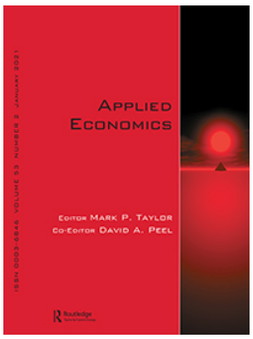

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National institutions and regional development at borders: evidence from the Americas

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ABSTRACT

This paper explores how discontinuities created by national borders can influence development across the Americas. We exploit the discontinuous nature of borders jointly with exogenous variation at the national level to identify discontinuous effects on proxies for economic development at the regional and pixel levels. We separate the effects of national institutions from local historical conditions. Our analysis yields three main findings. First, we find important discontinuities in development across national borders for the Americas. Second, we also show that they are, for the most part, caused by institutional differences at the national level and not for differences at the regional level in geography, climate, endowments, and pre-colonization conditions. Third, we also present evidence that differences in national institutions affect human capital at the regional level.

KEYWORDS

Economic development; night-time lights data; national borders; institutions

JEL CLASSIFICATION

O40; O47; O57

I. Introduction

Economists have long been interested in how country-specific features affect economic development. In many situations, country-characteristics correlate with many dimensions that make researchers unable to identify the mechanisms to explain the effects. One of the leading mechanisms suggested in the literature corresponds to the effects of institutions on development (e.g., Acemoglu, Johnson, and Robinson 2001, 2002, 2006; Acemoglu, Gallego, and Robinson 2014; Acemoglu et al. 2019). However, institutions also correlate with other potential determinants of development (e.g., Acemoglu, Gallego, and Robinson 2014; Glaeser et al. 2004; Gennaioli et al. 2012; Nunn 2009, 2014, among others).

The first motivation of this paper is to explore the effect of being located in a specific country by looking at the discontinuous changes produced in development at the national borders and then study whether differences in national institutions explain differences in development across borders. To this end, we study the case of the Americas, a region with huge between and within country differences in development (Engerman and Sokoloff 1997, 2002; Acemoglu and Dell, 2010; Dell 2010; Bruhn and Gallego 2012; Acemoglu et al. 2012; Acemoglu, García-Jimeno, and Robinson 2015; Mitton 2016; Valencia 2018) and where the colonial experience lasted longer and more deeply than in other regions (Winn 2006). By combining the national borders with detailed information on national and local conditions, we aim to identify the potential effect of institutions on development.

Exploring the effect of institutions on development can be difficult. For this reason, previous research has used historical conditions to identify exogenous variation on national institutions and provide evidence that tries to identify effects on development at the country level. Few papers have used variation across national borders to identify the effects of development with conflicting results with different and conflicting results. Accordingly to the recent survey by Michalopoulos and Papaioannou (2018), the only previous studies are Michalopoulos and Papaioannou (2013, 2014) and Pinkovskiy (2013, 2017).

Michalopoulos and Papaioannou (2013, 2014), using night-lights data, focus on border discontinuities in Africa to study the role of subnational development of deeply rooted pre-colonial ethnic institutions and national institutions.[1] While they

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[1]There are several papers using night-time luminosity to explore different historical natural experiments all over the world, but most of them focus on within country borders (Michalopoulos and Papaioannou 2018).

find a strong relationship between pre-colonial ethnic institutions and subnational development, they show that national institutions cannot explain it. In Pinkovskiy (2013, 2017) also use satellite night data to document the existence of present-day and growth discontinuities across national borders. Arguing that those discontinuities identify lower bound effects on economic activity, his results point towards the importance of national-level variables (e.g., institutions and culture) on determining development, relative to local-level variables. The contradiction between the results of both papers is probably related to the fact that Pinkovskiy (2013) does not identify border discontinuities just for Africa, but it does so for the World. We interpret this as a consequence that countries outside Africa exhibit more delimited de facto borders.[2]

We centre our analysis in the Americas. Europeans have colonized no region as long and as deeply as the American continent. The colonial legacy, that lasted over three centuries in most of Latin America and that finished few decades ago in most of the Caribbean, continues to configure the economy, politics, culture, and society in general in multiple ways (Winn 2006). Colonial experiences varied greatly both between countries and within them. However, several papers document that historical institutions and conditions at the local level affect development in the Americas (e.g., Dell 2010; Bruhn and Gallego 2012; Naritomi, Soares, and Assunção 2012; Mitton 2016; Valencia 2018). We use data capturing this variation in this paper to control for local conditions and validate our identification strategy. This condition is one of the reasons why we decided to focus our work in the Americas.

The data used in our paper reveals large differences across neighbouring regions belonging to different countries. For instance, economic development in Amazonas, Colombia, and Texas, USA, is nearly 3.5 times larger than in Amazonas, Brazil, and Tamaulipas, Mexico, respectively. This phenomenon is not exclusive to some regions, but it is generalized and significant in our sample of 17 American countries. We show that crossing from a poor to a richer country implies, on average, a 37% increase in regional per capita GDP.

The fact that contiguous regions around borders plausibly share an important number of features that potentially determine development (e.g., geography, culture, among others) suggests that there may be national characteristics that affect subnational development, at least for the contiguous regions. This paper exploits regional discontinuities across borders to try to identify the fundamental causes of development. By comparing pairs of contiguous regions, instead of just cross-section regions or countries, we are considering the effect of many observable and unobservable features that are common to those regions.

Even though we will compare neighbouring regions, there still can be space for possible omitted variable bias or measurement error that might provoke attenuation bias for variables measured at the national level. To account for this problem, we will use an instrumental variable approach, using different instruments used in the previous literature for national institutions and education (historical variables *at the country level* such as setter mortality, population mortality, and missionary activity) and will use different measures of institutions and human capital to deal with potential measurement error.[3] The use of IVs jointly represents a key difference with previous works that also only exploit border discontinuities (Michalopoulos and Papaioannou 2013, 2014; Pinkovskiy 2013, 2017), without directly testing the role of institutions.

To perform the analysis, we construct pairs of contiguous regions in different countries, that is, regions that share a national border. After evidencing the discontinuity in contemporaneous development across national borders (using both GDP per capita and night-lights at the region level), we proceed to show that these differences are not due to pre-existing differences in development and in geographical characteristics. To do this, we use pre-colonial population density (Bruhn and Gallego 2012) as a proxy for pre-colonial development, and we show that border regions in now richer

[2]This is consistent with results in Michalopoulos and Papaioannou (2014), where they show that the effect of national institutions is stronger for areas located closer to the capital city of the country.

[3]Notice that in this case potential endogeneity is different that when running regressions *at the country* level because *local* development is not likely to influence *national* institutions.

countries did not have a higher population density before colonies were established. This result is robust to the use of alternative pre-colonial development measures, such as the pre-colonial health index (Steckel and Rose 2002). We also find that there are no differences in important geographical characteristics.

We then proceed to estimate the effect of the differential of institutional quality on the difference in income in pairs of neighbour regions. We proceed using three complementary approaches. First, following Acemoglu, Gallego, and Robinson (2014), we use (capped) settler mortality as an instrument for contemporaneous institutions in an IV approach. We find a positive and economically and statistically significant effect of country institutions on local development. This result is robust to the inclusion of several controls for geographic conditions, pre-colonial population density, and pre-colonial ethnic fixed effects. Second, to understand to what extent measurement error can explain the differences between IV and OLS estimates, we implement an alternative approach in which we use an alternative measure of institutions as an IV. Our results suggest IV estimates very similar to the first approach, which confirms the relevance of measurement error to explain our results. Third, we implement a series of tests to understand whether our results are robust to adding controls for human capital both at the country and local levels. We follow the approach in Acemoglu, Gallego, and Robinson (2014) and use IVs for human capital and estimate other models for further robustness. Results produce estimates consistent with our first approach.

We also implement a series of robustness exercises. First, we exclude the observations of Canada and the US, the two richest countries in our sample, from all the regressions. The results are very similar. Second, we run regressions considering the fractal measure of Alesina, Easterly, and Matuszeski (2011) as a proxy for the exogeneity of the national border, and we find results that are consistent with our main results in this sample. Third, following several recent papers (Michalopoulos and Papaioannou 2013, 2014; Pinkovskiy 2013, 2017), we also study the main hypothesis of the paper using the information on light density at the pixel level.

Finally, we study some possible mechanisms to explain the effect of *national institutions* on contemporaneous development at *the regional level*. In particular, we study economic inequality, political representation, and human capital accumulation. In line with previous papers, we find positive and significant effects on local education and political representation. In sum, these results suggest that regions in countries with better institutions not only are more developed but also have more educated populations and political systems with better representation. We consider all these results as a contribution to the previous literature.

The rest of the document is organized as follows. In Section 2, we introduce the data used in the study and the identification strategy. Section 3 presents evidence of discontinuities on current development, and Section 4 shows the results, and Section 5 ends the document with the conclusions.

II. Data and identification strategy

Unit of analysis

We will use the region as analysis unit, and we define a region as a general name for what is called differently in each country, such as State in the USA, Brazil, and Mexico; Province in Argentina, Uruguay, and Paraguay; Region in Peru and Chile, etc. They all share the common feature of being the immediate lower subnational administration unit. As countries have different subdivisions, we opted for this instead of using the last unit available, such as counties or municipalities, since we consider regions more comparable than counties.

As we compare contiguous regions in neighbouring countries, we first identify pairs of regions as those that share a national border. More specifically, we define a border b as the limit between two contiguous regions in two different countries. For example, Figure 1 shows the frontier between Colombia and Ecuador. The region of Nariño in Colombia is contiguous to three regions in Ecuador: Esmeraldas, Carchi, and Sucumbíos. Nariño, for this reason, is going to be paired with those three Ecuadorean regions and is going to share a different border b with each one of them.[4] In this sense, each pair of regions is going to be

Figure 1. Borders.

characterized by a border *b* and each region by a border *b* and the country where it belongs.

In particular, we use, from a total of 418 regions in our countries of analysis, 141 regions that are located on the national border and have enough data.[5] Nevertheless, this is not our final sample size, since we would want to compare, each region with each one that is neighbouring with. Coming back to Figure 1, for the frontier between Ecuador and Peru, we see that Loreto its neighbour of four regions; therefore, we want to have Loreto as much as four times, each for each region in Ecuador that its neighbour with.

In that sense, this is equivalent to using Geographical Nearest Neighbour Matching to construct 'pairs' of neighbouring regions. We can see this idea in Figure 2, where we present in grey and dark colours the times each region enters in our dataset. This highlights the fact that we will be using a region that is in the border with another country as many times as neighbours it has.

For that reason, even though we have 141 regions in our dataset, our tables, and statistical analysis will be presenting results with 300 observations that correspond to our matched regions.

[4]The line between A and B is the border between Nariño and Esmeraldas; the line between B and C is the border between Nariño and Carchi and so on.
[5]We exclude regions from four countries that didn't have data on Regional GDP per capita, such as Costa Rica, French Guaiana, Guyana, and Suriname. We were not able to collect data on other 4 regions. In total, we lost 19 regions for this reason. In addition to this, we also exclude 34 regions that had a frontier of less than 20 km of length or where we do not have data on our instrument (settler mortality).

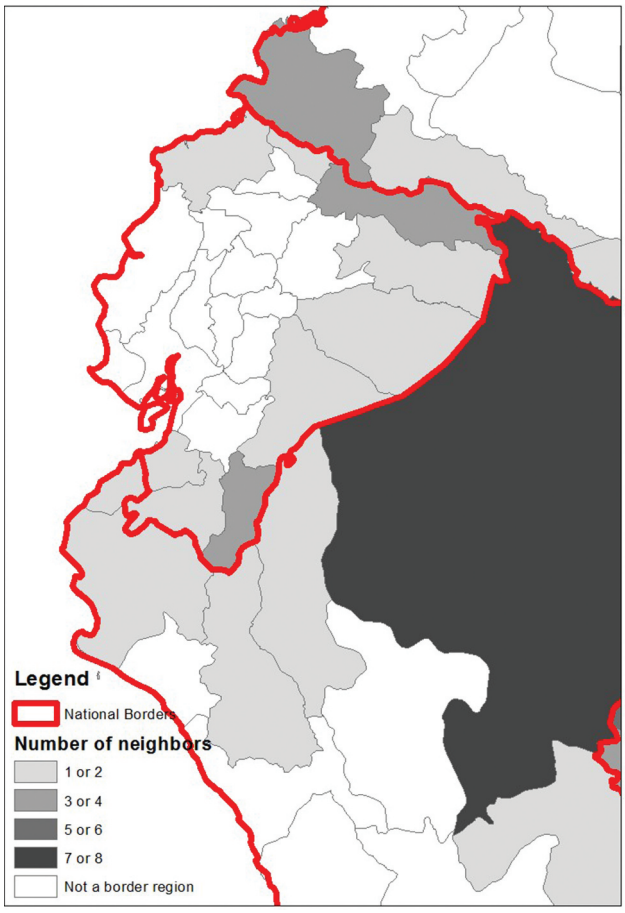

Figure 2. Neighbour matching.

Data

We will mostly use measures at the regional level of contemporaneous and pre-colonial development, geography, historical determinants of institutions, human capital, political representation, and inequality.

At the national level, we use development, current institutions, and historical determinants. We use income per capita in 2005, constructed by Genniaoli et al. (2013). Since there is a high heterogeneity of sources for regional income between countries in America, we further check our results using night-lights data. Using satellite images on light density as a proxy for economic activity was first discussed by Henderson, Storeygard, and Weil (2012) and Pinkovskyi and Sala-i-Martin (2016). Besides, they have recently been used by influential studies on development long-run Michalopoulos and Papaioannou (2013, 2014) and Henderson et al. (2018), and are now increasingly used in recent literature (See Michalopoulos and Pappaioannou, 2018, for a comprehensive survey).

To account for pre-colonial development, we use the following two measures as proxies: population density, compiled by Bruhn and Gallego (2012), and a precolonial health index that comes from the Backbone of History Project (Steckel and Rose 2002) and it is also used by Bruhn and Gallego (2012). As argued by Acemoglu, Johnson, and Robinson (2002), before colonization, only developed areas could have the infrastructure to harbour a large population, the reason for which pre-colonial population density could be a good proxy for pre-colonial development. Steckel and Rose's health index is based on archaeological data, and its usage as a development proxy relies on that more developed areas would also be more healthy areas.

We also use several geographic observables such as rainfall, temperature, altitude, a malaria index, whether a region is landlocked or not, and latitude and longitude of that same centroid.

To account for the regional historical determinants of institutions, we use the good, bad, and ugly colonial activities in the Americas indicators constructed by Bruhn and Gallego (2012). This are dummies that capture a typology of the nature of the first colonial economic activity in which European settlers engaged when they arrived in the region: if the activity displayed economies of scale, the activity, and the region is classified as 'bad'; if it lacked economies of scale the activity was classified as 'good'', unless the region had a large population, in which case the activity (and the region) was classified as 'ugly'.

Our main indicator of human capital is the average years of schooling of the population aged over 15 in 2005 and comes from Gennaioli et al. (2013). To measure political representation, we use legislative malapportionment, which denotes a discrepancy between the share of legislative seats and the share of the population held by electoral districts, from Bruhn, Gallego, and Onorato (2010). We use the Gini coefficient, estimated from national household surveys, when available, as our inequality measure.

Regarding the national level variables, we use GDP per capita in 2005 from the Penn World Tables. Our main measure of institutions is the

rule of law index for 2005 from the Worldwide Governance Indicators constructed by the World Bank (Kaufmann, Kraay, and Mastruzzi 2013). This measure has also been used recently by Acemoglu, Gallego, and Robinson (2014) and Michalopoulos and Papaioannou (2014). As an instrument for national institutions, we use the log of potential settler mortality capped at 250 (as in Acemoglu et al. 2012, and Acemoglu, Gallego, and Robinson 2014). The argument for the use of this variable as an instrument for current institutions is well known to the literature and will be presented with the IV results. In Table 1, we present descriptive statistics of all the main variables used in the study in the following sections.

Continuities and discontinuities in borders

We will first document the existence of large income discontinuities across borders, similarly as Pinkovskiy (2013, 2017), to motivate the paper and because we will use these results as a benchmark for the following analyses, which will employ an IV identification.

$$_{bc} = \propto_0 + \propto_1 Rich_{bc} + \propto_b + \varepsilon_{bc} \qquad \text{Equation 1}$$

Where y_{bc} is the GDP per capita or the luminosity density in the region in the country c in the border b. *Rich* is a dummy that takes the value 1 when the region in the border b and the country c belongs to the country with the highest GDP per capita between the two countries sharing the border b. We also include a border fixed effect, $\propto_b$, to compare only between contiguous regions.

A key advantage of comparing contiguous regions is that we can better account for omitted unobservable or difficult-to-measure variables, which are plausibly the same at both sides of the border. These variables can be, for example, geographic, ethnic, and cultural traits. However, our hypothesis, which argues in favour of the importance of national-level institutions on development, requires that the income differences between regions are the product of the limits of the countries that were set after the colonization. Therefore, to support our claim, we need not be able to trace these regional differences to precolonial income differences, when national borders - and, hence, national institutions as we know them today- did not exist.

Table 2 presents the results of Equation 1. Columns 1 to 4 present the impact of being part of a richer country on current regional GDP, and columns 5 to 8 do the same for night-time luminosity. In columns 1 and 5, we do not include any control variable. In columns 2 and 6, we include geographic controls (altitude, etc.), columns 3 and 7 include pre-colonial density as control, and finally, columns 4 and 8 include ethnicity fixed effects. All estimations include border fixed effects. The point estimate shows that regional income per capita increases by 20% and on 26% night-time luminosity on average when if we cross to a richer country.

Equation 1 can also be used to show that our results are not driven by pre-colonial differences in development in neighbouring regions. As discussed before, we use two different measures for pre-colonial development: pre-colonial population density and the health index. These results are presented in Table 3. In columns 1 and 2, we present the results of Table 2 of columns 1 and 5, to make an easier comparison.

Columns 3 to 8 present the same estimation replacing current income for geographical and pre-colonial measures of economic activity with different specifications. In geographical characteristics, we find that there are no statistically significant differences in temperature, latitude, longitude, and being landlocked.

Table 1. Descriptive statistics.

	N	Mean	SD	Min	Max
Dependent Variables					
Regional GDP per capita	300	8,93	0,86	7,12	10,92
Ln (Lights density)	300	−12,03	0,88	−14,00	−8,93
Treatment Variables					
Dummy – Rich Country	300	0,50	0,50	0,00	1,00
Rule of Law Index	300	−0,20	1,02	−1,59	1,79
Ln (Settler mortality)	300	4,06	0,54	2,71	5,10
Expropriation Risk Index	300	7,14	1,41	5,00	10,00
Geographic Controls					
Dummy if Landlocked	300	0,66	0,47	0,00	1,00
Standardized Latitude	300	0,26	0,16	0,00	0,61
Elevation of the Capital city	300	0,48	0,79	0,00	4,05
Average Temperature	300	19,20	6,72	2,38	28,98
Averange Rainfall	300	1,30	1,03	0,00	8,13
Latitude	300	−4,10	27,34	−54,33	55,17
Longitude	300	−74,09	16,06	−124,77	−50,80
Pre-colonial Density Controls					
Ln (Pre-colonial Regional Population density)	300	−0,19	1,94	−6,91	3,48
Dummy if Bad Endowments	300	0,14	0,34	0,00	1,00
Dummy if Good Endowments	300	0,59	0,49	0,00	1,00

See Section Data for more details.

Table 2. Discontinuity and continuity in contemporaneous and precolonial development.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Regional GDP per Capita				Night Lights Luminosity			
Rich country	0.303***	0.288***	0.284***	0.203**	0.249***	0.260***	0.239***	0.261***
	(0.050)	(0.053)	(0.055)	(0.087)	(0.058)	(0.052)	(0.057)	(0.091)
Observations	300	300	300	300	300	300	300	300
Border FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Precolonial population density	No	No	Yes	Yes	No	No	Yes	Yes
Colonial activities dummies	No	No	Yes	Yes	No	No	Yes	Yes
Ethnicity FE	No	No	No	Yes	No	No	No	Yes

Significance level *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors in parentheses. Each specification includes border fixed effects.

Table 3. Falsification using pre-colonial development.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	GDP	Lights	Pre-Colonial Pop. Dens.	Temperature	Landlocked	Latitude	Longitude	Pre-Colonial Health
Rich country	0.303***	0.249***	−0.126	0.254	−0.058	−0.497	0.054	0.016
	(0.050)	(0.058)	(0.223)	(0.357)	(0.048)	(0.313)	(0.290)	(0.162)
Observations	300	300	300	300	300	300	300	37
Border FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Geographic controls	No	No	Yes	No	No	No	No	No
Precolonial population density	No	No	No	Yes	Yes	Yes	Yes	No
Colonial activities dummies	No	No	Yes	Yes	Yes	Yes	Yes	No

Significance level *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors in parentheses.

As discussed before, we use different measures for pre-colonial development, such as pre-colonial population density and the health index. As can be seen, there is no impact on being part of a richer country in pre-colonial development, which is coherent with the comparability of neighbour regions that currently differ in economic development due to differences in national institutions.

Overall, the evidence above supports the claim that neighbouring regions were not significantly different in pre-colonial development, and, therefore, development before national borders where in place cannot account for current income discontinuities.

III. Main results

Border effects

In this section, we study the relationship between national institutions and economic development. We start by estimating an OLS regression that controls for several determinants of income using the following specification.

$$y_{bc} = \beta_0 + \beta_1 I_{bc} + X_b^{'}\gamma + \alpha_b + \varepsilon_{bc} \qquad \text{Equation 2}$$

Where y_{bc} represents the development of the region in border b belonging to country c. As discussed above, we use two measures for regional development: regional GDP per capita and night light density.[6] I_{bc} denote our national institution's measure, the rule of law index, α_b are border fixed effects and $X_b^{'}$ denote a set of controls. This specification enables us to compare the effect of national institutions within each pair of regions, thus comparing two areas that are plausibly very similar, as previously discussed.

One advantage of using regions as units is that we can control for several variables at the local level. So, even if we think that comparing between neighbouring regions already accounts for many things, it is always helpful to be able to include some of those (and other) determinants at a local level as a robustness check and to test our identification assumption further. Because of this, and given the extensive debate on the extent to which geography determines growth, in many specifications we control for a set of geographic variables that include rainfall, average temperature, altitude, a malaria index (Gallup and Sachs 2001; Sachs and Malaney 2002), distance to the equator (Bloom and Sachs 1998) and being landlocked (MacKellar, Woergoetter, and Woerz 2000).

[6]When using satellite data, we follow Michalopoulos and Papaioannou (2014) and use the logarithm of average lights plus a small number (0.01), since some observations have a value of zero for lights. Thus, following Pinkovskiy (2013, 2017), our variable is log[(0.01+ lights)*area/population].

The coefficients of these variables reflect the importance of weather and temperature over development, as highlighted Gallup, Sachs, and Mellinger (1999), Masters and McMillan (2001), and Sachs (2001) among others. Besides, we also include some location controls (Gallup, Sachs, and Mellinger 1999) that correspond to latitude and longitude of the centroid of the region and the distance of this centroid to the national border. The distance to the national border captures potentially lower levels of development of border areas (Michalopoulos and Papaioannou 2013). As noted in Acemoglu and Dell (2010), local institutions, as opposed to national institutions, can and possibly will account for much of the regional variation in income. For this reason, in other specifications, we include the historical determinants of local institutions as measured by the good, bad, and ugly dummies and the precolonial population density (Bruhn and Gallego 2012).

Table 4 presents the results for the OLS estimation. In Panel A, the dependent variable is the regional GDP per capita, whereas, in Panel B, the dependent variable reflects the level of economic activity proxied by night-lights data. Column 1 reports the unconditional estimation, and then we gradually include location and geographic controls until column 4 in which we also include local historical determinants. In Panel A, the first three estimates are significant to 99%, and the last column is not statistically significant. In Panel B, none of the estimates is statistically significantly different from zero.

However, there are at least three main reasons for which the previous results are not causal. First, there may be omitted variables in equation 2, which vary across the national (and regional) border, and that, hence, are not accounted for when comparing neighbour regions. We should expect that many other political and economic variables determined by the identity of the local government (e.g., public good provision, education, etc.) also change at the border. Second, there might be a measurement error in the rule of law index, as discussed in Acemoglu, Johnson, and Robinson (2001), (2014)).

Not controlling for them will likely bias the coefficients because we would be attributing to institutions part of the effect of these other determinants. Lastly, in a simple OLS estimation, it is very difficult to separate the effect of national from local institutions over regional GDP. For this reason, we need a source of exogenous variation of institutions at the national level.

Table 4. OLS estimation.

	(1)	(2)	(3)	(4)
Panel A: Dependent Variable is GDP per capita				
Rule of Law	0.230***	0.165***	0.179***	0.211
	(0.056)	(0.051)	(0.051)	(0.137)
Observations	300	300	300	300
Panel B: Dependent Variable is lights density				
Rule of Law	0.054	−0.005	0.003	−0.074
	(0.061)	(0.062)	(0.062)	(0.110)
Observations	300	300	300	300
Border FE	Yes	Yes	Yes	Yes
Geographic controls	No	Yes	Yes	Yes
Precolonial population density	No	No	Yes	Yes
Colonial activities dummies	No	No	Yes	Yes
Ethnicity FE	No	No	No	Yes

Significance level *** $p < 0.01$, ** $p < 0.1$. Robust standard errors in parentheses. Each specification includes border fixed effects.

Instrumental variables for national institutions

To account for all these problems, we use an instrumental variable approach instrumenting our measure of current institutions with settler mortality (Acemoglu, Johnson, and Robinson 2001; Acemoglu, Gallego, and Robinson 2014). Using equation 2 as the second stage, we describe the first stage with the following equation.

$$I_{bc} = \gamma_0 + \gamma_1 Log\_Settler\_Mortality_{bc} + X_b^{'}\gamma + \delta_b + u_{bc}$$

Equation 3

The argument behind the use of settler mortality as an instrument for institutions is well known to the literature and holds that different institutions arose in different European colonies depending on the type of colonization they experienced. On the one hand, in some colonies, Europeans established extractive institutions, which were designed to extract resources and not to protect property rights.

On the other hand, in other colonies, Europeans created more inclusive institutions, which tried to mimic the ones that already existed in Europe and that put emphasis on property rights protection, and that constrained the power of the executive (that is, protected citizens from expropriation). Whether Europeans chose to impose one kind of

institution or the other depended on the disease environment of the place they were settling in: in more European-friendly environments where the settlers' probability of dying was lower, inclusive institutions were established; the opposite happened were disease and death was more abundant. In this way, we follow Acemoglu, Johnson, and Robinson (2001) and suppose that institutions are persistent over time to use settlers' mortality rate as an exogenous variation of the quality of present institutions to estimate the impact of institutions over economic development.

Table 5 shows the results for the first and second stages of the estimation for the different specifications, in the same fashion as the OLS results. Before turning to the results of interest, Panel C shows a strong and positive relationship between the rule of law index and log settler mortality, which, under the historical argument, suggests that better institutions developed in areas where settlers had a better chance of surviving. Statistically, we can confidently dismiss any potential weak instrument problems given that the Cragg Donald statistic for the unconditional estimation is 41.62, very superior to the critical value of 16.38 suggested in the Stock and Yogo (2005) tables, while the statistic is 17.77 for the last estimation, again, larger than the critical value.

All coefficients in Panels A and B are larger than their OLS counterparts, which suggests that the attenuation bias was bigger than any upward bias caused by omitted variables. Possibly more important, the coefficient remains stable to the inclusion of more and more controls. Overall, the IV coefficients appear to be robust to different specifications, which allow us to conclude an important part of the income differences we observe when crossing from one country to another can be accounted for differences in national institutions.

Table 5. Instrumental variables.

	(1)	(2)	(3)	(4)
Panel A: Dependent Variable is GDP per capita				
Rule of Law	0.383***	0.417***	0.481***	0.258**
	(0.017)	(0.047)	(0.056)	(0.106)
Observations	300	300	300	300
Panel B: Dependent Variable is lights density				
Rule of Law	0.398***	0.286***	0.298***	0.669***
	(0.044)	(0.052)	(0.063)	(0.184)
Observations	300	300	300	300
Panel C: First Stage				
Log Settler Mortality	−1.377***	−1.401***	−1.542***	−1.953***
	(0.055)	(0.134)	(0.155)	(0.516)
Observations	300	300	300	300
Border FE	Yes	Yes	Yes	Yes
Geographic controls	No	Yes	Yes	Yes
Precolonial population density	No	No	Yes	Yes
Colonial activities dummies	No	No	Yes	Yes
Ethnicity FE	No	No	No	Yes

Significance level *** $p < 0.01$, ** $p < 0.1$. Robust standard errors in parentheses. Each specification includes border fixed effects.

Table 5 shows the results for the first and second stages of the estimation for the different specifications, in the same fashion as the OLS results. Before turning to the results of interest, Panel C shows a strong and positive relationship between the rule of law index and log settler mortality, which, under the historical argument, suggests that better institutions developed in areas where settlers had a better chance of surviving. Statistically, we can confidently dismiss any potential weak instrument problems given that the Cragg-Donald statistic for the unconditional estimation is 41.62, very superior to the critical value of 16.38 suggested in the Stock and Yogo (2005) tables, while the statistic is 17.77 for the last estimation, again, larger than the critical value.

All coefficients in Panels A and B are larger than their OLS counterparts, which suggests that the attenuation bias was bigger than any upward bias caused by omitted variables. Possibly more important, the coefficient remains stable to the inclusion of more and more controls. Overall, the IV coefficients appear to be robust to different specifications, which allow us to conclude an important part of the income differences we observe when crossing from one country to another can be accounted for differences in national institutions.

Measurement error in institutions

To understand to what extent measurement error can explain the differences between our IV and OLS estimates, we implement a complementary approach in which we use an alternative measure of institutions as an IV. If measurement error is classical, we can use a proxy as an instrument for another proxy and obtain causal estimates not affected by attenuation bias. The idea of this is to test if the new IV point estimate using this alternative instrument is similar to the one obtained with our original instrument. If this is the case, it

would be evidence that the main correction that the IV is doing is to the attenuation bias caused by the measurement error in our institution's quality variable (Rule of Law).

$$I_{bc} = \delta_0 + \delta_1 Risk\_of\_Expropriation_{bc} + X_b^{'}\gamma + \delta_b + u_{bc}$$

Equation 4

In addition to this, we also present a modified version of the second stage where we include our previous instrument, the log. of settler mortality as a control variable. In the spirit of an over-identification test, if the effect of institutions on development is estimated without bias, then adding an instrumental variable in the estimating equation should produce a statistically insignificant estimate for this variable and a coefficient on institutions similar to the one estimated using our main instrumental variable.[7]

$$y_{bc} = \alpha_0 + \alpha_1 \hat{I}_{bc} + Log\_Settler\_Mortality_{bc} + X_b^{'}\gamma + \delta_b + u_{bc}$$

Equation 5

We can see the results of this analysis in Table 6. Results are presented in the same order as previous ones, comparing the results of Equations 3 and 5. Our alternative IV results suggest that our original IV estimates were, in fact, correcting for measurement error, because we observe that corrected estimate using an alternative instrument that should correct only for this type of error, and not for omitted bias or reverse causality problems, is in the same order of magnitude than the presented in Table 5. Reassuringly, all the estimates on settler mortality (our main instrumental variable) are not different from 0.

Robustness to other explanations: human capital

Finally, we will test whether our results are robust to adding controls for human capital at both the country and local levels. As seen in the recent debate in the literature (Acemoglu, Johnson, and Robinson 2001; Glaeser et al. 2004; Acemoglu et al. 2009; Gennaioli et al. 2012; Acemoglu, Gallego, and Robinson 2014), there are other potential explanations for long-run development, such as human capital accumulation.

We follow the approach in Acemoglu, Gallego, and Robinson (2014) and use IVs for human capital and estimate other models for further robustness. We would use the following instruments: 'Primary Enrollment in 1900' and 'Presence of Catholic Priests' (see Acemoglu, Gallego, and Robinson 2014, for more details on the instruments).

The idea is to explore if our estimate is robust to the inclusion of different proxies for human capital. First, we use a measure of human capital in the present at the national level (from Acemoglu, Gallego, and Robinson 2014). Then we also study the robustness of the results to the inclusion of

Table 6. Measurement error: Alternative IV.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent Variable is GDP per capita								
Rule of Law	0.412***	0.425***	0.412***	0.409***	0.414***	0.368***	0.399***	0.493***
	(0.043)	(0.062)	(0.051)	(0.079)	(0.054)	(0.079)	(0.129)	(0.185)
Log Settler Mortality		0.058		−0.010		−0.174		0.460
		(0.089)		(0.126)		(0.149)		(0.367)
Observations	300	300	300	300	300	300	300	300
Panel B: Dependent Variable is lights density								
Rule of Law	0.326***	0.294***	0.365***	0.420***	0.360***	0.402***	0.657***	0.649**
	(0.055)	(0.076)	(0.070)	(0.113)	(0.069)	(0.108)	(0.192)	(0.276)
Log Settler Mortality		−0.143		0.189		0.161		−0.040
		(0.123)		(0.172)		(0.192)		(0.603)
Observations	300	300	300	300	300	300	300	300
Border FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Precolonial population density	No	No	No	No	Yes	Yes	Yes	Yes
Colonial activities dummies	No	No	No	No	Yes	Yes	Yes	Yes
Ethnicity FE	No	No	No	No	No	No	Yes	Yes

Significance level *** $p < 0.01$, ** $p < 0.1$. Robust standard errors in parentheses. Each specification includes border fixed effects.

[7] See Table 8 in Acemoglu, Johnson, and Robinson (2002) for a similar exercise.

Table 7. Institutions, Human Capital and Border Effects: IV Estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
	Reference	OLS	Bad Control	IV M.E.	Semi-Structural	Structural
Rule of Law	0.408***	0.206***	0.426***	0.392***	0.365***	0.369***
	(0.039)	(0.044)	(0.052)	(0.053)	(0.052)	(0.054)
Catholic Priests					−0.078***	
					(0.012)	
Primary enrolment in 1900					−0.001	
					(0.002)	
Years of schooling in 2005		0.034*	−0.025	0.045**		0.022
		(0.017)	(0.017)	(0.023)		(0.021)
Observations	300	300	300	300	300	300

Significance level *** p < 0.01, ** p < 0.1. Robust standard errors in parentheses. Each specification includes border fixed effects.

Table 8. Sample selection robustness checks.

	Regional GDP per capita			Night Lights Luminosity		
	(1)	(2)	(3)	(4)	(5)	(6)
Rule of Law	0.414***	0.236***	0.398***	0.360***	0.306***	0.329***
	(0.054)	(0.067)	(0.050)	(0.069)	(0.096)	(0.064)
Observations	300	248	284	300	248	284
Border FE	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Precolonial population density	Yes	Yes	Yes	Yes	Yes	Yes
No CAN-USA	Yes	Yes	Yes	Yes	Yes	Yes
Exogenous borders	No	Yes	No	No	Yes	No

Significance level *** p < 0.01, ** p < 0.1. Robust standard errors in parentheses. Each specification includes border fixed effects. In columns (3) and (6), we use the fractal measure of borders from Alesina, Easterly, and Matuszeski (2011) to define 'exogenous' borders-those that are closer to be straight lines.

historical determinants of education. The results presented in Table 7 are consistent with our main results.

In column 1, we present a benchmark estimate based on what we presented in Table 6. In column 2, we perform a similar analysis to what we presented in the first column of Table 2, but including education. As could be expected, these estimates are not causal, but we present them to benchmark the analysis of previous tables. As expected, the point estimate presented in column 1 of Table 2 is reduced since both are capturing part of the same variation.

In column 3, we repeat the analysis of Table 6, but including Education without instrumenting for it (thus using it as a 'bad control'), as expected this reduces the point estimated presented in Table 5 significantly since this instrument affects current development also through present education.

In column 4, we instrument education with another proxy of education at the national level[8] to test for measurement error bias in education too. Notably, when we do this, the estimated coefficient on institutions is, again, very similar to our previous estimates. Nevertheless, since there is still the risk of endogeneity in our education and institutions measure, we would now use the same strategy used in Acemoglu, Gallego, and Robinson (2014), which is using as instruments for education: i) Primary education enrolment in 1900, and ii) the number of catholic priests at the beginning of XX century.

In column 5, we present our reference estimate but including as controls the education instruments (what Acemoglu, Gallego, and Robinson 2014 calls the semi-structural approach). Finally, in column 6, we use this instrument for education and, therefore, present both institutions and education instrumented (what Acemoglu, Gallego, and Robinson 2014 calls the structural approach). In both cases, we find that our institution's estimate is robust and the education's estimate, in the last column is not statistically significant.

Robustness

In Table 8, we present robustness exercises related to the selection of the sample in two different ways. The first is to address the tentative hypothesis that one might suspect that the US border with Canada and with Mexico is explaining an important part of

[8]We use the national average of regional education of Gennaioli et al. (2012).

the variation found in this paper. We, therefore, exclude from the sample. We present the results in column 2 and column 5, for the regional GDP and night lights luminosity, respectively.

In addition to this, although most of America's national borders follow geographic accidents (such as rivers or mountains), some of them can be endogenous. For example, many borders were set in postwar pacts (e.g., Chile-Bolivia and Chile-Peru) and others to prevent wars (e.g., Uruguay-Argentina). As Spolaore (2012) argues, 'borders of national states are not taken as given (exogenous) but are the endogenous outcomes of decisions by agents, who interact with each other while pursuing their goals under constraints.'

Despite the above, as with the frontiers of African nations, America's national borders appear to be a good example of limits artificially drawn by the settlers. 'When independence arrived in the early 19th century, the new states were controlled by the European elites, who formed states based on these colonial demarcations [...] Although there were some wars that altered a few borders, today's Latin American states still correspond closely to Spanish colonial divisions' (Alesina, Easterly, and Matuszeski 2011). Even more, a great part of the colonial administrative units (that, later on, became countries) had little relationship with the geographic demarcations of the pre-colonial ethnic groups.[9]

To takes this into account, we present in columns 3 and 6 the results for the sub-sample with the most-plausible exogenous borders, for this reason, we construct a 'fractal measure of borders' (Alesina, Easterly, and Matuszeski 2011) which defines exogeneity if the border is closest to a straight line. We then use only the sub-sample for which the border is more similar to a straight line (above the median). As could be seen in Table 8, both exercises give us statistically significant results for both outcomes.

Estimation at the pixel level

While using the national borders as our identification strategy, the first idea that comes to mind is to use a geographical regression discontinuity, similar to the one performed by Pinkovskiy (2013, 2017) and Michalopoulos and Papaioannou (2013, 2014). Nevertheless, this is plausible to use only for night lights luminosity, not for GDP per capita, because in the case of night lights the unit of aggregation can be reduced more than the regional level.

To test for the robustness of our estimate, we implement a Geographical Regression Discontinuity Design using pixels as our observation measure, national borders as the discontinuity, and the distance to the border as the running variable. We present the specification in Equation 6.

$$y_{pbc} = \propto_0 + \propto_1 Rich_{bc} + f\left(distance_{pbc}\right) + \propto_b + \varepsilon_{pbc} \quad \text{Equation 6}$$

Here our outcome, y_{pbc} represents the luminosity in pixel p, which is near to the border b, in the country c.[10] Again, *Rich* is a dummy variable that takes value one if the pixel is in the rich country in the border b. $\propto_b$ are the border fixed effects and, ε_{pbc} is the error term. The term $f\left(distance_{pbc}\right)$ represents a non-parametric polynomial of the distance of the pixel in country c to the national border b.

As could be observed, the main difference with respect to specification in Equation 2, is that we are using pixel-level data and that we include a non-parametric measure of the distance of pixel p, at country c, to the border b. In Figure 3, we present the idea behind this exercise using the border between Nicaragua and Costa Rica.

The results of this estimate are presented in Table 9, and there we find that the effect of being in a rich country is positive and statistically significant. We present two estimates; the first column presents the estimate selecting the optimal bandwidth using the method of Imbens and Kalyanaraman (2012), and the second column presents the result using the method of Calonico, Cattaneo, and Titiunik (2014). Both estimates are smaller in size than the one estimated in Table 2, but this probably is related that while we compare neighbouring regions, here we are only comparing the area closest to the

[9]For example, the Mayas, located in southern Mexico, Guatemala and Central America, were divided between the Nueva España Viceroyalty and the Captaincy General of Guatemala. The Captaincy General of Chile, the Viceroyalty of the Río de la Plata and the Real Audiencia of Charcas divided the Incas between Argentina, Bolivia, Peru and Chile.

[10]We follow Pinkovskiy (2013) and define y_{pbc} as follows: $y = a + b\ log(1 + lit) + c\ log\ (area) + d\ log\ (population)$, with the following values $a = 13{:}85;\ b = 0{:}64;\ c = 0{:}65;\ d = -0{:}44$.

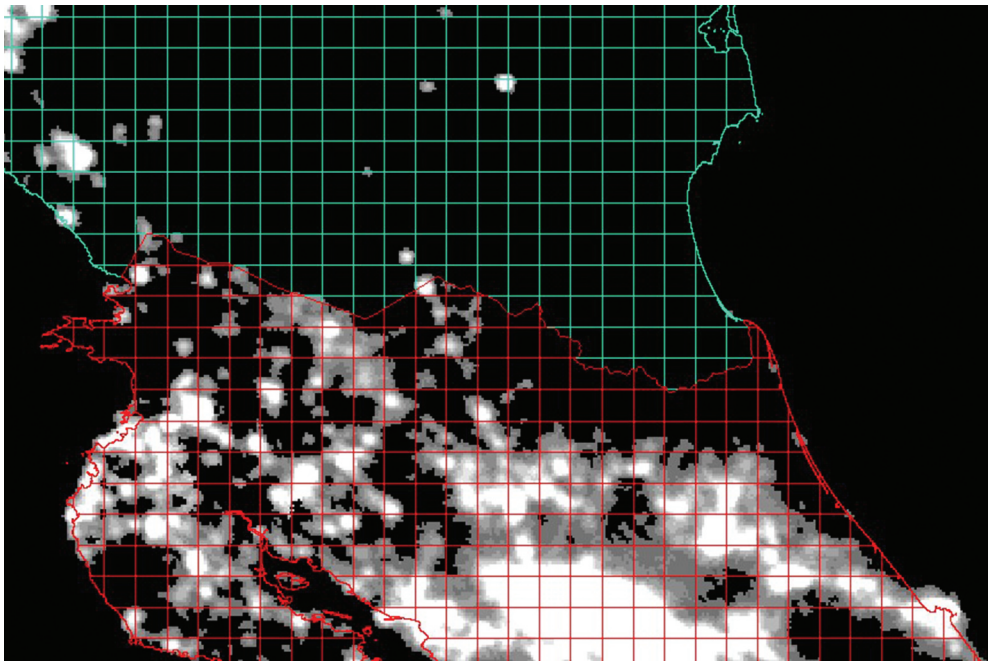

Figure 3. Pixel data of night lights luminosity in the border between Costa Rica (south) with Nicaragua (north).

Table 9. Semi-parametric RD estimates using pixel-level data: rich country dummy.

	IK	CCT
RD Estimate	0.065***	0.045**
	(0.016)	(0.023)
Observations	36,189	18,043
Bandwidth	0.68	0.33

Significance level *** p < 0.01, ** p < 0.1.

Table 10. Semi-parametric RD estimates using pixel-level data: High institutions dummy.

	IK	CCT
RD Estimate	0.029***	0.024**
	(0.006)	(0.008)
Bandwidth	0.98	0.43

Significance level *** p < 0.01, ** p < 0.1.

border, therefore, since most borders do not concentrate most of the cities or firms, this would be a lower bound of the true estimate.

We use pixel data to also study whether moving from a pixel with better institutions improves our proxy for development, see equation 7 below. We estimate a model similar to equation 6 but in this case the treatment variable is *Better_Institutions*, a dummy taking a value of 1 if the institutions in pixels located in country c are better than in the neighbouring country.

$$y_{pbc} = \propto_0 + \propto_1 Better\_Institutions_{bc} + f\left(distance_{pbc}\right) + \propto_b + \varepsilon_{pbc} \quad \text{Equation 7}$$

Results are presented in Table 10. Results show that moving to a pixel located in a country with better institutions is associated with an increase in our proxy for development. The effects again are smaller than when running regressions at the regional level but they are still significant.[11]

Some evidence on mechanisms

Finally, we discuss possible mechanisms of transmission and persistence of the effect of institutions on variables at the local level. We consider three different channels of persistence previously proposed by the literature: political representation, income inequality, and human capital. The results of these are presented in Table 11.

Political representation

Following Bruhn, Gallego, and Onorato (2010), we use political malapportionment, which denotes

[11]Running a regression of light density at the regional level on a dummy of having better institutions and all the relevant fixed effects produces a coefficient of 0.125 (with a standard error of 0.060). So the standardized effect at the regional level is 0.14 while the standardized effect at the pixel level is between 0.040 and 0.042.

Table 11. Mechanisms.

	Log. Political Representation		Log. GINI		Average Years of Education	
	(1)	(2)	(3)	(4)	(5)	(6)
Rule of Law	−0.817***	−1.334***	−0.006	0.054***	1.710***	1.529***
	(0.095)	(0.165)	(0.000)	(0.013)	(0.073)	(0.172)
Observations	277	277	250	250	295	295
R-squared	0.433	0.503	0.600	0.815	0.762	0.829
Border FE	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	No	Yes	No	Yes	No	Yes
Precolonial population density	No	Yes	No	Yes	No	Yes
Colonial activities dummies	No	Yes	No	Yes	No	Yes

Significance level *** $p < 0.01$, ** $p < 0.1$. Robust standard errors in parentheses. Each specification includes border fixed effects.

a discrepancy between the share of legislative seats and the share of the population held by electoral districts as our measure of political representation. The argument is that an uneven distribution of legislative seats can be used to over-represent electoral districts dominated by the political elite and, in this way, help them maintain de facto political power. It can be argued that countries with better institutions (i.e., countries whose institutions favour less the elites) will have less over-representation, precisely because elites will not be able to develop this mechanism to maintain de facto power. Columns 1 and 2 show a negative and significant coefficient, consistent with the previous argument.

Income inequality

Using the log Gini index as the dependent variable, columns 3 and 4 show that better institutions *at the national level* do not appear to have a significant effect on income inequality *at the local level*. This is consistent with the results of Acemoglu et al. (2008). In their paper, they use municipal evidence for Cundinamarca, Colombia, to question the trade-off between economic inequality and economic development, arguing that the relevant inequality for development is political, not economical. On the same line, Bruhn and Gallego (2012) show that the colonial activity type does not seem to have effects on development through inequality.

Human capital

We study now whether institutions *at the national level* affect human capital *at the local level*. This is a hypothesis consistent with the results in Acemoglu and Dell (2010) and Dell (2010), where institutions affect incentives for human capital accumulation. The positive and significant estimates of columns 5 and 6 show that consistent with the previous story, better institutions induce more human capital, as reflected by the average years of schooling.

IV. Conclusions

The question on the fundamental causes of development is present in several lines of research in economics and, more generally, the social sciences. Our paper exploits the borders in the Americas and several identification strategies to study the effect of national institutions on development at the regional level. In particular, we use both instrumental variable estimates and a geographic regression discontinuity design. Our results imply that institutions at the national level have an economic and statistically significant effect on development at the local level. Furthermore, we show that these differences are not driven by differences in development before colonization at the local level. We also show that our results are robust to differences in human capital at the national level. Finally, we present suggestive evidence that implies that the effect of institutions at the national level produces differences in human capital and political representation at the local level. Taken together, the findings in this paper complement the literature on the fundamental causes of development with a new source of exogenous variation in the context of comparison of development across borders in the Americas.

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