not clear why French legal origin should be an attractive instrument for human capital or the type of institutions that Acemoglu et al. focus on.[9] Acemoglu & Johnson (2005) show that French legal origin, conditional on settler mortality or population density in 1500, has no or little explanatory power for protection against expropriation or constraints on the executive but has a large effect on contracting institutions, such as the efficiency of courts or legal formalism (which is in turn essentially orthogonal to settler mortality and population density). So it is far from obvious that the combination of settler mortality and French legal origin can be a plausibly exogenous source of variation in human capital and institutions.[10]

Finally, Glaeser et al. (2004) report panel regressions of changes in various measures of political institutions (e.g., constraints on the executive), over five-year periods, on the levels of income per capita, years of schooling, and the level of constraints on the executive and country fixed effects—but without time effects, which implies that part of the identification comes from a time-series correlation of world averages of these measures. They present similar regressions in which the dependent variable is the change in years of schooling over the same five-year period. The results here are that, although years of schooling are correlated with changes in political institutions (significantly in three-quarters of the specifications), political institutions are not correlated with changes in schooling. Regressing the change on the level with country fixed effects—and without time fixed effects—is a rather unusual specification. In related empirical work, Acemoglu et al. (2005, 2008b, 2009) use a standard panel data model (regressing levels on levels with time and country fixed effects) and find no evidence of a causal effect of income or measures of educational

[8]It is particularly unfortunate that there is no clear justification for some of the instruments to create plausible variations in one endogenous variable versus the other, as both endogenous variables in this case, institutions and human capital, are correlated, and thus various misspecifications become more likely (see the discussion in Acemoglu 2004).

[9]In fact, one might even question whether French legal origin is plausibly exogenous in this context. For example, according to the database of legal origins used by Glaeser et al., all of Latin America is coded as having French legal origin. But this is not because of the colonial transplantation of legal traditions but is because of endogenous choices by these countries after independence. For example, although Mexico was invaded by the French during the short-lived rule of Maximilian I between 1864 and 1867, the Mexican Civil Code of 1870, partially inspired by the Napoleonic Code, was adopted not by the French but by the subsequent Benito Juárez regime.

[10]The first stages of Acemoglu & Johnson's (2005) regression show that it is settler mortality that has the most effect on both human capital and institutions.

attainment on democracy. Acemoglu et al. (2014) in turn show that there is a robust and sizable impact of democracy on income per capita.[11]

Gennaioli et al. (2013) report OLS regressions in which all the explanatory variables including measures of human capital and institutions are treated as exogenous. As explained above, this strategy is unlikely to be informative about the causal effects on economic development of human capital and institutions (and we return to this issue further below). Their only remedy for omitted variable biases is to include country fixed effects. Gennaioli et al. (2013, p. 107) note that "by using country fixed effects, we avoid identification problems caused by unobservable country-specific factors." This appears, at least to us, to be insufficient because, as discussed in detail in Acemoglu & Robinson (2012), institutions, as much as human capital, vary across regions within countries—think of the US South versus US North, or the north versus the south of Italy, Brazil, or India.[12] Although Gennaioli et al. (2013) find no evidence that institutional variation explains within-country variation in income per capita, this probably reflects their measures of institutions, which are particularly likely to be ridden with measurement error, and their reliance on OLS regressions, which is likely to attenuate the impact of institutions in the presence of differential measurement error, as explained above.[13]

3. COLONIZATION AND HUMAN CAPITAL

One of the critiques raised against the interpretation of the European colonial experience in terms of institutions is that different patterns of European settlement created not only institutional variation, but also direct variation in human capital. As mentioned above, Glaeser et al. (2004), in particular, argue that the different development paths of North America and South America, for example, were created not by institutional differences but by differences in initial human capital endowments of early colonists. They state that "it is far from clear that what the Europeans brought with them when they settled is limited government. It seems at least as plausible that what they brought with them is themselves, and therefore their know-how and human capital" (Glaeser et al. 2004, p. 289). It is plausible, but it turns out not to be correct.

The historical evidence suggests that the exact opposite of this claim may be true: The conquistadors who colonized South America were more educated than the British and other Europeans who

[11]Easterly & Levine (2013) present OLS regressions in which the proportion of the population of European descent in former colonies in the colonial period is positively correlated with income per capita in 2005. When they include measures of human capital or institutions along with European settlement, the former two are significant, but the latter is not, suggesting that both may be channels via which European settlement is working. But the measures of the proportion of the population of European descent are averages taken centuries after colonization (e.g., 1700–1750 for Jamaica and 1551–1807 for El Salvador) and are outcomes of the incentives and opportunities to colonize, which depended on institutions, among other things potentially influencing GDP today. In addition, their OLS regressions suffer from the same endogeneity and differential measurement error concerns discussed above.

[12]In fact, a recent burgeoning literature documents and exploits the sizable institutional variation within countries. Some prominent examples include Banerjee & Iyer (2005) and Iyer (2010) for India, Acemoglu et al. (2008a, 2012a, 2013) for Colombia, Dell (2010) for Peru, Naritomi et al. (2012) for Brazil, Bruhn & Gallego (2012) for the Americas, and Michalopoulos & Papaioannou (2013) for Africa. Importantly, all of these papers find strong evidence of the effect of institutions on long-run economic development at the within-country level.

[13]Their main measures of institutions are from the World Bank's Enterprise Survey and focus narrowly on a number of regulations affecting firm profitability collected from the urban and formal sector (e.g., number of days spent meeting with tax authorities in the past year or whether access to finance or land is a severe obstacle to business). Besides the measurement error issue, it is not even clear how to interpret these variables and what aspect of institutions they represent. Most poor countries have too little taxation and too small a government (e.g., Acemoglu 2005), so meeting with tax authorities might be good, not bad. No doubt financial markets are less developed in poor regions, but this could be for a plethora of reasons unrelated to institutional differences. These problems probably explain why, as Gennaioli et al. (2013, p. 128) report, "on average, the quality of institutions is lower in the richest region than in the poorest one." This too contrasts sharply with the pattern that the literature on within-country institutional variation finds in many different countries.

colonized North America. Lockhart (1972, p. 35, table 8) provides a detailed analysis of those who accompanied Pizarro on his conquest of Peru. Conquistadors typically signed a contract at the start of their expedition, and the existing contracts allowed Lockhart to calculate that 76.6% could sign their name (this is the basic test used by historians for literacy in the premodern world). Other information, such as diaries, letters, and books, suggests that 51% of the conquistadors could read and write. Similar exercises have been undertaken by Avellaneda (1995, p. 74, table 4.1) for the conquistadors in five different expeditions to New Granada (Colombia). He calculates that average literacy was 78.7%. Other evidence is consistent with very high rates of literacy among Spanish conquistadors. Literacy in Spain was much lower, around 10% (Allen 2003, p. 415). But (*a*) conquistadors mostly came from urban areas, Castille and Andalucia, which had higher literacy, and (*b*) many were hidalgos, second and third sons of nobles who could not inherit land under Spanish law.

So the Spanish conquistadors were a selected sample of the relatively highly educated. What about the Europeans who came to North America? Literacy was of course trending up over this period in Europe, and as Clark (2005) points out, it increased rapidly in seventeenth-century England. This would certainly lead one to anticipate that English colonists, such as those who sailed to Plymouth aboard the Mayflower in 1620, who were arriving later than their Spanish counterparts, and who were religious nonconformists placing heavy emphasis on literacy, should have had far higher human capital than Spanish conquistadors. Even if they did, however, such migrants were not at all representative of the early settlers of British North America, most of whom were indentured laborers (Greene 1988, Galenson 1996). On balance it turns out that the first settlers of British North America were a bit more literate than the British population on average, but this still made them less literate than the Spanish settlers of South America. For example, Galenson's (1981) study of indentured laborers finds that in 1683–1684, 41.2% of a sample of 631 indentured laborers were able to sign their name on their contract of indenture (table 5.2, p. 71). Because 80% of the European population in seventeenth-century Virginia came as indentured laborers, the average literacy rate was certainly less than that of the Spanish conquistadors, even if the remaining 20% had all been literate (something rather unlikely given that male literacy in England was around 60% in the late seventeenth century, according to Clark 2005).

Grubb (1990) pulls together a large number of studies of literacy in colonial America. Jury lists provide one rich source of information about literacy and suggest a figure of 54% for Virginia in the 1600s. Other sources suggest a slightly higher number, perhaps 60% in the 1600s. What about New England? In the evidence that Grubb presents for the period between 1650 and 1700, literacy was around 55% for rural areas of New England, not much different from Virginia, and 77% for Boston. Although this number is as high as those for the Spanish conquistadors, it is for a much later period and it is not representative. The high number for Boston also reflects higher rates of urban literacy everywhere in the colony.[14]

By the nineteenth century, literacy and educational attainment were much higher in North America than in Latin America (Engerman & Sokoloff 2011). But this has nothing to do with whether Europeans brought much or little human capital with them when they first settled and has everything to do with institutions that later developed in different colonies. Some colonies made the decision to invest in education and build schools, which was in turn an outcome of their different economic and political institutions, whereas others invested in holding back the large majority of the population rather than investing in their human capital.

[14]For example, literacy in New York in 1675–1698 was 74.8%. For Philadelphia, data from wills for the period 1699–1706 suggest a literacy rate of 80.0%.

Thus the historical evidence does not support the claim that what distinguishes colonies such as the United States, which developed inclusive institutions, from those such as much of Latin America, which developed extractive institutions, is that the early European settlers brought higher levels of human capital endowments to the former than to the latter group.

4. DATA AND DESCRIPTIVE STATISTICS

We use two data sets and several historical variables. In our cross-country analyses, we use a data set including 62 former colonies. In our within-country analyses, we use a data set including information for 684 subnational regions (coming from 48 different former colonies). **Table 1** presents descriptive statistics for both samples, and in this section, we provide definitions and sources for our main variables and also explain the potential exogenous sources of variation in human capital today we use.

4.1. Cross-Country Data

Our main dependent variable is the log of GDP per capita [purchasing power parity (PPP) basis] in 2005 from the Penn World Tables. Our main indicator of current educational attainment for the cross-country analysis is average years of schooling of the population above age 15 in 2005 (from Barro & Lee 2013a,b and Cohen & Soto 2007).[15] The average country included in our sample has a population with about six years of schooling (roughly corresponding to the educational level of Algeria).

Our main measure of institutions is the rule of law index for 2005 from the Worldwide Governance Indicators constructed by the World Bank (Kaufmann et al. 2013).[16] We use this index because it provides the most up-to-date measure of broad institutions, close to the date at which our dependent variable is measured (2005). This indicator by construction can go from −2.5 to 2.5. The descriptive statistics presented in the top half of **Table 1** imply that our sample has somewhat lower levels of rule of law than the world, with an average of −0.33 and a median of −0.56.

In terms of instrumental variables for institutions in our cross-country analysis, we use the log of potential settler mortality (capped at a maximum level of 250, as in Acemoglu et al. 2012b) and the log population density in 1500 (from Acemoglu et al. 2002). We already explained the motivation for these variables in Section 1.

4.2. Sources of Variation in Human Capital

Our main source of potentially exogenous variation in human capital is Protestant missionary activity in the early twentieth century.[17] In the cross-country analysis, we use the share of Protestant missionaries per 10,000 people in the 1920s from the path-breaking work of Woodberry (2004, 2012). We complement the information provided by Woodberry's work with information from the *World Atlas of Christian Missions* (Dennis et al. 1911) for five countries with missing information in Woodberry (2004, 2012): Australia, Canada, Malta, New Zealand, and

[15]The countries for which we use schooling data from Cohen & Soto (2007) are Angola, Burkina Faso, Ethiopia, Madagascar, and Nigeria.

[16]The rule of law index "captures perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence" (Kaufmann et al. 2013).

[17]This source of cross-country variation has been used previously in Woodberry (2004, 2012) and Gallego & Woodberry (2009).

Table 1 Summary statistics

	Observations	Mean	SD
Cross-country sample			
Log GDP per capita	62	8.291	1.213
Years of schooling	62	6.179	2.878
Rule of law	62	−0.33	0.90
Primary school enrollment in 1900	62	16.66	23.05
Protestant missionaries in the early twentieth century	62	0.458	0.547
Log capped potential settler mortality	62	4.445	0.961
Log population density in 1500	62	0.545	1.727
Dummy for different source of Protestant missions	62	0.081	0.275
Latitude	62	0.181	0.134
British colony	62	0.387	0.491
French colony	62	0.242	0.432
Africa	62	0.419	0.497
Asia	62	0.145	0.355
America	62	0.387	0.491
Cross-region sample			
Log GDP per capita	684	8.359	1.213
Years of schooling	684	5.683	3.053
Temperature	684	21.436	5.794
Inverse distance to coast	684	0.858	0.137
Landlocked region	684	0.519	0.500
Presence of Protestant missionaries in early twentieth century	684	0.526	0.500
Capital city	684	0.0746	0.263
Log population density before colonization	642	0.867	2.386

Readers are referred to Section 4 for variable definitions and sources. Abbreviation: SD, standard deviation.

the United States.[18] In all our empirical analysis, we add a dummy indicating that we use a different source of information for these five countries.

Christian missionaries played an important role in the development of the educational systems in former colonies, perhaps because they "wanted people to read the Scriptures in their own

[18]In particular, we use the share of the number of ordained foreign missionaries per 10,000 people, to be consistent with Woodberry's definition.

language" (Woodberry 2004, p. 27; see also Gallego & Woodberry 2010; Nunn 2010, 2014; Frankema 2012; Woodberry 2012).

Arguing that Protestant missionary activity in the early twentieth century is excludable from regressions of long-run economic development is more challenging. First, missionaries clearly chose where to locate.[19] Second, missionary activity differed between British and French colonies and also across different continents. Third, as also argued by Woodberry, missionary activity may have influenced the path of institutional development, including the emergence of democracy, as well as the schooling system and early human capital.[20] Fourth, missionary activity may have also impacted long-run development by influencing the current religious composition of the population. Nevertheless, conditional on continent dummies; the identity of the colonial power; and, crucially, institutions; the allocation of missionaries across and within countries may have been largely determined by idiosyncratic factors and may be a candidate for an instrument for human capital (and, in robustness checks, we also control for the direct effect of religion).[21] In what follows, although we first report models that do not control for continent dummies, the identity of colonial power, and institutions, our main models do condition on these variables (and we in fact see that there is some evidence of upward bias when these variables are not conditioned on). We also provide support for this source of variation using a falsification exercise.

The average country in our cross-country sample had 0.46 Protestant missionaries per 10,000 people (see the top half of **Table 1**). This is equivalent to the presence of missionaries in the Dominican Republic and Honduras. However, there is a significant degree of variation across countries: Whereas the median country (Nigeria and India) had 0.26 Protestant missionaries per 10,000 people, the country located at the 5th percentile of the distribution (Vietnam) had only 0.01 missionaries, and the country located at the 95th percentile (Jamaica) had 1.81 missionaries per 10,000 people. This variation is related to several determinants of missionary activity mentioned above (see also Gallego & Woodberry 2009, 2010; Woodberry 2012).

Another source of variation in human capital today that we utilize is primary school enrollment rates in 1900 (relative to the population aged between 6 and 14). The data come from Benavot & Riddle (1988) and have been used previously by Gallego (2010). The top half of **Table 1** presents the huge variation in this variable in our sample.[22] This variation reflects certain institutional and idiosyncratic differences across colonies. Gallego (2010), for instance, documents that countries that were administered in a more decentralized fashion have higher enrollment rates. Nevertheless, there is also considerable idiosyncratic variation in this variable, related, for example, to policy

[19]This concern is the reason why we do not use Catholic missionaries as a source of potentially exogenous variation in schooling. This variable is correlated with schooling outcomes, as documented by Gallego & Woodberry (2009). However, in a number of falsification exercises, we also found that the allocation of Catholic missionaries in the early twentieth century was correlated with schooling outcomes in 1900.

[20]Relatedly, Nunn (2014) argues that Protestant missionaries may have had a positive impact on development through their effects on beliefs (in particular, about gender roles).

[21]For example, Colombia has 0.05 Protestant missionaries per 10,000 compared to Paraguay's 0.65. This seems largely related to the hegemony of the conservative political forces in Colombia from the late 1880s until the 1930s. The shift of power to liberals thereafter led to a surge in Protestant missionary activity in the country. In Chile and Paraguay, in contrast, various innovative (and idiosyncratic) strategies by missionaries may have been important in leading to high rates of Protestant missionary activity (see, e.g., Inman 1922). In sub-Saharan Africa, Congo-Brazzaville had a very high presence of Protestant missionaries in the early twentieth century, in part because of the efforts for "the Protestant dream of a 'chain' across Africa, along the River Congo" (Sundkler & Steed 2000).

[22]We imputed an enrollment of 0.6% for countries with missing information. This corresponds to the enrollment rate in 1880 in Cameroon. Our results are robust to using different values for this imputed level.

priorities of their leaders.[23] This variable also captures variation in human capital that was developed at the beginning of the twentieth century, which is a period before the big expansion of Protestant missionaries. Once again conditional on our usual controls, and conditional on institutions in particular, this variable provides another plausible source of variation in human capital.[24] Differences in nineteenth-century enrollment rates also appear to have persisted to the present (e.g., as shown by our first stages below). This type of persistence in human capital is quite common and has various causes. Gallego (2010) provides evidence on the persistence of differences in schooling and discusses its potential causes.

4.3. Regional Data

We use the income per capita variable in 2005 constructed by Gennaioli et al. (2013), in most cases corresponding to GDP per capita (PPP basis).[25] Our main indicator of current educational attainment is again average years of schooling of the population above age 15 in 2005 from Gennaioli et al. (2013). The average region has about 5.7 years of schooling (similar to the schooling levels in the Veraguas region in Panama).

We again utilize historical variation in Protestant missionaries as an exogenous source of variation in average years of schooling today, but our key variable is the location of mission stations rather than the total number of missionaries normalized by population. Specifically, we code a dummy variable for whether there is a Protestant mission station in each region using the maps of Protestant mission stations in 1916 available on the Project on Religion and Economic Change website (**http://www.prec.com**).[26]

Many forces determined the location of mission stations within countries. First, as Nunn (2014) discusses for the case of Africa, geography and climate played a significant role. Second, there is path dependence in terms of previous missionary work (Nunn 2014). Third, variation was created because missionaries followed different strategies when faced with competing religious denominations (as noted in Gallego & Woodberry 2010). In particular, in some places, partly responding to the regulations imposed by different colonial powers, missionaries from one denomination entered into direct competition with missionaries from a different denomination and colocated with them. In others, there was spatial differentiation, leading them to locate their mission very far from that of the other group (see Gallego & Woodberry 2010). Fourth, missionaries were mainly interested in conversions and therefore may have wished to go, when possible, to places with a large native population. However, as Nunn (2014) discusses for the case

[23]For instance, the significant difference in enrollment levels between Argentina (33.9%) and Chile (21.7%) in large part results from the policy priorities between 1868 and 1874 of Argentine president Domingo Sarmiento, who aggressively promoted education to modernize Argentina. One can find similar examples in other continents. For instance, the differences between India (4% enrollment in 1900) and Sri Lanka (22%) seem to be related to reforms that gave local authorities in Sri Lanka more power to determine educational policy (Gallego 2010).

[24]If anything, this variable might be correlated with other positive influences on GDP per capita today, and in that case, it will cause an upward bias in our estimates of the effect of human capital and a downward bias in the effect of institutions (with an argument similar to that in the appendix of Acemoglu et al. 2001).

[25]For the cases in which GDP at the regional level is not available, Gennaioli et al. (2013) use expenditure, wages, gross value-added, or aggregate expenditure to estimate income per capita. In our sample of regions, the eight countries (accounting for 69 of our 684 regions) for which income data are constructed with information different from GDP are Cameroon, Gabon, Malawi, and Nicaragua (using expenditure data); Ghana and Nigeria (using income); Morocco (using GDP and expenditure); and Vietnam (using wages).

[26]These maps are similar to those presented in Roome (1924) and reported in Nunn (2014) for the case of African regions. A similar variable has been used as a determinant of schooling at the regional level in Africa in Gallego & Woodberry (2010) and Nunn (2014).

of Africa, some Protestant missionaries sought to reach more marginalized people in peripheral areas. Therefore, the relationship between the location of Protestant missionary stations and population density is ambiguous. Motivated by this, we present a robustness exercise in which we control for a proxy for population density before colonization.

We also control for a number of proxies for transportation costs (dummies for whether the region was landlocked and proxies of distance to the sea) and also add controls for climate conditions and a dummy for the capital of the country around 1920 being located in a particular region. This is closely related to the approach followed by Nunn (2014) for the case of Africa.

As in the case of our cross-country analysis, there are obvious challenges to the use of the mission station dummy as an instrument for average years of schooling at the regional level. Although the existing literature makes the link to schooling credible, there are the usual challenges to the exclusion restriction. First, despite the above arguments, there may still have existed a residual tendency for mission stations to be placed in areas that were more prosperous or that had greater development potential. Second, Protestant missionaries may have impacted development today through other mechanisms than schooling. Our main response to these concerns is that to the extent that these potential omitted variable biases are important, they will lead to an upward bias in the returns to human capital, and if so, our results showing more limited returns to human capital become even more telling.

The bottom half of **Table 1** presents evidence that 53% of the 684 regions included in our analyses had missionary activity around 1916. Interestingly, there is significant within-country variation in our dummy for the presence of missionaries.[27]

We do not have reliable within-country measures of institutions. In our cross-regional analysis, we therefore focus on estimating the returns to human capital using variation in the presence of Protestant missionaries as a potentially exogenous source of variation. In a robustness check, we use a proxy for (the log of) the population density before colonization, which might have influenced the regional path of institutional development.[28]

5. CROSS-COUNTRY EVIDENCE

In this section, we start with cross-country evidence, turning to cross-regional evidence in the next section. We first show the correlation between human capital and institutions, on the one hand, and GDP per capita today, on the other. The correlations between these two variables and current prosperity likely reflect various omitted variable biases, however—even when we instrument for human capital differences (without controlling for the effect of institutions). We then present semistructural models in which we instrument for one of institutions and human capital and control for various historical determinants of the other to reduce the extent of the omitted variable bias. These models significantly reduce the impact of human capital on current prosperity and show broadly similar effects of institutions on GDP per capita. Finally, we present 2SLS and limited information maximum likelihood (LIML) models in which both institutions and human capital are simultaneously treated as endogenous. These models also show a robust effect of institutions and a much more limited (and quantitatively plausible) effect of human capital on GDP per capita.

[27]Three countries have no Protestant missionaries at all (Benin, Burkina Faso, and Niger), and 10 countries have missionaries present in all of their regions (Bangladesh, Egypt, India, Malawi, Nigeria, Pakistan, South Africa, Sri Lanka, Uganda, and Zambia).

[28]This variable is constructed using information from Bruhn & Gallego (2012) and Goldewijk et al. (2010) and from country-specific sources, which are described in the **Supplemental Appendix**.

Supplemental Material

5.1. Ordinary Least Squares Regressions

In **Table 2**, we start with OLS regressions showing the correlation between prosperity today (measured by GDP per capita in 2005) and measures of human capital and institutions. As noted above, our sample consists of 62 former colonies for which we have data on historical variables such as potential mortality rates of European settlers, population density in 1500, and Protestant missionary activity at the turn of the twentieth century. As with all of our other regressions, we report in this table standard errors that are robust against arbitrary heteroscedasticity.

Column 1 shows the bivariate relationship between average years of schooling and log GDP per capita in 2005. There is a significant relationship with a coefficient of 0.352 [standard error (SE) = 0.027]. The coefficient estimate is very large. As noted in Section 1, with an elastic supply of capital, no externalities, and no omitted variable bias, the coefficient on years of schooling should match the coefficient estimated in micro data regressions of (log) individual wages on individual years of schooling (see Acemoglu 2009, chapter 3). These coefficients are typically estimated to be between 0.06 and 0.10 (corresponding to returns to schooling of 6–10%) (see, e.g., Card 1999). Because the coefficient of 0.352 is quite precisely estimated in this column, a 95% confidence interval easily excludes returns in the neighborhood of 0.06 or 0.10. This result thus suggests that there are either very large human capital externalities, which are not supported by existing estimates (e.g., Rauch 1993, Acemoglu & Angrist 2001, Duflo 2004), or a severe omitted variable bias. The rest of the evidence we present in this article supports the omitted variable bias interpretation.

Column 2 turns to the bilateral correlation between the rule of law index and log GDP per capita. There is a very strong correlation between these two variables as well, with a coefficient of 0.930 (SE = 0.096). Column 3 includes both average years of schooling and the rule of law index in the regression. Both these variables continue to be statistically significant, but the coefficient on the rule of law index is considerably smaller, 0.315 (SE = 0.128), whereas there is only a small decrease in the coefficient of the average years of schooling, which is now 0.287 (SE = 0.035) and thus remains very large relative to the micro estimates mentioned above.

The remaining columns of the table add various other controls to these models. The controls are latitude (the absolute value of the distance from the country to the equator); dummies for the continents of Africa, America, and Asia (the "other" category, including Australasia, is the omitted group); and dummies for British and French colonies (the omitted group is the other European colonies). As explained in the previous section, all these variables are potentially important controls. Most importantly for our context, British, French, and other European colonies may have had both different institutional legacies and human capital policies, and they also have encouraged and allowed different types of missionary activities. These controls generally have little effect on the statistical significance of our estimates or their quantitative magnitudes. For example, including all the above-mentioned controls simultaneously in column 12 reduces the coefficient of average years of schooling to 0.248 (SE = 0.050) and increases the coefficient on the rule of law index slightly to 0.428 (SE = 0.168). Interestingly, none of these controls is individually significant at the 5% level in this column (more generally, latitude and British and French colony dummies are never significant, and continent dummies are not significant when both human capital and institutional variables are included).

There are other potential problems in interpreting the results in **Table 2**. As discussed in Section 1, one comes from the likelihood of differential measurement error in human capital and institutions. To the extent that there is greater measurement error in our measure of institutions than in the measure of human capital, and particularly because human capital is in part determined by institutions and thus correlated with them, the effect of institutions will tend to be attenuated and load on to human capital. This will cause downward bias in the estimates of the effect of

Table 2 Ordinary least squares (OLS) cross-country regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent variable: log GDP per capita in 2005											
Years of schooling	0.352 (0.027)		0.287 (0.035)	0.332 (0.033)		0.286 (0.034)	0.304 (0.048)		0.229 (0.049)	0.322 (0.049)		0.248 (0.050)
Rule of law		0.930 (0.096)	0.315 (0.128)		0.865 (0.128)	0.280 (0.161)		0.818 (0.149)	0.411 (0.169)		0.821 (0.154)	0.428 (0.168)
Latitude				1.072 (0.757)	0.801 (0.866)	0.46 (0.845)	1.11 (0.725)	0.067 (0.862)	0.288 (0.784)	1.132 (0.731)	0.053 (0.921)	0.301 (0.774)
Africa							−0.243 (0.350)	−0.726 (0.345)	0.005 (0.374)	−0.263 (0.348)	−0.736 (0.356)	0.000 (0.366)
America							0.015 (0.214)	0.437 (0.270)	0.456 (0.281)	−0.087 (0.256)	0.435 (0.288)	0.348 (0.314)
Asia							0.055 (0.428)	−0.263 (0.325)	0.192 (0.367)	0.095 (0.426)	−0.266 (0.332)	0.249 (0.367)
British colony										−0.216 (0.244)	−0.004 (0.257)	−0.269 (0.240)
French colony										0.042 (0.288)	0.021 (0.347)	0.024 (0.280)
Observations	62	62	62	62	62	62	62	62	62	62	62	62
R^2	0.699	0.47	0.729	0.711	0.476	0.731	0.718	0.655	0.75	0.724	0.655	0.758

These are OLS regressions with one observation per country. Readers are referred to Section 4 for variable definitions. Standard errors robust against heteroscedasticity are in parentheses.

institutions and upward bias in the estimates of the effects of human capital. This is a further reason for trying to develop instruments for education and institutions because, provided that the measurement error is classical, instrumental variable estimates would correct for this problem. Another factor that might cause an upward bias in the estimated coefficient on human capital in OLS regressions comes from reverse causality: Higher income levels may, through various channels, cause higher schooling.

In summary, in OLS regressions, both human capital and institutional variables appear to be strongly correlated with current prosperity. For usual reasons, however, these correlations cannot be read as causal, and in this context, there is a prima facie case that omitted variables are potentially important as the coefficient on average years of schooling tends to be about five times the magnitude that would be consistent with micro evidence.

5.2. Semistructural Models

In this subsection, we make our first attempt at reducing the potential omitted variable bias in the OLS regressions reported in **Table 2**. We start with semistructural models in which either institutions or human capital is treated as endogenous, while we directly control for historical determinants of (potential instruments for) the other. Evidently, these models are closely related to the full 2SLS models in which both institutions and human capital are treated as endogenous and instrumented with the same variables.[29] As shown below, our semistructural models do a fairly good job of reducing the omitted variable bias and, perhaps not surprisingly, lead to broadly similar results to those in our full 2SLS models.

We start in **Table 3** with a falsification exercise for the validity of Protestant missionary activity in the early twentieth century as a source of excludable variation in human capital. Much of the missionary activity took place at the beginning of the twentieth century and should not have had an impact on education in the nineteenth century. We test this idea in columns 1–4 of **Table 3** using data for a sample of 24 countries for which missionary activity clearly started after 1870 [in this table, we do not control for the dummy for the source of the Protestant missionary activity data as they all have the same source, from Woodberry (2004, 2012)]. This table shows that either in bivariate regressions with primary school enrollment in 1870 (or the earliest date available for these countries) or when we control for the same variables as in **Table 2**, there is no significant correlation between Protestant missionary activity in the early twentieth century and the fraction of the population enrolled in primary school in 1870. The coefficient estimates do move around but are never close to being significant.

In the next four columns (5–8), we look at the relationship between the fraction of the population enrolled in primary school in 1940—that is, after several decades of Protestant missionary work—and Protestant missionary activity in the early twentieth century for the same sample of 24 countries. Our results show a stronger correlation between these two variables (the coefficient of Protestant missionary activity is between 2.5 and 6.3 times the estimates in columns 1–4). In column 8, where we control for latitude, colonizer identity, and continent dummies, the coefficient on the Protestant missionary variable is significant at the 10% level (with a p value $= 0.08$). These results therefore suggest that between 1870 and 1940, a considerably stronger correlation between Protestant missionary activity and human capital emerged.

[29]In other words, this strategy is a mixture of 2SLS models and a reduced-form estimation of such models. In this reduced-form estimation, one would regress the left-hand-side variable on all instruments (naturally leaving out the endogenous regressors). Here, we are including all of the instruments for one of the endogenous regressors, while directly instrumenting for the other.

Table 3 Falsification exercise, Protestant missionaries, cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent variable: primary school enrollment in 1870				Dependent variable: primary school enrollment in 1940				Dependent variable: years of schooling in 2005			
Protestant missionaries in the early twentieth century	1.694 (4.124)	5.370 (4.180)	1.833 (2.608)	2.589 (3.446)	10.649 (11.917)	13.624 (12.735)	8.076 (6.830)	11.050 (5.949)	2.172 (1.298)	2.924 (1.312)	2.329 (0.754)	2.483 (0.791)
Latitude		31.174 (22.810)	26.926 (14.399)	38.032 (16.044)		25.226 (39.748)	18.368 (28.301)	36.689 (26.537)		6.371 (4.054)	5.589 (3.051)	5.576 (3.412)
Africa			0.649 (1.159)	−2.105 (2.188)			−5.781 (3.710)	−10.862 (3.587)			−2.262 (0.559)	−2.307 (0.831)
America			13.623 (2.860)	7.524 (3.894)			17.991 (4.279)	13.184 (5.327)			1.112 (0.631)	1.589 (1.115)
French colony				−6.481 (3.568)				−4.122 (5.079)				0.596 (0.848)
British colony				−1.602 (3.077)				13.413 (12.801)				1.440 (0.937)
Observations	24	24	24	24	24	24	24	24	24	24	24	24
R^2	0.004	0.107	0.562	0.611	0.056	0.078	0.568	0.629	0.063	0.089	0.776	0.795

These are ordinary least squares regressions with one observation per country. The sample includes former colonies where Protestant missionary activity started after 1870. Readers are referred to Section 4 for variable definitions. Standard errors robust against heteroscedasticity are in parentheses.

Finally, in the last four columns (9–12), we look at the relationship between average years of schooling in 2005 (our usual measure of human capital today) and Protestant missionary activity in the early twentieth century for the same sample of 24 countries. These models are very similar to our cross-country, first-stage regressions, except for the sample. Similar to our first-stage regressions, there is now a strong effect of Protestant missionary activity on human capital. Overall, the results in **Table 3** support our key assumptions that Protestant missionaries in the early twentieth century did not differentially select into areas with higher human capital, but they did then impact human capital investments in the areas where they located.

In **Table 4**, we report models in which average years of schooling are treated as endogenous, while we control for our two key historical variables generating plausibly exogenous sources of variation in historical institutions: potential settler mortality (capped as in Acemoglu et al. 2012b) and log population density in 1500. The bottom half of the table reports the first-stage relationship between average years of schooling today and our instruments for human capital, Protestant missionary activity in the early twentieth century and primary school enrollment in 1900.

Column 1 is the most parsimonious specification and does not include any variables other than average years of education, except for a dummy for different sources of Protestant missionary data, which, as explained in Section 4, ought to be included whenever we include Protestant missionary activity in the first or the second stage. In particular, it does not include historical determinants of institutional development. The bottom half of **Table 4** shows that there is a strong first stage, with both instrumental variables being statistically significant and an F-statistic for the excluded instruments of approximately 26.[30]

In the second stage, this model leads to a large effect of average years of schooling on log GDP per capita in 2005, with a coefficient similar to the OLS models in **Table 2**, 0.314 (SE = 0.054). Because some of the later models have weaker first stages, throughout we also report 95% (heteroscedasticity-adjusted) Anderson-Rubin (AR) confidence intervals, which are robust against weak instrument problems and heteroscedasticity (Mikusheva & Poi 2006, Chernozhukov & Hansen 2008). This interval also comfortably excludes a zero effect.

Columns 2–4 in **Table 4** add the same controls as in **Table 2** (latitude, continent dummies, and dummies for French and British colonies), which have little impact on the first or the second stage.[31] For example, when all these controls are included simultaneously in column 4, the coefficient estimate on the average years of schooling is 0.317 (SE = 0.116).[32]

If the estimates in columns 1–4 did correspond to the causal effect of human capital on (log) GDP per capita, they would again be much larger than the micro estimates. However, even though these models do instrument for variation in human capital today, they do not control for the effect of institutions.

[30]The dummy for different sources of information for our missionary data is not statistically significant.

Supplemental Material

[31]The estimates for the effects of these control variables in **Tables 4–8** are not reported to save space and can be found in the **Supplemental Appendix**.

[32]The first stage is considerably weaker, and as a result, the 95% AR confidence interval now marginally includes zero. Despite the low values of the F-statistic for the excluded instruments, the Kleibergen & Paap (2006) tests reported at the bottom of **Table 2** suggest that we can reject the hypothesis that the model is underidentified (i.e., the excluded instruments are not correlated with the endogenous regressor). The Kleibergen & Paap test is a Lagrange multiplier (LM) test of the rank of a matrix (Baum et al. 2010): Under the null hypothesis that the equation is underidentified, the matrix of reduced-form coefficients on the L excluded instruments has rank $= K - 1$, where K is the number of endogenous regressors. Under the null, the statistic is distributed as chi-squared with degrees of freedom $= (L - K + 1)$. A rejection of the null indicates that the matrix is full column rank (i.e., the model is identified).

Table 4 Semistructural regressions, years of schooling, cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Second-stage regressions												
	Dependent variable: log GDP per capita in 2005											
Estimation method	2SLS								LIML			
Years of schooling	0.314 (0.054)	0.305 (0.054)	0.274 (0.101)	0.317 (0.116)	0.177 (0.106)	0.171 (0.106)	0.131 (0.128)	0.178 (0.134)	0.177 (0.106)	0.171 (0.106)	0.122 (0.135)	0.170 (0.144)
AR confidence intervals	[0.17, 0.44]	[0.16, 0.44]	[−0.00, 0.48]	[−0.01, 0.56]	[−0.15, 0.41]	[−0.16, 0.40]	[−0.34, 0.43]	[−∞, 0.55]	[−0.16, 0.41]	[−0.16, 0.40]	[−0.35, 0.43]	[−0.36, 0.54]
Log capped potential settler mortality					−0.475 (0.181)	−0.450 (0.189)	−0.427 (0.209)	−0.449 (0.199)	−0.475 (0.181)	−0.450 (0.189)	−0.435 (0.217)	−0.454 (0.204)
Log population density in 1500					−0.114 (0.062)	−0.121 (0.062)	−0.107 (0.060)	−0.085 (0.065)	−0.114 (0.062)	−0.121 (0.062)	−0.109 (0.061)	−0.087 (0.067)
Kleibergen & Paap (2006) test (p value)	0.00	0.00	0.00	0.00	0.03	0.03	0.02	0.01	0.03	0.03	0.02	0.01
Overidentification test (p value)	0.99	0.76	0.43	0.44	0.87	0.94	0.48	0.50	0.87	0.94	0.48	0.50
First-stage regressions												
	Dependent variable: years of schooling											
Estimation method	2SLS								LIML			
Primary school enrollment in 1900	0.088 (0.016)	0.088 (0.016)	0.051 (0.017)	0.051 (0.018)	0.069 (0.016)	0.072 (0.017)	0.046 (0.018)	0.047 (0.021)	0.069 (0.016)	0.072 (0.017)	0.046 (0.018)	0.047 (0.021)
Protestant missionaries in the early twentieth century	0.938 (0.423)	0.958 (0.425)	1.173 (0.318)	1.168 (0.362)	0.657 (0.444)	0.577 (0.462)	0.935 (0.406)	0.938 (0.431)	0.657 (0.444)	0.577 (0.462)	0.935 (0.406)	0.938 (0.431)
Log capped potential settler mortality					−1.042 (0.359)	−1.104 (0.403)	−0.602 (0.461)	−0.629 (0.502)	−1.042 (0.359)	−1.104 (0.403)	−0.602 (0.461)	−0.629 (0.502)
Log population density in 1500					−0.131 (0.139)	−0.120 (0.145)	−0.067 (0.148)	−0.061 (0.180)	−0.131 (0.139)	−0.120 (0.145)	−0.067 (0.148)	−0.061 (0.180)
R^2	0.599	0.599	0.718	0.718	0.677	0.68	0.734	0.734	0.677	0.68	0.734	0.734

(*Continued*)

Table 4 *(Continued)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
F-statistic excluded instruments	25.94	25.49	18.84	12.02	12.62	12.42	8.7	5.58	12.62	12.42	8.7	5.58
Observations	62	62	62	62	62	62	62	62	62	62	62	62
	Control variables included in first and second stage											
Dummy for different source of Protestant missions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Continent dummies	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Colonial origin dummies	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

The top half of the table presents second-stage regressions with years of schooling instrumented using Protestant missionaries and primary school enrollment in 1900, and the bottom half presents the corresponding first-stage regressions, with one observation per country. All variables are described in the main text. Standard errors robust against heteroscedasticity are in parentheses. AR confidence intervals correspond to the 95% Anderson-Rubin confidence intervals robust against weak instruments and heteroscedasticity. The p values of the Kleibergen & Paap (2006) test correspond to a test in which the null hypothesis is that the equation is underidentified, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions + 1). The p values of the overidentification test correspond to a Hansen overidentification test, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions). Abbreviations: 2SLS, two-stage least squares; LIML, limited information maximum likelihood.

We control for institutions in column 5 without any covariates. The log of potential settler mortality is significant both in the first and in the second stage, whereas the log population density in 1500 is marginally significant in the second stage but not in the first stage.[33] More importantly, the 2SLS coefficient estimate for the effect of average years of schooling on log GDP per capita today is considerably smaller than in columns 1–4, 0.177, and only marginally significant (and the 95% AR confidence interval now comfortably includes zero).

As we include our usual controls, the quantitative magnitude of the second-stage coefficient estimate remains similar or becomes a little smaller, and is now far from being statistically significant. In addition, in all cases, the confidence intervals comfortably include returns in the neighborhood of the micro estimates of the effects of human capital on earnings, in the 0.06–0.10 range.[34]

Columns 9–12 of **Table 4** estimate the same models as in columns 5–8, but with LIML, which is median unbiased for overidentified models when there are weak instruments (see, e.g., Davidson & MacKinnon 1993, Staiger & Stock 1997, Angrist & Pischke 2008). The overall picture is very similar to that in columns 5–8, with results broadly consistent with the micro estimates.[35]

Overall, we conclude that our semistructural estimation strategy—controlling for historical determinants of institutions directly and instrumenting for average years of schooling today with their historical determinants—significantly reduces the estimates of the effect of human capital on GDP per capita today and brings these estimates in line with the micro estimates.

Table 5 is the polar opposite of **Table 4**. It treats the rule of law index as endogenous while simultaneously controlling for the historical determinants of human capital today—Protestant missionary activity and primary school enrollment in 1900. Column 1 is again the most parsimonious specification and does not include any variable other than the rule of law index. The bottom half once again shows the first-stage relationship, which always uses both (capped) log potential settler mortality and log population density in 1500 as instruments. The first-stage relationship for column 1 is strong, with an F-statistic of approximately 32 (although log population density in 1500 is significant only at 10%). The effect of the rule of law on GDP per capita is estimated fairly precisely and is quite large (1.413; SE = 0.177).[36] The overidentification test again provides support for the validity of instruments.

Columns 2–4 add our standard controls: latitude, continent dummies, and dummies for French and British colonies. As in **Table 4**, this weakens the first stage somewhat (although now log population density in 1500 is also significant in many specifications). But the second-stage relationship remains very similar quantitatively and in terms of its precision to that in column 1. In

[33]The first-stage relationship for human capital is again somewhat weak (F-statistic of 12.62), but the Kleibergen & Paap test suggests the model is not underidentified (p value of 0.03).

[34]In all these models, the overidentification tests do not reject the null hypothesis, providing some, but limited, support for the exclusion restrictions, whereas the Kleibergen & Paap test comfortably rejects the hypothesis that the model is underidentified.

[35]In fact, the LIML models lead to estimates and standard errors that are quite similar to those in the 2SLS models. As Angrist & Pischke (2008) discuss, this pattern suggests that even though the first-stage F-statistics are on the low side, the estimates are unlikely to be driven by weak instrument problems. Reassuringly, this pattern of very similar LIML and 2SLS results is common to all the models.

[36]Quantitatively, this parameter estimate implies that moving from the 25th to the 75th percentile of the distribution of the rule of law (approximately from Sierra Leone's value of −0.92 to Sri Lanka's value of −0.08) increases GDP per capita by 169 log points. This is roughly approximately 75% of the income gap between Sierra Leone and Sri Lanka. This magnitude is similar to that obtained in Acemoglu et al. (2001).

Table 5 Semistructural regressions, rule of law, cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Second-stage regressions												
	Dependent variable: log GDP per capita in 2005											
Estimation method	2SLS								LIML			
Rule of law	1.413 (0.177)	1.634 (0.274)	1.346 (0.194)	1.361 (0.212)	1.705 (0.378)	1.791 (0.450)	1.519 (0.298)	1.424 (0.275)	1.714 (0.383)	1.794 (0.453)	1.592 (0.337)	1.579 (0.353)
AR confidence intervals	[1.15, 1.92]	[1.21, 2.66]	[0.86, 1.86]	[0.88, 1.84]	[1.06, ∞]	[1.06, ∞]	[0.75, ∞]	[0.67, ∞]	[1.06, ∞]	[1.06, ∞]	[0.75, ∞]	[0.67, 2.57]
Primary school enrollment in 1900					0.018 (0.009)	0.020 (0.009)	−0.009 (0.010)	−0.005 (0.010)	0.018 (0.009)	0.020 (0.009)	−0.009 (0.011)	−0.006 (0.011)
Protestant missionaries in the early twentieth century					0.059 (0.212)	−0.001 (0.222)	0.184 (0.170)	0.261 (0.180)	0.058 (0.213)	−0.002 (0.222)	0.170 (0.177)	0.233 (0.194)
Kleibergen & Paap (2006) test (p value)	0.0	0.030	0.020	0.020	0.030	0.120	0.060	0.060	0.030	0.120	0.060	0.060
Overidentification test (p value)	0.230	0.390	0.250	0.140	0.690	0.810	0.340	0.170	0.690	0.810	0.350	0.190
First-stage regressions												
	Dependent variable: rule of law											
Estimation method	2SLS								LIML			
Log capped potential settler mortality	−0.597 (0.098)	−0.476 (0.111)	−0.292 (0.109)	−0.231 (0.109)	−0.402 (0.113)	−0.366 (0.122)	−0.235 (0.103)	−0.226 (0.107)	−0.402 (0.113)	−0.366 (0.122)	−0.235 (0.103)	−0.226 (0.107)
Log population density in 1500	−0.081 (0.058)	−0.083 (0.054)	−0.152 (0.051)	−0.160 (0.050)	−0.062 (0.056)	−0.069 (0.057)	−0.111 (0.057)	−0.126 (0.060)	−0.062 (0.056)	−0.069 (0.057)	−0.111 (0.057)	−0.126 (0.060)
Primary school enrollment in 1900					−0.002 (0.007)	−0.004 (0.007)	0.006 (0.008)	0.003 (0.009)	−0.002 (0.007)	−0.004 (0.007)	0.006 (0.008)	0.003 (0.009)

(*Continued*)

Table 5 *(Continued)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Protestant missionaries in the early twentieth century					0.021 (0.171)	0.066 (0.173)	0.049 (0.165)	0.004 (0.176)	0.021 (0.171)	0.066 (0.173)	0.049 (0.165)	0.004 (0.176)
R^2	0.508	0.551	0.638	0.656	0.603	0.612	0.664	0.669	0.603	0.612	0.664	0.669
F-statistic excluded instruments	31.82	13.48	9.55	8.32	6.27	4.58	5.07	5.45	6.27	4.58	5.07	5.45
Observations	62	62	62	62	62	62	62	62	62	62	62	62
	Control variables included in first and second stage											
Latitude	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Continent dummies	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Colonial origin dummies	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Dummy for different source of Protestant missions	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The top half of the table presents second-stage regressions with rule of law instrumented using log capped potential settler mortality and log population density in 1500, and the bottom half presents the corresponding first-stage regressions, with one observation per country. All variables are described in the main text. Standard errors robust against heteroscedasticity are in parentheses. AR confidence intervals correspond to the 95% Anderson-Rubin confidence intervals robust against weak instruments and heteroscedasticity. The p values of the Kleibergen & Paap (2006) test correspond to a test in which the null hypothesis is that the equation is underidentified, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions + 1). The p values of the overidentification test correspond to a Hansen overidentification test, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions). Abbreviations: 2SLS, two-stage least squares; LIML, limited information maximum likelihood.

all cases, both standard confidence intervals and the AR confidence interval at 95% comfortably exclude a zero effect of this measure of institutions on GDP per capita today.

Columns 5–8 add the historical determinants of human capital today as additional controls, which are the two variables we used as instruments for human capital in **Table 4** (Protestant missionary activity in the early twentieth century and primary school enrollment in 1900). We also include a dummy for the source of Protestant missionary data.[37] In contrast to the pattern seen in **Table 4**, in which the inclusion of historical variables related to institutions significantly reduced the magnitude and statistical significance of the human capital variable, the inclusion of historical variables related to human capital differences across countries has little impact on the relationship between the rule of law index and GDP per capita today: The coefficient estimates for rule of law are very similar to those in columns 1–4 and are always statistically different from zero when we use standard confidence intervals. Because the first stages are often weakened by the inclusion of historical variables, the (heteroscedasticity-corrected) AR confidence intervals become wider and in some specifications take the form of an unbounded interval. Nevertheless, these intervals consistently and comfortably exclude zero and thus indicate a significant impact of institutions on GDP per capita.[38]

In columns 9–12, we replicate the results from columns 4–8 using LIML models. The overall picture is again very similar to that in columns 5–8, with point estimates and standard errors slightly larger than in the corresponding 2SLS estimates.

We therefore conclude from the semistructural models that the relationship between institutions and current prosperity is considerably more robust than that between human capital and current prosperity. Moreover, once we move toward controlling for institutions, the magnitude of the estimates of the impact of human capital on GDP per capita declines from the implausibly large magnitudes and approaches the magnitudes seen in micro estimates. This bolsters the case that models that do not appropriately control for the effect, and the determinants, of institutions tend to suffer from a serious omitted variable bias, inflating the effect of human capital variables.

5.3. Full Two-Stage Least Squares Models

We next estimate models in which both institutions and human capital are simultaneously treated as endogenous and instrumented using the same historical variables we utilize above. Our baseline full 2SLS results are reported in **Table 6**. The bottom half of this table provides the first stages for the two endogenous variables. These first-stage relationships are quite similar to those seen in the context of **Tables 4** and **5**. It is reassuring that the first stages show a pattern in which the instruments are statistically significant only for the relevant variables (i.e., Protestant missionaries and primary school enrollment rates in 1900 for schooling, and potential settler mortality and population density in 1500 for institutions).

Column 1 includes only average years of schooling and the rule of law index (as well as the dummy for the source of Protestant missionary data). The first stages for this regression are given in columns 1–5 in the bottom half of **Table 6**. The coefficient on average years of schooling is 0.223 (SE = 0.073), thus smaller than that in column 1 of **Table 4**. The coefficient on the rule of law index is 1.126 (SE = 0.355), also a little smaller than that in the first column of **Table 5**. However, as

Supplemental Material

[37]As mentioned above, we report the estimates of this and other control variables in the **Supplemental Appendix** to save space.

[38]Acemoglu et al. (2012b) argue that the relevant statistical issue is again whether the confidence interval does or does not exclude zero.

Table 6 Full two-stage least squares (2SLS) and limited information maximum likelihood (LIML) estimates, cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Second-stage regressions								
	Dependent variable: log GDP per capita in 2005							
Estimation method	2SLS				LIML			
Years of schooling	0.223 (0.073)	0.224 (0.074)	0.069 (0.129)	0.186 (0.142)	0.217 (0.077)	0.218 (0.078)	−0.019 (0.194)	0.094 (0.244)
Rule of law	1.126 (0.355)	1.123 (0.378)	1.324 (0.390)	1.062 (0.374)	1.168 (0.387)	1.170 (0.415)	1.701 (0.674)	1.464 (0.730)
Kleibergen & Paap (2006) test (p value)	0.10	0.260	0.110	0.070	0.10	0.260	0.110	0.070
Overidentification test (p value)	0.620	0.60	0.20	0.120	0.630	0.620	0.340	0.220
First-stage regressions								
	Dependent variable: years of schooling				**Dependent variable: rule of law**			
Primary school enrollment in 1870	0.069 (0.016)	0.072 (0.017)	0.046 (0.018)	0.047 (0.021)	−0.002 (0.007)	−0.004 (0.007)	0.006 (0.008)	0.003 (0.009)
Protestant missionaries in the early twentieth century	0.657 (0.444)	0.577 (0.462)	0.935 (0.406)	0.938 (0.431)	0.021 (0.171)	0.066 (0.173)	0.049 (0.165)	0.004 (0.176)
Log capped potential settler mortality	−1.042 (0.359)	−1.104 (0.403)	−0.602 (0.461)	−0.629 (0.502)	−0.402 (0.113)	−0.366 (0.122)	−0.235 (0.103)	−0.226 (0.107)
Log population density in 1500	−0.131 (0.139)	−0.120 (0.145)	−0.067 (0.148)	−0.061 (0.180)	−0.062 (0.056)	−0.069 (0.057)	−0.111 (0.057)	−0.126 (0.060)
R^2	0.677	0.68	0.734	0.734	0.603	0.612	0.664	0.669
Observations	62	62	62	62	62	62	62	62
	F-statistic for excluded instruments in relevant equation							
Institutions	6.44	6.37	1.22	1.25	6.27	4.58	5.07	5.45
Schooling	12.62	12.42	8.70	5.58	0.05	0.16	0.79	0.10
	Control variables included in first and second stage							
Dummy for different source of Protestant missions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Continent dummies	No	No	Yes	Yes	No	No	Yes	Yes
Colonial origin dummies	No	No	No	Yes	No	No	No	Yes

The top half of the table presents second-stage regressions with rule of law and years of schooling instrumented using the log capped potential settler mortality, log population density in 1500, Protestant missionaries, and primary enrollment in 1900, and the bottom half presents the corresponding first-stage regressions, with one observation per country. All variables are described in the main text. Standard errors robust to heteroscedasticity are in parentheses. Coefficients and standard errors for the constants and the control variables are not reported to save space (the **Supplemental Appendix** presents all the estimates). The p values of the Kleibergen & Paap (2006) test correspond to a test in which the null hypothesis is that the equation is underidentified, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions + 1). The p values of the overidentification test correspond to a Hansen overidentification test, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions).

Supplemental Material

covariates are added, the rule of law index remains robustly significant and around the same magnitude, and the average years of schooling become insignificant and its quantitative magnitude declines substantially. For example, in column 3, which includes latitude and continent dummies, the rule of law index has a coefficient of 1.324 (SE = 0.390), and the coefficient on average years of schooling declines to 0.069 (SE = 0.129). When we also include British and French colony dummies, the coefficient estimate for average years of schooling increases to 0.186, but it is even more imprecisely estimated (and the coefficient on the rule of law index declines slightly but remains precisely estimated and significant).

In columns 5–8, we also report LIML models, which show very similar quantitative and qualitative results. For example, with all the covariates, the coefficient estimate for the effect of average years of schooling on log GDP per capita today is 0.094 (SE = 0.244), thus again in the ballpark of the 0.06–0.10 range from micro estimates, and the estimate of the coefficient on the rule of law index is similar to before, 1.464 (SE = 0.730).[39]

Table 7 probes the robustness of the results in **Table 6**. In this table, we only report LIML models to save space. Our main robustness checks are as follows. (*a*) We drop the four neo-Europes (the United States, Canada, Australia, and New Zealand), where the path of institutional development may have been different, and the nature of missionary activity was certainly quite distinct. (*b*) We control for the current prevalence of falciparum malaria, as an overly conservative test for whether some of the effect of potential settler mortality may be working through the current prevalence of malaria (why this is overly conservative is explained in detail in Acemoglu et al. 2001, including their appendix). (*c*) We control for a variety of variables measuring humidity and temperature. (*d*) We control for the fraction of the population with different religious affiliations in 1900 so that we can isolate the effect of Protestant missionary activity from the direct effect of religion.[40]

In all cases, the results are very similar to those in **Table 6**. The coefficient on the rule of law index is always between 1.15 and 1.49 and always significantly different from zero, whereas the coefficient estimate on average years of schooling is never statistically different from zero. In many models, it hovers in the ballpark of the micro estimates. The only exception is in the case of the models in which we control for religious affiliation in 1900, where the estimates increase in magnitude but remain statistically indistinguishable from zero.

In summary, the results from the full 2SLS/LIML models, in which both institutions and human capital are instrumented using historical sources of variation, show a fairly robust effect of institutions on current prosperity and a much more limited effect of human capital. It would be wrong to read these results as implying that human capital does not have a robust effect on GDP per capita. Rather, the results suggest that the impact of human capital is imprecisely estimated but, if anything, is in the ballpark of micro estimates.[41]

Supplemental Material

[39]A potential concern with our full 2SLS models is that one of the instruments for institutions (log population density) and one of the instruments for human capital (primary school enrollment rates in 1900) have somewhat weaker justifications than our other instruments and may be invalid, despite the evidence supporting their validity in the overidentification tests. To investigate this issue, in the **Supplemental Appendix** we estimate exactly identified models using only (capped) potential settler mortality and Protestant missionary activity in the early twentieth century as excluded instruments. Such exactly identified models may be approximately unbiased (or have less bias) in the presence of weak instruments (Angrist & Pischke 2008). The results from these models confirm our conclusions but are, unsurprisingly, less precisely estimated.

[40]It is more informative to use religious affiliations in 1900, as current religious affiliations are likely to be endogenous to Protestant missionary activity in the early twentieth century.

[41]And this is in turn economically quite plausible in view of the small magnitude of the estimates of human capital externalities we discuss above.

Table 7 Robustness exercises: full limited information maximum likelihood models, second-stage regressions, cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	**Excluding neo-Europes**		**All**					
Years of education	0.034 (0.199)	0.174 (0.196)	−0.043 (0.191)	0.041 (0.198)	0.009 (0.136)	0.120 (0.196)	0.119 (0.186)	0.197 (0.187)
Rule of law	1.432 (0.679)	1.149 (0.595)	1.382 (0.591)	1.218 (0.501)	1.193 (0.386)	1.181 (0.389)	1.487 (0.687)	1.336 (0.662)
Kleibergen & Paap (2006) test (p value)	0.09	0.08	0.08	0.11	0.01	0.05	0.14	0.13
Overidentification test (p value)	0.33	0.24	0.52	0.57	0.85	0.81	0.66	0.47
	Control variables included in first and second stage							
Dummy for different source of Protestant missions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continent dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Colonial origin dummies	No	Yes	No	Yes	No	Yes	No	Yes
Falciparum malaria index in 1994	No	No	Yes	Yes	No	No	No	No
Humidity variables	No	No	No	No	No	Yes	No	No
Temperature dummies	No	No	No	No	No	Yes	No	No
Religion affiliation in 1900	No	No	No	No	No	No	No	Yes
Observations	58	58	61	61	62	62	62	62

These are second-stage regressions with the rule of law and years of education instrumented using the log capped potential settler mortality, log population density in 1500, Protestant missionaries, and primary school enrollment in 1900, with one observation per country. Columns 1 and 2 present regressions excluding the neo-Europes (Australia, Canada, New Zealand, and the United States). All the remaining columns present estimates for the complete sample. In columns 3 and 4, we control for the fraction of the population who live in an area where falciparum malaria is endemic in 1994 (from Gallup et al. 1998). In columns 5 and 6, we add the following controls for humidity and temperature: average, minimum, and maximum monthly high temperatures; minimum and maximum monthly low temperatures; morning minimum and maximum humidity; and afternoon minimum and maximum humidity (from Parker 1997). In columns 7 and 8, we add controls for the share of the Catholic, Muslim, and Protestant population in 1900 from the *World Christian Encyclopedia*. All variables are described in the main text. Standard errors robust against heteroscedasticity are in parentheses. The p values of the Kleibergen & Paap (2006) test correspond to a test in which the null hypothesis is that the equation is underidentified, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions + 1). The p values of the overidentification test correspond to a Hansen overidentification test, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions).

5.4. Does Human Capital Cause Institutions?

A final question in this context is whether differences in human capital cause current institutional differences. To investigate this issue, we put our measures of institutions today on the left-hand side of regressions identical to those reported in **Table 4**. The results are reported in **Table 8**. The first-stage regressions for this model are identical to those in the bottom half of **Table 4** and are omitted to save space.

The pattern we see in **Table 8** is quite similar to the one in **Table 4**. In the models in columns 1–4, although the AR confidence intervals do include zero, there is a fairly robust positive correlation

Table 8 Effects of years of schooling on institutions, second-stage regression, cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent variable: rule of law											
Estimation method	2SLS								LIML			
Years of schooling	0.081 (0.034)	0.073 (0.034)	0.173 (0.062)	0.163 (0.077)	−0.021 (0.072)	−0.028 (0.072)	0.086 (0.062)	0.039 (0.087)	−0.022 (0.072)	−0.031 (0.073)	0.086 (0.063)	0.038 (0.088)
AR confidence intervals	[−0.01, 0.16]	[−0.02, 0.15]	[−0.05, 0.32]	[−0.02, 0.34]	[−0.27, 0.13]	[−0.28, 0.12]	[−0.09, 0.25]	[−∞, 0.23]	[−0.27, 0.13]	[−0.27, 0.12]	[−0.09, 0.25]	[−∞, 0.23]
Log capped potential settler mortality					−0.425 (0.163)	−0.406 (0.179)	−0.180 (0.093)	−0.197 (0.103)	−0.426 (0.163)	−0.409 (0.180)	−0.180 (0.093)	−0.198 (0.103)
Log population density in 1500					−0.065 (0.061)	−0.072 (0.061)	−0.105 (0.049)	−0.124 (0.052)	−0.065 (0.061)	−0.073 (0.062)	−0.105 (0.049)	−0.124 (0.052)
Kleibergen & Paap (2006) test (p value)	0.0	0.0	0.0	0.0	0.030	0.030	0.020	0.010	0.030	0.030	0.020	0.010
Overidentification test (p value)	0.810	0.570	0.930	0.930	0.840	0.630	0.810	0.80	0.840	0.630	0.80	0.80
Observations	62	62	62	62	62	62	62	62	62	62	62	62
	Control variables included in first and second stage											
Dummy for different source of Protestant missions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Continent dummies	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Colonial origin dummies	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

These are second-stage regressions with years of schooling instrumented using Protestant missionaries and primary school enrollment in 1900. All variables are described in the main text. Standard errors robust against heteroscedasticity are in parentheses. AR confidence intervals correspond to the 95% Anderson-Rubin confidence intervals robust to weak instruments and heteroscedasticity. The p values of the Kleibergen & Paap (2006) test correspond to a test in which the null hypothesis is that the equation is underidentified, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions + 1). The p values of the overidentification test correspond to a Hansen overidentification test, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions). Abbreviations: 2SLS, two-stage least squares; LIML, limited information maximum likelihood.

between (the exogenous component of) the average years of schooling and the rule of law index. However, in columns 5–8, when we introduce settler mortality and population density in 1500 as additional controls, the coefficient on average years of schooling becomes insignificant, and half of the time even has the wrong sign. The results in columns 9–12 using LIML are very similar to those in columns 5–8. Reassuringly, in these models, the coefficient on settler mortality is negative and statistically significant in all columns, although the magnitude of the coefficient halves when we add all the covariates.

These regressions therefore suggest that, although human capital and institutions are positively correlated, as one would expect when institutions impact economic development via all the key proximate determinants, there is no causal impact of human capital on institutions.

6. CROSS-REGIONAL EVIDENCE

In this section, we provide evidence on the effects of human capital on long-run economic development using data from 684 regions from 48 countries.

6.1. Ordinary Least Squares Regressions

Table 9 shows some basic OLS regressions in a similar spirit to **Table 2** but without the institutional variables. The dependent variable is the log of GDP per capita at the regional level, and all specifications include country fixed effects. The first column is the simplest specification in which the only explanatory variable is average years of schooling, in addition to the country fixed effects. Here we use the entire sample of 1,495 observations from Gennaioli et al. (2013) to verify that the baseline results we obtain for our 684 regions are similar to the results in the larger sample.[42]

The estimated coefficient in column 1, 0.282 (SE = 0.013), is highly significant, and its magnitude is similar to those seen in **Table 2**. In column 2, we estimate the same specification on our sample of 684 regions. The results are almost identical, with a coefficient estimate of 0.281 (SE = 0.016).

Adding various covariates does not appreciably change the estimates. Columns 3 and 4 include a quadratic in the distance to the coast (potentially capturing the idea that more isolated regions may be poorer), a dummy for whether the administrative unit is landlocked, and two temperature variables. The estimate of the coefficient on average years of schooling is remarkably stable across the columns in this table. Finally, column 6 includes a control for our proxy for the population density before colonization. Results barely change, and the population density variable is negative and significant, as in Bruhn & Gallego (2012).

6.2. Two-Stage Least Squares Models

In **Table 10** we use the presence of a Protestant mission in the early twentieth century, but now at the regional level, to instrument for average years of schooling. The bottom half of the table contains the first-stage regressions. All columns again include country fixed effects. In column 1 of the bottom half of the table, we see a strong first-stage relationship (e.g., the F-statistic is 28.49). In the second stage, however, once we instrument for average years of schooling, its coefficient falls in

[42]The reason for the difference between the two samples is simple: Given the justification of our instrument, we cannot include Europe, where Protestant missionary activity is not a plausible source of exogenous variation in human capital.

Table 9 Ordinary least squares (OLS) regressions, cross-region sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: log GDP per capita					
Years of schooling	0.282 (0.013)	0.281 (0.016)	0.263 (0.018)	0.258 (0.019)	0.256 (0.019)	0.264 (0.020)
Capital city			0.153 (0.055)	0.156 (0.055)	0.159 (0.055)	0.148 (0.055)
Inverse distance to coast				−0.727 (1.191)	0.235 (1.316)	−0.141 (1.324)
Squared inverse distance to coast				0.473 (0.801)	−0.131 (0.873)	0.167 (0.881)
State without a sea coastline dummy				−0.062 (0.031)	−0.065 (0.033)	−0.049 (0.033)
Average yearly temperature (Celsius)					−0.033 (0.015)	−0.024 (0.015)
Squared average yearly temperature (Celsius)					0.001 (0.000)	0.001 (0.000)
Log population density in 1500						−0.034 (0.008)
Observations	1,495	684	684	684	684	642
R^2	0.947	0.936	0.938	0.938	0.938	0.938

These are OLS regressions with one observation per region. Readers are referred to Section 4 for variable definitions. All regressions include a full set of country fixed effects. Standard errors robust against heteroscedasticity are in parentheses.

magnitude toward the micro estimates and often becomes statistically insignificant. In column 1, for example, the coefficient is 0.203 (SE = 0.063), considerably smaller than the noninstrumented coefficient in column 2 of **Table 9**, which was 0.281.

In column 2, we again include the dummy variable for whether the region contains the capital city, and the coefficient on average years of schooling falls further and becomes insignificant (0.132; SE = 0.093). Adding the rest of the covariates, including the log population density before colonization in the last column, does little to change this pattern.

We conclude from this evidence that, as in the cross-country regressions, once we instrument for differences in human capital today and include the controls that are necessary to make this instrumental variables strategy valid, the magnitude of the effect of human capital on GDP per capita today falls from very high to plausible levels that are consistent with the micro evidence. One difference from the cross-country evidence is that in the models reported in this section, instrumenting for differences in human capital is sufficient to achieve this result, whereas in the cross-country models, controlling for institutions is also necessary. This probably reflects the fact that Protestant missionary activity is not a valid source of variation unless we control for institutions at the country level, but conditional on country fixed effects, it provides a more plausible source of variation in within-country variation in human capital.

Table 10 Instrumental variables (IV) regressions, cross-region sample

	(1)	(2)	(3)	(4)	(5)
IV regressions, cross region					
	Dependent variable: log GDP per capita				
Years of schooling	0.203 (0.063)	0.132 (0.093)	0.119 (0.099)	0.123 (0.100)	0.143 (0.110)
Capital city		0.383 (0.168)	0.382 (0.168)	0.379 (0.169)	0.350 (0.189)
Inverse distance to coast			−3.604 (2.389)	−2.779 (2.616)	−2.689 (2.695)
Squared inverse distance to coast			2.503 (1.648)	1.980 (1.785)	1.950 (1.841)
State without a sea coastline dummy			−0.047 (0.035)	−0.051 (0.038)	−0.037 (0.037)
Average yearly temperature (Celsius)				−0.025 (0.018)	−0.018 (0.018)
Squared average yearly temperature (Celsius)				0.001 (0.000)	0.000 (0.000)
Log population density in 1500					−0.030 (0.010)
Kleibergen & Paap (2006) test (*p* value)	0.0000	0.0000	0.0000	0.0000	0.0003
First-stage regressions					
	Dependent variable: years of schooling				
Protestant missionaries in early twentieth century	0.484 (0.087)	0.334 (0.077)	0.317 (0.074)	0.314 (0.073)	0.280 (0.075)
Capital city		1.675 (0.145)	1.570 (0.144)	1.563 (0.144)	1.613 (0.149)
Inverse distance to coast			−20.352 (3.779)	−21.649 (4.245)	−20.617 (4.272)
Squared inverse distance to coast			14.424 (2.428)	15.231 (2.694)	14.520 (2.715)
State without a sea coastline dummy			0.120 (0.105)	0.122 (0.112)	0.119 (0.120)
Average yearly temperature (Celsius)				0.046 (0.042)	0.046 (0.042)
Squared average yearly temperature (Celsius)				−0.001 (0.001)	−0.001 (0.001)
Log population density in 1500					0.028 (0.027)

(*Continued*)

Table 10 (*Continued*)

	(1)	(2)	(3)	(4)	(5)
R^2	0.901	0.920	0.927	0.927	0.929
F-statistic excluded instruments	28.49	17.62	17.12	16.98	12.86
Observations	684	684	684	684	642

The top half of the table presents second-stage regressions with years of schooling instrumented using the presence of Protestant missionaries in the region, and the bottom half presents the corresponding first-stage regressions, with one observation per region. All variables are described in the main text. Standard errors robust against heteroscedasticity are in parentheses. All regressions include a full set of country fixed effects. The p values of the Kleibergen & Paap (2006) test correspond to a test in which the null hypothesis is that the equation is underidentified, and under the null, the statistic is distributed as chi-squared with degrees of freedom = (number of overidentifying restrictions + 1).

7. CONCLUSION

In this article, we revisit the relationship among institutions, human capital accumulation, and long-run economic development. This has been a topic of intense debate over the past decade. One view, proposed by Acemoglu et al. (2001) and Acemoglu & Robinson (2012), and inspired by North & Thomas (1973), focuses on institutions as the fundamental determinant of development. According to this view, the Great Divergence in levels of prosperity that has occurred over the past 250 years is a consequence of societies having very different types of institutions. Most of the empirical literature on this topic is agnostic about the channels via which institutions impact long-run development, and it is plausible that they do via all of TFP and human and physical capital accumulation.

Another view, emanating from Lipset (1959), maintains that it is the process of modernization—comprising, inter alia, economic growth, educational expansion, and structural change—that drives institutional change.

The evidence presented in this article supports the first view.

We do find, as reported in **Table 2**, that in simple OLS regressions, both human capital and institutional variables are statistically significant (as in Glaeser et al. 2004, table 4, p. 281). Yet we do not consider these results to be very meaningful given both the bad control problem discussed in Section 1 and the serious endogeneity concerns. One of the main strategies we use in this article to overcome this problem is to build on the work of Gallego (2010), Gallego & Woodberry (2009, 2010), and Woodberry (2012) and construct two sources of plausibly exogenous variation in human capital (the presence of Protestant missionaries in the early twentieth century and primary school enrollment rates in 1900). We show that the Protestant missionary variable satisfies an important falsification exercise, suggesting that it is indeed uncorrelated with prior schooling but strongly correlated with subsequent investments in human capital, and both regressions satisfy overidentification tests.

We also build on Acemoglu et al. (2001, 2002, 2012b) instrumenting for current institutions, in this article the rule of law index, using historical sources of variation in the sample of former colonies—in particular, related to potential settler mortality and the density of the indigenous population before colonization.

The current article is of course far from the final word on this topic. Future research will need to focus on other, more credible sources of variation both between and within countries to understand how human capital contributes to economic and social development and interacts with institutions (which it certainly does). The interactions are probably much more complex and interesting, for example, as suggested by the recent work of Friedman et al. (2011), who provide evidence from one randomized trial in Kenya strongly inconsistent with key aspects of modernization theory. Their results instead suggest that greater education can be a path to more discontent, depending on the institutional and social context. There is also ample room for developing better measures of subnational institutions and exploiting the rich subnational variation in institutional development paths and development outcomes, building on and contributing to a burgeoning literature we briefly discuss above.

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