The Colonial Origins Of Comparative Development: Comment.
A Solution to the Settler Mortality Debate

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The Colonial Origins Of Comparative Development: Comment. A Solution to the Settler Mortality Debate

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Abstract

I address David Albouy's (2006) critique of the data constructed by Daron Acemoglu, Simon Johnson and James Robinson (2001). The contribution of this paper is to instrument for settler mortality rates that are collected from historical sources – and that may be measured with error – with a geographic model of the determinants of disease. I first establish that my instruments are significant predictors of mortality and are otherwise excludable to institutions. Among other things, the excludability is established by a falsification exercise, in which I document that the geographic potential for mortality strongly affected institutions in former colonies, yet it had no effect on institutions in the rest of the world. This differential effect settler mortality had on development can only be rationalized by the early institution building hypothesis that Acemoglu et al. argue for. I next repeat the analysis of Acemoglu et al. instrumenting for the historical mortality rate with its geographic projection. The instrumented mortality rate is a highly significant predictor of institutional quality. Moreover, this result is true when instrumenting for either the original data or the revised mortality series of Albouy. This result is also true when accounting for the population that the historical data was sampled from. Turning to the instrumental variable estimations, I show that also the relation between institutions and income is highly significant and that the associated importance of institutions for international income differences is substantial. Again this finding is true when using either of the two historical series and also when accounting for the population that the historical data was sampled from. I thus conclude that the empirical results presented in Acemoglu et al. indeed reflect their early institution building hypothesis rather than measurement error.

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1 Introduction

With their seminal work on the effect colonization policies had on comparative development, Daron Acemoglu, Simon Johnson and James Robinson (2001) develop a strong case for the importance of property rights institutions as the ultimate cause of economic prosperity. Building on earlier work of Knack and Keefer (1995), Mauro (1995), La Porta et al. (1998), and Hall and Jones (1999), the authors argue that early settlement policies were influenced by the mortality rates potential settlers faced in the colonies. They show that the number of Europeans that settled in a colony influenced the colonization policies adopted by imperialist nations, with very different associated institutional outcomes.

The authors' most important contribution is to construct an instrumental variable – the mortality rate of European settlers in former colonies – that is strongly related to institutional quality, yet has no direct impact on income differences. Using their measure of the settler mortality rate in former colonies, the authors establish that institutions are the main determinant of economic prosperity.

Collecting the mortality rates of settlers during colonization is a difficult task, and Acemoglu et al. rely on several sources to construct their series of mortality rates for 64 countries. Their most important source is Curtin (1989), who collected mortality rates from soldiers stationed in former colonies. However, this measure is not available for many nations and the authors thus rely on mortality rates of bishops from Guiterrez (1986) and on mortality rates of laborers from Curtin (1998).

When working with data that originated up to 300 years ago and is collected from various sources, it is inevitable that coverage is incomplete and that the data is measured with some error. David Albouy's (2006)[1] comment on the quality of this data, however, does not address the mere fact that there may be additional noise in the data. Rather, he asserts that the settler mortality data suffers from systematic shortcomings that create a measurement error correlated with economic outcomes, hence generating an artificial correlation between mortality estimates and institutional outcomes.

[1] Two versions of Albouy's critique exist (Albouy (2004) and (2006)). In what follows below, I refer only to the data constructed for the 2006 version, which can also be downloaded for David Albouy's webpage (http://socrates.berkeley.edu/~albouy/).

His critique focuses in particular on three issues. First, he claims that the data is measured inconsistently when there are multiple mortality rates to choose from for a country. For example, this includes deviating from Acemoglu et al.'s pre-specified rule of always using the earliest available mortality rate. Second, he argues that the mortality rates are sampled from very different populations such as bishops, soldiers stationed in barracks, or soldiers in campaign. These groups had very different living conditions, with very different associated mortality rates even for the same disease environment. Third, he points out the fact that Acemoglu et al. extrapolate many mortality rates (28 of the 64 countries use original sources) and argues that this is done in an arbitrary way.

Acemoglu et al. (2005) and (2006) answer each of his criticisms in detail. They conclude that his revisions of the mortality rates "reflect a long list of mistakes on his part in coding and selecting data" (Acemoglu et al. (2005), p. 39).

In this paper, I do not examine whether the original data was assembled in a "consistent" manner, nor do I attempt to do this for the David Albouy's revisions. Doing so would be somewhat pointless, since there is not only disagreement on which mortality values to choose from within a historical source such as Curtin (1998). In addition, one can find multiple mortality rates across historical sources. Given the very large supply of potential sources for historical mortality rates one could come up with,[2] it is always possible to construct a mortality series that either confirms or rejects the validity of Acemoglu et al.'s theory of colonial origins of comparative development.

The current paper is motivated by this fundamental problem when working with historical data and the importance of Acemoglu et al. It is far from proven that Albouy's concerns are justified; however, the sheer importance of the "colonial origins" theory of comparative development for the new comparative economics literature makes it worthwhile to establish the validity of this theory in detail and beyond any level of doubt. For example, according to the social citation index, of all articles published in the American Economic Review in the time of 2000 to 2002, Acemoglu et al. ranks second in citations (237 up to February 2007). Moreover, the settler mortality series is a prominent instrument for institutions and doubts about the quality of this data also bring in question numerous other articles. For example, Easterly and Levine (2003) use it to examine several competing theories of comparative development, while Rodrik et al. (2004) use it to examine the effect trade has on prosperity conditional on the quality of institutions.

[2] For example, neither Albouy nor Acemoglu et al. consult military archives for first hand soldier mortality data, nor do they search for newspaper sources that published mortality rates during colonialism.

The contribution of this paper is to show that there is a straightforward solution to Albouy's criticisms: when in doubt about the endogeneity of a variable, economists try to find instruments for that variable. This paper instruments for the settler mortality rates with a geographic model of the determinants of disease. Geography is measured precisely; is shown to be strongly related to mortality; is also shown to be otherwise excludable to institutions; and is thus a valid instrument for mortality. The methodology of this paper is related to the work of Anthony Kiszewski et al. (2004), who instrument for the potentially endogenous level of malaria with a measure of the geographic potential for the disease. Similarly, I construct several geographic projections of the historical mortality estimates obtained by Acemoglu et al. and the ones obtained by Albouy.

When instrumenting for mortality with geography, I address all of the three criticisms brought forward by Albouy. First, when constructing my instruments for mortality, I only rely on precisely measured geographic information as instrumental variables such as average temperature or monthly rainfall. Since all potential sources for these variables coincide, there is no room for subjectivity or "inconsistencies" when constructing my instruments. Second, when estimating the relation between geography and disease, I also account for the different populations the mortality rates were sampled from. More specific, I include dummies for whether the data was collected from soldiers during campaign, from forced laborers, or from bishops to the estimation of the geographic model of mortality. I then predict this model, yet I partial out the effect of the population dummies. Hence, while the original data might be influenced by the population the data was sampled from, my measure of the geographic potential of mortality is not. Third, when estimating the relation between geography and mortality, I do not extrapolate certain mortality rates, but I use the estimated relation between geography and mortality to instrument for the countries with missing direct estimate of disease. That is, I do not extrapolate certain mortality values to neighboring nations in an arbitrary way, but I argue that the relation between geography and disease is the same in the countries with direct mortality estimates and in the rest of the sample. Using this rule, I am able to instrument also for the extrapolated mortality rates in a systematic and consistent way.

In addition to showing that my geographic instruments are relevant predictors of mortality, I also establish the excludability of these instruments by a variety of robustness tests, additional instruments for institutions and the associated overidentification tests, and by an additional falsification exercise.

In the falsification exercise, I document that while the constructed measures of the geographic potential for mortality – that are very strong predictors of institutions in the sample of former colonies – are not at all related to institutional outcomes in a sample of 60 nations that have not been colonized. If the correlation between the geographic potential for mortality and institutions is a result of the direct effects disease environment has had on development, this effect is present in all countries equally. The latter is not the case, and I therefore conclude that the only channel through which early disease environment did influence institutional development was indirect through European settlements and colonization policies.[3] Summarizing, my instruments for settler mortality are valid because they are strongly correlated with mortality rates and otherwise excludable from the estimation because they influence property rights only indirectly through settler mortality.

After constructing four measures of the geographic potential of disease (for each of the two historical sources: a geographic projection of mortality and a geographic projection of mortality adjusted for the population the rate was sampled from) and establishing their validity as instruments, I repeat the analysis of Acemoglu et al. instrumenting for mortality with its geographic projection. Overall, my results strongly support Acemoglu et al.'s hypothesis of the colonial origins of comparative development. First, *I show that the relation between institutional outcomes and the instrumented mortality rate is highly significant in both the sample of Acemoglu et al. and when using the data constructed by Albouy.* The latter finding is robust to inclusion of controls and to accounting for the population the historical data was collected from (bishops, from forced labor or during campaign). While I find that the population from which the historical mortality estimate was sampled from does have a significant impact on mortality rates,[4] accounting for this effect does not change the relation between mortality and institutions. I conclude that the first stage results of Acemoglu et al. indeed do reflect their "early institution building" channel rather than measurement error, as is asserted by Albouy.

[3] Although the identification of the instrumental variable estimations requires that early disease environment affects development only through mortality and therefore institutions, the estimations do allow for direct effects of geography on development. Geography, measured by latitude, the potential for malaria, and numerous other controls, is both a significant and economically sizeable determinant of development also when the colonial origins channel is accounted for.

[4] Moreover, the effect the sampling population had on mortality rates is very large. For example, I estimate (Table 1 Column 3) that conditional on the disease environment, soldiers in campaign are roughly three and a half times as likely to die from disease than soldiers stationed in barracks.

Second, I establish the effect institutions have on income by using the constructed measures of early disease environment to instrument for institutional outcomes. Not surprising given the strong relationship between the potential for mortality and institutions, I find very similar results as do Acemoglu et al. *For both mortality series and both in a statistical and economic sense, institutions are a highly significant determinant of economic prosperity.*[5] This result is again shown to be robust to the inclusion of controls, to accounting for how the historical data was collected, and also to including additional instruments for institutions.

A striking result of this paper is that while the relation between mortality and development is very different when using Albouy's data as opposed to the one from Acemoglu et al., the relation is nearly identical once I instrument for each of the two historical estimates of mortality. While these two series differ substantially, they are both affected by geography in a very similar way. Correspondingly, for both the results relating mortality to institutions and for the results relating institutions to income, it does not matter which series is instrumented for.

Summarizing, my findings lead me to conclude that while collecting accurate mortality data that originated several hundred years ago may be difficult or nearly impossible, the proposed method of instrumenting for the historical estimate with a geographic model of disease can deal with these issues and can also allow to establish the validity of Acemoglu et al.'s theory of the colonial origins of development. I also find no evidence that their point estimates are biased by measurement error that is correlated with economic outcomes.

The next section briefly discusses Albouy's three criticisms of the data constructed by Acemoglu et al. Section 3 presents the relation between geography and disease and section 4 constructs and discusses the instruments for mortality. Section 5 presents the main result of this paper, the relation between instrumented mortality and institutional outcomes. Next, section 6 establishes that the instruments are indeed excludable by a falsification exercise. Thereafter, I present the results for the importance of institutions on income differentials in Section 7 and I establish the robustness of these results in Section 8. I introduce further instruments for institutions and the associated overidentification tests in Section 9. Section 10 concludes.

[5] To quantify this statement, of the 160 (40 for each of the four measures of the geographic potential for mortality) IV specifications estimated that relate income to institutions, I find that the first and second stage coefficients of interest are significant at the 5% level in 159 instances, in 144 specifications they are significant at the 1% level, and in the majority of the cases they are significant at far higher levels.

2 Albouy's Critique

In this section, I briefly list the three main criticisms brought forward by Albouy without taking a position on the validity of these arguments. The interested reader is referred to Albouy's note and to Acemoglu et al. (2005) and (2006), who answer each of his criticisms in detail.

Albouy's first – and most controversial – criticism is that the data is measured inconsistently when there are multiple possible mortality rates to choose from. Among other things, he argues that Acemoglu et al. deviate from their pre-specified rule of always using the earliest available mortality rate for a given country. For example, Albouy focuses on the case of Sudan. He claims that Acemoglu et al. violate their pre-specified rule of always choosing the first available mortality rate. Acemoglu et al. (2006), in turn devote a full two pages to justify their choice of mortality rate for Sudan, stating that their "actual coding rule was to take the first *peacetime* number where available" (p.19, emphasis added). Albouy's discussion of single values is very detailed an shall not be reproduced in this paper. However, it is important to note that Albouy's main claim is not that his data reflects the true mortality rates prevailing during colonialism better than does the data of Acemoglu et al. Rather, his more general argument is that if one consistently follows a pre-specified rule of how to pick mortality rates, the correlations between mortality and institutions are much weaker than when working with the data of Acemoglu et al.[6]

Albouy's second criticism concerns the comparability of the different sources of mortality rates. The true mortality rate for the average settler during colonization is not available and Acemoglu et al. thus rely on mortality data collected from soldiers, bishops, and laborers. Albouy argues that these populations had very different living conditions, with very different associated prevalence of disease and consequently mortality rates even for the same disease environment. Moreover, for the soldier mortality data from Curtin (1989), Acemoglu et al. sometimes use data collected during a campaign, yet in other instances they use data collected from soldiers that were stationed in barracks. Campaign mortality data were "66 to 2000 percent higher than barracks rates" (Albouy (2006) p. 6). Hence, the results of Acemoglu et al. are biased if European powers had to fight relatively more campaigns in countries with worse initial and current institutions.

[6] This leads him to conclude that "it seems unlikely that a convincing set of settler mortality rates by country can ever be constructed" (p. 17). While this may be true when considering only historical sources, this paper shows that one can construct such a series by exploiting the close link between mortality and geography.

The third criticism concerns the way in which Acemoglu et al. extrapolate mortality rates to neighboring countries. Albouy argues that the pattern of assigning mortality rates is "questionable," that countries could have been assigned mortality data from *various* neighboring countries, and that Acemoglu et al. present no clear criterion how they assign mortality rates to other countries.[7]

Summarizing, Albouy claims that the original mortality data suffers from "a number of inconsistencies, comparability problems and questionable geographic assignments" (abstract). He then repeats the analysis of Acemoglu et al., with a very strong result: *addressing any of his three criticisms alone leads to a weak relation between settler mortality and institutions.*

3 Geography and Mortality

If indeed the mortality data of Acemoglu et al. is measured with error, one needs instrumental variables that are relevant predictors of mortality and otherwise excludable to institutions. This section establishes that geography is a relevant predictor of mortality, and I later establish that it is excludable, i.e. only related to institutional quality through settler mortality.

I instrument for the historical mortality series with geographic information such as average temperature, average rainfall and a Mediterranean climate dummy. These variables are measured precisely, have been constant throughout the last 300 years,[8] and are strongly correlated with germs and disease. While one might disagree on the details of how to collect data from historical sources, the contribution of this paper is to use geographic variables that are measured precisely and that therefore yield a unique projection of mortality. The procedure of instrumenting for the observed level of a disease with the geographic potential for it is a standard instrumental variable estimation. The methodology of this paper is related to Kiszewski et al. (2004), who instrument for the potentially endogenous level of the prevalence of malaria with the geographic potential for the disease. Their work has been applied by Sachs (2003) to argue that the geographic potential for malaria has large effects on economic development.

When constructing the geographic potential for settler mortality, I also address Albouy's "comparability problem." To make the mortality series comparable across the populations they

[7] Albouy also argues that even if the data was assigned correctly, the standard errors have to be adjusted for the fact that there are only 36 different mortality rates, which are assigned to 64 countries.

[8] Recent developments could raise doubts on whether all aspects of geography have indeed been constant throughout modern history. Note, however, that for these instruments to be valid, it is only neccesarry that climate changes are uncorrelated to early institutions set up during colonization.

were sampled from, I simply add population dummies to the specifications, which control for the fact that the data was sampled from soldiers stationed in barracks, from soldiers in campaign, from bishops, or from forced laborers. Indeed, doing so improves the fit of the estimation considerably.

In Table 1, I estimate the relationship between geography and mortality for the 64 countries in the analysis of Acemoglu et al. (in Columns 1 to 3 of Table 1) and the 62 countries in the analysis of Albouy (in Columns 4 to 6). When working with Albouy's data, I focus on his revised series "mort_a2." He also offers a series titled "mort_a1," which is less revised than the mort_a2 series. The results presented in this paper do not depend on choosing either of his two mortality series, but to point out that Albouy's revisions are not important when instrumenting mortality with geography, I use the series that differs most from the data of Acemoglu et al.

In Column 1 of Table 1,[9] I present a simple model of geography and mortality. The dependent variable is the natural logarithm of the settler mortality rate collected by Acemoglu et al. Higher average temperature is associated with higher levels of disease. The independent variables (from Parker (1997)) are average annual temperature, minimum monthly rainfall, maximum monthly rainfall and a Mediterranean climate dummy. For better comparability, all variables except the dummies are standardized. For example, in the model of Column 1, the estimated coefficient of (standardized) average temperature is 0.46. The standard deviation of average annual temperature is 5.09, implying that a 1 degree Celsius warmer climate is associated with a 9% higher level of mortality. I also evaluate the impact of rainfall and its seasonal variation. Areas with pronounced dry (low minimum monthly rain) or wet seasons (high maximum monthly rain) are characterized by high mortality rates. Mirroring this finding of low variation in climate being associated with healthier living conditions, Mediterranean climate is associated with lower prevalence of disease.

How relevant are my instruments for mortality? Throughout Table 1, I report the p-value corresponding to the joint null-hypothesis that these 4 geographic variables (average temperature, minimum and maximum monthly rain, Mediterranean dummy) do not matter for mortality. I reject the null hypothesis at the 1% significance level in all regressions presented in Table 1, and most of the time I also reject at far higher levels of significance.

Is the selection of these four variables exhaustive? In Column 2, I add the standardized distance from the equator and the standardized fraction of the population living in temperate areas (KGPTEMP as is used in Sachs (2003)) to the estimation. Conditional on the information

[9] In all estimations presented in this paper, robust Huber/White/sandwich standard errors are reported in brackets. Where applicable, the data is clustered.

of the previous model, these two regressors are not significant predictors of mortality and the F-score of the model actually decreases substantially when including these two variables. I have also added several other geographic controls, and except for the Malaria Ecology variable from Kiszewski et al. (2004), these are not improving the fit of the model of mortality. I do not include Malaria Ecology in the specifications of Table 1, so that I can later test for the direct effect malaria has on income in the instrumental variables regressions presented below.

In Column 3, I address Albouy's second major criticism: the data is collected from different populations, with very different living conditions and different mortality rates even for the same disease environment. I thus add dummy variables to the estimation that equal one if the data was collected from soldiers in a campaign, from forced laborers or from bishops respectively. Indeed, the population the data was sampled from has a strong influence on mortality. Compared to the omitted group – soldiers stationed in barracks – soldiers in a campaign are $Exp[1.27] \approx 3.5$ times as likely to die from disease, and this difference is significant. Also forced laborers are more likely to die from disease (again by a large factor) while bishops faced a slightly lower mortality rate. Accounting for Albouy's argument that the sampling population has to be taken into account improves the fit of the model considerably and also changes the estimated coefficients of the geographic variables (compare Column 1 and 3).

In Columns 4 to 6, I repeat the analysis relating geography to mortality, yet I use Albouy's data as the dependent variable. The results mirror the previous findings. Average temperature, minimum and maximum monthly rainfall, and a dummy for Mediterranean climate strongly impact mortality (Column 4). The same cannot be said for latitude or KGPTEMP (Column 5) and the F score of the model decreases when these two variables are added to the estimation. In Column 5, only average temperature is significant, but the joint significance of regressors 1 to 4 is easily rejected at the 1% level. Dummy variables for data sources from campaigns, forced laborers and bishops are not significant by themselves (Column 6); however a joint test of significance cannot be rejected at the 5% level.

Table 1 - Mortality and Geography

	(1)	(2)	(3)	(4)	(5)	(6)
	Sample of Acemoglu et al.			*Sample of Albouy*		
	Dependent Variable is the Natural Logarithm of Mortality from Acemoglu et al. 2001			**from Albouy 2006 (Revision 2)**		
Avg. temperature (standardized)	0.46 [0.12]**	0.44 [0.14]**	0.41 [0.12]**	0.42 [0.15]**	0.39 [0.17]*	0.39 [0.12]**
Monthly rain (min, standardized)	-0.52 [0.09]**	-0.63 [0.18]**	-0.24 [0.11]*	-0.35 [0.11]**	-0.4 [0.23]	-0.24 [0.11]*
Monthly rain (max, standardized)	0.24 [0.10]*	0.26 [0.12]*	0.17 [0.10]	0.3 [0.17]	0.27 [0.17]	0.26 [0.20]
Mediterranean climate dummy	-1.09 [0.32]**	-1.19 [0.35]**	-0.89 [0.27]**	-0.84 [0.36]*	-0.75 [0.51]	-0.8 [0.34]*
% pop. in temperate zones (standardized)		0.3 [0.20]			0.28 [0.23]	
Latitude (standardized)		-0.27 [0.16]			-0.37 [0.21]	
Forced laborer dummy			1.16 [0.28]**			-0.05 [0.23]
Bishop dummy			-0.55 [0.31]			-0.22 [0.37]
Campaign dummy			1.27 [0.33]**			0.68 [0.35]
p-value regressors 1 to 4	<0.001	<0.001	<0.001	<0.001	0.003	<0.001
p-value population dummies	-	-	<0.001	-	-	0.017
No. of observations	64	60	64	62	58	62
No. of clusters	36	34	36	37	33	37
R-squared	53%	55%	65%	45%	47%	50%

Notes: In Table 1, the dependent variable is the natural logarithm of mortality, for Columns 1 to 3 from Acemoglu et al. and for Columns 4 to 6 from Albouy, Revision 2; Forced Laborer, Bishops and Campaign dummies are taken from Albouy; KGPTEMP is not available for the Bahamas, Hong Kong, Malta, and Singapore; for other regressors see main text; throughout Table 1, I report a p-value for the joint null hypothesis that the coefficients of Average temperature, Monthly minimum and maximum rain and the Mediterranean dummy are equal to 0; where applicable I also report a p-value for the joint null hypothesis of the population dummies; clustered and robust standard errors reported in brackets; * significant at 5%; ** significant at 1%

4 Measures of Early Disease Environment

In this section, I construct two geographic instruments for the mortality data of Acemoglu et al. and two instruments for the data of Albouy.[10] For each of Albouy's and Acemoglu et al.'s series, I first create a geographic instrument for mortality and then a second one that also partials out the influence the sampling population had on mortality rates. In this section, I also show that the discrepancy between the data of Albouy and the one of Acemoglu et al. vanishes when the proposed strategy of instrumenting with geography is employed.

The four variables of the geographic potential for disease correspond to the specifications of Columns 1, 3, 4, and 6 of Table 1 presented in the previous section. Predicting the models of Columns 1 and 3 that relate mortality to geography is straightforward. I denote the predicted variable from Table 1, Column 1 by "DE AJR" (Disease Environment using Acemoglu Johnson and Robinson) and the one from Column 3 by "DE Albouy." Each variable is simply the projection matrix of the geographic variables on mortality, i.e. the geographic potential for disease. I do not predict the models of Columns 2 and 5, but doing so would not lead to any different conclusions than presented below.

To address Albouy's concern about the comparability of the mortality rates, I next predict the models of Columns 3 and 6, yet I partial out the effect the sampling population had on mortality. For example, while soldiers in campaign were indeed more likely to die from disease than soldiers stationed in barracks, the mortality of both groups was affected by geography in the same way. Thus, while there is no historical source that presents mortality rates from a homogenous population, the geographic model can take into account the population that the data was sampled from. I predict two measures that are adjusted for the population. "DE AJR adjusted" refers to Column 3 that uses the 64 countries of Acemoglu et al. and adjusts for the group dummies. Similarly, "DE Albouy adjusted" refers to Column 6 that uses the 62 countries of Albouy and adjusts for the group dummies.[11]

Table 2 presents summary statistics of the two original and the four constructed measures of disease and a pair-wise correlation diagram. The pair-wise correlation is calculated over 64

[10] The appendix lists the data created in this section, and also provides further graphs that compare the mortality rates from historical sources to the constructed measures of early disease environment.

[11] Since soldiers stationed in barracks are the omitted group in the models of Column 3 and 6, I hence construct a variable that measures the potential mortality of soldiers stationed in barracks for all countries of the sample. Taking a different population (for example Bishops) as the omitted group would only shift the constant in the regressions presented below.

or 62 observations. All variables correlate strongly and significant far beyond the 1% level. The correlation coefficient between the original data from Albouy and Acemoglu et al. is equal to 0.861. This is substantially lower than the correlation between the predicted measures of early disease. The correlation between the two unadjusted measures of disease environment (DE AJR and DE Albouy 2) is equal to 0.988 and the one between the two measures adjusted for population (DE AJR adjusted and DE Albouy adjusted) equals 0.994. Since both mortality series are affected by geography in a similar way, the geographic projection of mortality is nearly identical when using Albouy's or Acemoglu et al.'s data.

To emphasize this point, Figure 1 plots the relations between the original data of Albouy and Acemoglu et al. (Figure 1 A) and the relation between the two unadjusted measures of early disease (Figure 1 B, The data is created from Columns 3 and 6 of Table 1). In Figure 1 A, all countries where Albouy makes no revisions to the data of Acemoglu et al. lie on the 45 degree line. Those that do not lie on the 45 degree line differ between the data sets, and differences of two log-points – roughly seven fold – are quite common. In contrast, the differences for the predicted measure of early disease environment (Figure 1 B) are extremely small, and all observations lie close to the 45 degree line. When instrumenting mortality with geography, the mortality series of Acemoglu et al. and Albouy are nearly identical and thus the results of this study are invariant to using either of the two data sets.

Table 2 - Data Summary and Pairwise Correlation Diagram

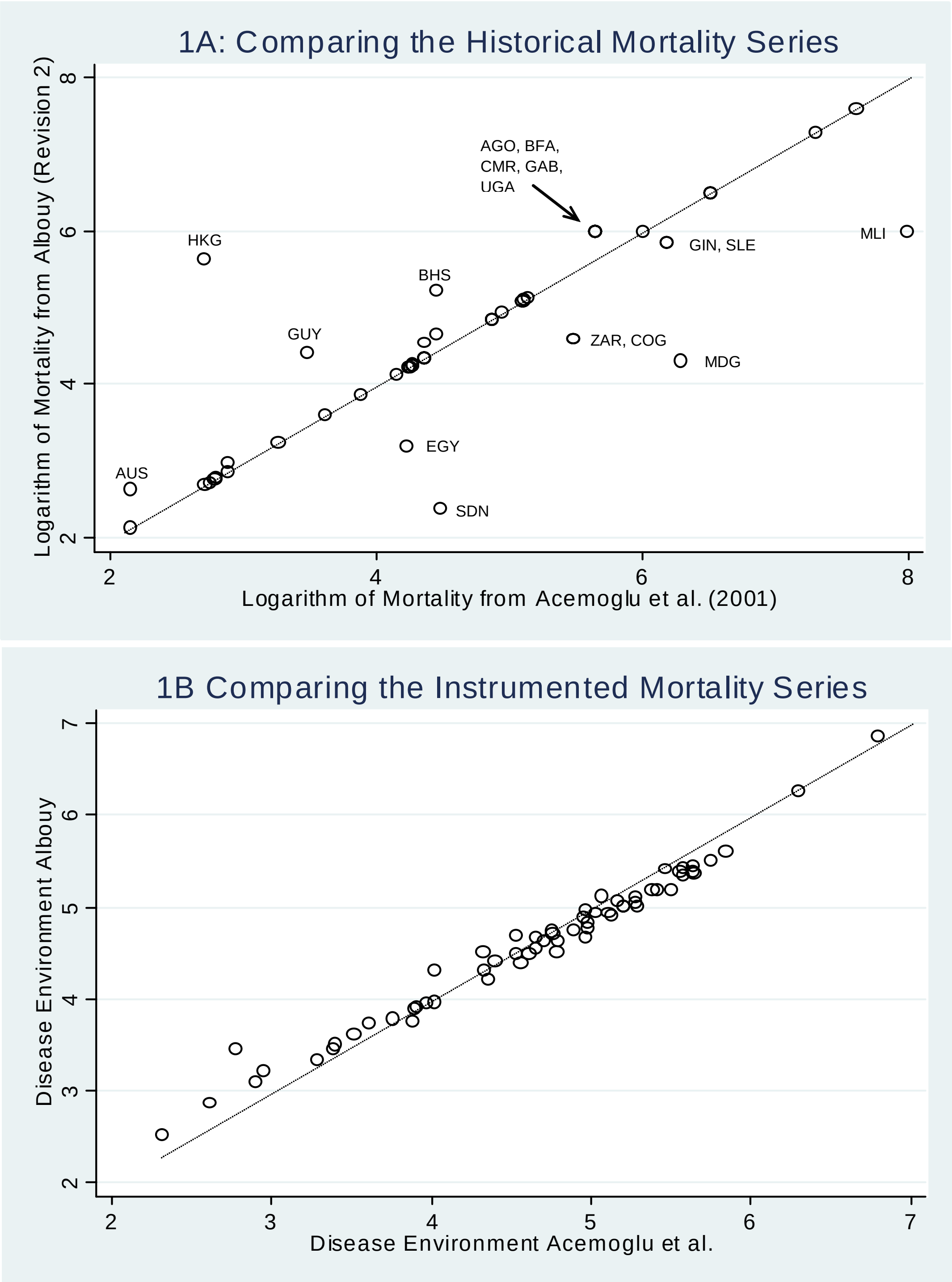
Summary Statistics

Series Name	*Obs.*	*Mean*	*Std. Dev.*	*Min*	*Max*
Ln Mortality AJR	64	4.65	1.25	2.15	7.99
DE AJR	64	4.65	0.91	2.31	6.78
DE AJR Adjusted	64	3.87	0.67	2.06	5.39
Ln Mortality Albouy (Rev. 2)	62	4.61	1.20	2.15	7.60
DE Albouy	62	4.61	0.81	2.52	6.87
DE Albouy Adjusted	62	4.23	0.70	2.41	6.18

Pairwise Correlations

	1.	2.	3.	4.	5.	6.
1. Ln Mortality AJR	1.000					
2. DE AJR	0.726	1.000				
3. DE AJR Adjusted	0.706	0.972	1.000			
4. Ln Mortality Albouy (Rev. 2)	0.861	0.662	0.662	1.000		
5. DE Albouy	0.719	0.988	0.987	0.671	1.000	
6. DE Albouy Adjusted	0.706	0.969	0.994	0.666	0.994	1.000

Figure 1 - Discrepancies Between Historical and Geographic Mortality Rates



Notes: The upper Figure 1A presents a scatter plot of the original mortality rates from Acemoglu et al. versus the revised rates from Albouy, Revision 2. In this figure, since the mortality rates collected from historical sources are in some cases extrapolated to neighboring nations, one hollow circle can represent multiple countries. The lower Figure 1B presents a scatter plot of the predicted disease environment using Acemoglu et al. versus the predicted disease environment using Albouy. In both figures the 45 degree line is displayed. In both figures, World Bank country codes are displayed if the respective two mortality rates (or disease environment estimates in 1B) differ by more than 0.3 log points (35%). The two countries where disease environment (both measures) exceeds 6 are Sierra Leone and Guinea.

5 Mortality and Institutions

In Tables 3.1 to 3.6, I report the main result of this paper. In all specifications, the instrumented mortality rate is a highly significant predictor of institutional quality. This result is the same when using the data of either Acemoglu et al. (Tables 3.2-3.3) or the one of Albouy (Tables 3.4-3.6). This result is also robust to controlling for the population the data was sampled from (Tables 3.2 and 3.6).

More specific, in Section 4, I construct four measures of early disease environment (for each of the two mortality series: Disease Environment and Disease Environment adjusted for population dummies). For each measure, I run 10 specifications using the robustness checks of Acemoglu et al. Of the total of 40 specifications, I find that instrumented mortality is significant at the 1% level in all instances. In addition to showing that the qualitative results of Acemoglu et al. are robust to instrumenting for the mortality data, I also do not find any evidence that their coefficients are biased: the magnitude of their (first stage) OLS coefficient of mortality is actually smaller than the magnitude of my (second stage) IV estimate for the instrumented mortality.

In Columns 1 to 8 in the Tables of this section, I repeat the robustness checks of Acemoglu et al. (Columns 1 to 8 in their Table 4, Panel A on p. 1386).[12] The dependent variable is the average protection from expropriation during 1985 to 1995 from Knack and Keefer (1995). In Columns 9 and 10, I provide an additional robustness check and use an alternative measure of institutional quality, the score for the "Rule of Law" averaged over 1996 to 2004 from Kaufman et al. (2005). In Tables 3.1 to 3.3, I use the settler mortality data collected by Acemoglu et al. and in Tables 3.4 to 3.6 I use the one of Albouy.

In Column 1, I present the simple regression relating mortality (in Table 3.1 and 3.4 the original mortality series; in the other tables the instrumented mortality rates) to institutional outcomes. I add latitude in Column 2 and, following Acemoglu et al., I continue to always including latitude in the following estimations as well, so that every even numbered column also controls for the direct channels of geography captured by the distance from the equator.

In Columns 3 and 4, I exclude the "Neo-Europes" – Australia, Canada, New Zealand and the US – from the estimation. In Columns 5 and 6, I exclude African countries from the estimation. In Columns 7 and 8, I add three continent dummies to the estimation. The latter are for Asia,

[12] Albouy presents similar robustness checks for various data revisions in his Table 2.

for Africa and Other, so that the excluded category is the Americas. Finally, in Columns 9 and 10, I use my alternative measure of institutional quality as dependent variable, the score for rule of law.

For convenience, in Tables 3.1 and 3.4, I repeat the first stage analysis of Acemoglu et al. (3.1) and Albouy (3.4),[13] with the known result: while settler mortality seems to be a robust estimator when using the original data, this is not the case for Albouy's revised series. This discrepancy is not a result of different standard errors, but comes from the coefficients of settler mortality being systematically smaller in magnitude when using the data of Albouy.

The discrepancies between the two series nearly vanish when I instrument for mortality with its geographic projection from Table 3.2 and 3.5 onwards. Because I instrument for mortality with geography, the first stage relation of Acemoglu et al.'s Table 4 is my second stage relation. I report the results for my four different projections of mortality.

In Tables 3.2 and 3.5, I report the results using the unadjusted measure of disease environment, which is constructed in Column 1 (for Table 3.2) and Column 4 (for Table 3.5) of Table 1. It should be noted that the regression in Column 1 of Panel B corresponds to a specification where mortality is instrumented with the four geographic variables used to construct the measure of disease environment. For both variables (DE AJR and DE Albouy) and all robustness tests, the instrumented mortality is a significant predictor of institutions.

The other specifications in this section differ from directly instrumenting with the geographic variables since the projection is accounted for the population that the data was sampled from. In Table 3.3 and 3.6, I report the results using the measure of disease environment that is adjusted for population dummies. The latter variables are constructed from Table 1, Columns 3 and 6 respectively. Again, for both variables and all robustness tests, I confirm the results of Acemoglu et al. and this is also true when working with Albouy's data.

The results of the specifications presented in this section strongly support the results of Acemoglu et al. Albouy argues that when taking into account any one of his criticisms, the relation between mortality and institutions is substantially weakened. In contrast, in this paper, the relation between mortality and institutions is very strong even when all his criticisms are taken into consideration.

[13] Note that throughout the paper, the latitude variable is standardized, which is not the case in Acemoglu et al. Also, some of the results in my Table 3.1 differ slightly (to two decimal places) from the results presented in Table 4 of Acemoglu et al. I use the data from their Table A2, which is rounded, hence explaining this discrepancy.

Table 3.1 - OLS Estimates Using the Data of Acemoglu et al.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	OLS Results for Average Protection Against Expropriation Risk								**for Rule of Law**	
Ln Mortality AJR	**-0.61**	**-0.52**	**-0.4**	**-0.4**	**-1.21**	**-1.14**	**-0.44**	**-0.35**	**-0.47**	**-0.38**
	[0.17]**	**[0.19]***	**[0.17]***	**[0.19]***	**[0.18]****	**[0.18]****	**[0.20]***	**[0.21]**	**[0.10]****	**[0.11]****
Latitude (standardized)		0.27		-0.01		0.13		0.27		0.27
		[0.19]		[0.20]		[0.15]		[0.20]		[0.12]*
Africa dummy							-0.27	-0.26		
							[0.33]	[0.31]		
Asian dummy							0.33	0.47		
							[0.49]	[0.53]		
other cont. dummy							1.23	1.05		
							[0.84]	[0.85]		
p-value of mortality	0.001	0.011	0.025	0.041	0.000	0.000	0.037	0.108	0.000	0.002
p-value of other controls		0.174		0.952		0.392	0.397	0.341		0.024
No. of observations	64	64	60	60	37	37	64	64	64	64
No. of clusters	36	36	33	33	19	19	36	36	36	36
R-squared	0.27	0.3	0.13	0.13	0.47	0.47	0.31	0.33	0.44	0.51

Notes: Columns 1 to 8 of Table 3.1 reproduce Columns 1 to 8 of Table 4, Panel B of Acemoglu et al. (p. 1386). The dependent variable is the 1985 to 1995 average of the score for "Protection Against Expropriation Risk," which is measured from 0 to 10 with a higher score associated with better protection. In Columns 9 and 10, the dependent variable is the 1996 to 2004 average of the score of "Rule of Law." This variable is standardized and again, a higher score is associated with better property rights institutions. The mortality rates are from Acemoglu et al. ; Clustered and robust standard errors reported in brackets; * significant at 5%; ** significant at 1%

Table 3.2 - IV Regressions for Institutional Outcomes Using DE AJR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	Panel B: Second Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
Ln Mortality AJR	**-0.97 [0.19]****	**-0.95 [0.22]****	**-0.7 [0.21]****	**-0.76 [0.23]****	**-1.77 [0.27]****	**-1.87 [0.34]****	**-1.09 [0.27]****	**-1.08 [0.31]****	**-0.68 [0.10]****	**-0.59 [0.15]****
Latitude (standardized)		0.02 [0.18]		-0.16 [0.19]		-0.11 [0.19]		0.02 [0.19]		0.15 [0.13]
Africa dummy							0.52 [0.47]	0.52 [0.48]		
Asian dummy							0.04 [0.43]	0.05 [0.45]		
other cont. dummy							-0.01 [0.77]	-0.01 [0.77]		
p-value of mortality	0.000	0.000	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000
p-value of other controls		0.916		0.388		0.553	0.705	0.816		0.234
	Panel A: First Stage for Ln Mortality AJR									
DE AJR	1 [0.13]**	0.92 [0.15]**	0.95 [0.15]**	0.9 [0.17]**	0.68 [0.15]**	0.62 [0.17]**	0.76 [0.13]**	0.71 [0.13]**	1 [0.13]**	1 [0.13]**
Latitude (standardized)		-0.13 [0.12]		-0.16 [0.15]		-0.1 [0.12]		-0.1 [0.09]		
Africa dummy							0.68 [0.24]**	0.66 [0.24]**		
Asian dummy							-0.64 [0.24]*	-0.67 [0.23]**		
other cont. dummy							-1.01 [0.21]**	-0.93 [0.21]**		
No. of observations	64	64	60	60	37	37	64	64	64	64
No. of clusters	36	36	33	33	19	19	36	36	36	36
R-squared	0.53	0.54	0.41	0.43	0.5	0.52	0.68	0.68	0.53	0.53

Notes: The instrumental variable estimations of Table 3.2 instrument for the estimate of settler mortality from Acemoglu et al. with DE AJR. Panel B presents the relation between instrumented mortality and institutional outcomes. Columns 1 to 8 reproduce Columns 1 to 8 of Table 4, Panel B of Acemoglu et al. (p. 1386). The dependent variable is the 1985 to 1995 average of the score for "Protection Against Expropriation Risk," which is measured from 0 to 10 with a higher score associated with better protection; In Columns 9 and 10, the dependent variable is the 1996 to 2004 average of the score for "Rule of Law." This variable is standardized and again, a higher score is associated with better property rights institutions; Throughout the table, the p-value of mortality and the p-value of other controls is reported; Panel A presents the corresponding first stage relation between mortality and disease environment. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 3.3 - IV Regressions for Institutional Outcomes Using DE AJR Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	Panel B: Second Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
Ln Mortality AJR	**-0.91**	**-0.87**	**-0.61**	**-0.67**	**-1.74**	**-1.87**	**-0.98**	**-0.93**	**-0.64**	**-0.51**
	[0.19]**	**[0.22]****	**[0.19]****	**[0.22]****	**[0.30]****	**[0.39]****	**[0.26]****	**[0.28]****	**[0.11]****	**[0.15]****
Latitude (standardized)		0.07		-0.12		-0.11		0.07		0.19
		[0.18]		[0.19]		[0.18]		[0.18]		[0.13]
Africa dummy							0.39	0.36		
							[0.43]	[0.44]		
Asian dummy							0.09	0.14		
							[0.42]	[0.44]		
other cont. dummy							0.2	0.2		
							[0.77]	[0.76]		
p-value of mortality	0.000	0.000	0.001	0.002	0.000	0.000	0.000	0.001	0.000	0.001
p-value of other controls		0.707		0.513		0.539	0.775	0.820		0.132
	Panel A: First Stage for Ln Mortality AJR									
DE AJR Adjusted	1.33	1.26	1.26	1.21	0.86	0.8	1.06	1.04	1.33	1.26
	[0.19]**	[0.24]**	[0.23]**	[0.27]**	[0.23]**	[0.30]*	[0.18]**	[0.21]**	[0.19]**	[0.24]**
Latitude (standardized)		-0.08		-0.08		-0.07		-0.03		-0.08
		[0.16]		[0.18]		[0.17]		[0.10]		[0.16]
Africa dummy							0.7	0.7		
							[0.22]**	[0.23]**		
Asian dummy							-0.83	-0.83		
							[0.25]**	[0.26]**		
other cont. dummy							-0.95	-0.94		
							[0.23]**	[0.22]**		
No. of observations	64	64	60	60	37	37	64	64	64	64
No. of clusters	36	36	33	33	19	19	36	36	36	36
R-squared	0.5	0.5	0.38	0.39	0.43	0.44	0.69	0.69	0.5	0.5

Notes: The instrumental variable estimations of Table 3.3 instrument for the estimate of settler mortality from Acemoglu et al. with DE AJR Adjusted. Panel B presents the relation between instrumented mortality and institutional outcomes. Columns 1 to 8 reproduce Columns 1 to 8 of Table 4, Panel B of Acemoglu et al. (p. 1386). The dependent variable is the 1985 to 1995 average of the score for "Protection Against Expropriation Risk," which is measured from 0 to 10 with a higher score associated with better protection; In Columns 9 and 10, the dependent variable is the 1996 to 2004 average of the score for "Rule of Law." This variable is standardized and again, a higher score is associated with better property rights institutions; Throughout the table, the p-value of mortality and the p-value of other controls is reported; Panel A presents the corresponding first stage relation between mortality and disease environment. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 3.4 - OLS Estimates Using the Data of Albouy, Revision 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	OLS Results for Average Protection Against Expropriation Risk								**for Rule of Law**	
Ln Mortality Albouy	**-0.42**	**-0.25**	**-0.13**	**-0.1**	**-1.06**	**-0.95**	**-0.13**	**0**	**-0.38**	**-0.25**
	[0.20]*	**[0.23]**	**[0.19]**	**[0.23]**	**[0.27]****	**[0.30]****	**[0.22]**	**[0.22]**	**[0.10]****	**[0.12]**
Latitude (standardized)		0.43		0.12		0.2		0.41		0.35
		[0.24]		[0.25]		[0.17]		[0.23]		[0.14]*
Africa dummy							-0.74	-0.68		
							[0.40]	[0.33]*		
Asian dummy							0.5	0.68		
							[0.51]	[0.55]		
other cont. dummy							1.83	1.53		
							[0.84]*	[0.83]		
p-value of mortality	0.045	0.291	0.508	0.660	0.001	0.005	0.553	0.988	0.001	0.051
p-value of other controls		0.082		0.646		0.276	0.040	0.016		0.017
No. of observations	62	62	58	58	37	37	62	62	62	62
No. of clusters	37	37	33	33	23	23	37	37	37	37
R-squared	0.11	0.18	0.01	0.02	0.35	0.37	0.24	0.3	0.26	0.37

Notes: Columns 1 to 8 of Table 3.4 reproduce Columns 1 to 8 of Table 4, Panel B of Acemoglu et al. (p. 1386) using the data of Albouy (Revision 2). The dependent variable is the 1985 to 1995 average of the score for "Protection Against Expropriation Risk," which is measured from 0 to 10 with a higher score associated with better protection. In Columns 9 and 10, the dependent variable is the 1996 to 2004 average of the score of "Rule of Law." This variable is standardized and again, a higher score is associated with better property rights institutions. Clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 3.5 - IV Regressions for Institutional Outcomes Using DE Albouy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	Panel B: Second Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
Ln Mortality Albouy	**-1.02**	**-1.02**	**-0.71**	**-0.79**	**-1.71**	**-1.79**	**-1.03**	**-1.02**	**-0.73**	**-0.65**
	[0.24]**	**[0.28]****	**[0.24]****	**[0.28]****	**[0.24]****	**[0.35]****	**[0.28]****	**[0.36]****	**[0.13]****	**[0.18]****
Latitude (standardized)		0		-0.17		-0.08		0		0.12
		[0.24]		[0.25]		[0.21]		[0.26]		[0.16]
Africa dummy							0.05	0.05		
							[0.50]	[0.52]		
Asian dummy							0.33	0.33		
							[0.50]	[0.54]		
other cont. dummy							0.18	0.18		
							[0.77]	[0.77]		
p-value of mortality	0.000	0.000	0.003	0.004	0.000	0.000	0.000	0.004	0.000	0.000
p-value of other controls		0.989		0.507		0.688	0.929	0.977		0.427
	Panel A: First Stage for Ln Mortality Albouy									
DE Albouy	1	0.89	0.92	0.86	0.81	0.76	0.84	0.75	1	1
	[0.16]**	[0.17]**	[0.19]**	[0.19]**	[0.16]**	[0.19]**	[0.14]**	[0.16]**	[0.16]**	[0.16]**
Latitude (standardized)		-0.16		-0.15		-0.07		-0.14		
		[0.14]		[0.17]		[0.13]		[0.12]		
Africa dummy							0.3	0.31		
							[0.33]	[0.33]		
Asian dummy							-0.51	-0.52		
							[0.27]	[0.29]		
other cont. dummy							-1.02	-0.92		
							[0.27]**	[0.25]**		
No. of observations	62	62	58	58	37	37	62	62	62	62
No. of clusters	37	37	33	33	23	23	37	37	37	37
R-squared	0.45	0.46	0.33	0.34	0.51	0.51	0.53	0.53	0.45	0.45

Notes: The instrumental variable estimations of Table 3.5 instrument for the estimate of settler mortality from Albouy with DE Albouy. Panel B presents the relation between instrumented mortality and institutional outcomes. Columns 1 to 8 reproduce Columns 1 to 8 of Table 4, Panel B of Acemoglu et al. (p. 1386). The dependent variable is the 1985 to 1995 average of the score for "Protection Against Expropriation Risk," which is measured from 0 to 10 with a higher score associated with better protection; In Columns 9 and 10, the dependent variable is the 1996 to 2004 average of the score for "Rule of Law." This variable is standardized and again, a higher score is associated with better property rights institutions; Throughout the table, the p-value of mortality and the p-value of other controls is reported; Panel A presents the corresponding first stage relation between mortality and disease environment. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 3.6 - IV Regressions for Institutional Outcomes Using DE Albouy Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	Panel B: Second Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
Ln Mortality Albouy	**-0.98**	**-0.94**	**-0.64**	**-0.72**	**-1.67**	**-1.74**	**-0.96**	**-0.93**	**-0.71**	**-0.59**
	[0.24]**	**[0.28]****	**[0.22]****	**[0.27]****	**[0.25]****	**[0.38]****	**[0.27]****	**[0.35]****	**[0.13]****	**[0.18]****
Latitude (standardized)		0.05		-0.14		-0.07		0.04		0.15
		[0.25]		[0.26]		[0.21]		[0.26]		[0.16]
Africa dummy							-0.01	-0.02		
							[0.47]	[0.50]		
Asian dummy							0.34	0.37		
							[0.49]	[0.53]		
other cont. dummy							0.3	0.31		
							[0.77]	[0.76]		
p-value of mortality	0.000	0.001	0.003	0.008	0.000	0.000	0.000	0.007	0.000	0.001
p-value of other controls		0.850		0.587		0.742	0.907	0.966		0.325
	Panel A: First Stage for Ln Mortality Albouy									
DE Albouy Adjusted	1.14	1.03	1.05	0.99	0.9	0.85	0.97	0.89	1.14	1.14
	[0.19]**	[0.20]**	[0.22]**	[0.22]**	[0.19]**	[0.24]**	[0.17]**	[0.20]**	[0.19]**	[0.19]**
Latitude (standardized)		-0.13		-0.11		-0.05		-0.1		
		[0.14]		[0.17]		[0.14]		[0.12]		
Africa dummy							0.33	0.33		
							[0.32]	[0.32]		
Asian dummy							-0.58	-0.59		
							[0.29]*	[0.30]		
other cont. dummy							-0.99	-0.92		
							[0.30]**	[0.28]**		
No. of observations	62	62	58	58	37	37	62	62	62	62
No. of clusters	37	37	33	33	23	23	37	37	37	37
R-squared	0.44	0.45	0.33	0.33	0.47	0.48	0.53	0.54	0.44	0.44

Notes: The instrumental variable estimations of Table 3.6 instrument for the estimate of settler mortality from Albouy with DE Albouy Adjusted. Panel B presents the relation between instrumented mortality and institutional outcomes. Columns 1 to 8 reproduce Columns 1 to 8 of Table 4, Panel B of Acemoglu et al. (p. 1386). The dependent variable is the 1985 to 1995 average of the score for "Protection Against Expropriation Risk," which is measured from 0 to 10 with a higher score associated with better protection; In Columns 9 and 10, the dependent variable is the 1996 to 2004 average of the score for "Rule of Law." This variable is standardized and again, a higher score is associated with better property rights institutions; Throughout the table, the p-value of mortality and the p-value of other controls is reported; Panel A presents the corresponding first stage relation between mortality and disease environment; All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

6 Excludability of the Instrument - A Falsification Exercise

Geography has been argued to have large direct effects on income and development (see for example Bloom and Sachs (1998), Gallup et al. (1998) and Diamond (1997)). To the same extent as in the original article of Acemoglu et al., it could thus be the case that "[...] mortality rates of settlers could be correlated with the current disease environment, which may have a direct effect on economic performance" (Acemoglu et al. (2001), p.1371).

To address the validity of their identifying assumption that settler mortality affects development only indirectly through institutions, Acemoglu et al. provide additional geographic controls and further instruments for institutions.[14] In addition to repeating the same checks, in this paper, I am able to offer an additional test of the direct effects early disease environment may have had on development.

In this section, I show that the measures of the geographic potential for mortality, which are strong determinants of institutions in the sample of former colonies, have no explanatory power in a sample of countries that have not been colonized.[15] If the relationship between (instrumented) mortality and institutional outcomes indeed reflects the direct effect geography has on development, I should find the same relation between early disease environment and institutional outcomes irrespective of whether a country has been colonized or not. On the other side, if Acemoglu et al.'s theory is valid, disease environment should affect development only in the sample of former colonies, yet not in the rest of the sample.

I construct the measures of disease environment for a sample of 60 non-colonies. The countries of this section are listed in Appendix 11.2 and have been selected on the basis of the following criteria. They never have been colonized, never had the status of a protectorate, and they were not subject to heavy slave trade. There are 36 countries that fullfill these requirements and have and available score for average protection of expropriation and 60 countries that fullfil these criterias and have an available score for rule of law.

For these 60 countries,[16] I construct the four measures of disease environment using the coefficients of Table 1 (Column 1 for DE AJR, Column 3 for DE AJR Adjusted, Column 5 for DE

[14] See also the discussion of the direct effects of disease environment on comparative development in Acemoglu et al. (2003).

[15] For a discussion of falsification exercises see Angrist and Krueger (1999)

[16] This sample includes some very small European nations, which might not be representative. Exclusion of Andora, Luxemburg, Monaco, and San Marino does not lead to a negative and significant effect of disease environment for any of the specifications of Tables 4.1 to 4.4.

Albouy, and Column 6 for DE Albouy Adjusted) and the geographic information from Parker (1997), i.e. I simply predict the model relating geography and mortality out of sample.

Figures 2 and 3 display the basic result of this section. In Figure 2A, I present a scatter plot between the predicted measure of disease environment (DE AJR) and average protection from expropriation in the 36 countries of the non-colony sample. Figure 2B repeats this for the sample of colonies in Acemoglu et al. While there is a clear downward relationship in the group of former colonies, this is not the case in the rest of the sample. A simple regression confirms this visual impression. When I regress protection from expropriation on DE AJR in the sample of former colonies, I find a significantly negative relation, and the corresponding R-squared is equal to 0.36. On the contrary, when I repeat the same exercise and regress the protection from expropriation on DE AJR (predicted out of sample) in the 36 countries that have not been colonized, the coefficient is not significant at all, and the R-squared is substantially smaller than in the previous regression (.06). The variable that is an extremely strong predictor in the sample of former colonies has no power in the sample of non-colonized countries.

In Figure 3, I repeat this comparison of the relation between disease environment and institutional outcomes using the score for rule of law. Again, there is a strong negative relation between these two variables in the group of former colonies (3B), yet none in the group of non-colonies (3A).

Quantifying this visual impression, I present the relation between my four measures of early disease environment and institutional outcomes in the group of non-colonies in tables 4.1 to 4.4. Again, the geographic potential of disease is predicted out of sample. The four measures of early disease environment correspond to Columns 1, 3, 4, and 6 of Table 1. Since the sample size is substantially larger, I first focus on the score for the rule of law as the dependent variable.

In the simple regression of disease environment on the rule to law (Column 1 in Tables 4.1 to 4.4), the coefficients of the respective measures of disease environment lie between -0.21 and -0.29 and are always insignificant. This is substantially smaller than the same relation in the group of former colonies, where the coefficients lie between -0.68 and -0.85 and are always significant.[17] Moreover, when I also control for latitude in Column 2, the coefficients of disease environment are actually positive in all instances (and not significant). Again, this contrasts sharply with

[17] The respective coefficients are reported in Panel A of Tables 5.1 to 5.4, Column 9 (Column 9 of Table 5.1 corresponding to Column 1 of Table 4.1 etc.). The same information is also reported in Column 9 of Tables 3.2, 3.3, 3.5, and 3.6, where the total effect of disease environment on institutions is equal to the product of the first stage coefficient for disease environment on mortality and the second stage coefficient of instrumented mortality on rule of law.

the relation between disease environment and institutions in the sample of former colonies (see Column 10 in Panel A of Tables 5.1 to 5.4).

I next address the potential worry that the large number of countries that were under Russian influence are solely responsible for this result and include a Warsaw Pact dummy to the estimation in Column 3. While this dummy is significantly negative, this does not affect the coefficients of the measures of early disease environment. Also, it should be noted that the selection of countries into the Warsaw Pact was probably not orthogonal to institutional quality, so that the negative coefficient of the Warsaw pact dummy may reflect either the negative impact of communism, or the simple fact that countries with bad institutions in the early 19th century were more likely to come under Russian influence.

In Column 4, I address another potential worry that a few Asian countries (for example Japan) are driving the results in the sample of non-colonies. I thus include a dummy for Asian countries and again find that this is not the case: in Column 4 of Tables 5.1 to 5.4, the coefficients for early disease environment are again positive and insignificant. In the sample of this section, there are only European and Asian countries and I could have therefore also included a dummy for European countries with the same results as in Column 4, except for the sign of the dummy coefficient. Since the Asian dummy is equal to one for many former Soviet countries, in Column 5, I include both the Warsaw Pact dummy and the Asian dummy to show that also when controlling for both these factors, disease environment is not a determinant of institutions in former colonies.

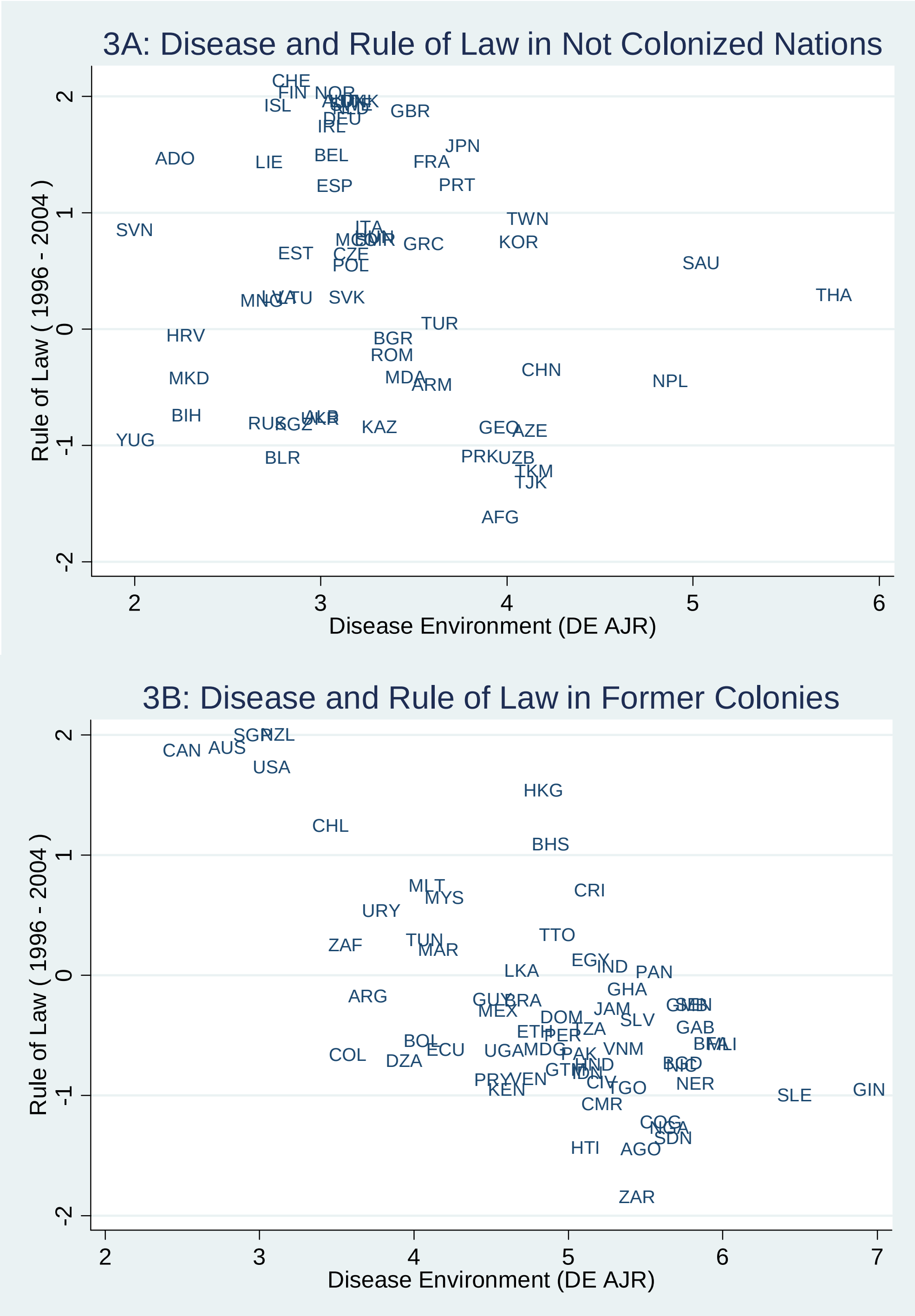
In Columns 6 to 10, I repeat the same exercise using the 1985 to 1995 average score of protection from expropriation as dependent variable. This measure is not available for many transition economies and there are thus only 36 countries in this sample. The findings are very comparable to using the rule of law as dependent variable: there is not a single case in which disease environment is a significantly negative determinant of institutions in the group of non-colonies, and the coefficients are substantially smaller than in the sample of former colonies (compare Table 4.1 to 4.2 to the Panel A of Tables 5.1 to 5.4, Columns 1 and 2). Furthermore, when I control for latitude in Column 7, the coefficients of disease environment are consistently positive and even mildly significant in one case (Table 4.2). Also when including the Warsaw Pact dummy (Column 8), a dummy for Asian countries (Column 9) or both (Column 10), the findings are similar to using the score for rule of law as a dependent variable.

In stark contrast to the results in the sample of former colonies of Acemoglu et al., there is no relation between disease environment and institutional outcomes in the sample of non-colonies. I conclude that my geographic instruments for settler mortality are excludable because they only influence institutional development in former colonies, which can only be explained by the early institution building effect mortality rates had in the process of colonization.

Figure 2 - The Differential Effect Disease Environment had on Protection from Expropriation

Notes: Figure 2 presents the relation between Early Disease Environment (DE AJR) and the 1985 to 1995 average of the score for Protection Against Expropriation Risk. The upper Figure 2A presents a scatter plot of these variables in 36 countries that have not been colonized. The lower Figure 2B presents the same relation for the sample of former colonies of Acemoglu et al.

Figure 3: The Differential Effect Disease Environment had on Rule of Law



Notes: Figure 3 presents the relation between Early Disease Environment (DE AJR) and the 1996 to 2004 average of the score for Rule of Law. The upper Figure 3A presents a scatter plot of these variables in 60 countries that were not colonized, have not been under the status of a protectorate and were not subject to heavy slave trade. The lower Figure 3B presents the same relation for the sample of Acemoglu et al. (2001).

Table 4.1 - DE AJR and Institutions in not Colonized Nations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sample of not colonized countries									
	Dependent Variable is: the Rule of Law					**Average Protection from Expropriation**				
DE AJR	-0.29 [0.18]	0.24 [0.26]	-0.28 [0.18]	0.24 [0.22]	0.2 [0.20]	-0.43 [0.33]	0.58 [0.48]	-0.49 [0.37]	0.07 [0.36]	0.02 [0.41]
Latitude (standardized)		0.06 [0.02]**					0.08 [0.03]**			
Warsaw Pact dummy			-1.27 [0.21]**		-1.19 [0.20]**			-0.55 [0.43]		-0.67 [0.43]
Asia dummy				-1.32 [0.33]**	-1.19 [0.26]**				-1.28 [0.52]*	-1.35 [0.56]*
P-value DE AJR	0.116	0.357	0.132	0.269	0.321	0.21	0.241	0.197	0.852	0.967
No. of observations	60	60	60	60	60	36	36	36	36	36
R-squared	0.03	0.15	0.35	0.24	0.51	0.06	0.25	0.09	0.21	0.26

Notes: Table 4.1 presents the relation between the disease environment (DE AJR) and institutional outcomes in a sample of countries that were not colonized, have not been under the status of a protectorate, and were not subject to heavy slave trade. DE AJR is constructed using the coefficients from Column 1 of Table 1 and the respective geographic information from Parker (1997). The Warsaw Pact dummy is one if a country was a member of the Warsaw Pact. The sample includes only European and Asian countries, thus the excluded category for the Asia dummy is Europe. Robust standard errors reported in brackets; * significant at 5%; ** significant at 1%

Table 4.2 - DE AJR Adjusted and Institutions in not Colonized Nations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sample of not colonized countries									
	Dependent Variable is: the Rule of Law					**Average Protection from Expropriation**				
DE AJR Adjusted	-0.21 [0.22]	0.62 [0.32]	-0.24 [0.22]	0.45 [0.26]	0.34 [0.24]	-0.36 [0.39]	1.16 [0.46]*	-0.44 [0.44]	0.25 [0.41]	0.18 [0.47]
Latitude (standardized)		0.07 [0.02]**					0.1 [0.02]**			
Warsaw Pact dummy			-1.28 [0.22]**		-1.17 [0.21]**			-0.52 [0.43]		-0.64 [0.44]
Asia dummy				-1.39 [0.31]**	-1.23 [0.25]**				-1.38 [0.49]**	-1.44 [0.54]*
P-value DE AJR Adj.	0.328	0.059	0.272	0.089	0.168	0.367	0.017	0.323	0.543	0.708
No. of observations	60	60	60	60	60	36	36	36	36	36
R-squared	0.01	0.19	0.33	0.26	0.52	0.03	0.31	0.06	0.22	0.27

Notes: Table 4.2 presents the relation between the disease environment (DE AJR Adjusted) and institutional outcomes in a sample of countries that were not colonized, have not been under the status of a protectorate, and were not subject to heavy slave trade. DE AJR Adjusted is constructed using the coefficients from Column 3 of Table 1 and the respective geographic information from Parker (1997). The Warsaw Pact dummy is one if a country was a member of the Warsaw Pact. The sample includes only European and Asian countries, thus the excluded category for the Asia dummy is Europe. Robust standard errors reported in brackets; * significant at 5%; ** significant at 1%

Table 4.3 - DE Albouy and Institutions in not Colonized Nations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sample of not colonized countries									
	Dependent Variable is: the Rule of Law					**Average Protection from Expropriation**				
DE Albouy	-0.27	0.55	-0.32	0.33	0.2	-0.47	0.92	-0.55	0.08	0.01
	[0.20]	[0.31]	[0.20]	[0.23]	[0.22]	[0.34]	[0.51]	[0.39]	[0.35]	[0.41]
Latitude (standardized)		0.07					0.09			
		[0.02]**					[0.03]**			
Warsaw Pact dummy			-1.3		-1.18			-0.56		-0.67
			[0.22]**		[0.21]**			[0.42]		[0.43]
Asia dummy				-1.35	-1.16				-1.28	-1.34
				[0.30]**	[0.25]**				[0.49]*	[0.53]*
P-value DE Albouy	0.182	0.082	0.113	0.151	0.365	0.181	0.079	0.165	0.831	0.986
No. of observations	60	60	60	60	60	36	36	36	36	36
R-squared	0.03	0.15	0.35	0.24	0.51	0.06	0.25	0.09	0.21	0.26

Notes: Table 4.3 presents the relation between the disease environment (DE Albouy) and institutional outcomes in a sample of countries that were not colonized, have not been under the status of a protectorate, and were not subject to heavy slave trade. DE Albouy is constructed using the coefficients from Column 4 of Table 1 and the respective geographic information from Parker (1997). The Warsaw Pact dummy is one if a country was a member of the Warsaw Pact. The sample includes only European and Asian countries, thus the excluded category for the Asia dummy is Europe. Robust standard errors reported in brackets; * significant at 5%; ** significant at 1%

Table 4.4 - DE Albouy Adjusted and Institutions in not Colonized Nations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sample of not colonized countries									
	Dependent Variable is: the Rule of Law					**Average Protection from Expropriation**				
DE AJR	-0.29	0.24	-0.28	0.24	0.2	-0.43	0.58	-0.49	0.07	0.02
	[0.18]	[0.26]	[0.18]	[0.22]	[0.20]	[0.33]	[0.48]	[0.37]	[0.36]	[0.41]
Latitude (standardized)		0.06					0.08			
		[0.02]**					[0.03]**			
Warsaw Pact dummy			-1.27		-1.19			-0.55		-0.67
			[0.21]**		[0.20]**			[0.43]		[0.43]
Asia dummy				-1.32	-1.19				-1.28	-1.35
				[0.33]**	[0.26]**				[0.52]*	[0.56]*
P-value DE Albouy Adj.	0.309	0.036	0.198	0.083	0.229	0.277	0.022	0.245	0.614	0.789
No. of observations	60	60	60	60	60	36	36	36	36	36
R-squared	0.03	0.15	0.35	0.24	0.51	0.06	0.25	0.09	0.21	0.26

Notes: Table 4.4 presents the relation between the disease environment (DE Albouy Adjusted) and institutional outcomes in a sample of countries that were not colonized, have not been under the status of a protectorate, and were not subject to heavy slave trade. DE Albouy Adjusted is constructed using the coefficients from Column 6 of Table 1 and the respective geographic information from Parker (1997). The Warsaw Pact dummy is one if a country was a member of the Warsaw Pact. The sample includes only European and Asian countries, thus the excluded category for the Asia dummy is Europe. Robust standard errors reported in brackets; * significant at 5%; ** significant at 1%

7 Income and Institutions

The previous sections establish that the four measures of disease environment are relevant instruments for mortality. In this section, I disentangle the relationship between institutions and income. Overall, my results show that (instrumented) institutions are highly significant determinants of international income differentials. In all specifications, the first stage relation between disease environment and institutions is significant at the 5% level, and in 34 of 40 specifications, it is also significant at the 1% level or higher. The second stage coefficient of instrumented institutions is highly significant in all specifications.

In the regressions presented in the tables of this section (Tables 5.1 to 5.4), I directly instrument for institutions with the constructed measures of early disease environment. When Acemoglu et al. use mortality as an instrument for institutions, they implicitly test the joint hypothesis that mortality rates influenced the size of European settlements and that European settlements influenced early institutions. I test the joint hypothesis that geography influenced mortality rates, that mortality rates influenced the size of European settlements, and that European settlements influenced early institutions.

Tables 5.1 to 5.4 repeat the specifications of Table 3.1 to 3.6, which are also the one's of Table 4 in Acemoglu et al. In Column 1, I present the simple IV estimation using disease environment to instrument for institutions. I add latitude in Column 2 and I continue to always including latitude in the following estimations as well, so that every even numbered column also controls for the direct channels of geography captured by the distance from the equator. In Columns 3 and 4, I exclude the "Neo-Europes" from the estimation. In Columns 5 and 6, I exclude African countries from the estimation. In Columns 7 and 8, I add three continent dummies to the estimation (Asia, for Africa and Other). Finally, in Columns 9 and 10, I use the alternative measure of institutional quality, the score for rule of law, as dependent the variable.

In Table 5.1, I present the results when instrumenting for institutions with the unadjusted measure of disease environment, DE AJR. In all specifications, both the first and second stage results relating disease environment to institutions and institutions to income differences are significant at the 5% level. The same is true when using DE AJR Adjusted as an instrument (Table 5.2), when using DE Albouy (Table 5.3) or De Albouy Adjusted (Table 5.4).

The 6 out of 40 instances where I find lower significance of the first stage relation are in Column 10 of Table 5.2, in Column 4 in Table 5.2, 5.3. and 5.4 and Column 8 of Table 5.3 and 5.4. In

Column 4 in Tables 5.2, 5.3 and 5.4, I have controlled for latitude and excluded the Neo-Europes. In Column 8 of Tables 5.3 and 5.4 I have also controlled for latitude and included continent dummies. In Column 10 of Table 5.2, I have only controlled for latitude, and the regression uses the score for rule of law as dependent variable. The second stage coefficient of institutions is significant in every one of the 40 estimated specifications.

What about the associated importance of institutions for prosperity? Compare, for example, the results of Acemoglu et al.'s most basic specification (their Table 4, Column 1) to the results obtained here. In Table 4 of Acemoglu et al., a one percent higher mortality is associated with a 0.61 percentage points lower score of rule of law (their Panel B). This in turn is associated with a 0.57 (=0.94*0.61) percent lower level of income. In the corresponding specification (in my Table 5.1, Column 1) of this paper, a one percentage point lower score of disease environment is associated with a 0.97 percentage point decrease of the score for protection from expropriation and a 0.72 percent income difference.

Incorporating that the variability of DE AJR (standard deviation equal to 0.91) is smaller than the one of the logarithm of mortality of Acemoglu et al. (standard deviation equal to 1.25), I find that in these two specifications a one standard deviation income is associated with a 0.713 (Acemoglu et al.) and 0.655 (my Table 5.1, Column 1) difference in the logarithm of income respectively. Moreover, when again evaluating the quantitative impact of institutions, but using the measure of disease environment obtained from Albouy (DE Albouy) in Column 1 of Table 5.3, again the point estimate is very similar. One standard deviation difference in DE Albouy is associated with a 0.735 (1.02*0.89*0.81) higher log income. Thus, I conclude that my results do not only confirm the colonial origins theory qualitatively, but they also lead to point estimates that are very similar to the ones of Acemoglu et al. and this is even true when using the data of Albouy.

Table 5.1 - IV Regressions of Log GDP per Capita for DE AJR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	Panel B: Second Stage Results for Log GDP per Capita									
Protection against expro-priation risk, 1985-1995	**0.85 [0.13]****	**0.86 [0.15]****	**1.13 [0.27]****	**1.06 [0.24]****	**0.66 [0.10]****	**0.68 [0.11]****	**0.74 [0.13]****	**0.75 [0.15]****		
Rule of law									**1.22 [0.15]****	**1.38 [0.29]****
Latitude (standardized)		-0.01 [0.12]		0.15 [0.15]		-0.05 [0.08]		-0.02 [0.11]		-0.2 [0.18]
Africa dummy							-0.66 [0.21]**	-0.66 [0.21]**		
Asian dummy							-0.8 [0.27]**	-0.81 [0.29]**		
Other continent dummy							-0.43 [0.52]	-0.43 [0.52]		
	Panel A: First Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
DE AJR	-0.97 [0.20]**	-0.88 [0.23]**	-0.67 [0.21]**	-0.68 [0.23]**	-1.2 [0.18]**	-1.15 [0.21]**	-0.82 [0.20]**	-0.76 [0.22]**	-0.68 [0.12]**	-0.55 [0.16]**
Latitude (standardized)		0.15 [0.17]		-0.04 [0.19]		0.08 [0.15]		0.13 [0.15]		0.23 [0.13]
Africa dummy							-0.21 [0.27]	-0.2 [0.27]		
Asian dummy							0.74 [0.38]	0.78 [0.40]		
Other continent dummy							1.08 [0.58]	0.98 [0.60]		
Observations	64	64	60	60	37	37	64	64	64	64
Clusters	36	36	33	33	19	19	36	36	36	36
R-squared (1st stage)	0.36	0.36	0.17	0.17	0.5	0.5	0.42	0.42	0.47	0.52

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 5.2 - IV Regressions of Log GDP per Capita for DE AJR Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
					Panel B: Second Stage Results for Log GDP per Capita					
Protection against expropriation risk, 1985-1995	**0.87 [0.15]****	**0.89 [0.18]****	**1.22 [0.29]****	**1.12 [0.29]****	**0.68 [0.11]****	**0.72 [0.12]****	**0.74 [0.14]****	**0.75 [0.16]****		
Rule of law									**1.24 [0.17]****	**1.5 [0.38]****
Latitude (standardized)		-0.03 [0.14]		0.14 [0.17]		-0.07 [0.09]		-0.02 [0.11]		-0.26 [0.23]
Africa dummy							-0.66 [0.21]**	-0.66 [0.22]**		
Asian dummy							-0.79 [0.27]**	-0.81 [0.29]**		
Other continent dummy							-0.43 [0.53]	-0.43 [0.54]		
		Panel A: First Stage for Average Protection Against Expropriation Risk							**for Rule of Law**	
DE AJR Adjusted	-1.21 [0.27]**	-1.09 [0.34]**	-0.77 [0.23]**	-0.81 [0.31]*	-1.51 [0.30]**	-1.49 [0.45]**	-1.04 [0.28]**	-0.96 [0.32]**	-0.85 [0.17]**	-0.65 [0.24]*
Latitude (standardized)		0.13 [0.21]		-0.07 [0.23]		0.01 [0.26]		0.09 [0.18]		0.23 [0.16]
Africa dummy							-0.3 [0.29]	-0.29 [0.28]		
Asian dummy							0.9 [0.44]*	0.91 [0.46]		
Other continent dummy							1.13 [0.67]	1.07 [0.68]		
Observations	64	64	60	60	37	37	64	64	64	64
Clusters	36	36	33	33	19	19	36	36	36	36
R-squared (1st stage)	0.3	0.31	0.12	0.12	0.42	0.42	0.39	0.4	0.4	0.44

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 5.3 - IV Regressions of Log GDP per Capita for DE Albouy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
					Panel B: Second Stage Results for Log GDP per Capita					
Protection against expro-priation risk, 1985-1995	**0.89 [0.15]****	**0.96 [0.19]****	**1.27 [0.32]****	**1.22 [0.32]****	**0.67 [0.10]****	**0.7 [0.11]****	**0.79 [0.16]****	**0.85 [0.20]****		
Rule of law									**1.24 [0.14]****	**1.52 [0.33]****
Latitude (standardized)		-0.11 [0.14]		0.07 [0.17]		-0.06 [0.08]		-0.09 [0.12]		-0.3 [0.21]
Africa dummy							-0.54 [0.21]*	-0.52 [0.24]*		
Asian dummy							-0.82 [0.28]**	-0.89 [0.32]**		
Other continent dummy							-0.53 [0.56]	-0.55 [0.60]		
	Panel A: First Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
DE Albouy	-1.02 [0.25]**	-0.91 [0.30]**	-0.65 [0.23]**	-0.68 [0.27]*	-1.38 [0.23]**	-1.35 [0.31]**	-0.86 [0.27]**	-0.77 [0.30]*	-0.73 [0.14]**	-0.58 [0.19]**
Latitude (standardized)		0.16 [0.19]		-0.05 [0.21]		0.03 [0.19]		0.14 [0.19]		0.23 [0.14]
Africa dummy							-0.26 [0.31]	-0.26 [0.31]		
Asian dummy							0.85 [0.39]*	0.87 [0.42]*		
Other continent dummy							1.23 [0.61]	1.13 [0.62]		
Observations	62	62	58	58	37	37	62	62	62	62
Clusters	37	37	33	33	23	23	37	37	37	37
R-squared (1st stage)	0.31	0.31	0.13	0.13	0.46	0.46	0.39	0.39	0.42	0.46

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 5.4 - IV Regressions of Log GDP per Capita for DE Albouy Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*w/o Neo Europe*		*w/o Africa*		*Full Sample*		*Full Sample*	
	Panel B: Second Stage Results for Log GDP per Capita									
Protection against expro-priation risk, 1985-1995	**0.91 [0.16]****	**1.01 [0.22]****	**1.34 [0.35]****	**1.29 [0.36]****	**0.68 [0.10]****	**0.73 [0.11]****	**0.8 [0.17]****	**0.88 [0.22]****		
Rule of law									**1.26 [0.15]****	**1.61 [0.39]****
Latitude (standardized)		-0.14 [0.16]		0.06 [0.19]		-0.07 [0.08]		-0.1 [0.13]		-0.34 [0.24]
Africa dummy							-0.53 [0.22]*	-0.5 [0.25]*		
Asian dummy							-0.83 [0.28]**	-0.91 [0.33]**		
Other continent dummy							-0.56 [0.57]	-0.6 [0.64]		
	Panel A: First Stage for Average Protection Against Expropriation Risk								**for Rule of Law**	
DE Albouy Adjusted	-1.12 [0.28]**	-0.97 [0.34]**	-0.68 [0.23]**	-0.71 [0.30]*	-1.5 [0.29]**	-1.48 [0.43]**	-0.93 [0.31]**	-0.83 [0.35]*	-0.81 [0.16]**	-0.61 [0.22]**
Latitude (standardized)		0.17 [0.21]		-0.06 [0.23]		0.02 [0.24]		0.14 [0.20]		0.23 [0.15]
Africa dummy							-0.32 [0.31]	-0.32 [0.31]		
Asian dummy							0.9 [0.42]*	0.91 [0.45]*		
Other continent dummy							1.25 [0.66]	1.16 [0.66]		
Observations	62	62	58	58	37	37	62	62	62	62
Clusters	37	37	33	33	23	23	37	37	37	37
R-squared (1st stage)	0.28	0.28	0.1	0.1	0.42	0.42	0.37	0.38	0.39	0.43

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

8 Robustness Tests

In the 8 tables of this section, I provide additional evidence that the results presented so far are robust to the inclusion of further geographic, sociological and economic controls. In all the 80 additional robustness tests, the instruments for institutions as well as instrumented institutions themselves are significant. In Tables 6.1 to 6.4, I present the robustness tests of Table 5 of Acemoglu et al. Again, this is done for my 4 measures of disease environment constructed from Table 1. In Tables 7.1 to 7.4, I present further robustness tests adding geographic and socioeconomic controls.

In Tables 6.1 to 6.4, I first include dummies for British and French colonies from La Porta et al. (1999) and (1998). Again, to control for the direct effects of geography, this is done with (Column 1) and without (Column 2) adding latitude to the estimation. I find that the colony dummies significantly influence on development, yet that they influence institutions (first stage) and income directly (second stage) in opposing ways. To check that the colonial origins theory of Acemoglu et. al. is also valid within the groups of former British colonies, Columns 3 and 4 restrict the sample to the 23 former British colonies. In the next two specifications, I include a French legal origin dummy, which is again significantly negative in the first and significantly positive in the second stage. Combining the two effects, French legal origins are on average associated with a very small effect on income. The same finding of two counteracting effects in the first and the second stage is true when testing for religion (Column 7 and 8 that add the percentage of the population of Catholic and the percentage of the population of Muslim believes) or including a French colony dummy and a French legal origin dummy at the same time (Column 9 and 10). However, regardless of which measure I control for, the first stage coefficient of disease environment is significant and instrumented institutions are a significant determinant of economic prosperity.

In Tables 7.1 to 7.4, I include further geographic and sociological controls. I first include the Geographic potential for mortality, Malaria Ecology, from Kiszewski et al. (2004) in Column 1 and 2. Higher potential for malaria is associated with both worse institutional outcomes and lower income directly, but neither effect is significant. Combining first and second stage effects and incorporating the fact that within the sample of Acemoglu et al., the standard deviation of Malaria Ecology is 8.43, a one standard deviation difference in this measure leads to a difference in log income of 0.2. A dummy for landlocked countries (Column 3 and 4) is significantly detrimental for institutional outcomes. Incorporating also the insignificant and positive coefficient in the second stage, being landlocked is associated (in Table 7.1, Column 3) with a 0.62 log points lower in income per capita, which is quite substantial. Next, in Column 5 and 6, I add an oil dummy that equals 1 if proven oil reserves are larger than 500,000 Barrels per capita. This is the case in Gabon, Mexico, and Venezuela. (The large oil reserves of Canada are not accounted for in Parker (1997)). Not surprising, oil rich countries are on average richer. More surprisingly, in the sample examined here, this effect seems to work through institutions rather than directly.

In the last four columns of Tables 7.1 to 7.4, I control for the three measures of Ethnic (Column 6), Linguistic (7), and Religious (8) Fractionalization from Alesina et al. (2003). Following the pioneering work of Mauro (1995), low scores for these measures (low scores imply homogenous countries) are thought to ease collaboration between groups and individuals in a country, and thereby improve institutional outcomes. I do find some support for this view only when I include all three measures of fractionalization at the same time (Column 10), and I also do find that only ethnic fractionalization has a significant influence on institutions. I also find in both Column 8 and 10 that linguistic fractionalization has negative direct effects on income levels.

Table 6.1 - IV Regressions of Log GDP per Capita with Additional Controls for DE AJR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*British Colonies*				*Full Sample*			
					Panel B: Second Stage Results for Log GDP per Capita					
Protection against expropriation risk, 1985-1995	**0.91 [0.14]****	**0.91 [0.17]****	**0.85 [0.14]****	**0.92 [0.20]****	**0.92 [0.13]****	**0.94 [0.16]****	**0.79 [0.09]****	**0.75 [0.12]****	**0.93 [0.14]****	**0.95 [0.18]****
Latitude (standardized)		0 [0.11]		-0.13 [0.14]		-0.02 [0.11]		0.06 [0.12]		-0.03 [0.12]
Bristish colony dummy	-0.55 [0.29]	-0.55 [0.30]								
French colony dummy	0.06 [0.32]	0.06 [0.34]							0.04 [0.26]	0.06 [0.30]
French legal origin dummy					0.76 [0.28]**	0.77 [0.30]*			0.76 [0.26]**	0.76 [0.29]**
% of population Muslim							0.003 [0.004]	0.002 [0.004]		
% of population Catholic							0.013 [0.003]**	0.012 [0.004]**		
				Panel A: First Stage for Average Protection Against Expropriation Risk						
DE AJR	-0.86 [0.17]**	-0.76 [0.20]**	-1.08 [0.21]**	-0.93 [0.30]**	-0.9 [0.17]**	-0.82 [0.20]**	-0.96 [0.17]**	-0.84 [0.21]**	-0.86 [0.18]**	-0.75 [0.21]**
Latitude (standardized)		0.16 [0.15]		0.26 [0.19]		0.15 [0.15]		0.18 [0.16]		0.19 [0.16]
Bristish colony dummy	0.7 [0.30]*	0.67 [0.29]*								
French colony dummy	-0.51 [0.35]	-0.58 [0.37]							-0.6 [0.31]	-0.67 [0.32]*
French legal origin dummy					-0.71 [0.32]*	-0.71 [0.31]*			-0.57 [0.32]	-0.56 [0.31]
% of population Muslim							-0.007 [0.005]	-0.009 [0.006]		
% of population Catholic							-0.011 [0.004]*	-0.012 [0.004]**		
Observations	64	64	23	23	64	64	64	64	64	64
Clusters	36	36	19	19	36	36	36	36	36	36
R-squared (1st stage)	0.45	0.46	0.47	0.5	0.41	0.42	0.41	0.42	0.43	0.45

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. The omitted group in Columns 1 and 2 is all countries that were neither French nor British colonies. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 6.2 - IV Regressions of Log GDP per Capita with Additional Controls for DE AJR Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*British Colonies*				*Full Sample*			
	Panel B: Second Stage Results for Log GDP per Capita									
Protection against expro-priation risk, 1985-1995	**0.91 [0.15]****	**0.91 [0.18]****	**0.82 [0.13]****	**0.9 [0.20]****	**0.91 [0.13]****	**0.92 [0.17]****	**0.78 [0.09]****	**0.7 [0.14]****	**0.92 [0.14]****	**0.93 [0.19]****
Latitude (standardized)		0 [0.12]		-0.12 [0.14]		-0.01 [0.11]		0.09 [0.13]		-0.02 [0.12]
Bristish colony dummy	-0.55 [0.28]	-0.55 [0.31]								
French colony dummy	0.06 [0.31]	0.06 [0.33]							0.03 [0.26]	0.05 [0.29]
French legal origin dummy					0.75 [0.27]**	0.76 [0.29]*			0.75 [0.26]**	0.75 [0.28]**
% of population Muslim							0.003 [0.004]	0.001 [0.004]		
% of population Catholic							0.013 [0.003]**	0.012 [0.003]**		
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE AJR Adjusted	-1.1 [0.22]**	-0.98 [0.29]**	-1.54 [0.26]**	-1.42 [0.50]*	-1.15 [0.22]**	-1.05 [0.29]**	-1.23 [0.23]**	-1.07 [0.31]**	-1.1 [0.23]**	-0.94 [0.30]**
Latitude (standardized)		0.13 [0.19]		0.11 [0.29]		0.11 [0.18]		0.16 [0.21]		0.17 [0.19]
Bristish colony dummy	0.84 [0.32]*	0.8 [0.32]*								
French colony dummy	-0.48 [0.35]	-0.54 [0.38]							-0.59 [0.30]	-0.65 [0.31]*
French legal origin dummy					-0.84 [0.33]*	-0.84 [0.33]*			-0.71 [0.34]*	-0.68 [0.34]*
% of population Muslim							-0.01 [0.006]	-0.012 [0.006]*		
% of population Catholic							-0.014 [0.005]**	-0.014 [0.005]**		
Observations	64	64	23	23	64	64	64	64	64	64
Clusters	36	36	19	19	36	36	36	36	36	36
R-squared (1st stage)	0.42	0.42	0.46	0.47	0.38	0.38	0.38	0.38	0.4	0.41

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. The omitted group in Columns 1 and 2 is all countries that were neither French nor British colonies. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 6.3 - IV Regressions of Log GDP per Capita with Additional Controls for DE Albouy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*British Colonies*				*Full Sample*			
					Panel B: Second Stage Results for Log GDP per Capita					
Protection against expropriation risk, 1985-1995	**0.93 [0.15]****	**0.98 [0.20]****	**0.86 [0.14]****	**0.97 [0.21]****	**0.94 [0.14]****	**0.99 [0.18]****	**0.79 [0.10]****	**0.79 [0.14]****	**0.94 [0.15]****	**1.01 [0.21]****
Latitude (standardized)		-0.07 [0.13]		-0.17 [0.15]		-0.08 [0.12]		0.00 [0.12]		-0.09 [0.13]
Bristish colony dummy	-0.64 [0.29]*	-0.66 [0.32]*								
French colony dummy	0 [0.30]	0.06 [0.35]							0.05 [0.27]	0.12 [0.33]
French legal origin dummy					0.71 [0.28]*	0.74 [0.32]*			0.7 [0.27]**	0.73 [0.31]*
% of population Muslim							0.003 [0.004]	0.003 [0.005]		
% of population Catholic							0.012 [0.003]**	0.012 [0.004]**		
					Panel A: First Stage for Average Protection Against Expropriation Risk					
DE Albouy	-0.92 [0.21]**	-0.82 [0.26]**	-1.26 [0.23]**	-1.16 [0.37]**	-0.96 [0.21]**	-0.88 [0.26]**	-1.04 [0.22]**	-0.9 [0.27]**	-0.92 [0.21]**	-0.8 [0.26]**
Latitude (standardized)		0.14 [0.18]		0.12 [0.23]		0.12 [0.16]		0.18 [0.19]		0.17 [0.17]
Bristish colony dummy	0.88 [0.31]**	0.83 [0.30]**								
French colony dummy	-0.49 [0.36]	-0.55 [0.38]							-0.62 [0.31]	-0.68 [0.33]*
French legal origin dummy					-0.87 [0.31]**	-0.85 [0.31]**			-0.73 [0.34]*	-0.69 [0.33]*
% of population Muslim							-0.01 [0.006]	-0.011 [0.006]		
% of population Catholic							-0.014 [0.004]**	-0.014 [0.004]**		
Observations	62	62	22	22	62	62	62	62	62	62
Clusters	37	37	21	21	37	37	37	37	37	37
R-squared (1st stage)	0.43	0.43	0.47	0.47	0.39	0.39	0.38	0.39	0.41	0.42

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. The omitted group in Columns 1 and 2 is all countries that were neither French nor British colonies. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 6.4 - IV Regressions of Log GDP per Capita with Additional Controls for DE Albouy Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*British Colonies*				*Full Sample*			
					Panel B: Second Stage Results for Log GDP per Capita					
Protection against expropriation risk, 1985-1995	**0.93 [0.16]****	**1 [0.21]****	**0.85 [0.14]****	**0.98 [0.21]****	**0.94 [0.14]****	**1 [0.19]****	**0.79 [0.10]****	**0.79 [0.15]****	**0.94 [0.16]****	**1.03 [0.22]****
Latitude (standardized)		-0.08 [0.13]		-0.18 [0.15]		-0.08 [0.12]		0.00 [0.12]		-0.1 [0.14]
Bristish colony dummy	-0.64 [0.29]*	-0.68 [0.33]*								
French colony dummy	0.01 [0.30]	0.08 [0.36]							0.05 [0.27]	0.14 [0.33]
French legal origin dummy					0.71 [0.28]*	0.75 [0.32]*			0.7 [0.27]**	0.74 [0.31]*
% of population Muslim							0.003 [0.004]	0.003 [0.005]		
% of population Catholic							0.012 [0.003]**	0.012 [0.004]**		
					Panel A: First Stage for Average Protection Against Expropriation Risk					
DE Albouy Adjusted	-1.02 [0.23]**	-0.91 [0.30]**	-1.47 [0.26]**	-1.43 [0.48]**	-1.07 [0.23]**	-0.98 [0.30]**	-1.15 [0.24]**	-0.98 [0.31]**	-1.01 [0.24]**	-0.87 [0.30]**
Latitude (standardized)		0.13 [0.19]		0.04 [0.28]		0.11 [0.18]		0.18 [0.21]		0.16 [0.19]
Bristish colony dummy	0.94 [0.32]**	0.9 [0.32]**								
French colony dummy	-0.49 [0.36]	-0.55 [0.38]							-0.62 [0.31]	-0.68 [0.32]*
French legal origin dummy					-0.94 [0.32]**	-0.92 [0.32]**			-0.8 [0.34]*	-0.75 [0.34]*
% of population Muslim							-0.011 [0.006]*	-0.013 [0.006]*		
% of population Catholic							-0.015 [0.004]**	-0.015 [0.005]**		
Observations	62	62	22	22	62	62	62	62	62	62
Clusters	37	37	21	21	37	37	37	37	37	37
R-squared (1st stage)	0.41	0.42	0.46	0.46	0.37	0.37	0.36	0.37	0.39	0.4

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. The omitted group in Columns 1 and 2 is all countries that were neither French nor British colonies. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 7.1 - Robustness Checks for the IV Regressions of Log GDP per Capita for DE AJR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(1), (2) w/o Malta				*Full sample*			*(8), (10) w/o Haiti, El Salvador*		
	Panel B: Second Stage Results for Log GDP per Capita									
Protection against expro-priation risk, 1985-1995	**0.72**	**0.73**	**0.87**	**0.88**	**0.85**	**0.86**	**0.78**	**0.75**	**0.88**	**0.79**
	[0.12]**	**[0.13]****	**[0.15]****	**[0.16]****	**[0.13]****	**[0.15]****	**[0.12]****	**[0.09]****	**[0.13]****	**[0.11]****
Malaria Ecology	-0.19	-0.2								
(standardized)	[0.13]	[0.13]								
Latitude (standardized)		-0.03		-0.01		-0.01				
		[0.10]		[0.12]		[0.12]				
Religious fractionalization									-0.7	-0.31
									[0.45]	[0.40]
Linguistic fractionalization								-1.01		-1.19
								[0.32]**		[0.38]**
Ethnic fractionalization							-0.76			0.58
							[0.41]			[0.48]
Oil dummy					0.08	0.08				
(reserves > 500,000 b /pop)					[0.25]	[0.29]				
Landlocked dummy			0.44	0.45						
			[0.37]	[0.38]						
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE Albouy Adjusted	-0.96	-0.88	-0.92	-0.83	-0.98	-0.88	-0.93	-0.95	-0.95	-0.81
	[0.23]**	[0.26]**	[0.20]**	[0.22]**	[0.21]**	[0.24]**	[0.21]**	[0.20]**	[0.19]**	[0.18]**
Malaria Ecology	-0.01	0								
(standardized)	[0.17]	[0.17]								
Latitude (standardized)		0.152		0.15		0.18				
		[0.169]		[0.16]		[0.17]				
Religious fractionalization									0.48	0.94
									[0.58]	[0.66]
Linguistic fractionalization								0.11		0.69
								[0.47]		[0.62]
Ethnic fractionalization							-0.36			-1.88
							[0.64]			[0.74]*
Oil dummy					1.22	1.29				
(reserves > 500,000 b /pop)					[0.52]*	[0.56]*				
Landlocked dummy			-1.26	-1.26						
			[0.40]**	[0.38]**						
Observations	63	63	64	64	64	64	64	62	64	62
Clusters	35	35	36	36	36	36	36	36	36	36
R-squared (1st stage)	0.35	0.36	0.42	0.43	0.39	0.4	0.36	0.36	0.36	0.42

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Malaria Ecology is a measure of the geographic potential for malaria from Kiszewski et al. (2004). The three measures of fractionalisation are taken from Alesina et al. (2003). All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 7.2 - Robustness Checks for the IV Regressions of Log GDP per Capita for DE AJR Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(1), (2) w/o Malta				*Full sample*			*(8), (10) w/o Haiti, El Salvador*		
					Panel B: Second Stage Results for Log GDP per Capita					
Protection against expro-	**0.71**	**0.73**	**0.89**	**0.92**	**0.87**	**0.89**	**0.8**	**0.76**	**0.88**	**0.81**
priation risk, 1985-1995	**[0.13]****	**[0.17]****	**[0.16]****	**[0.19]****	**[0.15]****	**[0.18]****	**[0.14]****	**[0.11]****	**[0.14]****	**[0.13]****
Malaria Ecology	-0.2	-0.2								
(standardized)	[0.13]	[0.13]								
Latitude (standardized)		-0.03		-0.03		-0.03				
		[0.12]		[0.14]		[0.14]				
Religious fractionalization									-0.7	-0.34
									[0.45]	[0.40]
Linguistic fractionalization								-1		-1.19
								[0.33]**		[0.39]**
Ethnic fractionalization							-0.74			0.62
							[0.43]			[0.50]
Oil dummy					0.06	0.03				
(reserves > 500,000 b /pop)					[0.26]	[0.33]				
Landlocked dummy			0.47	0.5						
			[0.39]	[0.41]						
					Panel A: First Stage for Average Protection Against Expropriation Risk					
DE Albouy Adjusted	-1.18	-1.06	-1.15	-1.02	-1.22	-1.07	-1.15	-1.17	-1.2	-0.97
	[0.33]**	[0.39]*	[0.27]**	[0.31]**	[0.28]**	[0.34]**	[0.29]**	[0.28]**	[0.25]**	[0.24]**
Malaria Ecology	-0.04	-0.04								
(standardized)	[0.18]	[0.19]								
Latitude (standardized)		0.14		0.14		0.17				
		[0.217]		[0.21]		[0.22]				
Religious fractionalization									0.79	1.32
									[0.63]	[0.68]
Linguistic fractionalization								0.02		0.58
								[0.49]		[0.64]
Ethnic fractionalization							-0.46			-2.05
							[0.68]			[0.78]*
Oil dummy					1.09	1.16				
(reserves > 500,000 b /pop)					[0.52]*	[0.57]*				
Landlocked dummy			-1.29	-1.3						
			[0.41]**	[0.39]**						
Observations	63	63	64	64	64	64	64	62	64	62
Clusters	35	35	36	36	36	36	36	36	36	36
R-squared (1st stage)	0.3	0.31	0.37	0.37	0.33	0.33	0.31	0.3	0.32	0.39

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Malaria Ecology is a measure of the geographic potential for malaria from Kiszewski et al. (2004). The three measures of fractionalisation are taken from Alesina et al. (2003). All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 7.3 - Robustness Checks for the IV Regressions of Log GDP per Capita for DE Albouy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(1), (2) w/o Malta				*Full sample*			*(8), (10) w/o Haiti, El Salvador*		
	Panel B: Second Stage Results for Log GDP per Capita									
Protection against expropriation risk, 1985-1995	**0.75 [0.14]****	**0.83 [0.18]****	**0.9 [0.16]****	**0.98 [0.20]****	**0.89 [0.15]****	**0.96 [0.20]****	**0.83 [0.14]****	**0.79 [0.11]****	**0.9 [0.15]****	**0.84 [0.14]****
Malaria Ecology (standardized)	-0.17 [0.15]	-0.18 [0.17]								
Latitude (standardized)		-0.12 [0.11]		-0.11 [0.14]		-0.11 [0.14]				
Religious fractionalization									-0.6 [0.45]	-0.36 [0.43]
Linguistic fractionalization								-0.85 [0.36]*		-1.07 [0.43]*
Ethnic fractionalization							-0.57 [0.43]			0.69 [0.52]
Oil dummy (reserves > 500,000 b /pop)					0.01 [0.26]	-0.13 [0.32]				
Landlocked dummy			0.45 [0.38]	0.55 [0.43]						
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE Albouy Adjusted	-0.99 [0.31]**	-0.89 [0.35]*	-0.98 [0.24]**	-0.87 [0.28]**	-1.03 [0.27]**	-0.89 [0.31]**	-0.98 [0.26]**	-1.01 [0.25]**	-1.01 [0.24]**	-0.84 [0.24]**
Malaria Ecology (standardized)	-0.04 [0.22]	-0.04 [0.23]								
Latitude (standardized)		0.165 [0.200]		0.16 [0.19]		0.2 [0.20]				
Religious fractionalization									0.74 [0.60]	1.21 [0.67]
Linguistic fractionalization								0.12 [0.59]		0.7 [0.69]
Ethnic fractionalization							-0.42 [0.71]			-2.03 [0.74]**
Oil dummy (reserves > 500,000 b /pop)					1.11 [0.57]	1.2 [0.61]				
Landlocked dummy			-1.35 [0.37]**	-1.34 [0.36]**						
Observations	61	61	62	62	62	62	62	60	62	60
Clusters	36	36	37	37	37	37	37	37	37	37
R-squared (1st stage)	0.3	0.31	0.38	0.38	0.33	0.34	0.31	0.31	0.32	0.39

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Malaria Ecology is a measure of the geographic potential for malaria from Kiszewski et al. (2004). The three measures of fractionalisation are taken from Alesina et al. (2003). All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 7.4 - Robustness Checks for the IV Regressions of Log GDP per Capita for DE Albouy Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(1), (2) w/o Malta				*Full sample*			*(8), (10) w/o Haiti, El Salvador*		
	Panel B: Second Stage Results for Log GDP per Capita									
Protection against expro-priation risk, 1985-1995	**0.76 [0.16]****	**0.86 [0.22]****	**0.92 [0.17]****	**1.03 [0.22]****	**0.91 [0.16]****	**1.01 [0.23]****	**0.85 [0.15]****	**0.81 [0.12]****	**0.91 [0.16]****	**0.86 [0.15]****
Malaria Ecology (standardized)	-0.17 [0.16]	-0.16 [0.18]								
Latitude (standardized)		-0.14 [0.13]		-0.14 [0.16]		-0.14 [0.17]				
Religious fractionalization									-0.61 [0.45]	-0.4 [0.44]
Linguistic fractionalization								-0.84 [0.37]*		-1.07 [0.44]*
Ethnic fractionalization							-0.55 [0.45]			0.74 [0.55]
Oil dummy (reserves > 500,000 b /pop)					-0.01 [0.27]	-0.2 [0.35]				
Landlocked dummy			0.48 [0.40]	0.62 [0.47]						
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE Albouy Adjusted	-1.05 [0.35]**	-0.92 [0.41]*	-1.06 [0.27]**	-0.93 [0.31]**	-1.12 [0.30]**	-0.94 [0.35]*	-1.06 [0.30]**	-1.08 [0.28]**	-1.11 [0.27]**	-0.89 [0.27]**
Malaria Ecology (standardized)	-0.07 [0.22]	-0.07 [0.24]								
Latitude (standardized)		0.173 [0.221]		0.16 [0.21]		0.21 [0.22]				
Religious fractionalization									0.89 [0.62]	1.39 [0.68]*
Linguistic fractionalization								0.06 [0.60]		0.63 [0.70]
Ethnic fractionalization							-0.46 [0.74]			-2.09 [0.77]**
Oil dummy (reserves > 500,000 b /pop)					1.05 [0.57]	1.16 [0.60]				
Landlocked dummy			-1.36 [0.38]**	-1.36 [0.37]**						
Observations	61	61	62	62	62	62	62	60	62	60
Clusters	36	36	37	37	37	37	37	37	37	37
R-squared (1st stage)	0.27	0.28	0.35	0.36	0.3	0.31	0.28	0.27	0.3	0.37

Notes: Panel B reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Malaria Ecology is a measure of the geographic potential for malaria from Kiszewski et al. (2004). The three measures of fractionalisation are taken from Alesina et al. (2003). All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

9 Additional Instruments and Overidentification Tests

In this section, I introduce further instruments for institutions and check whether the instrumented institutional scores are mutually consistent, i.e., I test the overidentified system. The additional instruments I use are scores of democracy in the early 19th century (the "Polity" score and the score for "Constraints on the Executive" from the Polity IV database), colonial and legal origin dummies and the fraction of the population from European descent.

The results are reported in the now familiar way, where Panel A of Tables 8.1 to 8.4 reports the first stage results with the new instruments included. In all but one specification, the measure of disease environment is a significant instrument, while most of the additional instruments are not significant determinants of property rights institutions. Panel C reports the instrumented second stage, again with similar results as in earlier specifications: institutions are a major determinant of economic performance. In Panel B, I report the overidentification tests. Since heteroscedasticity is a potential concern, I do not report a Hausman test for the overidentification restriction, but I report the Sargan-Hansen C-statistic for the orthogonality of the measures of disease environment (also the Hausman test does not reject in any specification).

In the 10 instrumental variable estimations for each of the four measures of disease environment of Tables 8.1 to 8.4, I first include the percentage of population in 1975 that is of European descent in Column 1. This is done to address the worry that the results presented so far are simply resulting from the fact that Europeans brought with them a culture geared towards better property rights protection (or brought with them more human capital as is the theory of Glaeser et al. (2004)). The fraction of the population from Europe is never a significant determinant of institutional quality. Additionally, the overidentification test are of extremely low power, with the lowest p-value in any of the Columns 1 equal to 0.44. In addition to testing the exclusion restriction, I also control for the direct effects geography has on development by adding latitude in Column 2. I find no different results than in Column 1, except that there is one case (Table 8.4, Column 2) in which the measure of disease environment is not significant when the additional instrument is included.

I next introduce two measures of early institutional measure. Following Acemoglu et al., I first focus on the early Polity score from the Polity IV data base as an additional instrument. This measure takes values between -10 and +10 and is higher for more democratic societies. In Column 3 of each of the Tables in this section, I include all countries with an available Polity score in the period of 1900 to 1910 (I use the earliest available score). Few countries in the sample of former colonies were independent at that time and there are thus only 25 observations. While the policy score is not significant, disease environment is and again, the overidentification test does not reject. I repeat this estimation in Column 4, but this time I include all countries with an available Polity score before 1961, hence yielding 48 countries, and similar results as in the previous specification. In Column 4, I again always use the earliest available Polity score after 1900 given that one was available before 1961.

Next, in Columns 5 and 6 of Tables 8.1 to 8.4, I repeat this exercise, but I focus on a sub-indicator of the Polity database, "Constraint on the Executive." This variable takes values between 1 and 7 and measures whether superimposed structures and rules effectively constrain the executive. I again include first all countries that have a Polity score in the period of 1900 to 1910 and still exist today (Column 5) and then those with a polity score before 1961 (Column 6). In both regressions, constraints on the executive are not significant, disease environment is, and the overidentification test does not reject.

I next add colony dummies (Columns 7 and 8) and a French colony and legal origin dummy (columns 9 an 10). When including a French and British colony dummy in Columns 7 and 8, I find that the British colony dummy is always positive and always significant, while the French colony dummy is always negative, but never significant. When including a French legal origin dummy and a French colony dummy, both are negative and sometimes significantly so. Also in Columns 7 to 10, disease environment is significant also when the respective instruments are included, and the overidentification test never rejects.

Overall, I conclude that the exclusion restriction that disease environment matters for institutions only indirectly through settler mortality is never rejected when adding various other instruments for institutions while also controlling for the direct effects of geography by including latitude.

Table 8.1 - Overidentification Tests for DE AJR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*Polity Score Available Before*					*Full Sample*		
			1911	*1961*	*1911*	*1961*				
	Panel C: Second Stage Results for Log GDP per Capita									
Protection against expropriation risk, 1985-1995	**0.87**	**0.89**	**0.49**	**0.73**	**0.48**	**0.71**	**0.75**	**0.71**	**0.73**	**0.67**
	[0.14]**	**[0.15]****	**[0.04]****	**[0.09]****	**[0.04]****	**[0.08]****	**[0.09]****	**[0.08]****	**[0.09]****	**[0.09]****
Latitude		-0.02						0.08		0.09
		[0.12]						[0.09]		[0.10]
	Panel B: Overidentification Tests									
p-value Sargan C statistic for DE AJR	0.442	0.387	0.565	0.578	0.531	0.275	0.131	0.25	0.118	0.225
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE AJR	-0.76	-0.75	-1.28	-1.03	-1.23	-1	-0.86	-0.76	-0.86	-0.75
	[0.25]**	[0.27]**	[0.17]**	[0.21]**	[0.20]**	[0.21]**	[0.17]**	[0.20]**	[0.18]**	[0.21]**
Latitude (standardized)		0.05						0.16		0.19
		[0.17]						[0.15]		[0.16]
% European descent (1975)	0.01	0.01								
	[0.01]	[0.01]								
Earliest Polity score			0.04	0.02						
			[0.03]	[0.03]						
Earliest executive const.					0.13	0.08				
					[0.08]	[0.08]				
French legal origin dummy									-0.57	-0.56
									[0.32]	[0.31]
British colony dummy							0.7	0.67		
							[0.30]*	[0.29]*		
French colony dummy							-0.51	-0.58	-0.6	-0.67
							[0.35]	[0.37]	[0.31]	[0.32]*
Observations	64	64	25	48	25	48	64	64	64	64
Clusters	36	36	11	28	11	28	36	36	36	36
R-squared (1st stage)	0.38	0.38	0.66	0.45	0.67	0.45	0.45	0.46	0.43	0.45

Notes: Panel C reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Panel B reports the p-value for the null hypothesis that the coefficient on average protection against expropriation (in Panel C) is the same when instrumented using the measure of disease environment in addition to the respective instruments. The C Statistic (difference-in-Sargan) is reported. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 8.2 - Overidentification Tests for DE AJR Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*Polity Score Available Before*					*Full Sample*		
			1911	*1961*	*1911*	*1961*				
			Panel C: Second Stage Results for Log GDP per Capita							
Protection against expro-priation risk, 1985-1995	**0.89**	**0.93**	**0.49**	**0.74**	**0.48**	**0.72**	**0.73**	**0.68**	**0.7**	**0.63**
	[0.15]**	**[0.18]****	**[0.04]****	**[0.10]****	**[0.03]****	**[0.09]****	**[0.09]****	**[0.09]****	**[0.09]****	**[0.10]****
Latitude		-0.05						0.09		0.12
		[0.13]						[0.09]		[0.10]
				Panel B: Overidentification Tests						
p-value Sargan C statistic for DE AJR Adjusted	0.559	0.532	0.587	0.500	0.537	0.229	0.124	0.293	0.120	0.289
			Panel A: First Stage for Average Protection Against Expropriation Risk							
DE AJR Adjusted	-0.85	-0.84	-1.56	-1.22	-1.5	-1.19	-1.1	-0.98	-1.1	-0.94
	[0.31]**	[0.37]*	[0.23]**	[0.28]**	[0.30]**	[0.28]**	[0.22]**	[0.29]**	[0.23]**	[0.30]**
Latitude (standardized)		0.01						0.13		0.17
		[0.21]						[0.19]		[0.19]
% European descent (1975)	0.01	0.01								
	[0.01]	[0.01]								
Earliest Polity score			0.05	0.03						
			[0.03]	[0.03]						
Earliest executive const.					0.15	0.11				
					[0.09]	[0.08]				
French legal origin dummy									-0.71	-0.68
									[0.34]*	[0.34]*
British colony dummy							0.84	0.8		
							[0.32]*	[0.32]*		
French colony dummy							-0.48	-0.54	-0.59	-0.65
							[0.35]	[0.38]	[0.30]	[0.31]*
Observations	64	64	25	48	25	48	64	64	64	64
Clusters	36	36	11	28	11	28	36	36	36	36
R-squared (1st stage)	0.33	0.33	0.62	0.38	0.62	0.39	0.42	0.42	0.4	0.41

Notes: Panel C reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Panel B reports the p-value for the null hypothesis that the coefficient on average protection against expropriation (in Panel C) is the same when instrumented using the measure of disease environment in addition to the respective instruments. The C Statistic (difference-in-Sargan) is reported. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 8.3 - Overidentification Tests for DE Albouy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*Polity Score Available Before*					*Full Sample*		
			1911	*1961*	*1911*	*1961*				
	Panel C: Second Stage Results for Log GDP per Capita									
Protection against expropriation risk, 1985-1995	**0.89**	**0.98**	**0.49**	**0.75**	**0.48**	**0.73**	**0.74**	**0.72**	**0.74**	**0.71**
	[0.15]**	**[0.18]****	**[0.04]****	**[0.09]****	**[0.04]****	**[0.09]****	**[0.09]****	**[0.08]****	**[0.09]****	**[0.09]****
Latitude		-0.12						0.03		0.03
		[0.13]						[0.08]		[0.08]
	Panel B: Overidentification Tests									
p-value Sargan C statistic for DE Albouy	0.939	0.828	0.564	0.485	0.527	0.223	0.154	0.255	0.149	0.276
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE Albouy	-0.73	-0.71	-1.47	-1.07	-1.41	-1.04	-0.92	-0.82	-0.92	-0.8
	[0.29]*	[0.33]*	[0.23]**	[0.27]**	[0.26]**	[0.27]**	[0.21]**	[0.26]**	[0.21]**	[0.26]**
Latitude (standardized)		0.05						0.14		0.17
		[0.19]						[0.18]		[0.17]
% European descent (1975)	0.01	0.01								
	[0.01]	[0.01]								
Earliest Polity score			0.04	0.03						
			[0.03]	[0.03]						
Earliest executive const.					0.14	0.1				
					[0.08]	[0.08]				
French legal origin dummy									-0.73	-0.69
									[0.34]*	[0.33]*
British colony dummy							0.88	0.83		
							[0.31]**	[0.30]**		
French colony dummy							-0.49	-0.55	-0.62	-0.68
							[0.36]	[0.38]	[0.31]	[0.33]*
Observations	62	62	25	48	25	48	62	62	62	62
Clusters	37	37	13	29	13	29	37	37	37	37
R-squared (1st stage)	0.34	0.34	0.65	0.39	0.66	0.4	0.43	0.43	0.41	0.42

Notes: Panel C reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Panel B reports the p-value for the null hypothesis that the coefficient on average protection against expropriation (in Panel C) is the same when instrumented using the measure of disease environment in addition to the respective instruments. The C Statistic (difference-in-Sargan) is reported. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

Table 8.4 - Overidentification Tests for DE Albouy Adjusted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		*Polity Score Available Before*					*Full Sample*		
			1911	*1961*	*1911*	*1961*				
	Panel C: Second Stage Results for Log GDP per Capita									
Protection against expropriation risk, 1985-1995	**0.9**	**1.01**	**0.49**	**0.75**	**0.48**	**0.73**	**0.74**	**0.72**	**0.73**	**0.7**
	[0.15]**	**[0.19]****	**[0.04]****	**[0.10]****	**[0.03]****	**[0.10]****	**[0.09]****	**[0.09]****	**[0.09]****	**[0.09]****
Latitude		-0.14						0.03		0.04
		[0.14]						[0.08]		[0.08]
	Panel B: Overidentification Tests									
p-value Sargan C statistic for DE Albouy Adjusted	0.920	0.982	0.576	0.444	0.531	0.199	0.144	0.244	0.144	0.279
	Panel A: First Stage for Average Protection Against Expropriation Risk									
DE Albouy Adjusted	-0.73	-0.71	-1.6	-1.12	-1.53	-1.1	-1.02	-0.91	-1.01	-0.87
	[0.30]*	[0.35]	[0.25]**	[0.30]**	[0.31]**	[0.30]**	[0.23]**	[0.30]**	[0.24]**	[0.30]**
Latitude (standardized)		0.04						0.13		0.16
		[0.21]						[0.19]		[0.19]
% European descent (1975)	0.01	0.01								
	[0.01]	[0.01]								
Earliest Polity score			0.05	0.03						
			[0.03]	[0.03]						
Earliest executive const.					0.15	0.12				
					[0.08]	[0.08]				
French legal origin dummy									-0.8	-0.75
									[0.34]*	[0.34]*
British colony dummy							0.94	0.9		
							[0.32]**	[0.32]**		
French colony dummy							-0.49	-0.55	-0.62	-0.68
							[0.36]	[0.38]	[0.31]	[0.32]*
Observations	62	62	25	48	25	48	62	62	62	62
Clusters	37	37	13	29	13	29	37	37	37	37
R-squared (1st stage)	0.32	0.32	0.62	0.35	0.63	0.37	0.41	0.42	0.39	0.4

Notes: Panel C reports the two-stage least-squares estimates with log GDP per capita in 1995 (PPP adjusted) as the dependent variable, and Panel A reports the corresponding first stage. Panel B reports the p-value for the null hypothesis that the coefficient on average protection against expropriation (in Panel C) is the same when instrumented using the measure of disease environment in addition to the respective instruments. The C Statistic (difference-in-Sargan) is reported. All specifications are estimated using Stata's ivreg2 command (see Baum et al. (2006)); clustered and heteroscedastic-robust standard errors are reported in brackets; * significant at 5%; ** significant at 1%.

10 Conclusion

This paper does neither assess whether Daron Acemoglu, Simon Johnson or James Robinson are consistent when they assemble their settler mortality data, nor does it examine the data revisions of David Albouy. For many countries, there are both multiple mortality rates within a given historical source such as Curtin (1998), as well as multiple mortality rates across historical sources. Thus, I argue that a discussion of how to choose mortality rates is somewhat pointless: one can never be sure that all possible historical sources of mortality rates have been exhausted, and therefore the validity of the colonial origins theory cannot be established beyond any doubt when using historical data.

The main contribution of this paper is to instrument for historical settler mortality rates with a geographic model of the determinants of mortality during colonization. The geographic variables used in this study – such as average temperature or rainfall – are measured precisely, are shown to be strongly related to mortality and the constructed instruments are also shown to be otherwise excludable to institutional outcomes.

After establishing the validity of my instruments, I repeat the analysis of Acemoglu et al. I find strong support for their theory of the colonial origins of comparative development. First, I show that the relation between institutional outcomes and the instrumented mortality rate is highly significant in both the sample of Acemoglu et al. and when using the data constructed by Albouy. The latter finding is robust to inclusion of controls and to accounting for the population the historical data was collected from (bishops, from forced labor or during campaign). Second, I establish the effect institutions have on international income differentials by using the constructed measures of disease environment to instrument for institutional outcomes. I find that for all measures of early disease environment constructed in this paper, institutions are a highly significant determinant of economic prosperity. In addition, my point estimates are similar to the ones of Acemoglu et al. I thus conclude that their results indeed do reflect the "early institution building" channel rather than measurement error, as is asserted by Albouy.

While I do not check the rules according to which the data is assembled, I show that the discrepancies between the two mortality series vanish when instrumenting in the proposed way. Although the two mortality series differ substantially, they are both affected by geography in a very similar way. Correspondingly, for both the results relating mortality to institutions and for the results relating institutions to income, it does not matter which series is instrumented for to generate the geographic projection of mortality. Both show that the colonial origins theory is statistically valid and economically highly relevant. Moreover, although I find that one aspect of David Albouy's critique is supported by the data (the "comparability problem"), accounting for the population the mortality rate was sampled from does not change the effect institutions have on prosperity.

This paper also has a second contribution because the developed instruments allow for an additional falsification exercise to disentangle the colonial origins theory from the potential direct effect disease environment may have had on institutional development. The geographic view of development (advocated for example by Gallup et al. (1998)) predicts that there should be a relation between disease environment and economic development in all countries equally. In contrast, the colonial origins theory predicts a relation between disease environment and development only in the group of former colonies. My measures of early disease environment can be constructed also for countries that have not been colonized and I can thus show that early disease environment did influence institutional development in former colonies, yet not in a sample of countries that have not been colonized. This differential effect disease environment has had on economic development can only be rationalized in the context of the colonial origins theory brought forward by Acemoglu et al.

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11 Data Appendix

11.1 Data for Former Colonies (Main Data)

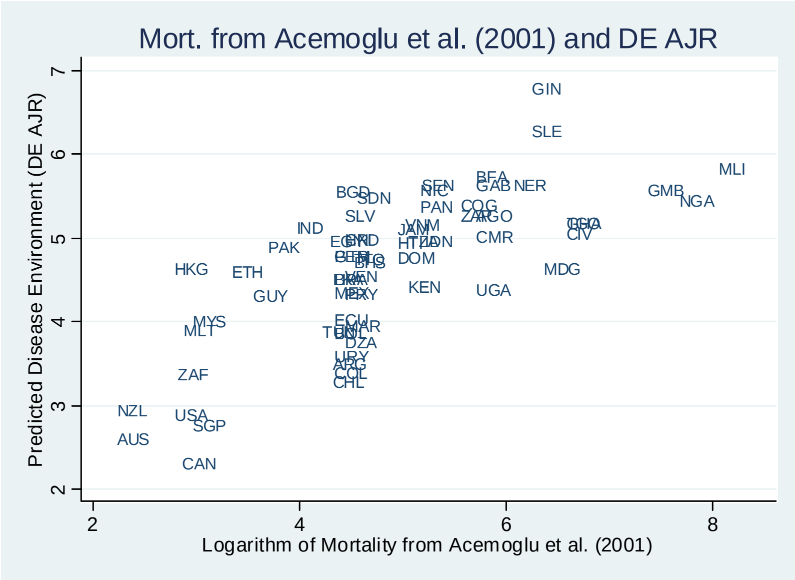

Figure A 1: The relation between the logarithm of mortality from Acemoglu et al. (2001) and its geographic projection (DE AJR from Table 1, Column 1).

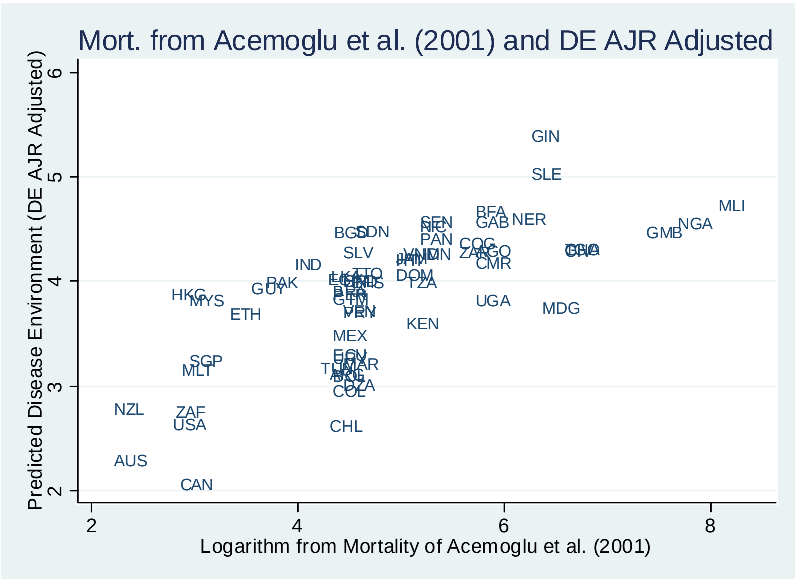

Figure A 2: The relation between the logarithm of mortality from Acemoglu et al. (2001) and the geographic projection adjusted for the sampling population (DE AJR Adjusted from Table 1, Column 3).

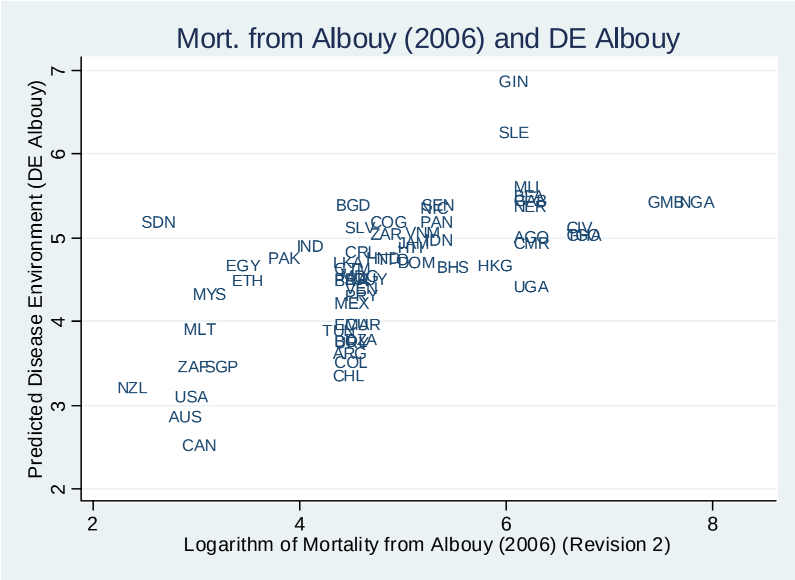

Figure A 3: The relation between the logarithm of mortality from Albouy (2006) and the geographic projection (DE Albouy from Table 1, Column 5).

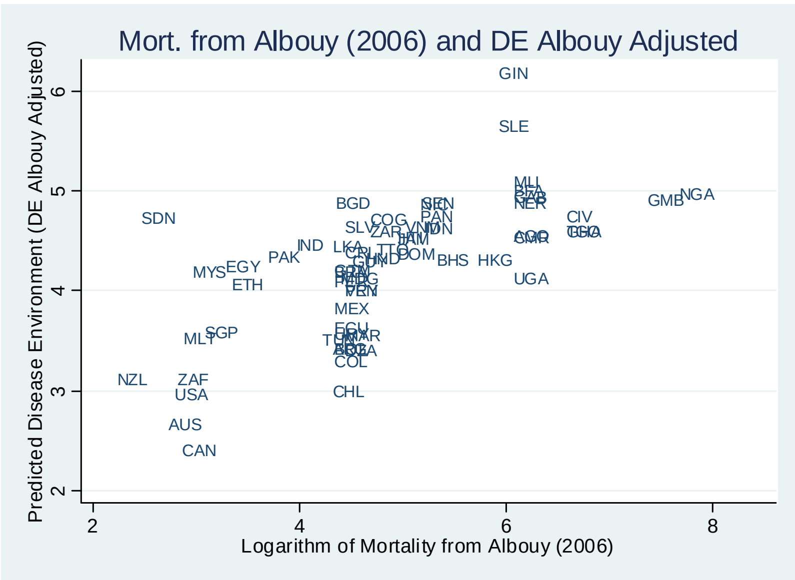

Figure A 4: The relation between the logarithm of mortality from Albouy (2006) and the geographic projection adjusted for the sampling population (DE Albouy Adjusted from Table 1, Column 6).

Table A1 - List of the Data (Main Analysis)

Country Name	*Worldbank Code*	*Avg. Exprop. Risk*	*Log GPD per Capita*	*Ln Mortality Acem. et al.*	*Ln Mortality Albouy*	*DE AJR*	*DE AJR Adj.*	*DE Albouy*	*DE Albouy Adj.*
Algeria	DZA	6.50	8.39	4.359	4.359	3.756	3.016	3.796	3.401
Angola	AGO	5.36	7.77	5.635	5.991	5.275	4.291	5.018	4.558
Argentina	ARG	6.39	9.13	4.233	4.233	3.509	3.120	3.628	3.429
Australia	AUS	9.32	9.90	2.146	2.646	2.604	2.300	2.877	2.673
Bahamas, The	BHS	7.50	9.29	4.443	5.242	4.701	4.002	4.650	4.305
Bangladesh	BGD	5.14	6.88	4.268	4.268	5.548	4.476	5.401	4.890
Bolivia	BOL	5.64	7.93	4.263	4.263	3.872	3.100	3.768	3.422
Brazil	BRA	7.91	8.73	4.263	4.263	4.520	3.905	4.501	4.193
Burkina Faso	BFA	4.45	6.85	5.635	5.991	5.745	4.673	5.510	4.999
Cameroon	CMR	6.45	7.50	5.635	5.991	5.018	4.186	4.946	4.541
Canada	CAN	9.73	9.99	2.779	2.779	2.310	2.057	2.525	2.409
Chile	CHL	7.82	9.34	4.233	4.233	3.278	2.624	3.351	3.002
Colombia	COL	7.32	8.81	4.263	4.263	3.386	2.963	3.515	3.306
Congo, Dem. Rep.	ZAR	3.50	6.87	5.481	4.605	5.265	4.279	5.062	4.598
Congo, Rep.	COG	4.68	7.42	5.481	4.605	5.402	4.365	5.203	4.713
Costa Rica	CRI	7.05	8.79	4.358	4.358	4.972	4.015	4.835	4.386
Cote d'Ivoire	CIV	7.00	7.44	6.504	6.504	5.054	4.301	5.129	4.744
Dominican Rep.	DOM	6.18	8.36	4.868	4.868	4.754	4.067	4.713	4.372
Ecuador	ECU	6.55	8.47	4.263	4.263	4.017	3.300	3.972	3.640
Egypt, Arab Rep.	EGY	6.77	7.95	4.217	3.207	4.956	4.031	4.683	4.256
El Salvador	SLV	5.00	7.95	4.358	4.358	5.272	4.276	5.118	4.646
Ethiopia	ETH	5.73	6.11	3.258	3.258	4.605	3.689	4.498	4.074
Gabon	GAB	7.82	8.90	5.635	5.991	5.633	4.573	5.457	4.951
Gambia, The	GMB	8.27	7.27	7.293	7.293	5.569	4.467	5.437	4.913
Ghana	GHA	6.27	7.37	6.504	6.504	5.189	4.307	5.024	4.600
Guatemala	GTM	5.14	8.29	4.263	4.263	4.787	3.843	4.646	4.208
Guinea	GIN	6.55	7.49	6.180	5.858	6.782	5.389	6.869	6.180
Guyana	GUY	5.89	7.90	3.471	4.431	4.316	3.936	4.520	4.297
Haiti	HTI	3.73	7.15	4.868	4.868	4.952	4.212	4.900	4.532
Honduras	HND	5.32	7.69	4.358	4.556	4.975	4.017	4.773	4.328
Hong Kong (Ch)	HKG	8.14	10.05	2.701	5.652	4.645	3.889	4.676	4.305
India	IND	8.27	7.33	3.884	3.884	5.122	4.162	4.915	4.465
Indonesia	IDN	7.59	8.07	5.136	5.136	4.959	4.274	4.974	4.624
Jamaica	JAM	7.09	8.19	4.868	4.868	5.101	4.228	4.944	4.526
Kenya	KEN	6.05	7.06	4.977	na	4.416	3.613	na	na
Madagascar	MDG	4.45	6.84	6.284	4.317	4.645	3.746	4.546	4.127
Malaysia	MYS	7.95	8.89	2.874	2.874	4.007	3.826	4.326	4.183
Mali	MLI	4.00	6.57	7.986	5.991	5.833	4.735	5.618	5.093
Malta	MLT	7.23	9.43	2.791	2.791	3.903	3.153	3.920	3.518
Mexico	MEX	7.50	8.94	4.263	4.263	4.353	3.494	4.217	3.825
Morocco	MAR	7.09	8.04	4.359	4.359	3.964	3.212	3.965	3.563
New Zealand	NZL	9.73	9.76	2.146	2.146	2.943	2.787	3.221	3.122
Nicaragua	NIC	5.23	7.54	5.096	5.096	5.571	4.521	5.362	4.864
Niger	NER	5.00	6.73	5.991	5.991	5.634	4.594	5.374	4.881
Nigeria	NGA	5.55	6.81	7.603	7.603	5.461	4.557	5.422	4.974
Pakistan	PAK	6.05	7.35	3.611	3.611	4.887	3.999	4.767	4.347
Panama	PAN	5.91	8.84	5.096	5.096	5.373	4.417	5.204	4.746
Paraguay	PRY	6.95	8.21	4.358	4.358	4.325	3.706	4.319	4.015
Peru	PER	5.77	8.40	4.263	4.263	4.779	3.873	4.522	4.107
Senegal	SEN	6.00	7.40	5.104	5.104	5.626	4.572	5.392	4.892
Sierra Leone	SLE	5.82	6.25	6.180	5.858	6.289	5.039	6.263	5.652
Singapore	SGP	9.32	10.15	2.874	2.996	2.768	3.237	3.466	3.592
South Africa	ZAF	6.86	8.89	2.741	2.741	3.381	2.750	3.467	3.123
Sri Lanka	LKA	6.05	7.73	4.246	4.246	4.519	4.052	4.704	4.442
Sudan	SDN	4.00	7.31	4.480	2.389	5.489	4.491	5.196	4.726
Tanzania	TZA	6.64	6.25	4.977	na	4.957	4.000	na	na
Togo	TGO	6.91	7.22	6.504	6.504	5.189	4.307	5.024	4.600
Trinidad and Tob.	TTO	7.45	8.77	4.443	4.666	4.745	4.082	4.749	4.414
Tunisia	TUN	6.45	8.48	4.143	4.143	3.886	3.169	3.898	3.511
Uganda	UGA	4.45	6.97	5.635	5.991	4.392	3.819	4.419	4.129
United States	USA	10.00	10.22	2.708	2.708	2.893	2.642	3.106	2.974
Uruguay	URY	7.00	9.03	4.263	4.263	3.600	3.273	3.747	3.569
Venezuela,	VEN	7.14	9.07	4.358	4.358	4.555	3.723	4.398	4.012
Vietnam	VNM	6.41	7.28	4.942	4.942	5.162	4.268	5.076	4.643

11.2 Data for Non-Colonies

This is the list of data used in Section 6. For these countries, the measures of mortality rates have been predicted out of sample using the respective coefficents from Table 1.

Table A1 - List of the Data (Falsification Exercise)

Country Name	*Worldbank* Code	*Rule of* Law	*Avg. Exprop.* Risk	*DE* AJR	*DE* AJR Adj.	*DE* Albouy	*DE* Albouy Adj.	*Warsaw Pact* Dummy
Afghanistan	AFG	.	-1.614	3.811	2.992	3.675	3.314	0
Albania	ALB	7.26	-0.749	2.857	2.427	3.132	2.871	1
Andorra	ADO	.	1.472	2.060	1.756	2.371	2.180	0
Armenia	ARM	.	-0.477	3.435	2.725	3.345	3.035	1
Austria	AUT	9.74	1.959	2.951	2.508	3.040	2.838	0
Azerbaijan	AZE	.	-0.870	3.977	3.207	3.840	3.494	1
Belarus	BLR	.	-1.105	2.645	2.214	2.750	2.561	1
Belgium	BEL	9.69	1.497	2.910	2.568	3.063	2.898	0
Bosnia and Herz.	BIH	.	-0.739	2.144	1.828	2.437	2.241	0
Bulgaria	BGR	9.04	-0.077	3.231	2.678	3.252	3.003	1
China	CHN	8.11	-0.347	4.026	3.173	3.939	3.555	0
Croatia	HRV	.	-0.055	2.118	1.894	2.468	2.306	0
Czech Republic	CZE	9.88	0.645	3.014	2.418	3.007	2.749	1
Denmark	DNK	9.72	1.962	3.053	2.552	3.100	2.875	0
Estonia	EST	.	0.658	2.717	2.281	2.810	2.617	1
Finland	FIN	9.72	2.036	2.717	2.281	2.810	2.617	0
France	FRA	9.71	1.440	3.446	2.924	3.468	3.225	0
Georgia	GEO	.	-0.842	3.796	3.091	3.712	3.393	1
Germany	DEU	9.88	1.813	2.958	2.509	3.039	2.835	0
Greece	GRC	7.48	0.738	3.392	2.750	3.462	3.115	0
Hungary	HUN	9.08	0.794	3.128	2.625	3.172	2.945	1
Iceland	ISL	9.70	1.926	2.642	2.252	2.774	2.599	0
Ireland	IRL	9.72	1.743	2.928	2.532	3.039	2.855	0
Italy	ITA	9.46	0.882	3.132	2.569	3.283	2.970	0
Japan	JPN	9.72	1.576	3.618	3.132	3.744	3.503	0
Kazakhstan	KAZ	.	-0.839	3.167	2.474	3.074	2.778	1
Korea, Dem. Rep.	PRK	5.02	-1.092	3.703	2.944	3.674	3.332	0
Korea, Rep.	KOR	8.57	0.753	3.904	3.156	3.948	3.601	0
Kyrgyz Republic	KGZ	.	-0.815	2.701	2.168	2.754	2.526	1
Latvia	LVA	.	0.281	2.627	2.200	2.727	2.541	1
Liechtenstein	LIE	.	1.438	2.595	2.357	2.835	2.715	0
Lithuania	LTU	.	0.270	2.717	2.281	2.810	2.617	1
Luxembourg	LUX	10.00	1.961	2.991	2.565	3.089	2.892	0
Macedonia, FYR	MKD	.	-0.420	2.129	1.813	2.409	2.214	0
Moldova	MDA	.	-0.411	3.292	2.746	3.318	3.070	1
Monaco	MCO	.	0.772	3.024	2.517	3.206	2.916	0
Mongolia	MNG	7.95	0.247	2.513	1.844	2.481	2.209	0
Nepal	NPL	.	-0.442	4.729	3.769	4.632	4.186	0
Netherlands	NLD	9.98	1.900	3.009	2.569	3.091	2.890	0
Norway	NOR	9.85	2.033	2.913	2.379	2.957	2.723	0
Poland	POL	7.81	0.549	3.010	2.501	3.067	2.839	1
Portugal	PRT	9.01	1.243	3.583	2.889	3.639	3.267	0
Romania	ROM	7.56	-0.218	3.216	2.643	3.244	2.987	1
Russian Fed.	RUS	8.50	-0.805	2.555	2.133	2.668	2.484	1
San Marino	SMR	.	0.772	3.132	2.569	3.283	2.970	0
Saudi Arabia	SAU	7.44	0.572	4.890	3.969	4.631	4.206	0
Slovak Republic	SVK	9.00	0.278	2.989	2.450	3.023	2.785	1
Slovenia	SVN	.	0.858	1.846	1.653	2.221	2.076	0
Spain	ESP	9.55	1.235	2.924	2.373	3.048	2.747	0
Sweden	SWE	9.50	1.934	2.996	2.450	3.022	2.782	0
Switzerland	CHE	9.99	2.137	2.685	2.437	2.918	2.792	0
Taiwan	TWN	9.24	0.949	3.946	3.535	4.143	3.918	0
Tajikistan	TJK	.	-1.316	3.987	3.177	3.834	3.473	1
Thailand	THA	7.64	0.296	5.605	4.578	5.413	4.922	0
Turkey	TUR	7.29	0.052	3.485	2.785	3.397	3.089	0
Turkmenistan	TKM	.	-1.219	3.990	3.204	3.828	3.477	1
Ukraine	UKR	.	-0.765	2.840	2.344	2.905	2.687	1
United Kingdom	GBR	9.76	1.875	3.322	2.828	3.362	3.133	0
Uzbekistan	UZB	.	-1.106	3.900	3.124	3.746	3.401	1
Yugo., Fed. Rep.	YUG	6.36	-0.952	1.846	1.653	2.221	2.076	0

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