Title

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Abstract

In a seminal contribution, Acemoglu, Johnson, and Robinson (2001) evaluate the effect of property rights institutions on national income using estimated mortality rates of early European settlers as an instrument for the risk of capital expropriation. Returning to their original sources, I find the settler mortality data suffer from a number of inconsistencies, comparability problems, and questionable geographic assignments. When various methods are used to deal with these issues, the first-stage relationship between mortality and expropriation risk is no longer robust and typically insignificant. Consequently instrumental variable estimates are unreliable and suffer from weak instrument pathologies.

Keywords: economic history, development, institutions, growth, colonialism, property rights, European settlement, mortality, measurement error, weak instrument.

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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 endeavor is complicated by the fact that the correlation between institutional and economic measures may reflect reverse effects of the economy on institutional development or the simultaneous influence of omitted factors on both economic output and institutions. AJR's strategy to circumvent these problems involves using an instrumental variable (IV) for property rights institutions, measured by risk of capital expropriation, in an equation determining GDP per capita across previously colonized countries.

AJR argue that during the colonial period, Europeans were more likely to settle places where they had a lower risk of dying from 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 argue, the direct effects of mortality from disease and European settlement on national income faded, while the indirect effect through property rights institutions persisted. Their argument motivates the use of potential European settler mortality rates as an instrument for risk of capital expropriation. With their innovative econometric model, AJR find a large effect of expropriation risk on GDP per capita, explaining much of the variation in income across countries.

Because historical sources containing information on mortality rates are thin, constructing potential European settler mortality rates is no easy task. AJR make a valiant effort to construct a good series, combining disparate sources containing the mortality rates of soldiers, bishops and laborers, proposing a series of rates across 64 countries. Researchers have been eager to use this new and interesting series, especially given its promise as an instrumental variable for institutions. Currently, over twenty published articles, and many more working papers, make use of AJR's settler mortality data.

The first section of this paper shows that despite AJR's ingenuity and diligence, there are a number of reasons to doubt the reliability and comparability of their European settler mortality rates and the conclusions which depend on them. First, AJR choose rates for some countries in a manner which appears inconsistent with the selection criteria they initially set. Second, the mortality rates from different populations of soldiers, bishops, and laborers seem often incomparable due to differences in the conditions and circumstances facing these populations. For example, AJR do not account for whether mortality rates come from soldiers who are out on a campaign or are based in their own barracks. Nor do AJR consider how pre-existing European settlement or economic development may make their measured mortality rates endogenous in a way beneficial to their hypothesis. Finally, a majority of countries in their sample are assigned mortality rates based on neighboring countries: given the wide variation in AJR's mortality rates for countries close together, such assignments seem often tenuous and sensitive to choices made in selecting neighbors. I show that because of these issues, the rates of many countries can differ by up to an order of magnitude depending on what choices are made. [1]

Not only do these issues cast doubt on the accuracy and usefulness of AJR's mortality data, but they undermine the finding that settler mortality rates are a robust predictor of expropriation risk. In the second section of this paper, the sensitivity of AJR's results to weaknesses in the data is examined using three data checks: revising the mortality rates of a few countries to eliminate evident inconsistencies, adding dummy variables in the regression analysis to indicate rates from laborers or campaigning soldiers, and dropping countries assigned mortality rates from other countries. Any one of these checks applied alone lowers the significance of mortality rates remarkably, demonstrating that shortcomings of the original data mask the correlation of mortality rates with many other possible determinants of institutions and GDP. When these checks are combined, the significance of mortality falls to very low levels, and point estimates sometimes reverse in sign. Because the empirical relationship between settler mortality rates and expropriation risk is weak and non-robust, the IV estimates of the effect of expropriation risk on GDP per capita become highly unreliable, suffering from a number of "weak instrument" pathologies: point estimates exhibit little stability and correctly computed confidence regions are often unbounded.[2]

[1] In their NBER Working Paper (2001), AJR provide estimates using alternate versions of the mortality data to deal with some issues related to mortality rates based on bishops and rates based on epidemic periods.
[2] Dollar and Kraay (2003) document evidence of the weak instrument problem when settler mortality is used with another instrument and two endogenous variables.

I. Problems with the Settler Mortality Data

Attempting to assemble a large and comparable set of mortality rates across countries, AJR construct their mortality rates in a sequence of four steps, described in their own Data Appendix. In the first step they take average mortality rates from a table in Curtin (1989, pp. 7-8) of European soldiers *from disease* (not combat) in the early to mid nineteenth century. In step two, AJR add new countries to their sample by using average mortality rates from a selection of military campaigns in Curtin (1998). AJR state that when more than one rate is available, they take the earliest rate.[3] In step three, they use mortality rates from Curtin et al. (1995) of African forced laborers who were moved to foreign disease environments. Also in step three, AJR assign mortality rates to neighboring countries which they say have similar disease environments. Finally in the fourth step, AJR use mortality rates of Latin American bishops from Gutierrez (1986), in combination with Mexican soldier data, to impute mortality rates for sixteen Latin American countries. Rates are given in the number of deaths per year per thousand at risk.

Although there are 64 countries in AJR's sample, these use only 36 distinct mortality rates, with only 28 countries being the source of their own mortality rate. A total of 16 countries have rates imputed from bishops and 4 from laborers. The settler mortality rates are catalogued in Appendix Table A1. In order to keep the discussion here relatively brief, a great deal of detail is left to the Appendix.

A. Inconsistencies

Assembling a consistent set of mortality rates from different sources is challenging, and it appears that AJR made some inconsistent choices in assembling their series.

One set of inconsistencies involves departures from their rule of always taking the first available rate. An example involves the Sudan: the first, second, and third mortality rates Curtin (1998) reports are *zero* (p.173), 10.9 (p.169), and 88.2 (p.173), from 1884, the first half of 1885, and the second half of 1885 when there were troop strengths of 4500, 7235, and 463, respectively.

[3] In AJR's own words (Data Appendix, p. 2), "Whenever Curtin provides more than one estimate, we use the earliest available estimate." See the Appendix (Section I) for further discussion of AJR's treatment of multiple estimates.

AJR choose the third and highest rate of 88.2, based on the smallest sample of soldiers, yet their rules imply they should choose zero; if they have grounds for believing that the reported rate of zero was an error, then their rules imply they should use 10.9. For Egypt and Madagascar, AJR also did not use the first available rates.[4]

A second set of inconsistencies involves not using average mortality rates. In most of their sample, AJR use average rates, but for countries which depend on data from coerced African laborers (from foreign disease environments, Curtin et al., 1995, pp. 463, 491), AJR instead use *maximum* mortality rates, although without stating so in their Data Appendix.[5] Thus for Congo AJR use a maximum rate of 240, rather than the average rate of 100; for Kenya they use the maximum rate of 145, perhaps because no average is reported. These choices are inconsistent with the rest of the data. In fact, AJR's argument for why laborer and soldier mortality are comparable depends on observed average mortality of black and white soldiers in Curtin (1968).

A peculiar inconsistency arises from mortality rates based on several French campaigns in western Mali, for which Curtin (1998) gives mortality statistics for two months in 1874 and from 1880 onwards. From what appears to be a misunderstanding of tables in Curtin's book – although there is little ambiguity in the text about where the campaigns occurred (pp. 74 to 89 with a map on p. 77) – AJR assigned Mali a rate of 2940 from 1874, Niger a rate of 400 from 1880-83, and Burkina Faso, Cameroon, Gabon, Angola and Uganda a rate of 280 from 1883 to 1884, as illustrated in Figure 1. This assignment pattern is hard to justify given that all three rates are from western Mali; all of these countries should have been given the same rate.

Furthermore, the pattern of mortality rate assignments from Mali is not intuitive. Countries could have been assigned mortality rates from other, often closer countries: Burkina Faso borders Ghana, Niger borders Algeria and Nigeria, Gabon and Angola border Congo, Cameroon borders Congo and Nigeria, and Uganda borders Kenya and Sudan; only Burkina Faso and Niger border Mali. It seems unlikely that all countries using Mali-based rates share the same disease environment. For example, Angola consists primarily of an elevated plateau region quite different from the rain forests of Gabon, or the savannas and deserts of Mali.

[4] Cases where specifics are not provided in the text are discussed in more detail in the Appendix.
[5] That the mortality rates are maxima is clear in the original source (Curtin, 1995). The relevant passages are quoted in the Appendix of this paper.

B. Limited Comparability and Possible Endogeneity of Mortality Rates

Curtin's works are concerned primarily with the health and mortality of soldiers during the European conquests of the nineteenth century.[6] Accordingly, he took as given the current circumstances and living conditions of the soldiers when comparing their mortality rates. However, these rates do not necessarily provide a good proxy for potential European settler mortality, which would ideally look at settlers with similar basic living conditions, subject to the constraints imposed by their environments.

Living conditions have a large effect on mortality rates from disease. Curtin (1989, pp. 40-61) discusses how access to clean water and adequate sewage disposal can prove instrumental in lowering mortality rates from waterborne diseases, such as typhoid and other gastrointestinal infections, which play a large role in many of the mortality rates observed (e.g. North Africa, Mexico, India, Vietnam). Adequate shelter, to keep warm and prevent the contagion of disease, and nutritious food, with sufficient vitamins, can also lower mortality from disease. Furthermore, protection from tropical disease is attainable at high altitudes far from infectious mosquitoes. Mortality rates of soldiers declined during the nineteenth century more because of improvements in living conditions than because of advances in tropical medicine (Curtin, 1989, pp. 37, 160).

The most widespread lack of comparability found in AJR's data comes from the intermingling of mortality rates of soldiers on campaign (from disease only) with mortality rates of soldiers in barracks. In his books, Curtin frequently makes reference to "barracks rates" and "campaign rates," asserting (1989, p. 4) 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." Soldiers on campaign take fewer precautions against disease and are less likely to have safe water, fresh food, decent shelter, or sewage disposal. Even in Europe, where the barracks rate for England was 15.3 (Curtin, 1989, p. 7), British troops on campaign in Spain from 1811 to 1814 and in the Netherlands in 1809 suffered high disease

[6] This is evident in (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 (1998), *Disease and Empire: The Health of European Troops in the Conquest of Africa.*

mortality rates of 118.6 and 332 (Balfour, 1845, p. 198).[7] Campaign rates are usually higher than barracks rates, although there is no stable relationship between the two. For the mid to late nineteenth century, Curtin (1998, p. 224) documents how during campaigns mortality from malaria increases by typically more than 100 percent, from gastrointestinal infections by more than 200 percent, and from typhoid by more than 600 percent, producing overall mortality rates which are 66 to 2000 percent higher than barracks rates.

The distinction between barracks and campaign rates affects AJR's analysis since campaign rates are more likely to be used in countries with high risk of capital expropriation and low GDP per capita.[8] 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. Measurement error correlated with endogenous variables can lead to inconsistent estimates and standard errors, both in ordinary least squares (OLS) and IV regressions.[9]

The effects of campaigning on mortality are evident in North Africa, where according to Curtin (1989, p. 17) mortality rates are similar to Southern Europe in more peaceful conditions.[10] AJR use campaign rates of 78.2 for Algeria (also assigned to Morocco) and 63 for Tunisia. This is about triple what is found in southern Europe in peaceful conditions, seen in their rate of 16.3 for Malta (70 kilometers east of Tunisia), as well as other rates from the same table (Curtin, 1989, pp.7-8) of 21.4 for Gibraltar (22 kilometers north of Morocco) and 25.2 for the Ionian islands. Most of the deaths from disease in North Africa were from typhoid and digestive diseases, with malaria playing a minor role (Curtin, 1989, p. 36; 1998, pp. 152, 158, 169).

For Malaysia AJR use a barracks rate of 17.7 from 1829-38 while for Indonesia they use

[7] This source was used in AJR's (2005) Response, although they did not mention these rates. AJR discovered many valuable additional sources in their Response, including Tulloch (1847), Cantlie (1974), and others mentioned in the Appendix.

[8] At 2 percent size one rejects the null hypotheses that either expropriation risk or log GDP per capita is unrelated to a dummy variable indicating whether a country's rate is taken from a campaign.

[9] AJR (footnote 17) mention that their data contain measurement error, but that "this measurement error 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.

[10] "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." AJR disagree (Response, p. 22) with my interpretation of this passage, although it seems clear.

a campaign rate of 180 from 1819-28. The supposition that the difference between these two rates reflects differences in the disease environment is hard to accept given Curtin's writing[11] and the proximity of the two countries: the eastern half of Malaysia shares the island of Borneo with Indonesia; the western half is separated from Indonesia (Sumatra) by the Strait of Malacca, at some points only a few kilometers wide. According to AJR (Appendix, p. 1), "This is because there exists substantial variation in disease environment, particularly for malaria, even in neighboring areas." However, if variation is so great, then it is hard to trust that such differences apply to the entire archipelago of Indonesia, which has over 6,000 inhabited islands.

A deeper consideration overlooked in AJR's data is the influence of pre-existing European settlement and economic development on measured mortality rates. This is of particular concern with countries in the Americas, first settled in the sixteenth and seventeenth centuries, Australia and New Zealand, first settled in the late eighteenth and early nineteenth centuries, and Malta, resettled at the turn of the twelfth century. With existing settlements or more economic development, soldiers had better access to water, food, and housing as well as local knowledge on how to deal with the environment. According to Curtin (1998, p. 113), "Typhoid had become a 'tropical disease' – because the tropical world is poor, not because of climate." If measured mortality rates are negatively related to economic development in the nineteenth century, and early development is positively related to current development, then this creates a bias in favor of AJR's hypothesis.[12]

For the United States, AJR use a barracks rate of 15 corresponding to northern American soldiers from 1829 to 1838, over two hundred years after the first permanent British colony, Jamestown, was established. The mortality rate of the Jamestown settlers was much higher than AJR's rate suggests: in the first year of settlement, 1607, Earle (1979) estimates that between 27 and 45 percent of colonists died from dysentery and typhoid.[13] Primarily because of a bad water

[11]"The very high mortality rate for the Dutch East Indies (Indonesia) was unrepresentative for the same reason the Algerian figure was. These years included those of the Java War, with tough campaigns, high casualties from combat and high disease rates" (Curtin, 1989, p.18).

[12] Furthermore, the assumption in AJR's over-identification test that European settlement is exogenous is hard to maintain as colonists were attracted to places where economic prospects were favorable.

[13] Curtin (1998, p. 116) refers to this source, although he cites a higher mortality number, stating "In 1607 to 1624… settlers in Jamestown in Virginia died at an annual rate of about 500 per thousand, and the principal killer may have been typhoid."

supply and crowding, the average mortality rate from disease for Jamestown until 1624 was an estimated 283 (p. 121). The Jamestown experience was not unique in North America: early European settlers in Quebec, Sainte Croix Island, and Plymouth also faced disastrous initial settler mortality, with 50 percent or more dying of scurvy and other diseases because of inadequate diets and housing (Bolton and Marshall, 1971; Trudel, 1973).

In 1624 the Crown took over the Jamestown colony from private hands as those in charge were deemed "unable to provide for the people's health and welfare," (Wells, 1979, pp. 158-9). Mortality then fell to a rate of 142 over the next decade (Earle, 1979, p. 121). This takeover raises the question of whether settler mortality rates were also endogenous to institutions: good early institutions may have lowered mortality rates for settlers and soldiers.[14]

Another concern is that year-to-year, or even month-to-month, fluctuations in mortality play a large role in AJR's rates as these are often based on short periods of time. While West Africa and the Caribbean were certainly unhealthy relative to other regions, it is not clear how much differences in observed mortality between countries within these regions are due to permanent differences rather than transitory ones. Curtin generally writes of differences in mortality between regions consisting of several countries, rather than between individual countries. Due to the volatility of mortality in the Caribbean, Curtin (1989, p. 25) observes "no island was systematically more healthy or less healthy than any other in the first of the nineteenth century." Actual permanent differences in mortality may be more collinear with climate, geography, and other variables than AJR's series, with its transitory noise, would suggest. For example, the rates for Gambia, Nigeria, and Mali of 1470, 2004, and 2940, are taken exclusively from epidemic periods when mortality rates temporarily spiked. They are an order of magnitude higher than rates of neighboring Senegal, Cameroon, and Algeria, of 164.66, 280, and 78.2.

The wide range of mortality rates AJR take from Mali alone, from 280 to 2940, illustrates how AJR's method is sensitive to timing. The abnormally high rate of 2940 comes from a two-month campaign in 1874 during an outbreak of yellow fever in which 49 percent of individuals

[14] Colonial attitudes were also important (Curtin, 1995, pp. 491-3); mortality in some parts of Africa was high because authorities refused to learn from natives on how to deal with their disease environment. Even the development of tropical medicine may have been endogenous to colonial interests: according to Curtin (1998, p. x), "The desire for tropical empire may well have been… the principal incentive for research and discoveries in tropical hygiene."

died (Curtin, 1998, p. 81). Although Curtin (p. 81) states that because of acquired immunity, "the annual rate and the rate of loss over two months would have been about the same," AJR take the two month rate of 490 and multiply it by six to infer an annual rate.

C. Questionable Matching of Mortality Rates to Neighboring Countries

Because of the paucity of available data, AJR extend the data they have on a limited set of countries to a set of 64 countries. AJR state they assign "a mortality number to a country if it neighbors a country for which we have data and has the same disease environment," (Appendix, p. 2). However, they provide little detail on how they judge when disease environments are similar. Given that AJR assign adjacent countries rather different rates, their mortality series is quite sensitive to how neighboring countries are chosen.

Moreover, if AJR's argument (Appendix, p.1) that large differences in mortality rates occur between neighboring countries because of differences in microclimates is correct, then it seems incongruous to assign neighboring countries the same mortality rate. It also raises the question of how to interpret a single mortality rate assigned to a country with multiple microclimates. If, on the other hand, microclimates do not matter and mortality varies little across neighboring countries, then mortality rates, free of measurement noise from transitory fluctuations or differences in living conditions, will likely be highly collinear with other variables which may affect institutions or GDP. Whether or not microclimates produce variations in mortality rates, estimation using these rates from limited data faces serious obstacles.

In a number of instances AJR's assignments seem questionable, including the case above where three rates from Mali are assigned to six countries. Another instance involves Hong Kong: here AJR use a (non-annualized) rate of 14.9 belonging to a British force which left Hong Kong in springtime but spent most of 1860 campaigning around Beijing, thousands of kilometers to the north. As AJR report in their Response (p. 32), a barracks mortality rate observed in Hong Kong (Tulloch, 1847, p. 254) from 1842 to 1845 was 285. One observer at the time called Hong Kong "an unhealthy, pestilential, unprofitable and barren rock" (Cantlie, 1974, p. 480).

For sixteen Latin American countries with no soldier mortality rates, AJR assign rates using a "benchmarking" system which ties the mortality rates of a handful of bishops in Gutierrez (1986) to a single campaign rate in Mexico. AJR explain (Appendix, p. 4)

> Gutierrez calculates the mortality of bishops aged 40-49 for three disease environments in the Caribbean, Central and South America: 10 (low), 11 (medium), and 23 (high) per 1000. We assume the ratio between bishop mortality levels was the same as the ratio between soldier mortality. We also assume that the type of disease environment (low, medium, high) was the same for bishops and soldiers. We then use Gutierrez's ratios with the Curtin (1998) estimate of mortality of 71 per 1000 for Mexico 1862-63 (a low mortality region in Gutierrez's classification), to generate estimates of mortality for Central and South America.

This method results in rates of 71, 78.1, and 163.3 for low, medium, and high mortality regions.[15]

This methodology may be problematic: the bishop data are quite thin, the proper assignment of countries to mortality regions is unclear, and alternative systems of benchmarking produce widely varying rates. The bishop mortality rates of 16.7, 17.5 and 32.8 found in Gutierrez (1986, Annexe 3) 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 ten years. Because of the small samples, the standard errors of the estimates are large (7.8, 7.2, and 8.6), and even the most powerful test that all three regions have the same mortality rate cannot be rejected at a level of 12 percent using a standard *F*-test. Probably because Latin America was already somewhat settled by Europeans in the seventeenth and eighteenth century, the mortality rates of the bishops are fairly low, resembling the mortality rates of adults aged 40 to 49 in Europe over the same period.[16] If bishops in Latin America died at rates similar to those in Europe, then possibly settlers also died at similar rates, rather than at the much higher rates implied by AJR's particular benchmarking system.

Another weakness of the Gutierrez (1986) data is that the information he provides on disease environments is very thin: he classifies disease environments according to temperature

[15] A map of the mortality rate assignments is shown in Appendix Figure A1.
[16] This is based on the mortality rates of Swedes. See the Appendix, p. 11, for more detail.

and uses it to classify a small number of selected cities, not countries. AJR's claim that Gutierrez "provides evidence that these regions share similar disease environments" is unsupported as it relies on Gutierrez's own assumption that places with similar temperatures have similar disease environments.[17] Often, AJR's assignment of mortality rates to countries depends on what city in a country they use. The Mexican campaign used to benchmark the Latin American rates (see the original source, Reynaud, 1898, pp. 102-22, for details) went from a city Gutierrez would classify as high mortality, Veracruz, where yellow fever struck the soldiers, to one classified as low mortality, Mexico City, making AJR's choice of Mexico as a low mortality country far from obvious. Classifying Mexico as a high mortality country to reflect the unhealthiness of Veracruz would make other Latin American mortality rates drop by 57 percent.

Perhaps the greatest weakness in this methodology is due to the wide variety of rates possible from different benchmarking systems. To begin with, the Mexican mortality rate of 71 is clearly from a campaign, implying that all of the benchmarked rates are campaign rates. Given the wide difference between this rate and the inferred Mexican bishop mortality rate of 16.7, it is a strong assumption that campaign rates will vary proportionately with bishop mortality rates. In their original paper, AJR use data on British sailors from Curtin (1964), who have mortality rates even lower than the bishops, to show that similar numbers might be achieved by using different benchmarks. However, as I show in the Appendix, other similar benchmarking systems would produce widely different estimates, typically below AJR's rates. For instance, in Guyana AJR use a soldier mortality rate of 32.18, originally from French Guiana (Curtin, 1989, p. 8); Gutierrez puts Cayenne, the capital of French Guiana, in the high mortality region, giving it a bishop mortality rate of 32.8. The close similarity of these two rates suggests that the original bishop mortality rates could be applied directly to the Latin American countries, producing rates a quarter of the magnitude used by AJR. In their own Response (p. 35), AJR propose a benchmarking system based on barracks rates in the Mediterranean, which produces a mortality rate for low mortality regions of 15.4, also close to the original bishop mortality rate of 16.7.

[17] Gutierrez states (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."

II. Sensitivity of AJR's Econometric Results

The above discussion should raise questions about any econometric results based on AJR's settler mortality data. For the sake of brevity, only AJR's original results are examined here.

Given the limitations of the data sources it is impossible to build a fully satisfactory revision of the settler mortality data. Instead, three checks, corresponding to the three types of problems discussed above, are used to test the robustness of AJR's results. The first check consists of making a small number of conservative revisions to AJR's mortality series, shown in Table 1, using only sources in AJR's original paper, to correct for evident inconsistencies discussed in Section I.A. The second check deals with comparability issues by adding a dummy variable in the regression equations to indicate if a mortality rate is taken from campaigning soldiers and a second dummy to indicate if the rate is taken from coerced African laborers. The third check drops countries with mortality rates which originate from outside their own borders, including the "benchmarked" Latin American data. Although not accounting for all the problems found in the data, sensitivity of the results to these checks should indicate whether AJR's conclusions are robust to major shortcomings in the data. The Appendix contains further detail on these checks and their coding as well as a few other checks.

Partialing a vector of control variables including a constant, AJR's econometric model can be written as the combination of a first stage equation $r_i = \beta m_i + \upsilon_i$ and a second stage equation $y_i = \alpha m_i + \varepsilon_i$, 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. IV estimates require an instrument which 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 OLS (ordinary least squares) estimates of π and β, i.e. $\hat{\alpha}_{IV} = \hat{\pi}_{OLS} / \hat{\beta}_{OLS}$. The discussion below looks first at the sensitivity of the OLS estimate of β, before going over the implications for the IV estimate of α.

Note that as mortality data are shared by some countries, any measurement error will be shared by those countries, introducing serial correlation into the residuals known as "clustering" (see Moulton, 1990). This invalidates conventional standard error formulas used by AJR, so the following tables present standard errors accounting for clustering and heteroscedasticity, now widespread in the literature (Wooldridge, 2001, pp. 152, 191).[18]

A. First Stage Estimates

Tables 2 through 4 show the first stage estimates obtained when one applies the three checks described above, using the type of controls found in AJR's original paper. The first five columns use geographic controls: latitude (measured in absolute degrees), continent dummies (Asia, Africa, and "Other," with the Americas as the reference), and omitting "Neo-Europes" (Australia, Canada, New Zealand, and the United States). These correspond to Columns (1), (2), (3), (7), and (8) in Table 4 of AJR's paper. The specification in column (6) uses climate controls from Parker (1997), similar to AJR's Table 6, column (1), except that it is more parsimonious: only one temperature variable, mean temperature, and one rain variable, minimum monthly rain, are used instead of four temperature and four humidity variables. Column (7) controls for percentage of the population of European descent in 1975, like AJR's Table 6, column (3). Column (8) controls for the percentage of the population living where *falciporum* malaria is endemic in 1994, identical to AJR's Table 7, column (1). Tables 3 and 4 also include a ninth column, omitting countries from Africa, like column (5) of AJR's Table 4 – no changes occur to this estimate in Table 2 as all of the inconsistencies corrected for are in Africa.

In light of the above data discussion, some of the control variables take on additional meaning, acting as imperfect controls for potential anomalies in the settler mortality data. As many countries on the same continent were colonized at roughly the same time, controlling for continents helps control for settlement timing's effect on measured mortality rates in the nineteenth century – although clearly imperfectly. Dropping the neo-Europes or controlling for population of European descent helps to control for the effect pre-existing settlement may have had on measured mortality rates.

The first stage results using the original data in Panel A of each table report that log mortality is a usually significant determinant of property rights, although the clustered standard

[18] AJR do not report clustered standard errors although they mention in their footnote 18 that clustering has "little effect on the standard errors." See Table 2, Panel A, for the differences.

errors are larger than the homoscedastic standard errors. Table 2 shows how the results are affected as the data inconsistencies are progressively eliminated. In Panel B, only the mortality rate for Sudan is changed, yet this has enough of an effect to lower the significance of log mortality below 10 percent in a number of specifications. The point estimates continue to fall as additional inconsistencies are eliminated, until in Panel E, all of the estimates are insignificant at 20 percent, except for the specification without controls.[19] The non-robustness of these regressions with controls signals that the mortality series free of inconsistencies is quite collinear with other variables. Despite this collinearity, the significance of the control variables improves with the revisions, implying that they may belong in the regression equation.

The results in Panel B of Table 3 demonstrate that adding dummy variables to indicate whether a mortality rate came from soldiers on campaign or from African laborers also reduces the estimated relationship between expropriation risk and log mortality, with the median significance level being 9 percent, although the campaign and laborer dummies are typically not significant. When the dummies are used with mortality data free of inconsistencies in Panel C, their significance improves remarkably, while the significance of settler mortality drops to very low levels in most of the specifications; it appears that inconsistencies in the original data obscure how the mortality rates depend on their sources.

In Panel B of Table 4, the 36 countries which had rates assigned from other countries are dropped from the sample.[20] The smaller sample size lowers the point estimates substantially, while only slightly increasing the size of the standard errors (relative to the clustered full sample – an indication of the benefits of clustering). Surprisingly, the control variables become more significant. Unless the relationship between settler mortality and expropriation risk is stronger in countries without directly measurable mortality rates, AJR's method of assigning mortality rates to neighboring countries produced first-stage estimates biased away from zero. In Panel C, the inconsistencies in the mortality data are also eliminated: when looking at the most reliable subset of consistently assembled data there is virtually no relationship between settler mortality and expropriation risk once any controls are included. When campaign and laborer dummies are

[19] First stage significance is lower when Mali-based countries are assigned a rate of 280 instead of 400.
[20] This includes rates based indirectly on bishops. If instead unadjusted bishop mortality rates are used directly, first stage significance falls more than if these countries are dropped, as shown in the Appendix.

added in Panel D, even the specification with no control variables becomes highly insignificant.

The only specification that remains significant is that for countries outside of Africa in column (9), which has no controls.[21] As Africa is an important area containing half the mortality observations, it is questionable whether this specification deserves particular attention, especially given the lack of other controls and remaining problems in the data. As seen in AJR's own paper, omitting Africa lowers IV estimates of the effect of expropriation risk on GDP, α, by about one half, close to the OLS estimate. Additional sensitivity checks in Appendix Tables A3 to A5 reveal that estimates of β without Africa are not always significant.

To make sure that results are not dependent on using expropriation risk as the measure of institutions, Appendix Tables A6 and A7 show results using alternate measures of institutions – Constraint on Executive in 1990 and Law and Order Tradition in 1995. These estimates reveal a similar lack of robustness and significance.

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 ($\beta \neq 0$) is not guaranteed, causing a *weak instrument* problem. This introduces a number of statistical pathologies in the instrumental variable estimates. First, as Nelson and Startz (1990) show, the central tendency of the IV estimator is biased away from the true value in the direction of the probability limit of the OLS estimator, and the distribution of the IV estimator is not asymptotically normal. Second, any violation of the excludability restriction ($E[m_i\varepsilon_i] = 0$) is typically made worse by a weak instrument (Wooldridge, 2001, pp. 101-2), leading to more biased IV estimates. Third, as shown by Dufour (1997), inference based on the IV estimate is complicated because conventional asymptotic confidence regions (point estimate $\pm\, t \times$ standard error), based on the Wald statistic, can be grossly incorrect. Confidence regions for α of the correct size can be built by inverting the AR statistic proposed by Anderson and Rubin (1949).[22] While using the AR test may seem

[21] Given this result, AJR's statement that (Data Appendix, p. 5) "We take comfort in the fact that our results are not affected when all African countries are dropped from the sample," seems overly strong.

[22] The Anderson-Rubin (AR) statistic for a given value α_0 is computed by regressing the residual $y_i - \alpha_0 r_i$, on the instrumental variable m_i and constructing a conventional F-statistic; it is essentially a finite-sample Lagrange Multiplier statistic. A confidence interval of size p can then be constructed by including in the

unorthodox – producing asymmetric, and sometimes disjointed and unbounded confidence regions – it provides an exact test as appropriate as a standard *t*-test in OLS, and is useful in understanding the exact implication of a weak instrument. When an instrument is strong, AR and Wald confidence regions are similar as the latter is not grossly incorrect. Fourth, 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 which cannot be estimated. Since mortality is a weak instrument in most cases, these test statistics are not reported to save space.

Table 5 presents a few representative cases of the IV estimates without controls in column (1), controlling for latitude in column (2), and continents in column (3), as the three checks are cumulatively placed on the data in Panels B, C, and D, moving down. In the first two estimates in Panel A, where the instrument is fairly strong, the AR and Wald 95 percent confidence regions are fairly similar. However, as the instrument grows weaker, the AR confidence regions get much wider than the Wald regions – column (3), Panel A, and column (1), Panel B – until they become unbounded when first stage significance falls below 5 percent in the remaining specifications. As the indirect least squares formula implies, once zero cannot be rejected for β, infinity cannot be rejected for α, an implication which the Wald confidence regions fail to capture. As the first stage relationship grows weaker, the AR confidence regions exclude smaller regions of the real line, the Wald confidence regions, sometimes erroneously, include zero, and the estimates of α become implausibly large, even accounting for measurement error. A value of α equal to two would imply some incredible conclusions: e.g. if Mexico and the United States had the same institutions (a 2.5 log difference) then the GDP per capita ratios of the two countries would go from less than 1 to 3 to over 40 to 1 in Mexico's favor. In column 2 of

interval all values of α such that the *F*-statistic has probability value less than p. Moreira (2003) proves that in the exactly identified case AR tests are uniformly most powerful amongst unbiased tests. Zivot, Startz, and Nelson (1998) show AR confidence regions take either a bounded form $[\alpha_L, \alpha_H]$, or an unbounded form $(-\infty,+\infty)$ or $(-\infty,\alpha_L]\cup[\alpha_H,+\infty)$; a region is unbounded when the confidence region for β of the same size p includes zero. Correction for clustering in the presence of weak instruments is still in development (e.g. Andrews, Moreira, and Stock, 2004), although a reasonable method, as recommended personally by Michael Jansson, is to use the standard clustering adjustment for the OLS regression used to compute the AR statistic. 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.

Panel D, the IV estimate is large and negative, as the corresponding first stage estimate is small and positive, while the reduced form estimate remains negative. Even with a fairly weak instrument some of the AR confidence regions exclude the region where most OLS estimates lie, between 0.0 and 0.6; however this result should not be taken too seriously as even a mild violation of the excludability restriction could invalidate these regions.

Overall, the volatile estimates and unbounded confidence region reveal that inference is greatly frustrated when the first stage is not highly significant. As any of the three checks alone cause significance levels to fall below 5 percent or more in most specifications, weak instrument problems seem unavoidable, unless no control variables are used – a specification with highly questionable identification.[23] When checks are used in combination, weak instrument problems worsen and even the specification without controls becomes problematic; only the specification without Africa provides a reasonably strong instrument, although it results in IV estimates much smaller than those emphasized by AJR. The reader should also be reminded that even the three checks combined do not necessarily account for all the problems found in the original settler mortality data and may understate how these problems affect AJR's results.

III. Conclusion

Given the paucity of plausible instruments in the cross-country growth literature it is regrettable to see that AJR's European settler mortality series suffers from some basic inconsistencies as well as a host of deeper issues concerning its comparability across different countries and types of sources. While AJR were right to point out that regions like West Africa and the Caribbean were unhealthy for Europeans, differences in mortality in AJR's data between countries within broad regions of the world are unreliable. Many differences appear largely due to transitory fluctuations, questionable assignment patterns, or differing circumstances of the populations observed rather than actual permanent differences in these countries. Given the limited data sources available, it seems unlikely that a convincing set of settler mortality rates by country can ever be constructed. As such, cross-country growth regressions cannot satisfactorily disentangle

[23] Particularly difficult to accept is the assumption that European settlement affected property rights institutions and nothing else which affected later output.

the effect of settler mortality from that of other variables which may explain institutions and growth, such as geography, climate, culture, and pre-existing development.

This analysis does not disprove AJR's theoretical hypotheses that potential European settler mortality rates had a strong effect on property rights institutions, or that these rights in turn had a large impact on economic performance. It only says that the statistical tools available are not powerful enough to accept or reject these hypotheses. However, it does create more room for alternative hypotheses which explain growth from other causes, including institutions other than contracts and property rights.

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TABLE 1: SETTLER MORTALITY RATE REVISIONS TO ELIMINATE INCONSISTENCIES

Country	Old Rate	New Rate	Reason
Sudan	88.2	10.9	Earliest (non-zero) available rate.
Egypt	67.8	24.7	Earliest available rate.
Madagascar	536.04	75	Earliest available rate.
Mali	2940	400	All countries had rates based on campaigns in Mali. Rate of 2940 not a true annual rate. 400 earliest available annual rate for Mali.
Niger	400	400	
Angola, Burkina Faso, Cameroon, Gabon, & Uganda	280	400	
Congo & Zaire	240	100	Maximum not average previously used.
Kenya & Tanzania	145	dropped	Average rate not available.

Additional information on these revisions found in the main text and the Appendix. All data sources are used in Acemoglu et al. (2001).

TABLE 2: FIRST STAGE ESTIMATES CUMULATIVELY ELIMINATING INCONSISTENT DATA CHOICES
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)
Panel A: Original data (N=64, J=36)								
Log mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.40	-0.42	-0.52
{homoscedastic s.e.}	{0.13}	{0.14}	{0.13}	{0.17}	{0.18}	{0.15}	{0.14}	{0.18}
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.19)	(0.22)
p-value of log mortality	0.001	0.01	0.03	0.04	0.11	0.04	0.03	0.02
p-value of controls	-	0.17	-	0.40	0.34	0.01	0.02	0.40
Panel B: Sudan given earliest available non-zero rate (N=64, J=36)								
Log mortality (β)	-0.53	-0.42	-0.31	-0.32	-0.23	-0.31	-0.33	-0.38
(heteroscedastic-clustered s.e.)	(0.19)	(0.21)	(0.18)	(0.23)	(0.23)	(0.20)	(0.20)	(0.25)
p-value of log mortality	0.01	0.05	0.10	0.17	0.34	0.13	0.12	0.14
p-value of controls	-	0.14	-	0.29	0.24	0.01	0.02	0.22
Panel C: Egypt and Madagascar given earliest available rate, cumulative from panel B (N=64, J=36)								
Log mortality (β)	-0.50	-0.38	-0.28	-0.27	-0.17	-0.27	-0.29	-0.33
(heteroscedastic-clustered s.e.)	(0.19)	(0.21)	(0.18)	(0.21)	(0.22)	(0.19)	(0.20)	(0.25)
p-value of log mortality	0.01	0.08	0.14	0.22	0.46	0.18	0.15	0.20
p-value of controls	-	0.14	-	0.17	0.16	0.003	0.01	0.14
Panel D: All Mali-based countries given the same rate, cumulative from panel C (N=64, J=34)								
Log mortality (β)	-0.49	-0.36	-0.25	-0.24	-0.11	-0.24	-0.27	-0.29
(heteroscedastic-clustered s.e.)	(0.20)	(0.24)	(0.19)	(0.22)	(0.23)	(0.20)	(0.21)	(0.27)
p-value of log mortality	0.02	0.14	0.21	0.30	0.64	0.23	0.22	0.30
p-value of controls	-	0.17	-	0.15	0.13	0.00	0.01	0.14
Panel E: Maximum laborer mortality rates replaced with averages, cumulative from panel D (N=62, J=33)								
Log mortality (β)	-0.46	-0.31	-0.20	-0.18	-0.04	-0.19	-0.22	-0.22
(heteroscedastic-clustered s.e.)	(0.20)	(0.24)	(0.19)	(0.22)	(0.23)	(0.20)	(0.21)	(0.27)
p-value of log mortality	0.03	0.21	0.30	0.43	0.85	0.34	0.30	0.43
p-value of controls	-	0.14	-	0.11	0.07	0.002	0.01	0.08

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). Revisions of the mortality data are given in Appendix Table A1. 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. N is the total number of observations and J is the number of clusters determined by the number of distinct mortality rates. *p-value of controls* are probability values from standard *F*-tests of whether the controls are significant in the regression.

"Neo-Europes" consists of Australia, Canada, New Zealand, and the United States. The three continent variables included are Africa, Asia, defined obviously, and Other, taken from AJR consists of Australia, Malta, 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). For lack of data, column (9) excludes Malta, Guyana and the Bahamas. See Table 1 for details on revisions and the text for more detail.

TABLE 3: FIRST STAGE ESTIMATES USING DUMMY VARIABLES INDICATING RATES FROM CAMPAIGNS OR LABORERS
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)	Without Africa (9)
Panel A: Original data (N=64, J=36)									
Log mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.40	-0.42	-0.52	-1.21
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.19)	(0.22)	(0.18)
p -value of log mortality	0.00	0.01	0.03	0.04	0.11	0.04	0.03	0.02	0.000
p -value of controls	-	0.17	-	0.40	0.34	0.01	0.02	0.40	-
Panel B: Campaign and laborer dummies added (N=64 J=36)									
Log mortality (β)	-0.45	-0.39	-0.31	-0.37	-0.30	-0.27	-0.27	-0.36	-1.11
(heteroscedastic-clustered s.e.)	(0.18)	(0.20)	(0.17)	(0.22)	(0.23)	(0.22)	(0.19)	(0.21)	(0.23)
p -value of log mortality	0.02	0.06	0.09	0.09	0.20	0.23	0.17	0.10	0.000
p -value of dummies	0.16	0.22	0.31	0.26	0.35	0.26	0.19	0.21	0.44
p -value of controls	-	0.27	-	0.75	0.66	0.021	0.02	0.41	-
Panel C: Campaign and laborer dummies with inconsistencies eliminated (N=62 J=33)									
Log mortality (β)	-0.31	-0.22	-0.14	-0.14	-0.04	-0.12	-0.10	-0.07	-1.11
(heteroscedastic-clustered s.e.)	(0.21)	(0.24)	(0.20)	(0.23)	(0.24)	(0.22)	(0.20)	(0.26)	(0.23)
p -value of log mortality	0.15	0.38	0.48	0.53	0.86	0.58	0.62	0.80	0.00
p -value of dummies	0.000	0.000	0.000	0.000	0.002	0.000	0.000	0.000	0.44
p -value of controls	-	0.25	-	0.33	0.28	0.006	0.007	0.07	-

See Appendix Table A1 for indicators of whether a country uses a campaign or laborer rate. *p* -value of dummies refers to an *F* -test of the joint significance of the campaign and laborer dummies. Table 2 has other details.

TABLE 4: FIRST STAGE ESTIMATES DROPPING COUNTRIES WITH RATES FROM OTHER COUNTRIES
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)	Without Africa (9)
Panel A: Original Data (N=64, J=36)									
Log mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.40	-0.42	-0.52	-1.21
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.19)	(0.22)	(0.18)
p -value of log mortality	0.001	0.01	0.03	0.04	0.11	0.04	0.03	0.02	0.000
p -value of controls	-	0.17	-	0.40	0.34	0.01	0.02	0.40	-
Panel B: Eliminating countries with rates from other countries (N=28 J=28)									
Log mortality (β)	-0.59	-0.42	-0.32	-0.31	-0.22	-0.26	-0.29	-0.38	-1.00
(heteroscedastic-clustered s.e.)	(0.19)	(0.22)	(0.19)	(0.20)	(0.23)	(0.22)	(0.21)	(0.24)	(0.28)
p -value of log mortality	0.01	0.07	0.10	0.13	0.35	0.25	0.19	0.12	0.004
p -value of controls	-	0.05	-	0.01	0.002	0.01	0.02	0.10	-
Panel C: Eliminating inconsistencies and countries with rates from other countries (N=27 J=27)									
Log mortality (β)	-0.37	-0.08	-0.04	-0.07	0.09	0.03	0.02	0.00	-1.00
(heteroscedastic-clustered s.e.)	(0.25)	(0.28)	(0.22)	(0.24)	(0.26)	(0.22)	(0.23)	(0.28)	(0.28)
p -value of log mortality	0.16	0.77	0.87	0.77	0.74	0.90	0.94	0.99	0.004
p -value of controls	-	0.03	-	0.01	0.00	0.000	0.003	0.01	-
Panel D: Eliminating inconsistencies and countries with rates from other countries, adding campaign and laborer dummies (N=27 J=27)									
Log mortality (β)	-0.16	0.07	0.06	-0.03	0.13	0.07	0.06	0.13	-0.88
(heteroscedastic-clustered s.e.)	(0.26)	(0.27)	(0.23)	(0.25)	(0.27)	(0.24)	(0.23)	(0.26)	(0.32)
p -value of log mortality	0.55	0.81	0.80	0.90	0.64	0.77	0.80	0.63	0.02
p -value of dummies	0.000	0.03	0.000	0.01	0.51	0.001	0.000	0.04	0.63
p -value of controls	-	0.03	-	0.03	0.01	0.000	0.016	0.03	-

See Appendix Table A1 for indicators of whether a country is the source of its own data. Tables 2 and 3 have other details.

TABLE 5: INSTRUMENTAL VARIABLE ESTIMATES AND CONFIDENCE REGIONS FOR REPRESENTATIVE CASES
(Second Stage Dependent Variable, Log GDP per Captia, 1995, PPP basis)

Control Variables	No Controls (1)	Latitude Control (2)	Continent Dummies (3)
Panel A: Original data			
Expropriation risk (α)	0.93	0.93	0.97
Wald 95% conf. region	[0.52, 1.34]	[0.42, 1.50]	[0.24, 1.70]
AR "95%" conf. region	[0.66, 1.83]	[0.64, 2.39]	[0.50, 9.02]
First stage *t*-statistic	-3.53	-2.70	-2.16
Panel B: Eliminating inconsistencies			
Expropriation risk (α)	1.09	1.34	1.88
Wald 95% conf. region	[0.36, 1.81]	[-0.24, 2.91]	[-2.04, 5.81]
AR "95%" conf. region	[0.69, 7.48]	$(-\infty, -2.17] \cup [0.74, +\infty)$	$(-\infty, -0.47] \cup [0.68, +\infty)$
First stage *t*-statistic	-2.26	-1.29	-0.80
Panel C: Adding campaign and laborer dummies, cumulative from Panel B			
Expropriation risk (α)	1.40	1.73	2.11
Wald 95% conf. region	[-0.14, 2.94]	[-1.41, 4.86]	[-3.57, 7.79]
AR "95%" conf. region	$(-\infty, -2.24] \cup [0.70, +\infty)$	$(-\infty, -0.63] \cup [0.70, +\infty)$	$(-\infty, -0.21] \cup [0.63, +\infty)$
First stage *t*-statistic	-1.46	-0.90	-0.63
Panel D: Eliminating countries with rates from other countries, cumulative from Panel C			
Expropriation risk (α)	2.12	-2.24	8.13
Wald 95% conf. region	[-3.82, 8.05]	[-23.3, 18.9]	[-118, 134]
AR "95%" conf. region	$(-\infty, -0.24] \cup [0.68, +\infty)$	$(-\infty, +\infty)$	$(-\infty, 0.01] \cup [0.60, +\infty)$
First stage *t*-statistic	-0.61	0.25	-0.13

AR "95%" Confidence Region refers to Anderson-Rubin (1949) confidence regions with 5 percent size. Corresponding first stage estimates using log mortality as an instrument for expropriation risk reported in columns (1), (2), and (4) of Table 2, Panels A and E; Table 3, Panel D, and Table 4, Panel E. See text and other tables for further details.

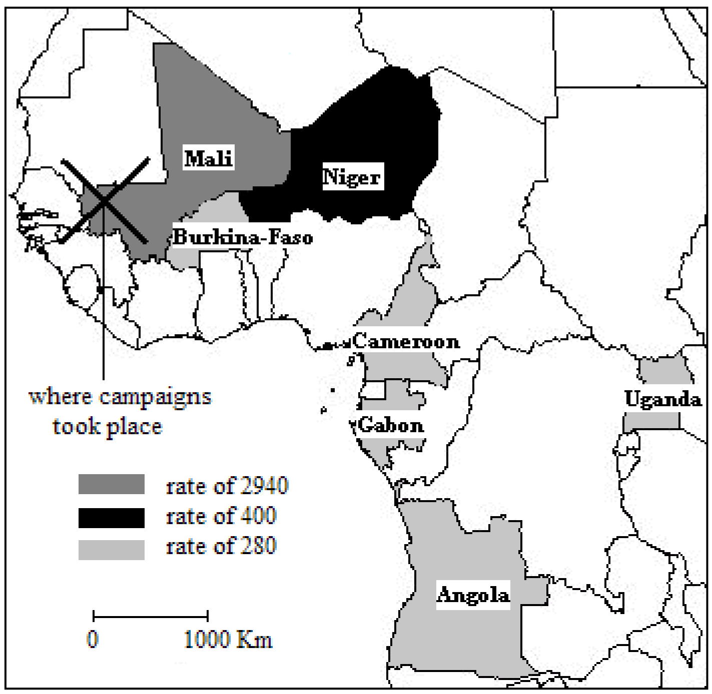

FIGURE 1: ASSIGNMENT OF MORTALITY RATES FROM MALI

APPENDIX TABLE A1: ORIGINAL MORTALITY RATES AND DATA INDICATORS

Country Name	Original Mortality	Campaign Rate	Laborer Rate	Rate From Within Country	"Benchmarked" Latin American Data
Angola	280	✓			
Argentina	68.9	✓			✓
Australia	8.55				
Burkina Faso	280	✓			
Bangladesh	71.41	✓		✓	
Bahamas	85				
Bolivia	71	✓			✓
Brazil	71	✓			✓
Canada	16.1			✓	
Chile	68.9	✓			✓
Cote d'Ivoire	668	✓			
Cameroon	280	✓			
Congo	240		✓	✓	
Colombia	71	✓			✓
Costa Rica	78.1	✓			✓
Dominican Republic	130				
Algeria	78.2	✓		✓	
Ecuador	71	✓			✓
Egypt	67.8	✓		✓	
Ethiopia	26	✓		✓	
Gabon	280	✓			
Ghana	668	✓		✓	
Guinea	483	✓			
Gambia	1470	✓		✓	
Guatemala	71	✓			✓
Guyana	32.18				
Hong Kong	14.9				
Honduras	78.1	✓			✓
Haiti	130				
Indonesia	170	✓		✓	
India	48.63			✓	
Jamaica	130			✓	
Kenya	145		✓	✓	
Sri Lanka	69.8			✓	
Morocco	78.2	✓			
Madagascar	536.04	✓		✓	
Mexico	71	✓		✓	
Mali	2940	✓		✓	
Malta	16.3			✓	
Malaysia	17.7			✓	
Niger	400	✓			
Nigeria	2004	✓		✓	
Nicaragua	163.3	✓			✓
New Zealand	8.55			✓	
Pakistan	36.99	✓			
Panama	163.3	✓			✓
Peru	71	✓			✓
Paraguay	78.1	✓			✓
Sudan	88.2	✓		✓	
Senegal	164.66			✓	
Singapore	17.7				
Sierra Leone	483	✓		✓	
El Salvador	78.1	✓			✓
Togo	668	✓			
Trinidad and Tobago	85			✓	
Tunisia	63	✓		✓	
Tanzania	145		✓		
Uganda	280	✓			
Uruguary	71	✓			✓
USA	15			✓	
Venezuela	78.1	✓			✓
Vietnam	140	✓		✓	
South Africa	15.5			✓	
Zaire	240		✓		

See text and Appendix for further details

Appendix for "The Colonial Origins of Comparative Development: An Investigation of the Settler Mortality Data"

Not for publication. To be posted on AER's website.

This appendix consists of four sections. The first explains why additional criteria mentioned in AJR's (2005) Response are not used for correcting inconsistencies found in the original data. The second section presents a detailed discussion of the mortality rates for different countries, including additional mortality rates mentioned in AJR's Response. The third section considers additional robustness checks of the relationship between mortality rates and property rights institutions. This includes using additional data from Acemoglu et al. (2005) in an extended revision of the data, building on the revisions catalogued in Table 1. The last section discusses mortality rates beyond AJR's original sample and whether rates can incorporate mortality incurred during the voyages from Europe to colonized countries.

A.I. Additional Criteria for Selecting Data Mentioned in AJR's Response

As mentioned in the main text, AJR state explicitly in their original Data Appendix that they take the first available mortality rate. In their Response AJR add that they "take the earliest *peacetime* number from Curtin when such data were available. In the absence of such a number we used the earliest *expedition* mortality" (pp. 4-5, original emphasis) This passage raises two issues: first, what is meant by "peacetime" and "expedition" mortality; second, whether this methodology is actually followed with the original data.

In Curtin (1989), the author refers to "peacetime" rates and to rates experienced during times of "warfare" or "campaigning;" in Curtin (1998) the author uses the term "barracks" rate instead of "peacetime" rate, and consistently refers to "campaign" rates, although this is clearly just a re-labeling of the previous concepts (p. 223 makes this clear). The author also uses the terms "expedition" and "campaign" interchangeably. Referring to their own idea of "expeditions" AJR state (Response, p. 5)

> most of these were small (a few hundred men) or medium-sized campaigns (a few thousand men) in which the soldiers were provided with a means of transportation and did not involve much fighting or serious casualties from enemy fire. This is quite similar

> to peacetime data since these latter data include episodes in which soldiers march, for example to change stations. We continue to believe this coding is sensible…

AJR appear to imply that their expeditions refer to rather gentle campaigns, although this depends on their own judgment as Curtin does not distinguish between different types of campaign rates. Furthermore, a number of expeditions were quite large and difficult. The rates AJR use for Algeria and Mexico come from campaigns involving tens of thousands of men in which the Europeans sometimes lost battles. In these two campaigns and others (e.g. Indonesia) it is not clear from Curtin's writing that soldiers were provided with any means of transportation. Moreover AJR's argument about fighting and marching misses the point that mortality rose during campaigns because of contaminated water supplies, poor sanitation, exposure to new environments, and because soldiers took fewer precautions against disease.

Moreover, this rule, stated by AJR in their Response, does not seem consistent with their practice. Documented in the list below are countries for which AJR used a campaign rate when a barracks (peacetime) rate was available from their sources.

Country	Source of Barracks Rate
Algeria	Curtin (1989, p. 199-200)
Bangladesh	Curtin (1989, p. 82)
Cameroon	Curtin (1989, p. 10)
Egypt	Curtin (1998, p. 9)
Ghana	Curtin (1989, p. 9)
Haiti	Curtin (1989, p. 9)
Indonesia	Curtin (1989, p. 82)
Madagascar	Curtin (1998, p. 181)
Morocco	Curtin (1989, p. 9)
Pakistan	Curtin (1989, p. 82)
Tunisia	Curtin (1989, p. 9)
Vietnam	Curtin (1998, p. 10)

Furthermore, in their original Data Appendix (p. 2), AJR state "The very high mortality from disease in Madagascar in 1895 is a reasonable estimate of what settlers would have expected because this was not a particularly well-organized campaign," seeming to prefer the later campaign rate for Madagascar although an earlier barracks rate is available.

In general, using the earliest peacetime rate instead of an earlier barracks rate for all countries would not necessarily improve the data. In the synopsis of his (1998) book, Curtin writes

> from about 1815 to 1914, the death rates of European soldiers, both those serving at home and abroad, dropped by nearly 90 percent. But this drop applied mainly to soldiers in barracks. Soldiers on campaign, especially in the tropics, continued to die from disease at rates as high as ever, in sharp contrast to the drop in barracks death rates.

Many of the earliest barracks rates available are from the late nineteenth century or the early twentieth century. Relative to the barracks rates from the early to mid nineteenth century, these rates would be too low. On the other hand, taking earlier campaign rates produces rates which are too high relative to contemporaneous barracks rates. Neither alternative is attractive. As barracks rates from the early nineteenth century are not available for most countries, a comparable mortality series across many countries cannot be constructed using barracks rates for this period.

In my revisions the principle of taking the earliest peacetime rate is disregarded as it would imply many more inconsistencies in AJR's original data and lead to many more, possibly disputable, changes to the original data without necessarily improving them.[i]

A.II. Region-by-Region Summary of Problems and Revisions

Below I discuss some remaining problems in the original data, how revisions are made, and why rates are classified as campaign rates or barracks rates. Mortality rates brought up in AJR's (2005) Response are also discussed, partly as they are used in an additional data check shown in the Section A.III.

Sub-Saharan Africa: The mortality rate taken from the Gold Coast (Ghana, also applied to Togo

[i] When multiple estimates are available within a country, AJR show a general preference for taking the lowest mortality rate. This seems implied by a statement in their Response (p. 33), "Our principle of using the lowest available rate for a country applies to a reasonably big area (e.g. a region that could accommodate a significant number of settlers)." AJR did not apply this rule to India, however, where they choose to give it the middle rate from Madras (see below). When the earliest versus lowest principles conflict, AJR reveal a preference for the earliest available rate in their choice of mortality rate for Vietnam, where they choose the earliest rate of 140 for Cochin China from 1861 over the lower rate of 60 for Tonkin from 1864 (Curtin, 1998, p. 239).

and Cote d'Ivoire) of 668 comes from 1823 to 1826. According to Curtin (1998, p. 181), "these troops were not on campaign during the whole four years, but their death rate from disease can be taken to be a campaign, rather than a barracks, death rate." The yearly data (Army Medical Department, 1840, p.7) show an increase in mortality from 1823 to 1824 when the Gold Coast war began. The Gold Coast War coincided with a yellow fever epidemic, which may have boosted these rates beyond the level normal for a campaign. In their Data Appendix, AJR consider using the Sierra Leone rate for Ghana, Togo, and Cote d'Ivoire, although this rate was highly impacted by the epidemic as well (see below).

The Gold Coast War also led to substantial troop buildups in the Sierra Leone Company, which included the Gambia. Referring to the mortality rate of 483 for Sierra Leone, Curtin (1998, p. 10) states, "The European mortality represented in the chart was not the normal West African experience, but only typical of what could happen during a yellow fever epidemic." Because the mortality rate is averaged using troop strengths as weights, and as there was a huge military build-up during the epidemic period – the number of troops rose from 6 in 1824 to 571 in 1825, dropping to 9 by 1830 – the mortality rate for the entire period of 1817 to 1836 strongly reflects the epidemic period (Feinberg, 1974). According to Curtin (1989, p. 18),

> The Sierra Leone rate of more than 400 per thousand was somewhat higher than usual, but peacetime rates of 100 to 200 per thousand had been common enough in the past and were to persist for several decades to come.

Given this passage and evidence that living conditions up until 1826 at the Sierra Leone camp were fairly abysmal, with "diseases of the stomach and bowels" taking away a fair number, this rate seems best classified as a campaign rate, although the troops may have spent a significant amount of time in over-crowded huts which served as barracks. [ii]

The deaths used to construct the mortality rate from Gambia actually make up a particularly hard hit sub-population of the Sierra Leone Command from 1825 to 1826, precisely

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[ii] The Army Medical Department (1840, pp. 6-9) describes these conditions, including the brackish water soldiers drank, "liable to cause slight affections in the bowels in persons unaccustomed to it." While soldiers had rations of one pound of meat and one pound of bread per day, the meat given to these soldiers was of poor quality, with little else being added to it unlike in other stations. The author states that some portion of the sickness and mortality was due to the privation of diet. All of the troops were also "commuted punishment men" of "the most degraded class of soldiers" and may have had higher mortality rates than typical soldiers.

during the yellow fever epidemic. AJR use this period to infer a mortality rate of 1470, which they state (Data Appendix, p. 3), “is high but not extraordinary for that time and place.” Since Gambia was part of the Sierra Leone Command, the deaths from Gambia are taken out of the Sierra Leone rate in the extended revision below to avoid double-counting by using the rate suggested by Tulloch of 350 (Army Medical Department, 1840, p. 7).[iii]

For an expedition of 159 Europeans in 1841 up the Niger which AJR use for Nigeria, Curtin (1998, p. 21) explains

> The longest time any of the steamers spent in the river that year was just over two months, but even for this brief period the death rate was 350 per thousand. Eighty-two percent of the crews came down with malaria, and the case-fatality rate was 30 percent. The standard statistical measure of annual deaths per thousand at risk is obviously impossible to determine… The death rate per month gives a measure more nearly comparable with similar expeditions, and the overall monthly death rate in this case, based on the number of days each man was at risk on a steamer in the Niger, comes to 162 per thousand, *per month*.

Despite the comparability issues which Curtin raises, AJR take the monthly rate of 162 and multiply it by 12 to infer an annual rate of 2004 for Nigeria. AJR appear to have misread this passage as they write (Appendix, p. 3) “In a period of several months, 82 percent of the Europeans died from malaria,” when in fact only 30 percent of this 82 percent died. Case-fatality rates from malaria are high, but far from 100 percent.

For Madagascar, AJR did not use the first available rate mentioned in the following passage (Curtin, 1998, p. 181) “In 1880, the peacetime garrison Nossi-Bé had an annual death rate of about 75 per thousand.” Instead, AJR used a rate from 1895 during the Madagascar Expedition, which Curtin (pp. 184-188) also refers to as a “campaign” as well as an “invasion.” This rate of 75 is used to eliminate the inconsistency in the data check shown in Table 2, Panel C. In Tables 3 and 4 where the inconsistency-free revision is used in conjunction with campaign and settler dummies, Madagascar is recoded as a barracks rate.

[iii] If the rates for Gambia and Sierra Leone are classified as barracks rates then the first stage estimates of β using the campaign dummies in Table 3 become slightly more significant without controls and slightly less significant in some other specifications.

How AJR assign three different mortality rates from one area in western Mali to seven different countries requires a lengthy discussion.

- Two tables in Curtin (1998) report mortality rates that belong to Mali, one titled "Haut-Senégal-Niger" on page 85, the other with rates marked "French Soudan" on page 238.
- The text and map in the chapter containing the "Haut-Senégal-Niger" table (pp. 74-89) make it clear that these rates refer to campaigns in western Mali.
- The table with the "French Soudan" lists yearly rates from 1884 to 1888 while the table with "Haut-Senégal-Niger" lists yearly rates from 1880 to 1892. The yearly rates reported for the five overlapping years are almost identical, differing sometimes by a tiny amount (i.e. 280, 225, 200, 221, and 116 versus 282, 225, 201, 222, and 117). From this similarity alone it is possible to infer that these rates refer to the same campaigns; reading the original source of the "French Soudan" rates (Reynaud, 1898) confirms this.
- Officially, "French Soudan" is the name for Mali from 1890 to 1899 and from 1920 to 1969. "Haut-Senégal-Niger" is the name for Mali and Burkina Faso from 1904 to 1920 and also for Niger from 1904 to 1911. It seems that AJR were unaware of these facts as they excluded Mali from both regions.
- Although "Haut-Senegal" and "Niger" are not synonyms, AJR state (Data Appendix, p. 3) "In Haut-Senegal (Niger) [sic], in 1880-83 there was a death rate of 400 per 1000 mean strength (Curtin 1998, p. 85)," to justify assigning the earliest mortality rate of 400 (1880-83) from the "Haut-Senégal-Niger" table to Niger only.
- The authors claim (Data Appendix, p.3), incorrectly, that "Burkina Fasu [sic], Central African Federation [sic], Chad, French Congo and Mauritania were part of French Soudan." and assign to these countries the first rate in the "French Soudan" table of 280 from 1884.[iv] This rate is then assigned to Angola, Cameroon, Gabon, and Uganda under the unexplained premise that their disease environments are similar.
- The rate of 2940 used for Mali comes from the two-month Logo expedition from 1874 (Curtin, 1998, p. 81), discussed in the main text.

[iv] The confusion over the "French Soudan" may be due partly to how the general term "Soudan" in French – as seen in older editions of *Le Petit Larousse* – refers to a large swath of land south of the Sahara from Mali (French Soudan), to Darfour in modern (Anglo-Egyptian) Sudan. However this area should have still included Mali, and excluded French Congo and most of the Central African Republic. The latter two countries were part of French Equatorial Africa, while Mali, Burkina Faso, and Niger were part of French Occidental Africa. It is also not clear why AJR assign the rate for laborers in Curtin et al. (1995) to Congo when they claim it was part of French Soudan, while Gabon received the rate of the French Soudan presumably because it was a neighbor of Congo.

The net result is that AJR assign three different rates, all from the same area in Mali, to three different sets of countries: a rate of 400 from 1880-3 to Niger, a rate of 280 from 1884 to the five dispersed countries of Burkina Faso, Cameroon, Gabon, Angola and Uganda, and an epidemic-based rate of 2940 from two months in 1874 to Mali. In the revision mentioned in Table 1, the first true annual rate of 400 is applied to all of these countries in order to achieve some consistency. One could also apply the rate of 2940, but this rate, the highest of all rates in AJR's data, is not appropriate for reasons cited in the main text. Assigning the rate of 400 here is more favorable to first stage significance than assigning the lower rate of 280, which would involve only changing the mortality assignment of two countries, Mali and Niger.

The laborer rate for Congo, also used for Zaire, comes from Curtin et al. (1995, p. 463)

> …workers had to be recruited elsewhere; and they were recruited by force from all parts of French Equatorial Africa… Many of these men came from the savanna country in Chad and Ubangi-Shari. They were therefore unprepared for the diseases they encountered in the forest and Mayombe highlands… They 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 annum at the peak of mortality…

The laborer rate for Kenya, also used for Tanzania, comes from this passage (p. 491)

> some of East Africa's highland peoples who lived in malaria-free areas knew that if they stayed overnight in lowland regions where mosquitos bred, they would be likely to suffer from malaria… The Europeans had the armed force to require that African men move to the lowlands… The result, in Kenya, was that workers from the highlands died at annual rates as high as one hundred and forty-five per thousand in Mombasa and nearby coastal regions on the eve of the First World War.

This passage implies that there were malaria-free highlands that may have been relatively healthy for Europeans, suggesting that the 145 rate may also be overstated as it refers to a relatively unhealthy area in Kenya. Given that no average is reported, Kenya and Tanzania are dropped to eliminate the inconsistency in the first data check.

AJR (Data Appendix, p. 3) are not specific about which examples in Curtin (1968) they refer to when justifying the use of African coerced labor as a lower bound for the mortality of Europeans. All the examples I find refer to troops, not laborers, with the precise origin of the troops probably different than the origins of the laborers. Furthermore only average mortality rates are reported. The mortality of the Congolese laborers may have been abnormally high as they were "underfed and ill-housed," (Curtin , 1995, p. 463). In the Ethiopian campaign of 1867-68 reported in Curtin (1998, pp. 45-6), badly treated laborers died at a much higher rate than the European soldiers they accompanied. Whatever the case, these examples referred to average and not maximum rates, and do not justify the practice of taking maximum rates.

North and North-East Africa: The high mortality observed in countries in this region appears to be the result primarily of waterborne diseases, such as typhoid, which stem from campaign conditions. Curtin names chapters in his (1998) book "Typhoid and the Egyptian Garrison" and "The Typhoid Campaigns: Northeastern Africa in the 1880s."

The Tunisian rate is from a table in Curtin (1998, p.152) whose title includes the "Tunisian Campaign of 1881." The mortality rate for Ethiopia is taken from a table in Curtin (1998, p. 44) labeled, "British Military Mortality on the Magdala Campaign, 1867-68."

The rate AJR used for Algeria from 1831-38 is clearly a campaign rate as the quote in footnote 10 of the main text makes obvious. Curtin also calls it "a major military operation" (1989, p. 28). In their Response, AJR cite a rate mentioned in passing in Tulloch (1847) that Algerian settlers experienced a mortality of 70; this is not altogether surprising given that this was during the initial settlement of Algeria and during a war – the conditions facing the settlers were far from ideal. Later experience would show Algeria to be quite healthy once sanitary conditions were established; the hundreds of thousands of European settlers who came to Algeria certainly attest to this (see Curtin et al., 1995, pp. 312-5, 434-5).

AJR state that they use peacetime numbers for Egypt and Sudan, although these are quite abnormal rates. The rate they use for Egypt of 67.8 (Curtin, 1998, p. 158) occurred during the first three months (October 9 to December 31, 1882) following a campaign and was unusually high because of dirty water and filthy Egyptian barracks the soldiers had to occupy, littered with human waste – clearly circumstances due to campaigning. This is probably why Curtin called it a "post-campaign" rate rather than a "barracks" rate. Within a few months mortality rates fell to 32.64 (Curtin, 1998, pp. 158-161), which seems like a truer barracks rate. Because it is reported first, the initial campaign rate of 24.7 (a rare case where the campaign rate was lower than the barracks rate) is used in the revision in Table 1. Besides being more consistent, this number is closer to the truer peacetime rate than the one used by AJR.

In their Response (2005, p. 22), AJR state that the first available mortality rate of zero for the Sudan is one which "Curtin clearly suggests is mistaken." This seems like a strong interpretation of Curtin's (1998, p. 173) text, "Medically, this brief period… appears as one of unaccountable success, unless there were errors in the reporting. The force had only 127 hospital admissions and no deaths at all from disease…" Moreover, even if AJR's claim is correct, it does not explain why they did not use the second rate of 10.9.

AJR might argue that they were looking for the first peacetime rate and that this is 88.2. However Curtin (pp. 173-4) says nothing about whether the campaign had finished by the late 1885 period, when mortality rates rose for the smaller remaining group. Typhoid caused half the deaths, implying that campaign-like conditions accounted for these deaths, making it an inappropriate "peacetime" rate. Given that the first rate was zero, using the rate of 10.9 for the Sudan does not seem to be an unreasonable choice for Sudan, especially as malaria and yellow fever were almost non-existent in this area (p. 169).

Asia and Oceania: Curtin (1998, p. 8) reports mortality rates for three presidencies in India: 36.99 for Bombay, 71.41 for Bengal, and 48.63 for Madras. AJR assign these rates to Pakistan, Bangladesh and India respectively. However all three presidencies overlapped with India; for reasons not explained, AJR choose to use the Madras rate for India.

Discussing the mortality rates he reports for India, Curtin (1989, p. 25) writes

> it reflects the high mortality from wartime years…. Campaigning was frequent especially in the two northern presidencies [Bengal and Bombay]. The comparative peace for Madras accounts for its lower mortality rates. After 1852, death rates fell sharply in all three presidencies, again the probable result of relative peace in these years.

Although it is not absolutely obvious what to do here, given the passage above, it makes sense to deem Bengal and Bombay campaign rates and Madras a barracks rate.

The footnote in the main text (n. 11) makes it clear that the rate for Indonesia is from a campaign. The mortality rate for Vietnam comes from a campaign which the original source, Reynaud (1898, pp. 92-101, 471), describes as being deadly due to the deplorable, dirty conditions the soldiers endured and multiple cholera epidemics, likely the result of contaminated drinking water.

The mortality rate of 14.9 AJR use for Hong Kong belongs to the British China Field Force who fought in 1860 during Arrow's War. As the rate was not annualized, and since most of the campaign was fought in Beijing where mortality was much lower than in Hong Kong, this rate cannot be considered a campaign rate for Hong Kong.

The rate of 17.7 used by AJR for Malaysia and Singapore is from a small sample in Penang, Malaysia. This mortality rate is repeated in Statistical Society of London (1841), which also gives a combined mortality rate of 20.0 for a much larger group of soldiers encompassing Penang, Malacca, and Singapore (p. 146). However, it should be kept in mind that the Portuguese and Dutch had been in this area for over 300 years.

As mentioned by AJR (2005) in their Response, Balfour (1845, p. 195) reports a mortality rate of 14.1, which is considerably different in logs from New Zealand's rate of 8.55.

The Caribbean and Latin America: AJR's mortality rates of 130 for Jamaica and 85 for the Windward Leeward Command (including Barbados, Guiana, and Trinidad and Tobago) from 1817 to 1836 appear to be barracks rates, although soldiers were suffering from the residual effects of war (Curtin 1989, pp. 25-8). AJR's application of Jamaica's rate to Haiti and the Dominican Republic and the Windward Leeward Command's rate to Trinidad and Tobago seems plausible as they are fairly close together and have similar geography. It is worth noting that Jamaica, Haiti, and the Dominican Republic have substantial highlands; in Jamaica these highlands (including Maroon Town, Phoenix Park, Montpellier, Mandeville) have much lower mortality rates in the neighborhood of 30, according to disaggregated data from Tulloch (1838b) and Balfour (1845). Overall mortality in Jamaica dropped in the 1840s as troops moved to new barracks in these highlands (Curtin 1989, pp. 27-8). In their Response, AJR (p. 33) rule out using these data as they judge the area to be too small – perhaps because they only noticed Maroon Town. However, Jamaica, Haiti, and the Dominican Republic have vast highlands over areas much larger than the smallest countries in the sample (Hong Kong, Singapore, and Malta).

The application of the Windward Leeward Command's rate of 85 to Bahamas does seem questionable given the Bahamas' much closer proximity to Jamaica than to Barbados, although the Bahamas does lie lower than Jamaica, which presumably makes it more unhealthy. Nowhere in Gutierrez (1986) do I see corroboration for AJR's claim (Data Appendix, p. 4) that “information from Gutierrez 1986 indicates that these were similar disease environments.” As AJR reported in their Response (p. 33), a mortality rate measured in the Bahamas is 189 (Tulloch, 1838b, p. 229) – double the figure AJR originally used.

AJR’s (2005) Response also mentions that Tulloch (1838a, p. 32) reports disaggregated mortality rates for Trinidad and Tobago (106.3) and Guiana (84). Trinidad has a lower rate (106.3) than Tobago (152.8) and so the former’s rate is used in the extended revision. Guyana’s previous rate 32.18 was based on French Guyana. Tulloch (1838b, p. 231) reports that a small contingent in Honduras suffered a mortality rate of 95.2; a case where the benchmarked rate of 78.1 was somewhat accurate.

Using the bishop data listed in the main text from Gutierrez (1986) one can test the hypothesis that regions share the same mortality rate. The *p*-values of the *F*-tests are as follows: low same as medium, 94 percent; low same as high, 17 percent; medium same as high, 17 percent; all three regions the same, 12 percent. The low and medium temperature regions are indistinguishable, and none of these tests can strongly reject the claim all of the relative differences are due to sampling error. The overall bishop mortality rate of 22.9 (standard error 4.4) is not significantly different from the death rate of Swedes, ages 40 to 49, of 18.32 from 1751 to 1759, found in the Human Mortality Database (Sundbärg, 1905).

Gutierrez defines low, medium, and high temperature regions as areas with mean temperatures of less than 20°C, 20°C to 24.9°C, and 25°C or greater, respectively, and assigns cities to the regions according to Showers (1979). In the text of his article, Gutierrez lists a number of cities in low and high temperature regions, although not in medium temperature regions. These cities are shown in Figure A1, along with AJR’s assignment of mortality rates to different countries based on the bishops. Using additional cities listed in Showers (1979) the following countries have cities in multiple regions:

- Low and medium: Bolivia, Ecuador
- Medium and high: El Salvador, Honduras
- Low, medium, and high: Brazil, Colombia, Mexico, Venezuela

Basing the classification on capital cities would produce a similar classification to AJR’s, except for Brazil which would change from low to medium. More discrepancies would occur if one were to redo their classification based on the mean temperature variable from Parker (1997) that AJR use as a control variable in their regressions: Brazil would change from low to high, El Salvador from medium to low; and Peru and Uruguay from low to medium.

The one city listed in the Bahamas, Nassau, has a mean temperature of 24.8°C, putting it in the medium mortality region – this is inconsistent with AJR's assertion above about "similar disease environments" with the Windward Leeward Command. Despite being in a medium mortality region, the Bahamas has a higher observed mortality (189) than Caribbean countries in the high mortality regions (85 and 130), casting doubt on AJR's benchmarking system.

AJR claim their results would be "virtually the same" (Data Appendix, p. 4) if they used the rate of 130 from Jamaica as their benchmark for the high mortality region, although this would lower mortality rates of Latin American countries by 20 percent. Lowering the mortality rates for Latin American lowers the first stage estimate while increasing the second stage IV estimate; this makes sense using the indirect least squares formula ($\alpha = \pi/\beta$) if the reduced form coefficient π changes by less than the first stage β. Alternatively, benchmarking with the rate of 85 from the Windward Leeward Command (Curtin, 1989, Table 1.5), or the rate of 84 from Guiana for the high mortality benchmark lowers Latin American mortality by almost 50 percent. On the other hand, using the Bahamas rate of 189 as a medium mortality benchmark results in mortality rates 141 percent higher.

As another check for the accuracy of their Latin American numbers, AJR benchmark their rates using naval station data for 1825-45 from Curtin (1964. p. 486) and the mortality rate of 483 from Sierra Leone. From this data they take the ratio of the mortality rate of "South American Stations" (7.7) to the mortality rate from the anti-slavery blockade off of the African coast (54.4), which may not apply specifically to Sierra Leone, to get 0.142. Multiplying this ratio by Sierra Leone's rate of 483, AJR impute a solider mortality rate of 68.9 for South America, which they apply directly to Argentina and Chile in their data instead of 71, without mentioning their justification. As 68.9 is close to the imputation of 71 for the low mortality region (including Argentina and Chile) using bishop data, AJR claim their benchmarking is robust.

AJR cross-validation is rather particular: using the same methodology with the other naval station data in Curtin (1968), we could calculate a number of other possible benchmarked rates for the low mortality region. Seven alternative examples are shown in the chart below. Another indication that the benchmarking system is flawed is seen by comparing the naval station rates to the soldier rates in the benchmarked countries. If benchmarking is appropriate, then the ratios between naval station rates should be similar to ratios between corresponding soldier rates (e.g. comparing Malta and Jamaica 16.3/130 should be close to 9.3/18.1). As these ratios are often widely different, often not even obeying the same ordinal rankings, benchmarking can produce widely varying and inconsistent imputed mortality rates and should not be trusted.

Naval Station (rate1)	Latin American Station Ratio (7.7/rate1)	Benchmarked Country (rate2)	Implied Rate for Low Mortality Region (7.7 x rate2/rate1)
African Station (54.4)	0.142	Sierra Leone (483)	68.9 (used by AJR)
African Station (54.4)	0.142	Gold Coast (668)	94.9
Mediterranean Station (9.3)	0.828	Gibraltar (21.4)	17.7
Mediterranean Station (9.3)	0.828	Malta (16.3)	13.5
Home Station (9.8)	0.786	England (15.3)	12.0
East Indian Station (15.1)	0.510	India (48.63)	24.8
West Indian Station (18.1)	0.425	Trinidad & Tobago (85)	36.2
West Indian Station (18.1)	0.425	Jamaica (130)	55.3

Looking at the actual benchmark rate used from Mexico, two mortality rates are listed for Mexico on facing tables (Curtin, 1998, pp. 238-9), a rate of 53 from Reynaud (1898) and a rate of 71 from an anonymous article in the *British Medical Journal* (1998) of which AJR take the latter. Although there is little explanation of these rates or why they differ, it is clear from the table is that they are from "campaigns." Neither rate is annualized, and in the original source, Reynaud (1898, 113-121 and pp. 471-2), I discovered the rate of 53 applies to the army, while the rate of 71 applies to the army, navy, and marines combined.[v] The mortality of the navy and marines (82) was higher as they were staying on the unhealthy coast, while the army eventually made it into the highlands. Using the mortality of the navy and marines to benchmark the high mortality region results in mortality rates 50 percent lower for Latin America; using the army rate to benchmark the low mortality region results in rates 25 percent lower.[vi]

On the whole, these cross-validations suggest that AJR's use of bishop mortality rates and other sources to impute soldier mortality rates is very sensitive to choices. AJR's own choices result in imputed Latin American mortality rates which seem to be above average among possible imputations, most comparable to campaign rates, and favorable towards finding a relationship between settler mortality and expropriation risk.

North America: For the United States the mortality rates of the first settlers did not reflect the

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[v] Using the text I was able to estimate the number of troop-years (34,319) that French troops were at risk and the number of deaths in this population (2,095) to calculate an overall annual mortality rate of 61.

[vi] American troops during the Mexican War apparently experienced a mortality rate of 100 (Adams, 1952, p. 194). Scott's 1847 campaign during this war took a similar route from Veracruz to Mexico City, although much campaigning occurred in what is now the United States and just south of the Mexican border.

mortality rates of soldiers in the nineteenth century. The first British colony, Roanoke, established in 1587 with over 100 settlers, disappeared within three years for reasons still unknown (Bolton and Marshall, 1971, p. 110). The second colony, Jamestown, had the difficult experience described in the main text. The third colony, Plymouth, established in 1620, had approximately one half of its 100 colonists die between November and February, mainly of scurvy (p. 138).

The first Canadian colonists also died at high rates when first settling. During the first winter spent by the French, from 1535 to 1536 in what is now Quebec City, 25 out of 110 men died, producing the rate of 227, although the number would have likely been higher had they not discovered how to cure scurvy from the Indians (Trudel, 1973, p. 27). The next winter that mortality was recorded, from 1542 to 1543, 50 out of 200 died from scurvy (p. 47), leading the French to abandon their colony. The French returned in 1604 to Sainte Croix Island where 36 out of 79 died (p. 151) the first winter. The next winter spent at Quebec from 1609 to 1610 saw 13 die of scurvy and 7 die of dysentery out of 28 inhabitants. However within a few more years, the settlers learned how to overcome scurvy and build shelter adapted for the harsh winters and saw their mortality rates decline.

During the colonial period epidemics hit the North American colonies fairly frequently. For example, a yellow fever epidemic struck New York City in 1702, killing 10 percent of the population in three months (Duffy, 1953, p. 146). See Duffy (1953), Wells (1975) and Gemery (2000) for more on the mortality of settlers in the North American colonies.

During the Civil War, white soldiers in the Union army had mortality rates from disease of 53.4 while black soldiers had mortality rates of 143.4 (Adams, 1952. p. 239). "Continued fever" (typhoid) and diarrhea-dysentery accounted for about half of these deaths, with malaria playing a minor role, as the soldiers faced campaign-type conditions. Although the Civil War was a major war, it is not clear that the actual campaigns suffered from worse conditions than soldiers on campaign in colonial countries, especially as American soldiers likely had better access to medical services, hospitals, and fresh food (see Adams, 1952). On the whole these rates are strikingly close to the mortality rate of French soldiers campaigning in Mexico in overlapping years, 1862 to 1863, mentioned above. [vii]

[vii] In the Revolutionary War British soldiers had a mortality rate in the neighborhood of 26 (Cantlie, 1974, p. 156), although the British soldiers spent much of their time in barracks in New York, Boston, and Halifax. Hessian mercenaries died at a rate of 34. American soldiers probably died at a rate of approximately 52 (very close to the Civil War rate), as they were apparently twice as sickly as the British, with sickness rates of 200 as opposed to 100 for the British, probably due to differences in campaign conditions.

Finally, it is worth noting that Mitchener and McLean (2003) collected mortality rates for all of the different states from 1829 to 1838. The rates directly measured (some are inferred) that mortality rates was not only higher in the South, but also in what were then less settled frontier states such as California (31) and Missouri (41).

A.III. Additional Checks

Appendix Table A3 reports first stage estimates for checks using revised data. As before Panel A contains estimates using the original data, while Panel B reports estimates with the inconsistencies reported in Table 1 eliminated. In Panel C mortality rates reported in sources from AJR's (2005) Response (pp. 107-8) are used to create geographically more precise data where previous rates were lacking. These revisions, shown in Appendix Table A2, build on the original mortality series with the inconsistencies from Table 1 removed. Although the data are still subject to many problems, these revisions should produce a better data set as they replace previously inferred mortality rates with actual rates. As shown, this higher quality data produces estimates closer to zero and more insignificant than the original data, or the revision eliminating only inconsistencies. This result using improved data found by AJR strengthens the conclusion that AJR's original results are not robust to shortcomings of their data.

Panel D shows how the results change by replacing in the original data only the rates for the United States and Canada with actual initial settler mortality rates from Jamestown (283) and Quebec (227). This is meant to be more of an illustration of how settlement timing issues are important rather than as a serious revision of the data. The first stage results are surprisingly sensitive to just these two changes, with log mortality becoming insignificant at 15 percent in most of the specifications, including column (9) without Africa. This suggests that results based on AJR's original data may be highly sensitive to the dependence of mortality rates on previous colonization and settlement.

Appendix Table A4 investigates the robustness of results to the treatment of bishop mortality using the original data. Panels B and C examine and compare how the first stage results are affected if countries in Latin America are simply dropped (as in Table 4) or assigned un-benchmarked bishop mortality rates directly. Given that AJR's benchmarking system is unreliable and produced rates in the upper range of possible benchmarking systems, it is worth considering how results would be affected if the mortality rates of bishops were simply applied directly without benchmarking (which produces rates close to those using lower benchmarks). When the Latin American data are simply dropped in Panel B, the results are only mildly affected. If instead the mortality rates are simply lowered to the actual bishop mortality rates, as in Panel C, the significance of the first stage is more highly impacted. Even the specification without Africa and no controls remains on the cusp of significance at 5 percent. Dropping the Latin American data completely in Table 4 in the main text proves more favorable to AJR's hypothesis than using the original bishop mortality rates. Furthermore, benchmarking the settler mortality data to the Mexican campaign rate is much more favorable to AJR's hypothesis than using the bishop data directly.

An alternative strategy of dealing with bishops is to use a dummy variable to indicate whether a mortality rate is derived from benchmarking, as shown in Panel D. Alone this dummy does not have much of an effect on the results. However, when used in conjunction with campaign rate and laborer dummies, used in Table 3, mortality becomes mildly insignificant in most specifications, much like the results in Panel B of Table 3.

The results shown in Appendix Table A5 use only mortality rates based on soldiers, separating campaign rates from barracks rates. Panels B and C, using the original data, show that the first stage relationship is much weaker among countries with campaign rates than among countries with barracks rates. Arguably this may be due to stronger (uncorrelated) measurement error in the campaign rates, although the IV estimates (not shown) using only the barracks rates are considerably lower than AJR's IV estimates ($\alpha < 0.7$). It may also be that other sources of bias play a bigger role with the barracks rates than with the campaign rates.

Panels D and E use mortality rates from the extended revision, also dropping countries with rates assigned from other countries. This provides arguably the most reliable sub-sample of mortality rates available. The results with the campaign rates reverse sign, casting doubt on the measurement error hypothesis, as better rates should make the relationship more negative and as attenuation would now mask a greater positive coefficient. Using the barracks rates produces often large but volatile estimates of β, revealing a particular sensitivity to the exclusion of the "neo-Europes" and European settlement. This suggests that the relationship between better security of property and lower mortality rates may be due partly to pre-existing European settlement.

Appendix Tables A6 and A7 check the robustness of the first stage using alternate measures of institutions – Constraint on Executive in 1990 and Law and Order Tradition in 1995

– while cumulatively applying the three checks in the main text. While these results show that in some cases the relationship between these variables and mortality rates is slightly more robust than for expropriation risk, the relationships do not stand up significantly in the lower panels when two or three checks are used together.

A.IV. Some Further Considerations

Travel Mortality

Another consideration AJR do not address is the mortality cost of traveling from Europe to potential colonies. 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 on ships, 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 nineteenth century, diseases on ships regularly killed 8 percent of passengers on the way to Southeast Asia and Oceania (Shlomowitz, 1989), and 10 percent of those on the way to South Africa (Riley, 1981). However, there is no clear model for how these "traveling" mortality rates should be combined with mortality rates within countries.[viii]

Settler Mortality for Countries Outside the Original 64

Larger data sets of mortality rates seen in Acemoglu et al. (2003), Easterly and Levine (2003), and Rodrik et al. (2004) include some additional countries. The rate of 93.7 for Afghanistan, originally from Reynaud (1898, pp. 58-61), would be a valid campaign rate if it were properly annualized. Curtin (1898, p. 172) reports an annual rate of 74.6 from the same campaign. The mortality rate used for Fiji comes from peaceful soldiers in New Caledonia in 1848 (Curtin, 1989, p.7), thus the appropriateness of this geographic match is uncertain. The rate of 34.6 used for Myanmar for 1829-38 (Curtin, 1989, p. 8), corresponds to a barracks rate, with the appropriate campaign rate being 119 for 1924-6, in Curtin (1989, p. 23), as mentioned earlier. Mauritius's

[viii] With some rough but sensible rates I came up with, interpolating monthly mortality rates and times, I tried fitting the first stage regression with the regressor $log(mort+\varphi travelmort)$, where AJR's original model imposes the restriction $\varphi = 0$. Estimating with non-linear least squares, I consistently found a negative and highly significant estimate for φ of -0.1, implying that places with higher travel mortality had better institutions, a result which appears inconsistent with AJR's theory.

rate of 30.5 for 1818-36 (Curtin, 1989, p.7) appears to refer to soldiers in barracks. The rates of 2.88 and 2.55 for France and the United Kingdom are from 1909 to 1913 (Curtin, 1989, p. 9), and are clearly too low as they come from a period much too late in history. The rates of 20.17 and 15.3 from the early-mid nineteenth century, listed on Curtin (1989, p.7) and mentioned by AJR, are certainly more comparable, although they do belong to indigenous soldiers in barracks. Other data points in these data sets are constructed by attaching mortality rates to adjoining countries. For example, in the data set of Rodrik et al. (2004) and Acemoglu et al. (2003), ten different African countries acquire the mortality rate for the "French Soudan" of 280 discussed earlier, including countries as far away as Burundi and Rwanda.

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[*] Source brought to attention in AJR’s (2005) Response, along with Barfour (1845), Cantlie (1974), and Tulloch (1847) mentioned in the main text.

APPENDIX TABLE A2: REVISIONS USING DATA FROM ACEMOGLU ET AL. (2005)

Country	Old Rate	New Rate	Reason
Hong Kong	14.9	285	Actual rate
Bahamas	85	189	Actual rate
Australia	8.55	14.1	Actual rate
Guyana	32.18	84	Actual rate
Honduras	78.1	95.2	Actual rate
Singapore	17.7	20	Actual (shared) rate
Trinidad	85	106.3	Lowest rate within Trinidad & Tobago
Sierra Leone & Guinea	483	350	Eliminates mortality from Gambia

Data sources from Acemoglu et al. (2005). See Appendix for more information.

APPENDIX TABLE A3: DATA CHECKS USING REVISED DATA
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)	Without Africa (9)
Panel A: Original data (N=64, J=36)									
Log mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.40	-0.42	-0.52	-1.21
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.19)	(0.22)	(0.18)
p -value of log mortality	0.001	0.01	0.03	0.04	0.11	0.04	0.03	0.02	0.000
p -value of controls	-	0.17	-	0.40	0.34	0.01	0.02	0.40	-
Panel B: Eliminating inconsistencies, same as Table 2, Panel E (N=62, J=33)									
Log mortality (β)	-0.46	-0.31	-0.20	-0.18	-0.04	-0.19	-0.22	-0.22	-1.21
(heteroscedastic-clustered s.e.)	(0.20)	(0.24)	(0.19)	(0.22)	(0.23)	(0.20)	(0.21)	(0.27)	(0.18)
p -value of log mortality	0.03	0.21	0.30	0.43	0.85	0.34	0.30	0.43	0.000
p -value of controls	-	0.14	-	0.11	0.07	0.002	0.01	0.08	-
Panel C: Eliminating inconsistencies and revising mortality rates with Acemoglu et al.(2005) data (N=62 J=37)									
Log mortality (β)	-0.42	-0.25	-0.13	-0.13	0.00	-0.14	-0.14	-0.15	-1.06
(heteroscedastic-clustered s.e.)	(0.20)	(0.23)	(0.19)	(0.22)	(0.22)	(0.20)	(0.21)	(0.25)	(0.27)
p -value of log mortality	0.05	0.29	0.51	0.55	0.99	0.48	0.51	0.55	0.001
p -value of controls	-	0.08	-	0.04	0.02	0.001	0.004	0.03	-
Panel D: Original data except with USA & Canada given initial settler mortality rates (N=64 J=36)									
Log mortality (β)	-0.45	-0.35	-0.40	-0.11	-0.08	-0.21	-0.27	-0.21	-0.55
(heteroscedastic-clustered s.e.)	(0.18)	(0.20)	(0.17)	(0.20)	(0.20)	(0.18)	(0.19)	(0.27)	(0.41)
p -value of log mortality	0.02	0.08	0.03	0.58	0.68	0.25	0.17	0.44	0.20
p -value of controls	-	0.08	-	0.04	0.02	0.001	0.004	0.03	-

Revised mortality rates used in panel C using Acemoglu et al. (2005) data given in Appendix Table A2. In Panel D, initial settler mortality rate for USA is 227 (replacing 15) and for Canada is 283 (replacing 16.1), all other rates from original series. N is the total number of observations and J is the number of clusters determined by the number of distinct mortality rates. See text and Table 2 for more detail.

APPENDIX TABLE A4: DATA CHECKS INVOLVING BISHOPS
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)	Without Africa (9)
Panel A: Original data (N=64, J=36)									
Log mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.40	-0.42	-0.52	-1.21
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.19)	(0.22)	(0.18)
p -value of log mortality	0.00	0.01	0.03	0.04	0.11	0.04	0.03	0.02	0.000
p -value of controls	-	0.17	-	0.40	0.34	0.01	0.02	0.40	-
Panel B: Eliminating countries with rates "benchmarked" to bishops (N=48 J=33)									
Log mortality (β)	-0.62	-0.51	-0.39	-0.40	-0.32	-0.29	-0.37	-0.36	-1.13
(heteroscedastic-clustered s.e.)	(0.17)	(0.20)	(0.17)	(0.20)	(0.23)	(0.19)	(0.20)	(0.22)	(0.23)
p -value of log mortality	0.001	0.02	0.03	0.06	0.17	0.14	0.07	0.10	0.000
p -value of controls	-	0.23	-	0.27	0.36	0.002	0.01	0.02	-
Panel C: Using bishop data directly (N=64 J=35)									
Log mortality (β)	-0.44	-0.34	-0.28	-0.30	-0.23	-0.24	-0.21	-0.26	-0.66
(heteroscedastic-clustered s.e.)	(0.15)	(0.15)	(0.12)	(0.16)	(0.18)	(0.16)	(0.17)	(0.20)	(0.32)
p -value of log mortality	0.01	0.03	0.03	0.08	0.20	0.15	0.22	0.19	0.050
p -value of controls	-	0.07	-	0.08	0.04	0.001	0.03	0.15	-
Panel D: Using a dummy variable for rates "benchmarked" to bishops (N=64 J=36)									
Log mortality (β)	-0.63	-0.54	-0.41	-0.42	-0.35	-0.39	-0.40	-0.41	-1.14
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.18)	(0.21)	(0.21)
p -value of log mortality	0.001	0.01	0.03	0.04	0.12	0.04	0.04	0.06	0.000
p -value of bishop dummy	0.11	0.16	0.64	0.25	0.31	0.05	0.002	0.01	0.37
p -value of controls	-	0.21	-	0.29	0.36	0.009	0.003	0.04	-
Panel E: Using separate dummy variables for campaign, laborer, and bishop rates (N=64 J=36)									
Log mortality (β)	-0.48	-0.41	-0.31	-0.39	-0.32	-0.31	-0.33	-0.35	-1.11
(heteroscedastic-clustered s.e.)	(0.20)	(0.23)	(0.19)	(0.22)	(0.24)	(0.22)	(0.19)	(0.22)	(0.23)
p -value of log mortality	0.03	0.08	0.12	0.09	0.20	0.18	0.09	0.12	0.000
p -value of all dummies	0.17	0.26	0.49	0.45	0.56	0.09	0.01	0.03	0.62
p -value of controls	-	0.32	-	0.63	0.69	0.020	0.01	0.18	-

Countries "benchmarked" to bishops given in Appendix Table A1. Results in Panel C derived by replacing mortality rates of 71 with 16.7, 78.1 with 17.5, and 163.3 with 32.8. See text and Table 2 for more detail.

APPENDIX TABLE A5: FIRST STAGE ESTIMATES SEPARATING CAMPAIGN RATES AND BARRACKS RATES
(Dependent Variable: Expropriation Risk)

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)	Without Africa (9)
Panel A: Original data (N=64, J=36)									
Log mortality (β)	-0.61	-0.52	-0.40	-0.44	-0.35	-0.40	-0.42	-0.52	-1.21
(heteroscedastic-clustered s.e.)	(0.17)	(0.19)	(0.17)	(0.20)	(0.21)	(0.19)	(0.19)	(0.22)	(0.18)
p -value of log mortality	0.001	0.01	0.03	0.04	0.11	0.04	0.03	0.02	0.000
p -value of controls	-	0.17	-	0.40	0.34	0.01	0.02	0.40	-
Panel B: Campaign rates only (N=26 J=18)									
Log mortality (β)	-0.10	-0.13	-0.10	-0.02	-0.06	-0.03	-0.05	0.06	0.77
(heteroscedastic-clustered s.e.)	(0.22)	(0.28)	(0.22)	(0.25)	(0.30)	(0.32)	(0.22)	(0.34)	(0.48)
p -value of log mortality	0.66	0.64	0.66	0.95	0.85	0.93	0.83	0.87	0.18
p -value of controls	-	0.74	-	0.00	0.00	0.68	0.00	0.33	0.000
Panel C: Barracks Rates Only (N=18 J=13)									
Log mortality (β)	-1.18	-1.04	-0.82	-1.27	-0.99	-0.79	-0.94	-0.96	-1.29
(heteroscedastic-clustered s.e.)	(0.20)	(0.25)	(0.28)	(0.36)	(0.41)	(0.30)	(0.26)	(0.16)	(0.21)
p -value of log mortality	0.000	0.00	0.02	0.00	0.03	0.02	0.00	0.00	0.000
p -value of controls	-	0.25	-	0.78	0.27	0.058	0.240	0.09	-
Panel D: Campaign rates only, extended revision, dropping countries with rates from other countries (N=14 J=14)									
Log mortality (β)	0.18	0.24	0.18	0.23	0.27	0.26	0.22	0.40	1.03
(heteroscedastic-clustered s.e.)	(0.25)	(0.29)	(0.25)	(0.26)	(0.29)	(0.26)	(0.25)	(0.24)	(1.60)
p -value of log mortality	0.48	0.42	0.48	0.40	0.37	0.33	0.41	0.13	0.567
p -value of controls	-	0.53	-	0.00	0.01	0.03	0.00	0.04	0.000
Panel E: Barracks rates only, extended revision, dropping countries with rates from other countries (N=17, J=17)									
Log mortality (β)	-0.81	-0.63	-0.29	-0.79	-0.58	-0.44	-0.43	-0.55	-0.75
(heteroscedastic-clustered s.e.)	(0.30)	(0.32)	(0.32)	(0.31)	(0.26)	(0.35)	(0.37)	(0.25)	(0.26)
p -value of log mortality	0.02	0.07	0.38	0.02	0.04	0.22	0.26	0.05	0.01
p -value of controls	-	0.15	-	0.05	0.02	0.032	0.157	0.01	-

See Appendix Table A1 for indicators of whether a country uses a campaign rate, excluding campaign rates based off of bishops. Barracks rates are those which are neither campaign, laborer, or bishop rates. Extended revisions used in Panels D and E explained in Table 1 and Appendix Table A2. See text and Table 2 for more detail.

APPENDIX TABLE A6: FIRST STAGE ESTIMATES USING CONSTRAINT ON EXECUTIVE IN 1990 AS INSTITUTIONS MEASURE

Control Variables	No Controls	Latitude Control	Without Neo-Europes	Continent Dummies	Continents Dummies & Latitude	Mean Temp and Min Rain	Percent European, 1975	Malaria in 1994	Without Africa
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Original data (N=34, J=60)									
Log mortality (β)	-0.97	-0.93	-0.86	-0.47	-0.46	-0.67	-0.57	-0.39	-0.76
(heteroscedastic-clustered s.e.)	(0.21)	(0.26)	(0.27)	(0.29)	(0.32)	(0.28)	(0.25)	(0.26)	(0.25)
p -value of log mortality	0.000	0.001	0.003	0.12	0.16	0.02	0.03	0.15	0.01
p -value of controls	-	0.70	-	0.000	0.000	0.02	0.000	0.001	-
Panel B: Eliminating inconsistencies, 1990 (N=31, J=58)									
Log mortality (β)	-0.81	-0.73	-0.64	-0.31	-0.28	-0.40	-0.39	-0.14	-0.76
(heteroscedastic-clustered s.e.)	(0.26)	(0.32)	(0.32)	(0.29)	(0.33)	(0.31)	(0.28)	(0.28)	(0.25)
p -value of log mortality	0.004	0.03	0.06	0.30	0.40	0.20	0.18	0.61	0.01
p -value of controls	-	0.61	-	0.000	0.000	0.01	0.000	0.000	-
Panel C: Eliminating inconsistencies and adding campaign and laborer dummies (N=31 J=58)									
Log mortality (β)	-0.61	-0.58	-0.53	-0.19	-0.18	-0.20	-0.21	0.19	-0.65
(heteroscedastic-clustered s.e.)	(0.33)	(0.42)	(0.37)	(0.29)	(0.34)	(0.31)	(0.30)	(0.23)	(0.28)
p -value of log mortality	0.07	0.17	0.16	0.51	0.60	0.51	0.49	0.41	0.04
p -value of controls	-	0.88	-	0.000	0.000	0.01	0.000	0.000	-
Panel D: Eliminating inconsistencies, adding dummies, and eliminating countries with rates from other countries (N=26, J=26)									
Log mortality (β)	-0.07	0.07	0.04	0.03	0.11	0.09	0.07	0.24	-0.22
(heteroscedastic-clustered s.e.)	(0.35)	(0.39)	(0.38)	(0.37)	(0.41)	(0.39)	(0.37)	(0.31)	(0.24)
p -value of log mortality	0.85	0.86	0.93	0.94	0.79	0.83	0.86	0.45	0.38
p -value of controls	-	0.14	-	0.22	0.24	0.27	0.07	0.02	-

Constraint on Executive in 1990 is on a scale from 1 to 7 with a higher score indicating more constraints, taken from the Polity III data set. Sample does not include the Bahamas, Hong Kong, Malta and Sierra Leone. See text and other tables for additional information.

APPENDIX TABLE A7: FIRST STAGE ESTIMATES USING LAW AND ORDER TRADITION IN 1995 AS INSTITUTIONS MEASURE

Control Variables	No Controls (1)	Latitude Control (2)	Without Neo-Europes (3)	Continent Dummies (4)	Continents Dummies & Latitude (5)	Mean Temp and Min Rain (6)	Percent European, 1975 (7)	Malaria in 1994 (8)	Without Africa (9)
Panel A: Original data (N=63, J=36)									
Log mortality (β)	-0.54	-0.42	-0.41	-0.39	-0.29	-0.39	-0.41	-0.46	-1.01
(heteroscedastic-clustered s.e.)	(0.13)	(0.15)	(0.14)	(0.14)	(0.15)	(0.16)	(0.14)	(0.16)	(0.15)
p -value of log mortality	0.000	0.01	0.01	0.01	0.06	0.02	0.01	0.01	0.000
p -value of controls	-	0.04	-	0.01	0.002	0.02	0.10	0.49	-
Panel B: Eliminating inconsistencies, 1990 (N=61, J=33)									
Log mortality (β)	-0.47	-0.33	-0.32	-0.26	-0.12	-0.30	-0.32	-0.30	-1.01
(heteroscedastic-clustered s.e.)	(0.14)	(0.18)	(0.15)	(0.16)	(0.17)	(0.17)	(0.16)	(0.20)	(0.15)
p -value of log mortality	0.002	0.07	0.04	0.12	0.47	0.09	0.06	0.13	0.000
p -value of controls	-	0.05	-	0.00	0.000	0.01	0.05	0.17	-
Panel C: Eliminating inconsistencies and adding campaign and laborer dummies (N=61 J=33)									
Log mortality (β)	-0.37	-0.26	-0.27	-0.24	-0.12	-0.25	-0.23	-0.20	-0.87
(heteroscedastic-clustered s.e.)	(0.16)	(0.18)	(0.16)	(0.17)	(0.18)	(0.18)	(0.16)	(0.21)	(0.15)
p -value of log mortality	0.03	0.16	0.10	0.17	0.50	0.16	0.16	0.34	0.000
p -value of controls	-	0.07	-	0.01	0.00	0.16	0.05	0.20	-
Panel D: Eliminating inconsistencies, adding dummies, and eliminating countries with rates from other countries (N=27, J=27)									
Log mortality (β)	-0.29	-0.10	-0.14	-0.15	0.00	-0.13	-0.08	-0.17	-1.04
(heteroscedastic-clustered s.e.)	(0.23)	(0.24)	(0.21)	(0.22)	(0.24)	(0.22)	(0.20)	(0.26)	(0.20)
p -value of log mortality	0.21	0.67	0.51	0.48	0.99	0.55	0.68	0.52	0.000
p -value of controls	-	0.14	-	0.22	0.24	0.27	0.07	0.02	-

Law and Order Tradition in 1995 is measured on a scale from 0 to 6, with a higher score meaning more law and order, from Political Risk Services. Sample does not include El Salvador. See text and other tables for additional information.

FIGURE A1: ASSIGNMENT OF MORTALITY RATES TO LATIN AMERICA USING "BENCHMARKING"