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

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Abstract

Acemoglu, Johnson, and Robinson (2001) established that economic institutions today are correlated with what was expected mortality for Europeans if they moved to countries within their colonial empires. David Albouy argues that this relationship is not robust. Specifically, he wants to drop all data from Latin America and much of the data from Africa, making up almost 60% of our sample. This is unwarranted - there is a great deal of specific information on the mortality of Europeans in those places during the colonial period. He also includes a "campaign" dummy that is coded inconsistently and seriously at odds with the historical record; even modest corrections undermine his claims. We also show that limiting the effect of outliers significantly strengthens our results, making them robust to even more extreme versions of Albouy's critiques.

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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).

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 willingness to establish 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 have "extractive" institutions imposed. We also posited that this early pattern of institutions has persisted over time and influences the extent and nature of institutions in the modern world. On this basis, we proposed using estimates of potential European settler mortality as an instrument for institutional variation in former European colonies 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 data collection and statistical work initiated by the British military in the mid-19th century. These data became part of the foundation of both contemporary thinking about public health (for soldiers and for civilians) and the life insurance industry (as actuaries and executives considered the risks inherent in overseas travel), and shaped the perceptions of Europeans - including potential settlers and their medical advisers.[2]

In his Comment on AJR (2001), David Albouy (2011) focuses on one part of our argument and claims two major problems: first, our Latin American data and some of our African data

[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.

[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. The base sample in AJR (2001) comprised 64 of these modern countries, which also had available GDP per capita and institutional quality measures.

are unreliable; and second, we inappropriately mix information from peacetime and "campaign" episodes.[3] To deal with the first issue, he discards completely almost 60% of our sample - effectively arguing there is no reliable information on the mortality of Europeans in those parts of the world during the colonial period. To deal with the second issue, he codes his own "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 completely unhelpful for determining whether institutions affect income today.

Neither of Albouy's claims is compelling. First, there is no justification for discarding most of our data. Ordinary Europeans, military establishments, the medical profession, and the rapidly developing life insurance industry collected, published, and discussed an abundance of relevant information on European mortality rates in many different parts of the world during the 19th century.[4] Our original coding for 64 modern countries and the additional robustness checks reported in AJR (2005) draw on this information. Simply throwing out data is certainly not a reasonable approach to deal with this wealth of information. We repeat below some of the extensive robustness checks that were originally reported in our working paper version, AJR (2000), and also show that the main results in AJR (2001) are robust to incorporating existing information on mortality rates in various reasonable ways (a point also made in AJR 2005, 2006, and 2008).

Albouy needs to discard almost 60% of our original sample in order to undermine our results. And even Albouy's preferred regression results turn out to be largely driven by one outlier, Gambia, which has very high mortality - combined with a relatively favorable coding of its institutions that stands at odds with its recent history. Limiting the effect of high mortality outliers - by capping mortality at 250 per 1000 per annum or by excluding Gambia as an extreme outlier - makes our results robust even in Albouy's smallest sample (i.e., with just 28 and 27 observations, respectively).

[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). In previous work, we rebutted those of his claims that do not appear in his AER Comment (see AJR 2005, 2006, and 2008), so this Reply only addresses those critiques that remain in his AER Comment.

[4] The information was available in medical and public health discussions (see AJR 2005, 2006, and 2008; Hirsch, 1888, summarizes the state of relevant medical knowledge at the end of the 19th century - see Appendix B below; much of this knowledge was built by A.M. Tulloch and his colleagues, starting earlier in the century, Tulloch 1838a, 1838b, 1838c, 1840, 1841, 1847). It was also manifest in the life insurance literature (see Institute of Actuaries, 1851-52, and Hunter, 1907). This information, together with the discussion in Curtin (1964), makes it clear that Albouy's claim that there is no reliable knowledge about mortality for Europeans in most of their colonial empires is simply untenable.

Second, Albouy argues that military "campaigns" pushed up mortality rates above what they would be in peacetime. However, there was little difference in practice between the activities that soldiers were engaged in during most colonial "campaigns" for which we have data and at other times. They marched, lived at close quarters, and were exposed to local disease vectors - particularly mosquitoes and contaminated water. Most of these campaigns did not involve much actual fighting; in some instances the soldiers traveled in the (then) luxury of steamers or on mules - or someone else carried their equipment. As a result, there appear to be no systematic differences in mortality rates between episodes that can reliably be classified as "campaigns" and the rest. We show that even with Albouy's own coding, which is problematic as we explain below, his "campaign" dummy is typically far from significant in first or second stage regressions. Moreover, it is difficult or impossible - and makes little sense - to systematically distinguish campaigns and non-campaigns on the basis of existing information.[5]

In addition and perhaps more importantly, Albouy's classification of campaigns is inconsistent and seriously at odds with the historical record in at least 10 cases. His results depend on this inconsistent and inaccurate coding. When we make even minor adjustments, it becomes even more apparent that there is no basis for his claims. Moreover, limiting the effect of very high mortality rates restores the robustness of our results even without correcting the inconsistencies in his coding (except in the very smallest of his samples, where the inconsistency and inaccuracy of his coding turn out to be somewhat more consequential).

We agree that robustness checks are important, particularly for any historical exercise of this nature. Specifically, it is reasonable to ask: Do the outliers in our data - i.e., very high mortality rates - reflect unusual, unrepresentative spikes in mortality that were not European experience on average, or not what they expected if they migrated to a particular place? We emphasized and attempted to deal with any such potential concerns in our original working

[5] Curtin, for example, mentions when some episodes involved campaigning, but this is far from providing the basis for a systematic coding. Specifically, he does not provide detail on what soldiers were doing when not engaged in a specific named campaign - probably because there are not good records of exactly how they spent their time or under what exact conditions (including how much marching they did and what kind of shelter they had). In addition, one of Curtin's (1998) main arguments is how militaries were able to bring down campaign mortality in the late 19th century, below what could be achieved if the troops stayed put (e.g., Chapter 3, "The March to Kumasi" in Curtin, 1998.)

We have also reviewed the historical literature on military campaigns in the 19th century, including but not limited to the multi-volume *History of the British Army* by Fortescue (1929). Most of the military activities that generate information on mortality that is picked up in our dataset were too low level and inconsequential to be covered in the standard histories. It is not possible to say how much of the soldiers' time was spent travelling rather than remaining stationary, or when they fired their weapons. The distinction that Albouy pursues is illusory in these historical episodes.

paper version, AJR (2000).[6] In particular, some of our highest mortality estimates may be very high because of epidemics, unusual idiosyncratic conditions, or small sample variation, and thus potentially unrepresentative 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).[7] In AJR (2001), we argued that very high mortality rates could be viewed as a form of measurement error and, provided that it did not significantly deviate from classical measurement error, this would not create an asymptotic bias for our IV procedure.

Partly prompted by an earlier version of Albouy's Comment,[8] in AJR (2005) we proposed the alternative strategy of capping mortality estimates at 250 per 1000.[9] The 250 per 1,000 estimate was suggested by A.M. Tulloch, the 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).[10] This capping strategy has several attractive features. First, provided that settler mortality is a valid instrument, a capped version of it is also a valid instrument.[11] Second, on a priori grounds one might expect that mortality rates above a certain level should not have much effect on settler behavior.[12] Third, it is an effective strategy for reducing

[6] 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.

[7] 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).

[8] As we discuss further in subsection 3.6, the current version of Albouy's Comment also recommends capping the mortality rate for Mali at a lower level because he thinks the reported mortality rate is too high. In particular, in his text, Albouy says that Mali should be reduced from 2,940 per 1,000 per annum to 478.2 per 1,000; but in his dataset (online, last checked September 19, 2011) he appears to cap it at 280 per 1,000. But he does not do this for other similarly high mortality rates. For example, he does not apply the same cap for Gambia - even though the very high mortality rate (1,470 per 1,000 p.a.) there was presumably due to an epidemic, as was the case in Mali. As discussed above, Gambia is a significant outlier that matters a great deal for Albouy's hypothesis.

[9] We follow Curtin and the 19th century literature by reporting mortality per 1000 mean strength (also referred to as "with 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.

[10] Note that 250 per 1000 is still a dauntingly high mortality rate. Potential settlers were definitely deterred by the prospect that about a quarter of their number would die within the first year, even if there was not a major epidemic - for example, of yellow fever. 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. Curtin (1964, p.86), for example, writes: "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."

[11] The AJR (2001) 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.

[12] For example, it is reasonable to presume that no Europeans, perhaps except the very small group of the

the impact of various types of measurement errors, which are likely to be present in settler mortality data.[13] Fourth, we show below that the specific level of cap we use has little effect on the results. This strategy not only further establishes the robustness of the results in AJR (2001) to reducing the effects of outliers, but also shows that, when the effect of outliers is so limited, even extreme versions of Albouy's other modifications leave these results largely robust.

Albouy proposes a number of other changes to our data. None of these are consequential for our results, but two are worth discussing - not only to dispel some of Albouy's claims, but also because they shed light on his approach. Albouy recodes our Mali data, arguing that it is too high due to an epidemic. But as mentioned above, systematic capping along these lines - to reduce the influence of outliers and potentially unrepresentative observations in a way that is consistent across countries - considerably strengthens our results. Albouy also objects to how we use the term "French Soudan" and claims that we have made a mistake in our coding. This is not only inconsequential, but our use of this term was quite explicitly explained in Appendix B of AJR (2000) and again in AJR (2005).[14]

Overall, Albouy's Comment amounts to a series of objections to our approach. All of these objections, upon closer inspection, are far from compelling, are often unfounded, and prove minor and largely inconsequential for the robustness of our results. The big picture from AJR (2001) remains intact and remarkably robust: Europeans were more likely to move to places that were relatively healthy, and when they moved in larger numbers, they imposed better institutions, which have tended to persist from the colonial period to today.

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 Albouy's concerns in detail. Section 4 concludes. Appendices A, B and C, which are available on our websites (linked to the AER website), provide further details on econometrics, data for contested observations, and historical background.

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most reckless adventurers, would have considered settling in places where they faced probabilities of death above 25% in the first year.

[13] Trimming (dropping observations with extreme values) and winsorizing (capping extreme values) are common strategies for dealing with outliers, potentially contaminated data, as well as classical and non-classical measurement error (see, e.g., Angrist and Krueger, 1999). A large literature in statistics investigates alternative strategies for reducing the influence of outliers, measurement error, and contaminated data. A particularly popular strategy is to reduce (or bound) the influence of outlying and less reliable observations (e.g., Huber, 1964, Rousseeuw, 1984). Capping - winsorizing - is one way of achieving this (e.g., Wilcox, 2001, Andersen, 2008). We discuss how trimming affects our results in subsection 3.1.

[14] The Appendix B to AJR (2000) was not only explicit that the term "French Soudan" refers to a large area of Western and Central Africa but also clearly named the countries this covered (see subsection 3.6 for details). Albouy prefers another definition of French Soudan, which is limited to a small part of West Africa. "Bilad al-Sudan," from which the term Sudan originates, means "the land of black people" in Arabic.

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. 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.[15] 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 similar (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 (Curtin, 1989, and 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

[15] 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 - and in places where they perceived mortality to be in this range. The available evidence not only enables us to have a fairly good idea about which parts of the globe had such very high mortality rates, but also establishes that Europeans were at the time well aware of these mortality rates.

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 is 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 definitely not from major wars involving mass 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, 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 data from periods withour major wars (preferably before 1850), and we tried to stick to this throughout.[16]

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 number from Curtin when such data were available - excluding episodes with significant wars.[17] The 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.[18]

In AJR (2000), we provided a detailed analysis of an alternative series without this type

[16] 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).

[17] Some of these data are from expeditions - but these were a common occurence in the colonial era. 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. These were all small expeditions and none of them get more than a passing mention in the multi-volume *History of the British Army* by Fortescue (1929); they were not seen as significant wars by the British military or its historians. Examples of significant conflicts from which we did not use data include the siege of Cartegena in 1742 (Curtin 1989, p.2) and the invasion of Egypt in 1882 (Curtin 1998, p. 158).

[18] 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.

Appendix B discusses the various sources available 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.

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 in Central and South America, his work contained estimates only for Mexico.[19] To supplement the numbers from Curtin, we used an article by Hector Gutierrez (1986) on the mortality rates of bishops in Latin America (i.e., Central and South America, including some data on the Caribbean).[20] 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.[21] 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 - making for mortality estimates that were more plausible, given the available qualitative evidence.[22] In AJR (2000), these issues were extensively discussed and we reported that our results were robust using either type of benchmarking (see again below).

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

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

[21] 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.

[22] This choice seemed less favorable to our hypothesis and thus also 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).

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 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. 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.[23]

We should note that as discussed in AJR (2000, 2001), the specification in the last row that includes current prevalence of malaria is highly problematic - and likely to bias results against finding both a significant first stage and second stage relationship - because the 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 emphasized the potential bias that it would create against our hypothesis was a serious concern.[24] As a matter of fact, this was the least robust specification in AJR (2001) as the results in Table 1A and Table 1B here also show. In what follows, unsurprisingly, this will be the specification in which 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

[23] We drop the African data because in an earlier comment Albouy did the same.

[24] 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).

mortality rates in neighboring countries.[25]

Table 1A begins with first stage results using the original AJR data (column 1), corresponding to columns 1 of Table 4 of AJR (2001).[26] 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.[27]

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, with log settler mortality as the instrument. In AJR (2001), we followed standard 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).[28] For the baseline estimate with clustering, 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 AR confidence interval is the union of two disjoint and unbounded intervals: $[-\infty, -4.72]$ and $[0.44+, \infty]$ (or $[-\infty, -27.23]$ and $[0.57, +\infty]$ without clustering). As also suggested 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

[25] 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).

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

[27] 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 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.

[28] We do this mostly because Albouy has emphasized the importance of using Anderson-Rubin confidence sets and reports only these in his comment. In fact, since there is only one endogenous regressor and one instrument, these make little difference relative to the more standard Wald confidence intervals that also allow non-spherical errors.

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.

hypothesis that institutions have no effect on GDP per capita. To be sure, such a confidence interval is still a sign of imprecise estimates and cause for concern, 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.

In summary, the different specifications in column 1 of Table 1B confirm the results in AJR (2001) that institutions have a significant positive effect on income per capita, though in specifications that include continent dummies and latitude together or malaria, the confidence sets are quite wide.

3 Are The AJR (2001) Results Fragile?

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 discussed the source of these data and flagged clearly when they were due to epidemics. If epidemics occurred with some regularity - or if they were rare and yet still affected European perceptions of mortality for settlers - such mortality data are highly relevant. But if these epidemics were one-off or seen as rare, then they introduce additional, perhaps significant measurement error. We first investigate whether extreme mortality rates and outliers drive the results in AJR (2001). Our strategy, as explained in the Introduction, is to cap the settler mortality rates so as to reduce the influence of extreme observations. This strategy not only helps us limit the influence of very high mortality rates (cfr. footnote 13), but is also reasonable on a priori grounds, since we may expect that European settlement behavior was not very sensitive to variations in mortality rates above a certain threshold.

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, p.7).[29] 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 10 in the Introduction). In our data,

[29] 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.

there are 13 countries with mortality rates above 250 per 1000 per annum.

The use of this cap is also consistent with the assessment of Philip Curtin, the leading historian of comparative colonial mortality rates. At the start of Curtin's 1998 book, *Disease and Empire: The Health of European Troops in the Conquest of Africa*, he says of West Africa: "At that time [before the 1860s], the disease mortality of newly arrived Europeans was higher than it was anywhere else in the world. It was so high, in fact, that one historical problem is to discover why people were willing to go to a place where the probability of death was about 50 percent in the first year and 25 percent a year thereafter" (p.1).

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).[30] 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. These results are not specific to capping the potential settler mortality rate at 250 - they apply with a range of reasonable caps as we show next.

Appendix Table 1 reports results using alternative caps. Columns 1 and 2 repeat the results with the original data from AJR (2001) and with the 250 cap. Columns 3-5 show that the results are very similar if we instead cap settler mortality at 150, 280 or 350 (see also column 6 of Table 1B in AJR, 2005). The rate of 150 per 1,000 is suggested by our reading of the contemporary life insurance literature.[31] A cap of 350 reflects the fact that, as the Curtin quote above illustrates, mortality for newly arrived Europeans could be higher than for people who survived the first year. The rate of 280 is another obvious alternative, since 5 of the 13 countries above 250 in our data are at 280. The results in each case is very similar to those in column 2, and generally considerably more stable than those in column 1, where outliers play a more consequential role.

As discussed in footnote 13, one might also attempt to deal with concerns related to unrepresentative data and measurement error by trimming the outliers rather than capping them. In this context, capping is a more attractive strategy both because the sample is already small,

[30] Note that a few of the highest mortality rates in AJR (2001) 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.

[31] The Institute of Actuaries (1851-52) provides comparative data that are broadly consistent with our original estimates (see Appendix B). But our reading of this source suggests that the highest expected mortality rates for insured civilians may have been around 150 per 1,000 per annum; see, for example, Sprague (1895) and Hunter (1907).

and because, as we discuss below, a wealth of other information suggests that places with very high mortality rates in our sample are indeed those with the highest mortality rates for Europeans - even though, most probably, these rates did not exceed 20% or 25% mortality by much. Nevertheless, for completeness, the remaining columns of Appendix Table 1 show the implications of trimming the very high mortality rates. In particular, column 6 drops observations with mortality above 250, and columns 7 and 8 do the same for 150 and 350, respectively.[32] Notably, the results are very similar to the other columns of the table, and the confidence intervals are more precisely estimated despite the smaller numbers of observations. This bolsters the case that reducing the influence of the very high mortality rates is a useful strategy for dealing with potential measurement error.

In summary, some of the mortality estimates from Curtin are very high, partly driven by unusual conditions, the impact of epidemics, or small samples. In AJR (2000, 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 in Curtin's original sources (in particular Tulloch's mid-19th century research), we capped mortality rates at 250 (per 1000 per annum). In the analysis below, for all relevant specifications we also show results including the mortality cap at 250. This mortality cap, or other reasonable caps, reduce the effect of extreme settler mortality observations. We will show below that capping mortality also demonstrates that, rather than the AJR (2001) results being fragile, it is Albouy's results that are far from robust.

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.[33] We agree that results

[32] Trimming observations above 280 is the same as trimming those above 350, since there are no observations with settler mortality rates between 280 and 350. Naturally, capping at 280 vs. 380 gives different results as columns 4 and 5 show.

[33] He also argues, for example, around Appendix Table A2, that we simply have no idea about relative mortality in South and Central America. But as we have discussed, in addition to the evidence from Gutierrez, there is quantitative evidence on relative mortality in South and Central America from British South American

using this procedure should be subject to robustness checks. This was the approach in AJR (2000) and in all our subsequent work.

Column 3 of Table 1A 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.[34] We continue to assign countries to mortality regions as in AJR (2001). With this alternative benchmarking, the results are almost identical in all specifications to those in column 1.[35] 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 (consisting of two disjoint intervals), but exclude zero.

Column 4 of Table 1A shows first stage results with the same measure of mortality (as in column 3) but now capped at 250 per 1,000. 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.[36] 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

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.

[34] 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 a mortality rate (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 27).

[35] 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 (both are in the low zone; see previous footnote).

[36] 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.

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.[37] This is a conservative - i.e., low - mortality coding for Latin America.[38] 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.[39]

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 consists of two disjoint intervals extending to infinity. 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 consist of two disjoint intervals 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.[40] In Curtin's data (1989, Table 1.1), Mauritius has a

[37] 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"). Note that the ratio of military to civilian mortality may have changed in the second half of the nineteenth century; this point is examined further in Acemoglu, Johnson, and Subramanian (2011).

[38] 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).

[39] To be clear, we are 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 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.

[40] 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), which

mortality rate of 30.5 per 1,000, while the West Indies average is 93.25.[41] 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 consist of two disjoint intervals. 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 consist of two disjoint intervals extending 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.

In summary, there is no basis whatsoever to discard all of our Latin American data as Albouy does. In contrast 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 discuss the remaining 20 countries that Albouy drops, which include 12 in Africa, 4 in the Caribbean, 3 in Asia, and Australia.[42] Albouy's proposition is that there is no reliable knowledge about 19th century mortality for Europeans in those places. Our contention is that for each of the countries under discussion, both Europeans at the time and we presently 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

provides the earliest comprehensive assessment of comparative mortality rates. The life insurance literature from this period developed rapidly and views were revised and refined subsequently; this is discussed further in Acemoglu, Johnson, and Subramanian (2011).

[41] 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).

[42] 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.

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 of that time by Hirsch (1888) and its modern-day equivalent, Kiple (1993).[43]

A brief summary of countries dropped by Albouy is as follows. On Australia, to which the New Zealand rate was assigned in AJR (2001), Tulloch (1847, p.253) provides an almost identical and independent mortality estimate (see also AJR, 2005). 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 Meikle (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

[43]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 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).

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.[44]

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 (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).

While the overall patterns of European mortality in West and Central Africa reflected in AJR (2000, 2001) are well supported by contemporary and modern sources, some of this validation is more qualitative than for our 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 sample 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 consist of two disjoint intervals, extending to infinity on either side. Though in most cases a zero coefficient of institutions in the second stage regression can be rejected, such wide (and disjoint) confidence sets are a clear cause for concern.

[44] 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 implies a mortality rate within 5-10 per 1,000 of the Morocco level.

Figures 1A and 1B, however, show that the first stage in Albouy's preferred sample is at least partly weakened by a single significant outlier, Gambia.[45] 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 terms of mortality (1,470 per 1,000). But in the 64 country sample of AJR (2001), it did not have a consequential impact on results. It becomes much more of an outlier when Albouy drops 36 other observations. In addition, there is strong reason to suspect that this institutions score is not a true reflection of institutional quality in Gambia. For example, there were military coups in Gambia in 1981 and 1994, and other political turmoil was manifest the late 1980s. There has not been a return to free and fair elections since 1994. Moreover, between 1981 and 1989, Gambia and Senegal were united as part of the Senegambia Confederation, but throughout Gambia receives a much higher institutions score than Senegal.[46]

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 to 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 consist of two disjoint intervals extending 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 consist of two disjoint intervals, and a zero effect can be rejected at 5%.

[45] 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.

[46] 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 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.

3.5 Albouy's "Campaign" Dummy

Albouy's second major 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 in 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.[47]

In fact, a major emphasis of Curtin (1998) is that some 19th century military expeditions could have low mortality for some of the reasons we discuss here. 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).

The historical record does not indicate systematic differences in mortality between what can be classified as "campaign" and the rest. Sometimes periods of fighting brought even less sanitary conditions and higher mortality rates. But at other times, the organizers of campaigns were able to hold down mortality below what would have been experienced by civilian settlers - for example when the troops were in country for just a short while during a relatively healthy time of year, or when the doctors in charge happened to be of the opinion that large doses of quinine must be taken (the general medical consensus, unfortunately, took a long time to converge on this point).

At other times, campaigning proved a good way to reduce military mortality compared to sitting in barracks. The mosquito vector for yellow fever has a very short range - less than a hundred yards (Curtin, 1998, p.9). In many instances, physically changing location - for campaign purposes or in a deliberate evacuation - was enough to reduce the impact of an epidemic. The third major cause of death was disease due to contaminated water, particularly typhoid, dysentery, and related diseases. Most of the contamination was caused by poor sanitation, in the sense that human waste was allowed to find its way into drinking water. Generally speaking, this contamination was a slow process due to poor design of waste

[47] Curtin does mention some campaigns as part of his historical discussion, but he does not offer a systematic non-campaigns vs. campaign distinction. Albouy quotes very selectively from Curtin (1989).

removal and storage (Curtin 1998, Chapter 5) - again, this was more likely to be a problem when people were in a fixed location for a prolonged period (i.e., not campaigning). If an expedition camps in one place and moves on the next day, fecal contamination of the water supply is less likely.

Even more importantly, Albouy's procedure for coding this dummy is 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 Curtin 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 further 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 when minimal corrections for inconsistency are made to his coding.

The first columns of Tables 3A and 3B show Albouy's first stage and second stage results for the full sample and includes his campaign dummies - as well as dummy for "slave labor" that he introduces. Although the inclusion of the campaign and slave labor dummies leads to wide confidence intervals in several specifications (in particular, in rows 3, 5, 6, 7 and 8), it is notable that these dummies themselves are not significant individually or jointly in either the first or the second stage estimation in any of these specifications.[48] This is of course unsurprising given the historical record discussed above indicating that there were no systematic differences between "campaigns" and non-campaigns.

For example, in column 1 of Table 3A, the estimates (standard errors in parentheses) of the campaign dummy in rows 1-8, respectively, are -0.72 (0.46), -0.71 (0.46), -0.43 (0.42) -0.31

[48] Albouy reports the estimates and the (lack of) statistical significance of these dummies in his Appendix Table A4 but not in his published text.

(0.38), -0.61 (0.49), -0.59 (0.49), -0.72 (0.43), and -0.88 (0.53). The corresponding numbers for the slave dummy in rows 1-3 and 5-8 (since the slave dummy is dropped for the regression without Africa) are –1.61 (0.89), -1.39 (0.91), -1.34 (0.89), -1.45 (0.93), -1.20 (0.94), and -1.40 (0.98). In none of these cases are the two dummies jointly statistically significant at 15% or less. They are also very far from statistical significance in the second stage regressions reported in Table 3B. Therefore, there does not appear to be any support from the data that these dummies, even in their miscoded form, belong either in the first or the second stage regressions. To the extent that they are affecting the AJR (2001) results, this is likely to be due to their spurious presence.

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 consist of a single interval excluding a zero effect of institutions, except in the specification with malaria.[49]

Column 3 implements the minimal corrections to Albouy's "campaign" dummy - just for Hong Kong and New Zealand, which are very clearly inconsistently coded as noted above. This minor correction also leads to more precisely estimated second stage results. Column 4 shows that if in addition we 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.[50] In both situations, clustered AR confidence sets again consist of a single interval, excluding zero effects in the second stage (except that in the specification with malaria, where they exclude zero marginally without clustering and do not exclude it with clustering). The campaign and slave dummies continue to be insignificant both in the first and second stages in these regressions.

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

[49] The campaign and slave dummies are even further from statistical significance in this case, suggesting that they may have been affecting the results in conjunction with the outliers. In particular, the coefficients and standard errors for the campaign dummy in rows 1-8 in this case are: -0.46 (0.40), -0.47 (0.41), -0.31 (0.38), -0.31 (0.38), -0.42 (0.42), -0.43 (0.43) -0.52 (0.39), and -0.54 (0.45). For the slave dummy the numbers in rows 1-3 and 5-8 are: -1.07 (0.82), -0.99 (0.84), -1.03 (0.85), -1.01 (0.86), -0.91 (0.89), -1.07 (0.86), and -0.97 (0.89). In none of these cases are the two dummies jointly statistically significant at 36% or less. The pattern is similar in other columns except that these dummies do become significant in some specifications in Albouy's preferred sample of 28, but they again become insignificant in our column 8 when the campaign dummy is corrected and settler mortality is capped at 250.

[50] When the campaign dummy is corrected, it becomes even less significant both in the first and the second stages, making it even clearer that there is no support from the data for this dummy (or for the slave dummy) to be included in the regressions.

those in AJR (2000, 2001); the clustered AR confidence sets are now more tightly estimated intervals, always comfortably excluding zero 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 and the campaign and slave labor dummies included.

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 pointed out in AJR (2001) and discussed above. 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 as documented in 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), but the real issue is appropriate sample size and the miscoded dummy. We therefore do not provide the details and refer interested readers to AJR (2005) on this issue.

Albouy feels that the Mali observation is too high and he recodes this to 280 per 1,000 in his alternative data series.[51] The Mali datapoint we used, 2,940 per 1,000 per annum, reflected an epidemic; we were transparent about this in AJR (2000) and at all subsequent points. As explained above, capping the mortality data series to reduce the effect of very high rates is a useful exercise - as long as this is done in a systematic and even handed manner. Just capping Mali, but not others, is ad hoc, selective, and unsatisfactory.[52]

In addition, Albouy claims that we mistakenly thought countries such as Gabon and

[51] As noted in footnote 8, we find 280 per 1,000 in his online dataset (last checked September 19, 2011). In his text, he says this value should be 478.2 per 1,000 per annum.

[52] Albouy's recoding of Mali, by itself, hardly makes any difference in the full sample or even in his preferred sample of 28. For example, with the full sample and no covariates, the coefficient on settler mortality is -0.59 and the clustered standard error is 0.24. This is the same point estimate and just a slightly higher standard error compared with using our original Mali estimate.

Cameroon border Mali. But we never made any such statement. In Appendix B of AJR (2000), we wrote "Angola, Cameroon, Rwanda, and Uganda receive the [mortality] estimate from French Soudan..." The French Soudan was, during the colonial period, a large area that included Mali but also other parts of West Africa, much of central Africa, and some of eastern Africa well down into what is now the Congo.[53] We were quite explicit on this usage of the term, stating on p.33: "For example, Burkina Fasu, Central African Federation, Chad, French Congo, and Mauritania were part of French Soudan." We used this large area - also known as French West Africa and French Equatorial Africa - to infer estimates for countries that were in or neighboring this region. This was clearly explained again in AJR (2005), where we also provided additional evidence supporting the mortality rates we originally used in this area, and Appendix B to this paper recaps some of the details. Unfortunately, Albouy has chosen to ignore all of this.

In any case, all of this discussion is completely inconsequential. Once again as shown in detail in AJR (2005) - and as ignored by Albouy in his Comment - the modification to these rates and changing the rate for Mali in general makes no difference to the results, and we refer to reader to AJR (2005) for details.

4 Concluding Comments

The AJR (2001) results on the relationship between potential settler mortality and institutions are highly robust to a range of checks and variations. Firstly, limiting the effect of high mortality outliers has no impact on the main results in AJR (2001). Capping mortality rates at 250 (or 150 or 280 or 350) per 1000 per annum not only leaves our results unchanged but by reducing the effect of outliers, it increases their robustness. In fact, this procedure to limit the effect of outliers is by itself sufficient to make the results in AJR (2001), to a very large extent, immune to Albouy's two main critiques - that all the data from Latin America and much of the data from Africa, making up almost 60% of our sample, should be discarded; and that a "campaign" dummy coded by himself should be included in the first stage.

Secondly, Albouy's critiques are simply unfounded. His arguments that there is no reliable information on settler mortality for much of the world are at odds with the historical record.

[53] Much of this territory was contested with other colonial powers, including the British. We have reviewed colonial-era maps and and the precise markings and terminology vary for this region. This is why we were explicit about which countries were in the French Soudan.

When writing the first version of his Comment, Albouy (2004) apparently understood French Soudan to be a much smaller area, confined to West Africa. We clarified our usage of this term to him a private letter in 2004 and have followed up in our public responses (AJR 2005, 2006, 2008). Albouy still insists on another definition of French Soudan, which is limited to part of West Africa. But this is just semantics.

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. Using a variety of sources, most of which were referenced in our earlier work, one can obtain several alternative series, which all give results very similar results to those in AJR (2001).

Moreover, even if one were to follow Albouy and discard 60% of the AJR (2001) sample, his results turn out to be driven in large part by an outlier in his sample of 28: Gambia. There are good reasons to suspect that the very high institutions score that Gambia receives is not accurate. Also dropping this observation (thus reducing the sample to 27) leads to results quite similar to those in AJR (2001).

Albouy's second critique is similarly unfounded. His arguments and coding of the "campaign" dummy are at odds with the historical record and his coding procedure is implemented inconsistently. Even modest corrections to these inconsistencies 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 clearly 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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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]	[-∞,-9.76] U [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]	[-∞,-20.92] U [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	[-∞,-27.23] U [0.57,∞]	[0.48, 1.93]	[-∞,-9.26] U [0.59,∞]	[0.49, 2.22]	[-∞,-1.86] U [0.63,∞]	[-∞,-44.57] U [0.53,∞]	[-∞,-10.11] U [0.61,∞]	[0.52, 2.54]
AR confidence interval, clustered	[-∞,-4.72] U [0.44,∞]	[0.30, 1.53]	[-∞,-3.62] U [0.47,∞]	[0.32, 1.64]	[-∞,-1.57] U [0.58,∞]	[0.47, 4.79]	[-∞,-4.94] U [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]	[-∞,-4.29] U [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]	[-∞,-0.36] U [0.54,∞]	[-∞,-2.46] U [0.45,∞]	[0.37,8.59]	[0.33,1.21]
AR confidence interval, clustered	[-∞,-3.75] U [0.25,∞]	[0.23,0.89]	[-∞,-1.46] U [0.28,∞]	[0.25,1.06]	[-∞,-0.37] U [0.62,∞]	[-∞,-3.58] U [0.48,∞]	[-∞,-2.71] U [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 descent in 1975	-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	[-∞, -33.55] U [0.52, ∞]	[0.41, 1.98]	[-∞, -205.15] ∪ [0.51, ∞]	[0.43,2.83]	[0.71,2.87]	[0.61,2.03]
AR confidence set, clustered	[-∞, -10.47] U [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	[-∞, -4.30] U [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	[-∞, -5.70] U [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	[-∞, -0.86] U [0.37, ∞]	[0.27, 5.26]	[-∞, -3.11] U [0.28, ∞]	[0.24,2.96]	[0.56,5.99]	[0.52,1.80]
AR confidence set, clustered	[-∞, -0.39] U [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	[-∞, -2.35] U [0.33, ∞]	[0.24, 2.07]	[-∞, -2.45] U [0.22, ∞]	[0.16,3.38]	[0.40,1.17]	[0.36,0.94]
AR confidence set, clustered	[-∞, -1.46] U [0.33, ∞]	[0.25, 1.36]	[-∞, -5.04] U [-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	[-∞, ∞]	[-∞, -21.20] ∪ [-0.09, ∞]	[-∞, -61.31] U [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
Second 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]	[-∞, -4.58] ∪ [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]	[-∞, -4.21] ∪ [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]	[-∞, -0.56] U [0.27, ∞]	[-∞, -6.96] U [0.46, ∞]
AR confidence set, clustered	[-∞, -19.88] U [0.75, ∞]	[0.56, 3.07]	[0.77, 20.94]	[0.62, 2.50]	[0.74, 5.99]	[0.64, 2.10]	[-∞, -0.38] U [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]	[-∞, -1.09] U [0.40, ∞]	[0.41,2.11]
AR confidence set, clustered	[-∞, -9.54] U [0.51, ∞]	[0.37, 1.77]	[0.51, 8.77]	[0.41, 1.61]	[0.54, 3.68]	[0.45, 1.46]	[-∞, -0.93] U [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	[-∞, -2.71] U [0.56, ∞]	[0.44, 3.52]	[-∞, -5.72] U [0.58, ∞]	[0.47, 2.60]	[0.57, 32.57]	[0.47, 1.96]	[-∞, ∞]	[0.22,3.51]
AR confidence set, clustered	[-∞, -1.16] U [0.37, ∞]	[0.14, 2.20]	[-∞, -2.80] U [0.42, ∞]	[0.24, 1.81]	[-∞, -27.45] U [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	[-∞, -5.13] U [0.54, ∞]	[0.31, 2.88]	[0.55, 6.82]	[0.38, 1.69]	[0.54, 2.37]	[0.41, 1.30]	[-∞, -0.33] U [0.13, ∞]	[0.17,17.78]
AR confidence set, clustered	[-∞, -2.35] U [0.54, ∞]	[0.13, 1.95]	[-∞, -37.42] U [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]	[-∞,-2.42] ∪ [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)

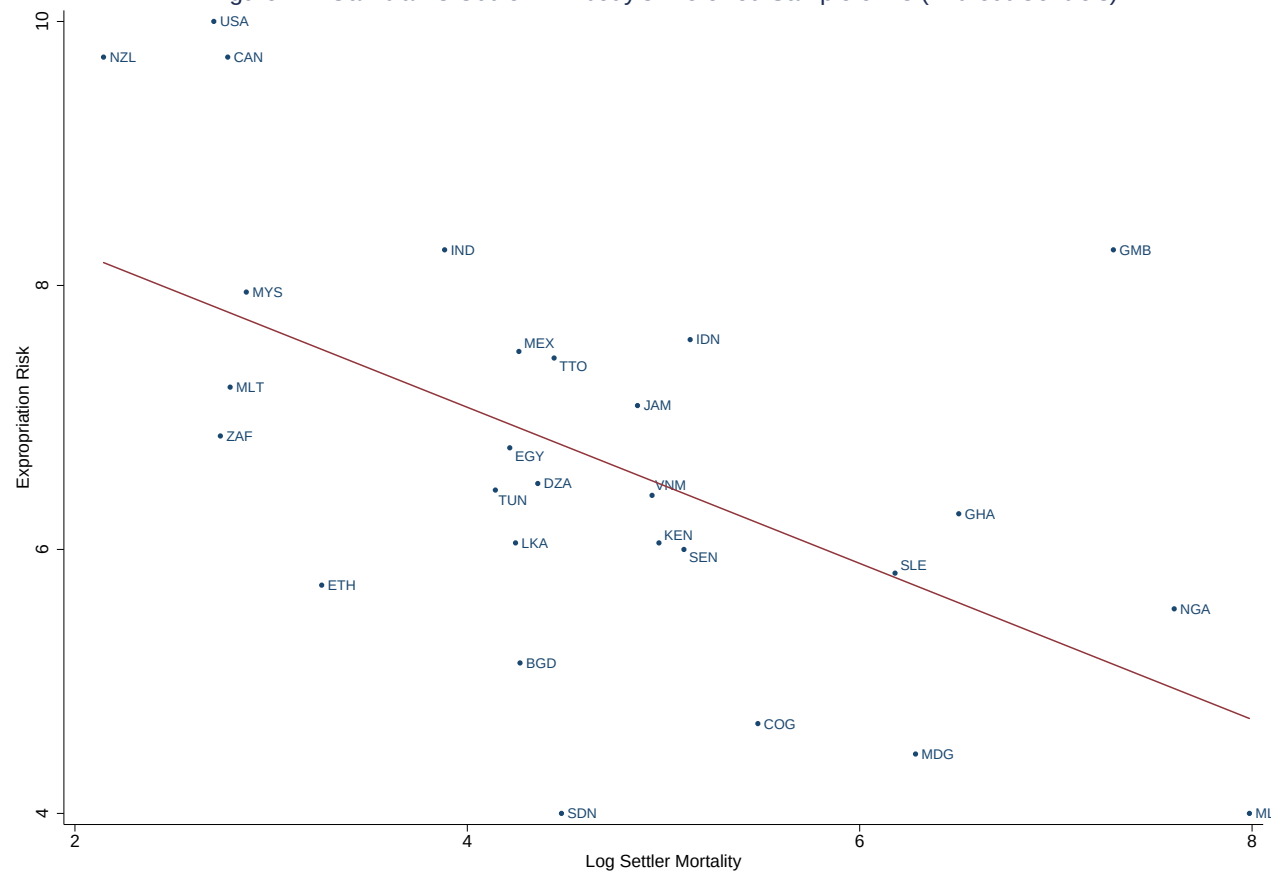

Figure 1B: Gambia As Outlier in Albouy's Preferred Sample of 28 (With Continent Dummies and Latitude)