American Economic Review 2012, 102(6): 3059–3076
http://dx.doi.org/10.1257/aer.102.6.3059

The Colonial Origins of Comparative Development: An Empirical Investigation: Comment[†]

By David Y. Albouy*

Acemoglu, Johnson, and Robinson (2001) (hereafter, AJR) is a seminal article that has reinvigorated debate over the relationship between property rights and economic growth. Following Knack and Keefer (1995), Mauro (1995), La Porta et al. (1998), Hall and Jones (1999), Rodrik (1999), and others, AJR endeavors to determine the causal effect of institutions that protect property rights, measured by risk of capital expropriation, on economic performance. This endeavor is complicated by the fact that the correlation between institutional and economic measures may reflect the reverse influence of economic growth on institutions or the simultaneous influence of omitted variables on both economic output and institutions. To circumvent these problems, AJR uses an instrumental variable (IV) for expropriation risk in an equation determining GDP per capita across previously colonized countries.

AJR argues that during the colonial era, Europeans were more likely to settle in places where they had a lower risk of dying from disease. Colonies where Europeans settled developed institutions that protect property better than colonies where Europeans did not settle. The article argues that, in the long run, the direct effects of mortality and European settlement on national income faded, while the indirect effect through property-rights institutions persisted. This argument motivates the use of potential European settler mortality rates as an instrument for the risk of capital expropriation. The AJR IV estimates of the effect of expropriation risk on GDP per capita are large, explaining much of the variation in income across countries.

The historical sources containing information on mortality rates during colonial times are thin, which makes constructing a series of potential European settler mortality rates challenging. AJR constructs this series by combining the mortality rates of soldiers (Curtin 1989, 1998), laborers (Curtin et al. 1995), and bishops (Gutierrez 1986) from different time periods, mostly prior to the twentieth century. Researchers have been eager to use this new series, particularly given its promise as an instrumental variable for institutions. Currently, over 20 published articles, and many more working papers, use the AJR settler mortality data in their econometric analyses.

*Albouy: University of Michigan, Department of Economics, Lorch Hall, 611 Tappan St., Ann Arbor, MI 48109-1220, and NBER (e-mail: albouy@umich.edu). I thank Raj Arunachalam, Raphael Auer, Pranab Bardhan, Christina Berkley, Chris Blattman, David Card, Brad DeLong, Gregory Clark, William Easterly, Rob Gillezeau, Tarek Hassan, Jim Hines, Chang-Tai Hsieh, Michael Jansson, Chad Jones, Annalisa Leibold, Ian McLean, Ted Miguel, Kris Mitchener, Marcelo Moreira, Maurice Obstfeld, Rohini Pande, Gerard Roland, Christina Romer, David Romer, Emmanuel Saez, Andrei Shleifer, Francesco Trebbi, and the participants at the Berkeley Development Lunch and the Economic History and Macroeconomics Seminars for their help, input, and advice. I am very grateful to Daron Acemoglu, Simon H. Johnson, and James A. Robinson for providing me with data, and for sharing with me a preliminary response and later formal responses to my work. Any mistakes are my own.

[†] To view additional materials, visit the article page at http://dx.doi.org/10.1257/aer.102.6.3059. To view the Appendix or a rejoinder to the reply to this comment, please visit http://www.nber.org/papers/w14130.

This comment argues that there are several reasons to doubt the reliability and comparability of their European settler mortality rates and the conclusions that depend on them. First, out of 64 countries in the sample, only 28 countries have mortality rates that originate from within their own borders. The other 36 countries in the sample are assigned rates based on conjectures the authors make as to which countries have similar disease environments. These assignments are generally unfounded and potentially contradictory. Six assignments are based on an incorrect interpretation of former colonial names for Mali. Another 16 assignments are extrapolated from thin bishop mortality data in Latin America from Gutierrez (1986), using a "benchmarking" procedure that can produce highly contradictory rates, depending on how the data are benchmarked. At a minimum, the sharing of mortality rates across countries requires that statistics be corrected for clustering (Moulton 1990). This correction alone noticeably reduces the significance of the results. If, in the hope of reducing measurement error, the 36 conjectured mortality rates are dropped from the sample, the point estimates relating mortality rates with expropriation risk become substantially smaller, particularly in the presence of covariates, which often gain significance.

Second, the mortality rates never come from actual European settlers, although some settler rates are available in the authors' sources. Instead, the data come primarily from European and American soldiers in the nineteenth century. In some countries, rates apply to soldiers at peace in barracks, while in others the rates apply to soldiers on campaign. As is well known, soldiers on campaign typically have higher mortality from disease. This causes problems as AJR uses rates campaigns more often in countries with greater expropriation risk and lower GDP, artificially favoring the article's hypothesis. In a few countries, the data include the peak mortality rates of African laborers, but these are not comparable with average soldier mortality rates. Controlling for the source of the mortality rates weakens the empirical relationship between expropriation risk and mortality rates substantially. Furthermore, if these controls are added and the conjectured data are removed, the relationship virtually disappears, suggesting that it is largely an artifact of the data's construction. Additional data provided by Acemoglu, Johnson, and Robinson (2005), an earlier reply to an earlier version of this comment, do not restore this relationship.[1]

Without a robust relationship between expropriation risk and mortality rates, the AJR IV estimates of the effect of expropriation risk on GDP per capita suffer from weak instrument problems: point estimates are unstable, and corrected confidence intervals are often infinite.

Section I below discusses problems with the settler mortality data, which should interest researchers using them, or any readers of any work employing them. Section II uses the same IV regression model used by AJR and tests the robustness and sensitivity of their hypothesis to problems in the mortality data.

[1] Albouy (2008) also discusses AJR's inconsistent mortality rate choices when multiple rates are available, and demonstrates how the empirical results are sensitive to these choices.

I. Problems with the Settler Mortality Data

The mortality rate data are constructed in four steps, as described in the AJR data Appendix. In their first step, the data include average mortality rates from a table in Curtin (1989, p. 7–8) of European soldiers from disease (not combat) in the early to mid-nineteenth century. In step two, the data also include observations from a selection of military campaigns in Curtin (1998), mainly from the late nineteenth century. The Appendix states that when more than one rate is available, the earliest rate is chosen. Step three incorporates the peak mortality rates of African laborers who were moved to foreign disease environments in the early twentieth century, seen in Curtin et al. (1995). Also in step three, mortality rates are assigned to neighboring countries on the premise that they have similar disease environments. Finally, in the fourth step, three mortality rates of Latin American bishops in the seventeenth and eighteenth centuries from Gutierrez (1986) are multiplied by a factor of 4.25, to benchmark them to a rate taken from a French campaign in Mexico over 1862 to 1863, and applied to 16 countries.

Mortality rates are expressed in the number of deaths per year per thousand at risk, and are cataloged in Table A1. In order to keep the discussion here brief, considerable detail is left to an Appendix on my website.

A. *The Matching of Mortality Rates to Neighboring Countries*

Thirty-six countries out of 64 have mortality rates that originate from outside their own borders. The authors state in their data Appendix (p. 3) that they assign "a mortality number to a country if it neighbors a country for which we have data and has the same disease environment." The text, however, does not document how similar disease environments are determined. In regions such as sub-Saharan Africa and Southeast Asia, neighboring countries in the data have widely differing rates, and so the series is very sensitive to how neighboring countries are chosen among possible candidates.

The authors' data Appendix argues (p.1) that large differences in mortality occur between neighboring countries "because there exists substantial variation in disease environment, particularly for malaria, even in neighboring areas," citing differences in microclimates.[2] Yet substantial variations in disease environments undermine the justification for assigning the same mortality rates to neighboring countries. With the paucity of documentation presented, it is difficult to defend the methodology of assigning very different rates to some neighboring countries, and then sharing the same rates across others. If instead disease environments vary little across neighboring countries, then much of the variation seen in the data is due to measurement error, and true mortality rates are likely collinear with other variables suspected to affect institutions or GDP. Either horn of this dilemma poses serious problems to the mortality series.

One set of mortality assignments, illustrated in Figure 1, comes from mortality rates that are all from French campaigns in western Mali, reported in Curtin (1998). A close reading of the text reveals the geographic origin of these rates,

[2] This passage arises when the authors assign a rate of 17.7 to Malaysia and 170 to neighboring Indonesia. In fact, Curtin (1989, p. 17–18) does not ascribe this difference to microclimates, but rather to the fact that soldiers were at war in Indonesia.

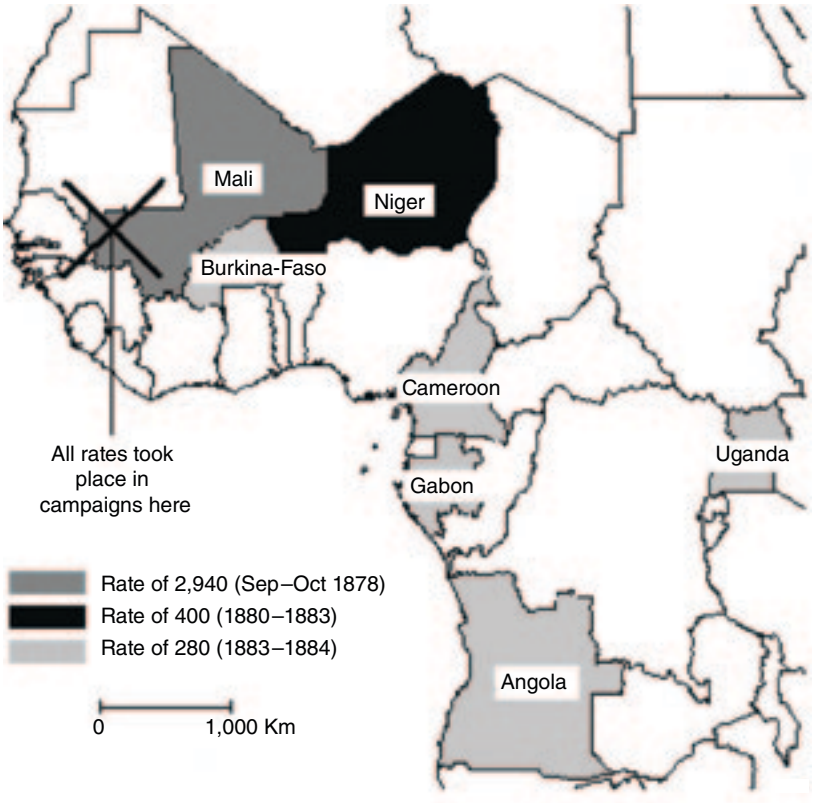

FIGURE 1. ASSIGNMENT OF MORTALITY RATES FROM MALI

making the AJR assignments difficult to explain. They appear to originate from a misguided interpretation of the changing geographic names for Mali, as explained in my Appendix. Summarizing briefly,

- Mali is assigned a rate of 2,940 from an acute yellow fever epidemic that killed 49 percent of an expeditionary force from September to October 1878 (Curtin 1998). AJR annualizes the rate, multiplying it by 6.[3]
- Niger is assigned a rate of 400 from 1880 to 1883 (Curtin 1998, p. 85; this rate is taken from a table labeled "Haut-Senegal-Niger," a territory that once held Niger as well as Mali).
- Burkina Faso, Cameroon, Gabon, Angola, and Uganda are assigned a rate of 280 from 1883 to 1884 (Curtin 1998, p. 238; this rate is taken from an entry for the "French Soudan," a territory that once held Burkina Faso as well as Mali).

There are two fundamental problems with these assignments. First, since all three rates come from western Mali, there is no possible logical basis for assigning each

[3] According to Curtin (1998, p. 81), the rate of 2,940 is an overestimate: because of acquired immunity, "the annual rate and the rate of loss over two months [490] would have been about the same." Averaging the mortality rates for Mali over time produces a rate of 478.2. As shown in the Appendix, replacing the rate of 2,940 with 478.2 lowers the significance of the results substantially.

of these rates to different countries. Second, there is no apparent justification for assigning rates from Mali to countries as far away as Angola and Uganda. The 6 countries with rates taken from Mali have neighbors with widely varying rates, from 78.2 in Algeria (which borders Niger) to 2,004 in Nigeria (which borders Niger and Cameroon). This large variation illustrates how assigning mortality rates from neighboring countries may be very sensitive to choice.

The procedure for assigning mortality rates to 16 Latin American countries using bishop data in Gutierrez (1986) also raises questions. Gutierrez does not provide mortality rates by country: rather, he categorizes cities with bishops into low-, medium-, and high-temperature regions, and assumes that cities with similar temperatures have similar disease environments, but never establishes that the disease environments within these regions are similar.[4]

The bishop rates (Gutierrez 1986) are based on 4, 5, and 10 deaths out of at-risk populations of 24, 28.5, and 30.5 bishops in each region over 10 years, resulting in mortality rates of 16.7, 17.5, and 32.8. These rates are not significantly different from each other, or from mortality rates of similarly aged contemporary males in Sweden of 18.32 (Sundbärg 1905), or from soldiers in barracks in England (15.3) or France (20.17) (Curtin 1989).[5] In his abstract, Gutierrez (1986) writes that the life expectancy at age 40 for bishops was 20.3 years in Latin America relative to 29 years in France, implying that mortality was about 43 percent higher than in Europe, with the difference accounted for only by deaths in the high-temperature region. Within Latin America, bishops born in Europe died at rates slightly lower than those born in the New World. This evidence suggests that settler mortality in most of Latin America was not much higher than in Europe.

Yet in the data the authors multiply the bishop mortality rates by 4.25 to benchmark them to a mortality rate in Mexico, from French soldiers campaigning from 1862 to 1863. Here, 71 out of a thousand died from disease, 4.25 times the low-temperature bishop rate of 16.7. With the many rates available in the sources, there are many other alternatives to benchmark the data, but AJR (p. 1383) claims that "alternative methods produce remarkably similar results."[6] As I document in my Appendix, alternative methods in fact produce remarkably dissimilar results, most of them lower. Across areas, the ratio of actual soldier to bishop mortality rates varies from 0.98 to 10.80, rather than staying constant as assumed.[7] The benchmarking system adopted for Latin America implies that Chile, Argentina, and Mexico are 4 times deadlier than the United States, as the latter is given a rate of 15 from US soldiers in the North from 1829 to 1838, a period of relative peace. In addition, the Mexican rate of 71 AJR uses is not annualized; based on dates and troop numbers in Reynaud (1898), I annualize the rate to 61. This rate is remarkably close to the

[4] A map showing the AJR assignments is given in my Appendix Figure A1. Gutierrez states (1986, p. 33, my translation) "we cannot study in a profound way the influence of climate on the mortality of Latin-American bishops in the seventeenth and eighteenth centuries, given the small number of observations, the diversity of environmental situations of which we do not know well the characteristics, and finally the lack of knowledge of the diseases which could affect adults having survived the perils of diseases in infancy and youth."

[5] An *F*-test that all three regions have the same mortality rate is not rejected at a level of 12 percent.

[6] In Acemoglu, Johnson, and Robinson (2005, p. 35), the authors propose a benchmarking system that produces a mortality rate for low-temperature regions of 15.4, close to the original bishop mortality rate of 16.7.

[7] In particular, see Appendix Table A2. The Mexican extrapolation itself raises issues as the French soldiers spent much time in Veracruz, a high-temperature area (Reynaud 1898, p. 102–22). Benchmarking the annualized rate to the high-temperature area lowers the benchmarking factor from 4.25 to 1.86.

mortality rate of white Union soldiers of 53.4 during the contemporary US Civil War (1861–1865), reported in Adams (1952).

The countries with mortality rates inferred from Mali and Mexico account for 22 of the 36 countries with conjectured rates. There are other issues with the remaining 14. For example, the rate of 14.9 for Hong Kong applies to a British force campaigning 1,200 miles away in Northern China, close to Beijing, over a period of 107 days. When annualized, the rate is 50.6 (Army Medical Department 1862). Moreover, as reported in Acemoglu, Johnson, and Robinson (2005), British soldiers actually in Hong Kong during peacetime died at a rate of 285 from 1842 to 1845 (Tulloch 1847)—19 times the original AJR rate—justifying one characterization of Hong Kong as "an unhealthy, pestilential, unprofitable, and barren rock" (Cantlie 1974, p. 480).[8] Clearly, the AJR method of assigning rates to neighboring countries is not just unreliable, but often deeply flawed, generating rates that may be far too high or too low.

B. *Campaigning Soldiers and African Laborers*

The cited works by Curtin are concerned primarily with the health and mortality of soldiers during the European conquests of the nineteenth century.[9] Accordingly, he took as given the current circumstances and living conditions of the soldiers when comparing their mortality rates. These rates do not necessarily provide a good proxy for potential European settler mortality, which would ideally compare settlers with similar living conditions, subject to the constraints imposed by their environments. Living conditions have a large effect on mortality rates from disease. Curtin (1989) discusses how clean water and adequate sewage disposal can drastically lower mortality rates from waterborne diseases such as typhoid and other gastrointestinal infections. Adequate shelter, nutritious food, and quinine prophylactics, long known to protect against malaria, also lower mortality from disease.

Variation in disease due to living conditions seriously affects the mortality data. One reason for this is that AJR combines the mortality rates (from disease alone) of soldiers in barracks with rates from soldiers on campaign, without adjustment. Yet Curtin emphasizes differences between what he terms "barracks rates" and "campaign rates" (this *exact* terminology is used repeatedly in Curtin 1998), asserting that "one of the fundamental facts of military medical experience [is that] troops in barracks are much healthier than troops on campaign, even disregarding losses from combat," (Curtin 1989, p. 4). Soldiers on campaign took fewer precautions against disease and were less likely to have safe water, fresh food, decent shelter, or sewage disposal. Consequently, "[t]he disease toll for soldiers on campaign was inevitably higher than it was in peacetime" (Curtin 1998, p. xi).

Curtin (1998) documents how during campaigns, mortality from malaria typically increases by more than 100 percent, from gastrointestinal infections by more than 200 percent, and from typhoid by more than 600 percent, resulting in mortality rates 66 to

[8] Many valuable sources are cited in Acemoglu, Johnson, and Robinson (2005), including Tulloch (1847), Cantlie (1974), Balfour (1845), and others mentioned in the Appendix.

[9] This is evident in Curtin (1989, p. xiii): "This book is a quantitative study of the relocation costs among European soldiers in the tropics between about 1815 and 1914," and the title of Curtin (1998): *Disease and Empire: The Health of European Troops in the Conquest of Africa*.

2,000 percent higher than barracks rates.[10] While AJR emphasizes the role of tropical diseases such as malaria and fever, much of the variation in the mortality data is due to digestive diseases that can occur outside the tropics when conditions are poor. Even in Europe, where barracks rates are usually below 25 (Curtin 1989), campaign rates rose as high as 332, seen by the British in the Netherlands in 1809 (Balfour 1845, p. 198).[11]

Curtin often discusses whether a mortality rate is from a campaign or not, making it possible to code a variable indicating which rates are from a campaign. In cases not discussed by Curtin, I code a rate as from a campaign when over half of soldier time was spent campaigning. Details of my coding are given in the Appendix.

Except in highly unusual circumstances (e.g., at Magdala in 1868), campaign rates tend to be higher than barracks rates in a given country, although there is no stable relationship between the two. The distinction between barracks and campaign rates affects the analysis as AJR uses campaign rates more often for countries with high risk of capital expropriation and low GDP per capita. For example, the United States and Canada are given barracks rates of 15 and 16.1, which are much lower than campaign mortality rates during the Civil and Revolutionary Wars—and much lower than the initial mortality rates of actual European settlers in the seventeenth century. Latin American countries are given campaign rates benchmarked through Mexico, making them appear comparatively much deadlier than most evidence suggests. Thus, measured mortality rates are endogenous: places with lower future security of property rights and lower output per capita essentially suffer from positive measurement error in their mortality rates. This creates artificial support for the hypothesis that mortality is negatively correlated with expropration risk and GDP per capita.[12]

The effects of campaigning on mortality are evident in North Africa, where according to Curtin (1989) mortality is similar to southern Europe in more peaceful conditions. This is seen in the rate of 16.3 for Malta, located just east of Tunisia, which is below Curtin's rate of 20.17 for France.[13] Instead, the mortality rates taken from campaigns are about 4 times as high: 63 for Tunisia, 67.8 for Egypt, and 78.2 for Algeria and Morocco. Most of these deaths were from typhoid and other non-tropical digestive diseases, with tropical malaria playing only a minor role (Curtin 1989, 1998).[14]

[10] Curtin's distinction is only twofold: he uses the terms "peacetime" and "barracks" interchangeably, as he does with the terms "campaign" and "expedition." Acemoglu, Johnson, and Robinson (2005, 2006) contain a threefold distinction between "peacetime," "expedition," and "wartime" rates, with the claim that peacetime and expedition rates are comparable with each other but not with wartime rates. This distinction is not found in Curtin's work, and seems contrary to Curtin's views since he emphasizes that higher disease mortality rates during expeditions and wartime are primarily due to living conditions, rather than actual fighting. Furthermore, the rates for Algeria, Indonesia, Mexico, and Sudan are from violent conflicts, which seem worthy of the term "wartime," despite the authors' claims that no wartime rates are used in the data.

[11] This source is cited in Acemoglu, Johnson, and Robinson (2005), although it does not mention these rates.

[12] AJR (footnote 17) admits that the data contain measurement error, but that it "does not lead to inconsistent estimates of the effect of institutions on performance." This is true only if measurement error is uncorrelated with the error term in the equation determining log GDP per capita, which does not appear to be the case.

[13] "Climatically the south shore of the Mediterranean was much like the north shore in Italy or southern France … The high Algerian figure [78.2] in the 1830s was certainly the result of campaigning in the conquest period. Within a decade or so, the Algerian death rate was close to the rates of the Mediterranean islands" (Curtin 1989, p. 17).

[14] Deaths from digestive diseases also play a large role in the rates for Mexico, India, and Vietnam. This may have more to do with preexisting poverty than with climate: Curtin (1998, p. 113) writes "Typhoid had become a 'tropical disease'—because the tropical world is poor, not because of climate." Earle (1979) estimates that in Virginia from 1618 to 1624, British settlers suffered a mortality rate of 283, primarily from dysentery and typhoid.

Table 1—Relationship of Main Variables to Campaign and Laborer Indicators

Dependent variable	Log mortality (1)	Expropriation risk (2)	Log GDP (3)
Original sample (64 countries)			
Campaign indicator	1.51	−1.40	−1.04
	(0.30)	(0.43)	(0.28)
Laborer indicator	1.68	−2.36	−1.96
	(0.27)	(0.74)	(0.33)
R^2	0.30	0.23	0.28
Correlation with log mortality			
Full	1.00	−0.52	−0.68
Partial, controlling for indicators	1.00	−0.36	−0.58

Notes: Expropriation risk is "Average protection against expropriation risk 1985–1995" as measured on a scale from 0 to 10, where a higher score represents greater protection by Political Risk Services. The original log mortality is the logarithm of European settler mortality rates from AJR. Heteroskedasticity-robust standard errors in parentheses. Partial correlations control for campaign and laborer indicators.

Another source of incomparability comes from the use of mortality rates from African laborers, coerced to move to foreign environments under harsh conditions (Curtin et al. 1995). Comparing rates from Africa in Curtin (1968), the AJR Data Appendix argues that the laborer rates provide a lower bound for soldier rates, as black soldiers had lower average mortality rates than white soldiers. Yet the rates used are from harshly treated black laborers, not soldiers. Second, all of the rates taken from Curtin et al. (1995) are *maximum* rates, and not average rates, as in Curtin (1968): in the Congo, the maximum rate was 240, while the average rate—listed in the same paragraph—was 100; in Kenya the maximum rate was 145, and no average is reported.[15]

The regression results in Table 1 make it clear that campaign and laborer rates are taken not only from places with higher measured mortality, which should occur mechanically, but more surprisingly from places with lower GDP per capita and worse property-rights institutions: in all instances, these relationships are highly significant at levels smaller than 1 percent. Also, when the data indicators are controlled for, the correlations between mortality and the other two variables fall, especially with the measure of expropriation risk.

Figures 2A and 2B present scatter plots relating mortality to expropriation risk and income per capita data, indicating whether a data point is conjectured from another country or taken from campaigning soldiers or African laborers. From these figures it is possible to see how all of the highest rates come from laborers and campaigning soldiers, and how the conjectured data largely reinforce patterns in the data.[16]

[15] Passages from Curtin et al. (1995), quoted in the Appendix, make it clear that the mortality rates are maxima.

[16] Among the nonconjectured rates, 11 are from barracks, 15 from campaigns, and 2 from laborers. Appendix Table A3 documents the correlation structure and how it biases the estimates in greater depth.

II. Sensitivity of the Empirical Results

The above discussion raises questions about any empirical results based on the constructed potential settler mortality data. For the sake of brevity, only results from the original article, AJR (2001), are examined here.[17]

The econometric model can be written as the combination of the first-stage and second-stage equations

$$r_i = \beta m_i + \upsilon_i; \tag{1}$$

$$y_i = \alpha m_i + \varepsilon_i, \tag{2}$$

where i indexes colonial countries, y_i is log GDP per capita, r_i is expropriation risk, m_i is log potential settler mortality, and υ_i and ε_i are error terms, with $E[m_i \upsilon_i] = 0$ by construction.[18] IV estimates require an instrument that is *relevant* ($\beta \neq 0$) and *excludable* ($E[m_i \varepsilon_i] = 0$). Letting $\pi = \alpha\beta$ and $\xi_i = \alpha\upsilon_i + \varepsilon_i$, the reduced form of the second-stage equation is given by $y_i = \pi m_i + \xi_i$. By the principle of indirect least squares, the IV estimator of α is the ratio of the ordinary least squares (OLS) estimates of π and β; i.e., $\hat{\alpha}_{IV} = \hat{\pi}_{OLS}/\hat{\beta}_{OLS}$. The analysis here first considers the OLS estimate of β, and afterwards the IV estimate of α.

Because mortality rates are shared by countries, the residuals are correlated because of clustering effects (see Moulton 1990). This invalidates the conventional standard errors and test statistics in the original paper, which assumes independent, homoscedastic errors. The standard procedure used to correct for these clustering effects, as well as heteroscedasticity (Froot 1989, Wooldridge 2002), is applied below.

More fundamentally, it is worthwhile to examine how sensitive the results are to robustness checks that account for the weaknesses in the data documented above. One check is to drop the 36 countries with conjectured mortality rates that originate from outside their own borders—including the benchmarked Latin American data—leaving a sample of 28 countries.

This check is similar, but not identical, to the check reported in columns 3 and 4 of the AJR Appendix Table A5 labeled "Earliest Available Data," with 30 countries (31 in AJR 2000), which is supposed to retain rates derived from their first two data-construction steps. Yet the AJR check retains Niger, Burkina Faso, Gabon, Guyana, and Singapore even though those rates are conjectured from elsewhere: the source of Niger, Burkina Faso, and Gabon's rate from Mali is already explained in Figure 1; Guyana's rate is extrapolated from French Guiana (Curtin 1989); Singapore's rate is extrapolated from the city of Penang, Malaysia, well to the north (Curtin 1989).[19] The AJR check omits Ghana and Nigeria, whose rates are native: Ghana's rates are from Gold Coast locations that span Dixcove to Accra, all within Ghana's modern borders; Nigeria's are from the Niger delta (Curtin 1998), entirely within Nigeria.

[17] The European settler mortality series features prominently in Acemoglu and Johnson (2005), Easterly and Levine (2004), and Rodrik, Subramanian, and Trebbi (2004), among others.

[18] Control variables may be accounted for by having all of the above variables refer to the residual projections of the original variables, after being regressed on the controls.

[19] Gabon is not listed in the AJR (2001) Appendix Data Table (A2) but without it the sample is missing one observation.

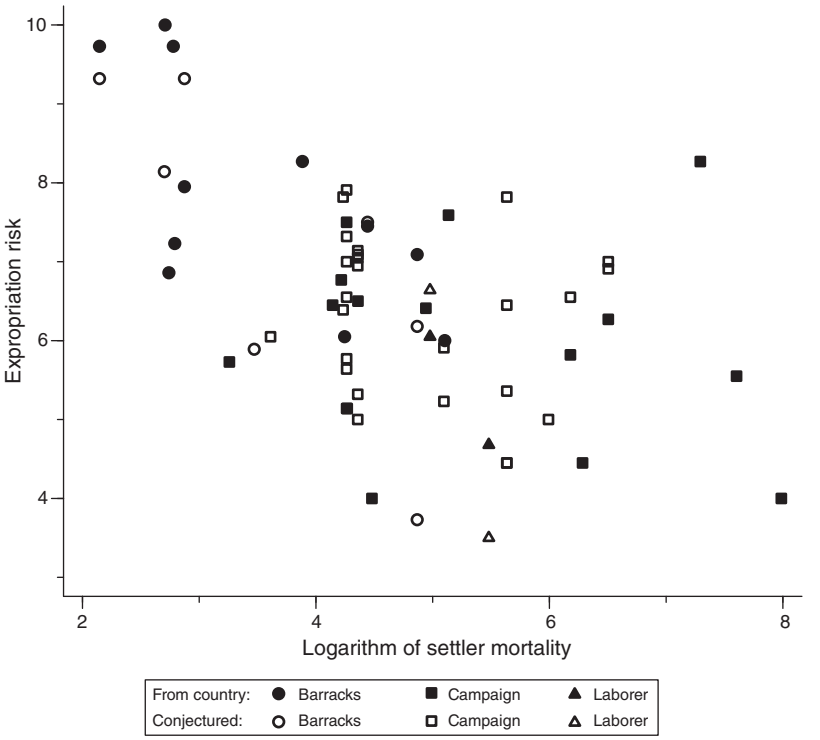

FIGURE 2A. EXPROPRIATION RISK AND SETTLER MORTALITY ACCORDING TO MORTALITY RATE CHARACTERISTICS

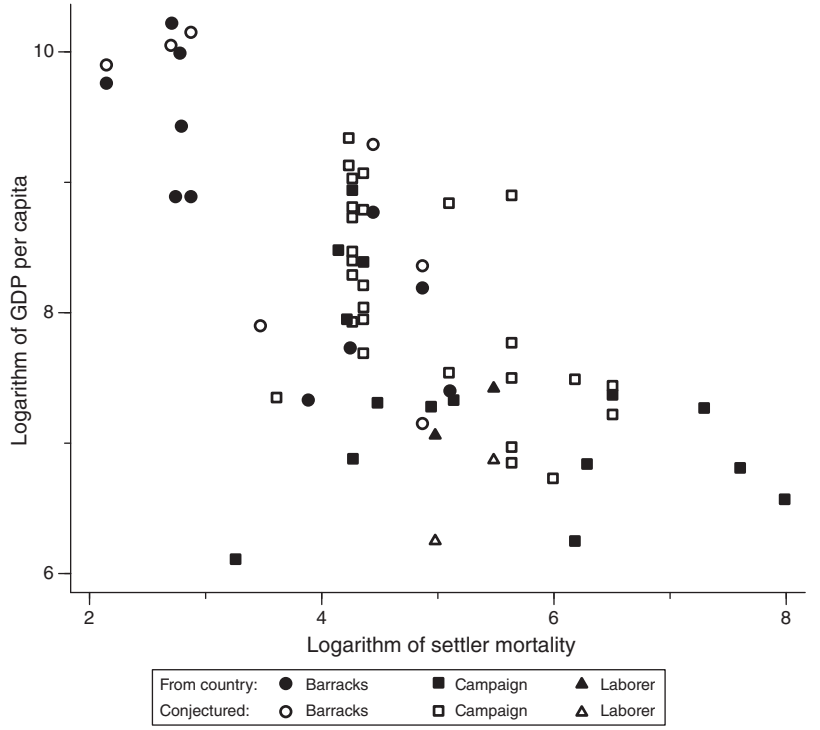

FIGURE 2B. INCOME PER CAPITA AND SETTLER MORTALITY ACCORDING TO MORTALITY RATE CHARACTERISTICS

The AJR check also retains Congo but not Kenya, although both should be omitted since they originate from AJR's third data step using laborer rates from the twentieth century. My check retains Congo and Kenya, since the rates originate from within these countries and are controlled for in the next check.

The second robustness check adds two control variables that indicate when a mortality rate is taken from campaigning soldiers or from imported African laborers to deal with the comparability issues of different data sources. As is standard, these variables are included in both the first- and second-stage regressions.

The third check adds new data introduced by Acemoglu, Johnson, and Robinson (2005). These data provide native rates for Australia, Bahamas, Guyana, Hong Kong, Honduras, and Singapore—expanding the sample size of nonextrapolated rates to 34—as well as unique rates for Sierra Leone and Trinidad and Tobago. These rates, as well as indicators for soldier campaign rates, laborer rates, and nonextrapolated rates are reported in Appendix Table A1.

A. *First-Stage Estimates*

Table 2 presents the first-stage estimates of β obtained when one applies the two checks described above, using controls in the original paper. The point estimates and standard errors for the control and indicator variables are reported in Table A4. Columns 2 through 5 use geographic controls: latitude (measured in absolute degrees); omitting "Neo-Europes" (Australia, Canada, New Zealand, and the United States); continent indicators (Asia, Africa, and "Other," with the Americas as the reference); and combining latitude and continent indicators. These correspond to columns 2, 3, 7, and 8 in Table 4 of AJR. Column 6 controls for the percentage of the population of European descent in 1975, like Table 6 of AJR, column 3. Column 7 controls for the percentage of the population living where falciporum malaria is endemic in 1994, as in Table 7 of AJR, column 1.

The first-stage results with the original data in panel A report that log mortality is usually a significant predictor of expropriation risk. The clustering adjustment does increase the size of the standard errors, making β insignificant at the 10 percent level in column 5.

Panel B applies the first robustness check, dropping conjectured rates, which causes the standard errors to widen and the point estimates of β to fall, albeit only noticeably with controls.[20] Interestingly, the controls generally become more significant, despite the smaller sample size. With the original sample, most control variables are not significant and lower estimates of β by less than half. Accordingly, the authors only consistently use latitude as a control variable. Yet in the smaller, more reliable subsample, all of the control variables grow appreciably in significance, while the point estimates of β fall considerably more. Thus, the conjectured mortality rates appear to mask the collinearity between the the controls and the more

[20] Although the smaller sample does reduce the power of statistical tests, its greater accuracy should raise the expected value of the point estimate β by reducing attenuation, at least in the case of classical measurement error and controls uncorrelated with mortality.

TABLE 2—FIRST-STAGE ESTIMATES
(*Dependent variable: expropriation risk*)

Control variables	No controls (1)	Latitude control (2)	Without Neo-Europes (3)	Continent indicators (4)	Continent indicators and latitude (5)	Percent European in 1975 (6)	Malaria in 1994 (7)
Panel A. Original data (64 countries, 36 mortality rates)							
Log mortality (β)	−0.61	−0.52	−0.40	−0.44	−0.35	−0.42	−0.52
{homoscedastic standard error}	{0.13}	{0.14}	{0.13}	{0.17}	{0.18}	{0.14}	{0.18}
(heteroscedastic-clustered SE)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.22)
p-value of log mortality	0.001	0.01	0.03	0.04	0.11	0.03	0.02
p-value of controls	—	0.17	—	0.40	0.34	0.02	0.40
Panel B. Removing conjectured mortality rates (28 countries and mortality rates)							
Log mortality (β)	−0.59	−0.42	−0.32	−0.31	−0.22	−0.29	−0.38
(heteroscedastic standard error)	(0.19)	(0.22)	(0.19)	(0.20)	(0.23)	(0.21)	(0.24)
p-value of log mortality	0.01	0.07	0.10	0.13	0.35	0.19	0.12
p-value of controls	—	0.05	—	0.01	0.002	0.015	0.10
Panel C. Original data, adding campaign and laborer indicators (64 countries, 36 mortality rates)							
Log mortality (β)	−0.45	−0.39	−0.31	−0.37	−0.30	−0.27	−0.36
(heteroscedastic-clustered SE)	(0.18)	(0.20)	(0.17)	(0.22)	(0.23)	(0.19)	(0.21)
p-value of log mortality	0.020	0.06	0.09	0.09	0.20	0.17	0.10
p-value of *indicators*	0.16	0.22	0.31	0.26	0.35	0.19	0.21
p-value of controls	—	0.27	—	0.75	0.66	0.02	0.41
Panel D. Removing conjectured mortality and adding campaign and laborer indicators (28 countries and mortality rates)							
Log mortality (β)	−0.35	−0.21	−0.18	−0.25	−0.14	−0.20	−0.22
(heteroscedastic standard error)	(0.22)	(0.25)	(0.22)	(0.23)	(0.26)	(0.23)	(0.26)
p-value of log mortality	0.12	0.42	0.42	0.28	0.60	0.39	0.40
p-value of *indicators*	0.03	0.06	0.08	0.34	0.44	0.14	0.07
p-value of controls	—	0.07	—	0.03	0.01	0.05	0.28
Panel E. Removing conjectured rates, adding campaign and laborer indicators, and revising with new data (34 countries and rates)							
Log mortality (β)	−0.41	−0.30	−0.19	−0.31	−0.19	−0.24	−0.30
(heteroscedastic standard error)	(0.20)	(0.21)	(0.21)	(0.21)	(0.22)	(0.22)	(0.23)
p-value of log mortality	0.05	0.17	0.36	0.16	0.39	0.28	0.20
p-value of *indicators*	0.02	0.04	0.04	0.30	0.41	0.07	0.06
p-value of controls	—	0.13	—	0.20	0.22	0.05	0.33

Notes: Standard errors, assuming uncorrelated homoscedastic errors, are shown in braces ({}) in panel A. All other standard errors and tests adjust for heteroscedasticity and clustering effects, where clusters are defined by countries sharing the same mortality rate; *p*-value of controls are probability values from standard *F*-tests of whether the controls are significant in the regression; *p*-value of indicators refers to an *F*-test of the joint significance of the campaign and laborer indicators. See Appendix Table A1 for indicators of whether a country's data is conjectured or is a rate from campaigning soldiers or laborers. "Neo-Europes" consist of Australia, Canada, New Zealand, and the United States, and are based off of three mortality rates. The three continent variables included are Africa, Asia, and Other, taken from AJR, consists of Australia, Malta, and New Zealand. Percent of European Descent in 1975 is the percent of the population of European descent in 1975 from AJR. Malaria in 1994 refers to percent of the population with endemic malaria in 1994 in Sachs and Gallup (2001), which does not contain data for Malta and the Bahamas. Revisions with new data from Acemoglu, Johnson, and Robinson (2005) are discussed in the Appendix and given in Table A1. See Table 1 and the text for more detail.

accurately measured rates.[21] Altogether, β is only significant at the 5 percent level in the specification without controls.[22]

Note that using the AJR "Earliest Available Data" sample, as seen in my Table A5, causes the point estimates of β to be almost the same as in the original sample. The standard errors do rise, however, lowering the significance of β below 5 percent in specifications 3 through 7, raising some of the weak instrument problems discussed below.

Using the original sample again, panel C demonstrates that controlling for whether a mortality rate comes from soldiers on campaign or from African laborers makes log mortality insignificant at the 5 percent level in all specifications with controls. This reduction in significance is the result of lower point estimates for β, as well as larger standard errors, and thus does not just come from the indicators using additional degrees of freedom. The campaign and laborer indicators, whose coefficients are reported in Table A4 of the Appendix, have negative signs, but tend to have limited statistical significance in the original sample.[23]

Panel D reveals that the campaign and laborer indicators become much more significant once the conjectured data are dropped. In all but one specification, the indicators and the control variables are more significant than settler mortality. In the specification without controls, seen in column 1, settler mortality is insignificant at a size of 10 percent, while in the other specifications with controls it is insignificant at 25 percent.[24]

Panel E, using new data from Acemoglu, Johnson, and Robinson (2005) with both checks in place, does improve the relationship between mortality and property rights somewhat, but not by enough to make the relationship robust. The expanded data also make the indicator variables more significant.[25]

[21] Appendix Table A5 also shows that dropping Congo and Kenya weakens the first stage further. Furthermore, Albouy (2008) reveals that using the unbenchmarked bishop mortality rates directly for Latin America lowers the first-stage estimates more than just dropping them, as is done here.

[22] Acemoglu, Johnson, and Robinson (2006) claims the results are robust when Africa is excluded from the sample. As explained in Albouy (2008), without Africa there are only 37 rates, of which only 13 are not conjectured. Also, there is no compelling reason for why Africa should be excluded. In fact, North Africa, with a hospitable Mediterranean climate but disappointing performance, provides an important counterexample to the theory. Even using the original data, excluding Africa lowers the IV estimate of α to 0.61, putting it close to the direct OLS estimate of equation (2) of about 0.52, which AJR originally rejects as being too small an estimate, motivating the IV approach. Third, there are no controls in the non-Africa sample: results without Africa or conjectured rates, based on 13 countries, are driven by the Neo-Europes—Canada, New Zealand, and the United States. The IV model assumes that European settlers changed property-rights institutions and nothing else which affected growth, an assumption that is clearly violated by these countries, where Europeans imported their entire civilization. The Neo-Europes should be excluded from the sample as they cannot support the AJR theory. A similar point is made by Fails and Krieckhaus (2010). Acemoglu, Johnson, and Robinson (2006) also claim that results are robust to capping mortality rates at 250, which primarily affects mortality rates in Africa. This ad hoc adjustment cancels out much of the variation within Africa and suggests revising the data altogether. Furthermore, other sources of variation that appear specious, such as the mortality difference between the United States and Argentina, become more important once this first adjustment is made.

[23] The rate for Hong Kong is kept at 14.9 and is not labeled a campaign rate, as it is not annualized and as actual data from the area suggest much higher mortality. Annualizing the rate to 50.8 and coding it as a campaign rate produces similar results.

[24] To ensure that results are not dependent on using expropriation risk as the measure of institutions, my Appendix Tables A7 and A8 show results using alternate measures: Constraint on Executive in 1990 and Law and Order Tradition in 1995. These estimates reveal a similar lack of robustness and significance.

[25] In my Appendix Table A6, I add the new data without the robustness checks and find the first-stage results to be weaker than with the original data.

B. *Instrumental Variable Estimates*

When the first-stage estimate of β is not significantly different from zero—a common occurrence in the results seen so far—the relevance assumption needed for IV estimates ($\beta \neq 0$) is not guaranteed, causing a *weak instrument* problem. This introduces a number of statistical pathologies to the IV estimates. Most importantly, inference based on the IV estimate using conventional asymptotic confidence regions (point estimate $\pm$ $t \times$ standard error), based on the Wald statistic, can be grossly incorrect (Dufour 1997). Confidence regions for α of the correct size can be built by inverting the AR statistic proposed by Anderson and Rubin (1949). While using the AR statistic seems unorthodox—producing asymmetric, and sometimes disjointed and unbounded, confidence regions—in the presence of a single weak instrument, it provides correct inference and an exact test as appropriate as a *t*-statistic in OLS. When the instrument is strong, AR and Wald confidence regions are similar, as the latter is not grossly incorrect.[26]

Table 3 presents the IV estimates and confidence regions corresponding to the first-stage results in Table 2. In panel A with the original data, weak instrument problems appear despite the stability of the point estimates. In columns 1 and 2, where the first stage is fairly strong, the AR and Wald 95 percent confidence regions are fairly similar. As the instrument weakens in columns 3 and 4, however, the AR confidence regions widen, until in column 5 it becomes unbounded: as the indirect least squares formula $\alpha = \pi/\beta$ implies, once zero cannot be rejected for β, infinity cannot be rejected for α.

As the robustness checks are applied in panels B through D, these weak instrument problems are aggravated: point estimates become unstable and the confidence regions expand until most of the regions in panels D and E equal the entire real line. Nevertheless, the point estimates of α get larger, which can also be understood through the formula $\alpha = \pi/\beta$, as the checks have a greater effect in reducing β than π.[27] The estimates of α are sometimes implausibly large, often approaching 2 in panel E: this would imply some incredible conclusions: e.g., if Mexico and the United States had the same property rights (a 2.5 point difference) then the GDP per capita ratios of the two countries would go from less than one-third to over 40 in Mexico's favor.[28]

The volatile estimates and unbounded confidence regions for α reveal how instrumental variable inference is frustrated when the first-stage estimate of β is not highly significant, which becomes quite an issue when problems with the mortality data are accounted for.

[26] Moreira (2009) proves that, in the exactly identified case, AR tests are the uniformly most powerful among unbiased tests. The AR confidence regions are said to have "95 percent confidence" because they have 5 percent size. It does not mean that the true α is within this region 95 percent of the time, but that the AR statistic computed is within the first 95 percent of the cumulative distribution of the statistic under the null hypothesis. With a weak instrument, Staiger and Stock (1997) show that conventional *F*-tests of significance for exogenous variables and over-identification tests (e.g., Sargan 1958) for the second stage are invalid. Correctly specified tests depend on parameters that cannot be estimated. Since mortality is a weak instrument in most cases, these test statistics are not reported to save space.

[27] This is seen for the second check in Table 1, as the partial correlation for expropriation risk is reduced more by controlling for the data indicators than that for log GDP per capita.

[28] As shown in Albouy (2008), when the Mali rate is also lowered to a more reasonable number, the estimate of α sometimes becomes large and negative, as the estimate of β becomes small and positive, while the reduced-form estimate of π remains negative. Results are also sensitive to inconsistently chosen rates for Egypt and Sudan.

TABLE 3—INSTRUMENTAL VARIABLE ESTIMATES AND CONFIDENCE REGIONS
(*First-stage dependent variable: expropriation risk; second-stage dependent variable, log GDP per capita,* 1995*, PPP basis*)

Control variables	No controls (1)	Latitude control (2)	Without neo-Europes (3)	Continent indicators (4)	Continent indicators & latitude (5)	Percent European in 1975 (6)	Malaria in 1994 (7)
Panel A. Original mortality (64 *countries,* 36 *mortality rates*)							
Expropriation risk (α)	0.93	0.96	1.24	0.97	1.07	0.92	0.62
Wald 95% conf. region	[0.52, 1.34]	[0.42, 1.50]	[0.35, 2.14]	[0.25, 1.70]	[−0.01, 2.16]	[0.28, 1.56]	[0.23, 1.01]
AR "95%" conf. region	[0.66, 1.83]	[0.64, 2.39]	[0.73, 7.04]	[0.50, 9.02]	$(-\infty, -3.08]$ U [0.41, $+\infty$)	[0.51, 6.45]	[0.28, 1.88]
Panel B. Removing conjectured mortality rates (28 *countries and mortality rates*)							
Expropriation risk (α)	0.87	0.82	1.15	1.12	1.25	0.94	0.71
Wald 95% conf. region	[0.43, 1.31]	[0.13, 1.51]	[−0.10, 2.40]	[−0.17, 2.42]	[−1.18, 3.67]	[−0.33, 2.21]	[−0.53, 1.96]
AR "95%" conf. region	[0.58, 2.01]	$(-\infty, -7.92]$ U [0.38, $+\infty$)	$(-\infty, -5.14]$ U [0.49, $+\infty$)	$(-\infty, -2.25]$ U [0.37, $+\infty$)	$(-\infty, +\infty)$	$(-\infty, -0.94]$ U [0.27, $+\infty$)	$(-\infty, +\infty)$
Panel C. Original data, adding campaign and laborer indicators (64 *countries,* 36 *mortality rates*)							
Expropriation risk (α)	1.09	1.15	1.45	1.06	1.19	1.18	0.66
Wald 95% conf. region	[0.32, 1.87]	[0.12, 2.18]	[−0.01, 2.91]	[0.07, 2.05]	[−0.30, 2.67]	[−0.29, 2.66]	[−0.50, 1.81]
AR "95%" conf. region	[0.62, 5.07]	$(-\infty, -17.59]$ U U [0.60, $+\infty$)	$(-\infty, -8.05]$ U [0.69, $+\infty$)	$(-\infty, -3.28]$ U [0.45, $+\infty$)	$(-\infty, -0.67]$ U [0.29, $+\infty$)	$(-\infty, -1.67]$ U [0.44, $+\infty$)	$(-\infty, +\infty)$ $(-\infty, +\infty)$
Panel D. Removing conjectured mortality and adding campaign and laborer indicators (28 *countries and mortality rates*)							
Expropriation risk (α)	1.02	0.90	1.51	1.23	1.44	1.13	0.64
Wald 95% conf. region	[−0.04, 2.08]	[−1.01, 2.81]	[−1.89, 4.91]	[−0.83, 3.29]	[−3.93, 6.80]	[−1.22, 3.49]	[−2.60, 3.87]
AR "95%" conf. region	$(-\infty, -1.82]$ U [0.36, $+\infty$)	$(-\infty, +\infty)$	$(-\infty, +\infty)$	$(-\infty, +\infty)$	$(-\infty, +\infty)$	$(-\infty, +\infty)$	$(-\infty, +\infty)$
Panel E. Removing conjectured rates, adding campaign and laborer indicators, and revising with new data (34 *countries and rates*)							
Expropriation risk (α)	1.31	1.11	1.91	1.66	1.36	1.72	1.52
Wald 95% conf. region	[−0.19, 2.80]	[−1.35, 3.57]	[−2.62, 6.45]	[−1.23, 4.55]	[−3.61, 6.32]	[−2.07, 5.51]	[−2.22, 5.25]
AR "95%" conf. region	$(-\infty, -2.86]$ U [0.41, $+\infty$)	$(-\infty, +\infty)$ $(-\infty, +\infty)$	$(-\infty, -0.29]$ U [0.17, $+\infty$)	$(-\infty, -0.24]$ U [−0.14, $+\infty$)	$(-\infty, +\infty)$ $(-\infty, +\infty)$	$(-\infty, +\infty)$ $(-\infty, +\infty)$	$(-\infty, +\infty)$ $(-\infty, +\infty)$

Notes: Panels present the instrumental variable estimates of Expropriation Risk on Log GDP per Capita, 1995, PPP basis, using Log Mortality as an instrument, and the control variables and sample selection described in Table 1. Wald 95% Conf. Region are the standard (erroneous) IV confidence regions based on the Wald statistic. AR *"95%"* Conf. Region are the confidence regions calculated from the Anderson-Rubin (1949) statistic as described in the text. Heteroscedasticity and clustering effects are corrected for all confidence regions. See text and Tables 1 and 2 for more details.

III. Conclusion

Given the paucity of instrumental variables in the cross-country growth literature, it is regrettable that the AJR mortality series suffers from severe measurement problems. While broad regions like West Africa and the Caribbean were clearly unhealthy for Europeans, the mortality differences in the series between neighboring countries are largely unreliable. Much of the variation in the mortality data is due to questionable mortality assignments, which often reflect transitory fluctuations or living conditions, rather than actual permanent differences among these countries.

Given the limited data sources currently available, it seems unlikely that a convincing set of settler mortality rates can be constructed. As such, cross-country growth regressions cannot disentangle the effect of settler mortality from that of other variables that may explain institutions and growth, such as geography, climate, culture, and preexisting development, leaving the AJR theoretical hypotheses without a strong empirical foundation. Moreover, any researchers who have used

TABLE A1—MORTALITY RATES AND DATA INDICATORS

	Original mortality	Soldier campaign rate	Laborer rate	Rate from within country	"Benchmarked" Latin-American rate[a]	Revised mortality[b]
Angola	280	x				
Argentina	68.9	x			x	
Australia	8.55					14.1
Burkina Faso	280	x				
Bangladesh	71.41	x		x		
Bahamas	85					189
Bolivia	71	x			x	
Brazil	71	x			x	
Canada	16.1			x		
Chile	68.9	x			x	
Côte d'Ivoire	668	x				
Cameroon	280	x				
Congo	240		x	x		
Colombia	71	x			x	
Costa Rica	78.1	x			x	
Dominican Republic	130					
Algeria	78.2	x		x		
Ecuador	71	x			x	
Egypt	67.8	x		x		
Ethiopia	26	x		x		
Gabon	280	x				
Ghana	668	x		x		
Guinea	483	x				
Gambia	1,470	x		x		
Guatemala	71	x			x	
Guyana	32.18					84
Hong Kong	14.9					285
Honduras	78.1	x			x	95.2
Haiti	130					
Indonesia	170	x		x		
India	48.63			x		
Jamaica	130			x		
Kenya	145		x	x		
Sri Lanka	69.8			x		
Morocco	78.2	x				
Madagascar	536.04	x		x		
Mexico	71	x		x		
Mali	2,940	x		x		
Malta	16.3			x		
Malaysia	17.7			x		
Niger	400	x				
Nigeria	2,004	x		x		
Nicaragua	163.3	x			x	
New Zealand	8.55			x		
Pakistan	36.99	x				
Panama	163.3	x			x	
Peru	71	x			x	
Paraguay	78.1	x			x	
Sudan	88.2	x		x		
Senegal	164.66			x		
Singapore	17.7					20
Sierra Leone	483	x		x		350
El Salvador	78.1	x			x	
Togo	668	x				
Trinidad and Tobago	85			x		106.3
Tunisia	63	x		x		
Tanzania	145		x			
Uganda	280	x				
Uruguay	71	x			x	
US	15			x		
Venezuela	78.1	x			x	
Vietnam	140	x		x		
South Africa	15.5			x		
Zaire	240		x			

Notes: The sample includes the 28 countries indicated by "Rate from within Country," plus 6 countries that have revised, but not original, mortality rates from within: Australia, Bahamas, Guyana, Hong Kong, Honduras, and Singapore. Revised rates for Sierra Leone and Trinidad and Tobago are from more geographically disaggregated data, as they were previously based on data shared with Gambia and the Lesser Antilles. Honduras is recoded as a non campaign rate when the data are revised. See the text and Appendix for further details.

[a] Column indicates the 28 countries included in the sample for panels B, D, and E in Tables 2 and 3.

[b] Results in panel E of Tables 2 and 3 use the eight "Revised Mortality" rates in place of the "Original Mortality" rates.

the AJR mortality series in their analyses may need to reconsider their conclusions in light of the data issues raised here.

REFERENCES

Acemoglu, Daron, and Simon H. Johnson. 2005. "Unbundling Institutions." *Journal of Political Economy* 113 (5): 949–95.

Acemoglu, Daron, Simon H. Johnson, and James A. Robinson. 2000. "The Colonial Origins of Comparative Development: An Empirical Investigation." National Bureau of Economic Research Working Paper 7771.

Acemoglu, Daron, Simon H. Johnson, and James A. Robinson. 2001. "The Colonial Origins of Comparative Development: An Empirical Investigation." *American Economic Review* 91 (5): 1369–1401.

Acemoglu, Daron, Simon H. Johnson, and James A. Robinson. 2005. "A Response to Albouy's 'A Reexamination Based on Improved Settler Mortality Data.'" Unpublished.

Acemoglu, Daron, Simon H. Johnson, and James A. Robinson. 2006. "Reply to the Revised (May 2006) version of David Albouy's 'The Colonial Origins of Comparative Development: An Investigation of the Settler Mortality Data.'" Unpublished.

Acemoglu, Daron, Simon H. Johnson, and James A. Robinson. 2009. "Report on 'The Colonial Origins of Comparative Development: An Investigation of the Settler Mortality Data,' by David Albouy (2008)." Unpublished.

Adams, George W. 1952. *Doctors in Blue: The Medical History of the Union Army in the Civil War*. New York: Henry Schuman.

Albouy, David Y. 2008. "The Colonial Origins of Comparative Development: An Investigation of the Settler Mortality Data." National Bureau of Economic Research Working Paper 14130.

Anderson, T. W., and Herman Rubin. 1949. "Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations." *Annals of Mathematical Statistics* 20 (1): 46–63.

Army Medical Department. 1862. *Statistical, Sanitary and Medical Reports of the Army Medical Department for the Year 1860*. London: Harrison and Sons.

Balfour, Edward. 1845. "Statistical Data for Forming Troops and Maintaining them in Health in Different Climates and Localities." *Journal of the Statistical Society of London* 8 (3): 193–209.

Cantlie, Sir Neil. 1974. *A History of the Army Medical Department*, *Volume I.* London: Churchill Livingstone.

Curtin, Philip D.1968. "Epidemiology and the Slave Trade." *Political Science Quarterly* 83 (2): 190–216.

Curtin, Philip D. 1989. *Death by Migration: Europe's Encounter with the Tropical World in the 19th Century*. New York: Cambridge University Press.

Curtin, Philip D. 1998. *Disease and Empire: the Health of European Troops in the Conquest of Africa*. New York: Cambridge University Press.

Curtin, Philip D., Steven Feierman, Leonard Thompson, and Jan Vansina. 1995. *African History: From Earliest Times to Independence.* 2nd ed. London: Longman.

Dufour, Jean-Marie. 1997. "Some Impossibility Theorems in Econometrics with Applications to Structural and Dynamic Models." *Econometrica* 65 (6): 1365–87.

Earle, Carville V. 1979. "Environment, Disease, and Mortality in Early Virginia." In *The Chesapeake in the Seventeenth Century*, edited by Thad W. Tate and David L. Ammerman, 96–125. Chapel Hill, NC: University of North Carolina Press.

Easterly, William, and Ross Levine. 2003. "Tropics, Germs, and Crops: How Endowments Influence Economic Development." *Journal of Monetary Economics* 50 (1): 3–39.

Fails, Matthew D., and Jonathan Krieckhaus. 2010. "Colonialism, Property Rights, and the Modern World Income Distribution." *British Journal of Political Science* 40 (3): 487–508.

Froot, K. A. 1989. "Consistent Covariance Matrix Estimation with Cross-Sectional Dependence and Heteroskedasticity in Finanical Data." *Journal of Financial and Quantitative Analysis* 24 (3): 333–55.

Gutierrez, Hector. 1986. "La Mortalité des Évêques Latino-Américains aux XVIIe et XVIIIe Siècles." *Annales de Démographie Historique* 1986: 29–39.

Hall, Robert E., and Charles I. Jones. 1999. "Why Do Some Countries Produce So Much More Output Per Worker than Others?" *Quarterly Journal of Economics* 114 (1): 83–116.

Knack, Stephen, and Philip Keefer. 1995. "Institutions and Economic Performance: Cross-Country Tests Using Alternative Measures." *Economics and Politics* 7 (3): 207–27.

La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny. 1998. "Law and Finance." *Journal of Political Economy* 106 (6): 1113–55.

Mauro, Paulo. 1995. "Corruption and Growth." *Quarterly Journal of Economics* 110 (3): 681–712.

Moreira, Marcelo J. 2009. "Tests with Correct Size when Instruments Can Be Abitrarily Weak." *Journal of Econometrics* 152 (2): 131–40.

Moulton, Brent R. 1990. "An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units" *The Review of Economics and Statistics* 72 (2): 334–38.

Raynaud, Gustave A. 1898. *Considérations sanitaires sure l'expédition de Madagascar et quelques autres expéditions coloniales, françaises et anglais.* Paris: Société française d'édition d'art.

Rodrik, Dani. 1999. "Where Did All the Growth Go?" *Journal of Economic Growth* 4 (4): 385–412.

Rodrik, Dani, Arvind Subramanian, and Francesco Trebbi. 2004. "Institutions Rule: The Primacy of Institutions over Geography and Integration in Economic Development." *Journal of Economic Growth* 9 (2): 131–65.

Sachs, Jeffrey D., and John L. Gallup. 2001. "The Economic Burden of Malaria." *The American Journal of Tropical Medicine & Hygiene* 64 (1–2): 85–96.

Sargan, J. D. 1958. "The Estimation of Economic Relationships Using Instrumental Variables." *Econometrica* 29 (3): 393–415.

Staiger, Douglas O., and James H. Stock. 1997. "Instrumental Variables Regression with Weak Instruments." *Econometrica* 65 (3): 557–86.

Sundbärg, G. 1905. "Tab 1: Döde efter alder och kön, 1751–1900." *Statistisk Tidskrift* 1905: 109–32.

Tulloch, A. M. 1847. "On the Mortality of Her Majesty's Troops Serving in the Colonies during the Years 1844 and 1845." *Journal of the Statistical Society of London* 10 (3): 252–59.

Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross-Section and Panel Data*. Cambridge, MA: MIT Press.

This article has been cited by:

1. Daron Acemoglu, , Simon Johnson, , James A. Robinson. 2012. The Colonial Origins of Comparative Development: An Empirical Investigation: Reply. *American Economic Review* **102**:6, 3077-3110. [Abstract] [View PDF article] [PDF with links]