National Institutions and Regional Development at Borders: Evidence from the Americas*

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Abstract

In this paper we present evidence that suggests there are important discontinuities in development across the national borders of the Americas. We also show that these discontinuities 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. We also present evidence that differences in national institutions affect human capital at the regional level.

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1 Introduction

No region has been colonized by Europeans as long and as deeply as the American continent. The colonial legacy, that lasted over three centuries in most of Latin America and that just finished in most of the Caribbean, continues to configure the economy, politics, culture and society in general in multiple ways (Winn 2006). Nevertheless, colonial experiences varied greatly both between countries and within them.

Very diverse development experiences coexist at the same time in America; for example, in 2005 USA and Canada alone accounted for 39% of the total GDP per capita of America, and the GDP per capita of USA was 15 times the GDP per capita of Honduras, the poorest country in the sample.[1] Differences as these stay large even when we compare between neighbor countries: the income per capita of Chile and USA is 4 times the one of their respective neighbors, Bolivia and Mexico, and GDP per capita of Argentina is 3 times the one of Paraguay. Even more, a closer look at the regional level reveals even larger differences. For example, Antofagasta region in Chile is 6 times larger than Potosí in Bolivia, the same magnitude in which Orellana in Ecuador outperforms Loreto, Peru. Likewise, economic development in Amazonas, Colombia, and Texas, USA, is nearly 3.5 times the one in Amazonas, Brazil, and Tamaulipas, Mexico, respectively.

This phenomenon is not exclusive to some regions, but it is generalized and significant to the average of border regions in our sample of 17 American countries. We show that crossing from a poor to a richer country implies, in average, a 37% increase in regional per capita GDP. Using night lights data, Pinkovskiy 2013 documented such a relation between border regions for the whole world, showing that luminosity increases 0.5 log points on average when one crosses to a richer country.

The previous facts open the question on what is behind such differences between contiguous regions, where many factors usually linked with growth, geography among them, are very similar -or, at least, not as different. Throughout this paper we will explore and defend the institutional hypothesis arguing and showing that a very important part of the regional variation in growth is due to differences in the quality of national institutions, which, at the same time, can be traced back to different colonial experiences (Acemoglu, Johnson & Robinson, 2001, 2002, 2006; Acemoglu, Gallego & Robinson, 2014). More specifically, the fact that contiguous regions plausibly share an important amount of features that potentially determine development (e.g. geography, culture, among others) suggest that there may be national characteristics that affect regional development. With this in mind, this paper exploits regional discontinuities across borders to try to identify the fundamental causes of development. By comparing two contiguous regions, instead of just regions or countries, we are taking into account the effect of many observable and unobservable features that are common to those regions. As said before, we will focus on exploring the effect of national level institutions on regional development arguing that significant amount of the regional discontinuity across national borders can be explained by them.

[1] With less than half the GDP per capita of Honduras, the poorest country in America is Haiti, but it is not in our sample.

Two potential problem arise. First, since regions in the borders are more porous than average regions, the estimates will be a lower bound. Second, even though we will compare neighbor regions, there still can be (un)observable factors that differ between them and that affect development, and some variables can be measured with error. To account for this problem we will use an instrumental variable approach, instrumenting national institutions with settler mortality. This represents a key difference with previous works that also explode border discontinuities (Michalopoulos & Pappaioannou, 2013, 2014; Pinkovskiy, 2013).

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, we proceed to show that these differences are not due to pre-existing differences in development. As the institutional argument rests in the importance of the colonial institutional heritage on modern institutions and, through them, on contemporaneous development, it is important to show that modern differences cannot be traced back to differences in pre-colonial factors. To do this, we use pre-colonial population density as a proxy for pre-colonial development and we show that border regions in now richer countries did not have 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. Also, our results obtain in every subsample when we divide the sample according the exogeneity of the national border (using Alesina et al. exogeneity index).

We then proceed to estimate the effect of the differential of institutional quality on the difference in income in pairs of neighbor regions. Using an instrumental variable approach we show that an important part of the difference in income can be accounted by differences in institutional quality, where regions in countries with better institutions tend to have higher economic performance. There are three main reasons to use instrumental variables. First, the quality of national institutions is not the only thing that varies discontinuously in a country's frontier; there can be other variables that change with the border (e.g., culture) that could also explain regional differences and that correlate with national institutions. Second, institutional quality is possibly subject of measurement error when using the rule of law index as its proxy, which can produce attenuation bias. Lastly, the exogenous variation at the national level can be use to separate from the effect of local institutions. Following Acemoglu et al. (2001) we use settler mortality as instrument for contemporaneous institutions.

Finally, we discuss some possible sources of persistence of the effect of history -or historical institutions- on contemporaneous development. In particular, we try to see if some persistence mechanisms proposed by the literature -those are, economic inequality, political representation, and human capital accumulation- are consistent with the data in our framework. In line with the literature, we show that economic inequality is unrelated with institutional quality, while this relation appears to be significant for political representation and human capital. On one hand, the evidence supports the claim that institutional quality has a causal effect on the over or sub political representation, and be viewed as a mechanism through which the elites transform de jure into de facto political power.

On the other hand, we find important effects of institutional quality on human capital differences and we show that, when controlling for national institutions and historical determinants of local institutions, human capital cannot explain development differences in neighbor regions. Even though this coefficient is biased, we argue that it is the upper bond of the true parameter, which suggest that the effects of institutions on human capital that we found represent the role of human capital as a transmission of institutions over development.

Among the related literature, other authors have already used border discontinuities to asses the role of national-level variables on development. Michalpoulos & Pappaioannou (2013, 2014) using night lights data focus in border discontinuities in Africa to study the role on subnational development of deeply rooted pre-colonial ethnic institutions and national institutions. While they find a string relationship between pre-colonial ethnic institutions and subnational development, they show that national institutions cannot explain it. The main difference between their framework and ours are (i) that countries in America, as opposed to Africa, exhibit more delimited de facto borders and (ii) that we account for possible omitted variable bias and/or measurement error attenuation bias using an IV approach. Pinkovskiy (2013) also use satellite night data to document the existence of growth discontinuities across national borders. Arguing that those discontinuities form lower bound for discontinuities in 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 rest of the paper has the following structure. Section 2 reviews the data. Section 3 Section 4. Section sec-regdisc

2 Data

As we compare contiguous regions in neighboring 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 Esmeraldas 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 Sucumbios. 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.[2] In this sense, each pair of regions is going to be characterized by a border b and each region by a border b and the country where it belongs.

For the analysis we use measures at the regional level of contemporaneous and precolonial development, geography, historical determinants of institutions, human capital, political representation and inequality. As countries have different subdivisions, this measures correspond most of the times to the highest or second highest possible subdivision within each country (e.g., states in USA and Brazil, provincias in Argentina and departamentos in Nicaragua, instead of, say, counties or muni-

[2] 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.

Figure 1. Borders

cipalities). At the national level we use development, current institutions and its historical determinants. We use income per capita in 2005 constructed by Genniaoli et al. (2013) as our main regional development measure. Since there is 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 as Henderson, Storeygard, and Weil (2012) and have recently been used by Michalopoulos & Pappaioannou (2013, 2014).

To account for precolonial 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 et al. (2002), before colonization only developed areas could have the infrastructure to harbor a large population, reason for which precolonial population density could be a good proxy for precolonial development. Steckel and Rose's health index is based on archaeological data and its usage as a development proxy relies in 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 dummies constructed by Bruhn and Gallego (2012). This dummies are based on the first colonial economic activity in which European settlers engaged when they arrived to 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. The argument is that activities that permitted great extraction of resources, because they had economies of scale, or that could be engaged in highly populated areas, where often native population was used as forced labor, fostered worst institutions that areas in which economic gains cam from small-scale, independent production.[3]

Our main indicator of human capital is average years of schooling of the population aged over 15 in 2005 and comes from Genniaoli 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 population held by electoral districts, from Bruhn et al. (2010). We use the Gini coefficient as our inequality measure.

Regarding the national level variables, we use GDP per capita (pp) 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, Kray and Mastruzzi, 2013). This measure has also been used recently by Acemoglu, Gallego and Robinson (2015) and Michalopoulos & Pappaioannou (2014). As an instrument for national institutions we use log of potential settler mortality capped at 250 (as in Acemoglu, Johnson and Robinson, 2012, and Acemoglu, Gallego and Robinson, 2014). The argument for the use of this variable as an instrument for current institutions is

[3]For a more extensive discussion see Engerman and Sokoloff (1997, 2002) and Bruhn and Gallego (2012).

well known to the literature and will be presented with the IV results.

3 Discontinuities and continuities

In this section we first document the existence of large income discontinuities mentioned to motivate this paper. To show the latter, we estimate how much the regional GDP per capita changes when crossing from one country to a richer one by estimating the following equation:

$$y_{b,c} = \alpha_0 + \alpha_1 \text{Rich}_{b,c} + \alpha_b + \varepsilon_{b,c} \tag{1}$$

where $y_{b,c}$ is the GDP per capita in the region in country c in the border b. *Rich* is a dummy that takes the value 1 when the region in the border b and in country c belongs to the country with highest GDP per capita between the two countries sharing the border b. We also include a border fixed effect, α_b, to compare only between contiguous regions. The point estimate (column 1, table 1) shows that regional income per capita increases in 37% on average when one crosses to a richer country.

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

The equation 1 can also be used to show that our results are not driven by precolonial differences in development in neighbouring regions. Table 1 shows the results of replacing current income for prehistorical measures of economic activity. As discussed before, we use two different measures for precolonial development: precolonial population density and the health index. The results are displayed in columns 2 and 6, respectively. Columns 5 and 7 do the same, but using a dummy equal to one when the region (instead of the country) is the one with highest income of the pair. All estimates are insignificant and, with exception of column 1, very close to 0.[4] Nevertheless, 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 where set in post-war pacts (e.g., Chile-Bolivia and Chile-Peru) and others to prevent wars (e.g., Uruguay-Argentina). As Spolaore (2012, p.1) 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

[4] If something, column 1 shows that there was a reversion of income.

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 et al. 2011). Even more, great part of the colonial administrative units (that later on became countries) had little relationship with the geographic demarcations of the precolonial ethnic groups.[5]

In order to capture the artificiality of the national borders, we use the "fractal" measure proposed by Alesina et al. (2011). To construct this measure, Alesina et al. compare national borders with geometric figures and use fractal dimensions to create a variable that measures the degree to which a border is artificial. We used this variable to create two subsamples, one that grouped only countries below the 66 percentile of the fractal measure for the whole sample, and another that grouped countries above the 33 percentile. Columns 2 and 3 replicates the first column but for those subsamples. Even though the estimate for the more artificially limited countries is lower than the one in column 3, both are not significant at the 10% level.

Table 1. Discontinuity and continuity in contemporaneous and precolonial development

	Current Income	Log precolonial population density				Health Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	est1	est2	est3	est4	est5	est6	est7
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Rich country	0.303***	-0.118	-0.090	0.269		0.008	
	(0.052)	(0.192)	(0.188)	(0.225)		(0.093)	
Rich region					-0.306		-0.145
					(0.191)		(0.110)
R-squared	0.881	0.669	0.721	0.653	0.674	0.000	0.045
Root MSE	0.434	1.609	1.450	1.675	1.596	0.235	0.229
N	322	322	275	243	322	39	39
Border FE	Yes	Yes	Yes	Yes	Yes	No	No
Sample	All	All	0 - 66%	33% - 100%	All	All	All
			Fractal measure pctile.				

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

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

[5]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 Rìo de la Plata and the Real Audiencia of Charcas divided the Incas between Argentina, Bolivia, Peru and Chile.

4 Main results

4.1 OLS and IV estimations

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

$$y_{b,c} = \beta_0 + \beta_1 I_{b,c} + X_b' \Phi + \alpha_b + \varepsilon_{b,c} \tag{2}$$

where $y_{b,c}$ is represents the development of region in border b belonging to country c. As discussed in the introduction, we use two measures for regional development: regional GDP per capita and nights light density. When we use satellite data we add a small number (0.01) since some observations may the value of zero, and we make a logarithmic transformation to account for outliers [6]. I denote national institutions measured as the rule of law index, α_b are border fixed effects and X' 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 neighboring 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 further test our identification assumption. 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 et al., 2000). The coefficients of these variables reflect the importance of weather and temperature over development, as highlighted by Myrdal (1968), Gallup et al. (1999) Masters and McMillan (2001) and Sachs (2001) among others. In addition, we also include some location controls (Gallup et al., 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 border captures potentially lower levels of development of border areas (Michalopoulos & Pappaioannou, 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 these 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 & Gallego, 2012).

Table 2 shows the results for the OLS estimation. In Panel A the the dependant 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

[6] This transformation is also used in Michalopoulos & Pappaioannou (2014)

Table 2. OLS estimation

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable is Ln Regional GDP per capita					
Rule of Law	0.230***	0.171***	0.167***	0.186***	0.249**
	(0.058)	(0.054)	(0.054)	(0.055)	(0.120)
R-squared	0.874	0.906	0.911	0.913	0.958
Root MSE	0.446	0.396	0.387	0.386	0.297
N	322	322	322	322	322
Panel B: Dependent variable is Ln(Lights Density)					
Rule of Law	0.016**	0.016*	0.017**	0.015*	0.053*
	(0.007)	(0.008)	(0.008)	(0.008)	(0.029)
R-squared	0.998	0.999	0.999	0.999	0.999
Root MSE	0.094	0.077	0.075	0.075	0.057
N	322	322	322	322	322
Location controls	No	Yes	Yes	Yes	Yes
Geographic controls	No	Yes	Yes	Yes	Yes
Precolonial population density	No	No	Yes	Yes	Yes
Colonial activities dummies	No	No	No	Yes	Yes
Ethnicity FE	No	No	No	No	Yes

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

historical determinants. Column 5 reports the results of weighted Least Squares, where each observation b, c is weighted by the length of the border b. In this way we weight more regions that share longer border and that are plausibly more alike. We believe this can better account for (un)observable variables that we are not able to include as controls. In Panel A, estimates are significant to 99% confidence levels from columns 1 to 4 , and falls to 90% in column 5. In Panel B the unconditional estimate is significant to 95%. However, when all controls are introduced the estimate falls to 90% significance levels and it is not significant when we then introduce length weights.

However, there are three main reasons for which the previous results are not causal. First, measurement error in the rule of law index. As it is pointed out in Acemoglu et al. (2001) (i) what we would like to capture is a cluster of institutions that includes other things besides rule of law and (ii) not only present but past institutions affect development. For these two reasons, our institutional measure is only imperfectly correlated with national institutions, which may cause attenuation bias. Second, most likely there are going to be omitted variables in equation 4 which vary across the national (and regional) border and that, hence, are not accounted for when comparing neighbor regions. We should expect that many other political economic variables determined by the identity of the local government (e.g., public good provision, education, etc) also change at the border. 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.

In order to account for all these problems, we use an instrumental variable approach instrumenting our measure of current institutions with settler mortality (Acemoglu et al. 2001, 2012, 2014). Using equation 4 as the second stage, we describe the first stage with the following equation

$$\text{Rule of law}_{b,c} = \gamma_0 + \gamma_1 log \text{ Settler mortality}_{b,c} + X_b'\Theta + \delta_b + v_{b,c} \tag{3}$$

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

Table 3 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 relation between the rule of law index and log settler mortality, which, in accordance to the historical argument, suggest 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 34.40, very superior to the critical value of 16.38 suggested in the Stock & Yogo tables.

All coefficients in Panels *A* and *B* are higher than their OLS counterparts, which suggest that the attenuation bias was bigger than any upward bias caused by omitted variables. Possibly more important, the coefficient remains pretty stable to the inclusion of more and more controls. Again, as in table 2, column 5 repeats column 4 but weights each pair of regions by the length of the border they share. Once again, the coefficient is significant and is even higher. Overall, the IV coefficients appear to be robust to different specifications, which allow us to conclude important part of the income differences we observe when crossing from one country to another can be accounted for differences in national institutions.

Table 3. Instrumental variables

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable is Ln Regional GDP per capita					
Rule of Law	0.383***	0.419***	0.370***	0.476***	0.311***
	(0.017)	(0.046)	(0.051)	(0.053)	(0.092)
Panel B: Dependent variable is Ln(Lights Density)					
Rule of Law	0.054***	0.029***	0.039***	0.043***	0.167***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.038)
Panel C: First Stage					
Log Settler Mortality	-1.330***	-1.613***	-1.704***	-1.790***	-1.563***
	(0.076)	(0.165)	(0.164)	(0.168)	(0.362)
N	322	322	322	322	322
Location controls	No	Yes	Yes	Yes	Yes
Geographic controls	No	Yes	Yes	Yes	Yes
Precolonial population density	No	No	Yes	Yes	Yes
Colonial activities dummies	No	No	No	Yes	Yes
Ethnicity FE	No	No	No	No	Yes

4.2 Falsification

As a further robustness check, we designed a falsification exercise in which we modified the national frontiers of every country in the sample in order pairs of regions that have the same quality of institutions and the same settler mortality (i.e., that originally belonged to the same country). We do this by moving the national border one region further as shown in figure 2 in the appendix, where the falsified borders are in red and the original borders in green. The exercise, then, consist in estimate the parameters falsifying the settler mortality rate according to the *new* borders, but using the original rule of law index. For example, Mendoza originally in Argentina is now, according to the new borders, part of Chile. Then, we impute to Mendoza the settler mortality of Chile and the rule of law index of Argentina.

The way in which we modified the national borders was motivated by three things: (i) maintain the drawing of the countries as much as possible, (ii) falsify most part of the regions (i.e., that both regions of the new pairs originally belonged to the same country), and (iii) maximize the number of observations. We obtain a total of 154 pairs of regions, from which we only use the 126 pairs that share the same original country.

Table 4 shows the results of the first stage of the IV denoted by equation 3, but using the new pairs of regions and the falsified settler mortality. Given that the difference in the *real* rule of law index between the new pairs of regions is zero, the expected value of the estimator of the falsified settler mortality is also zero. The unconditional specification in column 1 evidences, however, a strong and negative relationship between settler mortality and modern institutions. This suggests there are omitted variables driving the results. Column 2 shows the results for the estimation including border fixed effects, which, as we have argued, is more effective in controlling for (un)observable differences between regions. As expected, the coefficient is considerably closer to zero and statistically insignificant. The results remain the same as we include further controls.

Table 4. Falsification

First Stage	Rule of Law			
	(1)	(2)	(3)	(4)
Ln Settler Mortality (falsified)	-1.217***	-0.005	0.007	0.004
	(0.077)	(0.012)	(0.032)	(0.041)
Location controls	No	No	Yes	Yes
Geographic controls	No	No	Yes	Yes
Precolonial population density	No	No	Yes	Yes
Colonial activities dummies	No	No	No	Yes
Border FE	No	Yes	Yes	Yes
N	308	308	308	308

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

From this discussion we can conclude that, in America, there are variables that correlate both with settler mortality and with economic development that can be upward biasing the results when we do not condition on them. On the other hand, the fact that the estimates, as expected, came close to zero in the estimation in differences suggest that these confounding variables have the particularity of being similar at both sides of the regional borders. Thus, comparing neighbor regions allow us to better account for them. This variables can be geographic unobservables, but also ethnic precolonial development and ethnic fragmentation (Michalopoulos & Pappaioannou, 2013, 2014, among others) and/or prehistoric development (Spalaore & Wacziarg, 2006, and Ashraf & Galor, 2013).

4.3 Persistence of institutions

In this section we discuss possible mechanisms of transmission and persistence of the effect of institutions. We consider three different channels of persistence previously proposed by the literature: political representation, income inequality and human capital. Table 5 presents the results of three IV estimations of the effect of national institutions on each of the possible mechanisms.

Political representation

Following Bruhn et al. (2010) we use political malapportionment, which denotes a discrepancy between the share of legislative seats and the share of population held by electoral districts, as our

Table 5. Mechanisms

	Log Political representation		Log Gini		Avg. school years	
	(1)	(2)	(3)	(4)	(5)	(6)
Rule of Law	-0.817***	-1.392***	-0.006	0.052***	1.710***	1.515***
	(0.095)	(0.169)	(0.004)	(0.013)	(0.073)	(0.165)
Precolonial population density	No	Yes	No	Yes	No	Yes
Colonial activities dummies	No	Yes	No	Yes	No	Yes
N	298	298	271	271	317	317

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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. For example, Bruhn et al. (2010) find that Latin American countries that were under dictatorships and that had over representation where more likely to transit towards democracy, precisely because elites are more willing to leave their *de jure* political power (i.e., transit to democracy) if they know they can preserve (part of) their *de facto* power through malapportionment. In this sense, it can be argued that countries with better institutions (i.e., countries whose institutions favor less (or do not favor at all) 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 log Gini index as the dependent variable, columns 3 and 4 show that better institutions do not appear to have a significant effect over income inequality. This is consistent with the results of Acemoglu, Bautista, Querubín & Robinson (2008). In their paper, they use municipal evidence for Cudinamarca, 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 have effects over development through inequality.

Human capital

For a long time, an intense discussion has emerged between those who promote human capital as one of the major (if not the major) fundamental cause of economic development (Gaeser et al., 2004, Genniaoli et al., 2013, etc) and those who argue in favor of institutions as the fundamental cause and label human capital as a secondary cause, though an important one (Acemoglu, Johnson and Robinson, 2012, Acemoglu, Gallego and Robinson, 2014, etc). Clearly, the interrelation between the two -whether is human capital that causes institutions or the other way around or the two at the same time- does not make the identification job an easy one, and back and forth evidence has appeared supporting each one of the sides.

We follow the conclusions of Acemoglu, Gallego and Robinson (2014) and think of institutions as the fundamental cause and human capital as a channel of transmission/persistence -and not the other way around- to explore whether the data is consistent with the claim that better institutions induce more human capital and, through it, affect development. We take this stand because this paper is about institutions, but also because we think that the empirical strategy of that paper is the most compelling one up to date. In their paper, Acemoglu, Gallego and Robinson are able to instrument both institutions and human capital in order to disentangle between the two effects over development. In other words, they manage to capture the exogenous variation of institutions and human capital and they find that when the effect of institutions remained as large as in the previous institutional literature supporting the role of institutions as a fundamental cause, the effect of human capital went down substantially, to the level of the mincerian returns of education found in the microeconomic estimations. 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.

5 Regression Discontinuity

Up to this point we have focused on regional level observations, but there are several reasons for wanting to explore the impact of institutions at a more disaggregated level. First, contiguous regions do not have the same shape or cover the same area. A neighbor region, as we have argued, continues to be a plausibly better counterfactual than probably any other region, but still we are sometimes comparing big to small regions or regions that cover areas further from the border to regions that are very close to it. Second, if the discontinuity on national level institutions is at the border, we would like to see if the impact we are finding is driven by areas far from the border or if it persist even when we look close to the border. By looking at areas closer to the border, we can further investigate the extent to which borders are porous and see if they are de facto more enforcing -in contrast to Africa, for example, as we have argued. With this in mind, we follow Michalopoulos and Papaioannou (2013, 2014) and perform a pixel level analysis using a regression discontinuity (RD) design using the following equation:

$$y_{p,b,c} = \beta_0 + \beta_1 \mathrm{I}_c^{High} + f(Distance_{p,b,c}) + X'_{p,b,c}\Phi + \alpha_b + \varepsilon_{p,b,c} \tag{4}$$

We divide each region in pixels p of 0.125 x 0.125 decimal degrees, which is equivalent to approximately 12.5 squared-km. Here our dependent variable $y_{p,b,c}$ is a dummy equal to 1 if pixel p is lit. As Michalopoulos and Papaioannou (2013, 2014) argue, looking at the extensive margin of luminosity helps to account for the non-linearity of light density and is easier to interpret. $f(Distance_{p,b,c})$ denotes the RD-polynomials of the distance of the centroid of the pixel to the national border; to be consistent with and more comparable to Michalopoulos and Papaioannou (2013, 2014), we employ specifications with polynomials of 3*rd* and 4*th* degree and we allow the coefficients to be different at each side of the border. Our coefficient of interest is I_c^{High}, which is a dummy that takes the value of 1 if the region of the pair belongs to the country with better institutions. In this way, we are going to be capturing the effect of crossing to a country with better institutions over the probability that a pixel is lit at the border. We also control for area and population of the pixel.

Table 6. RD Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth	All pixels		100 km		50 km	
Panel A: All pixels						
High Institutions	0.107***	0.095***	0.050***	0.048***	0.050***	0.049***
	(0.003)	(0.004)	(0.008)	(0.009)	(0.009)	(0.009)
R-squared	0.235	0.235	0.381	0.381	0.406	0.406
Root MSE	0.352	0.352	0.341	0.341	0.331	0.331
N	567,492	567,492	39,718	39,718	21,261	21,261
Panel B: Only populated pixels						
High Institutions	0.063***	0.088***	0.038***	0.048***	0.042***	0.049***
	(0.005)	(0.005)	(0.011)	(0.009)	(0.012)	(0.009)
R-squared	0.320	0.321	0.379	0.381	0.402	0.406
Root MSE	0.393	0.392	0.377	0.341	0.368	0.331
N	297,482	297,482	26,697	39,718	14,117	21,261
Order of polynomial	3rd	4th	3rd	4th	3rd	4th
Pixel area & pop. dens.	Yes	Yes	Yes	Yes	Yes	Yes

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

Table 6 shows the estimates of the RD approach. Columns (1) and (2) of Panel A use all the pixels of each region and indicates that the probability of seeing a lit pixel significantly increases in around 10% when crossing to a country with higher institutional quality. Given that the distance of pixels is not balanced across the border and that we have reasons to look closer to the border, the next two pairs of columns only consider pixels 100 km and 50 km around the border (or 50 km and 25 km at each side, respectively). The magnitude of the coefficients drop to half but remain positive and highly significant. They show an increase of 5% in the probability that a pixel is lit, which is economically relevant given that the regions of our sample have 30% of lit pixels on average. This result is robust to different degrees of the RD-polynomials and does not decrease even further when we decrease the bandwidth to 50 km (or 25 km at each side of the border). This last result suggest that even if borders are porous, they are not porous enough to dissipate the effect of crossing to a country with better institutions. In other words, frontiers in America appear to de facto limit the reach of national level variables.

Given that some borders are dictated by mountains or deserts and may contain large unpopulated areas, as a robustness check and to be sure that these unpopulated area are not affecting the results, Panel B excludes pixels with no population, even if we were already controlling for population in the previous specifications. As before, the coefficients are positive and economically and statistically significant, and tend to show the same pattern when narrowing the bandwidth.

Summing up, performing pixel level regressions allowed us to look closer to the border, where we can assess even better the potential omitted variable biases that could have persisted from the regional level analysis. In this way, we showed that development changes discontinuously at the border when we cross from one country to another with better institutions.

6 Conclusions

Using subnational level data for all the countries in the Americas, this paper shows that an important part of the differences in economic development that we observe when crossing from one country to another can be explained by differences in the quality of national institutions. In particular, using settler mortality as source of exogenous variation for institutions, our estimates show that changes in institutional quality of one standard deviation carry changes of 0.63 standard deviations in log GDP per capita. This effects are to be interpreted in a causal manner, and they are not generated by differences in geographic characteristics and/or precolonial population density, among other determinants.

We also discuss some possible mechanisms of transmission and persistence of the effect of institutions on income. We find a strong impact of institutions over political representation and human capital. Regarding political representation, this suggests that regions with better institutions will be less over represented politically, which is consistent with previous literature. At the same time, positive relationship found between institutions and schooling is consistent with the idea that improvements in the quality of institutions translates into higher GDP per capita through increases in human capital.

Furthermore, the RD estimates show that our results are robust to different identification setups. Regions in America can be long enough so that fixed effects for contiguous regions in neighboring countries cannot account for all unobservables. By using an RD estimation strategy we are able to account for narrower bands, where it is more likely that non-observables are equal beyond reasonable doubts.

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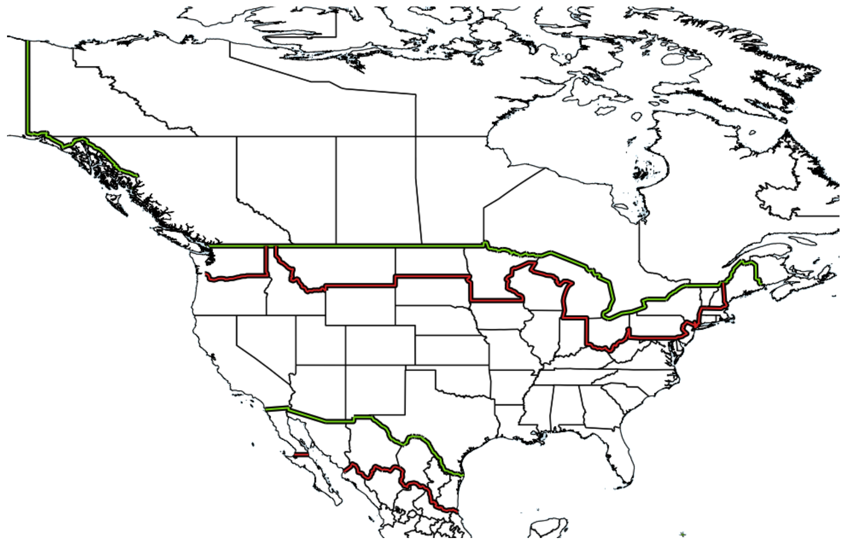
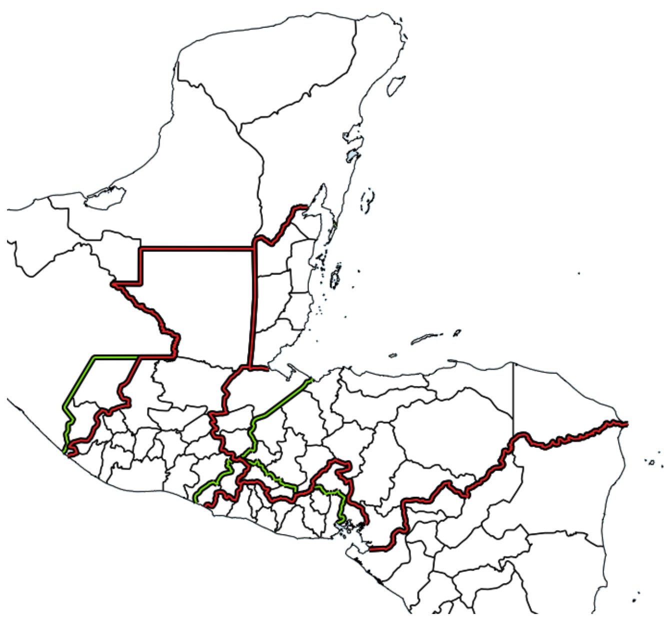
Appendix

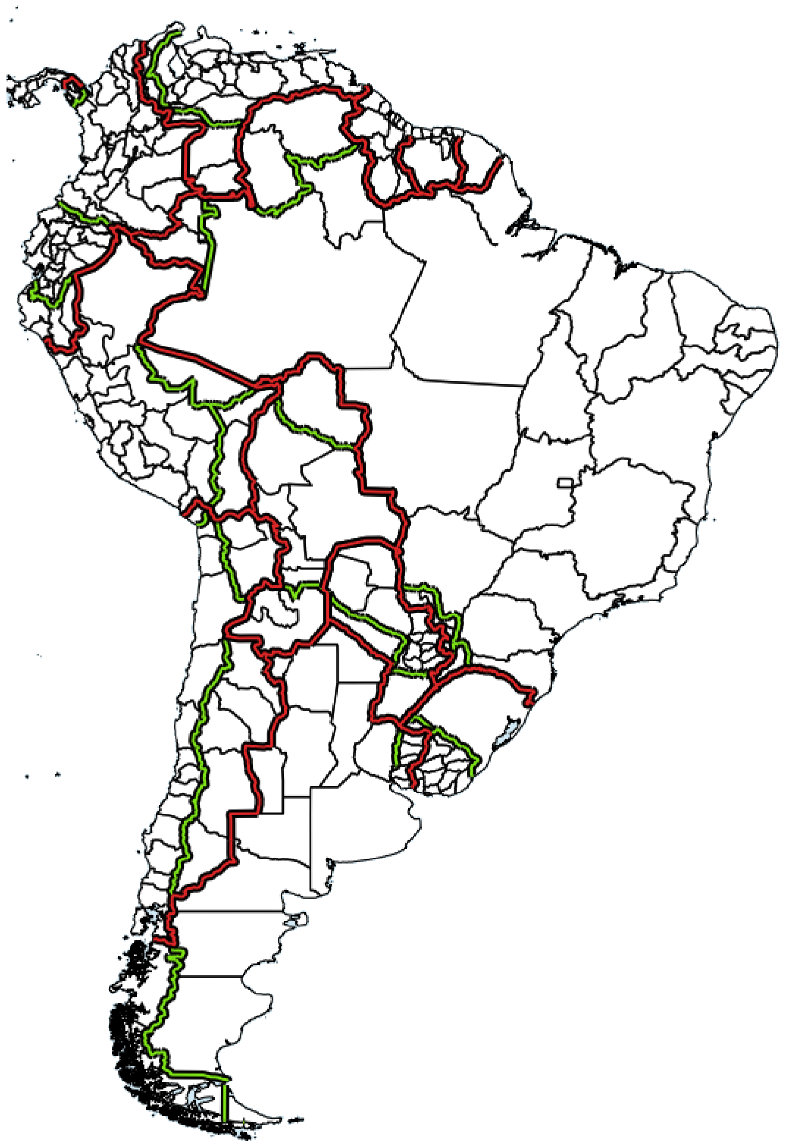


(a) North America



(b) Central America

Figure 2. Real and falsified borders



(a) South America

Real and falsified borders

Table A1. Human Capital vs Institutions

	Reference	OLS	Bad control	Semi-Sctructural	Structural
	(1)	(2)	(3)	(4)	(5)
Panel A: Second stage					
Rule of Law	0.302***	0.206***	0.233***	0.327***	0.558***
	(0.036)	(0.044)	(0.087)	(0.077)	(0.118)
Years of Schooling in 2005		0.034*	0.026		-0.116**
		(0.017)	(0.032)		(0.046)
Catholic Priests				-0.079***	
				(0.012)	
Primary enrollment in 1900				0.000	
				(0.002)	
Panel B: First stage for Rule of Law					
Ln Population density 1500	0.200***		0.178**	0.174**	0.174**
	(0.075)		(0.076)	(0.069)	(0.069)
Log Settler Mortality	-1.802***		-1.387***	-2.259***	-2.259***
	(0.180)		(0.219)	(0.448)	(0.448)
Years of Schooling in 2005			0.110***		
			(0.040)		
Catholic Priests				-0.001	-0.001
				(0.037)	(0.037)
Primary enrollment in 1900				-0.012	-0.012
				(0.009)	(0.009)
Panel C: First stage for Years of Schooling					
Catholic Priests					0.192***
					(0.046)
Ln Population density 1500					0.309***
					(0.055)
Log Settler Mortality					-2.054***
					(0.640)
Primary enrollment in 1900					0.044***
					(0.012)