The Colonial Origins of Comparative Development: A Reexamination Based on Improved Settler Mortality Data

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Second Revised Draft
September 27, 2004

I thank Pranab Bardhan, Chris Blattman, Brad DeLong, Chang-Tai Hsieh, Guido Imbens, Michael Jannson, Thomas Lemieux, Edward Miguel, Marcelo Moreira, James Powell, Gerard Roland, David Romer, Emmanuel Saez, and the participants at the Berkeley Development Lunch for their input and advice. I am particularly grateful to David Card and Chad Jones for their lengthy discussions and suggestions, to Francesco Trebbi for providing me with data, and especially to Daron Acemoglu, Simon Johnson, and Jim Robinson for their openness, graciousness, and cooperation. Any errors are my own. Please e-mail any comments to albouy@econ.berkeley.edu.

Abstract

In a seminal contribution, Acemoglu, Johnson, and Robinson (2001) evaluate the effect of property rights institutions on national income using mortality rates of early European settlers as an instrument for the risk of capital expropriation. Going back to AJR's original sources, I find AJR's mortality figures suffer from a lack of geographical relevance, statistical precision, and cross-country comparability. With two revised series, each based on alternative assumptions, I show that the relationship between mortality and expropriation risk is not highly significant or robust, leading to a weak instrument problem which makes instrumental variable estimation problematic and statistical tests very low in power. AJR's results appear to depend on a small number of measured data points. Excluding poorly measured countries in the sample, or using alternate measures of expropriation risk do not overturn my main results.

Introduction

Acemoglu, Johnson and Robinson's seminal paper (2001) – henceforth AJR – has reinvigorated debate over the relation of property rights institutions to economic growth. Following research by Knack and Keefer (1995), Mauro (1995), La Porta et al. (1998) Hall and Jones (1999), Rodrik (1999) and others, AJR endeavor to determine the causal effect of property rights institutions on economic performance. This is complicated by the fact that the positive statistical correlation between institutional and economic measures may reflect reverse effects of the economy on institutional development or the simultaneous influence of unknown factors on both economic output and institutions. AJR's strategy to circumvent these problems involves an instrumental variable (IV) technique.

AJR argue that during the colonial period Europeans were more likely to settle places where they had a lower risk of dying from foreign disease. Colonies where Europeans settled developed institutions that protected property rights more than colonies where Europeans did not settle. In the long run, the authors claim, the direct effects of mortality from disease and European settlement on national income faded, while the indirect effect through property rights institutions lasted. This argument validates using European settler mortality rates as an IV for risk of capital expropriation – a measure of property rights institutions – in an equation determining GDP per capita. With their innovative econometric model, AJR find the effect of expropriation risk on GDP to be quite large, explaining much of the variation in GDP per capita across countries.

A number of researchers have used AJR's innovative measure of potential European settler mortality for further research, seen in Acemoglu et al. (2002), Rodrik et al. (2002), Sachs and McArthur (2002), Dollar and Kraay (2003), Acemoglu and Johnson (2003), Easterly and Levine (2003), Acemoglu et al.(2004), and Glaeser et al. (2004). However, few have given a second look at the mortality data so central to this line of empirical research. Because Acemoglu, Johnson and Robinson (2000) provided an excellent Data Appendix – available in the *NBER Working Paper* version – and graciously gave me a copy of their data, I was able to delve deeper.

In my investigations I found that a number of the mortality rates used by AJR are lacking in geographical relevance, statistical precision, or comparability across countries. Despite the fact that some of these problems appear insurmountable with the data currently available, I create two revised series of settler mortality rates to help mitigate these problems in a way which should be more appropriate for AJR's cross-country regressions. To see if my revisions make a difference to AJR's conclusions I rerun a number of the specifications in AJR, as well as some

specifications similar in spirit, using the original and revised mortality data.

When the revised mortality data are used, the "first stage" relationship between settler mortality and expropriation risk tends to be weaker, less robust, and less significant than in AJR's original paper. This creates a "weak instrument" problem, causing IV estimates to suffer from undesirable small sample properties, and still graver problems for statistical inference as tests have very low power.[1] With a weak instrument traditional Wald statistics are misleading and should be replaced by the statistic proposed by Anderson and Rubin (1949). In most cases, correctly specified 95% confidence regions of the true causal effects of expropriation risk are unbounded, containing arbitrarily large positive and negative values. In fact, even with AJR's original data a mild weak instrument problem occurs in some with certain control variables.

In section I, I review some of the major issues with the data and the consequent revisions, leaving some detail to the Data Appendix. In section II, I discuss econometric issues relating to measurement error and weak instruments. In section III, I present the results and discuss the sensitivity of estimates to a small number of countries, to exclusion of countries with poor measures of mortality, and to alternate measures of property rights institutions.

I. The Mortality Data: Problems and Revisions

Finding good measures of potential European settler mortality is a challenging task: true early settler mortality rates are difficult to find where Europeans did settle and impossible to find where they never did. To overcome these difficulties, AJR make great use of mortality rates of European soldiers *from disease* (not combat) as they went to fight in various colonial countries over the 19th Century. These mortality rates are from the writings of Philip Curtin (1964, 1968, 1989, and 1998) who pieced together many disparate sources. In order to fill in some of the many remaining holes in the soldier data, AJR also use mortality rates of bishops in Latin America in the 17th and 18th centuries from Gutierrez (1986) and of blacks working in harsh conditions in Africa during late colonial times from Curtin et al. (1995).

A. Data Problems and Revisions in General

As alluded to earlier, the types of problems with the data can usually be classified into three categories: geographical irrelevance, statistical imprecision, and incomparability across countries

[1] It should be noted that Dollar and Kraay (2003) discovered a weak instrument problem when trying to combine the approaches in AJR with Frankel and Romer (1999) in a model with two instruments and two endogenous variables. The case here involves only one instrument and one endogenous variable.

in a way appropriate for this type of regression.

Geographical irrelevance refers to when the mortality estimate is based on a source which does not correspond well to the country in question. In some cases this comes from a possible misunderstanding in determining where the data are originally from and, in other cases, from contestable judgments as to how one extends 36 distinct mortality rates from sources to the 64 countries in the sample. In the revisions I attempt to improve the geographical relevance of the data, although the result is still imperfect as I try to keep the sample as large as possible to maximize the power of the econometric analysis.

Statistical imprecision refers to cases where mortality rates are based on relatively brief periods of time, small numbers of people at risk of death, or exceptional circumstances, which given the volatility of mortality data make them of questionable statistical worth. Curtin himself often advises the reader on how representative a given mortality rate is for the given region. Often this problem can be remedied by finding better sources, or by averaging several estimates from the original sources, although this sometimes involves abandoning AJR's rule of using only the earliest available figure for each country. This rule – which AJR themselves do not apply in all cases[2] – appears suboptimal as the earliest available numbers are often from small isolated incidents of questionable comparability and statistical precision. In several cases I take an average of several rates (usually appropriately weighted) to reduce noise. Furthermore, an average reduces the impact of a discretionary choice when several estimates are available. As AJR use many of Curtin's own averages, this technique should not be too controversial.

Incomparability across countries refers to cases where the mortality data appear to differ across countries for reasons other than underlying differences in potential European settler mortality. The most common form of this incomparability comes from the intermingling of mortality rates from soldiers on campaign with other mortality rates from soldiers in barracks. As Curtin (1989, p. 4) asserts "one of the fundamental facts of military medical experience [is] troops in barracks are much healthier than troops on campaign, even disregarding losses from combat." In the 19th Century, European soldiers are typically at peace in countries already colonized. This may introduce an endogenous measurement error problem as the negative correlation between measured mortality rates and European settlement (and the associated property rights) may be due to the fact that places already colonized have lower mortality rates for Europeans than places in

[2] e.g. Sudan, Egypt, India, and arguably Venezuela and the United States.

the process of colonialization or conquest. Curtin was interested in colonialization in the 19th Century, and as such his mortality rates give a fairly good characterization of the contemporary situation given the existing differences across countries. Mortality rates appropriate for AJR's model, on the other hand, should refer to the same (presumably early) stage of colonialization across countries.

Given the existing data sources, a single data revision that reduces all of these problems to a satisfactory level appears impossible. Instead I produced two different revised data sets with different relative strengths, although neither is flawless. The first set, or "high revision," uses mortality rates of soldiers that are usually on campaign (or comparable rates) and includes all 64 countries. The advantages of these data are that they are more widely available and, as Curtin (1998, pp. 229-30) argues, are less likely to change over time as advancements in medicine are made – a worthy consideration as mortality rates are taken from throughout the 19th century. The primary disadvantage of these rates are that idiosyncratic factors specific to campaigns may have a large effect on mortality rates, introducing a large degree of noise into the figures.

The second set, or "low revision" uses mortality rates from soldier in barracks or of comparable data. While these rates are probably subject to less noise, these data are less available and are more sensitive to changes in time, requiring that they be taken over a narrower time span. Consequently the low revision data set is smaller, containing only 43 of the original 64 countries.[3]

B. Data Problems and Revisions: Region-by-Region

The 47 changes for the high revision and the 34 changes for the low revision are shown in the Appendix Table and are illustrated in Figure 1, which graphs the revised settler mortality rates against the originals rates by country. The left panel shows that the high revision generally raised the rates of countries with lower original rates and lowered the rates of a number of the higher original mortality countries. The right panel shows that the low revision generally lowered mortality rates, especially for countries with higher original mortality rates. In general, where some doubt about the data remains, I tend to make conservative adjustments, giving the benefit of the doubt to AJR's original judgments. In many cases the revised rates are the same or only slightly different not because there was no reason to change them, but rather because of a lack of information to change them with. Mortality rates are given in deaths per 1000 at risk per year.[4]

[3]The high revision rates should reflect better the mortality rates of earlier settlers.

[4]In their unpublished appendix AJR provide estimates using alternate versions of the mortality data. However these alternate versions are not as nearly as different, only really taking into account some of the problems with the African data, and do not compensate for the same issues I discuss here.

Important specific problems and revisions are discussed below on a region-by-region basis, leaving lesser problems and revisions, as well as further discussion, to the Data Appendix. While I maintain that the revised data set are better for estimation purposes, the revised mortality rates for a number countries, especially in Latin America and Central Africa, are still highly tenuous. Although my estimates should be more accurate and comparable than AJR's they most certainly still suffer from considerable measurement error. Any empirical results based on the currently available mortality data, mine or not, should be treated with considerable caution.

Sub-Saharan Africa: While Africa has some of the richest mortality data, AJR make a number of objectionable judgments here, using high rates of soldiers on small campaigns during epidemics and peak mortality rates of black slaves of questionable comparability, as well as assigning rates to countries far away from a rate's origin.

The high mortality rates of Gambia and Nigeria and Mali (of 1470, 2004, and 2940) are based on small, sporadic campaigns of less than a few hundred soldiers during periods of exceptionally high mortality. For Mali, AJR cite a rate from a two-month expedition up the Senegal River in 1874, during a particularly severe outbreak of the yellow fever. AJR take the monthly rate for this campaign of 245 (Curtin, 1998, p. 81) and multiply it by 12 to infer an annual rate with replacement of 2940. Curtin (p. 81) however points out that "the annual rate and the rate of loss over two months would have been about the same." Curtin (p. 10) also prefaces Gambia's rate saying that it "was not the normal West African experience, but only typical of what could happened during a yellow fever epidemic." The Nigerian rate is based off a two month expedition of 159 soldiers in 1841 who went into the most malaria-infested part of the Niger at the peak of malaria season and suffered 55 deaths (p.21). These mortality rates, computed "with replacement," do not seem to correspond to the average annual mortality rates a typical settler in these countries would face. Using the same methodology for the settlers of Plymouth in 1621, when approximately half the settlers died in three months, would result in a rate of 2000 for the United States (Bolton and Marshall, p. 1971) instead of 53.4. Instead, for Mali I use the average rate of 200.24, Curtin's average from a number of campaigns in the "French Soudan" (1998, p. 87) for years 1883-88, while Gambia and Nigeria are given rates of 353 and 266.5 for reasons given in the Data Appendix.[5]

The mortality rate of 240 AJR use for the Congo and Zaire comes from a very different source: African workers "recruited by force from all parts of French Equatorial Africa" to work

[5] AJR do try alternate figures for these countries for their Working Paper (2000) version, but not used in the principal analysis or in the published version.

on the Congo-Ocean railroad in the early 20th century. These workers, Curtin et al. (1995, p. 463) explains, were "unprepared for the diseases they encountered" and "were also underfed and ill-housed. As a result the overall death rate reached 100 per thousand per annum, and as high as 240 per thousand the peak of mortality…" AJR's decision to use the peak rather than the average mortality rate is not explained, although use of the peak is favorable to their hypothesis. The rate of 145 taken from Curtin et al. (1995, p. 491) for Kenya and Tanzania comes from the forced migration of African men from the relatively disease-free highlands to the coastal plain where they "died at annual rates *as high as* one hundred and forty-five per thousand…" (emphasis added). AJR's claim that mortality rates of African blacks are a lower bound for the mortality of Europeans whites, based on Curtin (1968), is unconvincing given the use of peak rates and the fact that these Africans were from different disease environments and were forced to work in slave-like conditions. There are no alternate estimates to replace these and so they are included only in the high revision, albeit with some skepticism.

AJR assign the mortality rate of soldiers campaigning on the Senegal River in western Mali (Curtin, 1998, pp. 82-89) for 1880-83 to Niger while they assign a rate from the same area from 1884 to Burkina Faso and far away Angola, Cameroon, Gabon, and Uganda; strangely, neither rate is assigned to Mali. AJR may have assumed that these rates came from separate campaigns as the rates for 1880 to 1892 are listed in one table (4.2) under the name "Haut-Senégal-Niger," while rates from a shorter period, 1883 to 88, in another table (A8.1) are under the name "French Soudan." While Curtin simply meant to refer to campaigns in western Mali, the authors understood that "Haut-Senégal-Niger" referred to Niger, which it did for 7 years, while "French Soudan" referred to countries such as Chad, French Congo, and the Central African Federation, which it never did.[6] Finally, the authors assign the rates for the French Soudan to countries they believed neighbored it. As alternate data sources for these countries are not available, I regroup the countries more sensibly, giving Niger and Burkina Faso the same rate as Mali; Uganda the same rate as Kenya; Gabon, Cameroon and Angola the same rate as Congo. Although most of these countries should probably be excluded altogether, these countries are included in the "high revision," being favorable to AJR's hypothesis.

All of the rates AJR use for Sub-Saharan Africa are appropriate for the high revision

[6] This misunderstanding may be due partly to the fact that the general term "Soudan" in French – as can be seen in older editions of *Le Petit Larousse* – refers to a large swath of land south of the Sahara from Mali, through Chad to the modern Sudan, although this certainly excludes the French Congo and most likely the Central African Republican. While much of this territory was controlled by the French, "le Soudan Français" referred more narrowly to modern Mali, since 1920, while "Haut-Senegal-Niger" referred to Mali and Burkina Faso (1904-1920) before World War I, and for a short period (1904-1911) included Niger. See Adaye and Crowder (1985) for additional information.

only, with the exception of South Africa. Additional mortality rates appropriate for the low revision are generally unavailable, with two exceptions: Saint Louis and Gorée in years 1852-73 with a rate of 72 (Curtin, 1998, p. 87), assigned to Senegal and Gambia, and the Gold Coast from 1859 to 1875, when a small group of non-commissioned officers sustained a mortality rate of 151.45 (Curtin, 1998, p. 70). These years are somewhat late for a barracks rate, however increasing this rate further for Senegal and Gambia would actually be unfavorable to AJR's hypothesis, while the Gold Coast are actually quite close to Feinberg's (1974) rate of 184.6 for Dutch soldiers in barracks during the 18th century, and therefore seem rather stable.

North Africa and Malta: According to Curtin (1989, p. 17) the higher mortality in North Africa relative to Southern Europe in the mid-19th Century is mainly due to wartime conditions faced by the soldiers. Yet this is not reflected in AJR's original mortality data. In fact, the mortality rate of soldiers in Tunisia in 1881 (63) was exceptionally high because of a typhoid epidemic brought by soldiers from France (Curtin, 1998, p. 152). To ensure comparability in the low revision, Malta's rate of 16.3 is used for Tunisia, and Gibraltar's rate of 21.4 (Curtin, 1989, p. 7) is used for Morocco and Algeria. Reciprocally, Tunisia's rate is given to Malta for the high revision.

In Sudan the first mortality rate available is precisely 0, in the Suakin expedition of 1884, consisting of 4,500 troops (Curtin, 1998, p.173); the second available rate of 10.9 (p.169) is from the same expedition during the first half of 1885 in Suakin with 7,253 troops. AJR adopt the third available rate of 88.2 (p. 173), from the same expedition during the second half of 1885 when there were only 463 troops. Taking the average mortality of the entire expedition up to and including the second half of 1885, weighted by troop strength, gives a total mortality rate of only 13.87. This rate is not favorable to AJR's hypothesis, but it is similar to neighboring Ethiopia's rate of 26.[7] Places like coastal Sudan and Ethiopia may have been healthy for Europeans, offering certain health benefits such as a reduction in deaths from tuberculosis (p. 43).

Latin America: The data for 15 Latin American countries are based on a questionable combination of the mortality rates of bishops of dubious statistical worth with the mortality rate of a single campaign of French soldiers in Mexico. Gutierrez (1986) divides Latin America into three regions: low, medium, and high temperature, in each and calculates the mortality rates of

[7] AJR claim that the mortality rates in Ethiopia were short because of evidence that it was "short and well managed." The short length however would tend to increase the monthly mortality rates, as mortality rates tend to be highest with first exposure to foreign diseases.. Also, while the campaign was well managed the point of view of engineering, medically it was not exceptional, unlike the Asante campaign of 1874 (Curtin, 1998, p.43)

bishops, ages 40 to 49 in the 17th and 18th centuries. Taking the ratio of 4, 5 and 10 deaths over "at risk" populations of 24, 28.5, and 30.5 bishops for each region, Gutierrez calculates annual mortality rates of 16.7, 17.5, and 32.8 for each region. Not surprisingly, the standard errors of the estimates are large – 7.8, 7.2, and 8.6, for rates respectively – I show in the Appendix, standard 2-sided *t*-tests cannot reject with 90% confidence that all of these regions have the same mortality rates, meaning that the ratios may be due purely to sampling error.

French soldiers campaigning in Mexico form 1862 to 1863 incurred a mortality rate of 71 Curtin (1998, p. 239). AJR assume that relative mortality ratios for bishops should hold for soldiers as well, and therefore, as Mexico is a low temperature country, assign all low temperature countries a rate of 71 and for moderate and high temperature countries assign rates of 78 and 163.3.[8] AJR claim in their Data Appendix that they would have gotten similar rates by using data from Jamaica or naval station data. However, I show in the Data Appendix that the cross-validations AJR compute are actually very selective and that most cross-validations would suggest that the figures for Latin America are too high, lending doubt to AJR's use of ratios based on the bishop data.

Although I find AJR's methodology objectionable, I did not come by better data for Latin America, and therefore, to keep the sample large, I used their methodology with some slight improvements described in the Data Appendix for the high revision. For the low revision I kept the bishop mortality data as is, as the upward adjustment for the Mexican campaign is was not appropriate and several cross-validations roughly agree with this.

North America: The original mortality rates of soldiers in the United States and Canada are appropriate for the low revision only. The United States rate of 15 from years 1829-1838 (Curtin, 1989, p. 7) is of American soldiers not involved in any major war or of recent European origin, and are stationed in the North (the South has a rate of 34). Although the mortality rates of European soldiers in the US are unavailable, the overall mortality rate from disease of white Union soldiers during the Civil War is 53.4 (Adams, 1952, p. 239), used for the high revision. While this rate is higher than AJR's rate, it is probably biased downwards since the soldiers are American and have some resistance to American diseases from childhood. Furthermore, 53.4 is much lower than the mortality rate of approximately 500 for the first settlers of Jamestown from 1607 to 1624 (Curtin, 1998, p. 116), who suffered from a variety of diseases.

For Canada, AJR gave a mortality rate of 16.1, for soldiers which Curtin (1989, p.7) labels as "British," although some were likely indigenous to the still British Canada, and were

[8] The ratios I found in Curtin were slightly different than the ones I found in Curtin.

similarly at relative peace. Campaign rates applicable to Canada come from two large American armies that camped in the northern Champlain valley in 1812, sustaining winter mortality rates from disease of 100 and 125 each, according to Everest (1981, p. 95). To be conservative and to acknowledge the perhaps bad circumstances of the Champlain campaign, I assign Canada the United States's rate of 53.4 for the high revision.

East Asia: The low mortality rates of Singapore, Malaysia, and Hong Kong, much lower than rates of Indonesia and Vietnam, are not based on strong data sources. For Singapore and Malaysia, AJR use the "Straits Settlement" rate of 17.1. This rate is quite low relative to nearby Indonesia's campaign rate of 170. AJR argue (Data Appendix, p. 1) that such proximate differences can occur "because there exists substantial variation in disease environments, particularly for malaria, even in neighboring areas." AJR do not recognize that the mortality rate for the "Straits Settlement" is from a calm garrison of the Madras Army in Penang, more than 500 km from Singapore. Singapore itself is less than 50 km from parts of Indonesia. Moreover, Curtin (1989, p. 17) states that the size of the Penang force was too small for the rate "to be significant." Given the high mortality rates of other areas in southeastern Asia, and historical evidence that mortality rates of natives in the Malay Peninsula were quite high from malaria and other tropical diseases (Kennedy, 1970, p. 226-7), an upward revision seems called for. The ratio of mortality rates of the Madras Army in Burma in times of war (1824 to 1826 in Curtin, 1989, p. 23) to times of peace from (1829 to 1838, Curtin, 1989, p. 8) is 119/34.6 = 3.439. The product of this ratio and 17.7 of 60.88 is used for the high revision. Albeit imperfect, this rate is the lowest (and most favorable towards AJR) of alternative estimates using averages of nearby areas, and conservatively preserves the mortality ranking of the Southeast Asian countries.

The mortality rate of 14.9 used for Hong Kong is for the British China Field Force who fought in 1860 during Arrow's War. Although British and French soldiers assembled in Hong Kong in the cooler spring months, the campaign started in July when they left for Beijing, 2000 km to the north, where most of the fighting occurred. As the data source for the mortality rate never mentions Hong Kong, the representativeness of this rate is doubtful. Also, the rate applies to a period of less than six months and should be adjusted upwards. Moreover, such a rate seems low for Hong Kong: according to Gregory (1978, p. 230) Hong Kong was unhealthy during the summer, with many falling ill and dying. The only other figure available for China is a campaign of two years when the French (Curtin, 1998, p. 239) helped fend off the Taiping rebellion around Shanghai, incurring a mortality rate of 118. The true mortality rate for Hong Kong may lie somewhere between these two rates, and so for lack of a better alternative I average the two to get

a guess of 66.45 for the high revision. Hong Kong is excluded altogether from the low revision.

II. Econometric Issues

Before moving on to the results section it is useful to review the econometric consequences of correlated measurement error which may appear in the mortality data, and of the weak instrument problem, which may invalidate the use of meaningful IV estimation. The econometric model used by AJR can be summarized by a system of equations

$$(1) \qquad R_i = \beta M_i + \mathbf{X}'_i \delta + \tilde{\eta}_i$$
$$(2) \qquad Y_i = \alpha R_i + \mathbf{X}'_i \gamma + \tilde{\varepsilon}_i$$

where Y_i is log GDP per capita on a PPP basis, R_i is expropriation risk on 1 to 10 scale, with a higher number implying greater security of property, M_i is log potential settler mortality, $\mathbf{X}_i$ is a vector of control variables including a constant, and $\tilde{\eta}_i$ and $\tilde{\varepsilon}_i$ are error terms. Partialing out the control variables, this system can be rewritten as

$$(1') \qquad r_i = \beta m_i + \eta_i$$
$$(2') \qquad y_i = \alpha r_i + \varepsilon_i$$

where the lower-cased letters are residuals of their upper-case counterparts from an ordinary least squares (OLS) regression on the control variables.[9] IV estimates require an instrument which is (i) *relevant* $\beta \neq 0$ and (ii) *excludable* $E[m_i\varepsilon_i] = 0$. Letting $\pi = \alpha\beta$ and $\xi_i = \varepsilon_i + \alpha\eta_i$, the reduced form equation for (2') is given by

$$(RF) \qquad y_i = \pi m_i + \xi_i$$

The IV estimator of α is the ratio of the OLS estimate π in (RF) to the OLS estimate of β in the first-stage equation (1), i.e. $\hat{\alpha}_{IV} = \hat{\pi}_{OLS} / \hat{\beta}_{OLS}$, as well as the ratio of sample covariances $\hat{\alpha}_{IV} = \hat{\sigma}_{my} / \hat{\sigma}_{mr}$, where $\sigma_{my} = E[m_i y_i]$ and $\sigma_{mr} = E[m_i r_i]$ are the population equivalents.

A. Measurement Error

Classical uncorrelated measurement error tends to bias down the size and significance of $\hat{\beta}_{OLS}$ and has no effect on the consistency of $\hat{\alpha}_{IV}$. Correlated measurement error, on the other hand, may increase the estimate and significance of $\hat{\beta}_{OLS}$ and could render $\hat{\alpha}_{IV}$ inconsistent. Modeling measurement error as $\mu_i = m_i - m_i^*$ where m_i^* is the true value of log settler mortality and m_i is its measured counterpart, I show in Appendix A in the case where μ_i may be arbitrarily correlated

[9] Expressed in matrix terms, $y = \left(I - X'(X'X)^{-1}X\right)Y$ where I is the identity matrix.

with any of the variables or error terms, the following probability limits apply to the estimators

$$(3) \qquad \text{plim}\hat{\beta}_{OLS} = \frac{\beta\sigma^2_{m*} + \sigma_{\mu r}}{\sigma^2_{m*} + \sigma^2_{\mu} + 2\sigma_{\mu m*}}$$

$$(4) \qquad \text{plim}\hat{\alpha}_{IV} = \alpha + \frac{\sigma_{\mu\varepsilon}}{\beta\sigma^2_{m*} + \sigma_{\mu r}}$$

where $\sigma_x{}^2 = E[(x_i)^2]$ and $\sigma_{wx} = E[w_i x_i]$ for any w_i and x_i. Equation (3) implies that if $\sigma_{\mu r}$ or $\sigma_{\mu m*}$ is non-zero, i.e. measurement error is correlated with measures of expropriation risk or mortality, then the OLS estimate may be biased *away* from zero, rather than towards zero. Equation (4) states that if $\sigma_{\mu\varepsilon} \neq 0$, i.e. measurement error is correlated with the error term in (2'), the excludability restriction is violated and the IV estimate is inconsistent. Although not shown here, correlated measurement error may artificially decrease the estimate of $\text{var}\left(\hat{\beta}_{OLS}\right) = \hat{\sigma}^2_{\hat{\eta}} / N\hat{\sigma}^2_m$, increasing the significance of $\hat{\beta}_{OLS}$ and the apparent strength of the IV design.

Letting n_i be a revised measure of mortality, with measurement error $v_i = n_i - m_i$ *, and difference $d_i = m_i - n_i = \mu_i - v_i$, the inconsistency of the estimates based on the revised mortality have formulas identical to (3) and (4) with "v" in the place of "μ". Other calculations provide "update formulas" of the revised estimates in terms of the original estimates and sample variances and covariances:

$$(5) \qquad \hat{\beta}_{\text{Revised}} = \frac{1}{\hat{\sigma}^2_n}\left[\hat{\beta}_{\text{Original}}\left(\hat{\sigma}^2_n + \hat{\sigma}^2_d + \hat{\sigma}_{dn}\right) - \hat{\sigma}_{dr}\right]$$

$$(6) \qquad \hat{\alpha}_{\text{Revised}} = \hat{\alpha}_{\text{Original}} - \frac{\hat{\sigma}_{d\hat{\varepsilon}}}{\hat{\beta}_{\text{Revised}}\hat{\sigma}^2_n + \hat{\sigma}_{dr}} = \hat{\alpha}_{\text{Original}} + \frac{\hat{\alpha}_{\text{Original}}\hat{\sigma}_{dr} - \hat{\sigma}_{dy}}{\hat{\beta}_{\text{Revised}}\hat{\sigma}^2_n + \hat{\sigma}_{dr}}$$

where the notation is straightforward. These formulas trace exactly where changes in parameter estimates come from.[10] The true impact of any remaining measurement error in the revised mortality depends on still unknown parameters about the measurement error in n_i, and could be important. Still, improved data should lead to improved estimates if the identification assumptions truly hold.

B. Weak Instruments

When the OLS estimate of β is not significantly different from zero - a strong indicator of a *weak instrument* – multiple statistical problems occur. First, as Nelson and Startz (1990) show for small samples, the central tendency of the IV estimator is biased away from the true value in the

[10] Examples are illustrated in footnote 14.

direction of the probability limit of the OLS estimator, and the distribution of the IV estimator is not approximated by its asymptotic distribution. Second, even small violations of the excludability restriction can impart a large degree of inconsistency in the IV estimates. Third, as shown by Dufour (1997), inference based on α is complicated as conventional IV confidence regions based on the Wald statistic (point estimate ± t × standard error) become grossly invalid.

Confidence regions for $\hat{\alpha}_{IV}$ of the correct size can be built by inverting the AR statistic proposed by Anderson and Rubin (1949). The AR statistic under the null that H_0: $\alpha = \alpha_0$ is the standard F-statistic of the instrument m_i regressed on the residual computed under the null $\varepsilon_i = y_i - \alpha_0 r_i$. Moreira (2003) proves that in the exactly identified case AR tests are uniformly most powerful amongst unbiased tests, making them optimal for this application. Confidence regions are constructed by computing the AR test over a fine grid of values for α_0, including in the region all values of α_0 the AR test cannot reject. Zivot, Startz, and Nelson (1998) show that all confidence regions will either take a bounded form $[\alpha_L, \alpha_H]$ or an unbounded form $(-\infty,\alpha_L]\cup[\alpha_H,+\infty)$, where the latter occurs whenever the first stage $\hat{\beta}_{OLS}$ is not significantly different from zero in the first stage at the same significance level. For the sake of comparison I also report conventional Wald 95% confidence regions, allowing the reader to see when they are fairly correct or not.[11]

As mortality data are shared by some countries, any measurement error will also be shared by those countries, introducing serial correlation into the residuals known as "clustering" (see Moulton, 1990). Correction of the standard errors for clustering effects in OLS and 2SLS Wald-based standard errors has become standard in the literature (see Wooldridge, 2001, p. 152 and p. 191). Correction for clustering in the presence of weak instruments is still in development (see Andrews, Moreira, and Stock (2004)), although a reasonable method is to use the OLS clustering formula for the regression used to compute the AR statistic.[12] This adjustment seems to perform well, as confidence intervals become unbounded for α_0 when first stage clustered first stage F-statistics cannot reject zero for β_0.

The standard over-identification test (Sargan, 1958) is used by AJR to check for the

[11] With one instrument, the weak instrument problem is relatively easy to understand heuristically if the IV parameter is viewed as the ratio of the reduced form parameter to the first stage parameter: $\alpha = \pi/\beta$. Say that $\pi < 0$ is known with certainty, but β is uncertain, but with 95% confidence is known to be in interval of the usual form $[\beta_L, \beta_H]$. Now, if $\beta_L < \beta_H < 0$ then the 95% confidence region for α is $[\pi/\beta_L, \pi/\beta_H]$. On the other hand, if $[\beta_L, \beta_H]$ contains zero, i.e., $\beta_L < 0 < \beta_H$, then the confidence region for α is of an unbounded form $(-\infty, \pi/\beta_H]\cup[\pi/\beta_L,+\infty)$. If β is statistically indistinguishable from small positive and negative values, then α is statistically indistinguishable from very large positive and negative values. Adding uncertainty to the reduced form parameter π widens these confidence intervals further.

[12] Based on a personal communication with Michael Jansson.

exogeneity of mortality as an instrument, assuming some other variable is also a valid instrument. Intuitively, this test checks whether IV estimates from different instruments are significantly different from each other, rejecting if they are. Failure to reject the over-identification test, as AJR themselves caution, does not imply that the control variables are valid instruments, only that separate IV estimators, using each instrument individually, are not statistically distinguishable from each other. With weak instruments, the IV estimates are hardly distinguishable from any value and thus over-identification tests have low power and little value. Furthermore, Staiger and Stock (1997) show that with weak instruments the over-identification statistic has a non-standard distribution depending on unknown parameters, and cannot be calculated correctly.[13]

III. Results

When either revised mortality series is used in place of the original mortality series, the significance of *log mortality* in the first stage results diminishes, eliminating the possibility of getting a precise IV estimate and casting doubt on the validity of settler mortality as an instrument. I also show that AJR's original stronger results depend on a small number of observations, and that excluding poorly measured countries, or using alternate measures of property rights institutions does not change my results significantly.

A. Means and Covariance Structure

Table 1 presents the sample means of the main variables – original and revised log mortality, their difference, log GDP per capita, and expropriation risk – for both the high revision and the low revision. Comparing the original data and the high revision, the two have almost the exact same mean, but the revised log mortality figure has about half the variance. In its sample of 43 countries, the low revision has a much lower mean, and also about half the variance as the original log mortality. Because both revisions have much lower variance in log mortality and slightly higher residual variance, $\text{var}\left(\hat{\beta}_{OLS}\right)$ goes up, decreasing the significance of the first stage.

For both revisions the difference in log mortality estimates d_i has a fairly high variance, low covariance with the revised mortality term, but fairly high covariance with log GDP and

[13]In fact the AR test and the over-identification test are almost identical. The over-ID test is calculated by using oth M_i and $\mathbf{X}_i$ are used as instruments to estimate $\hat{\alpha}_{2SLS}$, and the resulting residual $\hat{\varepsilon}_i = Y_i - \hat{\alpha}_{2SLS} R_i$, which is then regressed on M_i, and $\mathbf{X}_i$. The F-statistic for the joint test of significance of all the variables, with one less degree of freedom in the numerator, gives the pseudo-F version of the Basmann (1960) over-identification test. When instruments are weak the over-ID test rejects for very few values of $\hat{\alpha}_{2SLS}$ just as the AR test rejects for very few values of α_0.

expropriation risk, which updating formulas (5) and (6) show could have large impact on the estimates. Yet, with no controls there is remarkably little change in the point as most of the covariance terms in (5) and (6) seem to counterbalance each other.[14] However, when control variables are added this sensitive balance is disturbed as the covariance terms (not shown) become smaller in different proportions, leading to drastically different and volatile point estimates, and even lower significance of *log mortality*.

B. First Stage Regressions

Table 2 presents the most important results relating to the first stage. Most of these specifications are taken directly from AJR, and the others are closely related. Columns (1)-(5) correspond to columns (1)-(3), (7)-(8) from AJR's Table 4. Columns (7)-(9) using climate controls is similar to AJR's Table 6, column (1), except that I use only one temperature variable, mean temperature, and one rain variable, minimum monthly rain, rather than the multiple temperature and humidity variables that AJR use from the same source (Parker, 1997). Column (9) with a control for percentage of the population of European descent in 1975 is from Table 5's column (3). Column (10) controls for the percentage of the population living where *falciporum* malaria is endemic in 1994 and is identical to AJR's Table 7, column (1).

Panel A shows the results using AJR's original data, including standard errors under the assumption of homoscedasticity and under clustering. *p*-values of *F*-tests, adjusted for clustering, for the significance of *log mortality* and of the controls are also shown. In all cases I was able to replicate AJR's estimates within a few percentage points, with all error probably due to rounding in the data. In column (1), the original AJR paper has an identical first stage estimate and standard error. For the most part these first stages perform rather well, although the clustered standard errors are substantially larger than the homoscedastic standard errors as the residuals of countries sharing the same mortality rates are positively correlated.[15] In columns (5) and (8) mortality is not highly significant in a way which would make it a strong instrument. Most of the

[14] For example, using equation (6) and the high revision, $\hat{\alpha}_{\text{Original}} = 0.93$ is at a value which makes the numerator of the difference term small as $\hat{\sigma}_{d\hat{\varepsilon}} = \hat{\sigma}_{dy} - \hat{\alpha}_{\text{Original}}\hat{\sigma}_{dr} \cong -0.41 + 0.52(0.93) = 0.083$. With control variables this impact tends to be larger. Also, as an illustration of the potential impact of measurement error, imagine that the remaining measurement error has a similar covariance structure to the difference between the revised and original mortality rates. Thus, $\sigma_{vy} = \lambda\hat{\sigma}_{dy}$, $\sigma_{vr} = \lambda\hat{\sigma}_{dr}$ and $\sigma_{vm^*} = \lambda\hat{\sigma}_{dn}$, for some $0 < \lambda < 1$. Under the null that the true parameters are equal to zero $\alpha = \beta = 0$, then equations (3) and (4) imply the estimates would be $\hat{\alpha}_{2SLS} = 0.78$ and $\hat{\beta}_{OLS} = -0.33$, with no controls and $\hat{\alpha}_{2SLS} = 0.84$ and $\hat{\beta}_{OLS} = -0.30$ with continent controls. These are uncomfortably similar to the actual estimates reported.

[15] Clustered standard errors are also higher than homoscedastic standard errors with the high revision, but rather similar with the low revision.

control variables are not highly significant except in columns (6)-(9) suggesting that weather variables or European descent background controls should be included in most of AJR's regressions in place of the relatively insignificant *latitude* control.[16]

Panels B and C displays results for the high and low revisions, respectively, including clustered standard errors and *p*-values of *F*-tests. The significance of mortality in these equations is never very strong, and in fact can be rejected at a significance level of 5% in all cases. In most cases the control variables are more significant than mortality, making them better predictors of expropriation risk. Also the point estimate of β fluctuates considerably, taking on values very close to zero in several specifications, and even turning positive in one specification. The marginal significance of log mortality is of greater consequence when in an instrumental variable framework than in a standard linear regression: because of the weak instrument problem lack of strong significance creates a serious inference problem seen in section C.

Behind the lack of significance of log mortality in the first stage is a simple story. Looking within continents such as Africa, the evidence supporting AJR's hypothesis is weak: the relatively healthy and safe example of South Africa and Tunisia are offset by the healthy, but relatively unsafe Sudan and Algeria. In the Americas the differences in mortality between the US and Canada and Latin America were mistakenly exaggerated in AJR's original data because it compared rates of peaceful soldiers with rates boosted by that of campaigning French soldiers in Mexico. Mortality rates in Singapore and Hong Kong were also artificially low, based on weak evidence. Furthermore, it appears that settler mortality is more highly correlated with variables like mean temperature, rain, and latitude, than AJR's original data with its many anomalous outliers. It's lack of significance relative to these variables suggest that while it may be an important factor affecting expropriation risk, the measure is too noisy to outperform other variables.

The results of column (9) in panels *B* and *C* and column (10) in panel *B* imply that the effect of settler mortality on expropriation risk cannot be easily disentangled from the current presence of malaria or population of European descent. Thus it is hard to say that settler mortality had any real causal influence on expropriation risk, as it may just act as a proxy for current European settlement or disease environment. For example, as Glaeser et al. (2004) argue, countries with a large fraction of people of European descent may have higher levels of human capital, leading to higher levels of GDP per capita as well as lower expropriation risk. This

[16] AJR decide these variables are insignificant based on the value of *F*-tests in the second stage equation without mentioning their significance in the first stage. With weak instruments, Staiger and Stock (1997) show that second stage *F*-tests are invalid.

would violate the excludability requirement for an IV estimate, leading us to draw a false conclusion that expropriation risk is determining levels of GDP per capita.

C. IV Estimates

Table 3 presents the IV estimates of the effect of expropriation risk on GDP, as well as 95% confidence regions using the Wald and AR statistics, adjusted for clustering. Figure 2 demonstrates how confidence regions using either statistic are constructed and how they compare using results from column (2) as an illustration. Confidence regions include all values of α where the value of the statistic is below the critical value. In the left panel, where mortality is a relatively strong instrument, the AR and Wald statistics are not very far apart for relatively low values, making the difference in 95% confidence intervals not very large. In the right panel, where mortality is a relatively weak instrument, the statistics are generally very different and lead to very different confidence regions. The lower the horizontal asymptote of the AR statistic, equal to the first stage *F*-statistic on mortality, the worse the Wald statistic performs relative to the AR for typical test sizes.

As is expected from the first stage results, the 95% AR confidence regions are unbounded – including both positive and negative infinity – whenever the *p*-value from the first stage exceeds 5%. In fact for a given first stage *p*-value, the 100(1 – *p*-value)% AR confidence region for the corresponding IV estimate is unbounded, as the AR asymptote will be below the critical value. The results in Table 3 demonstrate that strong evidence about the true value of α is not possible with the weak instrument problem. The point estimates get at times implausibly large, reflecting a small first stage coefficient, even turning large and negative in one specification. Little can be ruled out as confidence regions for α in a number of specifications include the entire real line.

D. Additional Checks of the First Stage

Table 4 analyzes the sensitivity of the first stage to various changes of the data in the "best-case" scenario with no control variables. In column (2) I analyze the sensitivity of results to omitting variables which may be less reliable. For the high revision I reduced the sample to a better subsample (listed in the Appendix Table) which eliminates observations based off of the extrapolation using Gutierrez data, African countries based off of slave data or places far away, and a few other countries such as Hong Kong for which no reliable estimate exists. This adjustment leads to lower significance of settler mortality. For the low revision I eliminated the 6 countries with mortality rates from later dates, as the barracks rates may be more sensitive to time

changes. This improves the significance of mortality by a small amount, but not enough for it to be robust to the addition of controls variables or the elimination of the Neo-Europes.

In column (3) I illustrate the sensitivity of results to changing a subsample of observations. For the high revision I identify the "Top 10" observations which changed the most from the original data (listed in the Appendix Table), and change them back to the original rates. This almost completely restores AJR's original results, highlighting how important the treatment of just a small subsample of countries is to their model. For the low revision, I take assign the Latin American countries which use the Gutierrez the high revision of mortality to illustrate the importance of the upward adjustment AJR use with the campaign in Mexico, as this improves the first stage fit considerably.

In column (4) Africa is eliminated from the sample which dramatically increases the significance of the first stage. The omission of Africa is a somewhat questionable practice as it eliminates a good deal of information, including North Africa, which has relatively low mortality and expropriation risk. Column (5), which also eliminates the four Neo-Europes, demonstrates that without Africa the Neo-Europes are responsible for most of the identification.

The first stage equation in column (6) is estimated using median regression which is more robust to outliers than conventional OLS. The low significance of mortality shown here even without control variables, suggest that even with the revised data that first stage significance rests rather heavily on outliers.

Table 5 explores the impact of using an alternate measure of property rights institutions, as AJR (2000) do in their *Working Paper*, on the first stage. Two different measures are shown here, "Constraints on Executive" in 1990 and "Law and Order Tradition" in 1995, which are used in AJR's Appendix Tables A4a and A4b. With no controls these two measures perform better than expropriation risk, meaning that settler mortality may perform moderately well as an instrument. However, in the presence of control variables for continents, mean temperature, or European descent in 1975, settler mortality is no longer highly significant or robust. It appears that there is no good way of avoiding the weak instrument problem in AJR's model with the revised data.

Conclusion

The discussion of the settler mortality above should make clear that for a large number of observations AJR's original data are lacking in either geographical relevance, statistical precision, or cross-country comparability. The conclusions drawn by AJR are in fact significantly more

sensitive to the flaws in the mortality data than their own analyses suggest. Neither of the revisions mortality rates meticulously assembled here had a strong enough relationship with property rights institutions variables to support the use of an IV methodology to estimate the causal effect of such institutions on national income. In most cases settler mortality fares worse at predicting property rights institutions than mean temperature or current health conditions or European descent. Perhaps a measurement error-free series of settler mortality would do significantly better than the revised series considered here, but at present there appears to be no better alternative. Further research could produce a more accurate mortality estimate better for testing AJR's model.

This analysis by no means disproves AJR's hypothesis that European settlement had a strong effect on property rights institutions which in turn had a large impact on economic performance. It says only that the statistical tools available are not powerful enough to prove it or disprove it. Those more interested in the validity of these statistical tools in this setting, as well as other issues related to AJR's analysis, should consider papers by Bardhan (2004), Przeworski (2004), and Glaeser et al. (2004). A final word should be given commending Acemoglu, Johnson, and Robinson for their openness, cooperation, and for writing such an interesting and easily reproducible article.

Appendix A

Assuming the excludability restrictions hold for the true variable

$$\text{cov}(m_i^*, \eta_i) = \text{cov}(m_i^*, \varepsilon_i) = 0$$

it is easy to derive that

$$\text{var}(m_i) = \sigma_{m^*}^2 + \sigma_\mu^2 + 2\sigma_{\mu m^*}$$

$$\text{cov}(m_i, r_i) = \text{cov}(m_i^* + \mu_i, \beta m_i^* + \eta_i) = \beta(\sigma_{m^*}^2 + \sigma_{\mu m^*}) + \sigma_{\mu\eta}$$

$$\text{cov}(m_i, y_i) = \text{cov}(m_i^* + \mu_i, \alpha\beta m_i^* + \alpha\eta_i + \varepsilon_i) = \alpha\beta(\sigma_{m^*}^2 + \sigma_{\mu m^*}) + \sigma_{\mu\eta} + \alpha\sigma_{\mu\eta} + \sigma_{\mu\varepsilon}$$

Using the fact that

$$\text{plim}\hat{\beta}_{OLS} = \frac{\text{cov}(m_i, y_i)}{\text{var}(m_i)} \quad \text{and} \quad \text{plim}\hat{\alpha}_{2SLS} = \frac{\text{cov}(m_i, y_i)}{\text{cov}(m_i, r_i)}$$

equations (3) and (4) follow from substituting in the expressions and a bit of algebra. Equations (5) and (6) follow from (3) and (4) by setting $\beta = \hat{\beta}_{\text{Re}vised}$, $\hat{\beta}_{OLS} = \hat{\beta}_{\text{Re}vised}$ etc. and solving.

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Data Appendix

Here I discuss some additional problems, revisions and issues not discussed in the main text. A spreadsheet with my exact calculations is available upon request.

Sub-Saharan Africa:

The high rate of 1470 for Gambia comes from a small British force, always numbering fewer than 120, over a period of 20 months from 1825 to 1826 on the Gambia river, as reported in Curtin (1998, p. 10) As AJR acknowledge in their alternate version of the data, the high Gambia mortality rate of 1470 is hard to square with the much lower rate for Senegal, which encircles Gambia, of only 165. Moreover, the rate for Sierra Leone is calculated from raw data of Sierra Leone and Gambia combined from 1819 to 1836, including this disastrous campaign up the river (Feinberg, 1974). Given the volatility of mortality rates, and the shared data it seems reasonable to assign to Gambia the same rate as Sierra Leone for the high revision. Feinberg (1974) also points out that making the mortality rates of Sierra Leone and the Gold Coast comparable with those used elsewhere, the rates for should be re-averaged by year to produce rates of 353 and 562, rather than 483 and 668, respectively. As these rates belong to soldiers subject to frequent campaigns (Curtin, 1989, p. 18, and Feinberg, 1974) these adjusted rates are used for the high revision.

For Nigeria it seems sensible to use the five-month Dahomey campaign of 1892 which had a monthly mortality rate of 22.21, annualized to 266.5 (Curtin, 1998, p. 101). While this campaign happened in present-day Benin, it was just a few kilometers from the Nigerian border. This rate could be biased downwards as 1892 is also rather late in date when it became standard practice to drink boiled water and take daily does of quinine. However, Curtin (1998, p. 104) believes that these practices were only rarely followed, and that fighting was generally very tough during this campaign, leading to a high mortality rate from disease.

The mortality rate used for South Africa is adjusted slightly upward from 15.5 for the peaceful Cape Colony, to 24.58 which is the average mortality rate for the entire Boer War of 1899 to 1902. This rate seems better for the high revision as it is from campaigning soldiers and is drawn from a much larger sample. The later date may bias the mortality rate downwards which tends to favor AJR's hypothesis.

North Africa: Similarly to Sudan, AJR do not take the first available mortality rate for Egypt. Instead, they take the post-campaign rate of 67.8 for Egypt form Table 6.2 in Curtin (1998, p.

158), even though the same table shows that during the actual campaign the rate was only 24.7. The campaign and post-campaign were both roughly 3 months long. To keep things balanced, I merely average the first three months in with the second three months to get a rate of 46.26. The table also shows that the rate in Egypt for the next year is 30.1.

Tunisia's rate of 63 is replaced with 63.3 since this is what is given Curtin (1998, p. 152). Malta, as a fairly peaceful naval station, has a reported mortality rate which is too low for comparison with warring foot soldiers in North Africa. Gregory (1996, p. 89) reports that during the Napoleonic Wars disease was a major strategic factor in the land battles fought there. Thus the Tunisian rate of 63.3 seems rather plausible for Malta.

The Caribbean: The mortality rates for Jamaica and the Windward Leeward Command (mainly in Barbados) seem acceptable for the high revision, as they appear to be from soldiers who were traveling and engaging in combat. However, as Curtin (1989, pp. 25-8) notes the rates are somewhat volatile and Jamaica and the Windward-Leeward Command change ordering after 1837 during more peaceful times. The application of Jamaica's rate to Haiti and the Dominican Republic and the Windward Leeward Command's rate to Trinidad and Tobago seems plausible, but the application of the Windward Leeward Command's rate to Bahamas seems questionable given the Bahamas' much closer proximity to Jamaica than to Barbados. Nowhere in Gutierrez (1986) do I see corroboration for AJR's claim (Data Appendix) that "information from Gutierrez 1986 indicates that these were similar disease environments." In fact, according to Curtin (1989, p. 25) "no island was systematically more healthy or less healthy than any other in the first half of the nineteenth century." Therefore, given the closer geographic proximity, I give the Bahamas Jamaica's rate of 130 for the high revision.

AJR apply the rate of 32 for French Guiana to (British) Guyana. This rate may not be representative for a number of reasons. First, it appears to be temporarily low, as it later rose to a level higher than Jamaica's which concurrently fell. Second, the Korou expedition in 1764 (Cohen, 1983) to Guiana saw 11,100 of its 12,000 die in its tropical climate. Third, it may also be sensible to apply the Windward-Leeward Command rate of 85 as it included soldiers who served in Guyana. These tend to suggest that Guyana's rate should be revised upwards, but because it is not clear how to do so, I conservatively stick to AJR's original figure.

Because of possible residual effects of war (Curtin, 1989, pp.25-8) Jamaica's rate (also applied to the Bahamas, Haiti and the Dominican Republic) is revised downwards for the low revision to 66 reflecting years 1837-46 (Curtin, 1989,p. 27). Similar years are used for Trinidad and Tobago (Windward-Leeward Command) and Guyana (French Guiana) so that these get rates

of 70 and 25, respectively, for the low revision.

Latin America The mortality rate from Mexico, which affects almost all rates for Latin America comes from an expedition synchronistic with the United States' Civil War, making the comparability of the US and Mexican rates rather plausible. Going back to the original source, Reynaud (1898), I found that the mortality rate actually applies for a period of two years during which troop levels varied. Using the detailed figures given by Reynaud (pp. 113-121 and pp. 471-2) I was able to determine the precise number of troop-years (34,319) that French troops were at risk and the number of deaths in this population (2095) to calculate an annual mortality rate of 61.

Using the data from Gutierrez (1986) I performed a series of *t*-tests for equality of means of the different regions, distinguished by average temperature. The *p*-value of the double-sided tests are as follows: low vs. medium, 94%; low vs. high, 17%; medium vs. high, 17%; low and medium pooled vs. high, 12%. None of these are highly significant.

Gutierrez does not define the regions exactly in the same way as AJR, as his data are for specific towns and cities, not countries. He does define low, medium, and high temperature regions as areas with mean temperatures of less than 20°C, 20°C to 25°C, and greater than 25°C, respectively. AJR do not explain how they group countries into low, medium, and high mortality regions. To make things transparent, I classify countries using the mean temperature variable from Parker (1997) that AJR use as a control variable in some of their regressions. Using this method there are some discrepancies with AJR's original classification as Costa Rica and Honduras are high temperature countries, and Bolivia, Brazil, Columbia, and Peru are medium temperature countries.[17] AJR use ratios of 1, 1.1 and 2.3 to multiply the Mexico mortality rate to get the mortality rates for countries in low, medium and high mortality countries. Instead I use the ratios I calculated exactly from Gutierrez as 1, 1.05, and 1.96.

AJR claim that the results would be "essentially the same" if they used Jamaica as the base case for calculating Latin American settler mortality using the Gutierrez estimates – lowering mortality rates somewhat in proportion to 130 over 163.3 – and that this would actually be favorable to their hypothesis. To the contrary, I find that lowering the Latin American mortality figures weakens the first stage estimate. Also had AJR used a base rate of 85 from the Windward Leeward Command (Curtin, 1989, Table 1.5) or later rates from Jamaica when it was

[17] AJR's footnote that adopting a low mortality estimate for Brazil is less favorable to their hypothesis does not appear to be true as Brazil has amongst the highest ratings for expropriation risk and GDP per capita in all of Latin America

not suffering from the aftermath of war, these inferred rates would be even lower.

As a third check for the accuracy of their Latin American numbers, AJR use the calculations based on naval station data for 1825-45 from Curtin (1964. p. 486). From this data they take the ratio of mortality rates of "South American Stations" (7.7) and the mortality rate from the anti-slavery blockade off of the African coast (54.4) to get 0.142. Multiplying by Sierra Leone's rate of 483, AJR infer a rate of 68.9 for Argentina and Chile (apparently where the South American Stations are located) which is close to the low temperature region's mortality of 71, which AJR take as a sign of the validity of their data. This evidence is rather selective: in fact the ratios of the naval station data are very different from the ratios in AJR's data. If we were to take the same ratio to the "Mediterranean Station" (9.3) of 0.827, and compare this with Gibraltar's rate of 21.4, we would predict a much lower mortality rate of 17.7. Similar exercises using Jamaica's rate of 84 from the same period (Curtin, 1989, Table 1.5) or from the "Home Station" of England would imply low mortality rates of 35.7 and 12.02, respectively. Using "East Indian Station" data or using the Windward Leeward Command's rate of 67.5 instead of Jamaica would also imply lower rates. On the whole, however, these cross-validations suggest that AJR's use of proportions to infer missing mortality rates may be invalid and that estimates of Latin American mortality rates may be too high.

North America The figure given by Adams (1952, p. 239) appears to have withstood some test of time as it is cited by Shryock (1972). Also black soldiers in the Union Army faced even higher mortality rates of 143.4 (Adams, 1952. p. 239) Accounts of soldiers campaigns in Canada during the Seven Years War, the War of Independence, and the War of 1812, are typically grim, making the high mortality rates given by Everest (1981, p. 95) not at all implausible.

These rates may be high as the Civil War was fought mainly in the Southern United States. One should bear in mind that the first year mortality rates of English and French settlers in North America in the 17th century in the colonies of Roanoke, Jamestown, Plymouth, and Quebec were all over 500 (see Bolton and Marshall, 1971, pp. 86, 100, 117, 138) – as high as the mortality rates of European colonists in West Africa during the late 18th century (Curtin, 1964, p. 483). Gemery (2000) and related sources point out that a number of places in the North American colonies, such as Virginia, suffered from relatively high mortality rates. More information on mortality in North America can be found in Haines and Steckel (2000).

Southeast Asia: For the final revision of the data, I replace the rate of 140 for Vietnam based solely on Cochin China in 1861, with an average based on the original source (Reynaud, 1898,

pp. 471-3). Since Cochin China in the South has an average mortality of 121.3 from 1861 to 1863 and Tonkin in the North has an average mortality of 94.6 from 1884 to 1888, I use a country-wide average of 108.1. The Dutch East Indies' (Indonesia) rate of 170 from Curtin (1989, p. 8) comes from Dutch soldiers fighting there (p. 18) and seems appropriate for the high revision. For the low revision, the first available rates from an apparently peaceful period in Indonesia (Ricklefs, 1993) from Curtin (1989, Table A37) in years 1863 to 1869 are averaged to get 64.34.

The source for Hong Kong is an anonymous two-page article from the *British Medical Journal* of 1898 (pp. 991-2) which does not say it is for Hong Kong, only that it is for the British "China Field Force" of 1860 and that the rate is "compiled from absolutely trustworthy data." The historical background can only be inferred indirectly (and with some remaining uncertainty) from sources such as Graham (1978). Although little specific information is available, the general impression from sources such as Graham (1978) and Kennedy (1970) are that Hong Kong, Singapore, and Malaysia were subject to deadly tropical disease such as cholera and malaria, and were not healthier by an order of magnitude relative to the places around them like Indonesia, as AJR's original data suggest.

South Asia: Rates for India, Bangladesh, Pakistan, and Sri Lanka are each based on soldiers in the Madras, Bengal, Bombay, (1830-38) and Ceylon (1817-36) in Curtin (1989, p. 8). For reasons not explained, AJR assign Bombay's rate of 36.99 to Pakistan and Madras' rate of 48.63 to India. While soldiers in Bengal were campaigning, the Madras rate was relatively low because the area was at peace (p. 25) as was Ceylon from 1818 onwards. I take averages of Curtin's (p. 23) mortality rates of the Madras' Army in Indian wars over the years 1793 to 1819, weighted by year, to get a mortality rate of 77.18 for India for the high revision. For Pakistan, I employ Bombay troop deaths when its armies were campaigning in Pakistan from 1839 to 1846 Curtin (p. 24) to get 63.38 for the high revision, as Bombay troops were not in Pakistan during the time considered by AJR,. The ratio of India's campaign rate to its barracks rate (1.59) is used to adjust the Sri Lanka figure upward for the high revision and the Pakistan and Bangladesh rates downward for the low revision. For this region I used information in Schwartzberg (1992).

Oceania: Soldiers in New Zealand, the country with the lowest original mortality rate, appear to have similar mortality rates in barracks and on campaign, as they incurred a mortality rate of only 7.43 during the Maori campaigns (Curtin, 1989, pp. 14-5) which is quote close to the barracks rate of 8.55, which is used for both the low and high revisions. AJR give Australia the same rate

as New Zealand, although Austin (1979. p. 259) reports higher rates even during peacetime of approximately 17 from 1839 to 1849, which I use in both revisions. Note that mortality rates to actually get as far as New Zealand or Australia were actually quite high (see below) making these very low mortality rates somewhat misleading.

Larger data sets of potential settler mortality rates seen in Rodrik et al. (2002) and Easterly and Levine (2003) include some additional mortality estimates. The rate for Afghanistan of 93.7 originally from Reynaud (1898, pp.58-61) would be valid if it were properly annualized: I believe it corresponds to a period of at least 22 months. The mortality rate for Fiji is from peaceful soldiers in New Caledonia in 1848 (Curtin, 1989, p.7) – whether this is accurate is uncertain. The rate used for Myanmar of 34.6, from Curtin (1989, p. 8) for 1829-38, should probably be replaced with the campaign rate of 119 for 1924-6, in Curtin (1989, p. 23). Mauritius's rate of 30.5 for 1818-36 (Curtin, 1989, p.7) should be adjusted upwards if it refers to soldiers in barracks. The low rates of 2.88 and 2.55 for France and the United Kingdom are from 1909 to 1913 (Curtin, 1989, p. 9) are clearly incomparable and much too low. The rates of 20.17 and 15.3 from the early-mid 19th century, listed on Curtin (1989, p.7) and mentioned by AJR, are certainly better, although they do belong to soldiers in barracks.

Other data points in these data sets are constructed by attaching mortality rates to adjoining countries. I would say in a number of cases this practice is rather questionable. For example in the data set for Rodrik et al. (2002), 10 different African countries the mortality rate for the "French Soudan" including Burundi and Rwanda.

Although I tried my best to expand on AJR's sample, there is very little additional mortality data to create comparable estimates of potential settler mortality. For those few countries which I may have a credible estimate, e.g. Myanmar, Mauritius, and Benin, I lack either comparable GDP or expropriation risk data. Other data points might be added by assigning countries the mortality rates of their neighbors, but I am not very comfortable with this technique because of the discretion it affords.

Another extension which I considered but did not incorporate was to add in the mortality cost of just getting to potential colonies from Europe. It seems reasonable that if mortality rates in a destination country deterred potential settlers, then so did the mortality rates on the voyage there. Grubb (1989) argues that potential migrants were in fact deterred from emigrating by the fear of dying, citing letters written back home of harrowing travel experiences. This is important as some places with relatively low mortality rates *in situ*, such as New Zealand and South Africa, are far from Europe. Before the 19th century, diseases on ships reguarly claimed 8% of

passengers on the way to the Southeast Asia and Oceania (Shlomowitz, 1989), and 10% of those on the way to South Africa (Riley, 1981). With data on a just a few travel routes it is straightforward to come up with monthly mortality rates while traveling, which can be used to infer the mortality rates of travel to almost all of the countries in the AJR sample. However, there is no clear theoretical model for how these "traveling" mortality rates should be joined to destination country mortality rates. [18]

[18] With some relatively rough but sensible rates I came up with, I tried fitting the first stage regression with the regressor *log(mort+φtravelmort)*, where AJR's original model imposes $\varphi = 0$. Estimating with non-linear least squares, I consistently found a negative and highly significant value for $\varphi = -0.11$, implying that places that with higher travel mortality had better institutions, a result which seems inconsistent with AJR's model.

TABLE 1: MEANS AND COVARIANCES OF MAIN VARIABLES

	Original Log Mortaltiy	Revised Log Mortality	Difference in Log Mortality	Log GDP per Capita	Exprop-riation Risk
High Revision					
Mean	4.65	4.62	0.02	8.05	6.52
			Covariance Matrix		
Original Log Mortality *m*	1.57				
Revised Log Mortality *n*	0.91	0.76			
Difference in Log Mortalty *d*	0.66	0.15	0.52		
Log GDP per capita *y*	-0.89	-0.48	-0.41	1.10	
Expropriation Risk *r*	-0.96	-0.44	-0.52	1.11	2.16
IV estimate α	0.93	1.10		OLS estimate α	0.52
Reduced Form estimate π	-0.57	-0.64			
First stage estimate β	-0.61	-0.58		Sample Size	64
First stage residual var σ^2	1.59	1.95			
Homoscedastic std. error (β)	0.13	0.20			
Low Revision					
Mean	4.23	3.35	0.88	6.94	8.44
			Covariance Matrix		
Original Log Mortality *m*	1.10				
Revised Log Mortality *n*	0.52	0.49			
Difference in Log Mortalty *d*	0.58	0.04	0.54		
Log GDP per capita *y*	-0.62	-0.40	-0.22	0.78	
Expropriation Risk *r*	-0.62	-0.28	-0.35	0.83	1.90
IV estimate α	0.99	1.43		OLS estimate α	0.44
Reduced Form estimate π	-0.56	-0.81			
First stage estimate β	-0.56	-0.57		Sample Size	43
First stage residual var σ^2	1.59	1.79			
Homoscedastic std. error (β)	0.15	0.24			

The variable *Original Log Mortality* is the logarithm of European settler mortality rates from Acemoglu, Johnson, and Robinson (2001). *High Revision and Low Revision* are the revised mortality rates explained in the text and given in the Appendix Table. *Difference in Log Mortality* is simply the difference between the two variables. *Expropriation Risk* is "Average protection against expropriation risk 1985-1995," by Political Risk Services. It is on a scale from 0 to 10, where a higher score represents greater protection of property against expropriation. *Log GDP Per Capita* is GDP per capita in 1995 on a PPP basis from the World Bank WDI database. See text and Table 2 for more detail.

TABLE 2: FIRST STAGE ESTIMATES
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continents: Asia, Afr, "Other" (4)	Continents & Latitude (5)	Mean Temp. (6)	Minimum Monthly Rain (7)	Mean Temp & Min Rain (8)	Percent European, 1975 (9)	Malaria in 1994 (10)
Panel A: Original Data										
Log Mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.45	-0.46	-0.26	-0.42	-0.52
{homoscedastic s.e.}	{0.13}	{0.14}	{0.13}	{0.17}	{0.18}	{0.14}	{0.13}	{0.15}	{0.14}	{0.18}
(clusered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.20)	(0.16)	(0.18)	(0.19)	(0.22)
Significance *p*- values										
Log Mortality	0.001	0.011	0.025	0.037	0.108	0.031	0.007	0.154	0.033	0.023
Controls	-	0.174	-	0.397	0.341	0.024	0.002	0.000	0.021	0.398
Panel B: High Revision										
Log Mortality (β)	-0.56	-0.34	-0.23	-0.17	-0.02	-0.25	-0.35	-0.06	-0.19	-0.17
(clusered s.e.)	(0.29)	(0.31)	(0.26)	(0.28)	(0.28)	(0.29)	(0.25)	(0.24)	(0.28)	(0.38)
Significance *p*- values										
Log Mortality	0.063	0.285	0.396	0.564	0.946	0.391	0.165	0.801	0.517	0.652
Controls	-	0.107	-	0.030	0.028	0.008	0.002	0.000	0.034	0.061
Panel C: Low Revision										
Log Mortality (β)	-0.57	-0.38	-0.15	-0.51	-0.38	-0.02	-0.41	0.18	-0.10	-0.72
(clusered s.e.)	(0.31)	(0.33)	(0.21)	(0.25)	(0.28)	(0.36)	(0.26)	(0.22)	(0.35)	(0.42)
Significance *p*- values										
Log Mortality	0.081	0.261	0.505	0.055	0.199	0.965	0.131	0.412	0.773	0.099
Controls	-	0.196	-	0.109	0.095	0.018	0.001	0.000	0.048	0.769

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 (Acemoglu, Johnson, and Robinson, 2001). Revision of the mortality data is given in the Appendix Table. Standard errors assuming homoscedasticitic errors are shown in parentheses {} in Panel A. All other standard errors and tests adjust for clustering effects. *Significance p-values* gives are standard F-tests of whether the log mortality or the controls are significant in the regression. Sample sizes are 64 with original mortality and the high revision, and 43 with low revision, except in column (3) with 60 and 39, respectively, and column (10) which excludes Malta, Guyana, and the Bahamas, giving sizes of 61 and 42 respectively. Number of clusters for original, high and low revisions are 36, 31, and 22, respectively, except in (3) 33, 28, 18, and (10) 35, 30, 21 The three continent variables included are *Africa* and *Asia*, defined obviously and *Other"* which includes Australia, New Zealand and Malta. "Neo-Europes" refer to USA, Canada, Australia, and New Zealand. Minimum monthly rainfall and mean temperature are taken from Parker (1997). Percent of *European Descent in 1975* is the percent of the population with European descent in 1975 from AJR. *Malaria in 1994* refers to percent of the population with endemic malaria in 1994 in Gallup and Sachs (2001). All variables except revised settler mortality and minimum monthly rainfall are used in AJR. See text for more detail.

TABLE 3: INSTRUMENTAL VARIABLE ESTIMATES

(First Stage Dependent Variable: Expropriation Risk; Second Stage Dependent Variable, Log GDP per captia, 1995, PPP basis)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continents: Asia, Afr, "Other" (4)	Continents & Latitude (5)	Mean Temp (6)	Minimum Monthly Rain (7)	Mean Temp & Min Rain (8)	Percent European, 1975 (9)	Malaria in 1994 (10)
Panel A: Original Mortality (N=64)										
Expropriation Risk (α)	0.93	0.96	1.24	0.97	1.07	1.06	1.05	1.41	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]	[0.33, 1.79]	[0.40, 1.71]	[-0.33, 3.14]	[0.28, 1.56]	[0.23, 1.01]
AR 95% Conf. Region	[0.66, 1.85]	[0.64, 2.33]	[0.73, 7.40]	[0.50, 7.51]	(-∞, -4.33] ∪ [0.43,+∞)	[0.69, 3.46]	[0.68, 2.50]	(-∞, -3.41] ∪ [0.66,+∞)	[0.52, 5.96]	[0.29, 1.82]
Panel B: High Revision (N=64)										
Expropriation Risk (α)	1.12	1.39	2.26	1.71	9.91	1.73	1.41	5.22	1.67	0.84
Wald 95% Conf. Region	[0.23, 2.02]	[-0.59, 3.37]	[-2.17, 6.69]	[-3.27, 6.69]	[-277, 297]	[-1.61, 5.07]	[-0.27, 3.09]	[-34.6, 45.1]	[-2.74, 6.08]	[-1.95, 3.62]
AR 95% Conf. Region	(-∞, -12.2] ∪ [0.63,+∞)	(-∞, -0.78] ∪ [0.66,+∞)	(-∞, -0.62] ∪ [0.78,+∞)	(-∞, +∞)	(-∞, +∞)	(-∞, -0.74] ∪ [0.76,+∞)	(-∞, -3.28] ∪ [0.68,+∞)	(-∞, -0.24] ∪ [0.81,+∞)	(-∞, 0.01] ∪ [0.30,+∞)	(-∞, +∞)
Panel C: Low Revision (N=43)										
Expropriation Risk (α)	1.43	1.92	4.55	1.25	1.54	35.45	1.77	-2.61	4.66	1.00
Wald 95% Conf. Region	[0.04, 2.82]	[-1.14, 4.98]	[-8.86, 18.0]	[0.36, 2.14]	[-0.13, 3.22]	[-1582, 1653]	[-0.45, 4.00]	[-9.45, 4.23]	-26.1, 35.43	[0.22, 1.77]
AR 95% Conf. Region	(-∞, -8.17] ∪ [0.72,+∞)	(-∞, -1.62] ∪ [0.83,+∞)	(-∞, -1.69] ∪ [1.09,+∞)	(-∞, -1.91] ∪ [0.72,+∞)	(-∞, -0.56] ∪ [0.74,+∞)	(-∞, -0.40] ∪ [1.13 ,+∞)	(-∞, -9.65] ∪ [0.83,+∞)	(-∞, -0.28] ∪ [1.78,+∞)	(-∞, -0.17] ∪ [0.84,+∞)	(-∞, -63.3] ∪ [0.59,+∞)

Panels present the IV estimates of *Expropriation Risk* on *Log GDP per Capita* using *Log Mortality* as an instrument using the same variables and sample sizes described in Table 2. *AR 95% Conf. Region* refers to confidence regions calculated the Anderson-Rubin (1949) statistic while *Wald 95% Conf Region* are the standard (erroneous) IV confidence intervals calculated using the Wald statistic. Clustering effects are corrected for in both.

TABLE 4: FIRST STAGE SENSITIVITY TO THE MORTALITY DATA
(Dependent variable: Expropriation Risk, no control varaibles included)

Panel A: High Revision	Full Sample (1)	Better Subsample (2)	Top 10 Changers Original (3)	No Africa (4)	No Africa or Neo-Europes (5)	Median Regress-ion (6)
Log Mortality	-0.56	-0.49	-0.63	-1.26	-0.55	-0.47
(clustered s.e.)	(0.29)	(0.33)	(0.18)	(0.27)	(0.56)	(0.42)
p- value	0.063	0.151	0.001	0.000	0.340	0.268
Sample Size	64	36	64	37	33	64
Clusters	31	26	35	17	14	-

Panel B: Low Revision	Full Sample (1)	No Late Obs (2)	Gutierrez Data High (3)	No Africa (4)	No Africa or Neo-Europes (5)	Median Regress-ion (6)
Log Mortality	-0.57	-0.82	-1.02	-0.94	-0.34	-0.33
(clustered s.e.)	(0.34)	(0.39)	(0.33)	(0.43)	(0.34)	(0.38)
p- value	0.081	0.053	0.006	0.044	0.338	0.390
Sample Size	43	37	43	35	31	43
Clusters	22	18	21	18	14	-

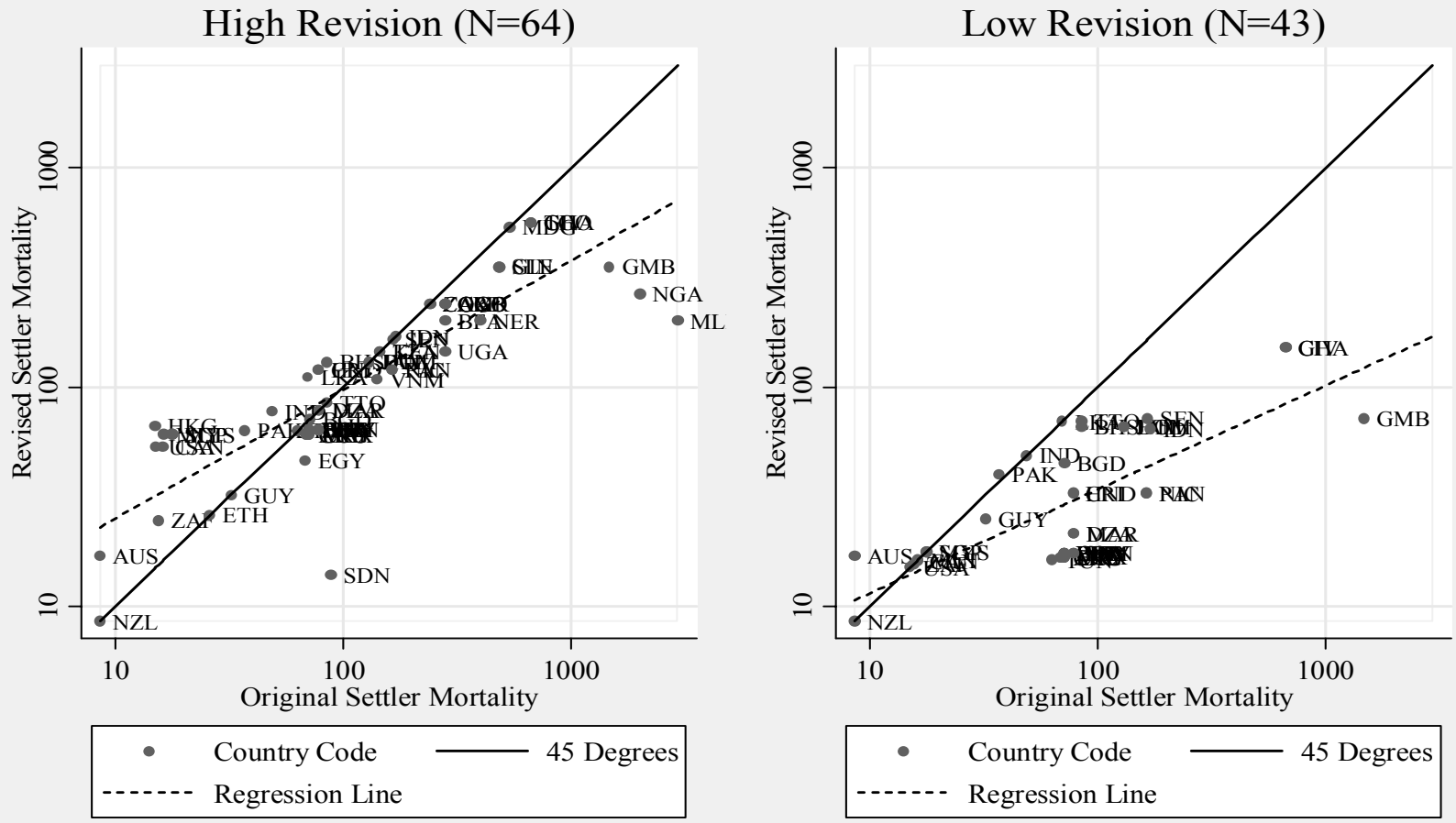
See Table 2 for explanations of variables and methods as well as text for additional information. *Clusters* refers to the number of different mortality rates, some of which are shared by different observations. Whether an observation is part of the *Better Subsample*, is a *Top 10 Changer*, considered *Late*, or used *Gutierrez Data* is listed in the Appendix Table. Median regression standard errors are bootstrapped based on 1,000 replications.

TABLE 5: FIRST STAGE REGRESSIONS USING ALTERNATE MEASURES OF PROPERTY RIGHTS INSTITUTIONS

Dependent Variable	Constraint on Executive, 1990				Law and Order Tradition, 1995			
Control Variables	No Controls (1)	Asia, Africa, "Other" (2)	Mean Temp (3)	European Descent in 1975 (4)	No Controls (5)	Asia, Africa, "Other" (6)	Mean Temp (7)	European Descent in 1975 (8)
Panel A: High Revision *(N=60)*					*(N=63)*			
Log Mortality (β)	-1.12	-0.34	-0.87	-0.50	-0.59	-0.30	-0.39	-0.35
(clusered s.e.)	(0.33)	(0.38)	(0.37)	(0.36)	(0.19)	(0.19)	(0.21)	(0.19)
Significance *p*- values								
Log Mortality	0.002	0.388	0.026	0.180	0.004	0.123	0.082	0.079
Controls	-	0.000	0.077	0.001	-	0.001	0.011	0.014
Panel B: Low Revision *(N=40)*					*(N=42)*			
Log Mortality (β)	-1.06	-0.53	-0.82	-0.40	-0.68	-0.55	-0.33	-0.31
(clusered s.e.)	(0.44)	(0.46)	(0.53)	(0.56)	(0.27)	(0.24)	(0.38)	(0.32)
Significance *p*- values								
Log Mortality	0.024	0.264	0.138	0.486	0.021	0.034	0.402	0.343
Controls	-	0.027	0.201	0.014	-	0.002	0.078	0.043

Constraint on Executive in 1990 is on a scale form 1 to 7 with a higher score indicating more constraints from the *Polity III* data set. *Law and Order Tradition in 1995* is measured on a scale from 0 to 6 where a higher score means more law and order, from Political Risk Services. Columns (1)-(4) do not include the Bahamas, Hong Kong, Malta, Sierra Leone and Columns (5)-(8) do not include El Salvador. All variables, except revised log mortality, are used by AJR. See other tables and text for more detail.

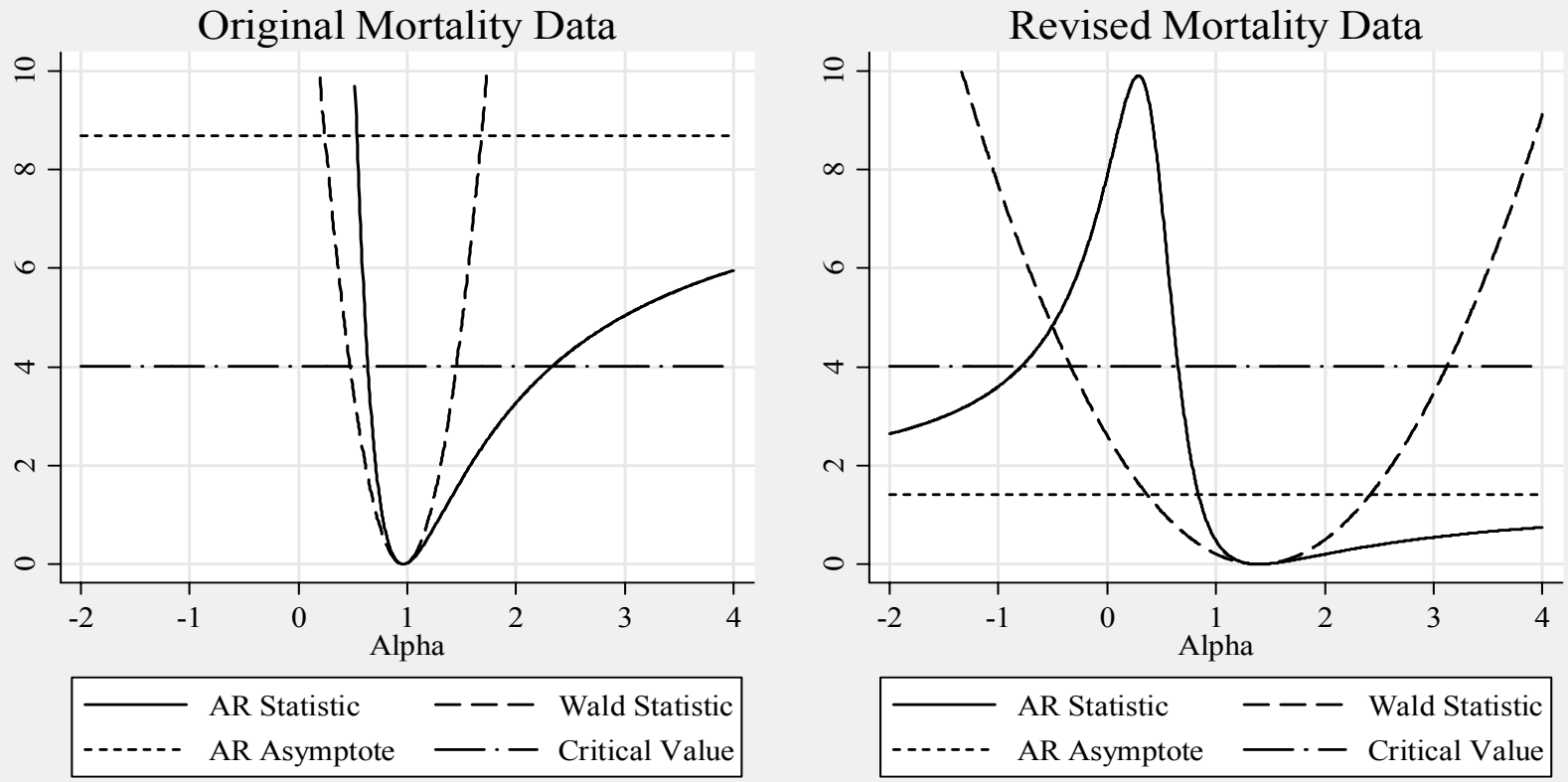
Figure 1: Original and Revised Mortality Compared



See text for details and Appendix Table 1 for exact rates

Figure 2: Anderson-Rubin and Wald Statistics Compared

Statistic for coefficient on Expropriation Risk with Latitude Control
in equation determining Log GDP per capita using Log Mortality as an instrument.



High revision of mortality data used. Critical value at 95%. See Table 3, col(2) for details.

APPENDIX TABLE: ORIGINAL AND REVISED MORTALITY DATA

Country Name	Code	Original Mortality	High Revision	Low Revision	Better Subsample	Top 10 Changer	Late Obs	Gutierrez Data
Angola	AGO	280	240					
Argentina	ARG	68.9	61	16.7				x
Australia	AUS	8.55	17	17	x			
Burkina Faso	BFA	280	200.24		x			
Bangladesh	BGD	71.41	71.41	44.98	x			
Bahamas	BHS	85	130	66				
Bolivia	BOL	71	64	17.5				x
Brazil	BRA	71	64	17.5				x
Canada	CAN	16.1	53.4	16.1	x	x		
Chile	CHL	68.9	61	16.7				x
Cote d'Ivoire	CIV	668	562	151	x		x	
Cameroon	CMR	280	240					
Congo	COG	240	240					
Colombia	COL	71	64	17.5				x
Costa Rica	CRI	78.1	119.9	32.8				
Dominican Re	DOM	130	130	66	x			
Algeria	DZA	78.2	78.2	21.4	x			
Ecuador	ECU	71	61	16.8				x
Egypt	EGY	67.8	46.26		x			
Ethiopia	ETH	26	26		x			
Gabon	GAB	280	240					
Ghana	GHA	668	562	151	x		x	
Guinea	GIN	483	353					
Gambia	GMB	1470	353	72	x	x	x	
Guatemala	GTM	71	64	17.5				x
Guyana	GUY	32.18	32.18	25	x			
Hong Kong	HKG	14.9	66.45			x		
Honduras	HND	78.1	119.9	32.8				
Haiti	HTI	130	130	66	x			
Indonesia	IDN	170	170	64.34	x		x	
India	IND	48.63	77.2	48.63	x			
Jamaica	JAM	130	130	66	x			
Kenya	KEN	145	145					
Sri Lanka	LKA	69.8	110.8	69.8	x			
Morocco	MAR	78.2	78.2	21.4	x			
Madagascar	MDG	536.04	536.04		x			
Mexico	MEX	71	61	16.8	x			x
Mali	MLI	2940	200.24		x	x		
Malta	MLT	16.3	61	16.3	x	x		x
Malaysia	MYS	17.7	60.88	17.7	x	x		
Niger	NER	400	200.24		x			
Nigeria	NGA	2004	266.5		x	x		
Nicaragua	NIC	163.3	119.9	32.8				
New Zealand	NZL	8.55	8.55	8.55	x		x	
Pakistan	PAK	36.99	63.4	39.92	x			
Panama	PAN	163.3	119.9	32.8				
Peru	PER	71	64	17.5				x
Paraguay	PRY	78.1	64	17.5				x
Sudan	SDN	88.2	13.87		x	x		
Senegal	SEN	164.66	164.66	72	x		x	
Singapore	SGP	17.7	60.88	17.7		x		
Sierra Leone	SLE	483	353		x			
El Salvador	SLV	78.1	64	17.5				x
Togo	TGO	668	562		x			
Trinidad and Tobago	TTO	85	85	70	x			
Tunisia	TUN	63	63.3	16.3	x			x
Tanzania	TZA	145	145					
Uganda	UGA	280	145					
Uruguary	URY	71	61	16.8				x
USA	USA	15	53.4	15	x	x		
Venezuela	VEN	78.1	64	17.5				x
Vietnam	VNM	140	108.1		x			
South Africa	ZAF	15.5	24.58	15.5	x			
Zaire	ZAR	240	240					

See text and Data Appendix for description and explanation of the variables.