A Response to Albouy's "A Reexamination Based on Improved Settler Mortality Data"[*]

Daron Acemoglu
Department of Economics, MIT

Simon Johnson
Sloan School of Management, MIT

James Robinson
Government Department, Harvard

March 21, 2005

Abstract

In a recent comment, David Albouy claims that the data series we constructed for European settler mortality in Acemoglu, Johnson and Robinson (2001) suffers from "inconsistencies, questionable judgements, and errors." He proposes two series, for "barracks" and "campaigns," that he claims are better measures of settler mortality, and shows his recodings weaken or eliminate the first-stage relationship between settler mortality and institutions today. In this note we show that his claims are without foundation. Our original coding of the data was not inconsistent, questionable, or erroneous. Instead, Albouy's results are entirely driven by inconsistent, incorrect, selective, and/or unreasonable revisions to our original data, particularly, but not exclusively, for Africa.

"Military returns [reports of disease and death] serve to indicate to the restless wanderers of our race the boundaries which neither the pursuit of wealth nor the dreams of ambition should induce them to pass, and to proclaim in forcible language that man, like the elements, is controlled by a Power which hath said, "Hither thou shalt come, but no further." (Tulloch, 1847, p. 259).

Beware and take care; Of the Bight of Benin: For one that comes out; There are forty go in (traditional sailor's song, from Curtin, 1969, p.282).

1   INTRODUCTION

David Albouy's paper "The Colonial Origins of Comparative Development: A Reexamination Based on Improved Settler Mortality Data," is a lengthy comment on our "The Colonial Origins of Comparative Development: An Empirical Investigation" published in the *American Economic Review*, December 2001 (henceforth AJR). In cross-country regressions, our paper showed that there was a strong relationship between measures of institutions in countries today (with a particular focus on the risk of expropriation) and the (logarithm of) mortality rates faced by Europeans if they tried to settle in the same place 150 or more years ago. Using "log settler mortality" as an instrument for modern institutions, we argued that institutions have a strong causal effect on GDP per capita today.

As part of our research strategy, we had to construct a series for European mortality rates. Our data construction strategy was transparent and quite simple. Essentially, we took data from the one table of early mortality rates for European soldiers in Curtin (1989), supplemented this with some additional later data from Curtin (1998), and proposed a way to merge these data with information on the mortality of bishops in Latin America from Gutierrez (1986) (see Section 2). We subjected our data to numerous robustness checks, and particularly emphasized that while the data for Africa were less reliable, our primary results held without any African observations. The main advantage of our data construction strategy was that it left little room for judgment regarding which data sources were reliable. In addition, we have made our data easily available, and numerous researchers have investigated and replicated all of our findings.

Nevertheless, according to Albouy there are "a number of inconsistencies, questionable judgments, and errors in the mortality estimates that induce an artificial correlation with expropriation risk and GDP per capita" (Albouy, 2004b, abstract). After recoding the data (or constructing his own data from various new sources), Albouy finds that the first-stage relationships are weakened and that our paper suffers from "weak instruments pathologies." We respond here by showing that Albouy's criticisms of our paper are without foundation, and result from the problematic way he selects from various data sources. Along the way, we show that the original AJR series was coded consistently and gives results that are close to reasonable alternative series that can be constructed with the available data.

Albouy reaches his conclusions by combining three strategies. These are (1) numerous revisions to our data series; (2) use of Anderson-Rubin (AR) confidence intervals rather than the standard Wald confidence intervals (Anderson and Rubin, 1949); (3) clustering the standard errors and confidence intervals. Of these three changes, by far the most important is the first, and this note will focus mostly on data issues. Albouy (2004b) constructs two alternative series, which he argues represent soldiers in barracks and soldiers on campaign, and shows that with both series our results are much weaker and log settler mortality is a weak instrument. Albouy argues his barracks series is a better measure of settler mortality because it removes

1

data that is based on "campaign" experience. His campaign series is presented as mortality from disease in comparable campaigns.

We disagree with almost all the revisions suggested by Albouy.[1] In Section 4, we discuss his objections to our original series in detail and show them to be unsupported by the facts. After reviewing all the available sources, we do not find any instance of a significant mistake or problem in the original AJR (2001) coding. Instead, there are a large number of unreasonable choices as well as some misquotations and mistakes in Albouy's revisions. Since a discussion of each data point in detail may be dull and difficult to follow for those not in the debate, Section 3 deals with the major problems in Albouy's revised series. These include:

- Selective assignment of mortality rates to some, but not all neighbors in West Africa, in his barracks series.

- Implausible assumptions regarding how to merge the Latin American data with the rest of the world, in his barracks series.

- For tropical places, insistence on using data after there had been a dramatic improvement in relevant military medical practices, both in his campaign and barracks series.

- Selective use of numbers for North America in his campaign series and for the Caribbean and Africa in his campaign and barracks series.

- Ignoring data from sources that he himself introduces, in his campaign and barracks series.

- Mixing of weighted and unweighted averages in constructing mortality rates over time, in his campaign series.

In each case, Albouy has made a dubious choice that favors his hypothesis. Correcting these problems has a dramatic effect on Albouy's results. Although these problems are discussed in greater detail in Section 3, it is useful to mention the most notable ones here. For example, just slightly changing Albouy's arbitrary assignment of rates in West Africa brings the results from his revised barracks series close to the results from our original series.[2] More concretely, Albouy acknowledges only two valid datapoints for his barracks series in West Africa, for Senegal and Ghana.[3] He assigns the Senegal rate to Gambia and the Ghana rate to Cote d'Ivoire, *but not*

---

[1]Some of the other revisions suggested by Albouy are very similar to or actually a subset of the robustness checks we reported in NBER working paper 7771 (Acemoglu, Johnson, and Robinson 2000). Albouy fails to acknowledge this point fully.

[2]In fact, our most significant and substantial disagreement concerns African numbers. This can be seen from the fact, documented below, that, without Africa, Albouy's series yield very similar results to our series. Albouy's barracks series implies that the mortality rate for potential European settlers in West Africa until the mid-nineteenth century was no worse than the rate for similar people in South Asia, in the East Indies, and in the West Indies. In his barracks series, Senegal and Gambia have a mortality rate of 72 per 1,000 p.a., while Sri Lanka has a rate of 69.8, Indonesia has a rate of 64.52, and Trinidad and Tobago has a rate of 70.

That the mortality rate was much higher for Europeans in West Africa than anywhere else in the world is a point of general agreement among medical historians, and was also medical and public opinion during the 18th and 19th centuries. Previous attempts to deny or ignore this evidence, most notably by T. Foxwell Buxton, have had disastrous consequences. An expedition backed by Buxton steamed up the Niger in 1841. Of the 159 Europeans, 48 died during the first two months on the river; "...the disaster was magnified in the public mind by the false hopes of the enthusiasts," (Curtin, 1964, p.303).

[3]He ignores (high mortality) data for Sierra Leone and Gambia and earlier (higher) mortality estimates for Senegal and Ghana. He also ignores our data from small expeditions in Mali and Nigeria.

to any of their other neighbors. No justification is offered by Albouy for this practice (see p. A2 of Albouy, 2004b). In the rest of his barracks series and throughout his campaign series (including for Africa), if Albouy assigns mortality rates to any neighbors (that are otherwise missing data), he assigns it to all neighbors! *Remarkably*, if we assign his West African rates either (a) to no neighbors or (b) to all neighbors, his results change significantly and become very similar to those reported in AJR. The same point can be seen from Albouy's own results. When he follows our robustness check of reporting regressions without Africa, his results are very similar to ours and are highly significant. Section 3 provides details on these points.

In a further example, Albouy assumes the mortality rates of bishops are directly comparable with mortality rates for soldiers in his barracks series (i.e., he includes them in the same series without any adjustment). But the available numbers suggest large differences between bishop and soldier mortality rates (approximately three-fold, see Section 3 and Appendix 2). Interestingly, in his own campaign series, he follows AJR by assuming the rates are not directly comparable, and need to be benchmarked in order to be sensibly included in the same series![4] In addition, Albouy always uses bishops' mortality rates calculated without replacement (i.e., deaths as a fraction of an assumed initial population level), whereas almost all the other mortality rates he uses are deaths as a fraction of mean strength, which is a calculation with replacement (see Section 4 and Appendix 2 for details).[5] Fixing these inconsistencies also has a very significant effect on his results and brings them close to our original findings.

We fundamentally disagree with the notion that there is much to be learned about settler mortality rates from a campaign series. The campaigns that Albouy combines are of very different nature and sizes, mixing small expeditions (a few hundred Europeans) with major wars (e.g., the U.S. Civil War). In addition, by requiring that a large military force be present, Albouy is delaying his selection of data from tropical places until after the mortality revolution of the mid/late nineteenth century. Despite this problematic procedure, we show that correcting a number of mistakes and problems in his series has a very significant effect on his results. For example, if we correct for his selective use of weighted and unweighted data,[6] and/or incorporate data from British campaigns in North America rather than the US Civil War, and/or incorporate data on Africa that he ignores, the results become similar to those in AJR. All of this is detailed in Section 3.

In addition to these major issues, Section 4 documents a large number of other problems in Albouy's data and arguments, including selective data reporting and misquotations. For example, Albouy excludes some of our data because of epidemics, but then selects other observations from epidemic periods (e.g., the U.S. Civil War and Malta in his campaign series). He codes some observations as missing in his barracks series, even though he has a campaign estimate (e.g., much of Africa, Vietnam, Hong Kong), while in other cases he is willing to infer a campaign rate from a barracks rate (e.g., Indonesia, Malaysia and Singapore). He uses the U.S. Civil War mortality estimate from Adams (1952) for his campaign series but not wartime

---

[4]By "benchmarking," we refer to the procedure in AJR whereby overlapping observations are used to convert relative mortality in the bishops series to the equivalent absolute mortality levels in the soldiers series. We review the detailed benchmarking procedure in Section 2 below; Section 5 presents further evidence supporting benchmarking.

[5]The point that these two methods give numbers that are not directly comparable is clear in sources that Albouy uses, see for example p.21 of Curtin (1998).

[6]In his campaign series, Albouy reports an unweighted average of mortality rates for almost all datapoints, including where it would be possible to calculate a weighted average (e.g., Vietnam), but uses a weighted average for Sudan and Mexico.

and peacetime estimates for Mexico that are a few pages away. He asserts our mortality estimates for the West Indies are wartime numbers, based on a selective quotation from Curtin, but they are plainly from a time of peace. He disregards information on how changing military procedures lowered mortality rates for soldiers in the tropics during the nineteenth century – thus making estimates of military death rates later in the nineteenth century less reliable as proxies for early European settler mortality – despite the fact that this is emphasized by Curtin, as well as by Cohen (1983), which is a new source that Albouy himself introduces. In fact, Albouy quotes rather selectively or misquotes from many sources that he introduces, including Austin (1979), Feinberg (1974), Graham (1978), Gregory (1996) and Kennedy (1970).[7] Taken individually, however, most of these issues do not have a major effect on the results, but are indicative of the general problems in his approach.[8]

We also look at the issue of weak instruments in Section 3, by calculating 95% AR (Anderson-Rubin) confidence intervals, which are robust in the presence of weak instruments. Consistent with other work that has looked at the issue of weak instruments in the AJR regressions (e.g., Chernozukov and Hansen, 2005), we conclude that there is no weak instrument problem either in our original AJR data series or in reasonable permutations – in the sense that the AR confidence interval excludes zero and any negative values in our core specifications (though there is a weak instrument problem in some specifications that include a large number of covariates, for example, in specifications that simultaneously control for continent dummies, latitude, and the fraction of the current population that is of European descent).[9]

Finally, in our original paper, we also reported that clustered confidence intervals do not make any substantive difference to our results (AJR, footnote 18, p. 1385). We show this in greater detail as we present results in Section 3 below.

We also take this opportunity to extend the description of the data in our original paper (especially compared with the shorter published version) and show robustness with additional data we have collected since publishing that work. Albouy's comment has convinced us that we were not detailed enough about data construction in our original paper. Most notably, we should have stated more explicitly that we chose the earliest peacetime number available, and when such information was not available, we took the earliest mortality estimate from

---

[7]For example, in using Austin, Albouy ignores this source's statement that the reported mortality rates include deaths during the voyage to Australia. He omits to mention Cohen's point that the French military found ways to lower mortality in the tropics during the nineteenth century (using means not available to potential settlers). From Feinberg he overlooks a favorable assessment of mortality from disease in British America. In Graham he ignores the point that Hong Kong was regarded as healthy enough to serve as a gathering point for a large invasion force. He ignores Gregory's assessment of mortality on Malta refers to an epidemic episode. He cites some passages from Kennedy and ignores others that do not support his hypothesis. He does not discuss the fact that his sources on British North America agree that high early death rates were due largely to exposure, malnutrition, and starvation. Moreover, we document a number of instances of misquotations from Curtin in Section 4.

[8]Albouy's suggestion of reassigning values in central Africa is a reasonable robustness check. Also, his concerns about campaigns can be largely addressed by dropping data from larger expeditions, such as Algeria, Ethiopia, Hong Kong, and Vietnam. None of these make a major difference to our results; see Appendix 3. We also agree with Albouy that it is useful to investigate whether large outliers have an undue effect on the results. We reported results that addressed this point in our working paper, and we extend this analysis here by capping mortality rates at various levels. If anything, reducing the effect of outliers in this way actually strengthens our results.

[9]In interpreting these results, it should be borne in mind that both the current prevalence of malaria and the current fraction of the population of European descent are outcome variables, clearly influenced by the broad cluster of institutions, so the fact that they are correlated with our instrumented institutions measures, thus somewhat weaken the first and second stages, is not surprising.

an "expedition" – 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 these did not involve much fighting or serious casualties from enemy fire.[10] This is quite similar to peacetime data, since these latter data include episodes in which soldiers march, for example to change stations.[11] We continue to believe this coding is sensible and the evidence presented here is supportive.[12]

Regarding the issue of additional data, we continue to investigate original sources for mortality estimates. In particular, we have sought out Curtin's key original sources, to see if they contain further relevant information.[13] We take this opportunity to report, in Section 5, our findings to date. Most of these data are close to our original estimates, but there is a significant potential revision for Hong Kong. Our newly collected information suggests two further ways of imputing mortality rates for Latin America and we report results from a variety of reasonable robustness checks. Overall, our results are slightly weaker but this is mostly due to a few outlying observations in Africa. AR confidence intervals, with clustering, typically exclude values below 0.3 (and often higher) for the second stage coefficient and in almost all cases they exclude a zero effect and negative effects; the standard (95%) lower bound confidence interval in our original work was also typically 0.3-0.4. However, the effect of institutions on income per capita is weaker when we include as covariates the current prevalence of malaria or the percent of population that was of European descent in 1975.

Overall, we conclude that the original data series used in AJR is close to any reasonable alternative that can be constructed with the available evidence, and using additional data from the original sources confirms this conclusion. There seems to be no foundation for the criticisms raised by Albouy.

The rest of the paper is organized as follows. Section 2 reviews the instrumental variable strategy and the basics of data construction in AJR. Section 3 explains the major problems in Albouy's two alternative series and presents results, including AR confidence intervals, using the original AJR series and corrected versions of Albouy's series. Section 4 assesses the other revisions made by Albouy. Albouy claims that there were significant errors in our original data and that his data are an improvement. We document that there is no evidence to support his claims and no significant errors in our original data. Section 5 presents further robustness checks based on new data that we have collected. Appendix 1 demonstrates that, again contrary to what Albouy claims, our original classification of countries to Gutierrez's mortality regions in Latin America is correct. Appendix 2 shows in detail the correct way to calculate Curtin-comparable mortality rates from Gutierrez's data for Latin America; this supports the numbers in AJR and not the numbers in Albouy. Appendix 3 shows that our

---

[10]We should also have stated more clearly that in a few cases (Vietnam and Hong Kong) we rely on mortality estimates from a table in Curtin (1998) p.239 that gives a mortality rate per campaign rather than per annum. Neither Curtin nor the original source, Anonymous (1898), indicates the precise length of campaign, although many of these lasted around a year (including travel time).

[11]The original sources are quite clear on this point. See, for example, Army Medical Department (1841, pp.27-28) on Ceylon, and Army Medical Department (1840) on troop activity in Western Africa. Marching or otherwise moving about the country was hazardous, but an unavoidable part of peacetime military activity, and quite comparable to experience on the smaller expeditions in our dataset.

[12]We also find that AJR's ratio between low and high mortality regions for bishops, from the Gutierrez data, was not quite right. We used an approximation that gave a ratio of 2.3, but in fact the correct ratio is 2.2 (see Appendix 2 for details). This does not affect the original AJR results within 2 decimal places, but still we report here results with the correct ratio.

[13]Albouy's suggested revisions do not make use of these original data sources.

results actually get stronger if we drop datapoints for which we do not have peacetime data or data from small expeditions.

2 Background

The main focus of AJR (2001) was to estimate the causal effect of a broad cluster of institutions on long-run development. We attempted this based on a particular theory regarding the development of institutions in former European colonies. Briefly, we argued that:

1. There were various types of colonization policies which created different sets of institutions. At one extreme, European powers set up "extractive states", which introduced neither any significant protection for private property nor any checks and balances against government expropriation. In these cases, the main purpose of the extractive state was to transfer resources of the colony to the colonizer. At the other extreme, Europeans settled in a number of colonies, creating various European offshoots. In these cases, settlers tried to replicate or extend European institutions, with great emphasis on private property and checks against government power.

2. The choice of colonization strategy was influenced by the feasibility of settlements. In particular, in places where the mortality rate from disease for Europeans was relatively high, the odds were against the creation of settler colonies with better institutions, and the formation of an extractive state was more likely.

3. The colonial-type state and related institutions persisted even after independence, so that there was a tendency for societies with worse colonial institutions to also have worse institutions after independence.

Based on these three premises, we suggested that the mortality rates expected by the first European settlers in the colonies could be an instrument for current institutions in these countries. Schematically our strategy was summarized as:

$$\begin{matrix} \text{(potential) settler} \\ \text{mortality} \end{matrix} \Rightarrow \text{settlements} \Rightarrow \begin{matrix} \text{early} \\ \text{institutions} \end{matrix} \Rightarrow \begin{matrix} \text{current} \\ \text{institutions} \end{matrix} \Rightarrow \begin{matrix} \text{current} \\ \text{performance} \end{matrix}$$

This strategy faced many conceptual and practical problems. In addition to issues of whether potential settler mortality could be excludable (for example, because it is correlated with inhabitants' current mortality), what type of proxies for a broad cluster of institutions should be used, and why institutions persist, there was the practical issue of constructing a reliable series for potential European settler mortality.

The paper was clear that any series would be subject to a significant degree of measurement error. Nevertheless, as argued in footnote 17, page 1383 in AJR, our hope was that any measurement error would be of the classical type, which would not lead to inconsistent IV estimates.

Most important, by its nature, *potential* settler mortality is often not observed. In places where the potential settler mortality was high, large numbers of settlers did not go, and it is difficult to obtain comparable measures of their mortality. Moreover, in the critical early periods for settlements and institutional development, data on mortality rates of European settlers are hard to find. Our strategy was therefore to use a homogeneous group of Europeans in these colonies to form an estimate of settler mortality rates. This strategy was made possible

6

by the fact that Philip Curtin in a series of works, most notably Curtin (1989) and Curtin (1998), but also Curtin (1964), reported comparable data on the disease mortality rates of European soldiers stationed in various colonies.

As a practical matter our approach was straightforward. We began with Table 1.1 of Curtin (1989), which is entitled, "Mortality of European Troops Overseas, 1817-38." This is a summary of Curtin's base data from around the world. Curtin's book is focussed on the relocation costs for Europeans, i.e., exactly the issue we are interested in, and he has a reputation for being very careful with data, so it made sense to take these estimates without any editing or selectivity. Note that while these data are for soldiers, for whom there is always likely to be some military activity (marching, engaging in exercises, travelling on ships, etc.), these base data are essentially *peacetime*.[14]

We followed Curtin's lead with regard to the data. Curtin (1989) and (1998) emphasized that mortality rates declined through the nineteenth century as European militaries became better at managing health issues. In particular, after 1850 there were dramatic declines in military mortality from disease in the tropics. This is emphasized by Curtin, for example, in the contrast between his Table 1.1 and the much lower death rates in his Table 1.2, "Mortality of European Troops Overseas, 1909-13." Curtin's work therefore focussed our attention on taking the earliest possible peacetime data (preferably before 1850), and we tried to stick to this throughout.[15]

While Curtin's Table 1.1 spanned most of the world, it did not report specific estimates for all countries. We therefore adopted the following coding rule. In each case we took the estimate from Table 1.1 if available. We then took the earliest *peacetime* number from Curtin when such data were available.[16] In the absence of such a number, we used the earliest *expedition* mortality.[17] The expedition mortality estimates came from Curtin (1989) or, if nothing relevant was in that source, from Curtin (1998). In addition, if it was likely that Europeans faced similar mortality rates in two countries but only one of them had a mortality estimate, we assigned the mortality rate from one country to the other. In the NBER working paper version, we provided a detailed analysis of an alternative series without this type of assignment and also some other robustness checks – Acemoglu et al., 2000, Table 5, columns 1 through 4 do not assign mortality rates to neighbors.

Since we followed this coding rule rather than make arbitrary judgment calls, some of the mortality rates in West Africa were extremely high, especially when the soldiers encountered a yellow fever epidemic.[18] We discussed that this was unlikely to bias the results, since even if the estimates came from small groups of soldiers, this would simply lead to measurement error, which should not lead to inconsistent IV estimates. Moreover, our instrument was the logarithm of settler mortality, so these high rates did not exert undue influence. Acemoglu

---

[14]Assessing our entire dataset, Appendix 3 reports results in which we drop observations for which we do not have peacetime information.

[15]From the perspective of our theoretical framework, we really needed potential settler mortality before 1800 – as we were arguing that this was the formative period for the colonized societies. But such data are not generally available, and in his estimates before 1850 Curtin offered data from before the improvement in European public health management (both in general and for the tropics in particular).

[16]Perhaps we should have stressed that in our view settler mortality would be best measured by peacetime estimates, where available. We thought this was obvious.

[17]An expedition is a group of men, often soldiers, travelling together for a particular purpose. This could be exploration, to open trade routes, to demonstrate force against a local ruler, or some combination of these activities.

[18]Other mortality rates from using this procedure, for example in Ethiopia, were very low.

et al., 2000, Table 5, already reported results using an alternative long averages for Africa. Nevertheless, in Section 3 we show again that outliers do not drive our results and in Section 4 we demonstrate that the observations that Albouy regards as outliers were fairly typical experiences.

The most important gap in Curtin's data is for Latin America. Curtin reported estimates for the Caribbean, but for Central and South America, Curtin's work contained estimates only for Mexico.[19] To supplement the numbers from Curtin, we used an article by Hector Gutierrez (1986) on the mortality rates of bishops in Latin America.[20] It seemed reasonable to assume the mortality rates of bishops and soldiers were not generally the same: bishops presumably resided in more comfortable and sanitary conditions than soldiers in barracks, they could escape epidemics more easily, and they must have had a much higher overall higher standard of living. When the series overlap, the Gutierrez mortality estimates are lower than the Curtin estimates.

For these reasons it is essential to benchmark the mortality rates of bishops to those of soldiers.[21] Gutierrez (1986) provided an estimate for Mexico (for which we had a Curtin estimate) and also for the Dominican Republic, which we assumed had a similar mortality rates to Jamaica (again, for which there is a Curtin estimate). Since we had two points of overlap, we could benchmark using either number, or some combination of the numbers. We decided to use the Mexican number, which was lower and therefore reduced the mortality rates in Latin America.[22] In the NBER working paper version we discussed this issue and reported that our results were robust using either type of benchmarking. We show the relevant regressions in Section 3 below. Section 5 provides some new evidence, comparing soldiers' mortality with officers' and civilians' mortality rates, that supports our approach to merging the two series.

Our objective in AJR was to construct a series of potential European settler mortality rates in the most transparent fashion from Curtin's work. We tried to avoid making judgment calls about which other sources are reliable. The only exception to this was the use of bishops' mortality rates from Gutierrez (1986). Since Albouy selects evidence from various secondary sources, issues of consistency and selectivity become particularly important, and we document below that this is a major source of problems for his series.

We turn now to the results from using the original AJR series and some reasonable alternatives, and then compare these with Albouy's results.

---

[19] There was a reference on p.2 of Curtin (1989) to an English attack on Cartegena in 1742. But the Gutierrez data for Colombia are for Bogota, and there is good reason to think this was not as unhealthy for Europeans as the Caribbean coast, so Curtin's information on Cartegena did not help us merge the Gutierrez and Curtin series.

[20] Specifically, we used data on bishops aged 40-49. Many of these bishops were born in Europe, so they would not have an acquired or inherited immunity to local diseases.

[21] Benchmarking here means merging the two series by using Gutierrez's relative mortality rates for bishops to impute mortality levels that are consistent with Curtin's data. This lets us calculate levels for Latin America. Obviously it is only appealing to benchmark using a country that is present in both datasets.

[22] This choice seemed less favorable to our hypothesis. Our checks using the Dominican Republic/Jamaica number indicated stronger results for us. Also using rates from Mexico in benchmarking the Gutierrez/Curtin series does not involve any assignment of mortality to neighbors. See Section 3 below for results using the alternative series constructed with benchmarking for the Domincan Republic/Jamaica.

3 Main Results

3.1 First Stage

The first stage relationship in AJR is the link between settler mortality, in logs, and a measure of institutions. Here we focus on our main measure of institutions, which is protection against the risk of expropriation. This is an OLS regression, with one observation per country.

For the sake of succinctness, Table 1 is structured to show results only for the log mortality variable. Each set of rows shows a different specification, with covariates and alternative samples that were presented in our *AER* article. The first set of rows has no additional covariates in the regression, the second set of rows includes latitude, the third set of row drops the neo-Europes (the USA, Canada, Australia, and New Zealand), the fourth set of rows drops all of Africa, the fifth set of rows includes continent dummies, the sixth set of rows includes the percent of the population in 1975 that was of European descent, and the seventh set of rows includes malaria (see AJR for the reasons to focus on these specifications). These are the specifications which Albouy also discusses.[23]

For each set of rows we show five numbers: the coefficient on log settler mortality, the homoscedastic standard error, the clustered standard error, the number of clusters, and the number of observations. The number of clusters is less than the number of observations because about half the estimates in our base *AER* sample are inferred from mortality rates in neighboring countries.

Albouy constructs two data series: a "barracks rate" and a "campaign rate". The barracks rate is Albouy's attempt to improve the measurement of potential European settler mortality. As already mentioned above, we do not believe that the campaign rate is useful to gauge the mortality rates faced by potential European settlers – Albouy's series comprises non-comparable events, ranging from small expeditions (a few hundred Europeans) to large scale conflict (e.g., the U.S. Civil War). Nevertheless, we examine his campaign series in our analysis for completeness and because it sheds light on some of the problems with his barracks series.

We next show that the only reason that either of Albouy's series give substantially different results from the ones we presented in AJR is because of a sequence of odd and indefensible coding decisions. Once we correct for these problems, the results are very similar to those obtained with the original series in AJR. In other words, with these corrected series, there is no basis to any of the claims in Albouy's paper. We focus first on whether there is a significant first stage relationship, then turn to the second-stage estimates (with or without clustered standard errors) and weak instrument issues.

3.1.1 Barracks Series

Table 1 begins with first stage results using the original AJR data (column 1) and an alternative AJR series (column 2). The results in column 1 are those of our original *AER* paper, and they are included here just for the sake of comparison. The coefficient is -0.61 and the standard error is 0.13; when we cluster the standard error, it rises to 0.17 and the coefficient remains highly significant.[24]

---

[23]Albouy also has some specifications with a temperature variable, but the source for this variable is not clear. He claims the variable is one of several temperature and humidity controls in AJR, but this does not appear to be the case. We are therefore unable to evaluate his claims in this regard and stand by the original results in AJR.

[24]In the original AJR series, we used the relative rates of 1, 1.1, and 2.3 between the Gutierrez regions. This was based on an approximate formula that converted Gutierrez's mortality rates into "with replacement"

The alternative AJR series in the second column was discussed in our *AER* article, but we did not have room there to report the results. In this series, we try an alternative way of linking the Curtin and Gutierrez datasets. Specifically, instead of assuming that Mexico is the common observation, we assume that Jamaica/Dominican Republic is the common observation.[25] In this case, the results are almost identical in all specifications to those in column 1; without Africa, we actually get a larger coefficient on log settler mortality (while the clustered standard error increases slightly from 0.18 to 0.21).[26] We continue to assign countries to mortality regions as in our *AER* article.[27]

Column 3 shows results using Albouy's barracks rate series. When there are no covariates, log settler mortality is still significant, with about the same coefficient as column 1, although with a standard error that is much larger (0.30 vs. 0.13), and a t-statistic of just under 2. As soon as we add latitude, log settler mortality becomes insignificant. It is also insignificant in all other specifications of this column, except for the set of rows without Africa. This is noteworthy since, as we will discuss below, Albouy's procedures and suggested numbers for Africa are most questionable.

One major problem of Albouy's barracks series is immediately apparent. Instead of the 64 countries in our base sample and in Albouy's campaign series, there are only 43 countries in his barracks series. This is because Albouy argues there are no data either directly or for a relevant neighbor for the remaining 21 countries, most of which are African (see his p. A2 on Africa).[28]

It turns out Albouy's results with this series are due to how he assigns mortality to neighbors within West Africa. Specifically, Albouy recognizes two valid barracks observations for West Africa: Senegal and the Gold Coast (Ghana). He then assigns the rates from these two countries to one (each) of their neighbors, but does not assign to all their neighbors.[29] The rate for Senegal is assigned to Gambia and the rate for Ghana is assigned to Cote d'Ivoire.[30] This partial assignment is completely arbitrary. Why should mortality rates be assigned to only two neighbors, rather than to all neighbors where Europeans apparently faced similar risk of death?[31] The consequences of not making these assignments or making the full set of

mortality rates that were comparable with the base data from Curtin. In Appendix 2 we show that the precise ratios should be 1, 1.1, and 2.2. This does not make any difference to our results in column 1.

[25] In the original AJR series, we assumed that the mortality rate in the Dominican Republic was the same as in Jamaica, and Albouy does not take issue with this point. Using the Mexico estimates as the benchmark implies mortality (per 1,000 p.a.) of 71 (low), 78.1 (medium), and 163.3 (high) in Gutierrez's three Latin American mortality regions (these numbers are used in the data series of column 1). If we use the Jamaica/Dominican Republic estimates, this gives rates for the three regions of 56.5 (low), 62.2 (medium), and 130 (high); these numbers are used in the data series of column 2. As in AJR we use the relative mortality ratios of 1, 1.1, and 2.3 between Gutierrez's three regions (see footnote 24).

[26] The number of clusters falls by 2. In the original AJR series, Argentina and Chile's estimates were based on naval stations. In the revised series they are derived just from bishops' mortality zones. Also, the mortality rate for the high-mortality zone is now, by assumption, the same as Jamaica/Dominican Republic.

[27] Albouy suggests we made a mistake in assigning bishops' mortality from Gutierrez to various countries in Latin America. In Appendix 1, we show that our assignment was correct and Albouy's assignment is wrong.

[28] Vietnam and Hong Kong are also coded as missing in his barracks series.

[29] Specifically, he assigns data from Senegal (actually the Senegalese islands of Saint Louis and Gorée) to Gambia (p. A2). He also assigns data from the Gold Coast to Cote d'Ivoire (p. A2).

[30] For other parts of the world, Albouy frequently assigns data to neighbors in his barracks series, including Tunisia (assigned the barracks rate of Malta, p.5), Morocco (Gibraltar, p.5), Bahamas, Haiti, and Dominican Republic (Jamaica, p. A3), and Guyana (French Guyana, p.A3). In his campaign rate he follows a similar pattern of assignment.

[31] Albouy offers no evidence that the death rates from disease for Europeans were very different among these neighbors. In fact, the rates for West African countries in his campaign series are quite similar (most are in the

assignments (i.e., to all neighbors) are dramatic.[32]

Column 4 drops the observations for Gambia and Cote d'Ivoire, i.e., we use Albouy's barracks series, but do not assign the Senegal and Ghana rates to any of their neighbors. The number of observations falls from 43 to 41 and the results change radically. In the first row, now the point estimate is -0.80, with a t-statistic of over 2.5. In the rows with latitude, for example, log settler mortality is again significant, with a coefficient of 0.62 and a clustered standard error of 0.33. There is a similarly large change to significance in the rows with continent dummies and with malaria.

Column 5 assigns the mortality rates of Senegal and the Gold Coast (using the numbers from Albouy's barracks series) to all their neighbors (countries with which they share borders), rather than to just two of these neighbors. Specifically, Albouy's Senegal rate (72 per 1,000 p.a.) is assigned to Gambia, Guinea and Mali, while the Gold Coast/Ghana rate (151 per 1,000 p.a.)[33] is assigned to Cote d'Ivoire, Sierra Leone, Togo, Burkina Faso, and Nigeria.[34] The number of observations rises from 43 in column 3 to 49. With this modification, the results again become remarkably similar to those of the AJR base sample in column 1 and quite different from those of Albouy's barracks series in column 4. The coefficient on log settler mortality is significant in almost all rows, with the exception of the specification that includes percent of European descent.[35]

The second major problem with Albouy's barracks series is his use of bishops' mortality rates. In his campaign series, Albouy benchmarks the Gutierrez and Curtin series (i.e., assumes an adjustment is needed to make them comparable), but in his barracks series he assumes that soldiers' mortality is directly comparable with bishops' mortality, so they can be included in the same series without any adjustment. This has no justification and is contradicted by available evidence, see Section 5.[36] He compounds the problem by using bishops' mortality rates without replacement, whereas the other numbers in his barracks series are with replacement (per 1,000 mean strength).[37] In this column we reinstate benchmarking, using the rate of 130 for Jamaica

---

range 350-560).

[32]It is also notable that one of the two countries for which the assignment is made is Gambia, which was already flagged as an outlier in our *AER* paper (mostly because of the particular baseline institutions variable used in AJR).

[33]The rate of 151 in the electronic version of Albouy's dataset is based on a mortality rate of 151.45 on his p. A2 (i.e., there is a rounding error).

[34]Nigeria does not border Ghana, but the Gold Coast included part of Togo, and this is very close to Nigeria (in fact, for his campaign rate, Albouy uses a rate from close by Nigeria – Benin in his case, p. A1.) Mauritania, Guinea-Bissau, and Benin were not included in the original AJR series as they were missing other data. We leave Niger out as it does not border Senegal and is quite far from the Gold Coast. Assigning the Gold Coast rate to Niger leads to similar results.

[35]Note that the point estimate in the row *without* Africa in column 5 is not the same as in columns 3 and 4, even though our adjustment is just for Africa. In column 5 we adjust Albouy's level of mortality measure, then take the logs. In columns 3 and 4 we use Albouy's log mortality measure (for the same barracks series). Apparently in his log series Albouy has an estimate for Indonesia that does not match what is in his level series (this is all using the dataset on his website in December 2004).

[36]Albouy (2004b) writes, p.11, "For the barracks rate the bishop rates are applied directly, on the assumption that the higher mortality rates of older bishops to younger soldiers is offset by the better living conditions of bishops, relative to soldiers in barracks..." He applies this reasoning to his barracks series, but not to his campaign series, which benchmarks the Curtin and Gutierrez data using his estimate for Mexico (i.e., the same procedure for AJR, although he has a slightly lower estimate for Mexico.) It is not clear why the barracks and campaign series are coded in this mutually inconsistent fashion.

[37]A few of our very high mortality estimates for Africa are not with replacement – we took them as we found them in Curtin. If we adjusted those appropriately, they would be even higher.

which, as we show below, is without a doubt a peacetime rate.[38]

Column 6 shows that even without any other changes in Albouy's data, this American correction has a major effect on all rows. For example, in the specification without any covariates, the coefficient estimate is -0.98 with a t-statistic of around 4 with or without clustering. Log settler mortality is highly significant in all specifications of column 6, except the one without the neo-Europes – and even there it is significant at the 10 per cent level. In all cases, the coefficients are larger than in the corresponding specification of column 1. Clustering the standard errors makes very little difference.

Column 7 adds the American correction of column 6 to the African correction of column 5 (i.e., assignment of Albouy's rates for Senegal and Ghana to all their neighbors). Again the results for log settler mortality are strong, and overall quite similar to the baseline AJR estimates in column 1. The t-statistics are consistently high – around 4 with latitude as a covariate, for example, and over 6 without Africa (and clustered standard errors).

None of the changes in this table address the fact that Albouy's barracks estimates for West Africa are very low – in fact, they are approximately the same as his estimates for the West Indies, East Indies, and South Asia, which contradicts a large body of existing evidence (see Section 4). We review these estimates further in Section 4 below, and find them to be implausibly low due to Albouy's selection criteria.

Throughout Table 1A clustering makes very little difference (as we claimed in AJR), particularly once we consistently assign mortality rates across neighbors in West Africa. In fact, in a number of cases the t-statistic for the effect of settler mortality on this measure of institutions is higher with clustering.

3.1.2 Campaign Series

We would first like to reiterate that we are dubious about the information content of Albouy's campaign series regarding the mortality rates facing potential European settlers. The main issue is that this series combines data from small expeditions and large-scale continent-wide wars, likely introducing a considerable amount of non-classical measurement error (a justification for such a campaign series is not offered in Albouy's paper). Nevertheless, we discuss Albouy's campaign series in detail to highlight that a large part of his results are due to inconsistencies in his data (e.g., between his barracks and campaign data).

Table 1B again begins with results using the original AJR data (column 1). These are reproduced from column 1 of Table 1A to make comparisons easier.

We used a few expeditions where there were no peacetime observations in Curtin's data. Most of these expeditions were small-scale, well fed, faced little enemy fire, and involved primarily travel by boat or mule. Albouy, in contrast, claims to seek out data from larger conflicts – he prefers observations with "significant" forces involved (p. A1) (although he is not consistent on the size of the force, e.g., for parts of West Africa he uses force sizes with just a few hundred Europeans). This obviously biases his data for tropical places towards episodes when military mortality had been brought down.

One rationale for Albouy's campaign series is that the AJR data contain expedition observations that are too high, i.e., unrepresentative of what he regards as normal rates, because the

---

[38]Using the ratios of 1, 1.1, and 2.2, the rates for Gutierrez's three mortality regions are therefore: 130 (high), 65 (medium), and 59.1 (low). We also use the rate of 130 for Jamaica, Haiti and the Dominican Republic, and 85 for Trinidad and Tobago – these are the original rates from AJR. Albouy prefers to assign the Jamaica rate to the Bahamas (rather than the Barbados rate), so we assume mortality of 130 in the Bahamas.

expedition occurred during an epidemic. He argues that the expedition itself led to unusually high death rates, i.e., the epidemic would not have happened were it not for the fact that soldiers were marching about (i.e., in the barracks they would somehow have been immune. We contest the thinking behind this point below in our detailed review of his African estimates. Before we get to that, in column 2 of Table 1B we offer the most transparent way of dealing with this potential issue – capping mortality rates. In effect, we limit the extent to which being on an expedition could influence the mortality rate. The capping also affects some obvious peacetime rates, thereby addressing Albouy's concern that we are using information from any kind of abnormal epidemic.

Specifically, we set the maximum mortality rate at 250 per 1,000 per annum. This is the rate Tulloch found to be typical in West Africa in the early nineteenth century (Curtin, 1990, p. 67).[39] As far as we know, from Tulloch and Curtin, West Africa was the highest mortality region for Europeans, so taking this cap for all of Africa seems reasonable. We have found very similar results using other rates that are generally considered by Albouy not to be abnormal (see column 6 of Table 1B).

In column 2, for the base specification of the first set of rows, the coefficient on log settler mortality increases in absolute value to -0.94, while the clustered standard error increases from 0.17 (in column 1) to 0.18. There is a similar pattern in all other rows, except the row without Africa (as the capping only affects African rates).[40]

Column 3 begins with his campaign series. Log settler mortality is now much less significant in almost all specifications. For example, in row 1, with clustering, the t-statistic is already below 2. It is striking, however, that without Africa, the results look very similar to the basic AJR specification of column 1. The difference between our estimates lies primarily in how Albouy treats African data.[41]

A basic issue with his campaign series is that Albouy selectively uses weighted and un-weighted averages when he takes rates across campaigns.[42] In his use of the Curtin data for his campaign series, Albouy uses unweighted averages in almost all cases, i.e., he simply averages across mortality rates in different campaigns, or phases of campaigns, without regard to the number of soldiers present at each point (this is what Curtin does when he averages across years or campaigns, so this is what Albouy is implicitly endorsing when he uses Curtin's data).[43] For Vietnam, where it would be possible to calculate weighted averages from data in Reynaud (1898), which Albouy cites, he chooses to use an unweighted average. However, in one case, Sudan, Albouy constructs a weighted average over three separate estimates (Albouy, 2004b, p.7), where the weights are the number of soldiers present at each phase. In addition, Albouy claims to construct a form of weighted average also for Mexico, based on Reynaud.

---

[39]It is also close to the rate of 209 per 1,000 p.a. for officers stationed in Sierra Leone and Cape Coast Command, 1819-36, on p.37 in Balfour (1849); ordinary soldiers had a much higher death rate. See Section 5 for more details.

[40]Note that a few of the highest mortality rates in the original AJR were used in the raw form reported in Curtin and are not "with replacement" rates. Capping mortality rates means that this definitely does not matter – with or without replacement, these rates would be above the level of the cap.

[41]One of AJR's basic specifications was without African data (columns 5 and 6 in AJR's Table 4, which was our first table with IV results.) Despite our having pointed this out to him in July 2004, Albouy still does not fully acknowledge these robustness checks.

[42]He is also not consistent on the number of campaigns over which he averages, but we will come to that point in our detailed section below.

[43]Note that the Tulloch data that underlie Curtin's base data, as used by AJR, are usually weighted averages. But in his campaign series, Albouy makes little uses of these data.

However, he does not provide the relevant details and we have not been able to reconstruct his calculation.

This procedure is likely to create biases because sensible generals will keep more soldiers on the ground when mortality is lower (e.g., during a more healthy season or after some obvious health problems have been overcome); soldiers can also be withdrawn if an epidemic seems imminent. If weighted averages could be computed consistently for campaigns, they would be worth looking at (although the endogeneity of force strength remains a big issue), but this is not possible for Albouy's campaigns given the available data. In all our revised campaign series, therefore, we recalculate Albouy's estimates as unweighted averages.[44]

For the Americas, there are two specific issues with the campaign series. Albouy assumes that the USA and Canada settler mortality rate is best proxied by deaths from disease from the US Civil War. This seems to have no justification. To start with, this war was a major conflict rather than a minor campaign or expedition; we take this up further in Section 4 below. If one really wanted to create a comparable campaign series, a more obvious and reasonable proxy would be the experience of British soldiers campaigning in the North America during the American War of Independence. A standard reference estimates a mortality rate from disease of 26 per 1,000 p.a. (Cantlie, 1974, p.156).[45]

Another difficulty that has significant implications for Albouy's campaign series is that he incorrectly assumes the data in Curtin for the Caribbean are wartime rates, even though these start in 1817 (the war in question is presumably the Napoleonic War, which is generally considered to have ended in 1815).[46] In fact, Alexander Tulloch (Curtin's source on this subject) explicitly calculated a wartime rate for the West Indies, 1796-1807, which is 244 per 1,000 p.a. (see Kiple, 1984, p.170). We use this corrected campaign rate for all the Caribbean islands.[47]

Column 4 reports results from implementing just these three changes (unweighted averages and the two Americas corrections). In the base specification, the coefficient on log settler mortality increases in absolute size and the standard error falls. Now the t-statistic is above 3 without clustering and above 2.5 with clustering. There are similar changes in many of the other specifications.

Column 5 adds a different set of corrections, mainly to the African data, which, as noted above, seem to be responsible for most of the discrepancy between our results and those of

---

[44]Curtin indicates that one of the three estimates for Sudan is probably bad data. We therefore disregard this in recalculating the Sudan unweighted mortality estimate as 49.55 per 1,000 p.a. In this calculation, we average the following rates from Curtin (1998) of 10.9 (p. 169) and 88.2 (p.169); we are following Albouy (p.7) in treating these as campaign rates. We have experimented with using Albouy's original Sudan coding and with other ways of constructing the average, and the results in column 7 of Tables 1B and 2B are similar, though somewhat less precise. For example, with Albouy's campaign estimate for Sudan of 13.87, with the remainder of the data as in column 7 of Table 1B, the point estimate falls in absolute value to -0.64, with a clustered standard error of 0.25 and a t-statistic of 2.6.

[45]Cantlie (1974) describes mortality from disease during the American War of Independence as follows: "....the average strength of the British forces was 39,196... for the period of six years. The deaths for the six years 1775-1780 amounted to 6,107 which gives an average rate of 1,018 or 2.6 per cent." (p.156). This is a rate of 26 per thousand per annum, and because it is based on mean strength (obviously with replacement), it is directly comparable with Curtin's estimates. However, compared with other datapoints in Albouy's campaign series, this is still a large expedition with a great deal of fighting.

[46]Albouy might have in mind the War of 1812, but this concluded in January 1815 with the Battle of New Orleans (and was formally ended in December 1814 by the Treaty of Ghent).

[47]Compared with the Albouy campaign series, this revision results in the loss of two clusters, as there were previously three separate rates for Caribbean islands.

Albouy. A major source of difference is that contrary to his practice with the barracks series, Albouy adds data that do not come from campaigns – in contrast, in the barracks series, he codes a country as missing if there are not what he considers to be appropriate data. As a first correction, in our column 5 revision, we follow his barracks principle and code as missing any countries where there are not – according to Albouy – actual campaign data (either directly or in neighbors). This drops most of central Africa, as well as Guyana.

Second, there are extensive data on mortality in West Africa that Albouy ignores. These are early data from various kinds of expeditions. According to Albouy's own reasoning, these data should be considered to be the outcomes of campaigns – people arrived with guns and either moved about or attempted to establish a base.[48] We therefore add these early mortality from exploration and expeditions in West Africa.[49] We also add other data from expeditions in Africa.[50]

Column 5 presents results for Albouy's campaign series with these corrections (unweighted averages, countries with no available data coded as missing, and expedition data). The results are quite similar to those in column 4 and to the AJR estimates in column 1. For example, without any covariates, the point estimate is -0.67 with a t-statistic of over 3. There is just one relatively weak result – in the specification that includes continent dummies.

Since Albouy is concerned about excessively high mortality rates from small expeditions, in column 6, we cap the campaign mortality rates in column 5 at 562 per 1,000 p.a. This is the highest rate that Albouy regards as representing potentially normal campaign mortality in the West African context (he uses it for Togo in his campaign series). In this case, the log settler mortality results are even stronger than in column 5.

In column 7 we modify the campaign series to reflect both the African corrections (with capping, from column 6) and the American corrections (from column 4). In this column the results are actually stronger than in the AJR base series. In all cases log settler mortality is strongly significant.[51] The t-statistic is around 4 without Africa and 2.48 (with clustering) when latitude is a covariate.

As with Table 1A, in Table 1B clustering makes very little difference once we correct Albouy's data (compare column 7, for example, with column 3).

3.2 Second Stages

Tables 2A and 2B report the results of second stage regressions, where the corresponding first stages are in Tables 1A and 1B. The second stage, to be clear, is an instrumental variables

---

[48]Nevertheless, we argue below that these early expeditions data could reasonably be considered to represent actual settler mortality rates, i.e., these are among the closest available estimates for what AJR were seeking to measure. These are essentially peacetime experiences. We include them in Albouy's campaign series because he includes similar expeditions in constructing the campaign series.

[49]Specifically, we use mortality rates, per 1,000 p.a., of 668 for the Ghana/Gold Coast from Curtin (1998, p.18; also in Feinberg 1974, p. 358), we assign this to neighboring Togo and Cote d'Ivoire also; 483 for Sierra Leone from Curtin (1989, Table 1.1); 250 for Senegal from Curtin (1990, p.67, this is an average of 220 for Saint Louis and 279 for Gorée, rounded up to the nearest whole figure), we assign this also to Guinea; and 718 for Gambia from Davies (1975, pp.91-92, from an expedition in 1721; note that the Gambia expedition of 1705 had lower mortality – 47 percent in the first year, without replacement, p.92 of Davies). For Nigeria we use the rate of 773 for Ouidah (in modern Benin) from Davies (1975, pp.90-91).

[50]Specifically, a rate of 870 from Mungo Park's second expedition, for Mali and Burkina Faso (Curtin, 1964, p.484), a rate of 830 from Laird's Niger expedition (Curtin, 1964, p.485), and 370 from Tuckey's expedition to the Congo river for Congo and Zaire (Curtin, 1964, p.484). These rates are without replacement.

[51]In this case, settler mortality is strongly significant even when we include the percent of population in European descent in 1975.

15

regression of log GDP per capita in 1995 on protection against risk of expropriation (both measures were the base in AJR), where the right-hand side variable is instrumented with log settler mortality.

These results further confirm the problems of the second stage estimates using Albouy's barracks and campaign series, but also show that our adjustments to these series lead to 2SLS estimates very similar to those in AJR. Specifically, in the barracks series once we require consistent coding of neighbors in West Africa, in almost all specifications, the broad measure of institutions is statistically significant (Table 2A). Moreover, once we incorporate all of the corrections in column 7 of Table 1A (which corresponds to column 4 in Table 2A), the coefficient estimates are similar to those in column 1, though typically slightly lower and with smaller standard errors. These are remarkable results given the fact that we are only using about two-thirds of the original sample.

In each set of rows, the second row is the 95% AR confidence interval, the third row is the AR confidence interval with clustering, the fourth row is the F-statistic from the first stage and the fifth row is the F-statistic from the first stage with clustering. The first two columns which use the original and alternative AJR series show that there is no major weak instruments problem. In most cases, the first-stage F-statistic is reasonably large, and the AR confidence interval always excludes zero, and in fact, it excludes estimates that are less than 0.3.

On the other hand, the weak instruments problem is apparent in column 3, where we use Albouy's barracks series: in all specifications, other than that without Africa, the F-statistic is small and the confidence interval includes potential negative effects (although it excludes a zero effect).

Interestingly, once the few corrections mentioned above are incorporated, the corrected Albouy barracks series (column 4) also shows no evidence of weak instruments. The F-statistics are comparable to those in the original AJR series, and in some cases in fact higher. With the exception of the specification without neo-Europes, the F-statistic is greater than 9, indicating that there is no weak instrument problem (e.g., see Stock, Wright and Yogo, 2002, Table 1 for suggested critical values). More importantly, in all cases, the AR confidence interval excludes coefficients less than 0.3.

In the campaign series, with the combined African and American adjustments, the effect of institutions on income per capita is actually stronger than in our original AJR work in all specifications (compare columns 1 and 4 in Table 2B). Nevertheless, unlike the results with the corrected barracks series, the F-statistic for the first stage with clustering in the revised campaign series is still lower than for the original AJR series.

As a reflection of this, the second stage estimates are typically less precise. While in the basic specification with no covariates the AR confidence intervals are relatively narrow and comfortably exclude zero, they become wider when we add covariates. Interestingly, in all cases, except in the specification with malaria and clustering, they exclude zero. This implies that, for those starting with the prior that institutions do not have a very large negative effect on income per capita, they provide evidence of positive effects of institutions on income per capita.[52] It is also noteworthy that without Africa, the confidence intervals are much narrower

---

[52]See Chernozukov and Hansen (2005) for a discussion of how to construct and interpret AR confidence intervals consisting of two non-overlapping intervals. Chernozukov and Hansen argue that one should only consider plausible values of the parameter before constructing the AR confidence intervals. In the context of our paper, which they use as an example, they look at coefficient values between -1 and 4. So according to Chernozukov and Hansen's procedure, the component of the confidence interval lying in the the negative real numbers is irrelevant.

and exclude values below 0.36 for all columns (including Albouy's series).

To reiterate, these results using the corrected Albouy series only consider a few specific changes, and they do not correct for the large number of other problems in Albouy's revisions, which we turn to next. Nevertheless, these results are useful for showing that imposing even a minimal level of consistency leads to findings very similar to those in the original AJR paper.

4 Detailed Analysis of Albouy's Revisions

We organize our discussion of Albouy's suggested revisions by region, as there are typically common issues in each region.

4.1 West Africa[53]

4.1.1 AJR's Series

Our West African estimates were all from Curtin. From Curtin (1989, Table 1.1) we took data on early soldiers in Sierra Leone and Senegal, and from Curtin (1998) we used data on soldiers and small expeditions somewhat later in the nineteenth century (for Gambia,[54] Gold Coast/Ghana,[55] Mali/French Soudan,[56] Nigeria[57]). Specifically, we took data from expeditions with a few hundred soldiers on short West African expeditions (travelling on steamers or on mules); these were essentially peacetime experiences, with reported deaths almost all from disease. Curtin (e.g., 1990) emphasized an important downward bias from using data later in the nineteenth century, as militaries became better at managing mortality during short expeditions during the nineteenth century, so we stayed away (as much as possible) from estimates after 1850.[58] In the robustness checks of our NBER working paper, however, we did check our results using longer averages of African data; our main results were unchanged.[59]

We assigned mortality rates to countries that were part of the same colonial area or neighbors in the cases of Niger (from Haut-Senegal-Niger), Burkina Faso (from French Soudan), Guinea (from Sierra Leone), Cote d'Ivoire and Togo (from Gold Coast/Ghana).[60]

---

[53]There are 11 West African countries in AJR: Burkina, Cote d'Ivoire, Ghana, Guinea, Gambia, Mali, Niger, Nigeria, Senegal, Sierra Leone, and Togo. All are in Albouy's campaign series, but only 4 are in his barracks series.

[54]The Gambia data are not from a campaign, as claimed by Albouy, but rather "soldiers on the Gambia in 1825" (Curtin, 1998, p.10). "In this case, between May 1825 and December 1826, fevers killed 279 British soldiers out of a force that was seldom more than 120 and often as low as 40." For confirmation this was a peacetime experience, see the original source, Army Medical Department (1840, p.13).

[55]The Ghana (partly the Cape Coast Command) estimate of 668 was for troops 1823-26 and officers 1819-36. The original number is in Army Medical Department (1840, p.19). For confirmation, see Balfour (1849, p.38).

[56]The Mali expedition (specifically to Logo in 1878) included 434 Europeans and 225 Africans, travelling by steamer (with a march of 10 miles at the end); 49 percent of the Europeans died in less than two months (Curtin, 1998, pp.80-81). In campaigns in the French Soudan, under the direction of General Gallieni, soldiers rode on mules (Reynaud, 1898, p.150).

[57]The Nigeria expedition in 1841 had 159 Europeans on three steamers; "the longest time any of the steamers spent on the river that year was just over two months" (Curtin, 1998, p.21). The Ghana rates were from a longer intervention, 1824-26 (Curtin, 1998, p.18).

[58]"A mortality revolution had nevertheless taken place during the nineteenth century in tropical Africa as it had in Europe" (Curtin, 1990, p.69). From Army Medical Department (1840, e.g., p.22) it is clear that the early mortality estimates for West Africa are underestimates as they do not include deaths of soldiers once they had been "invalided" home.

[59]It is unfortunate that, despite an explicit private communication on this point, Albouy refuses to acknowledge (e.g., p.7) that we used long averages for the African data in the robustness checks of Table 5 in our NBER working paper version.

[60]Albouy complains (p.9) that we assign a rate of 400 (Curtin, 1998, p.85) from Mali to Niger, but this is

Albouy is concerned about our assignment of the estimate of 280 from Curtin (1998), p.238, Table A8.1, for "French Soudan." The term French Soudan is ambiguous, as Albouy points out. As far as we know from Curtin and Reynaud, these were minor campaigns, with little fighting, mostly in present day Mali. Alternative assignments to Mali and its neighbors (e.g., assigning our original Mali estimate to neighbors, or using the estimate of 400 per 1,000 on p.85 of Curtin, 1989) make little difference to our results.[61]

4.1.2 Albouy's Barracks Series

For his barracks series for West Africa, Albouy claims there are only two valid data points: Senegal (from 1852-73) and the Gold Coast (1859-75).[62] He applies the rate for Senegal to Gambia (only) and the Gold Coast to Ghana and Cote d'Ivoire (only), a procedure that we regard as indefensible and assessed in Section 3 above. There are three further problems with Albouy's selection of these numbers for his barracks series.

First, he rejects our estimates for Senegal and Sierra Leone (both from Curtin, 1989, Table 1.1) with no good reason.[63] Albouy justifies not using the Sierra Leone rate as the basis for his barracks rate because these (and other) rates "belong to soldiers subject to frequent campaigns (Curtin, 1989, p.18 and Feinberg, 1974)". But neither author makes any such claim, and this is not what is in the key original source (Army Medical Department 1840).[64] No reason is given for rejecting our Senegal estimate (and using it only for the campaign series).

Second, the barracks rates Albouy chooses are from quite late in the nineteenth century, and they are strikingly low given the experience of potential settlers and the assessment of their medical advisers over the preceding decades.[65] By the 1780s, for example,

> "it was known in any case that West Africa was much more dangerous than the West Indies. The best medical opinion was, indeed, opposed to the kind of establishments that already existed there. Lind argued that European garrisons for the West African posts should be reduced to the smallest possible numbers and moved to hulks anchored off shore" (Curtin, 1964, p.86).[66]

---

assignment to a neighbor. Elsewhere in his series, including for Africa, he is willing to do the same.

[61]Our original Mali estimate was very high, so we were reluctant to use this for all neighbors. But using this would be a reasonable robustness check. Assigning the rate of 2920 to Niger, Burkina Faso, and Cameroon (the last not a neighbor, but close and a neighbor of Nigeria, which has a similarly high rate in our base data), gives a parameter estimate of -0.54, with a clustered standard error of 0.13, without other covariates. If we assign 400 to Mali, Burkina Faso and Cameroon (Niger is already at 400 in our base data), the coefficient is -0.62 and the standard error is 0.18. In the first case, the coefficient falls slightly in absolute value, but the standard error also declines, and in second case there is almost no change (compare with column 1, Table 1A). There is a similar pattern in other specifications.

[62]In addition, Albouy says that he uses the first 10 years of peacetime data when available (p.7), but for the Gold Coast he uses 17 years (1859-1875). If he had used just the first 10 years, the rate would have been higher (176.51 vs. the rate of 151.45 that Albouy uses; see Curtin, 1998, p.70, Table 3.2).

[63]In his Table 4, column 1, Albouy claims to find that including a dummy for campaigns eliminates the effect of log settler mortality in AJR's first stage, using AJR's own data. But this turns out to be sensitive to his coding of Sierra Leone and Senegal. Both are peacetime rates from Curtin (1989, Table 1.1) and they are quite consistent with other peacetime data from the same period, but Albouy codes them as campaigns. If this coding is corrected, then the coefficient on log mortality becomes –0.40 (clustered s.e. 0.17).

[64]Curtin says, (1989, p. 18), "The Sierra Leone rate of more than 400 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." This is *not* saying that the Sierra Leone estimate was from a campaign.

[65]Curtin (1990) documents in detail how rapidly mortality for Europeans declined after 1850.

[66]The first edition of Lind's *Diseases in Hot Countries* was published in 1768.

Albouy's selections for West Africa, which are very close to his estimates for parts of the Caribbean (see footnote 1 above), are almost certainly serious underestimates of early settler mortality.

Third, Albouy explicitly rejects any observations with high mortality (although we showed in Section 3 above that our results are not driven by outliers). This rules out our data for Mali and Nigeria, which are drawn from Europeans travelling in relative comfort on steamers.[67]

To explain this selection procedure, Albouy argues that ignoring data from epidemics represents an improvement. This is a strange justification given the frequency of epidemics. Periodic yellow fever epidemics were not only typical – they definitely deterred settlement (the key point for testing our theory). For example, Curtin notes:

> "Sierra Leone's reputation was given a final turn downward in 1829 by the appearance of a disastrous yellow fever epidemic, which came just in time to influence the Parliamentary Committee of 1830. There would be no extension of the British sphere of influence in West Africa" (Curtin, 1964, p. 197).

Yellow fever was a long standing problem – Curtin (1975b, p. 5) says "Yellow fever epidemics attached the European posts in 1750, 1759, 1764, 1769, and 1778. The epidemic of 1778 killed more than 80 per cent of the Europeans on Gorée at the time."

We do not know how Albouy determined that the "true annual rate" does not include these deaths. The appendix in Curtin (1964) documents an impressive list of early and disastrous attempts by Europeans to spend time in West Africa.[68] The same book provides ample evidence that these mortality rates actually deterred European settlements.[69] Very high mortality rates were typical of early European experience with West Africa (e.g., see K. G. Davies, 1975).[70] West Africa was regarded as the "White Man's Graveyard" for good reason.

Furthermore, with regard to what should or should not be regarded as unusually high mortality, Albouy completely ignores an important point made by a source that he introduces – mortality in West Africa for early arrivals was systematically higher than the mortality for people who survived the first year. Albouy cites Feinberg (1974), but fails to mention that this source establishes that Dutch mortality during the eighteenth century in West Africa averaged 185 per thousand, with a high (when there were many new arrivals) of 408 per thousand. This is for people who did not campaign (Feinberg, 1974, pp.367-368).[71]

---

[67] On p.8, Albouy says that AJR's estimates for Gambia, Nigeria and Mali "are based on small campaigns of fewer than a few hundred soldiers over short periods during exceptionally bad epidemics, which Curtin explains were atypical." We have searched all the known publications by Curtin for such a statement, without finding it. Curtin does say these losses were high, but he also documents at great length many other instances of very high mortality (much higher than Albouy enters in his revised data).

[68] Each of these samples is quite small – in part because so many people died early on that others did not want to try. But the coverage of attempts to spend time on the ground – i.e., to actually settle – is quite comprehensive (across most of years from the end of the eighteenth century into the early nineteenth century, across civilians and military personnel, across different segments of society.) Death rates for new arrivals in their first year were often around 50% (i.e., half of them died within the first year). Repeated settlement attempts were defeated in large part because of these high mortality rates (see, for example, Curtin, 1964, Chapter 4).

[69] "And again, most Europeans who went to West Africa before 1830 did not, in fact, go voluntarily," (Curtin, 1964, p.181).

[70] Davies reports that of every 10 servants sent to West Africa by the Royal African Company, about 6 died during their first year in Africa (p.16). This estimate is cited favorably and fits with the broader evidence discussed in Curtin (1975a), on and around p.94.

[71] Albouy wants to use the correction to Tulloch's (and Curtin's) numbers for Sierra Leone and the Gold Coast that Feinberg (1974) proposes on p. 368. But Albouy does not apply this same correction to other data

Albouy argues for using only campaigns with a "substantial number of soldiers" (Section A.I, first paragraph), as he is concerned that small campaigns or expeditions may yield unrepresentative mortality estimates. Two main problems with this approach are evident for this region.

First, Albouy rejects our samples as too small, but then uses some data from comparable size forces in his own work.[72] For example, on p.A1, he says "The campaign along the Senegal River in the 1880's from Mali is the earliest campaign involving a substantial number of soldiers over a period of several years." But Curtin (1998 , p.84) says, "The number of French troops involved was tiny compared to the Magdala campaign, or even to the 2,000 Europeans and West Indians that marched to Kumasi in 1874... Through the 1880s, the total annual force of tirailleurs and marines in any campaign season varied between 400 and 700...". The tirailleurs were not European.

The second problem with this approach is that the number of soldiers on the ground may often be endogenous – the sensible politician or general will commit more soldiers during the healthy time of year or after important health problems with an area have been resolved. Albouy is correct that the number of soldiers on expeditions is smaller for the earlier period we use, but this itself is an outcome. Because the high mortality rates quickly became obvious after initial European contact, large forces were not sent to West Africa. Not surprisingly, Albouy's estimates based on this principle are all from quite late in the nineteenth century.[73]

Note that following Albouy's method, countries with very high mortality rates are not allowed to enter his dataset until mortality rates had fallen sharply.[74] Not only does his procedure move the estimates further from what would have faced early European settlers, it does so in a way that will produce lower estimates (relative to true mortality rates) in places with higher mortality.[75]

For example, in the case of Mali, Albouy uses 200.24 for the campaign rate. But this estimate is from the 1880s, i.e., well after medical technology for the tropics had improved.[76] The same problems arise with Albouy's revision for Nigeria, for which he prefers a campaign in Benin that occurred in 1892.[77]

from Tulloch so the effect is to make data in his series less comparable rather than more comparable. Feinberg proposes this correction in part to make the Tulloch data comparable with Dutch mortality on the Gold Coast in the eighteenth century – but Albouy does not use these numbers (which are higher than his barracks estimates for West Africa.)

[72]For example, p.7 of Army Medical Department (1840) shows that the death rate in the Sierra Leone Command was actually higher when there were more troops on the ground. Mean strength in 1825-27 was 571, 471, and 345, and the death rate was 783, 726 and 145 per 1,000 p.a. respectively. This sample size is larger than the number of Europeans present in some of Albouy's datapoints.

[73]For example, Albouy uses 1883-88 for Mali and 1892 for Nigeria (p. A1).

[74]This is a different version of the same problem that arises from his insistence on using weighted averages for some countries and not others – see our discussion of Sudan in the previous section and below.

[75]Albouy's campaign estimates suggest that many African countries were healthier for Europeans than most of Central America and the Caribbean. This is an amazing finding that is not, in our view, supported by the evidence.

[76]Consider again the epidemiology reviewed by AJR. Plasmodium falciparum was and is the plasmodial parasite in West Africa; it is often fatal in the first attack; and "the chance of an individual living [in West Africa] without receiving an infective mosquito bite is negligible" (Curtin, 1964, p.73). Yet Albouy's highest barracks rate for Africa is 151 per 1,000.

[77]Curtin (1989) makes it very clear, for example in Table 2, that by the early twentieth century mortality rates for Europeans in tropical countries were around 1/10 to 1/5 of what they had been in the middle of the nineteenth century.

It is remarkable that Albouy cites Cohen (1983) but does not mention a main point of this work – that the French military learned how to reduce mortality during its campaigns in Africa, making campaigns from the later nineteenth century not readily comparable with earlier data. This is a key point which we emphasized in AJR.

4.2 NORTH AND NORTH-EAST AFRICA[78]

4.2.1 AJR's Series

Our North Africa data are from Curtin. Curtin (1989, Table 1.1) has estimates for Algeria (1831-38).[79] From Curtin (1998) we have an estimate for Tunisia in 1881, for Egypt in 1882, for Sudan in 1885. The Algeria rate is assigned to Morocco. Our estimate for Ethiopia is from Curtin (1998), for 1867-68.

Albouy objects that we were not consistent in our coding with regard to two data points: Sudan and Egypt. For both countries, we took the first available peacetime number, which happened to follow a campaign. In both instances we were consistent with our coding rules, though as we noted in the introduction, we could have been clearer in our exposition of this, which perhaps helps to account for Albouy's confusion.

4.2.2 Albouy's Barracks Series

There are three problems with Albouy's barracks series for this region. First, Albouy drops some data that fits his own criteria for barracks. Specifically, there is no barracks rate coded in his data for Egypt, even though Albouy (p.7) recognizes the availability of such an estimate from Curtin.[80] Similarly, for the Sudan, he cites Curtin precisely on the point that the rate for the occupying force (after the military action) was 88.2. He explicitly refers to this number on p.7 but does not use it in his dataset. These were our Egypt and Sudan peacetime estimates.

Second, Albouy rejects mortality rates that are "too high" (just as with West Africa). Specifically, on p.5, footnote 5, Albouy says "the mortality rate of 63 of soldiers in the Tunisian campaign was exceptionally high because of a typhoid epidemic brought by soldiers from France (Curtin, 1998, p.152)." But Curtin does not suggest that the mortality was exceptionally high; in fact he suggests that typhoid had become a persistent problem in Tunisia. Also, on p.153, he points out that this particular typhoid outbreak may have originated in North Africa.

Third, Albouy assigns mortality rates between countries with no real justification. On p.5, Albouy proposes to use the mortality rates for Malta (16.3) and Gibraltar (21.4) for Tunisia and Morocco respectively as their "barrack rates."[81] He argues that these places were similar in terms of the diseases faced by Europeans. In particular, he writes on p.5 that for North Africa "according to Curtin (1989, p.17) mortality rates are similar to Southern Europe in

---

[78]There are 6 North and North-East African countries in the AJR data: Morocco, Algeria, Tunisia, Egypt, Sudan, and Ethiopia. All are in Albouy's campaign data, but only 3 are in his barracks data: Morocco, Algeria, and Tunisia.

[79]Tulloch (1847, p. 259) puts the mortality rate of French settlers in Algeria at 70 per 1,000 per annum. He does not specify exact dates, but from the context this appears to be data for the 1840s.

[80]On p.7, Albouy says of Egypt "According to Curtin (p.161) the high post-campaign rate was partly due to earlier campaigning." This is not what Curtin says or implies. The closest relevant statement on this page is, "The outbreak [of typhoid] seems to have been caused principally by the condition of the Cairene barracks and water sources at the moment of the British occupation." Elsewhere, Curtin makes it clear that contaminated water was an ongoing peacetime problem, and in fact the campaign found temporary methods of dealing with this.

[81]The Malta and Gibraltar rates are from Curtin (1989, Table 1.1).

more peaceful conditions." However, Curtin does not make such a statement and we could not find any other remark by him supporting this claim.[82]

4.2.3 Albouy's Campaign Series

Albouy takes our Algeria rate for his campaign series in Algeria and Morocco. Albouy changes the rate for Tunisia from 63 (in AJR) to 63.3 in his campaign series. Keep in mind that these data are used in log form, so this is a change from 4.147885 to 4.143135. This is not an issue of any consequence.

There are several points about the campaign series for these countries. First, Albouy claims that his campaign rate for Malta is also the same as that of Tunisia. But in his data, the campaign rate is 61 for Malta and 63.3 in Tunisia. It is not clear how he constructed an estimate of 61 for Malta.

Second, Albouy uses 13.87 as a campaign rate for Sudan, despite the fact that this number requires using a data point that Curtin clearly suggests is mistaken.[83] Justifying his low estimate, Albouy says (footnote 10 on p.7) that "Places like Sudan and Ethiopia were healthy for Europeans, offering certain health benefits such as a reduction in deaths from tuberculosis ([Curtin], p.43)." But Curtin does not mention Sudan on or around p.43. He is speaking only of Ethiopia. It is true, as Albouy points out in the same footnote that on p.229 Curtin says "The Ethiopian success was made possible by an uniquely benign topical (sic) disease environment...". But Ethiopia was unique, according to Albouy's own quote, not the same as Sudan.

Third, there is a further puzzle in the rates for Morocco and Algeria. Albouy says (p. A2) "I also follow AJR's practice of assigning Morocco's mortality rate to Algeria." But Algeria's campaign rate is 78.2 (the original AJR estimate) and the barracks rate is 21.4, while Tunisia's rates are 63.3 and 16.3 respectively in Albouy's dataset. The origin of the 21.4 estimate is unclear. Is this from the third column of Table 1.8 in Curtin (1989, p. 36), which is cited in Albouy's footnote 5? But this is 21.93 not 21.4. Why doesn't he use the estimate of 81.54 for 1837-46 in the second column of this table?

4.3 Central Africa[84]

4.3.1 AJR's Series

For Central Africa in AJR we relied on two sources. First, we assigned a mortality rate of 280 from French Soudan to Angola, Cameroon, Chad, the Central African Republic, Gabon, and Uganda. This estimate is from Curtin (1998), p.238, Table A8.1.[85] Second, from Curtin we had estimates of mortality rates for Africans working away from their homes, for Congo and Kenya. We took the highest observed values of these rates to represent a minimum for Europeans in

---

[82]What Curtin says on p. 17 is (1) "Climatically, the south shore of the Mediterranean was much like the north shore in Italy or southern France," and (2) "Within a decade or so [after the conquest period of the 1830s] the Algerian death rate was closer to the rates of the Mediterranean islands." The first statement is about climate, not disease environment. The second statement is about a specific moment in time, after the French had learned about how to cope with the Algerian disease environment, i.e., an outcome of the conquest.

[83]On p.173, Curtin (1998) says, "Medically, this brief period from February to April [1884] 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, though it suffered 31.36 deaths per thousand from enemy action."

[84]There are seven central African countries in AJR: Congo-Brazzaville, Congo/Zaire, Angola, Cameroon, Gabon, Tanzania, and Kenya. All are in Albouy's campaign series, but none are in his barracks series.

[85]Burundi and Rwanda were missing other data, so they are not in the original AJR base sample.

those places.[86] The Kenya rate (145) was assigned to Tanzania, and the Congo/Zaire rate (240) was assigned to Congo-Brazzaville and Zaire; these rates and the underlying source (Curtin et al. 1995) was stated clearly on p.33 in our NBER working paper.

Albouy argues that the regional assignment should be different (p.10). Albouy objects to our assignment of mortality rates within Africa and says [for his campaign rate] "I regroup the countries more sensibly, giving Niger and Burkina Faso the same rate as Mali; Uganda the same rate as Kenya; Gabon, Cameroon and Angola the same rate as Congo." He does not document on what basis he makes this assignment. It is a different assignment, but is it an improvement? Reasonable people might disagree on this, but whatever the case, switching these countries in our original series makes little difference to our results – it is primarily a switch from 280 to 240 or vice versa (although the estimate for Kenya is lower, at 145 per 1,000).[87]

4.3.2   Albouy's Barracks Series

Albouy codes all these countries as missing in his barracks data. This is despite the fact that he assigns rates for his campaign series. For other parts of the world, e.g., Asia, when he has a campaign estimate, he is willing to assign a barracks rate (based on his view of the relative barracks-campaign ratio). As we have already discussed above, this practice is inconsistent with what he does for his own campaign series and has dramatic effects on the results.

4.3.3   Albouy's Campaign Series

Albouy assigns a campaign rate of 100 to Angola, Cameroon, Congo, Gabon, and Zaire, based on the argument that Europeans would have faced the same death rate as the average death rate of non-local Africans. But this is hard to defend.[88] For example, Kiple (1984), Chapter 10, discusses the relative mortality rates of Europeans and Africans in places with malaria and other tropical diseases. The data, from the same underlying source as Curtin uses, suggests that before tropical medicine improved in the mid-nineteenth century, the death rate for Europeans would be 2-3 times the death rate for Africans (and sometimes higher – see Tables 4 and 5 on pp.170-171, Kiple 1984).[89] This supports our estimates rather than Albouy's.

4.4   OTHER AFRICA[90]

AJR's South Africa rate is 15.5 in Curtin 1989 (Table 1.1, for Cape Colony).[91] Albouy accepts this for his barracks rate and uses a rate of 24.58 from the Boer war for his campaign rate. There is no challenge to AJR's data here, just a further mixing of different scale conflicts in Albouy's "campaign" series.

---

[86]In contrast, Albouy wants to use the average death rate for Africans as the average death rate for Europeans. In our discussion of his campaign series for Central Africa, below, we cite evidence that suggests these groups did not have similar death rates when malaria and yellow fever were present.

[87]Albouy, p.9, speaks of "compounding errors with serious consequences" for our Africa data. This we do not find.

[88]Even within Albouy's series, these rates are close to those for the Indian subcontinent and lower than for much of Central America and the Caribbean. We have already argued that such a coding does not pass a "reality check" and is simply inconsistent with a huge body of historical evidence.

[89]Curtin (1998, Table 1.1, p. 8) reports deaths from disease in the Sierra Leone Command, 1816-37, as 26.5 for Africans and 478 for Europeans.

[90]The only Southern African countries for which AJR had data are South Africa and Madagascar.

[91]Army Medical Department (1840) puts the rate on the Cape Frontiers at only 9.8 (p. 17b).

Albouy accepts AJR's rate for Madagascar, but uses this for his campaign series. He codes the barracks rate as missing. As we noted already, this is inconsistent given that in other contexts he is willing to apply barracks-campaign mortality ratios to fill gaps in his data.

4.5 The Caribbean[92]

4.5.1 AJR's Series

AJR applied Curtin's rates of the Windward and Leeward Command to Trinidad and Tobago (which was within the command), and to the Bahamas; we also applied the rate for Jamaica to Haiti and the Dominican Republic. Curtin's data for this region, as in much of his 1989 book, is from the work of Alexander Tulloch.

4.5.2 Albouy's Barracks Series

Albouy proposes to improve our data by adjusting the rates to separate out barracks and campaigning. He claims, on p.A2, Curtin (1989, pp.25-8) says that the mortality rates for Jamaica and the Windward and Leeward Command were for soldiers "who were traveling, campaigning...". But Curtin says no such thing and the original source, Tulloch writing in Army Medical Department (1838), is also clear this is peacetime.[93] Explaining his revision, on p.A3, Albouy says "The decade from 1837 to 1846 appears more peaceful for soldiers in the Caribbean," but there is no evidence in Curtin or elsewhere that the earlier periods with data were not peaceful. Albouy wants to disqualify the period 1817-26, claiming that it was not peacetime, but he has no basis for this. He also ignores the period 1827-36, without any basis. Again, Albouy has not applied his rule of taking the data from the first 10 years of peace – even with his incorrect definition of peace.[94]

The Curtin-Tulloch numbers are for all ranks (ordinary soldiers and officers combined). Remarkably, Albouy's barracks rate of 66 per 1,000 p.a. for Jamaica is below the death rate of officers alone (83.4 on p.37 in Balfour 1849). Soldiers consistently had higher death rates than officers at this time around the world (see Balfour 1849 and the further discussion in Section 5 below).

Also, Curtin (1989, p.28) makes it plain there were changes in how the troops were housed during this period, which likely accounted for a large mortality decline. As with his West Africa data, Albouy ignores improvements in medical technology.

4.5.3 Albouy's Campaign Series

Albouy's campaign numbers for the Caribbean confuse peacetime with wartime. The underlying source for Curtin (Alexander Tulloch) reports a proper wartime mortality estimate for the Caribbean: 244 per 1,000 p.a. between 1796 and 1807 (Kiple 1984, p. 170). Albouy should use this number.

---

[92]There are data on five modern Caribbean countries in AJR: the Bahamas, Dominican Republic, Haiti, Jamaica, and Trinidad and Tobago. All are in Albouy's campaign and barracks series.

[93]The series of studies involving or based on the work of Tulloch and Balfour are all focussed on peacetime. When there was a serious campaign that affected mortality, it is discussed explicitly (e.g., the material on Burma in Army Medical Department 1841).

[94]On p.A3, Albouy says that in terms of relative mortality, "Jamaica and the Windward-Leeward command change ordering after 1837 during more peaceful times", i.e., Jamaica had lower mortality. This is the point he is trying to establish by ignoring the data before 1837. But in 1859-68, the Windward-Leeward Command had a mortality of 13 per 1,000 while Jamaica had a mortality of 21.

Albouy also is inconsistent in taking the rate of 130 as Jamaica's campaign rate (p. A3). Following his own argument and coding rules, he should have taken 143 from Curtin (1989) Table 1.5, p. 27, for 1817-26 (rather than 130 from Table 1.1 for 1817-36). He should also have taken 93 (instead of 85) for Trinidad and Tobago, and 29 for Guyana.

4.6   Central and South America[95]

4.6.1   AJR's Series

AJR's rates were based on the mortality rates of bishops from Gutierrez. As explained in Section 2 above and in more detail in Appendix 1, we merged these data with Curtin's estimates to produce a consistent series.

Albouy claims that we misallocated mortality rates to countries. We show in Appendix 1 that he is not correct on this point.

4.6.2   Albouy's Barracks Series

The main issue is that Adams (1952), a source introduced by Albouy, has peacetime rates for Mexico of 85 and 110 (p. 223), as well as a campaign mortality rate of 100 p.a. (p.194). For some reason (not explained or even mentioned in his comment), Albouy ignores these numbers.

For the barracks series, Albouy asserts it is an improvement to assume that the death rate of bishops was the same as that of ordinary soldiers (and settlers). This is implausible (at least during the period in question, bishops were relatively rich and well nourished, and lived in much better circumstances), and it leads to very low estimates – for example, Albouy uses a barracks mortality rate of 16.8 for Mexico, which is very far from what his own source (Adams) indicates. We reviewed the implications of correcting this point in Section 3 above.

In addition, as discussed in Section 3, Albouy calculates the mortality rates of bishops incorrectly. Appendix 2 shows how to convert them into with-replacement equivalents.

4.6.3   Albouy's Campaign Series

For the campaign series, Albouy proposes to change estimate for Mexico from 71 to 61 and there is some reassignment of relative mortality rates within Latin America.[96] As mentioned above, AJR tried two other methods of calculating mortality in Latin America (one based on rates relative to Jamaica and the other using mortality from naval stations).[97]

---

[95]AJR have data on 18 countries in Central and South America: Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Ecuador, Guatemala, Guyana, Honduras, Mexico, Nicaragua, Panama, Peru, Paraguay, El Salvador, Uruguay, and Venezuela. All are in Albouy's campaign and barracks series.

[96]In footnote 6, Albouy says AJR picked the higher mortality estimate of two from Curtin. We used the number that appeared to be an annualized rate, but we may have misunderstood Curtin. In any case, in light the new data from Adams (1952), our error appears to have been small (and much smaller than the new error proposed by Albouy).

[97]The southern part of South America may have been healthier than suggested by AJR's data, but it is hard to know for sure. Cantlie (1973, pp. 286-289) reports that a British force suffered a total mortality rate of 353 in a strength of 10,508 over a nine month period during 1807 in South America (mostly around Buenes Aires). Of the 353 deaths during this peirod, 203 were from wounds, accidents and other injuries. The remaining 150 deaths were from various kinds of disease, giving a rate of 14.3 per 1,000. But this is a nine months rate, 699 people remained in hospital at the end of the period, and an undetermined number of men stayed on ships during the year and only set foot on land only part way through 1807. The reported numbers also seem to include only soldiers who were hospitalized (i.e., if they died before reaching hospital, they are not included.)

Albouy says, p. A3, that his Mexico estimate is based on "the precise number of troop-years (34,319) that French troops were at risk and the number of deaths in this population (2,095)." He cites pp.113-121 and pp.471-72 from Reynaud, but those pages do not contain the information necessary to make this calculation.[98] Furthermore the number of deaths during this period, January 1862-November 1863, is stated unequivocally on p.471 of Reynaud to be 1,410 (army) plus 1,299 (sailors) plus 718 (Marines). Even with Reynaud's adjustments for deaths from enemy fire, we cannot figure out how to make these add to 2,095. Also, in his detailed recalculation it is not clear whether Albouy is accounting for the fact that some of these soldiers were not European, thus it makes no sense to include them (if we are trying to measure potential European settler mortality).

4.7 USA and Canada

4.7.1 AJR's Series

AJR's rates are 16.1 for Canada and 15 for the US from the Northern United States, in Curtin (1989, Table 1.1).[99]

4.7.2 Albouy's Barracks Series

Albouy accepts our USA and Canada estimates for his barracks series.[100]

4.7.3 Albouy's Campaign Series

Albouy's campaign estimates likely tell us nothing about the mortality of settlers. But they do tell us something about his methods.[101]

Albouy claims the right estimates for the USA and Canada in his campaign series are from disease in the US civil war. This is a strange choice, because he is implicitly claiming that the US civil war, a mass struggle involving armies of hundreds of thousands raging across half a continent for more than 4 years, is comparable to small local short-lived punitive expeditions in some parts of Africa (as well as slightly larger operations in Mexico and some other places). The US Civil War was clearly an exceptional experience and arguably marked the beginning of modern warfare, with its associated disease burden for soldiers. Many, although not all, colonial

---

[98]On p.113, Reynaud states the total number troops in Mexico on January 1, 1863, and on p.114 he compares this with the total number of troops who arrived during 1862, but he does not indicate when troops exactly left or died during that year. Also, for the largest group of troops (22,230) he does not indicate when exactly they arrived between August 23 and November 9, which is a large window (p.114)

[99]These were times of peace. For more detail see Army Medical Department (1839 and 1853). The death rate for Nova Scotia and New Brunswick, 1817-36, was 14.7 per 1,000 p.a. (Army Medical Department, 1839, p.14b). The rate of 16.1 is for "upper and lower Canada" (p.25b).

[100]The original source for the U.S. data, Lawson (1840, e.g., p.327) observes that American troops in this peacetime period did more marching and fighting than the British troops covered by Tulloch's data. He suggests this is one reason the mortality rate for the Southern Division of the US Army was higher, at 30 per 1,000 p.a.

[101]Albouy argues that the campaign rate for Canada must be high because there was high mortality among American troops who spent the winter of 1812-13 in the northern Champlain valley. But he neglects to point out that his source, Everest (1981, p.95) attributes a large (but not precise number) of these deaths to "diseases resulting from exposure." "On both sides of the lake December was the most deadly month, partly because of the accumulation of ailments from the exposures of the campaign and partly because of the severity of the weather" (Everest, 1981, p.95). Albouy is claiming something – albeit vaguely – can be inferred about disease environment from the fact that camping outdoors in the winter is unhealthy. Such an inference seems unwarranted to us.

campaigns were moderate marches with some firepower demonstration.[102] Total deaths from disease on the Union side alone are estimated by Adams (1952) to be around 200,000 – 3 or 4 *orders of magnitude* larger than in most of the other campaigns that Albouy considers.

Albouy's choice of the US Civil War rate is particularly striking given that there is another, more comparable estimate of mortality for British soldiers campaigning in North America during the War of Independence: 26 per 1,000 p.a. (Cantlie, 1974, p.156; it is not clear that all of these deaths were from disease). The forces engaged were smaller than in the US Civil War and the conflict was more sporadic, rather as in African campaigns.[103] The mortality is for British soldiers, as with much of the Curtin data. Many of the deaths were related to exposure in harsh winters, which does not tell us anything about the diseases faced by settlers (who typically live in houses), and there were some notable instances of military mismanagement. Nevertheless, we used this number in our revision of Albouy's campaign series in Section 3 above, and showed that it makes a considerable difference to his results.

There is also an inconsistency in Albouy's coding across regions that becomes clear when we consider the US Civil War. He claims, on p.7, "I apply AJR's rules more consistently except when the rules lead to mortality rates entirely based on severe epidemic periods."[104] However, he selectively includes data from epidemics. Health conditions in the Union army during the U.S. civil war involved severe epidemics, and these were far from normal according to the sources that he himself cites (e.g., Shryock 1962 [cited in the July version of Albouy's paper, Albouy 2004a], Adams 1952).

Albouy also points to some instances of high mortality at other times in parts of North America. No doubt there were some episodes.[105] In point 2 of our theoretical framework, repeated in Section 2 above, the key words are "mortality rate from disease for Europeans ." The fact that many people died when forced to spend a northern winter with little food and poor housing, as in Albouy's North American examples discussed in his p. A5, tells us little if anything about the diseases that potential European settlers would face.[106] It is striking that in his campaign series Albouy assigns a mortality rate to the USA and Canada (53.4) that is quite close to the rate for Trinidad and Tobago (85), despite the fact that one of his sources, Wells (1975), "[compared with Caribbean islands] Mortality among the populations of the mainland colonies seems to have been much lower."

---

[102]The campaigns that Albouy prefers typically involved a few thousand soldiers.

[103]But still this was a large war by the standards of other datapoints in Albouy's campaign series: the British forces had an average strength of 39,196 during 1775-1780 (Cantlie, 1973, p.156). The death rate for Hessians was higher than for the British.

[104]Actually, Albouy hardly applies AJR's coding rules at all. As a small example, the next paragraph on p.7 makes it clear that he follows a rule that is different from AJR – regarding AJR's use of the earliest available estimate from Curtin, on p.7, Albouy says, "I prefer to use averages." He goes on to say that where necessary he constructs his own average "either over the duration of the military campaign used, or over the first ten years of available peacetime data" (p.7).

[105]Albouy cites Bolton and Marshall (1971) on mortality in the United States. On p. 8 he cites p.1971 of this work, which does not exist, but we have tracked down the precise page references on his p. A7. Albouy notably neglects to mention that this source attributes the high initial mortality in Plymouth during the winter of 1620-21 as due to lack of housing and "scurvie" (p.138). He also (on p. A7) cites p.86 of the same book on Quebec, where again the issue was the first winter, and p.100 where the issue was in part an Indian massacre in 1689. For Jamestown there was apparently some malaria, but also starvation (see p.117 of Bolton and Marshall). The issue is not whether one, two or five settlements failed, but rather how Europeans fared more generally in a disease environment – this is what we are trying to measure.

[106]Albouy cites Feinberg (1974) as an authority, without mentioning Feinberg's assessment that while there was high mortality for some early settlers in North America, there is evidence that mortality quickly declined "to a rate lower than or comparable to those of England and France" (note 34 on p.370).

4.8 Southeast and East Asia[107]

4.8.1   AJR's Series

Our estimates came from Curtin. Specifically, we used the Straits Settlements estimate in Table 1.1 (Curtin, 1989) for both Malaysia and Singapore.[108] For Indonesia we use the Dutch East Indies estimate in the same table. For Vietnam we took the first available Cochin China rate, for the French army in 1861, in Table A8.2 of Curtin (1998).[109] For Hong Kong we used the China Field Force rate for the British army in 1860, from the same table.

4.8.2   Albouy's Barracks Series

Albouy accepts our estimate of the mortality rate for Malaysia and Singapore in his barracks series. He codes Vietnam and Hong Kong as missing, even though he assigns a campaign rate. This is inconsistent with what he does elsewhere (e.g., South Asia).

For Hong Kong, Albouy points out that the China Field Force did not fight in Hong Kong, but then assigns Hong Kong the annualized mortality of this force as a campaign rate and not as a barracks rate.[110]

For the Indonesian barracks rate, on p.6 Albouy says he uses the "first available rate from a peaceful era, 1859 to 1868 (Curtin, 1989, p.201)"; this is recorded as 64.52 in his dataset. But Table A37 on that page in Curtin gives a first available rate of 78.65 for 1859.[111] The same table makes it plain that mortality declined over time, so later estimates cannot be good measures of early settler mortality – again, the same problem as with Albouy's West African estimates.[112] Also, Albouy's objection to the AJR estimate of 170 for Indonesia is that this was "during the Java War" and makes it sound like there was heavy campaigning throughout the period in question. But the estimate is for 1819-28, while the Java War broke out only in 1825. He uses our Indonesia estimate for his campaign series.

4.8.3   Albouy's Campaign Series

Albouy's selection of data for his campaign series is highly problematic. First, Albouy does not want to use the first available estimate for Cochin China, preferring a campaign rate that is an average over three years – the rates fell from 140, to 117, and then to 107. But this likely reflects exactly the kind of military-related medical learning that will seriously bias his

---

[107]There are five countries from Southeast and East Asia in AJR: Hong Kong, Indonesia, Malaysia, Singapore and Vietnam. All are in Albouy's campaign series, but Hong Kong and Vietnam are not in his barracks series.

[108]The estimate is from Penang; both Malacca and Singapore were part of the Straits Settlement.

[109]Albouy claims on p. A5, "It is unclear according to AJR's method whether to take the earliest rate or the rate from the region with the lowest rate." Our method was to take the earliest rate. If there were two rates for different parts of a country from the same period, as in the Gutierrez data for Colombia for example, then we took the lowest.

[110]Albouy argues that the rate reported in Curtin is not an annual rate. But the underlying British Medical Journal publication is not clear on this point. Albouy makes a heroic assumption about the period covered. Moreover, in this instance Albouy "annualizes" the available rate, but does not apply the same principle for other data drawn from the same source.

[111]Presumably what Albouy is actually doing is using an average for Indonesia over 10 years. For India, we would note, he averages over a different number of years.

[112]Albouy ignores the point, made by a source that he introduces (Ricklefs, 2001, p.118), which is that by the early 18th century, the VOC's [Dutch East India Company's] headquarters at Batavia had acquired, "the reputation for pestilential mortality which it would retain well into the nineteenth century." Malaria was apparently a leading cause of death.

estimates. Albouy also codes the barracks rate as missing yet he is willing to infer barracks rates from campaign rates elsewhere.

Second, Albouy uses the ratio of barracks to campaign deaths in Burma (which he calculates as 3.44), and multiplies this by the Singapore/Malaysia barracks rate to give a campaign rate for Singapore and Malaysia.[113] This ratio is very different in different places, even within his dataset and Burma is quite far from Singapore. We find it hard to see such data constructions as improvements over the data we used in our original paper.

Third, for Hong Kong, Albouy assumes that the rate of 14.9 in AJR is for only six months, but his own source, Graham (1978) says the British contingent left India in February and began to arrive in Hong Kong in March. Albouy himself acknowledges the campaign ended in November. But then the troops had to leave China. In truth, we do not know the exact length of time covered by the campaign estimate, but it may well have been 10-12 months, rather than the six months assumed (contrary to information in his own sources) by Albouy.[114]

Finally, Albouy constructs weighted averages sometimes (e.g., Sudan) and unweighted averages at other times (e.g., Vietnam). From a source that he cites, Reynaud, p. 471, Albouy could calculate the weighted average mortality 1861-63 for Vietnam (from Cochin China). But instead he reports the unweighted average. There is no reason to be inconsistent in this fashion.

For this region, as with others, Albouy makes different assumptions. There is no reason to think these are better than the assumptions in AJR. In fact, on the whole they seem rather less likely to lead to sensible measures of early settler mortality.

4.9   South Asia[115]

4.9.1   AJR's Series

All our data for South Asia came directly from Curtin (1989, Table 1.1). We assigned the available rates to modern countries as follows: Bangladesh from Bengal, Madras for India, and Bombay for Pakistan. We also used the rates from Ceylon for Sri Lanka.[116] These data are all from before 1838.[117]

4.9.2   Albouy's Barracks Series

Albouy accepts our India and Sri Lanka rate for his barracks series, but decides that our Bengal/Bangladesh rate belongs in his campaign series. He does not use our Bombay/Pakistan estimate, but prefers a different assignment (as with West Africa, he prefers later estimates when medical technology had improved.) We do not understand the reasoning for this, but in any case nothing affects our results significantly.

---

[113]There is a justification on p. A6, where Albouy cites Kennedy (1970, pp.226-7) as saying "mortality rates of natives in the Malay Peninsula were quite high from malaria and other tropical diseases." But on those pages Kennedy does not indicate precise mortality rates, just the presence of some diseases. It is therefore impossible to infer anything about mortality relative to other colonies from this material. There is also no mention of the disease environment in Singapore on these pages.

[114]On p.386, Graham (1978) says Kowloon was "an apparently healthy site for a barracks or camping ground...".

[115]There are four South Asian countries in AJR: Bangladesh, India, Pakistan, and Sri Lanka. All are in Albouy's campaign and barracks series.

[116]See Army Medical Department (1841, p. 8) for the original estimate and more detail. Mortality may have been lower 1820-26, but the data are not strictly comparable.

[117]Curtin also reports a Coastal Burma rate that is appropriate for Burma/Myanmar, but this country is missing other data so it was not in the original AJR series.

4.9.3 Albouy's Campaign Series

For his campaign series, Albouy uses the ratio of India's campaign to barracks rate (of 1.61) to adjust rates up or down around the region (but at different rate from the Burma/Myanmar adjustment he uses for Southeast Asia).[118]

There is also an important inconsistency in the number of campaigns used in the South Asian data. While Albouy sometimes uses just one campaign when more than one is available (e.g., Nigeria – Albouy uses an 1892 campaign from Benin, but there was also an 1890 campaign in Benin), in the case of the Madras Army (p. A6) he averages over three campaigns (while his source, Curtin, 1989, p.23, lists six campaigns.)[119]

4.10 OTHER[120]

4.10.1 AJR's Series

For New Zealand and Malta, our data are from Curtin (1989, Table 1.1). We assign the New Zealand rate to Australia.

4.10.2 Albouy's Barracks Series

Albouy does not contest our New Zealand data. He uses this for both his campaign and barracks series. He also accepts our estimate for Malta in his barracks series.

Albouy adjusts Australia from 8.55 to 17 (for both his campaign and barracks series) based on Austin (1979). The cited page, Appendix 11 on p.259, contains a graph of regimental and total force mortality, but no numbers, so exactly how Albouy calculated his rate of 17 for 1839-1848 is unclear. In any case on p.69 of the same book, Austin says, "It is believed all voyage deaths were included in the first monthly strength return after arrival, and if this is so the mortality rates shown in Appendix 11 are distorted to this degree." So, according to Albouy's own source, these data are not really comparable to what is in Curtin.

4.10.3 Albouy's Campaign Series

Malta's campaign rate is 61 in Albouy's data (p.5). Albouy says that this is the same as Tunisia, but Tunisia's campaign rate is 63.3 in his data. This is a further puzzle. Albouy's rationale for a high rate for Malta is apparently p.89 in Gregory (1996). Albouy neglects to mention that this page refers to the death rates of a French garrison during a long siege, i.e., when there was not enough food. A significant number of people died of scurvy, which is a nutritional deficiency and nothing to do with the diseases faced by potential European settlers (p.89, last full paragraph, Gregory 1996). On pp.89-90, Gregory says, "It was ... the threat of starvation, that forced Valletta's eventual surrender." Near the end, the French commander wrote home, "Courage and determination are not lacking, but the stomach commands both"

---

[118]On p.6, Albouy claims that because the "Madras army" campaigned or was stationed in a number of locations, those places should have the same mortality rate. This raises the campaign mortality estimate for Malaysia and Singapore, but really makes no sense. On p. A6, Albouy says "The ratio of India's campaign rate to its barracks rate is 161"; this is obviously wrong.

[119]Curtin's (1989) rates on p.23 are 59 for 1793-98, 111 for 1799-1800, and 88 for 1801-5. Albouy's estimates are "weighted by year"; i.e., he assigns the rate to each year and then averages over the whole period. But why stop in 1805? This is not even the "first 10 years" rule that Albouy claims to follow.

[120]Three other countries were included in AJR's data: Australia, New Zealand, and Malta. All are in Albouy's barracks and campaign series.

(Gregory, 1996, p.90).

It is interesting that for both Australia and New Zealand, Albouy codes the campaign and barracks rate as the same, i.e., he is implicitly acknowledging that this is possible in neo-Europes. However, there is no discussion of why this is not the case in other neo-Europes. This is just one more anomaly amongst many in his paper.

4.11 Overall Assessment

Albouy proposes numerous changes to our data. We have examined all these changes and traced back his new material to the original sources. We have also examined closely all his quotations and citations from Curtin. Our assessment has been mostly based on sources and data that he claims to accept, but in a couple of cases (e.g., Cantlie on the US and Davies on West Africa) we have brought in relevant new information.

Without exception, we find his objections to our data to be based on (1) taking the wrong data from Curtin, e.g., on the Caribbean, (2) misquoting sources or taking material out of context (e.g., on Hong Kong or Malta), and (3) other idiosyncratic decisions or mistakes which we detailed above.

In his barracks series, the main problems are the arbitrary assignment to neighbors in West Africa, refusing to code most of Africa even when information is available, the insistence on using mortality rates for Africa that are from late in the nineteenth century, and the assumption that bishops and soldiers had the same level of mortality in Latin America. None of these constitute an improvement over our data.

In his campaign series, he mixes data from quite different military experiences. This series in no way contains comparable data. By definition, it is a less good measure of settler mortality than our original series. It is also striking that he consistently selects very low estimates for most of Africa.

Albouy claims to have improved the data in AJR. We have looked hard but cannot find any improvement. He is right however that the estimate in AJR for Tunisia should be 63.3 not 63 (but then he applies this to his campaign rate, not to his barracks rate, without adequate justification.)

5 Additional Data

By basing the analysis in AJR on the work of Curtin, we were relying on a leading historian's assessment of what are relevant and good data. Curtin was interested in a similar question – the full title of his 1989 book is *Death by Migration: Europe's Encounter with the Tropical World in the Nineteenth Century* – but he did not have in mind an instrumental variable approach and shows no interest in modern measures of institutions or GDP per capita.

However, it seems reasonable to examine Curtin's original sources for any information that he did not regard as worth including. None of these data are unequivocally better than the original AJR series and we prefer our original series, as this is based on data filtered by Curtin. However, the following material does suggest additional reasonable robustness checks.

Most of these data come from the work of Major (later General and Sir) Alexander M. Tulloch, who pioneered the study of military mortality statistics. We also report here material from people who worked with Tulloch, including T. Graham Balfour and other members of the Statistical Society of London and various commissions of enquiry.

First, there is a mortality estimate for European soldiers in Hong Kong from Tulloch's writings.[121] On p.254, Tulloch (1847) reports the average ratio of mortality per 1000 of strength, 1842-1845, to be 285. This is not a long average, as in the rest of Tulloch's work, and it is not yet clear that this is the earliest available estimate, but we still take this rate for our new robustness series. It is also not certain that all these deaths are from disease, but it does fit with Cantlie's (1974) negative assessment of Hong Kong, pp.477-481.[122]

Second, on the important question of mortality for civilians in North Africa, Tulloch (1847, p.259) reports annual mortality among French settlers (not soldiers) in Algeria as 70 per 1,000.[123] AJR's original rate was 78.2 per 1,000, so we were close (and, from the context, Tulloch may have been reporting just an approximation). In our robustness series, we take 70 as the rate for Algeria and Morocco.

Third, Tulloch also reports a mortality estimate for British troops in Honduras (specifically, what was formerly known as British Honduras and is now Belize).[124] This speaks to the issue of whether we correctly merged (benchmark) the Gutierrez and Curtin data. Our previous merge was based on Mexican data, which Albouy objects is for a campaign, and on Jamaican/Dominican Republic data. In our base AJR data, we imputed a rate of 78.1 per 1,000 for Honduras; Albouy argues that our procedure produces mortality rates that are too high. The rate reported by Tulloch (1838b, p. 229 and 230) is 95.2 per 1,000, albeit based on a small sample.[125] The rate in our robustness series therefore changes and we add one more cluster (assuming the same mortality rate for Belize and Honduras).[126]

Fourth, there is new information for the West Indies. For the Windward and Leeward Command, Curtin (1989, Table 1.1) used the estimate of 85 per 1,000; this appears in Tulloch (1838a) on p.131. The rate using only deaths that can be "accurately traced as having taken place from disease" (p.131) is 78.5 per 1,000. However, Tulloch (1838a) has a specific estimate over the period 1817-36 for Trinidad of 106.3 and for Tobago of 152.8.[127] In our robustness series, we use an unweighted average of these two: 129.6 per 1,000. These two islands were among the least healthy of the Command.[128]

Tulloch (1838a) reports a mortality from disease rate in British Guiana of 84 per 1,000

---

[121]This is in an article that is not cited by Curtin, and which we previously overlooked.

[122]The death rate for White troops in China in 1859 was put at 41.93 by Balfour (1861), or 59.35 including invalids who died on the way home (not usually included in early mortality estimates), and 52.04 in Southern China in 1860, which includes invalids left in Hong Kong (Army Medical Department 1862). Was there perhaps a big mortality difference between Hong Kong island (presumably covered by these statistics) and the Kowloon Peninsula (where the troops mustered)? Select Committee (1866) suggests part of the answer – troops were sent to Hong Kong when already sick, thus raising the measured mortality rate. However, the evidence and proceedings of this committee suggest there was malaria in the area.

[123]See also Committee of Inquiry (1867). This committee finds that military mortality 1831-46 was 80 per 1,000 p.a. (p.38), and mortality among settlers was similarly high (60 per 1,000, 1833-47, p.9). From their analysis, e.g., pp. 15 and 18, it seems that much of this mortality was due to malaria. The decline in mortality subsequently (see, for example, p.17) was due in part to some well placed public works, including drainage.

[124]The troops were stationed primarily near what is now Belize City (p. 230).

[125]With an average strength of 21, there were 30 deaths definitely due to disease (of which 27 were from fever) and 33 total deaths (Tulloch, 1838b, pp.229-230; see also Army Medical Department, 1838, p.77). We use the deaths definitely due to disease in this calculation.

[126]Belize was missing other data in our original AJR series and is also not included in our revised series.

[127]The average strength of forces was 310 in Trinidad and 170 in Tobago (Balfour, 1845, p.202).

[128]Tulloch notes, (1838a, p.134) that the highest mortality in Trinidad was 1817-20, when the annual rate was as high as 398 per 1,000; "since then various improvements in the positions of the troops have taken place, and the mortality has been reduced to the average of the other stations in the command" (p. 134).

over 1817-36 (pp. 131 and 133).[129] Previously we used the rate from French Guyana, so this direct estimate is presumably preferable and we use it in our robustness series.

For Jamaica, Curtin uses a rate of 130 per 1,000 (1989, Table 1.1; see Army Medical Department, 1838, p.44). The rate in Tulloch (1838b) is 128 per thousand, for European troops, over 1817-37. However, confirmed deaths from disease give a mortality rate of only 121.3 (Tulloch, 1838b, pp. 217-218).[130] There was high mortality in almost all parts of Jamaica, but there was one place – Maroon Town – with much lower mortality.[131] 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), rather than one healthy spot. In this instance it seems plausible therefore to take 121.3 per 1,000 for our robustness series.[132]

As Jamaica is the benchmark for linking the Curtin and Gutierrez data in our revised series, changing the Jamaica datapoint triggers changes in all our Latin American estimates. The low mortality region has a death rate of 55.1, the medium mortality region has 60.7, and the high mortality region has 121.3 per 1,000 p.a.

For the Bahamas, we used Curtin's estimate for the Windward and Leeward Command. Tulloch (1838b, p.229) reports that with an average strength of 27, the Bahamas had 102 verified deaths from disease over 20 years (1817-37), which is an average annual death rate of 189 per 1,000.[133] Most of the mortality occurred during epidemics in 1819 and 1823 and Tulloch attributes this high death rate to the unfortunate location of one fort, which was particularly vulnerable to yellow fever. We use the rate of 189 per 1,000 in our revision.

Fifth, Tulloch (1847, p.253) reports mortality prior to 1836 in New South Wales and Van Diemans Land (Australia) as 14 per 1,000, with about the same rates in 1844-45.[134] AJR used the rate of 8.55 from New Zealand. We use 14 in our revised series.

There is some additional corroboration in Balfour (1845, p. 195), although it is not clear to what extent he is drawing on Tulloch's work. Balfour reports a death rate for New South Wales of 14.1 (dates not specified), for Canada of 20 (1817-36), for Jamaica of 143, for the Bahamas "a small detachment" (1817-36), and other rates that are very close to what AJR used from Curtin (1989).[135] These are peacetime data, Balfour (1845) says quite clearly: p. 197, "These rates... only exhibit the mortality during peace, when the troops are placed under the most favourable circumstances."[136]

---

[129]The average strength of the force was 884 (Balfour, 1845, p.201).

[130]But Army Medical Department (1838) says that including invalids who died on the way home or shortly thereafter would raise the death rate to 143 per 1,000 p.a.

[131]Maroon Town is at an altitude of over 2,000 feet. Some of the deaths there were people sent there already as invalids. The true mortality rate may have been as low as 22 per 1,000, "being the same as the Foot Guards in London on the average of the last seven years" (Tulloch, 1838b, p.226). The average force strength in Maroon Town during this period was 190, while the force strength in the highest mortality areas ranged from 137 to 726 (Balfour, 1845, p.203). Most of the island was quite unhealthy, with mortality rates ranging from 73.5 to 178.9 per 1,000.

[132]Using this estimate may make our series less comparable – probably Curtin was using mortality rates based on total deaths in all countries.

[133]Total deaths were 107, of which five were "causes not known." If we use total deaths, the mortality rate would be 198 per 1,000.

[134]Austin (1979) suggests that Australian mortality numbers included deaths on the voyage, in which case these numbers should be adjusted downwards to make them strictly comparable. In the introduction to part I of Army Medical Department (1840), Tulloch remarks that deaths from disease in Australia were "little more than" 10 per 1,000.

[135]The New South Wales estimate is from "Inspector-General Marshall's work on Invaliding," and the other data are from "Colonel Tulloch's Reports on the Health of the British Army" (Balfour, 1845, p.195).

[136]Balfour (1845, p.198) documents that "the mortality during war is very high, much higher than in most

Sixth, regarding Singapore, there is some new (to us) information, but it is qualitative rather than quantitative.[137] A committee of the Statistical Society of London (1841) (a source for Curtin 1989, Table 1.1), wrote "On the whole, the town is distinguished by its salubrity; and it is a remarkable fact, that notwithstanding that the settlement is surrounded by marshes, and is exposed to many of the causes which are usually supposed to create malaria, malignant remittant fever has not been known there since its formation" (p.139)[138]

With respect to Penang, the same committee wrote, "the salubrity is greatly superior to that of the Carnatic [here meaning the eastern coast, see Statistical Society of London (1840) p.115] or the province of Bengal" (p.140). Malacca is given a similarly positive assessment for "European constitutions" (p.141).

Within the Straits Settlements, the committee reports data on European troops for Penang only. From 1829-1838, the mean strength was 51 and there were 9 deaths, giving a death rate of 17.7 per 1,000. This is the rate in Curtin (1989), Table 1.1. We continue to use this rate for Malaysia and Singapore in our revised series.

Results using this revised robustness series are shown in column 2 of Table 3A. The result in the first two set of rows, with no covariates and with latitude, are almost the same as in column 1 (the original AJR results). The t-statistic has fallen slightly. There is a bigger effect in the specification without the neo-Europes, as here the recoding of Hong Kong means the t-statistic with clustering is just below 2. However, without Africa the results remain strong; the t-statistic is over 3 with clustering. Our results are also weaker with continent dummies and with malaria. With clustering, percent of European descent is significant (coefficient 0.017, s. e. 0.008), while settler mortality is not significant.

This series contains our original data for Africa, which Albouy argues contains estimates that are too high. We therefore cap the series at a rate of 250 per 1,000, which is the rate Tulloch found to be typical in West Africa in the early nineteenth century (Curtin, 1990, p. 67). As we mentioned in Section 3 above, West Africa was likely the highest mortality region for Europeans, so taking this cap for all of Africa seems reasonable.

The results are shown in column 3 of Table 3A. In all cases, log settler mortality is significant, typically with a larger t-statistic than in column 1; the effect of capping is similar to that shown in column 2 of Table 1B. The estimate without Africa is hardly changed (only Hong Kong is affected), so the effect is coming from reducing the variance within Africa. This is ironic given Albouy's claim that many of our African estimates are in some sense too high.

5.2 Further information for Africa

A key issue is obviously the relative mortality rates of West Africa, the West Indies, the East Indies, and North America. Some further, independent data is provided by Curtin (1969, chapter 10) who reports the death rates of Europeans trading out of Bristol over various routes in 1784.[139] The mortality per thousand per annum was 219 for the Slave trade (involving a

---

of our unhealthy colonies." He provides numerical examples from Europe (average annual mortality from the Walcheren campaign [in the Netherlands] of 1809 was 348.7; in the Peninsular, 1811-14, it was 160.9), as well as Ceylon (death rate 1818-19 of 218), and Burma (death rate in first year of war, 1824, of 485 per 1,000).

[137]The authors of this report co-operated with Tulloch (Statistical Society of London, 1840, p.114)

[138]They also say, p.139, that in other parts of the island, "it is stated that fevers and dysentery are frequent." This supports AJR's contention that Singapore was healthier than its immediate surroundings.

[139]Other evidence discussed in the same chapter by Curtin indicates that the death rates for these English crews in the slave trade were similar to those for French slave traders around the same time. It is also striking that while death rates for transported slaves fell during the century, they stayed high for crews, "Apparently

call in Africa and in the West Indies), 41 in the East India trade, 23 in the West India trade, and in the range 10-11 for the trade to Newfoundland, Greenland, and St. Petersburg. The very high mortality rate in the Slave trade was due, in Curtin's view, primarily to the greater risks of exposure to malaria and yellow fever; the additional mortality (compared with the West Indies or East Indies) is in line with AJR's series for settler mortality, and completely different from the relative rates in Albouy's barracks series. See also Curtin (1968).

5.3 FURTHER ROBUSTNESS CHECKS FOR SOUTH AMERICA

With regard to mortality in South America, Tulloch (1841) contains information that suggests a further reasonable way to construct alternative estimates. The South American Command ranged from the tip of the continent to the Equator (mouth of the Amazon) on the east coast and to 30 degrees north (almost the modern US border) on the west coast. For the most part, this naval force remained in harbour, as this was considered the most effective way to protect British trade (pp.15-16). Within the Command, Tulloch says, "all parts are alike healthy."

The convincing detail here is that, despite high temperatures, there was little fever and no yellow fever among British sailors. "Of the 10 harbours frequented by our vessels, 7 lie within the tropics, and the temperature is considerably higher than at Vera Cruz, Havannah, or Jamaica, where yellow fever abounds; yet among an average force of 2,465 men, only 23 deaths occurred by fever of all kinds, in the course of 7 years, being 1 3/10 per 1000 annually – a smaller proportion than either in the Mediterranean, or among the most select class of the civil population at home" (p.15).[140]

The death rate was low: 7.7 per 1000, which Tulloch says is perhaps "unparalleled in the annals of Medical Statistics." This would not, however, have been the mortality of soldiers because, as Tulloch (1841) argues at length for the Mediterranean, there are many reasons why soldiers died at higher rates.[141] In the Mediterranean, where the naval death rate was comparable, the soldiers' mortality rate was almost exactly twice as high (18 per 1000 for soldiers compared with 9.3 per 1000 for sailors.)[142]

This information suggests we can code mortality in coastal South America (other than parts in the West Indies/Caribbean) at a level of 15.4. This rate would apply to Brazil, Uruguay, Argentina, Chile, Peru, and Ecuador. Applying our principle of the lowest rate for a country would apply this also to Colombia and Mexico (both of which have territory that also borders the Caribbean).[143] Bolivia and Paraguay are land-locked, so this information is not helpful

---

the slave traders discovered ways to improve health conditions for slaves in transit, but they were less successful in meeting the dangers of malaria and yellow fever, the principal killers of strangers to the West African coast" (p.283).

[140]The ports were Rio de Janeiro, Bahia, Pernambuco (Recife), Para (Belem) in Brazil, Buenos Aires (Argentina), Valparaiso and Coquimbo in Chile, Callao (Peru), Panama, and San Blas (Mexico). See Medical Department of the Navy (1840, p.33 and 1841, p.39). This source warns that because ships move around, it is hard to infer the mortality of one particular place from the squadron's mortality rate.

[141]Sailors were invalided home more easily. Also, they served for terms of 3-4 years typically and had to pass a new medical before reenlisting. Sailors had a better officially-provided diet and higher pay.

[142]For sailors in the East India Command, operating primarily in the Bay of Bengal and adjoining areas, mortality for sailors from disease was 15.1 per 1,000 p.a.. Balfour (1845, pp.77-78) argues this can reasonably be compared with the mortality from disease for European soldiers stationed in Ceylon, for whom mortality was 46.2 per 1,000. This suggests the ratio of army to navy mortality from disease may be higher when there is more malaria present.

[143]In our revised series, we have taken Jamaica/Dominican Republic as our link between the Curtin and Gutierrez series, so this is not affected by changing the rate for Mexico. However, in this series we only use the Gutierrez data when we do not have direct relevant information from Tulloch.

and we continue to rely on the original AJR estimates.[144] Overall, this new data adds one cluster in our regressions.

In this revised series, there is a greater difference between mortality in the West Indies/Caribbean and in South America, compared with our original series. This fits with Tulloch's (1841) qualitative assessment, although this is based exclusively on naval data for most of South America.

Results using the revised AJR series with this new calculation for South America are shown in column 4 of Table 3A (i.e., this also includes the changes incorporated in column 2). Compared with column 2, the results do not change much. The coefficients are smaller in most rows, but the standard errors are typically lower also. The biggest difference is to the results without Africa, where the coefficient is now -0.72 instead of -1.02, and the standard error is unchanged at 0.30; the t-statistic with clustering is now 2.4.

When we cap this revised series at 250 in column 5, the results in the full sample with Africa are stronger. For example, with latitude as a covariate, the t-statistic with clustering is 2.8. Without the neo-Europes, the t-statistic with clustering is 3, and alongside malaria, the t-statistic on log settler mortality is 2.5.

5.4 RELATIVE MORTALITY OF SOLDIERS, OFFICERS, AND CIVILIANS

An important ingredient of Albouy's barracks series is the argument that the bishops data should not be benchmarked to the soldiers data, but rather incorporated directly, i.e., the mortality of bishops should be regarded as directly comparable to the mortality of soldiers, so data on bishops can be added to data on soldiers without any adjustment. This seems unreasonable, as bishops and soldiers lived under different conditions, and when they overlap, for example for Mexico and Jamaica/Dominican Republic, there is a three-fold difference between the mortality rates of bishops and soldiers).[145] Here we substantiate this point further by looking at the mortality rates of officers and civil servants. If we are correct that bishops, aged 40-49, had lower death rates than soldiers, we should expect to see lower death rates for middle aged civilians compared with soldiers in places where the data are available.

The British records contain information on two relevant comparisons with ordinary soldiers: officers, and civil servants. Tulloch (1838c, p.432), reports death rates per 1,000 of various ranks for officers in the Bengal Presidency.[146] At around that time, the death rate for ordinary soldiers in this region was 71.41 (Curtin, 1989, Table 1.1). The total mortality rate for officers was 31.2, with majors (average age 40) dying at a rate of 41 per 1,000 p.a. and the death rate for lieutenant colonels at 48.4 per 1,000.[147] Despite the fact that the death rates for officers are apparently calculated from total deaths, i.e., not just deaths from disease, they are

---

[144]Bolivia was not landlocked until it lost its coast to Chile as a result of the 1879 War of the Pacific, but the British naval data do not appear to pertain to that coastline. This method gives higher mortality in Bolivia and Paraguay than their neighbors, and this may not be reasonable. However, lowering their mortality estimates to that of their neighbors would not affect our results; nor would other reasonable reassignments of rates within Latin America.

[145]As far as we have been able to ascertain, the basic mortality rates from Tulloch exclude officers (e.g., see Abstract No. 1 of the Appendix to part 1 of Army Medical Department 1840). So our basic mortality data pertain to deaths from ordinary soldiers (unless otherwise noted).

[146]Tulloch does not state precisely the period covered by these mortality statistics, but it is probably 1817-36 or thereabouts.

[147]In contrast, officers and older civil servants had less, if any, of a mortality advantage relative to ordinary soldiers in the Madras Presidency, but this seems to have been an exception (Statistical Society of London, 1840, p.142). The reported death rates in this comparison may include all deaths, not just from disease.

significantly lower than soldiers' mortality rates.[148]

Also relevant is the death rate of British civil servants in the Bengal Presidency, for which Tulloch reports averages over the period 1790 to 1836. The death rate for those aged 40 to 45 was 35.4 and the rate for those aged 45 to 50 was 36.4. Note that these civil servants were allowed to return to England for 3 years after they had completed between 10 and 15 years' service, "which of course must have a material tendency in reducing the mortality" (Tulloch, 1838, p.432). As far as we know, bishops did not have the same opportunity.

Balfour (1849, p. 34) provides further support for the contention that the mortality of ordinary soldiers was always above that of European civilians at the same time and place. From 1809 to 1828, "Civilians" (meaning Europeans who were government or quasi-government employees) died at rates of 23.8 per 1,000 in Madras, 25.1 in Bengal, and 31.7 in Bombay.[149] The ratio of civilian to soldier mortality in India was in the range of 1.17 to 2.85.[150]

The death rate for Church Missionaries in Sierra Leone was calculated by Tulloch to have been about 170 per 1,000 p.a. in the early nineteenth century, at a time when the mortality of troops was two or three times higher (Army Medical Department 1840, pp.7-8).[151] The death rate for civilians in the Cape Coast Command was 120-130 per 1,000 p.a. just before a period when the troops' death rate was 668.3 per 1,000 (Army Medical Department, 1840, pp.19-20).

The death rate in Ceylon, 1824-36, was 54.5 per 1,000 for private soldiers and 33.2 for officers (Army Medical Department, 1841, p.50). About 7.6 per 1,000 of this officer mortality rate was likely not from disease. In the Windward and Leeward Command, 1818-36, when soldiers' mortality was 78.5, officers mortality was 42 per 1,000 p.a. (Army Medical Department, 1838, p.5 and p.73).

Overall, then, it would be reasonable to assume that the death rate for soldiers was at least twice as high as for civilians, where the rates for both are calculated as a percent of mean strength.[152] For bishops, in our view, the difference would likely to be closer to 3:1 than to 2:1, as their profession is generally less dangerous than that of military officers and they probably lived better than ordinary civil servants. This is in line with, although slightly lower than, the original AJR estimates for Latin America and well within the range considered by our robustness checks. Appendix 2 shows bishops' mortality rates in the range of 18.3 (low mortality region) to 39.8 (high mortality region); AJR's estimates for soldiers were in the range

[148]Tulloch (1838c) reports similar findings for the West Indies on p.437, "in years of ordinary salubrity... while the annual mortality of the troops generally was 78 1/2 per 1000 from ascertained diseases, in the Windward and Leeward Command, that of the officers was but 42; and in Jamaica, where the troops lost 121 per 1000 by ascertained diseases, the mortality of the officers amount to only 83 4/10 per 1000." But Tulloch is careful to point out that when epidemics struck, the officers had no advantage.

[149]Among comparable people in England, the death rate was 9.1 per 1,000 per year, 1801-32. So the mortality rate for European civilians in India was 2-3 times that in England, which is at the lower end of the range for India-England in military mortality (Curtin, 1989, Table 1.1, has an estimate of 15.3 for England, 48.63 in Madras, 71.41 in Bengal, and 36.99 in Bombay).

[150]Balfour (1849, e.g., Tables IV and V) reviews officers and soldiers relative mortality in British garrisons around the world. Soldiers typically had mortality around twice that of officers, with a higher ratio in higher mortality places. At mortality levels similar to those in Latin America, we see Canada (officers 10.9 and troops 20), Mauritius (officers 14.7 and troops 30.5), Ionian islands (officers 17.5 and troops 28.3), Ceylon (officers 33.2 and troops 54.5). Malta is an exception, with the two death rates very close (officers 16.9 and troops 18.7). In no case do officers and troops have the same death rates.

[151]It is hard to know the exact relative mortality because the period and precise locations differed. But Tulloch, the author of this report, was clear that the death rate for soldiers was generally higher than for civilians in Western Africa and most other high mortality places.

[152]The original AJR method for merging the Gutierrez and Curtin data implies higher relative death rates of bishops and soldiers: 3.9 for the low mortality region, for example, and 4.1 for the high mortality region.

68.9 to 163.3.

5.5 Declining Mortality During the Nineteenth Century

We also disagree with Albouy on the importance of the decline in mortality during the nineteenth century. Tulloch and his colleagues have data and assessments that speak directly to the issue. The main point here, which was emphasized by Curtin, is that until the careful measurement and analysis of military mortality, there was little systematic effort to reduce the death rate for soldiers. Measurement, in this case, changed the reality.

The Note by the Editor (of the Statistical Society of London), at the end of Tulloch (1838c, p.444), is most instructive. Tulloch's statistics "have already been the means of drawing the attention of Government to the condition of troops serving in the West Indies, and under the humane and intelligent directions of the Secretary-at-War, have led to many important ameliorations in their condition. Fresh provisions have been substituted for salt; improved barrack and hospital accommodation, and healthier localities have been provided for the white troops; and in those islands which proved so exceedingly unhealthy to them garrisons of black troops have been substituted."[153]

Tulloch (1847, pp.253-254) provides numbers supporting the argument that measures taken by the authorities reduced mortality across the board in colonies after the late 1830s. The decline in death rates for high mortality places were particularly impressive (p.254), with declines of 50% or more in the West Indies, Jamaica, and Ceylon.

Why did military mortality decline so steadily during the nineteenth century? In part this was the product of the very measures we are using. Once mortality was measured systematically and convincingly, it became obvious that reducing mortality was both desirable and possible. Following the first Tulloch reports there were major changes in the terms of service, food, accommodation and other services provided to soldiers (Balfour 1872, p.1 and pp.19-20; see also Curtin, 1989, p.44).[154] For example, the practice of sending invalids home became more common after the 1850s. "This meant those who would have died on the spot a decade or so earlier were "invalided" instead and may well have died elsewhere" (Curtin, 1986, p.378).

Note that while Balfour and others accurately located health spas, their observations did not change the mortality rate faced by potential European settlers in most of Jamaica, India, Ceylon and elsewhere. Determining how death rates varied with precise location could reduce military mortality, as soldiers could be stationed a few hours or days march from where they might be needed. But it did not affect the attractiveness of a place to European settlers – in fact, knowing more exactly (and publishing prominently) that only a few hill towns were healthy for Europeans likely discouraged more general settlement.

---

[153]Troop substitution was an important tool for managing the death rate of Europeans. Speaking of the high death rate in Hong Kong, Tulloch (1847, p.255) says "Arrangements being now in progress for having the duty performed by Malays in that colony, instead of Europeans, it may be expected that no further necessity will exist, after the present year, for so great an expenditure of life."

[154]Balfour was one of the pioneers in understanding how to reduce military mortality. In some of his early work, Balfour (1845b, p.200) recommended moving troops to places where the new measures showed mortality was lower, for example within Jamaica. This was apparently done in 1844 (Balfour, 1845b, p.204). Within Ceylon, the death rate varied from 23 in Galle to 97.1 in Badulla (Balfour, 1845b, p.204). Balfour recommended increasing the military contingent at Niuera Elia, where the death rate was 24. Balfour appears to have been the first person to point out this systematic difference. Mortality in some hill stations of India was less than in England, at least for European children (Balfour, 1845b, p.205) and the same would likely have been true for troops (p. 206); see also Balfour (1847). Not surprisingly "convalescent stations" were placed in these spots.

6   Further Results on Weak Instruments

Finally, we have also looked further into the issue of weak instruments. The results presented in Tables 2 and 3 show that using confidence intervals that are robust to the problem of weak instruments makes little difference to our results, and almost in all cases we can exclude a coefficient of zero. In addition, the F-statistic for the first-stage instrument is generally high, in most specifications higher than the level of (approximately) 9 suggested in Stock, Wright and Yogo (2002, Table 1). However, when we use a large number of covariates simultaneously, in particular, dummies for continents, latitude and control for the current disease environment or for the percent of the population of European descent, the first-stage relationship becomes significantly weaker and weak instruments problems emerge. We do not find this surprising, since with only 64 data points, there is only a limited amount of variation in the data and the current disease environments and the fraction of the population of European descent are potentially endogenous variables (see footnote 9).

The issue of weak instruments is only a concern when using Albouy's data series. Using even slightly corrected version of Albouy's barracks series shows no evidence of weak instrument problems in our main specifications, for example, judged by the AR confidence intervals.

We have also used different approaches to gauge the issue of weak instruments. In particular, we followed the new tests suggested by Hahn and Hausman (2002), which compare various backward and forward instrumental-variable regressions of the outcome variable on the endogenous regressor (or the other way around). To implement these tests, we used the fact that we have information on percent of the population in 1900 who were of European descent, which we previously used as a proxy for European settlements and as an additional instrument to check the validity of our approach. Using this variable and the most recent version of the Hahn and Hausman (2002) procedure, we again found no evidence for weak instruments.[155] Interestingly, the Hahn-Hausman test not only indicates that there is no weak instrument problem with our original series or with the corrected Albouy series, but it also shows that there is no weak instrument problem with Albouy's original campaign series, and a weak instrument problem arises with Albouy's original barracks series only in the specification that controls for the fraction of the population of European descentin 1975. The details of these tests are available upon request.

7   Conclusion

In this response, we evaluated Albouy's criticisms of our data and results. Our conclusion is that there is no foundation to any of the criticisms raised by Albouy. At best, they reflect a long list of mistakes on his part in coding and selecting data. We also used additional data collected since the publication of our paper to show new results similar to those reported in the original AJR paper.

We do not claim that we have looked at all the available primary data sources. There are numerous potential sources that could contain new material on the mortality rates faced by potential European settlers. We would be very interested in seeing work that carefully incorporates these data. Unfortunately, Albouy's work does not attempt or accomplish this.

---

[155]We thank Jerry Hausman for discussion and suggestions on this point.

8 References

Acemoglu, Daron, Simon Johnson, and James Robinson (2000) "The Colonial Origins of Comparative Development: An Empirical Investigation," NBER Working Paper 7771, June.

Acemoglu, Daron, Simon Johnson, and James Robinson (2001) "The Colonial Origins of Comparative Development: An Empirical Investigation," *The American Economic Review*, Vol 91, No. 5, December, 1369-1401.

Adams, George Worthington (1952) *Doctors in Blue: The Medical History of the Union Army in the Civil War*, Louisiana State University Press, Baton Rouge.

Albouy, David (2004a) "The Colonial Origins of Comparative Development: A Reinvestigation of the Data," University of California – Berkeley, July.

Albouy, David (2004b) "The Colonial Origins of Comparative Development: A Reexamination Based on Improved Settler Mortality Data," University of California – Berkeley, December.

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

Anonymous (1898) "Army Medical Efficiency" *The British Medical Journal*, October 1, 991-992.

Army Medical Department (1838) *Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in the West Indies*, London.

Army Medical Department (1839) *Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in the United Kingdom, the Mediterranean, and British America*, London.

Army Medical Department (1840) *Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in Western Africa, St. Helena, the Cape of Good Hope, and the Mauritius*, London.

Army Medical Department (1841) *Statistical Report on the Sickness, Mortality, and Invaliding among Her Majesty's Troops serving in Ceylon, the Tenasserim Provinces, and the Burmese Empire*, London.

Army Medical Department (1853) *Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in the United Kingdom, the Mediterranean, and British America*, London.

Army Medical Department (1862) *Statistical Report on the Health of the Army in 1860*, London.

Austin, M. (1979) *The Army in Australia 1840-50: Prelude to the Golden Years*, Australian Government Publishing Service, Canberra.

Balfour, T. Graham (1845a) "Comparison of the Sickness, Mortality, and Prevailing Diseases among Seamen and Soldiers, as Shown by the Naval and Military Statistical Reports" *Journal of the Statistical Society of London*, Vol. 8, No. 1, March, 77-86.

Balfour, T. Graham(1845b) "Statistical Data for Forming Troops and Maintaining them in Health in Different Climates and Localities" *Journal of the Statistical Society of London*, Vol. 8, No. 3, September, 193-209.

Balfour, T. Graham (1847) *Statistical Report on the Sickness and Mortality among the Troops serving in the Madras Presidency*, Stark & Company, Edinburgh.

Balfour, T. Graham (1849) "Additional Observations on the Means of Maintaining

Troops in Health" *Journal of the Statistical Society of London*, Vol. 12, No. 1, February, 33-42.

Balfour, T. Graham (1861) *Statistical Report on the Health of the Army in 1859*, Army Medical Department, London.

Balfour, T. Graham (1872) "Comparative Health of Seamen and Soldiers, as Shown by the Naval and Military Statistical Reports" *Journal of the Statistical Society of London*, Vol. 35, No. 1, March, 1-24.

Bolton, Herbert Eugene and Thomas Maitland Marshall (1971) *The Colonization of North America: 1492-1783*, Hafner Publishing Company, New York.

Cantlie, Sir Neil (1974) *A History of the Army Medical Department, Volume I*, Churchill Livingstone, Edinburgh and London.

Chernozhukov, Victor and Christian Hansen (2005) "A Simple and Practical Approach to Heteroskedasticity and Autocorrelation Consistent Inference with Weak Instruments," Working paper, University of Chicago, January 20.

Cohen, William B. (1983) "Malaria and French Imperialism," *Journal of African History*, 24(1), 23-35.

Committee of Inquiry (1867) "Report on the Causes of Reduced Mortality in the French Army Serving in Algeria," Presented to both Houses of Parliament, War Office, Great Britain, George Edward Eyre and William Spottiswoode for Her Majesty's Statistical Office.

Curtin, Philip D. (1961) ""The White Man's Grave:" Image and Reality, 1780-1850," *The Journal of British Studies*, Vol. 1, no, 1, November, 94-110.

Curtin, Philip D. (1964) *The Image of Africa,* University of Wisconsin Press, Madison.

Curtin, Philip D. (1968) "Epidemiology and the Slave Trade," *Political Science Quarterly,* 83:181-216.

Curtin, Philip D. (1969) *The Atlantic Slave Trade: A Census*, University of Wisconsin Press, Madison.

Curtin, Philip D. (1975a) *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade*, The University of Wisconsin Press, Madison.

Curtin, Philip D. (1975b) *Economic Change in Precolonial Africa: Supplementary Evidence*, The University of Wisconsin Press, Madison.

Curtin, Philip D. (1985) "Medical Knowledge and Urban Planning in Tropical Africa," *The American Historical Review*, Vol. 90, No. 3, June, 594-613.

Curtin, Philp D. (1986) "African Health at Home and Abroad," *Social Science History*, 10:4, Winter, 369-398.

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

Curtin, Philip D. (1990) "The End of the "White Man's Grave"? Nineteenth-Century Mortality in West Africa," *Journal of Interdisciplinary History*, Vol. 21, No. 1, Summer, 63-88.

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

Curtin, Philip D., Steven Feierman, Leonard Thompson, and Jan Vansina (1995) *African History: From Earliest Times to Independence, Longman, London and New York*, Second Edition.

Davies, K.G. (1975) "The Living and the Dead: White Mortality in West Africa, 1684-1732," in *Race and Slavery in the Western Hemisphere: Quantitative Studies*, edited by Stanley

L. Engerman and Eugene D. Genovese, Princeton University Press, Princeton, 83-98.

Everest, Allan S. (1981) *The War of 1812 in the Champlain valley*, Syracuse University Press, Syracuse.

Feinberg, H.M. (1974) "New Data on European Mortality in West Africa: The Dutch on the Gold Coast, 1719-1760," *The Journal of African History*, Vol. 15, No. 3, 357-371.

Graham, Gerald S. (1978) *The China Station: War and Diplomacy 1830-1860*, Clarendon Press, Oxford.

Gregory, Desmond (1996) *Malta, Britain, and the European Powers, 1793-1815*, Associated University Presses, Cranbury, NJ.

Gutierrez, Hector (1986) "La Mortalite des Eveques Latino-Americains aux XVIIe et XVIII Siecles," *Annales de Demographie Historique*, 29-39.

Hahn, J. and J. Hausman (2003) "Weak Instruments: Diagnosis and Cures in Empirical Econometrics," *American Economic Review*, 93(2), 118-125.

Kennedy, J. (1970) *A History of Malaya*, Macmillan, St Martin's Press, London.

Kiple, Kenneth F. (1984) *The Caribbean Slave: A Biological History*, Cambridge University Press, Cambridge.

Lawson, Thomas (1840) *Statistical Report on the Sickness and Mortality in the Army of the United States*, Surgeon-General's Office.

Lever, J.W.C. (1839) "On the Sickness and Mortality Among the Troops in the United Kingdom. Abstract of the Statistical Report of Major Tulloch." *Journal of the Statistical Society of London*, Vol. 2, No. 4, July, 250-260.

Medical Department of the Navy (1840) *Statistical Reports on the Health of the Navy [part I]*, Parliamentary paper No. 159, Great Britain, London.

Medical Department of the Navy (1841) *Statistical Reports on the Health of the Navy for the years 1830, 1831, 1832, 183, 1834, 1835, and 1836*, William Clowes and Sons, London.

Reynaud, Gustave A (1898) *Considerations sanitaires sure l'expedition de Madagascar et quelques autres expeditions coloniales, francaises et anglais*, Paris, Societe Francaise d'Edition d'Art.

Ricklefs, M.C. (2001) *A History of Modern Indonesia since c.1200*, Third Edition, Stanford University Press, Stanford.

Select Committee (1866) *Report from the Select Committee on Mortality of Troops (China)*, House of Commons, Parliament, Great Britain.

Showers, Victor (1979) *World Facts and Figures*, John Wiley and Sons, New York.

Shryock, Richard H. (1962) "A Medical Perspective on the Civil War," *American Quarterly*, Summer, 14(2), 161-173.

Staiger, Douglas and James H. Stock (1997) "Instrumental Variables Regression with Weak Instruments," *Econometrica*, May, 65(3), 557-586.

Stock, James H., Jonathan H. Wright and Motohiro Yogo (2002) "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments" *Journal of Business and Economics Statistics*, Vol. 20, 518-529.

Statistical Society of London (1840) "Report of a Committee of the Statistical Society of London, Appointed to Collect and Enquire into Vital Statistics, Upon the Sickness and Mortality Among the European and Native Troops Serving in the Madras Presidency, from the Year 1793 to 1838," *Journal of the Statistical Society of London*, Vol. 3, No. 2, July, 113-143.

Statistical Society of London (1841) "Second Report of a Committee of the Statistical Society of London, appointed to collect and enquire into Vital Statistics, upon the Sickness and Mortality among the European and Native Troops serving in the Madras Presidency," *Journal of the Statistical Society of London*, Vol. 4, No. 2, July, 137-155.

Tulloch, A.M. (1838a) "On the Sickness and Mortality Among the Troops in the West Indies [part I]," *Journal of the Statistical Society of London*, Vol. 1, No. 3, July, 129-142.

Tulloch, A.M. (1838b) "On the Sickness and Mortality Among the Troops in the West Indies [part II]," *Journal of the Statistical Society of London*, Vol. 1, No. 4, August, 216-230.

Tulloch, A.M. (1838c) "On the Sickness and Mortality Among the Troops in the West Indies [part III]," *Journal of the Statistical Society of London*, Vol. 1, No. 7, November, 428-444.

Tulloch, A.M. (1841) "Comparison of the Sickness, Mortality, and Prevailing Disease among Seamen and Soldiers, as Shewn by the Naval and Military Statistical Reports," *Journal of the Statistical Society of London*, Vol. 4, No. 1, April, 1-16.

Tulloch, A.M. (1847) "On the Mortality Among Her Majesty's Troops Serving in the Colonies during the years 1844 and 1845," *Journal of the Statistical Society of London*, Vol. 10, No. 3, September, 252-259.

Wells, Robert V. (1975) *The Population of the British Colonies in America before 1776*, Princeton University Press, Princeton.

9 Appendix 1: Albouy's recoding and Gutierrez's three mortality zones

Gutierrez identifies three mortality regions in Latin America, low, medium, and high, based on average temperature (for which his source is Showers 1979). Albouy claims that we misclassified countries to regions.[156] Albouy (2004b) states (p.10) that Gutierrez "presents no catalogue of countries as AJR suggest." In fact, on pp.34-35 Gutierrez lists many cities in Latin America and assigns them to mortality regions. He is not clear on all the cities in his medium mortality region, but he is specific on the temperature range for this region (between 20 and 24.9 degrees C) and on p.33 identifies his source on temperatures clearly as Showers (1979).[157]

We (AJR and Albouy) agree on the following 10 countries: in the low mortality region, Argentina, Chile, Ecuador, Mexico, Uruguay; in the medium mortality region, Paraguay, El Salvador, and Venezuela; and in the high mortality region, Nicaragua and Panama.

We disagree on the following seven countries. Of these, in five cases our "low" mortality region coding becomes medium in Albouy's coding and in two cases our "medium" mortality region coding becomes high in Albouy's coding.

- Bolivia: low (AJR); medium (Albouy)

- Brazil: low (AJR); medium (Albouy)

- Colombia: low (AJR); medium (Albouy)

- Costa Rica: medium (AJR); high (Albouy)

- Guatemala: low (AJR); medium (Albouy)

- Honduras: medium (AJR); high (Albouy)

- Peru: low (AJR); medium (Albouy)

In 5 cases (Bolivia, Brazil, Colombia, Guatemala and Peru) we are following Gutierrez directly, i.e., he explicitly names one or more of the main cities in that country as being in his low mortality region. So there is no reason for Albouy to recode. However, in these cases his recoding makes no real difference, as Albouy is just moving a low coding to a medium coding, and both sides agree there was only a small difference in mortality between these two regions.

So the issue is only Costa Rica and Honduras, which we code as medium and Albouy codes as high. Here we coded based on Showers' reported temperature for the capital city, matching this to the medium region in Gutierrez classification. Temperatures in Showers are in Fahrenheit; conversions to centigrade are by the authors. The rate for Costa Rica is 68.5 F, i.e., 20.3 C. This is for San Jose, the only Costa Rican city in Showers' data. The rate for Honduras is 70.3 F, i.e., 21.3 C. Here we use data from Tegucigalpa, the capital.

It is true that the Parker data show higher average temperatures for these two countries. But the right temperature is not the issue – the issue is whether the countries were in regions defined by Gutierrez as medium temperature. Gutierrez used Showers and so did we.

---

[156]Albouy also says, p. 10, "AJR's own classification is not explained," while we feel it was plain that we were following Gutierrez. He also says he uses "the more transparent classification of countries according to temperatures in Parker (1997)," while we feel what we did is perfectly transparent and replicable – we followed Gutierrez as closely as we could.

[157]To preempt any complaint about this source, let us be clear that the 1979 edition of Showers work contains all the relevant temperature data (this is not true, for example, of the 3rd edition.) Gutierrez cites the 1979 edition.

We now discuss the conversion of mortality ratios (e.g., from Guttierrez) into mean strength mortality rates (i.e., the standard form for Curtin's data). To do this, assume that all individuals are homogeneous and face the death rate of d, which means that they die at the Poisson rate d, or equivalently that during a time interval of Δt, the probability that they die is $d \cdot \Delta t + o(\Delta t)$ (where $o(\Delta t)$ goes to zero faster than Δt as $\Delta t \to 0$).

Denote the number of deaths from a population that is always at x during a time interval of T by $D(x,T)$, which clearly implies:

$$E(D(x,T)) = d \cdot x \cdot T$$

Thus if $T = 1$ corresponds to one year, then $1000d$ is mortality per 1000 mean strength. The problem therefore is to estimate d.

First note that d is not the number we obtain from simple mortality rates, since these give the death rates of the population that starts at some level x (without replacement). Therefore, what we need to do is to calculate $D_M(x,T)$, the number of deaths from a population that starts at x during time interval T. Let us divide the time interval T into $T/\Delta t$ segments to obtain an approximation to $D_M(x,T)$,

$$
\begin{aligned}
D_M^{\Delta t}(x,T) \quad \approx \quad & d \cdot \Delta t \cdot x \\
& + d \cdot \Delta t \cdot x \cdot (1 - d \cdot \Delta t) \\
& + d \cdot \Delta t \cdot x \cdot (1 - d \cdot \Delta t)^2 \\
& + ... \\
& + d \cdot \Delta t \cdot x \cdot (1 - d \cdot \Delta t)^{T/\Delta t}
\end{aligned}
$$

Intuitively, after the first interval, $d \cdot \Delta t \cdot x$ people have died, so there are $x - d \cdot \Delta t \cdot x$ people left 'at-risk", and $x \cdot d \cdot \Delta t \cdot (1 - d \cdot \Delta t)$ of those die, so that after the second interval, there are $x \cdot (1 - d \cdot \Delta t) - x \cdot d \cdot \Delta t \cdot (1 - d \cdot \Delta t) = x \cdot (1 - d \cdot \Delta t)^2$ people left, etc.

Moreover, as $\Delta t \to 0$

$$
\begin{aligned}
D_M(x,T) = \lim_{\Delta t \to 0} D_M^{\Delta t}(x,T) \quad &= \quad \lim_{\Delta t \to 0} \left[d \cdot \Delta t \cdot x \cdot \left(\frac{1 - (1 - d \cdot \Delta t)^{\frac{T}{\Delta t}+1}}{1 - (1 - d \cdot \Delta t)} \right) \right] \\
&= \quad x \cdot \lim_{\Delta t \to 0} \left(1 - (1 - d \cdot \Delta t)^{\frac{T}{\Delta t}+1} \right)
\end{aligned}
$$

This implies

$$
\begin{aligned}
D_M(x,T) \quad &= \quad x - x \cdot \lim_{\Delta t \to 0} (1 - d \cdot \Delta t)^{\frac{T}{\Delta t}+1} \\
&= \quad x - x \cdot \lim_{\Delta t \to 0} \exp \left[\ln (1 - d \cdot \Delta t)^{\frac{T}{\Delta t}+1} \right] \\
&= \quad x - x \cdot \exp \left[\lim_{\Delta t \to 0} \left(\left(\frac{T}{\Delta t} + 1 \right) \ln (1 - d \cdot \Delta t) \right) \right] \\
&= \quad x - x \cdot \exp \left[\lim_{\Delta t \to 0} (T + \Delta t) \times \lim_{\Delta t \to 0} \frac{\ln (1 - d \cdot \Delta t)}{\Delta t} \right]
\end{aligned}
$$

Applying L'Hopital's rule to the second term, we have

$$D_M(x, T) = x - x \cdot \exp\left[T \lim_{\Delta t \to 0}\left(\frac{-d/(1 - d \cdot \Delta t)}{1}\right)\right]$$
$$= x \cdot \left(1 - e^{-d \cdot T}\right)$$

which is the formula we want. Now from $D_M(x, T)$, we can obtain d. Notice that d does not have natural units, it is in units of whatever is normalized to $T = 1$. For comparability with a Curtin's numbers, we take $T = 1$ to correspond to one year.

Given this formula, we can easily calculate d and mean strength per thousand death rates. In particular, from Gutierrez, we have the $D_M(1000, 10)$ as equal to 167 in low-temperature areas, 175 in medium-temperature areas and 328 in high-temperature areas. Applying our formula, we have for low-temperature areas: $0.167 = 1 - e^{-10d_L}$. Taking logs on both sides and rearranging, we obtain

$$d_L \simeq 0.01827$$

or 18.3 per thousand per year. For medium-temperature areas, we have $0.175 = 1 - e^{-10d_M}$, which implies

$$d_M \simeq 0.01923$$

or 19.2 per thousand per year. Finally, for high-temperature areas, $0.328 = 1 - e^{-10d_H}$, which gives:

$$d_H \simeq 0.03975,$$

or 39.7 per thousand per year.

This implies that the ratios that should be used for benchmarking the Gutierrez and Curtin data are:

$$\frac{\text{high}}{\text{low}} \simeq \frac{3975}{1827} \simeq 2.2$$
$$\frac{\text{medium}}{\text{low}} \simeq \frac{1923}{1827} \simeq 1.1.$$

11   APPENDIX 3: DOES AJR'S SERIES CONTAIN COMPARABLE DATA?

Albouy claims that AJR mix peacetime and non-peacetime data in a way that produces a non-comparable series. We have covered most of the details above, but here review the key datapoints at issue.

For the West Indies, Albouy claims our data are for wartime, but he is wrong. He does not dispute that our data are peacetime for the U.S., Canada, New Zealand, and South Asia. For Egypt and Sudan we use peacetime numbers. These happen to follow a campaign, but this is often the case with military mortality statistics.

One observation at issue is that of Mexico. This is important as this country provides the benchmarking for the original AJR series. But for Mexico there is a comparable peacetime estimate from a source that Albouy introduces (Adams, 1952), even though Albouy himself ignores this number. Also, as discussed in Sections 3 and 4 we have other ways of benchmarking the Gutierrez and Curtin series, and these give similar results.

So the peacetime issue comes down to: West Africa, North Africa (other than Egypt), Vietnam, Ethiopia, and Hong Kong. For West Africa, as we showed in detail in Section 4, the non-peacetime estimates are for small expeditions for which the European soldiers were

46

usually provided with transportation by steamer or mule, and locals often carried the heavy loads. These are very close to being peacetime experiences – keep in mind that soldiers "in barracks" did not generally sit around doing nothing, they marched, exericised, and impressed locals with various demonstrations of their firepower.

For Algeria we present a new peacetime number, in Section 5, which is in line with our original series for Algeria, Tunisia, and Morocco. For Hong Kong there is a new peacetime number that is quite different from our original series. Again the implications are discussed in Section 5.

However, for Vietnam and Ethiopia, it is reasonable to drop these observations as an additional robustness check. It is also possible that the Java War experience makes the Indonesia data less than fully comparable. So we can also drop that observation. As mentioned in Section 5, it is not clear what are the correct data for Hong Kong, so we can start by dropping that observation.

Without these four observations, our first stage has 60 observations, and the estimate for the effect of log settler mortality on institutions today is -0.636 (up in absolute terms from -0.613 using AJR's original series), with a clustered standard error of 0.183 (up from 0.174). Without Africa, the coefficient estimate is now -1.39 (compared with -1.21 in AJR's base sample) and the clustered standard error is 0.16 (compared with 0.18). Without the neo-Europes, the coefficient is -0.40 (compared with -0.4) and the clustered standard error is 0.18 (compared with 0.17).

If we use our revised data series, from Section 5, which includes the new Hong Kong data and other changes (so we are comparing with column 2 in Tables 3A,B), there are 61 observations. The first stage coefficient without covariates is -0.60 (compared with -0.56) and the clustered standard error is 0.172 (compared with 0.169). Without Africa, the estimated coefficient is -1.12 (compared with -1.02) and the clustered standard error is 0.33 (compared with 0.30). Without the neo-Europeans, the coefficient is -0.36 (compared with -0.33) and the standard error is 0.168 (compared with 0.172).

Table 1A
First Stage Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Alternative series for settler mortality			
	Original AJR series	Alternative AJR series	Albouy barracks series	Albouy barracks series, without assignment to neighbors in West Africa	Albouy barracks series, consistent West African assignment to neighbors	Albouy barracks series, American correction	Albouy barracks series, consistent West African assignment to neighbors plus American correction
			Dependent variable is protection against risk of expropriation				
No covariates	-0.61	-0.60	-0.59	-0.80	-0.68	-0.98	-1.03
(standard error)	(0.13)	(0.13)	(0.30)	(0.32)	(0.23)	(0.24)	(0.22)
(clustered standard error)	(0.17)	(0.17)	(0.31)	(0.32)	(0.23)	(0.25)	(0.22)
Number of clusters	36	34	22	22	22	20	20
Number of observations	64	64	43	41	49	43	49
With latitude	-0.52	-0.50	-0.41	-0.62	-0.53	-0.90	-0.97
(standard error)	(0.14)	(0.14)	(0.31)	(0.33)	(0.25)	(0.28)	(0.26)
(clustered standard error)	(0.19)	(0.19)	(0.33)	(0.33)	(0.25)	(0.27)	(0.24)
Number of clusters	36	34	22	22	22	20	20
Number of observations	64	64	43	41	49	43	49
Without neo-Europes	-0.40	-0.39	-0.16	-0.34	-0.36	-0.48	-0.59
(standard error)	(0.13)	(0.13)	(0.26)	(0.29)	(0.22)	(0.26)	(0.25)
(clustered standard error)	(0.17)	(0.17)	(0.21)	(0.23)	(0.15)	(0.26)	(0.21)
Number of clusters	33	31	18	18	18	16	16
Number of observations	60	60	39	37	45	39	45
Without Africa	-1.21	-1.25	-0.97	-0.97	-0.94	-1.38	-1.38
(standard error)	(0.22)	(0.23)	(0.41)	(0.41)	(0.40)	(0.27)	(0.27)
(clustered standard error)	(0.18)	(0.21)	(0.43)	(0.43)	(0.43)	(0.22)	(0.22)
Number of clusters	19	17	18	18	18	16	16
Number of observations	37	37	35	35	35	35	35
With continent dummies	-0.44	-0.43	-0.41	-0.73	-0.61	-0.87	-0.88
(standard error)	(0.17)	(0.17)	(0.33)	(0.34)	(0.28)	(0.29)	(0.26)
(clustered standard error)	(0.20)	(0.21)	(0.29)	(0.29)	(0.20)	(0.34)	(0.27)
Number of clusters	36	34	22	22	22	20	20
Number of observations	64	64	43	41	49	43	49
With percent of European descent in 1975	-0.42	-0.40	-0.53	-0.33	-0.27	-0.77	-0.78
(standard error)	(0.14)	(0.14)	(0.32)	(0.35)	(0.27)	(0.25)	(0.23)
(clustered standard error)	(0.19)	(0.19)	(0.26)	(0.38)	(0.29)	(0.25)	(0.24)
Number of clusters	36	34	22	22	22	20	20
Number of observations	64	64	43	41	49	43	49
With malaria	-0.52	-0.50	-0.12	-0.79	-0.82	-1.14	-1.14
(standard error)	(0.18)	(0.18)	(0.33)	(0.43)	(0.41)	(0.26)	(0.26)
(clustered standard error)	(0.22)	(0.22)	(0.36)	(0.42)	(0.43)	(0.25)	(0.25)
Number of clusters	35	33	21	21	21	19	19
Number of observations	62	62	41	39	47	41	47

OLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Originalsettler mortality series and covariates are from AJR (2001); Albouy series is from Albouy (2004b). Construction of alternative series is explained in section 3 of the text and summarized here in the column heading.

Table 1B
First Stage Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Alternative series for settler mortality						
	Unweighted averages		*Mixing weighted and unweighted*	*Unweighted averages*			
	Original AJR series	Original AJR series, capped at 250	Albouy campaign series	Albouy campaign series, correct American data	Albouy campaign series, with expeditions in Africa	Albouy campaign series, with expeditions in Africa (capped)	Albouy campaign revision, correct American data and expeditions in Africa (capped)
	Dependent variable is protection against risk of expropriation						
No covariates	-0.61	-0.94	-0.53	-0.66	-0.67	-0.73	-0.73
(standard error)	(0.13)	(0.16)	(0.21)	(0.20)	(0.17)	(0.18)	(0.17)
(clustered standard error)	(0.17)	(0.18)	(0.30)	(0.26)	(0.22)	(0.22)	(0.19)
Number of clusters	36	36	31	29	31	31	31
Number of observations	64	64	64	64	57	57	57
With latitude	-0.52	-0.86	-0.33	-0.47	-0.53	-0.58	-0.60
(standard error)	(0.14)	(0.19)	(0.22)	(0.21)	(0.18)	(0.20)	(0.19)
(clustered standard error)	(0.19)	(0.20)	(0.31)	(0.27)	(0.24)	(0.21)	(0.20)
Number of clusters	36	36	31	29	31	31	31
Number of observations	64	64	64	64	57	57	57
Without neo-Europes	-0.40	-0.66	-0.19	-0.25	-0.44	-0.48	-0.44
(standard error)	(0.13)	(0.18)	(0.21)	(0.21)	(0.17)	(0.18)	(0.18)
(clustered standard error)	(0.17)	(0.20)	(0.27)	(0.24)	(0.22)	(0.19)	(0.17)
Number of clusters	33	33	28	27	28	28	28
Number of observations	60	60	60	60	53	53	53
Without Africa	-1.21	-1.21	-1.26	-1.05	-1.40	-1.40	-1.13
(standard error)	(0.22)	(0.22)	(0.37)	(0.28)	(0.37)	(0.37)	(0.28)
(clustered standard error)	(0.18)	(0.18)	(0.25)	(0.33)	(0.22)	(0.22)	(0.32)
Number of clusters	19	19	17	16	16	16	16
Number of observations	37	37	37	37	36	36	36
With continent dummies	-0.44	-0.81	-0.15	-0.34	-0.40	-0.44	-0.49
(standard error)	(0.17)	(0.22)	(0.23)	(0.22)	(0.21)	(0.23)	(0.20)
(clustered standard error)	(0.20)	(0.25)	(0.29)	(0.27)	(0.23)	(0.23)	(0.20)
Number of clusters	36	36	31	29	31	31	31
Number of observations	64	64	64	64	57	57	57
With percent of European descent in 1975	-0.42	-0.73	-0.17	-0.31	-0.41	-0.45	-0.48
(standard error)	(0.19)	(0.19)	(0.21)	(0.21)	(0.18)	(0.20)	(0.19)
(clustered standard error)	(0.18)	(0.20)	(0.29)	(0.26)	(0.25)	(0.24)	(0.22)
Number of clusters	36	36	31	29	31	31	31
Number of observations	64	64	64	64	57	57	57
With malaria	-0.52	-0.92	-0.17	-0.43	-0.43	-0.48	-0.58
(standard error)	(0.18)	(0.22)	(0.25)	(0.24)	(0.25)	(0.27)	(0.24)
(clustered standard error)	(0.22)	(0.23)	(0.36)	(0.30)	(0.31)	(0.30)	(0.25)
Number of clusters	35	35	30	28	31	31	31
Number of observations	62	62	62	62	56	56	56

OLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Originalsettler mortality series and covariates are from AJR (2001); Albouy series is from Albouy (2004b). Construction of alternative series is explained in section 3 of the text and summarized here in the column heading.

Table 2A
Second Stage Regressions

	(1)	(2)	(3)	(4)
		Alternative series for settler mortality		
	Original AJR series	Alternative AJR series	Albouy barracks series	Albouy barracks series, consistent West African assignment to neighbors plus American correction
	Dependent variable is log GDP per capita in 1995			
No covariates	0.93	0.96	1.40	0.76
AR confidence interval	[0.66,1.40]	[0.72,1.48]	[-oo,-51.25]U[0.80,oo]	[0.52,1.16]
AR confidence interval, clustered	[0.66,1.84]	[0.67,1.99]	[-oo,-9.87]U[0.72,oo]	[0.41,1.33]
F-stat, first stage	23.34	22.23	3.92	21.67
F-stat, first stage, clustered	12.45	11.78	3.53	21.51
With latitude	0.96	1.01	1.81	0.70
AR confidence interval	[0.65,1.79]	[0.69,1.95]	[-oo,-2.27]U[0.84,oo]	[0.42,1.25]
AR confidence interval, clustered	[0.62,2.90]	[0.66,3.38]	[-oo,-1.81]U[0.74,oo]	[0.29,1.26]
F-stat, first stage	13.48	12.78	1.70	13.69
F-stat, first stage, clustered	7.30	6.93	1.52	16.05
Without neo-Europes	1.24	1.30	4.10	1.03
AR confidence interval	[0.79,3.15]	[0.82,3.77]	[-oo,-1.24]U[1.10,oo]	[0.51,4.28]
AR confidence interval, clustered	[0.79,7.39]	[0.75,8.13]	[-oo,-1.79]U[1.08,oo]	[0.25,3.37]
F-stat, first stage	8.89	8.76	0.38	5.79
F-stat, first stage, clustered	5.54	5.49	0.58	7.77
Without Africa	0.61	0.65	0.91	0.49
AR confidence interval	[0.41,0.87]	[0.45,0.94]	[0.57,4.10]	[0.29,0.72]
AR confidence interval, clustered	[0.43,0.89]	[0.44,1.02]	[0.45,8.42]	[0.35,0.62]
F-stat, first stage	30.62	28.28	5.69	25.58
F-stat, first stage, clustered	45.98	35.65	5.12	40.14
With continent dummies	0.97	1.00	1.21	0.88
AR confidence interval	[0.59,3.32]	[0.60,3.97]	[-oo,-3.36]U[0.67,oo]	[0.62,1.67]
AR confidence interval, clustered	[0.49, 10.08]	[0.50, 27.42]	[0.72,151.84]	[0.60, 1.97]
F-stat, first stage	6.49	6.00	2.76	11.86
F-stat, first stage, clustered	4.68	4.33	4.36	9.82
With percent of European descent in 1975	0.92	0.99	3.93	0.62
AR confidence interval	[0.56,2.35]	[0.60,2.95]	[-oo,-0.46]U[0.75,oo]	[0.33,1.27]
AR confidence interval, clustered	[0.51, 6.91]	[0.55, 38.04]	[-oo,-0.23]U[0.72,oo]	[0.13, 1.44]
F-stat, first stage	8.67	7.57	0.14	11.23
F-stat, first stage, clustered	4.92	4.27	0.12	10.31
With malaria	0.62	0.66	0.96	0.47
AR confidence interval	[0.32,1.42]	[0.35,1.73]	[-oo,-3.92]U[0.52,oo]	[0.28,0.72]
AR confidence interval, clustered	[0.27, 2.24]	[0.29, 2.77]	[-oo,-2.65]U[0.60,oo]	[0.32, 0.65]
F-stat, first stage	8.64	7.52	3.13	18.98
F-stat, first stage, clustered	5.63	5.34	3.18	20.75

2SLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Originalsettler mortality series and covariates are from AJR (2001); Albouy series is from Albouy (2004b). Construction of alternative series is explained in section 3 of the text and summarized here in the column heading; the first stage for columns 1 through 3 in the corresponding column of Table 1A, the first stage for column 4 is in column 7 of Table 1A.

Table 2B
Second Stage Regressions

	(1)	(2)	(3)	(4)
	Alternative series for settler mortality			
	Unweighted averages		*Mixing weighted and unweighted*	*Unweighted averages*
	Original AJR series	Original AJR series, capped at 250	Albouy campaign series	Albouy campaign revision, correct American data and expeditions in Africa (capped)
No covariates	0.93	0.82	1.27	0.90
AR confidence interval	[0.66,1.40]	[0.62,1.14]	[0.81,4.51]	[0.66,1.40]
AR confidence interval, clustered	[0.66,1.84]	[0.60,1.23]	[-∞,-4.70]U[0.71,∞]	[0.60,1.91]
F-stat, first stage	23.34	35.55	6.37	19.12
F-stat, first stage, clustered	12.45	28.09	3.07	10.30
With latitude	0.96	0.79	1.62	0.95
AR confidence interval	[0.65,1.79]	[0.55,1.26]	[-∞,-3.06]U[0.81,∞]	[0.62,2.01]
AR confidence interval, clustered	[0.62,2.90]	[0.54,1.24]	[-∞,-0.68]U[0.69,∞]	[0.54,3.17]
F-stat, first stage	13.48	21.82	2.24	10.14
F-stat, first stage, clustered	7.30	19.26	1.07	6.29
Without neo-Europes	1.24	1.04	2.98	1.27
AR confidence interval	[0.79,3.15]	[0.67,2.05]	[-∞,-1.53]U[1.11,∞]	[0.76,4.92]
AR confidence interval, clustered	[0.79,7.39]	[0.63,2.27]	[-∞,-0.58]U[1.00,∞]	[-∞,-36.03]U[0.66,∞]
F-stat, first stage	8.89	13.22	0.82	6.08
F-stat, first stage, clustered	5.54	11.27	0.49	3.79
Without Africa	0.61	0.61	0.67	0.62
AR confidence interval	[0.41,0.87]	[0.41,0.89]	[0.40,1.23]	[0.39,0.99]
AR confidence interval, clustered	[0.43,0.89]	[0.43,0.89]	[0.37,1.19]	[0.40,0.93]
F-stat, first stage	30.62	30.62	11.48	16.36
F-stat, first stage, clustered	45.98	45.98	25.16	16.48
With continent dummies	0.97	0.78	2.57	0.90
AR confidence interval	[0.59,3.32]	[0.52,1.45]	[-∞,-0.45]U[0.79,∞]	[0.55,3.02]
AR confidence interval, clustered	[0.49, 10.08]	[0.42, 1.56]	[-∞,-0.11]U[0.62,∞]	[-∞,-0.65.40]U[0.44,∞]
F-stat, first stage	6.49	13.32	0.40	6.25
F-stat, first stage, clustered	4.68	10.61	0.26	3.90
With percent of European descent in 1975	0.92	0.71	2.20	0.87
AR confidence interval	[0.56,2.35]	[0.44,1.29]	[-∞,-0.73]U[0.73,∞]	[0.49,3.00]
AR confidence interval, clustered	[0.51, 6.91]	[0.33,1.27]	[-∞,-0.14]U[0.56,∞]	[-∞,-11.50]U[0.35,∞]
F-stat, first stage	8.67	15.32	0.64	6.35
F-stat, first stage, clustered	4.92	12.92	0.35	3.67
With malaria	0.62	0.50	1.58	0.56
AR confidence interval	[0.32,1.42]	[0.28,0.83]	[-∞,∞]	[0.21,1.80]
AR confidence interval, clustered	[0.27, 2.24]	[0.23,0.77]	[-∞,∞]	[-∞,∞]
F-stat, first stage	8.64	18.49	0.46	5.82
F-stat, first stage, clustered	5.63	16.71	0.23	3.40

2SLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Originalsettler mortality series and covariates are from AJR (2001); Albouy series is from Albouy (2004b). Construction of alternative series is explained in section 3 of the text and summarized here in the column heading; the first stage for columns 1 through 3 in the corresponding column of Table 1B, the first stage for column 4 is in column 7 of Table 1B.

Table 3A
First Stage Regressions

	(1)	(2)	(3)	(4)	(5)
		Alternative series for settler mortality			
	Original AJR series	AJR series with robustness data from Tulloch	AJR series with robustness data from Tulloch, capped at 250	AJR series with robustness data from Tulloch and new alternative estimates Latin America	AJR series with robustness data from Tulloch and new alternative estimates Latin America, capped at 250
	Only coefficient and standard error for log settler mortality are reported				
No covariates	-0.61	-0.56	-0.87	-0.50	-0.70
(standard error)	(0.13)	(0.13)	(0.17)	(0.12)	(0.15)
(clustered standard error)	(0.17)	(0.17)	(0.19)	(0.15)	(0.19)
Number of clusters	36	38	38	39	39
Number of observations	64	64	64	64	64
With latitude	-0.52	-0.45	-0.75	-0.39	-0.59
(standard error)	(0.14)	(0.15)	(0.20)	(0.13)	(0.17)
(clustered standard error)	(0.19)	(0.19)	(0.23)	(0.17)	(0.21)
Number of clusters	36	38	38	39	39
Number of observations	64	64	64	64	64
Without neo-Europes	-0.40	-0.33	-0.55	-0.31	-0.46
(standard error)	(0.13)	(0.14)	(0.19)	(0.12)	(0.15)
(clustered standard error)	(0.17)	(0.17)	(0.21)	(0.13)	(0.15)
Number of clusters	33	34	34	35	35
Number of observations	60	60	60	60	60
Without Africa	-1.21	-1.02	-1.05	-0.72	-0.73
(standard error)	(0.22)	(0.25)	(0.25)	(0.23)	(0.23)
(clustered standard error)	(0.18)	(0.30)	(0.29)	(0.30)	(0.30)
Number of clusters	19	21	21	22	22
Number of observations	37	37	37	37	37
With continent dummies	-0.44	-0.36	-0.66	-0.34	-0.54
(standard error)	(0.17)	(0.17)	(0.22)	(0.16)	(0.19)
(clustered standard error)	(0.20)	(0.20)	(0.25)	(0.16)	(0.19)
Number of clusters	36	38	38	39	39
Number of observations	64	64	64	64	64
With percent of European descent in 1975	-0.42	-0.33	-0.60	-0.26	-0.44
(standard error)	(0.14)	(0.16)	(0.21)	(0.15)	(0.20)
(clustered standard error)	(0.19)	(0.20)	(0.25)	(0.19)	(0.24)
Number of clusters	36	38	38	39	39
Number of observations	64	64	64	64	64
With malaria	-0.52	-0.41	-0.78	-0.35	-0.60
(standard error)	(0.18)	(0.19)	(0.23)	(0.17)	(0.21)
(clustered standard error)	(0.22)	(0.22)	(0.27)	(0.19)	(0.24)
Number of clusters	35	36	36	37	37
Number of observations	62	62	62	62	62

OLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Originalsettler mortality series and covariates are from AJR (2001). Construction of alternative series is explained in section 5 of the text and summarized here in the column heading.

Table 3B
Second Stage Regressions

	(1)	(2)	(3)	(4)	(5)
			Alternative series for settler mortality		
	Original AJR series	AJR series with robustness data from Tulloch	AJR series with robustness data from Tulloch, capped at 250	AJR series with robustness data from Tulloch and new alternative estimates Latin America	AJR series with robustness data from Tulloch and new alternative estimates Latin America, capped at 250
No covariates	0.93	0.94	0.81	0.99	0.89
AR confidence interval	[0.66,1.40]	[0.67,1.52]	[0.59,1.19]	[0.71,1.66]	[0.64,1.35]
AR confidence interval, clustered	[0.66,1.84]	[0.64,1.98]	[0.57,1.28]	[0.69,2.12]	[0.63,1.59]
F-stat, first stage	23.34	17.96	26.19	16.74	22.17
F-stat, first stage, clustered	12.45	11.18	20.86	11.04	14.12
With latitude	0.96	0.98	0.78	1.06	0.90
AR confidence interval	[0.65,1.79]	[0.62,2.23]	[0.50,1.38]	[0.68,2.66]	[0.58,1.77]
AR confidence interval, clustered	[0.62,2.90]	[0.59,5.03]	[0.45,1.36]	[0.67,5.41]	[0.58,2.10]
F-stat, first stage	13.48	9.58	14.65	8.81	12.00
F-stat, first stage, clustered	7.30	5.44	10.99	5.83	7.88
Without neo-Europes	1.24	1.31	1.08	1.32	1.14
AR confidence interval	[0.79,3.15]	[0.76,6.20]	[0.63,2.95]	[0.80,4.58]	[0.70,2.91]
AR confidence interval, clustered	[0.79,7.39]	(-∞,-37.16]U[0.69,∞)	[0.55,3.87]	[0.78,8.22]	[0.70,2.93]
F-stat, first stage	8.89	5.89	8.10	6.99	8.92
F-stat, first stage, clustered	5.54	3.92	6.57	5.48	8.98
Without Africa	0.61	0.55	0.55	0.66	0.66
AR confidence interval	[0.41,0.87]	[0.28,0.89]	[0.30,0.89]	[0.36,1.30]	[0.37,1.29]
AR confidence interval, clustered	[0.43,0.89]	[0.24,0.85]	[0.27,0.85]	[0.32,2.38]	[0.35,2.22]
F-stat, first stage	30.62	16.28	17.00	10.08	10.43
F-stat, first stage, clustered	45.98	11.32	12.69	5.57	5.83
With continent dummies	0.97	0.88	0.66	0.87	0.68
AR confidence interval	[0.59,3.32]	[0.44,15.14]	[0.34,1.45]	[0.45,7.97]	[0.35,1.62]
AR confidence interval, clustered	[0.49, 10.08]	(-∞,-2.70]U[0.21,∞)	[0.00,1.47]	[0.32,12.83]	[0.16,1.32]
F-stat, first stage	6.49	4.39	8.76	4.69	8.06
F-stat, first stage, clustered	4.68	3.28	6.99	4.39	8.25
With percent of European descent in 1975	0.92	0.93	0.63	1.11	0.77
AR confidence interval	[0.56,2.35]	[0.46,11.76]	[0.27,1.62]	(-∞,-3.86]U[0.52,∞)	[0.31,7.82]
AR confidence interval, clustered	[0.51, 6.91]	(-∞,-1.57]U[0.41,∞)	[-0.13,1.55]	(-∞,-0.88]U[0.49,∞)	(-∞,∞)
F-stat, first stage	8.67	4.58	8.02	2.96	4.63
F-stat, first stage, clustered	4.92	2.63	5.85	1.81	3.28
With malaria	0.62	0.57	0.43	0.68	0.53
AR confidence interval	[0.32,1.42]	[0.14,3.27]	[0.13,0.84]	[0.25,220.58]	[0.20,1.28]
AR confidence interval, clustered	[0.27, 2.24]	(-∞, ∞)	[-0.24, 0.71]	(-∞,-1.08]U[0.01,∞)	[-0.06, 1.10]
F-stat, first stage	8.64	4.97	11.11	4.02	7.95
F-stat, first stage, clustered	5.63	3.52	8.37	3.32	6.04

2SLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Originalsettler mortality series and covariates are from AJR (2001). Construction of alternative series is explained in section 5 of the text and summarized here in the column heading; the first stages are in the corresponding columns of Table 3A.