Hither Thou Shalt Come, But No Further: Reply to "The Colonial Origins of Comparative Development: An Empirical Investigation: Comment"*

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Abstract

David Albouy expresses three main concerns about the results in Acemoglu, Johnson and Robinson (2001) on the relationship between potential settler mortality and institutions. First, there is a general concern that there are high mortality outliers, potentially affecting this relationship, with which we agree. However, limiting the effect of outliers has no impact on our substantive results and if anything significantly strengthens them, in fact making them robust to even extreme versions of his other critiques. His second argument that all the data from Latin America and much of the data from Africa, making up almost 60% of our sample, should be dropped is arbitrary - there is a great deal of well-documented comparable information on the mortality of Europeans in those places during the relevant period. His third argument that a "campaign" dummy should be included in the first stage is at odds with the historical record and is implemented inconsistently; even modest corrections undermine his claims.

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> 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, emphasis added).
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> 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 [in *Diseases in Hot Countries* in 1768] 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).

1 Introduction

In Acemoglu, Johnson, and Robinson, henceforth AJR, (2001), we advanced the hypothesis that the mortality rates faced by Europeans in different parts the world after 1500 affected their settlements and choice of colonization strategy. Places that were relatively healthy (for Europeans) were - when they fell under European control - more likely to receive better economic and political institutions. In contrast, places where European settlers were less likely to go were more likely to receive "extractive" institutions. We also posited that this early pattern of institutions has persisted over time and influences the extent and nature of institutions around the world today. On this basis, we proposed using estimates of potential European settler mortality as an instrument for institutional variation around the world today.

Data on settlers themselves are unfortunately patchy - particularly because not many went to places they believed, with good reason, to be most unhealthy. We therefore followed the lead of Philip Curtin (1989 and 1998) who compiled data on the death rates faced by European soldiers in various overseas postings.[1] Curtin's data were based on pathbreaking statistical work that was initiated by the British military in the mid-19th century. These data became part of the foundation of both the contemporary thinking about public health (for soldiers and for civilians) and the life insurance industry (as it considered the risks inherent in overseas travel), and shaped the perceptions of Europeans - including potential settlers and their medical

[1] The data are also appealing because - at the same point in time - soldiers tend to live under fairly similar conditions in different countries, i.e., in a military cantonment or camp of some kind. Also, while conditions changed as medical knowledge advanced, Curtin and other sources provide a great deal of detail regarding what military doctors knew, when they knew it, and when they were able to get commanding officers to implement health-improving reforms. Curtin (1998) is particularly good on such details.

advisers.[2]

In his comment on AJR (2001), David Albouy (2011) focuses on one part of our argument.[3] Specifically, he raises three main concerns regarding our underlying data on early potential European mortality rates: (1) the highest measured rates are too high relative to the actual or perceived potential mortality for Europeans (e.g., p.2, p.4, pp.7-8 and in his Appendix); (2) our Latin American and some of our African data are unreliable (e.g., pp.4-5); and (3) the data are not consistent because some are taken from military "campaigns" (pp.6-9). Albouy proposes strategies for dealing with the latter two concerns. In particular, he discards *completely* almost 60% of our sample, and codes a "campaign" dummy. His comment argues that each of these strategies separately weakens our results and together they undermine our first stage results sufficiently that our instrument (potential European settler mortality) becomes unhelpful for determining whether institutions affect income today.

Albouy's first concern is an important one with which we wholeheartedly agree - and had emphasized in our original working paper version, AJR (2000).[4] It is reasonable to worry that some of our highest mortality estimates may be too high because of epidemics, unusual conditions, or small sample variation, and may thus not be representative of mortality rates that would ordinarily have been expected by soldiers or settlers. This concern was our main rationale for using the logarithm of mortality rates (to reduce the impact of outliers; see AJR, 2000, 2001).[5] In AJR (2001), we argued that such variation could be viewed as a form of measurement error, and provided that it did not significantly deviate from classical measurement error, would not create an asymptotic bias for our IV procedure. In AJR (2005), we instead used the alternative and, we now believe, superior strategy of capping mortality estimates at 250 per 1000 (with replacement),[6] which was suggested by A.M. Tulloch, the

[2] We augmented the data from Curtin with estimates of bishops' mortality from Gutierrez (1986) benchmarked to overlapping mortality rates from Curtin. Using these approaches, we were able to compute estimates of potential settler mortality for 72 countries. 64 of these modern countries, which also had other key data used in our analysis, made up the base sample in AJR (2001).

[3] While his current comment differs considerably from the 2006 version (which in turn was different from the 2005 variant, which itself was quite different from both the 2004 vintages), the conclusions remain the same (Albouy 2004a, 2004b, 2005, 2006). As we have rebutted his various points (see AJR 2004, 2005, 2006, and 2008), Albouy has shifted his logic, his data, and his regressions while continuing to assert the same findings. We will not repeat our responses to issues on which Albouy has already retreated and instead here focus on his published critique.

[4] AJR (2000) contained a long list of robustness checks motivated by this and related issues, including on how to best benchmark Latin American data to Curtin's data (see in particular Table 5 there). These were not ultimately published in AJR (2001) due to space constraints. Albouy's initial comment on our paper did not cite AJR (2000) and the robustness checks therein (Albouy 2004a). Though he now cites AJR (2000), there is less than full acknowledgment that that our original robustness checks dealt with many of the issues he raises.

[5] Other strategies we employed to deal with this issue in AJR (2000) included constructing alternative African series, using information from "long" data series from Curtin. See Table 5 in AJR (2000).

[6] We follow Curtin and the 19th century literature by reporting mortality per 1000 *mean strength* or *with*

leading authority of the day, as the maximum mortality in the most unhealthy part of the world for Europeans (see Curtin, 1990, p.67, Tulloch, 1840, p.7).[7] This modification has no substantive effect on the findings in AJR (2001); in fact, it strengthens them, so that even extreme versions of Albouy's other modifications leave the results largely robust. This we believe is the most important bottom line: *Results from AJR (2001) are highly robust and they become more robust once one takes steps to limit the effect of high mortality outliers.*[8]

Albouy's other concerns are greatly exaggerated and his suggested approaches are arbitrary and inconsistent. Once one limits the effect of very high mortality rates, these other strategies used by Albouy are largely inconsequential, but it is important to set the record straight. To deal with the second concern, Albouy simply labels a large part of our original data, mostly from Latin America and Africa, as "unreliable" and discards them - reducing the sample size to 28 from our original 64. This is despite the fact that we have a great deal of information about mortality - and how it was perceived by Europeans - in those countries, and much of this was documented in our earlier work.

Ordinary Europeans, European military establishments, the medical profession and the extensive life insurance industry were not only interested in mortality rates around the world but published considerable relevant information.[9] Our original coding and the additional robustness checks reported in AJR (2005) are based on and consistent with this information. Simply discarding data is certainly not the optimal way of dealing with this wealth of information, especially in view of the paucity of data on early European settler and soldier mortality rates. We repeat below robustness checks from AJR (2000) and also show that the main results from AJR (2001) are robust to incorporating existing information on mortality rates in different ways.

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replacement, meaning that the mortality rate refers to the number of soldiers who would have died in a year if a force of 1,000 had been maintained in place for the entire year. Throughout all mortality rates are per 1000 mean strength or with replacement.

[7] Two important points are worth noting. First, 250 per 1000 is still an exceptionally high mortality rate. Potential settlers were definitely deterred by the prospect that about 20% of their number would die within the first year. After early attempts ended in tragedy for would-be settlers, Europeans viewed much of Africa as the "White Man's Grave" and did not seriously attempt to build settlements there. Second, capping potential settler mortality or its logarithm should not create any inconsistency. Our assumption is that potential settler mortality is orthogonal to the second stage error term. If so, any monotone transformation thereof would also be orthogonal to this error term and thus a valid instrument.

[8] The results are very similar if, instead of the 250 per 1,000 per annum mortality, we use alternative caps such as 100, 350, or 400.

[9] The information was available in medical and public health discussions (see AJR 2005, 2006, and 2008). It was also manifest in the life insurance literature. Europeans were understandably interested in the mortality rates for themselves in all other parts of the world - and there was a well-informed debate around this issue throughout the 19th century. The information involved was imperfect and evolved over time, but Europeans definitely had expectations regarding mortality rates almost everywhere.

One needs to throw out almost 60% of our sample, as Albouy does, in order to obtain different results. And even those regressions turn out to be largely driven by one outlier, Gambia, which has an unusually favorable coding of its institutions standing in contrast with its recent history and becomes much more consequential in the smaller sample. Limiting the effect of high mortality outliers by capping mortality at 250 per 1000 per annum or also excluding the outlier Gambia makes our results robust even when one does not use any of the observations that Albouy objects to (i.e., with just respectively 28 and 27 observations).

Albouy's third concern is that some of our data are taken from military campaigns while others are not. To deal with this, he proposes to introduce a coding for whether or not our data are drawn from a "campaign" and to use that dummy in the first stage regression. However, there is little difference in practice between what soldiers were doing during most colonial "campaigns" and other times, and it does not in general make sense - and is not possible - to distinguish campaigns and non-campaigns. The more important problem here, however, is the manner in which this is done. Albouy is highly inconsistent in his coding, and even minor corrections to the way in which he codes this dummy restores the robustness of our results. In addition, once again, limiting the effect of very high mortality rates largely restores the robustness of our results even without correcting the inconsistencies in his coding.

Albouy proposes a number of other adjustments to our data, including adjusting how we use mortality rates from Mali and proposing alternative ways to use new data we introduced in AJR (2005). Both these issues are inconsequential as we have already shown in detail (AJR 2005, 2006, and 2008); consequently these points will be discussed only briefly below.

The rest of the paper is organized as follows. Section 2 reviews our hypothesis and data we used to test it; we also briefly present our original results across a range of specifications. Section 3 considers each of Albouy's three concerns in turn. Section 4 concludes. Appendixes A, B and C, which are available online, provide further details on econometrics, data for contested observations, and historical background.

2 Background

2.1 Theory and Data

The main focus of AJR (2001) was to estimate the causal effect of a broad cluster of institutions on long-run development. Briefly, we argued that 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 against 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 and settlers tried to replicate or extend European institutions, with great emphasis on private property and checks against government and elite power. These colonial institutions have tended to persist. This choice of colonization strategy was in turn naturally influenced by the feasibility of settlements - 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. Based on this reasoning, we suggested that the *potential* mortality rates expected by early European settlers in the colonies could be an instrument for current institutions in these countries.

Of course, by its nature, *potential* settler mortality is often not observed.[10] 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 sometimes hard to find - and we should worry about whether these groups were demographically similiar (e.g., in terms of age structure or social background). 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 by the fact that Philip Curtin in a series of works, most notably Curtin (1989) and Curtin (1998), but also Curtin (1961, 1964), reported comparable data on the disease mortality rates of European soldiers stationed in various colonies. Curtin also took a view on how Europeans perceived mortality in various parts of the world - and discussed how this view was shaped by the available data over time.

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 data are *peacetime* rates - they are definitely not from major wars involving massed armies and large-scale casualties. Curtin (1989, 1998) emphasized that mortality rates declined through the 19th century as European militaries became better at managing health issues. In particular,

[10] Albouy still complains that data do not come from actual settlers (p.2). But AJR (2000, 2001) were very clear that these were *potential* settler mortality rates, and of course, Europeans did not and should not have settled in places where the annual mortality rates run in the range of 20% or higher.

after 1850 there were dramatic declines in military mortality from disease in the tropics (see, e.g., the contrast between Tables 1.1 and 1.2 in Curtin, 1989). 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.[11]

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. In the absence of such a number, we used the earliest *expedition* mortality.[12] 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 on the basis of other information that Europeans faced similar mortality rates in two countries but only one of them had an estimate, we assigned the mortality rate from one country to the other.[13]

Appendix B discusses the various sources we can use to evaluate whether the disease ecologies are sufficiently similar to reasonably assign a mortality rate from one country to its modern neighbor. We use the historical and contemporary literature on historical geography, both in the form of text and maps. We also include the relevant medical literature - because this speaks to the issue of conditions under which some diseases, such as malaria, become prevalent. The 19th century literature on life insurance is also helpful on some key points.

In AJR (2000), we provided a detailed analysis of an alternative series without this type of assignment and also some other robustness checks (see, e.g., Table 5, columns 1-4). 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 (though other mortality estimates, such as for Ethiopia, were very low). Our use of logarithm of mortality rates was in part motivated by these very high mortality rates.

The most important gap in Curtin's data is for Latin America. Curtin reported estimates for the Caribbean, but for Central and South America, his work contained estimates only for

[11] From the perspective of our theoretical framework, we really needed potential settler mortality before 1800 - during the formative period of colonization for most of these places. 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).

[12] 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. Curtin (1998) reviews data from a number of these experiences in Africa, including against the Ashanti and in Ethiopia.

[13] In constructing our dataset we prefered simplicity and transparency. Albouy contends that we do not have any information about countries to which mortality is "assigned" from neighbors. This is incorrect, as we showed in our earlier replies (AJR 2005, 2006, 2008). We summarize this additional information in Section 3.

Mexico.[14] To supplement the numbers from Curtin, we used an article by Hector Gutierrez (1986) on the mortality rates of bishops in Latin America (i.e., Central and South America, including some data on the Caribbean).[15] Naturally, the mortality rates of bishops and soldiers were unlikely to be the same: bishops presumably resided in more comfortable and sanitary conditions than soldiers in barracks; they could escape epidemics more easily; and overall they must have had a much higher standard of living. When the series overlap, the Gutierrez mortality estimates are lower than the Curtin estimates. To create a comparable series, we therefore benchmarked the mortality rates of bishops to those of soldiers.[16] Gutierrez provides 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 - which made for estimates that were more plausible, given the available qualitative evidence.[17] In AJR (2000), these issues were extensively discussed and we reported that our results were robust using either type of benchmarking (see again below).

2.2 Baseline Results

The first stage relationship in AJR (2001) 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 brevity, Table 1A 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 AJR (2001). The first set of rows has no additional covariates in the regression, the second set of rows includes latitude, the third set drops the neo-Europes (the USA, Canada, Australia, and New Zealand), the fourth set drops all of Africa, the fifth set

[14] 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.

[15] 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.

[16] Namely, we combined 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.

[17] This choice seemed less favorable to our hypothesis and thus preferable on these grounds. Our checks using the Dominican Republic/Jamaica number indicated slightly 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 the alternative results, using the Dominican Republic/Jamaica for the benchmarking, in Tables 1A and 1B (columns 3 and 4).

includes continent dummies, the sixth set includes continent dummies and latitude, the seventh set includes the percent of the population in 1975 that was of European descent, and the eighth set of rows includes malaria (see AJR for the reasons to focus on these specifications). These are the specifications which Albouy also discusses - our rows match the columns in his Tables 2 and 3, with the exception that we also report results without any African data.[18]

We should note that as discussed in AJR (2000, 2001), the last row specification that includes current prevalence of malaria is highly problematic and is likely to bias results against finding both a significant first stage and second stage relationship because current prevalence of malaria is endogenous, generally driven by institutional and income per capita differences. We included this specification in AJR (2000, 2001) for completeness but noted the potential bias that the specification would create against us was a serious concern.[19] As a matter of fact, this was the least robust specification in AJR (2001) as the results in Table 1A,B here also show. In what follows, unsurprisingly, this will be the main specification where Albouy's strategies sometimes lead to less robust results.

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 potential settler mortality estimates in the AJR (2001) sample are inferred from mortality rates in neighboring countries.[20]

Table 1A begins with first stage results using the original AJR data (column 1), corresponding to columns 1 of Table 4 of AJR (2001).[21] 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.[22]

[18] We drop the African data because in an earlier comment Albouy did the same (although now he has retreated from this critique).

[19] In particular, we wrote: "Since malaria was one of the main causes of settler mortality, our estimate may be capturing the direct effect of malaria on economic performance. We are skeptical of this argument since malaria prevalence is highly endogenous; it is the poorer countries with worse institutions that have been unable to eradicate malaria." (p. 1391). We also provided examples of richer countries with better institutions successfully eradicating malaria, including the U.S. eliminating it from the Panama Canal zone and Australians from Queensland. Acemoglu and Johnson (2007) provide additional evidence that differences in malaria prevalence today are unlikely to account for significant differences in income per capita across countries.

In addition, Albouy uses a malaria variable which is different from the one in AJR (2000, 2001) and the provenance of which is unclear. In what follows, we consistently use the original data from AJR (2000, 2001).

[20] Such clustering may be viewed as somewhat conservative since we have quantitative and qualitative corroborating evidence from other sources on mortality rates on all the countries in our sample (for example, from the literature on life insurance, part of which was discussed in AJR, 2005).

[21] This matches column 9 in Table 3 of AJR (2001).

[22] 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 mortality rates "with replacement" comparable with the base data from Curtin. In Appendix 2 of AJR (2005) we showed that the

Across the broad range of other specifications in Table 1A our first stage results are similar. The parameter point estimate does not move much across rows. When we drop the neo-Europes, the estimated coefficient is smaller but the standard error is also reduced. Without Africa, the results become significantly stronger. Table 1B shows the equivalent second stage results, in which we regress log GDP per capita in 1995 on institutions, which log settler mortality as the instrument. In AJR (2001), we followed the prevailing practice at the time and reported standard errors. Here we instead report the Anderson-Rubin (AR) 95 percent confidence set (allowing for non-spherical error structure due to clustering and heteroscedasticity), which is consistent when the first stage may be weak (see, for example, Chernozhukov and Hansen, 2005).[23] For the baseline estimate, this confidence set has a lower bound of 0.66 and an upper bound of 1.72, around a point estimate of 0.93.

The two exceptions are the specification with continent dummies and latitude and the one with malaria. In the former case, with clustered standard errors, the coefficient on settler mortality is -0.35 and the standard error is 0.19 in the first stage. In the second stage, the Anderson-Rubin confidence interval is the union of two disjoint and unbounded intervals: $[-\infty, -4.72]$ and $[0.44+, \infty]$ (or in fact $[-\infty, -27.23]$ and $[0.57, +\infty]$ without clustering). As also argued by Chernozhukov and Hansen (2005), the lower interval is irrelevant: not only does it not even include the point estimate, 1.07, but such large negative estimates make neither economic nor econometric sense. Therefore, we interpret this as evidence that the 95 percent confidence set excludes zero and reasonable negative estimates, allowing us to statistically reject the hypothesis that institutions have no effect on GDP per capita. To be sure, such a confidence interval is still a sign of relatively imprecise estimates, since it is much wider than the confidence sets in our other specifications - though it still enables us to reject the hypothesis that the second stage coefficient is zero. The pattern is similar with malaria - the confidence set consists of two disjoint intervals, but still rejects a zero coefficient.

Throughout the rest of the paper, whenever the AR confidence interval consists of two disjoint intervals, one of them a subset of negative reals, we only report the upper interval to save space in the tables (in each case, whenever the reported confidence interval extends to positive infinity on the right, it should be clear that there is a negative disjoint interval in the negative reals).

In summary, the different specifications in column 1 of Table 1B confirm the results in

exact ratios should be 1, 1.1, and 2.2. This does not make any difference, within 2 significant figures, to our results in column 1.

[23] Our AR confidence intervals do not always match those reported by Albouy. This seems to be a consequence of his use of an insufficiently fine grid. Our procedure is described in Appendix A.

AJR (2001) that institutions have a significant positive effect on income per capita, though in specifications that include continent dummies and latitude together and malaria, confidence sets are quite wide.

3 Response to Albouy's Concerns

3.1 Concerns About Very High Mortality Rates

As we noted in AJR (2000), some of the data, particularly from Africa, may have had excessively high mortality rates. In the Appendix to AJR (2000) we reviewed the origin of these data - and flagged clearly when they were due to epidemics. If epidemics occured with some regularity - or if they were rare and yet still affected European perceptions of mortality for settlers - such mortality data should be included for our purposes. But if these epidemics were one-off or seen as rare, then including them introduces additional, perhaps significant measurement error.

In column 2 of Table 1A we show the effects of capping mortality at 250 per 1,000 per annum. This is the rate that Tulloch, the pioneer in this area, estimated to be average European soldier mortality rate "for West Africa in general" from 1792 through 1840 (Curtin, 1990, p.67; see Tulloch, 1840, p7).[24] Tulloch and his colleagues also regarded that region as the most unhealthy part of the world for Europeans in the early 19th century. And of course, 250 per 1000 per annum is still a *very high* mortality rate, sufficient to discourage anybody but the most reckless from permanent settlement (see footnote 7 in the Introduction).

In column 2, for the base specification in the first set of rows, the coefficient on log settler mortality in the first stage increases in absolute value to -0.94 (compared with -0.61 in column 1), 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).[25] Now in all cases, the AR confidence sets for the second stage are much more precisely estimated, and never extend to infinity and always exclude zero. We should also note that these results are not specific to capping the potential settler mortality rate at 250. Using caps of 100, 350 and 400 leads to very similar results (see column 6 of Table 1B in AJR, 2005).

In summary, we agree that some of the mortality estimates from Curtin are too high, partly driven by unusual conditions, the impact of epidemics, or small samples. In AJR (2000,

[24] This rate of 250 per 1,000 is also close to the rate of 209 per 1,000 per annum for officers stationed in Sierra Leone and Cape Coast Command, 1819-36, on p.37 in Balfour (1849); ordinary soldiers had a higher death rate.

[25] 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.

2001), we discussed this issue at length and used logarithms to reduce the impact of these very high mortality rates. In AJR (2005), we went one step further and following the information consistent original sources (in particular Tulloch's original research), we capped mortality rates in 250 (per 1000 per annum). In the analysis below, for all relevant specifications we also show results including the mortality cap at 250. As we discuss, this mortality cap typically strengthens our results. In fact, it typically makes our approach impervious to Albouy's other critiques. That is, even if we were to accept these other critiques, *which we definitely do not*, with this mortality cap they do not undermine - or even much impact - our main findings.

3.2 Does Discarding Data Make Sense? Latin America

Albouy claims that we lack any reliable data for 36 countries in our base AJR sample. He drops those countries completely in Panels B and D in his Table 2 (first stage) and Table 3 (second stage), running regressions with just 28 countries. Of the 36 countries which Albouy drops, 16 are in Central and South America. These were coded using the Gutierrez procedure discussed above. In this subsection, we discuss these 16 countries, returning to the remaining 20 countries in the next subsection.

Albouy is concerned that our Latin American data are not reliable because he does not like the particular way we benchmark Gutierrez data with Curtin data.[26] We agree that results using this procedure should be subject to robustness checks. This was the approach in AJR (2000) and in all subsequent work.

Column 3 reports results using an alternative series. This was discussed but not explicitly shown in AJR (2001). It was later shown in detail in AJR (2005). In this series, we offer an alternative way of linking the Curtin and Gutierrez datasets. Specifically, instead of benchmarking using Mexico, we use Jamaica/Dominican Republic.[27] We continue to assign countries to mortality regions as in AJR (2001).[28] With this alternative benchmarking, the results are

[26] He also argues, e.g., around Appendix Table A2, that we simply have no idea about relative mortality in South and Central America. But as we now discuss, in addition to the evidence from Gutierrez, there is quantitative evidence on relative mortality in South and Central America from British South American naval stations and from life insurance rates for sailors in South America from Institute of Actuaries (1851-52). In addition, Institute of Actuaries (1851-52) and Hunter (1907) indicate that the life insurance industry took a clear view on mortality in this region relative to other regions and also on how mortality varied between countries. This view is entirely consistent with our benchmarked data.

[27] In the original AJR (2001) 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 per annum) 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 columns 3 and 4. As in AJR (2001), we use the relative mortality ratios of 1, 1.1, and 2.3 between Gutierrez's three regions (see footnote 22).

[28] Albouy (2004) suggested we made a mistake in assigning bishops' mortality from Gutierrez to various

almost identical in all specifications to those in column 1.[29] The second stage results in column 3 of Table 1B are also very similar to those in column 1. The AR clustered confidence sets in the specifications that control for continent dummies and latitude and for malaria are again fairly wide (extending to infinity on the right, and thus also containing another interval in the negative reals), but exclude zero.

Column 4 of Table 1A shows first stage results with the same measure of mortality but now capped at 250 per 1,000 as in column 2. The results are now stronger, more precisely estimated and more robust. The AR confidence sets in all cases comfortably exclude zero (and never extend to infinity).

As an alternative to using the Gutierrez data, we can also use information on mortality directly from British "South American" naval stations in modern Argentina, Brazil, Chile, Peru, and Panama; Bryson (1847) gives this as 7.7 per 1,000.[30] These data can be used without any benchmarking to Gutierrez's data, though naturally they do need to be converted into what they imply for soldier mortality - as the death rate for soldiers was typically higher than for sailors when the two types of forces were stationed in the same area. From Tulloch (1841), we know the mortality of the British naval force (in the Mediterranean) from disease was 9.2 per 1,000 and the mortality of the military force (on the ground in that region) from disease was 18. We use this ratio (1.96) for the South American station to convert naval mortality of 7.7 per 1,000 to military mortality of 15.07.[31] This is a conservative - i.e., low - mortality coding for Latin America.[32] In addition, again erring on the conservative side, we assume in this approach that settler mortality is missing for the remaining countries of South America and for those parts of Central America for which we previously used the Gutierrez data. This drops 11 countries from our sample, leaving us with only 53 observations.[33]

countries in Latin America. In Appendix 1 of AJR (2005), we showed that our assignment was correct and Albouy's (2004) proposed alternative assignment was wrong. He subsequently dropped that assignment.

[29] 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 high mortality bishops' zone mortality rate is now the same as Jamaica/Dominican Republic, by assumption.

[30] These naval stations were in Rio de Janeiro, Buenes Aires, Bahia, Pernambuco, Para, Valparaiso, Callao, Coquimbo, and San Blas (Statistical Reports on the Health of the Navy, 1841, p.39). There is also a San Blas in Mexico but our assessment is that the station was in San Blas, Panama. Curtin (1964) cites Bryson (footnote 16 on p.486); we have also checked Bryson (1847, pp.177-78) directly.

[31] This would put the low end of Latin American mortality almost exactly at the same level as for the United States, which is 15 per 1,000 per annum (directly from Curtin 1989, Table 1.1, p.7, for "Northern United States").

[32] Institute of Actuaries (1851-52, pp.169-170) suggests that mortality rates for civilians within 15 degrees of the equator in South America were close to those of military personnel in "East Indies and China". In the healthier Southern Cone, mortality rates were deemed close to those of Mauritius (which is 30.5 in Curtin 1989, Table 1.1, p.7).

[33] To be clear, we also not using any information from Gutierrez in this series. In our baseline series, we use Gutierrez for 16 countries. We are dropping these 11 countries in this case not because we believe that the data

Column 5 in Table 1A reports results using this series, which is labeled as "Naval Stations, Method 1." Compared with our baseline results in column 1, the coefficient is now smaller in absolute value (-0.54). Settler mortality is robustly significant in the basic regressions (the first four sets of rows), but in the last four specifications confidence sets become wider (e.g., when we introduce continent dummies) and this is reflected in Table 1B where the clustered AR confidence sets are quite wide now and extend to infinity on the right. Nevertheless, in all of these cases these confidence sets exclude zero.

Column 6 in Table 1A and Table 1B shows parallel results using the same series as in column 5, but now with mortality capped at 250 per 1,000. This mortality cap again strengthens our results and now confidence sets extend to infinity only in the specification with malaria, but continue to comfortably exclude a zero effect in the second stage.

In column 7 we use data from naval stations in a different way. We compare life insurance rates for sailors on the "South American Station" from Institute of Actuaries (1851-52, p.170), with the rates for places that are also covered by the earliest Curtin mortality estimates (1989, Table 1.1). According to the same life insurance source, the healthiest parts of Latin America were determined to have the same mortality rates as Mauritius while the least healthy parts were slightly below the West Indies.[34] In Curtin's data (1989, Table 1.1), Mauritius has a mortality rate of 30.5 per 1,000, while the West Indies average is 93.25.[35] We use these rates for Latin America.

With this alternative series, our first stage results are robust and very similar to what we find with the original AJR data. Table 1B shows that in the specifications with continent dummies and latitude and with malaria (but not in the other specifications), the AR clustered confidence sets are again wide and extend to infinity on the right. Nevertheless, as is the case in all of these specifications, they do comfortably exclude zero.

Column 8 reports results for the same series if we cap maximum mortality at 250 per 1,000. Now the results are again more precise and all AR confidence intervals are more tightly estimated and never extend to infinity. In fact, the results are very consistent with and confirm those in AJR (2001) as a comparison with columns 1 and 2 show.

for them are not reliable (as we have explained *this is definitely not the case*). Instead, we are doing this as a highly conservative robustness check.

[34] Specifically, in the language of life insurance, the "extra premium" for mortality above the British death rate recommended for the South American Station was 40 shillings (so we apply this to Argentina, Brazil, Chile, Peru, and Panama as these were part of the Station). For the rest of Latin America we use the extra premium for the North American and West Indian Station, which was 80 shillings. In the same data, the extra premium for Mauritius was 40 shillings. This approach gives a plausible estimate for parts of the continent closer to the West Indies but it is probably on the higher side for Uruguay. See Institute of Actuaries (1851-52).

[35] These West Indies data points (mortality rates per 1,000 per annum) are: 130 (Jamaica), 85 (Windwards and Leewards), 106.87 (Guadeloupe), 112.18 (Martinique) and 32.18 (French Guiana).

In summary, there is no basis whatsoever to discard all Latin American data as Albouy does. Contrary to Albouy's claims, there are several alternative sources of information on mortality in Latin America. Using our original source, Gutierrez (1986), with different benchmarking procedures or these alternative data sources produce similar mortality rates, which are also consistent with available qualitative evidence. Different sources of data for Latin America and different benchmarking procedures lead to very similar and robust results.

3.3 Does Discarding Data Make Sense? Remaining Countries

The previous subsection discussed Latin American data. Here we only discuss the remaining 20 countries Albouy drops, which include 12 in Africa, 4 in the Caribbean, 3 in Asia, and Australia.[36] Albouy's proposition is that either the Europeans during the colonial period had no view of mortality in the area in question, or we have no knowledge of their view, or both. Our contention is that for each of the countries under discussion, both Europeans at the time and we now have information on potential settler mortality - although undoubtedly there is measurement error in both.

In this subsection, we summarize the state of knowledge about disease and mortality, and briefly document that for each observation Albouy wishes to drop, there is considerable evidence supporting the mortality estimates used in AJR (2001). More details for each of these observations are provided in Appendix B. The discussion here is short both because of space constraints and because, as the next subsection shows, even dropping so many observations has little effect on the robustness of the results in AJR (2001).

Our main procedure was to assign mortality rates from one country to its neighbors, based on our reading of the relevant disease ecologies, i.e., taking a position that the climatic and other environmental conditions for disease were similar in the country for which we had direct data and the country to which we were making the assignment. Curtin (1964, 1989, 1998) shows that differential rates of mortality for Europeans in the early 19th century were due primarily to local conditions for malaria, yellow fever, typhoid, dysentery, cholera, and other so-called "tropical" diseases - though there was also a great deal of variation even within the tropics. In assessing disease environments and the knowledge about disease in the 19th century, we use the definitive work by Hirsch (1888) and its modern-day equivalent, Kiple (1993).[37]

[36] In sub-Saharan Africa, Albouy drops 11 countries: Angola, Burkina Faso, Cote d'Ivoire, Cameroon, Gabon, Guinea, Niger, Tanzania, Togo, Uganda, Zaire; and in North Africa he drops Morocco. In the Caribbean, Albouy drops the Bahamas, Dominican Republic, Guyana, and Haiti. In Asia he drops Hong Kong, Singapore, and Pakistan.

[37] Kiple's team has the benefit of hindsight and contains today's leading medical historians but might be considered somewhat distant from events and perceptions of the nineteenth century. Between Hirsch (1888) and

A brief summary is as follows. On Australia, to which the New Zealand rate was assigned in AJR (2001), as reported in AJR (2005) Tulloch (1847, p.253) provides an almost identical and independent mortality estimate. On Singapore, AJR (2001) used the Straits Settlements information. Our numbers are confirmed by Statistical Society of London (1841), as reported in AJR (2005), and by Kiat (1978). On Guyana, AJR (2001) used the mortality rate from French Guyana. This is consistent with the public health literature (Roberts, 1948, Mandle, 1970), and in addition, there is independent information from Tulloch (1838a), and the life insurance literature (Meikle, 1876, Hunter 1907). On the Dominican Republic and Haiti, AJR (2001) used mortality information from Jamaica, and there is independent confirmation from Institute of Actuaries (1851-52) and Hunter (1907). On the Bahamas, AJR (2001) used information from the Windward and Leeward Command; there is independent confirmation from Tulloch (1838b) and Miekle (1876). On Hong Kong, AJR (2001) used the China Field Force rate from the British Army; this is backed-up by Army Medical Department (1862) and Tulloch (1847). On Pakistan, AJR (2001) used the information from Bombay; there is independent confirmation from Institute of Actuaries (1851-52) and Hunter (1907). On Morocco, AJR (2001) used the mortality rate from Algeria. The mortality rates from Tunisia and Egypt were also similar and the Institute of Actuaries (1851-52, p.169) confirms that these countries had roughly the same mortality level.[38]

In summary, for all of the non-African observations dropped by Albouy, there is independent information supporting the rates used in AJR (2001). We documented this in a detailed manner in AJR (2005). There is no reasonable argument for dropping these data.

On West Africa, AJR (2001) used data primarily from Curtin (1989), specifically, mortality rates of soldiers from Sierra Leone, Senegal, Gambia, Gold Coast, Mali, and Nigeria. The general approach is supported qualitatively and quantitatively by Curtin (1964), and Bruce-Chwatt and Bruce-Chwatt (1977). There is additional confirmation from Institute of Actuaries (1851-52) and Kuczynski (1948).

For Central Africa, we used data from Curtin (1998) and Curtin et al (1995). Our use of these data is consistent with evidence in Kiple (1984) and our estimates are supported by assessments in the life insurance literature - e.g., Institute of Actuaries (1851-52), Sprague

Kiple (1993), there is Clenow (1903), whose volume benefits from the medical advances at the end of the 19th century but who is still close to the major mortality events of that century. There is no indication in Clenow (1903) that our assessments based on Kiple (1993) are off the mark on anything that matters for our analysis. We also checked the assessments in Kiple (2003) against Kuczynski (1948), Lancaster (1990) and for malaria, Bruce-Chwatt (1993).

[38] Morocco has an extra premium for mortality over British levels of 40 shillings. The category "Mediterranean, Barbary and Tripoli" has an extra charge of 20 shillings which, in this system reckoning, implies a mortality rate within 5-10 per 1,000 of the Morocco level.

(1895), and Hunter (1907). On the basic similarity of disease ecology between West and Central Africa there is general support in Patterson (1995) and infectious disease-by-disease confirmation in Hirsch (1881) and American Geographical Society (1951a,b,c,d,e).

In summary, the overall patterns of European mortality in West and Central Africa reflected in AJR (2000, 2001) data are well supported by contemporary and modern sources and literatures, some of this validation is more qualitative than for the non-African observations. Below we report regressions that drop West and Central African data as an additional robustness check.

3.4 Albouy's Preferred Sample

Albouy proposes to use a sample of just 28 countries (Panel B in his Tables 2 and 3). First stage results with this variable are shown in column 1 of Table 2A, which has the same set of rows as Table 1B. Second stage results are shown in Table 2B.

In Albouy's preferred sample, our first stage is weakened as soon as covariates are added. In particular, the confidence sets in specifications without neo-Europes, with continent dummies, with continent dummies and latitude, with percent of European descent in 1975 and with malaria are very wide and extend to positive infinity on the right (and thus also include a disjoint interval lying entirely in the negative reals, which is not reported). Though in most cases a zero coefficient of institutions in the second stage regression can be rejected, such wide confidence sets are cause for concern.

Figures 1A and 1B, however, show that the first stage in Albouy's preferred sample is at least partly weakened by a significant outlier.[39] Gambia has a very high institutions score (8.77, compared to an average of 6.51 in the whole sample and 5.88 in Africa) and was always an outlier in this sense. But in the 64 country sample of AJR (2001), it did not have as consequential an impact on the results. It becomes much more of an outlier when Albouy drops 36 other observations (Gambia's potential settler mortality is 1470 per 1,000). In addition, there is reason to suspect that this institutions score is not a true reflection of institutional quality in Gambia. For example, there have been military coups in Gambia in 1981 and 1994, and other political turmoil in the late 1980s. There has not been a return to free and fair elections since 1994.[40]

[39] Figure 1A is for the specification without covariates and Figure 1B is for the specification with continent dummies and latitude. Gambia is similarly an outlier in the other specifications.

[40] The military leader of the 1994 coup, Yahya Jammeh has reinvented himself as a civilian president but remains in power through elections that are judged as corrupt. Even before 1994, Gambia had serious political problems. In 1981, there was a military coup against the independence leader Sir Dawda Jawara, who asked help from the Senegalese, and the next year they formed the Senegambia Confederation between the two countries

Column 2 shows that dropping Gambia also from the sample (thus reducing it to 27 countries) restores the results back to a pattern very similar with those in AJR (2001). The impact of institutions in the second stage is estimated more precisely and none of the clustered AR confidence sets now extend to infinity. Confidence intervals in all specifications except the one with malaria comfortably exclude a zero effect in the second stage.

Column 3 shows that capping mortality rates at 250 also has a major impact on Albouy's results. Column 4 shows the results without Gambia and with the 250 mortality cap, which are again very similar and confirm the robustness of the AJR (2001) estimates.

Columns 5 and 6 follow up on the discussion in the previous two subsections and add back the Latin American, Caribbean, Asian and Australian data that Albouy dropped - thus excluding only the West and Central African data that Albouy would like to drop. This gives us a sample of 51. Column 5 reports results without capping and column 6 with the 250 cap. In both cases, the results are very similar to those in AJR (2000, 2001), and in all cases the second stage estimates are fairly precise, the clustered AR confidence intervals never extend to infinity, and a zero effect can be rejected at 5%.

3.5 Albouy's "Campaign Dummy"

Albouy's third concern is that some of our data are taken from military campaigns while others are not. To deal with this, he proposes to introduce a coding for whether or not our data are drawn from a "campaign" and to include that dummy in the first stage regression. Despite Albouy's claims, except during times of major wars (which are excluded from the data), there is little difference in practice between what soldiers were engaged during "campaigns" and other times. As a result, it does not in general make sense, and in fact it is not possible, to systematically distinguish campaigns and non-campaigns, and Curtin does not do so (though he mentions some campaigns as part of his historical discussion, quite contrary to what Albouy claims, Curtin does not make a systematic non-campaigns vs. campaign distinction; this is presumably because he does not view this distinction as so important).[41]

which lasted until 1989 (see, e.g., Hughes and Perfect, 2008). Throughout this period Senegal has a low institutions score, so Gambia's high score is truly puzzling.

[41] Albouy quotes selectively from Curtin (1989). But a major point made by Curtin (1998) is that some 19th century military expeditions could have low mortality - when they were designed to be fast-moving and limited duration and particularly when they avoided the worst malaria season. For example, explaining the low mortality for British soldiers on the Magdala campaign (chapter 2 in his book) and the Asante campaign (chapter 3) in the 1870s, Curtin (1998, p.30) writes: "In fact, the Magdala campaign *was* the engineer's war. It was commanded by an engineering officer and hailed by observers as a triumph of logistical planning. The Asante campaign was the doctors' war, perceived as the first evidence that modern medicine made it possible for European troops to act safely in the tropical world." (italics in original).

Equally important, Albouy's procedure for coding this dummy seems inconsistent and extremely selective. For example, Albouy decides, very consequentially for his results, that New Zealand is a non-campaign rate even though Curtin discusses (1989, p. 13) losses from battles in New Zealand - British troops were "campaigning" in New Zealand against Maori tribes. Curtin (1989, p. 13) states:

> "The most unusual feature of military death in New Zealand over these five years was the fact that deaths from accident and battle exceeded deaths from disease ... The high number of deaths in battle is evidence of heavy campaigning."

As another example of inconsistency, consider Hong Kong (data from the China Field Force). As the name suggests, the China Field Force was a field force engaged in fighting (and in this instance, Curtin actually says so explicitly - see Table A8.2, p.239, in 1998). But Albouy chooses to code this as a "non-campaign" rate.

These and other inconsistencies in Albouy's coding (and the general point that such a distinction has little meaning) are discussed in greater detail in Appendix C. The rest of this subsection reviews Albouy's results with his "campaign" dummy and how they change significantly once either the impact of high mortality outliers is limited by capping mortality estimates at 250 per 1000, or minimal corrections for inconsistency are made to his dummy

Column 1 of Table 3B shows Albouy's results for the full sample but including his campaign and slave labor dummy (the first stages are in Table 3A). This leads to wide confidence sets in several specifications. Column 2 shows that simply capping potential European settler mortality at 250 again restores the results essentially back to those obtained in AJR (2001). Once again, the second stage is estimated more precisely and the clustered AR confidence sets do not extend to infinity and always exclude zero except in the specification with malaria.

Column 3 implements the minimal corrections to Albouy's "campaign" dummy (just for Hong Kong and New Zealand). This too leads to more precisely estimated second stage results. Column 4 shows that if in addition we also introduce the mortality capping at 250 per 1,000 per annum, the results are fairly precisely estimated and very similar to those in AJR (2000, 2001), as can be seen by comparing the estimates and the standard errors to those in column 1 of Table 1B. In both the situations, clustered AR confidence sets never extend to infinity and exclude insignificant effects in the second stage (except that they exclude zero only marginally in the specification with malaria).

Column 5 considers the more extensively corrected campaign dummy (see Appendix C for details). Column 6 reports results from this extensively corrected campaign dummy together

with the 250 per 1000 mortality cap. The results are once again very much consistent with those in AJR (2000, 2001); the clustered AR confidence sets never extend to infinity and always comfortably exclude insignificant effects.

Finally, column 7 presents Albouy's results when all his strategies are combined (only 28 observations and his coding of the campaign dummy). These results, of course, are highly imprecise with very wide confidence sets, often not excluding zero. Column 8 shows that dropping Gambia, correcting the inconsistencies in Albouy's campaign dummy, and capping mortality at 250 leads the results broadly similar to those in the AJR (2001) baseline - even with almost 60% of the sample discarded.

We therefore conclude that none of Albouy's strategies have a major impact on the results in AJR (2001) once one limits the impact of very high, outlier mortality rates. Most of the results are remarkably robust. The only specification in which the second stage estimates are sometimes insignificant is the one that includes current prevalence of malaria, which is a specification that biases results against finding significant effects as discussed in AJR (2001). Moreover, even modest corrections to Albouy's strategies also lead to similar results.

3.6 Minor Points

Albouy also presents results using a small modification of the series from AJR (2001), partly based on AJR (2005). Use of this slightly modified series makes little difference (see AJR, 2005) - unless of course the sample is reduced to 28 observations and the miscoded campaign dummy is included (Panel E of Albouy's Table 3).

Albouy also complains about how we use data from Mali, but this issue has only trivial effects on the first stage results; compare Panel E of Albouy's Appendix Table A5 and Panel B of his Table 2. Moreover, his criticism of what we did is based on misreading our work, as we explained in AJR (2005). Since his recoding makes no difference whatsoever to our results, again as shown in AJR (2005), we do not recap these responses here to save space.

4 Concluding Comments

Albouy's comment expresses three main concerns about the results in AJR (2001) on the relationship between potential settler mortality and institutions. First, there is a general concern that there are high mortality outliers, potentially affecting this relationship. We agree with this concern and this was the rationale for using the logarithm of mortality, as well as many robustness checks reported in AJR (2000). Notably, limiting the effect of high mortality

outliers has no impact on the main results in AJR (2001). Capping mortality rates at 250 per 1000 per annum, as in AJR (2005), not only leaves our results unchanged but - as should be expected - by reducing the effect of outliers, it increases their robustness. In fact, using this strategy, the results in AJR (2001) are largely robust to even extreme versions of his other critiques.

Albouy's second argument is that all the data from Latin America and much of the data from Africa, dropping almost 60% of our sample. This is arbitrary. We have summarized here - and shown at greater length in AJR (2000, 2001, 2005, 2006, 2009) - that there is a great deal of well-documented comparable information on the mortality of Europeans in those places during the relevant period. This information is consistent with the mortality rate estimates used in AJR (2001). There is no basis for discarding most of our data.

When Albouy discards all these data and reduces the sample to only 28 countries, his results are largely driven by observations with excessively high mortality rates, especially an outlier, Gambia. Gambia has a very high institutional index, which stands in stark contrast to its recent history. Either using our procedure of capping mortality estimates at 250 to reduce the impact of high mortality outliers or dropping Gambia (thus reducing the sample to just 27 countries) again shows that the results are robust.

Albouy's third argument is that a "campaign" dummy should be included in the first stage. His arguments here are at odds with the historical record and his coding procedure is implemented inconsistently. Even modest corrections to these inconsistencies or again capping mortality estimates at 250 to reduce the impact of outliers overturn his results and show that the main findings in AJR (2001) are robust.

Albouy's other concerns about Mali are minor, are based on a misreading of our work - as explained in AJR (2005) - and in any case have no meaningful effect on our results. Similarly, his slight modification of the data in AJR (2001) based on AJR (2005) is also not consequential.

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Not-for-Publication Appendixes

Appendix A: Anderson-Rubin Confidence Intervals

To construct Anderson-Rubin (AR) confidence sets without clustering (with spherical errors), we first calculate the Anderson-Rubin test statistic and then invert it by solving a series of quadratic polynomial inequalities. The solution to these inequalities will correspond to a finite interval, the union of two infinite intervals, the whole real line, or an empty set. A fast, accurate algorithm for solving these inequalities has been developed and programmed for Stata by Anna Mikusheva and Brian Poi, in the form of the *condivreg* module. This approach is faster and more accurate than inverting the AR test statistic using a grid test, which performs a series of hypotheses tests $H_0 : \beta = \beta_0$ where β_0 belongs to a grid (Mikusheva 2010). More details about the algorithm and its STATAS implementation can be found in Mikusheva and Poi (2006).

Mikusheva and Poi's algorithm is applicable only to the spherical case. To calculate the clustered AR confidence sets (with non-spherical errors), we must invert the AR test statistic through grid testing. We do this by using the *rivtest* module in STATA which is documented in detail in Finlay and Magnusson (2009). In particular, as outlined by Chernozhukov and Hansen (2008), we first regress a transformed dependent variable, $Y - X\beta_0$ on the instrument, Z, where X is the endogenous regressor: $Y - X\beta_0 = Z\alpha + \epsilon$. We then test that $\alpha = 0$ (which is implied by $\beta_0 = 0$) using a conventional robust covariance matrix estimator. Finally, the AR test statistic is inverted using a grid test. For the results reported here, we searched an evenly spaced grid containing 1600 points (the maximum allowed by the Stata program) on an interval 25 times the width of the Wald confidence interval. We checked robustness to using wider and narrower intervals, as well as to searching on an evenly spaced grid ranging from -20 to 20, and in all cases the estimates changed very little from those reported here.

Appendix B: Information on Disease Ecology

The relevant literature is large, beginning with Lind's *Diseases in Hot Countries,* the first edition of which was published in 1768 and which helped form early perceptions. Hirsch (1888) provided the comprehensive 19th century compilation and assessment of knowledge; Volume 1 of his *Handbook of Geographical and Historical Pathology* covers "Acute Infective Diseases". As this was written before the transmission of disease by mosquito was properly understood - first published in German in 1881, its three volumes are the result of 25 years of work - it is particularly valuable as a window on contemporary understanding of Medical Geography.

Our core assessments below are based primarily on the modern equivalent to and update of Hirsch, which is *The Cambridge World History of Human Disease*, edited by Kenneth F. Kiple (1993).[42] This volume contains more than 1,000 pages on almost all known human diseases, including sets of chapters on "The Geography of Human Disease" (by region), "The History of Human Disease in the World Outside Asia," and "The History of Human Disease in Asia." We checked the assessments there with H.O. Lancaster's *Expectations of Life: A Study in the Demography, Statistics, and History of World Mortality*, which contains chapters by region.

As a way to check that these general assessments fit with the detailed geographes in question (including with modern borders), we also checked a series of large world maps published in 1951 by the American Geographical Society, from *The Geographical Review*, Vol.41, 1951. This provide information on the incidence of infectious disease in every country of the world. Of particular value for our purpose is the "Distribution of Malaria Vectors" (American Geographical Society, 1951a; also useful are American Geographical Society 1951b, 1951c, and 1951d), which shows the type of mosquito ("Species of Anopholes") present in every country - this has an important effect on the potential prevalence of the more serious forms of malaria - as well as the distribution of malaria parasites. This map provides references, by country, with most of its sources dating from the 1920s, 1930s and early 1940s. This graphic was obviously drawn after the early colonial period that is our focus here, but in most of Africa, Asia, and Latin America infectious disease rates were not brought down until the 1940s - and almost little progress was made towards eradicating malaria, yellow fever or other mosquito borne disease before the 1940s (see Acemoglu and Johnson, 2007). As Curtin (1989) discusses the importance of yellow fever epidemics in accounting for relatively high mortality during the 19th century, we also make use of the American Geographical Society's map showing the "Distribution of Dengue and Yellow Fever."[43] We check these maps against the latest available information in the medical geography literature, focused on epidemics, in the form of Hoff and Smith (2000) and Cliff, Haggett, and Smallman-Raynor (2004).

R.R. Kuczynski's three volume *Demographic Survey of the British Colonial Empire* also helps as a further cross-check. Published in 1948, this contains a great deal of the available historical demographic and public health information by country - particularly for African

[42] Kiple's team has the benefit of hindsight and contains today's leading medical historians but might be considered somewhat distant from events and perceptions of the nineteenth century. Between Hirsch (1888) and Kiple (1993) there is Clenow (1903) - whose volume benefits from the medical advances at the end of the nineteenth century but who is still close to the major mortality events of that century. There is no indication in Clenow (1903) that our assessments based on Kiple (1993) are off the mark on anything that matters for our analysis.

[43] For more on the role of yellow fever in high European mortality before 1900, see Oldstone (1998), Chapter 5, "Yellow Fever".

colonies. His three volumes cover West Africa (Volume I), South Africa, East Africa, Mauritius and Seychelles (Volume II), and West Indian and American Territories (Volume III). The lack of serious progress through the 1940s against infectious disease in many places were still colonies also makes twentieth century conditions in some places quite relevant for assessing the pattern of disease ecologies in the 19th century.

In terms of specific diseases, Curtin emphasizes the importance of differential incidence of malaria - and variation in the types of malaria across regions (also see Bruce-Chwatt and Bruce-Chwatt 1977). We use the updated version of Curtin's recommended text on this issue, which is Bruce-Chwatt's Essential Malariology, Third Edition (Gilles and Warrell 1993). We supplemented this information with Desowitz (1991) and Bradley (1992). The most useful sources in the historical record of malaria eradication efforts are League of Nations Health Organization (1932), Expert Committee on Malaria (1947) and Bulletin of the World Health Organization (1954). Conybeare (1948), Stolnitz (1955), Davis (1956), Caldwell (1986), and Preston (1980) are also helpful - on the point that malaria rates declined only from the 1940s so our sources from the early 1900s and later (up to and including Kuczynski 1948 and the American Geographical Society 1951a) are relevant for assessing 19th century disease ecologies.

We also looked carefully through the demographic and public health literature for specific articles on health conditions before 1940 that would shed light on local disease ecology - particularly anything that would suggest neighboring countries did not share the same disease ecology. This includes searching journals such as *Demography*, *Population Studies*, *Population and Development Review*.

This literature allows us to look carefully for any neighbor "anomalies", i.e., conditions under which neighboring countries would not share the same disease ecology. Perhaps the leading example is the disease barrier provided by the Sahara desert. In this regard, Albouy makes an important and elementary mistake when he argues that just because some West African countries border some North African countries, these two sets of countries might have a similar disease ecology - and therefore similar mortality rates for Europeans in the 19th century. Patterson (1993) and Kuhnke (1993) - adjoining chapters in Kiple (1993) - make it very clear that this is not the case: West Africa and North Africa have fundamentally different disease ecologies, particularly with regard to the conditions for mosquitos, the vectors for key diseases that killed a high percentage of exposed Europeans before 1850.

The actuarial literature, which developed rapidly after about 1850, provides a useful cross-check - typically based on decades of experience for particular life insurance companies. Institute of Actuaries (1851-52) reports insurance rates used by "London Offices" in mid-century.

Meikle (1876) assesses life insurance experience for Europeans outside of Europe. Hunter (1907) provides a review of life insurance experience in the last decades of the 19th century around the world - and suggests a classification of countries by mortality category.

The remainder of this sub-section reviews each instance when Albouy drops our data.[44]

Australia

In AJR (2001) we assigned the New Zealand rate (8.55 per 1,000) to Australia, based on Marshall (1993) and the broader disease ecology literature. Curtin (1989) has a lengthy discussion of health conditions in New Zealand, including why exactly it has always been malaria free. In our assessment, Australia shared those characterisitics. Albouy finds this unconvincing and drops Australia from his core dataset.

However, in writing AJR (2005), we found that 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. Albouy now accepts this rate for his extended mortality dataset.

But we also pointed out that on the first page of his introduction to part I of Army Medical Department (1840), Tulloch argues that "more than a fifth part arose from violent or accidental deaths" and "Thus the mortality from disease alone could have amounted to little more than one per cent annually, being lower than in any other Colony, except the Eastern Provinces of the Cape of Good Hope, to which the climate of Australia is in many respects similar." In other words, Tulloch puts deaths from disease in Australia at 10 per 1,000.

To err on the conservative side, we used the rate of 14 per 1,000 in our "Tulloch" revised dataset, with results shown in Tables 3A and 3B of AJR (2005), so that we could examine whether our initial assumption of 8.55 per 1,000 makes any difference - and it does not. But in any case there is no defensible rationale for dropping Australia - our data about 19th century mortality in this country are almost as good as our data for Europe.

Singapore

AJR (2001) used the Straits Settlements estimate in Table 1.1 (Curtin, 1989) for both Malaysia and Singapore.[45] Albouy accepts our estimate of the mortality rate for Malaysia but discards the data from Singapore. But, as we reported in AJR (2005), there is strong qualitative

[44] We do not deal here with points that he raised before but that he has now withdrawn from his regression analysis. See AJR (2005, 2006, and 2008) for those details.

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

confirmation of our coding.[46] 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)[47] There is no mention of malaria or yellow fever in the early medical history of Singapore; see Kiat (1978).

Furthermore, Albouy allows Singapore in his "revised mortality" series, but with the same rate it has in the AJR base sample. Dropping Singapore from his core dataset is therefore contradictory as well as making no sense.

Guyana

Tulloch (1838a) reports a mortality from disease rate in British Guiana of 84 per 1,000 over 1817-36 (pp. 131 and 133).[48] Previously we used the rate from French Guyana (32.18 per 1,000; Table 1.1 in Curtin 1989), so this direct estimate is presumably preferable and we use it in our robustness series. The public health literature on British Guyana itself does not indicate any anomaly that would suggest its disease ecology is different from that of French Guyana (Roberts, 1948; Mandle, 1970). Albouy drops Guyana from his base sample.

Hunter (1907, p.401) puts "British, Dutch, and French Guiana" in the same mortality category ("tropical").

Dominican Republic and Haiti

For the Dominican Republic and Haiti we used the Jamaica mortality rate of 130 per 1,000. Albouy drops these datapoints. The extensive disease ecology and historical literature for the Caribbean distinctly indicates that the Dominican Republic and Haiti were on the high side of typical mortality.

Institute of Actuaries (1851-52) reports an extra premium for life insurance in all the West Indies of 100 shillings. Hunter (1907, p.401) explicitly puts Haiti in the same category as other Caribbean countries, including Martinique and Guadeloupe (mortality rates of 112.18 and 106.87 respectively in Table 1.1 of Curtin 1989.)

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

[47] 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.

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

Bahamas

For the Bahamas, we used Curtin's estimate for the Windward and Leeward Command (85 per 1,000). 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.[49] 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.

Albouy drops the Bahamas from his core sample.

Meikle (1876, p.277) assesses mortality in the Bahamas as very similar to that in Mauritius (which is 30.5 per 1,000 in Table 1.1 of Curtin 1989.)

Hong Kong

For Hong Kong we used the China Field Force rate for the British army in 1860, from Table A8.2 of Curtin (1998), which is 14.9 per 1,000. Albouy discards Hong Kong, regarding it as completely missing data for his core sample. For AJR (2005) we looked at the historical record and found legitimate discussion over which estimate to use for Hong Kong as various numbers are available.

The death rate for "White troops" in China in 1859 was put at 41.93 per 1,000 by Balfour (1861) - 59.35 per 1,000 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). Jannetta (1993) and Leung (1993) give no indication that Hong Kong had a disease ecology that was significantly different from nearby parts of mainland China. And we can find no indication in American Geographical Society (1851a) or other maps that Hong Kong was any kind of disease anomaly.

However, there is a higher mortality estimate for European soldiers in Hong Kong from Tulloch's writings. On p.254, Tulloch (1847) reports an average ratio of mortality per 1000 of strength, 1842-1845, to be 285.[50] This is not a long average, as in the rest of Tulloch's work, but we still take this rate for our robustness series in AJR (2005). It is also not certain that all these deaths are from disease, but it does fit with Cantlie's negative assessment of Hong Kong (Cantlie 1974).

[49] 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.

[50] This is in an article that is not cited by Curtin, and which we overlooked in writing AJR (2001) - we reported the data in AJR (2005).

Tulloch's very high mortality estimate does not fit the fact that the British and French used Hong Kong as a gathering point for the China Field Force in 1860. Why would they have done this if the place were known to be so unhealthy? As Graham (1978, p.386) says, Kowloon was "an apparently healthy site for a barracks or camping ground...". 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 some malaria in the area at that time.

In Institute of Actuaries (1851-52, p.169) all of China is in the same category of "extra premium" for life insurance (60 shillings for civilians and 80 shillings for military). Hunter (1907, p.401) puts the entire Chinese Empire, "south of 30 degrees North latitude", in the same mortality category ("tropical").

When there are varying estimates, we can average or use the alternatives or find some other way to combine the data. But to discard the observation completely, i.e., to treat it as " missing", is not appealing.

Pakistan

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 (71.41), Madras for India (48.63), and Bombay for Pakistan (36.99 per 1,000). We also used the rates from Ceylon for Sri Lanka (69.8).[51] These data are all from before 1838. Albouy disputes - and drops - only the data for Pakistan.

But the British perception of health conditions in modern day Pakistan and nearby regions was very close to their view of mortality around Bombay. Bhardwaj (1993) gives no indication that the area covered by modern Pakistan is significantly different from other parts of the northerly-western region of what is now India.

For Pakistan we have gone carefully through the extensive British reports on military mortality in 19th century India. In our assessment, the British area of operations close to and including modern Pakistan, the expected mortality rate was at or close to what we included in our original series.

In Institute of Actuaries (1851-52, p.169) all of India is in the same category for "extra

[51] 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.

premium" on life insurance (60 shillings for civilians and 80 shillings for military). In Hunter (1907, p.401), all of (then-British) India is placed in one mortality category ("tropical")

Morocco

In AJR, the mortality rate for Algeria (78.2 per 1,000) is assigned to Morocco; while Tunisia was 63 per 1,000 and Egypt was 67.8. Institute of Actuaries (1851-52) puts the extra premium for Europeans traveling to Morocco at 40 shillings, which is the same as for an "Eastern Tour" that includes Egypt and other parts of the Middle East;.

West Africa[52]

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 19th century (for Gambia,[53] Gold Coast/Ghana,[54] Mali/French Soudan,[55] Nigeria[56]). 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 19th century, as militaries became better at managing mortality during short expeditions during the 19th century, so we stayed away (as much as possible) from estimates after 1850.[57] 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.

For Africa, we assigned mortality based on the literature on disease ecologies - erring on

[52] There are 11 West African countries in AJR: Burkina, Cote d'Ivoire, Ghana, Guinea, Gambia, Mali, Niger, Nigeria, Senegal, Sierra Leone, and Togo.

[53] The Gambia data are "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).

[54] 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).

[55] 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).

[56] 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).

[57] "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.

the side of using relatively low mortality rates and not those conspicuously from epidemics. This assignment is supported also by the life insurance literature. We have always emphasized that the data for some parts of Africa are less reliable than for other regions - and this has motivated our robustness checks without Africa (see AJR 2005 and the tables in this paper; this was also the focus of AJR 2006). But disregarding all African data completely is an inappropriate approach given the extensive available information. There is without doubt a great deal of measurement error in the African data but there is also much information about early European mortality in that region - and Europeans at the time were well aware of this.

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).[58] Albouy drops these five countries.

But according to Curtin's *Image of Africa* (Curtin 1964, e.g., chapter 3), the Europeans had a clear and negative view of mortality throughout West Africa. There is no hint in the historical record that any of these places were regarded as potentially more healthy. From Curtin 1964, p.71, the discussion is all about West Africa.

Speaking of the late 18th century (i.e., before Tulloch's pioneering statistical work), Curtin (1964, p.71) articulates the perceptions of Europeans this way, " West African mortality figures were not widely publicized or given statistical precision, but the region's general reputation for having a "deadly climate" rested on a basis in fact. Somewhere between 25 and 75 percent of any group of Europeans newly arrived on the Coast died within the first year. Thereafter, the death rate was much less, perhaps on the order of 10 per cent per annum, but still substantial. Any European activity demand a price in European lives that was not only intrinsically high, but considerably higher than the cost of similar activity in the West Indies or South Asia. Slightly later calculations of military mortality over twenty years show a loss of 483 per thousand mean strength among European troops in West Africa, against only 78.5 per thousand in the West Indies. Civilian life insurance premiums charged by British firms for different tropical regions tell a similar story: European mortality was roughly four times as high in West Africa as it was in India or the West Indies." The early mortality rates reviewed in Bruce-Chwatt and Bruce-Chwatt 1977, pp.43-50) are entirely consistent with this assessment.

The explorer Richard Burton described Lagos Government House in 1863 as a "corrugated iron coffin or a plank-lined morgue containing a dead Governor once a year" (Bruce-Chwatt

[58] Albouy complains (p.9) that we assign a rate of 400 (Curtin, 1998, p.85) from Mali to Niger, but this is assignment to a neighbor with the same disease ecology.

and Bruce-Chwatt 1977, p.47). Bruce-Chwatt and Bruce-Chwatt (1977, p.47) report that "the annual death rate for these high officials [Governors of Sierra Leone] was around 200 per 1,000". Kuczynski (1948, volume 1, pp.40-153) provides more details on the mortality rates of Europeans in Sierra Leone and other parts of British West Africa during the 19th century; this is completely consistent with the work of Tulloch and Curtin.

Albouy is also 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.[59]

In Institute of Actuaries (1851-52, p.169), the extra premium for life insurance in West Africa is 160 shillings in Senegambia and 120 shillings in the rest of the region. According to this source, these were the highest mortality places in the world for Europeans.

Chapter VII of Hirsch (1881, section 60, pp.198-202) identifies all of West Africa as an intensely malarial area. His sources on more inland West Africa were more limited - but Patterson (1993) is clear that this is the same disease ecology (unlike, for example, North Africa, which is quite different).

Central Africa

From Central Africa, Albouy drops Angola, Cameroon, Gabon, Tanzania, Uganda, Zaire. For Central Africa mortality estimates were particularly hard to come by and in AJR (2001) we proceed cautiously by relying on two relatively conservative numbers. 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. The disease ecology literature suggests there was less yellow fever in Central Africa than in West Africa (see American Geographical Society 1951e), but still significant amounts of falciporum malaria.

Second, from Curtin we had estimates of mortality rates for Africans working away from

[59] 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 in AJR 2005). There is a similar pattern in other specifications.

their homes, for Congo and Kenya. We took the highest observed values of these rates to represent a minimum for Europeans in those places. 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.

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-19th 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).[60]

Sprague (1895, p.69) writes in the life insurance literature, "The Central Congo district has such a bad name that the mortality among Europeans resident there is said to be about 25 per-cent." According to Institute of Actuaries (1851-52), all of Central Africa is in the same high category of mortality for Europeans - requiring an extra premium on life insurance of 120 shillings. Hunter (1907, p.402) puts West Africa and Central Africa in the same mortality category ("higher than tropical scale"). American Geographical Society (1951e) shows all of West and Central Africa to be in the same endemic yellow fever zone. American Geographical Society (1951a) shows West and Central Africa share the same prevalence of *anopholes gambaie* - the primary vector for falciporum malaria.

Hirsch (1881, p.199) indicates that the Congo Coast was somewhat more healthy - with less malaria - than West Africa, but considerably less healthy than southern Africa. Patterson (1993) does not mention any significant difference in disease ecology between West and Central Africa.

Dropping Congo/Zaire makes no sense - as this is the area covered by our data and other available information. Albouy does not drop Kenya, for which we also had similar data directly.

Appendix C: "Campaigns"

We have reexamined the historical record for every one of our observations. While we do not claim to have established definitively whether there was or was not significant campaigning in each episode covered by our settler mortality estimates, here are some blatant examples of miscoding "campaigns" and "barracks" in Albouy.

We also indicate whether we recode the observation as campaign in either our minimal or

[60] 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.

extended recoding (note: all countries recoded as campaign in the minimal recoding are coded as campaign in the extended recoding). The choice of whether a country is in the minimal or extended recoding category is somewhat arbitrary, but doesn't make a significant difference to our results.

Jamaica - 1817-1836, this period includes the largest slave uprising in Jamaica's history known as the Baptist War in 1831. So there is fighting and campaigning during the period under consideration. This war is discussed in every book on Jamaican history; a much cited academic article is Reckord (1968). In our extended recoding, Jamaica is coded as a campaign.

Sri Lanka - Curtin has this number from 1817-1836. The Dutch had controlled the whole of the Island except for the Kingdom of Kandy; the British fought a series of wars after 1803 to annex this. The 3rd Kandyan War, took place 1817-1818, which is inside the period covered by Curtin. This war was big and it is discussed in every history of Sri Lanka. For instance, Peebles (2006, p. 50) notes that 1,000 British troops died. In our extended recoding, Sri Lanka is coded as campaign.

Malaysia and Singapore - these data are from the Straits Settlement 1829-1838. In 1831-32 the British fought the Naning War. Mills (1966) describes this in Chapter 7 pp. 115-128 and notes on page 115 that there was 9 months of campaigning. The war took place near Melaka, part of the Straits Settlement, for which we have data. Mills says that Indian soldiers were involved but he also continually talks about British forces. This is a war with British forces campaigning, right in the middle of the period Curtin defines. In our extended recoding, Malaysia and Singapore are coded as campaign.

Hong Kong – 1860 China field force. This number comes from Table A8.2 in Curtin (1998, p. 239) and in this table this is described as a "campaign." Albouy must have misread this table, and in our minimal recoding Hong Kong is coded as campaign.

New Zealand – This is discussed in greater detail in the main text above. The quote here from Curtin (1989, p. 13) tells all, "The most unusual feature of military death in New Zealand over these five years was the fact that deaths from accident and battle exceeded deaths from disease ... The high number of deaths in battle is evidence of heavy campaigning." In our minimal recoding, New Zealand is coded as campaign. In our extended recoding, Australia (for which data are derived from New Zealand) is also coded as campaign.

Senegal – Curtin's period is 1819-1838. During this period the French colony was basically just Gorée and St Louis islands in the mouth of the Senegal River. However, the French were very interested in expanding their commercial interests and started to build forts up the Senegal River at Dagna (1821) and Merinaghen (1822) (Oloruntimehin, 1974, p. 356). They also sent

many missions into the interior. The French attempt to control trade started conflict.

"Thus, for instance, in 1832 the French in Senegal fought the Trarza Moors to establish their control over the gum trade. The same situation applied in the relation between the French, the Moors and the Jolof state of Walo in 1835. Military involvement of this nature was often protracted," (Oloruntimehin, 1974, pp. 356-367).

So once more it is incorrect that they were sitting in barracks. In our extended recoding, Senegal is recoded as campaign.

Trinidad and Tobago - this gets a mortality rate of 85 from the Windwards and Leewards 1817-1836. Curtin notes p. 25, "the central station was Barbados, but at times troops from the command served as far to the north as St Kitts and as far to the southeast as British Guiana".

This is significant. In 1823 was the massive Demerara Slave rebellion in Guyana. The beginning of this period also almost includes Bussa's Rebellion, a huge slave revolt in Barbados in 1816. A standard reference to this is Beckles (2006); see chapter 5 on Bussa's rebellion and aftermath. In 1817 they were still hanging people so there certainly was a large military force in operation and keeping the peace. The seminal book on the Demerara slave revolt is Da Costa (1994).

Blackburn notes (1988, p. 430) in the context of the repression of the Demerara rebellion, "The Governor called out well-armed troops and militia, including a detachment of one of the West India Regiment." Da Costa refers to this on page 217, so it appears likely that the troops stationed in Barbados saw action in both the Bussa and Demerara rebellions during this period.

In our extended recoding, we code Trinidad and Tobago as campaign.

South Africa. This rate comes from the Cape Colony 1818-1836. As far as we can find, Curtin says nothing specific about the presence or absence of military activity in Cape Colony. However, this period includes both the 5th and the 6th Xhosa Wars on the Eastern Frontier of the Cape. These involved British troops, etc. so we do not know exactly where the numbers in Curtin come from in terms of these campaigns – but the period clearly includes major campaigns.

The Xhosa Wars are discussed in all standard histories of South Africa, for example Thompson (2001, chapter 3).

In our extended recoding, South Africa is coded as campaign.

USA - this is for American troops 1829-1838. But US soldiers were obviously fighting Indian wars in this period. Again, Curtin does not discuss this number, but this period

includes a number of Indian wars: the Second Seminole War in Florida, 1835-1842; The Black Hawk War 1832; and the Creek War of 1836.

Material on these wars appears in all standard histories of the US. For example, in the shorter Oxford History, Jones (1995, p. 118) writes, "The Seminole War of 1835-42 involved large-scale operations in the Florida swamps and cost the United States 1,500 men and $50 million."

This period also saw the forced removal of many Indians tribes following the passage of the 1830 Removal Act; see Banner (2005) – these removals were organized by the army.

In our extended recoding, the USA is recoded as campaign.

Summary

Our minimal recoding covers just Hong Kong and New Zealand. Our extended recoding covers those two countries, plus Jamaica, Malaysia, Singapore, Sri Lanka, Australia, Senegal, South Africa, Trinidad and Tobago, and the USA.

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Table 1A

First Stage Regressions, Alternative Mortality Series

	Alternative series for settler mortality							
	Original AJR series	Original AJR series, capped at 250	Benchmarking to Caribbean	Benchmarking to Caribbean, capped at 250	Using Naval Stations, Method 1	Using Naval Stations, Method 1, capped at 250	Using Naval Stations, Method 2	Using Naval Stations, Method 2, capped at 250
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable is protection against risk of expropriation							
No covariates	-0.61	-0.94	-0.59	-0.91	-0.54	-0.77	-0.58	-0.88
(standard error)	(0.15)	(0.16)	(0.15)	(0.17)	(0.14)	(0.16)	(0.15)	(0.16)
(clustered standard error)	(0.17)	(0.18)	(0.17)	(0.18)	(0.16)	(0.18)	(0.17)	(0.17)
Number of clusters	36	36	36	36	35	35	36	36
Number of observations	64	64	64	64	53	53	64	64
With latitude	-0.52	-0.86	-0.5	-0.83	-0.43	-0.66	-0.49	-0.79
(standard error)	(0.17)	(0.18)	(0.16)	(0.19)	(0.15)	(0.19)	(0.16)	(0.18)
(clustered standard error)	(0.19)	(0.20)	(0.19)	(0.20)	(0.17)	(0.20)	(0.18)	(0.20)
Number of clusters	36	36	36	36	35	35	36	36
Number of observations	64	64	64	64	53	53	64	64
Without neo-Europes	-0.4	-0.66	-0.38	-0.64	-0.35	-0.52	-0.38	-0.61
(standard error)	(0.15)	(0.18)	(0.15)	(0.18)	(0.13)	(0.16)	(0.14)	(0.18)
(clustered standard error)	(0.17)	(0.20)	(0.16)	(0.19)	(0.14)	(0.16)	(0.16)	(0.18)
Number of clusters	33	33	33	33	32	32	33	33
Number of observations	60	60	60	60	49	49	60	60
Without Africa	-1.21	-1.21	-1.23	-1.23	-0.82	-0.82	-1.11	-1.11
(standard error)	(0.20)	(0.20)	(0.23)	(0.23)	(0.28)	(0.28)	(0.22)	(0.22)
(clustered standard error)	(0.18)	(0.18)	(0.21)	(0.21)	(0.29)	(0.29)	(0.20)	(0.20)
Number of clusters	19	19	19	19	18	18	19	19
Number of observations	37	37	37	37	26	26	37	37
With continent dummies	-0.44	-0.81	-0.42	-0.78	-0.32	-0.56	-0.41	-0.73
(standard error)	(0.19)	(0.25)	(0.19)	(0.25)	(0.17)	(0.21)	(0.18)	(0.23)
(clustered standard error)	(0.20)	(0.25)	(0.20)	(0.25)	(0.18)	(0.21)	(0.19)	(0.22)
Number of clusters	36	36	36	36	35	35	36	36
Number of observations	64	64	64	64	53	53	64	64
With continent dummies and latitude	-0.35	-0.72	-0.33	-0.68	-0.25	-0.46	-0.33	-0.63
(standard error)	(0.20)	(0.26)	(0.20)	(0.25)	(0.18)	(0.23)	(0.19)	(0.24)
(clustered standard error)	(0.21)	(0.26)	(0.21)	(0.25)	(0.20)	(0.22)	(0.20)	(0.23)
Number of clusters	36	36	36	36	35	35	36	36
Number of observations	64	64	64	64	53	53	64	64
With percent of European descent in 1975	-0.42	-0.73	-0.39	-0.7	-0.31	-0.5	-0.39	-0.67
(standard error)	(0.17)	(0.19)	(0.17)	(0.20)	(0.17)	(0.21)	(0.16)	(0.19)
(clustered standard error)	(0.19)	(0.20)	(0.19)	(0.21)	(0.19)	(0.24)	(0.19)	(0.21)
Number of clusters	36	36	36	36	35	35	36	36
Number of observations	64	64	64	64	53	53	64	64
With malaria	-0.43	-0.81	-0.4	-0.8	-0.13	-0.39	-0.39	-0.74
(standard error)	(0.21)	(0.22)	(0.22)	(0.24)	(0.20)	(0.22)	(0.21)	(0.22)
(clustered standard error)	(0.24)	(0.24)	(0.25)	(0.26)	(0.21)	(0.24)	(0.23)	(0.23)
Number of clusters	35	35	35	35	34	34	35	35
Number of observations	62	62	62	62	51	51	62	62

OLS regressions, with one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. All variables are from AJR (2001). Dependent variable is protection against risk of expropriation; independent variable is log settler mortality. Column 1 uses original settler mortality series from AJR (2001) as independent variable. Column 2 uses original settler mortality series, capped at 250 per 1,000 per annum. Column 3 uses alternative settler mortality series, benchmarking Latin American data to Jamaica/Dominican Republic. Column 4 uses same series as column 3, but capped at 250 per 1,000. Column 5 uses mortality data directly from naval stations, without benchmarking. Column 6 uses same series as column 5, but capped at 250 per 1,000. Column 7 uses mortality data from naval stations, with life insurance data, without benchmarking. Column 8 uses same series as column 7, but capped at 250 per 1,000.

Table 1B
Second Stage Regressions, Alternative Mortality Series

	Alternative series for settler mortality							
	Original AJR series	Original AJR series, capped at 250	Benchmarking to Caribbean	Benchmarking to Caribbean, capped at 250	Using Naval Stations, Method 1	Using Naval Stations, Method 1, capped at 250	Using Naval Stations, Method 2	Using Naval Stations, Method 2, capped at 250
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable is log GDP per capita in 1995							
No covariates	0.93	0.82	0.96	0.85	1.01	0.94	0.98	0.86
AR confidence interval	[0.68,1.40]	[0.62,1.14]	[0.71,1.47]	[0.64, 1.20]	[0.74,1.63]	[0.70, 1.40]	[0.72, 1.50]	[0.65, 1.23]
AR confidence interval, clustered	[0.66,1.72]	[0.60,1.19]	[0.69,1.85]	[0.63, 1.29]	[0.72,1.90]	[0.69, 1.50]	[0.70, 1.85]	[0.64, 1.31]
F-stat, first stage	23.34	35.55	22.06	33.53	18.26	24.52	21.95	32.36
F-stat, first stage, clustered	12.45	28.09	11.72	25.31	11.96	19.00	12.05	25.11
With latitude	0.96	0.79	1.01	0.85	1.07	0.96	1.03	0.87
AR confidence interval	[0.65,1.78]	[0.55,1.24]	[0.68,1.94]	[0.59, 1.36]	[0.70, 2.44]	[0.65, 1.79]	[0.70, 1.99]	[0.61, 1.41]
AR confidence interval, clustered	[0.64,2.49]	[0.55,1.20]	[0.68,2.86]	[0.59, 1.35]	[0.71, 3.44]	[0.67, 1.82]	[0.69, 2.90]	[0.62, 1.40]
F-stat, first stage	13.48	21.82	12.67	20.37	9.66	13.44	12.52	19.46
F-stat, first stage, clustered	7.30	19.26	6.89	17.14	6.10	10.37	6.93	16.32
Without neo-Europes	1.24	1.04	1.30	1.11	1.32	1.20	1.31	1.13
AR confidence interval	[0.78,3.09]	[0.67,1.99]	[0.82,3.35]	[0.73, 2.18]	[0.82, 3.81]	[0.77, 2.74]	[0.83, 3.37]	[0.74 2.25]
AR confidence interval, clustered	[0.76,5.43]	[0.65,2.10]	[0.78,5.97]	[0.70, 2.35]	[0.83, 4.72]	[0.78, 2.61]	[0.80, 5.60]	[0.72, 2.36]
F-stat, first stage	8.89	13.22	8.61	12.74	7.77	10.16	8.70	12.46
F-stat, first stage, clustered	5.54	11.27	5.43	10.77	6.19	10.38	5.64	11.09
Without Africa	0.61	0.61	0.65	0.64	0.93	0.93	0.68	0.67
AR confidence interval	[0.41,0.87]	[0.41,0.87]	[0.45,0.94]	[0.44, 0.94]	[0.59, 2.26]	[0.59, 2.26]	[0.47, 1.01]	[0.47, 1.01]
AR confidence interval, clustered	[0.44,0.85]	[0.45,0.85]	[0.46,0.94]	[0.47, 0.94]	[0.57, 2.32]	[0.57, 2.32]	[0.48, 0.99]	[0.48, 0.99]
F-stat, first stage	30.62	30.62	27.62	27.62	8.64	8.64	24.26	24.26
F-stat, first stage, clustered	45.98	45.98	36.16	36.16	8.16	8.16	32.41	32.41
With continent dummies	0.97	0.78	1.00	0.81	1.21	0.96	1.04	0.84
AR confidence interval	[0.59,3.20]	[0.52,1.42]	[0.60,3.95]	[0.52, 1.53]	[0.64,∞]	[0.56, 3.51]	[0.63, 4.02]	[0.55, 1.64]
AR confidence interval, clustered	[0.52, 4.87]	[0.45, 1.43]	[0.55, 6.14]	[0.46, 1.51]	[0.63,∞]	[0.53, 2.14]	[0.58, 4.97]	[0.49, 1.52]
F-stat, first stage	6.49	13.32	5.89	12.10	3.34	6.22	5.96	11.59
F-stat, first stage, clustered	4.68	10.61	4.42	10.03	3.20	7.35	4.79	10.90
With continent dummies and latitude	1.07	0.80	1.12	0.84	1.39	1.04	1.17	0.88
AR confidence interval	[0.57,∞]	[0.48, 1.93]	[0.59,∞]	[0.49, 2.22]	[0.63,∞]	[0.53,∞]	[0.61,∞]	[0.52, 2.54]
AR confidence interval, clustered	[0.44,∞]	[0.30, 1.53]	[0.47,∞]	[0.32, 1.64]	[0.58,∞]	[0.47, 4.79]	[0.49,∞]	[0.39, 1.72]
F-stat, first stage	3.71	8.52	3.36	7.67	1.87	3.80	3.37	7.25
F-stat, first stage, clustered	2.72	7.74	2.52	7.38	1.57	4.25	2.66	7.83
With percent of European descent in 1975	0.92	0.71	0.99	0.77	1.23	1.03	1.02	0.79
AR confidence interval	[0.55,2.31]	[0.44,1.27]	[0.59,2.92]	[0.48, 1.47]	[0.66, 30.44]	[0.58, 4.05]	[0.61, 3.13]	[0.49, 1.56]
AR confidence interval, clustered	[0.54, 4.32]	[0.37,1.21]	[0.57, 9.08]	[0.42, 1.42]	[0.66,∞]	[0.56, 6.22]	[0.58, 9.67]	[0.44, 1.45]
F-stat, first stage	8.67	15.32	7.45	13.27	4.17	6.12	7.19	12.38
F-stat, first stage, clustered	4.92	12.92	4.20	10.60	2.61	4.44	4.17	10.30
With malaria	0.67	0.52	0.74	0.56	2.03	1.08	0.79	0.61
AR confidence interval	[0.29,2.93]	[0.27,0.95]	[0.32,10.24]	[0.29,1.09]	[0.54,∞]	[0.45,∞]	[0.37,8.59]	[0.33,1.21]
AR confidence interval, clustered	[0.25,∞]	[0.23,0.89]	[0.28,∞]	[0.25,1.06]	[0.62,∞]	[0.48,∞]	[0.34,∞]	[0.30,1.12]
F-stat, first stage	5.38	13.95	4.27	11.90	0.46	2.45	4.41	11.43
F-stat, first stage, clustered	3.11	11.45	2.50	9.18	0.41	2.68	2.77	10.00

2SLS regressions, with one observation per country, corresponding to first-stage regressions in Table 1A. Coefficients and standard errors for covariates, where included, are not reported to save space. All variables are from AJR (2001). Dependent variable is log GDP per capita in 1995. Right-hand side variable is protection against expropriation, instrumented by log settler mortality. Column 1 uses original settler mortality series from AJR (2001) as the instrument. Column 2 uses original settler mortality series, capped at 250 per 1,000 per annum. Column 3 uses alternative settler mortality series, benchmarking Latin American data to Jamaica/Dominican Republic. Column 4 uses same series as column 3, but capped at 250 per 1,000. Column 5 uses mortality data directly from naval stations, without benchmarking. Column 6 uses same series as column 5, but capped at 250 per 1,000. Column 7 uses mortality data from naval stations, with life insurance data, without benchmarking. Column 8 uses same series as column 7, but capped at 250 per 1,000.

Table 2A

First Stage Regressions, Using Albouy Preferred Sample

	Alternative samples for settler mortality, using original AJR mortality series					
	Albouy Sample of 28	Albouy Sample of 28, without Gambia	Albouy Sample of 28, mortality capped at 250	Albouy Sample of 28, without Gambia, mortality capped at 250	Original AJR series, without contested observations in West and Central Africa	Original AJR series, without contested observations in West and Central Africa, mortality capped at 250
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable is protection against risk of expropriation					
No covariates	-0.59	-0.74	-0.95	-1.06	-0.66	-1.02
(standard error)	(0.19)	(0.15)	(0.24)	(0.22)	(0.18)	(0.18)
(clustered standard error)	(0.19)	(0.15)	(0.24)	(0.22)	(0.19)	(0.19)
Number of clusters	28	27	28	27	34	34
Number of observations	28	27	28	27	53	53
With latitude	-0.42	-0.59	-0.74	-0.88	-0.57	-0.94
(standard error)	(0.22)	(0.15)	(0.29)	(0.26)	(0.19)	(0.21)
(clustered standard error)	(0.22)	(0.15)	(0.29)	(0.26)	(0.20)	(0.21)
Number of clusters	28	27	28	27	34	34
Number of observations	28	27	28	27	53	53
Without neo-Europes	-0.32	-0.48	-0.52	-0.66	-0.43	-0.7
(standard error)	(0.19)	(0.12)	(0.23)	(0.20)	(0.18)	(0.21)
(clustered standard error)	(0.19)	(0.12)	(0.23)	(0.20)	(0.18)	(0.22)
Number of clusters	25	24	25	24	31	31
Number of observations	25	24	25	24	49	49
Without Africa	-1.00	-1.00	-1.00	-1.00	-1.21	-1.21
(standard error)	(0.28)	(0.28)	(0.28)	(0.28)	(0.20)	(0.20)
(clustered standard error)	(0.28)	(0.28)	(0.28)	(0.28)	(0.18)	(0.18)
Number of clusters	13	13	13	13	19	19
Number of observations	13	13	13	13	37	37
With continent dummies	-0.31	-0.48	-0.63	-0.75	-0.5	-0.89
(standard error)	(0.20)	(0.13)	(0.22)	(0.18)	(0.21)	(0.26)
(clustered standard error)	(0.20)	(0.13)	(0.22)	(0.18)	(0.21)	(0.26)
Number of clusters	28	27	28	27	34	34
Number of observations	28	27	28	27	53	53
With continent dummies and	-0.22	-0.4	-0.52	-0.66	-0.41	-0.78
(standard error)	(0.23)	(0.15)	(0.27)	(0.22)	(0.21)	(0.27)
(clustered standard error)	(0.23)	(0.15)	(0.27)	(0.22)	(0.22)	(0.27)
Number of clusters	28	27	28	27	34	34
Number of observations	28	27	28	27	53	53
With percent of European de	-0.29	-0.46	-0.49	-0.64	-0.48	-0.81
(standard error)	(0.21)	(0.15)	(0.29)	(0.25)	(0.19)	(0.20)
(clustered standard error)	(0.21)	(0.15)	(0.29)	(0.25)	(0.20)	(0.21)
Number of clusters	28	27	28	27	34	34
Number of observations	28	27	28	27	53	53
With malaria	-0.28	-0.44	-0.65	-0.7	-0.49	-0.87
(standard error)	(0.26)	(0.22)	(0.32)	(0.30)	(0.22)	(0.21)
(clustered standard error)	(0.26)	(0.22)	(0.32)	(0.30)	(0.24)	(0.23)
Number of clusters	27	26	27	26	33	33
Number of observations	27	26	27	26	51	51

OLS regressions, with one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Variables are from AJR (2001). Dependent variable is protection against risk of expropriation; independent variable is log settler mortality. Column 1 uses original settler mortality series from AJR (2001) as independent variable but Albouy's preferred sample of 28 countries. Column 2 is the same as column 1, but drops Gambia. Column 3 is the same as column 1, but caps mortality at 250 per 1,000 per annum. Column 4 is the same as column 1, but drops Gambia and caps mortality at 250. Column 5 is the same as column 1, but drops contested observations for West and Central Africa. Column 6 is the same as column 5, but caps mortality at 250.

Table 2B

Second Stage Regressions, Using Albouy Preferred Sample

	Alternative samples for settler mortality, using original AJR mortality series					
	Albouy Sample of 28	Albouy Sample of 28, without Gambia	Albouy Sample of 28, mortality capped at 250	Albouy Sample of 28, without Gambia, mortality capped at 250	Original AJR series, without contested observations in West and Central Africa	Original AJR series, without contested observations in West and Central Africa, mortality capped at 250
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable is protection against risk of expropriation					
No covariates	0.87	0.74	0.83	0.75	0.87	0.77
AR confidence set	[0.57, 1.64]	[0.50, 1.12]	[0.56, 1.40]	[0.52,1.13]	[0.63,1.32]	[0.56,1.08]
AR confidence set, clustered	[0.60, 1.82]	[0.55, 1.02]	[0.59, 1.35]	[0.56,1.07]	[0.62,1.62]	[0.57,1.12]
F-stat, first stage	12.47	22.31	17.13	24.38	22.55	33.34
F-stat, first stage, clustered	9.24	24.16	15.63	24.28	12.30	29.22
With latitude	0.82	0.63	0.73	0.62	0.89	0.75
AR confidence set	[0.40, 5.79]	[0.32, 1.31]	[0.35, 2.54]	[0.30,1.31]	[0.59,1.56]	[0.51,1.17]
AR confidence set, clustered	[0.42, 19.00]	[0.35, 0.97]	[0.30, 1.63]	[0.24,0.98]	[0.60,2.09]	[0.51,1.14]
F-stat, first stage	4.93	10.85	6.44	10.26	14.96	22.56
F-stat, first stage, clustered	3.62	14.42	6.50	11.84	8.02	19.49
Without neo-Europes	1.15	0.84	1.13	0.91	1.15	1.00
AR confidence set	[0.52, ∞]	[0.41, 1.98]	[0.51, ∞]	[0.43,2.83]	[0.71,2.87]	[0.61,2.03]
AR confidence set, clustered	[0.52, ∞]	[0.44, 1.51]	[0.51, 5.61]	[0.42,1.90]	[0.68,5.50]	[0.60,2.14]
F-stat, first stage	3.82	9.90	3.98	7.29	8.78	11.50
F-stat, first stage, clustered	3.00	16.18	4.99	10.76	5.29	9.90
Without Africa	0.90	0.90	0.90	0.90	0.61	0.61
AR confidence set	[0.52, 2.09]	[0.52, 2.09]	[0.52, 2.09]	[0.52,2.09]	[0.41,0.87]	[0.41,0.87]
AR confidence set, clustered	[0.63, 1.61]	[0.63, 1.61]	[0.63, 1.61]	[0.63,1.61]	[0.45,0.85]	[0.45,0.85]
F-stat, first stage	9.87	9.87	9.87	9.87	30.62	30.62
F-stat, first stage, clustered	12.30	12.30	12.30	12.30	45.98	45.98
With continent dummies	1.12	0.81	0.90	0.77	0.93	0.81
AR confidence set	[0.50, ∞]	[0.41, 2.15]	[0.44, 4.74]	[0.40,1.75]	[0.59,2.26]	[0.55,1.40]
AR confidence set, clustered	0.47, ∞]	[0.37, 1.38]	[0.39, 1.93]	[0.34,1.27]	[0.54,3.04]	[0.51,1.46]
F-stat, first stage	2.85	8.57	5.65	10.89	8.30	15.02
F-stat, first stage, clustered	2.48	12.83	8.06	16.83	5.61	11.77
With continent dummies and latitude	1.25	0.77	0.87	0.70	0.99	0.83
AR confidence set	[0.37, ∞]	[0.27, 5.26]	[0.28, ∞]	[0.24,2.96]	[0.56,5.99]	[0.52,1.80]
AR confidence set, clustered	[0.12, ∞]	[0.09, 1.71]	[-0.35, 4.35]	[-0.13,1.35]	[0.45,20.44]	[0.42,1.56]
F-stat, first stage	1.24	5.12	2.82	6.17	5.02	9.89
F-stat, first stage, clustered	0.91	7.06	3.61	9.05	3.57	8.61
With percent of European descent in 1975	0.94	0.65	0.82	0.65	0.67	0.60
AR confidence set	[0.33, ∞]	[0.24, 2.07]	0.22, ∞]	[0.16,3.38]	[0.40,1.17]	[0.36,0.94]
AR confidence set, clustered	[0.33, ∞]	[0.25, 1.36]	[-0.02, ∞]	[-0.03,1.71]	[0.33,1.12]	[0.30,0.90]
F-stat, first stage	2.53	7.20	2.80	5.32	16.42	22.69
F-stat, first stage, clustered	1.83	9.78	2.99	6.27	14.80	23.30
With malaria	0.71	0.65	0.72	0.67	0.56	0.54
AR confidence set	[-∞, ∞]	[-0.09, ∞]	[0.21, ∞]	[0.19,2.41]	[0.32,0.97]	[0.31,0.87]
AR confidence set, clustered	[-∞, ∞]	[-0.17,3.80]	[0.04,3.56]	[0.07,1.79]	[0.32,0.91]	[0.31,0.83]
F-stat, first stage	1.24	3.92	3.95	6.12	16.72	22.50
F-stat, first stage, clustered	1.09	3.93	4.07	5.39	14.62	19.06

2SLS regressions, with one observation per country, corresponding to first-stage regressions in Table 2A. Coefficients and standard errors for covariates, where included, are not reported to save space. Variables are from AJR (2001). Dependent variable is log GDP per capita in 1995. Right-hand side variable is protection against expropriation, instrumented by log settler mortality. Column 1 uses original settler mortality series from AJR (2001) as the instrument but Albouy's preferred sample of 28 countries. Column 2 is the same as column 1, but drops Gambia. Column 3 is the same as column 1, but caps mortality at 250 per 1,000 per annum. Column 4 is the same as column 1, but drops Gambia and caps mortality at 250. Column 5 is the same as column 1, but drops contested observations for West and Central Africa. Column 6 is the same as column 5, but caps mortality at 250.

Table 3A

First Stage Regressions, With Corrections to Albouy's "Campaign Dummy"

Alternative codings for campaign dummy, using original AJR mortality series

	AJR mortality series, Albouy campaign dummy	AJR mortality series, capped at 250; Albouy campaign dummy	AJR mortality series, minimal correction to Albouy campaign dummy	AJR mortality series, capped at 250; minimal correction to Albouy campaign dummy	AJR mortality series, extended correction to Albouy campaign dummy	AJR mortality series, capped at 250; extended correction to Albouy campaign dummy	AJR mortality series, Albouy sample of 28; campaign dummy	AJR mortality series, capped at 250; Albouy sample of 28; extended correction to Albouy campaign dummy; dropping Gambia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable is protection against expropriation							
No covariates	-0.45	-0.77	-0.52	-0.84	-0.6	-0.91	-0.35	-0.96
(standard error)	(0.17)	(0.21)	(0.17)	(0.19)	(0.15)	(0.17)	(0.22)	(0.26)
(clustered standard error)	(0.18)	(0.21)	(0.18)	(0.19)	(0.18)	(0.18)	(0.22)	(0.26)
Number of clusters	36	36	36	36	36	36	28	27
Number of observations	64	64	64	64	64	64	28	27
With latitude	-0.39	-0.72	-0.45	-0.79	-0.53	-0.86	-0.21	-0.86
(standard error)	(0.19)	(0.23)	(0.18)	(0.21)	(0.17)	(0.19)	(0.25)	(0.27)
(clustered standard error)	(0.20)	(0.23)	(0.20)	(0.21)	(0.19)	(0.19)	(0.25)	(0.27)
Number of clusters	36	36	36	36	36	36	28	27
Number of observations	64	64	64	64	64	64	28	27
Without neo-Europes	-0.31	-0.54	-0.33	-0.57	-0.39	-0.63	-0.18	-0.5
(standard error)	(0.16)	(0.22)	(0.16)	(0.20)	(0.16)	(0.20)	(0.22)	(0.25)
(clustered standard error)	(0.17)	(0.20)	(0.17)	(0.19)	(0.17)	(0.20)	(0.22)	(0.25)
Number of clusters	33	33	33	33	33	33	25	24
Number of observations	60	60	60	60	60	60	25	24
Without Africa	-1.11	-1.11	-1.16	-1.16	-1.22	-1.22	-0.88	-0.98
(standard error)	(0.28)	(0.28)	(0.22)	(0.22)	(0.20)	(0.20)	(0.32)	(0.29)
(clustered standard error)	(0.23)	(0.23)	(0.19)	(0.19)	(0.17)	(0.17)	(0.32)	(0.29)
Number of clusters	19	19	19	19	19	19	13	13
Number of observations	37	37	37	37	37	37	13	13
With continent dummies	-0.37	-0.7	-0.41	-0.74	-0.46	-0.8	-0.25	-0.73
(standard error)	(0.20)	(0.27)	(0.20)	(0.26)	(0.20)	(0.26)	(0.23)	(0.20)
(clustered standard error)	(0.22)	(0.27)	(0.21)	(0.25)	(0.21)	(0.25)	(0.23)	(0.20)
Number of clusters	36	36	36	36	36	36	28	27
Number of observations	64	64	64	64	64	64	28	27
With continent dummies and latitude	-0.3	-0.63	-0.34	-0.67	-0.38	-0.72	-0.14	-0.66
(standard error)	(0.22)	(0.29)	(0.21)	(0.28)	(0.20)	(0.26)	(0.26)	(0.23)
(clustered standard error)	(0.23)	(0.29)	(0.22)	(0.27)	(0.21)	(0.26)	(0.26)	(0.23)
Number of clusters	36	36	36	36	36	36	28	27
Number of observations	64	64	64	64	64	64	28	27
With percent of European descent in 1975	-0.27	-0.55	-0.34	-0.63	-0.42	-0.71	-0.2	-0.61
(standard error)	(0.17)	(0.21)	(0.17)	(0.20)	(0.17)	(0.19)	(0.23)	(0.27)
(clustered standard error)	(0.19)	(0.22)	(0.19)	(0.20)	(0.19)	(0.20)	(0.23)	(0.27)
Number of clusters	36	36	36	36	36	36	28	27
Number of observations	64	64	64	64	64	64	28	27
With malaria	-0.23	-0.62	-0.35	-0.73	-0.47	-0.83	-0.1	-0.6
(standard error)	(0.22)	(0.24)	(0.23)	(0.23)	(0.22)	(0.22)	(0.28)	(0.33)
(clustered standard error)	(0.24)	(0.26)	(0.25)	(0.25)	(0.24)	(0.24)	(0.28)	(0.33)
Number of clusters	35	35	35	35	35	35	27	26
Number of observations	62	62	62	62	62	62	27	26

OLS regressions, with one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Variables are from AJR (2001). Dependent variable is protection against risk of expropriation; independent variable is log settler mortality; all regressions include Albouy's "slave labor" dummy. Column 1 uses original settler mortality series from AJR (2001) as independent variable but includes Albouy's campaign dummy. Column 2 is the same as column 1, but caps mortality at 250 per 1,000 per annum. Column 3 is the same as column 1 but uses our minimal correction of the campaign dummy. Column 4 is the same as column 3 but caps mortality at 250. Column 5 is the same as column 1 but uses our extended correction of the campaign dummy. Column 6 is the same as column 5, but caps mortality at 250. Column 7 is the same as column 1 but uses Albouy's preferred sample of 28. Column 8 is the same as column 7, but uses our extended correction of the campaign dummy, drops Gambia, and caps mortality at 250.

Table 3B
First Stage Regressions, With Corrections to Albouy's "Campaign Dummy"

Alternative codings for campaign dummy, using original AJR mortality series

	AJR mortality series, Albouy campaign dummy	AJR mortality series, capped at 250; Albouy campaign dummy	AJR mortality series, minimal correction to Albouy campaign dummy	AJR mortality series, capped at 250; minimal correction to Albouy campaign dummy	AJR mortality series, extended correction to Albouy campaign dummy	AJR mortality series, capped at 250; extended correction to Albouy campaign dummy	AJR mortality series, Albouy preferred sample; campaign dummy	AJR mortality series, capped at 250; Albouy preferred sample; extended correction to Albouy campaign dummy; dropping Gambia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable is log GDP per capita in 1995							
No covariates	1.09	0.86	1.01	0.84	0.93	0.80	1.02	0.83
AR confidence set	[0.69, 2.61]	[0.56, 1.54]	[0.69, 1.85]	[0.58, 1.31]	[0.67, 1.45]	[0.59, 1.16]	[0.42, ∞]	[0.53,1.44]
AR confidence set, clustered	[0.65, 3.96]	[0.51, 1.54]	[0.65, 2.52]	[0.56, 1.35]	[0.65, 1.78]	[0.59, 1.18]	[0.44, ∞]	[0.58,1.34]
F-stat, first stage	9.21	15.17	13.89	21.65	20.91	30.53	3.11	15.17
F-stat, first stage, clustered	5.9	13.78	8.02	19.2	11.49	26.27	2.57	13.17
With latitude	1.15	0.85	1.06	0.82	0.96	0.79	0.9	0.66
AR confidence set	[0.66, 4.87]	[0.50, 1.81]	[0.66, 2.64]	[0.52, 1.48]	[0.64, 1.76]	[0.54, 1.25]	[-∞, ∞]	[0.33,1.49]
AR confidence set, clustered	[0.60, 34.78]	[0.41, 1.69]	[0.62, 4.35]	[0.48, 1.41]	[0.64, 2.30]	[0.55, 1.18]	[-∞, ∞]	[0.28,1.08]
F-stat, first stage	5.91	10.52	8.69	14.61	13.81	21.09	0.89	8.94
F-stat, first stage, clustered	3.67	9.43	5.31	13.71	7.82	19.7	0.67	9.83
Without neo-Europes	1.45	1.13	1.4	1.12	1.24	1.03	1.51	1.2
AR confidence set	[0.78, 22.39]	[0.62, 4.15]	[0.79, 7.44]	[0.65, 3.15]	[0.76, 3.42]	[0.64, 2.20]	[0.27, ∞]	[0.46, ∞]
AR confidence set, clustered	[0.75, ∞]	[0.56, 3.07]	[0.77, 20.94]	[0.62, 2.50]	[0.74, 5.99]	[0.64, 2.10]	[0.26 ∞]	[0.48,6.67]
F-stat, first stage	4.34	6.39	5.44	7.83	8.09	11	0.93	3.28
F-stat, first stage, clustered	3.17	6.89	3.94	8.57	5.11	10.28	0.67	4.11
Without Africa	0.66	0.66	0.64	0.64	0.61	0.61	0.92	0.96
AR confidence set	[0.41, 1.08]	[0.41, 1.08]	[0.42, 0.96]	[0.42, 0.96]	[0.41, 0.88]	[0.41, 0.88]	[0.39, 14.18]	[0.53,2.93]
AR confidence set, clustered	[0.45, 1.02]	[0.45, 1.02]	[0.45, 0.93]	[0.45, 0.93]	[0.44, 0.84]	[0.44, 0.84]	[0.54, 2.03]	[0.61,1.78]
F-stat, first stage	17.88	17.88	24.5	24.5	30.42	30.42	4.65	7.55
F-stat, first stage, clustered	23.03	23.03	37.47	37.47	51.1	51.1	7.52	11.86
With continent dummies	1.06	0.81	1.03	0.82	0.96	0.79	1.23	0.81
AR confidence set	[0.58, 20.72]	[0.48, 2.00]	[0.60, 5.34]	[0.51, 1.74]	[0.59, 2.86]	[0.51, 1.47]	[0.40, ∞]	[0.41,2.11]
AR confidence set, clustered	[0.51, ∞]	[0.37, 1.77]	[0.51, 8.77]	[0.41, 1.61]	[0.54, 3.68]	[0.45, 1.46]	[0.29, ∞]	[0.37,1.38]
F-stat, first stage	4.2	8.35	5.29	10.09	7.08	12.67	1.48	9.06
F-stat, first stage, clustered	2.98	6.82	3.96	8.63	5.04	9.88	1.21	12.96
With continent dummies and latitude	1.19	0.83	1.15	0.84	1.05	0.8	1.44	0.68
AR confidence set	[0.56, ∞]	[0.44, 3.52]	[0.58, ∞]	[0.47, 2.60]	[0.57, 32.57]	[0.47, 1.96]	[-∞, ∞]	[0.22,3.51]
AR confidence set, clustered	[0.37, ∞]	[0.14, 2.20]	[0.42, ∞]	[0.24, 1.81]	[0.48, ∞]	[0.31, 1.48]	[-∞, ∞]	[-0.10,1.28]
F-stat, first stage	2.45	5.64	3.06	6.75	4.09	8.33	0.37	5.62
F-stat, first stage, clustered	1.73	4.81	2.31	6.13	3.19	7.64	0.29	8.3
With percent of European descent in 1975	1.18	0.73	1.03	0.72	0.91	0.7	1.13	0.69
AR confidence set	[0.54, ∞]	[0.31, 2.88]	[0.55, 6.82]	[0.38, 1.69]	[0.54, 2.37]	[0.41, 1.30]	[0.13, ∞]	[0.17,17.78]
AR confidence set, clustered	[0.54, ∞]	[0.13, 1.95]	[0.50, ∞]	[0.21, 1.39]	[0.52, 3.78]	[0.32, 1.18]	[-∞, ∞]	[-0.01,1.90]
F-stat, first stage	2.88	6.01	5.02	9.35	8.49	13.94	1.01	4.22
F-stat, first stage, clustered	2.01	6.19	3.38	9.85	5.02	12.76	0.78	5.05
With malaria	0.84	0.48	0.75	0.52	0.68	0.53	1.17	0.8
AR confidence set	[-∞, ∞]	[0.00,1.91]	[0.21, ∞]	[0.20,1.19]	[0.31,2.44]	[0.27,0.97]	[-∞, ∞]	[0.21,9.14]
AR confidence set, clustered	[-∞, ∞]	[-0.41,1.08]	[-∞, ∞]	[0.07,0.98]	[0.27,11.30]	[0.23,0.85]	[-∞, ∞]	[0.09,12.90]
F-stat, first stage	1.15	5.48	2.98	9.09	5.97	13.67	0.14	4.46
F-stat, first stage, clustered	0.95	5.75	2.00	8.54	3.69	12.01	0.13	3.28

2SLS regressions, with one observation per country, corresponding to first-stage regressions in Table 3A. Coefficients and standard errors for covariates, where included, are not reported to save space. Variables are from AJR (2001). Dependent variable is log GDP per capita in 1995. Right-hand side variable is protection against expropriation, instrumented by log settler mortality. Column 1 uses original settler mortality series from AJR (2001) as the instrument but includes Albouy's campaign dummy. Column 2 is the same as column 1, but caps mortality at 250 per 1,000 per annum. Column 3 is the same as column 1 but uses our minimal correction of the campaign dummy. Column 4 is the same as column 3 but caps mortality at 250. Column 5 is the same as column 1 but uses our extended correction of the campaign dummy. Column 6 is the same as column 5, but caps mortality at 250. Column 7 is the same as column 1 but uses Albouy's preferred sample of 28. Column 8 is the same as column 7, but uses our extended correction of the campaign dummy, drops Gambia, and caps mortality at 250.

Figure 1A: Gambia As Outlier in Albouy's Preferred Sample of 28 (Without Controls)

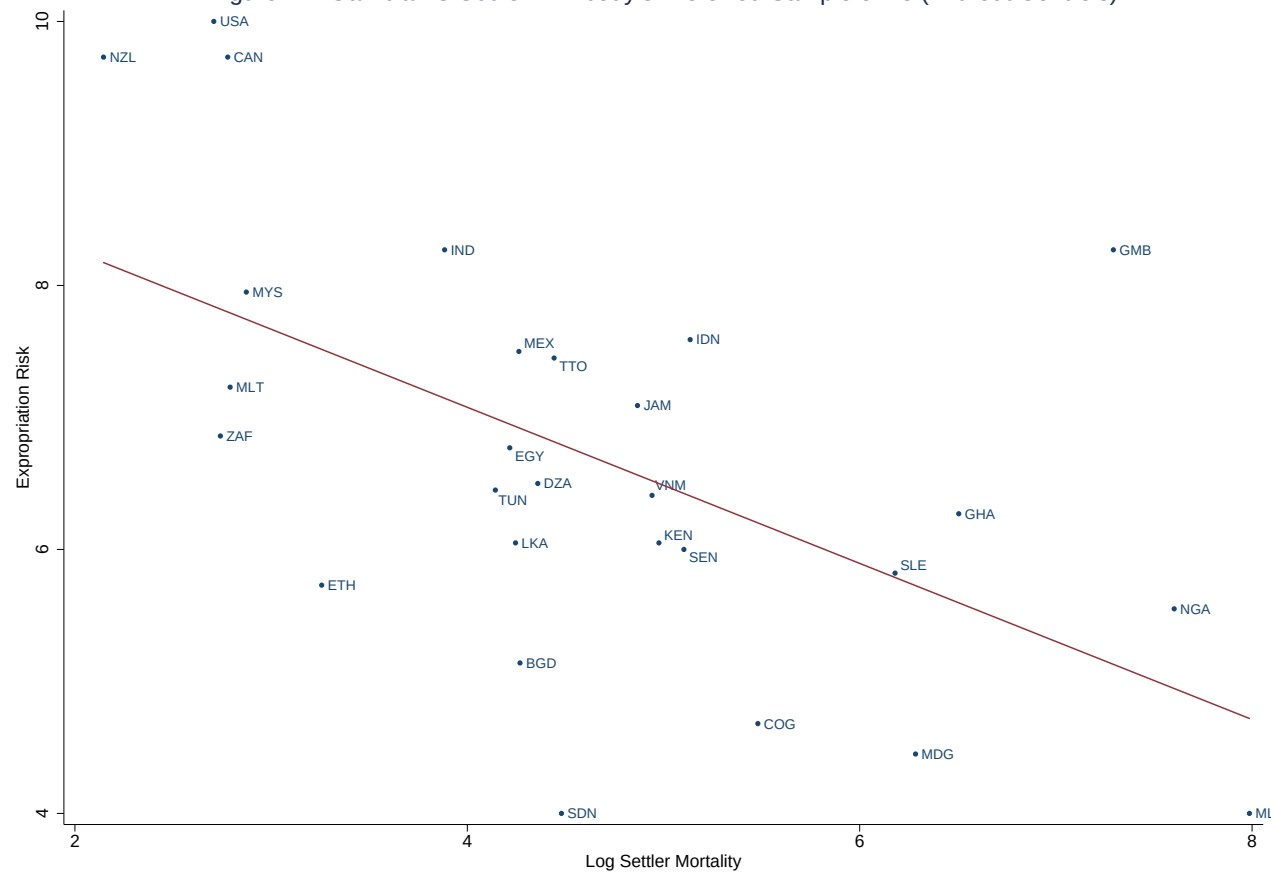

Figure 1B: Gambia As Outlier in Albouy's Preferred Sample of 28 (With Continent Dummies and Latitude)