September 11, 2006

Reply to the Revised (May 2006) version of David Albouy's "The Colonial Origins of Comparative Development: An Investigation of the Settler Mortality Data."

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Abstract

David Albouy's (2006) third comment on our "Colonial Origins of Comparative Development" (hereafter AJR, 2001) is not an improvement on his earlier work. The substantive disagreement between AJR (2001) and Albouy's current and previous comments revolve around Africa. Specifically, Albouy changes a number of African data points. His selective edits are highly implausible, but this is beside the point. As documented in our original 2000 working paper, in our 2001 published article, in our previous 2005 response to Albouy, and in this note, **our results regarding the positive effect of institutions on income per capita are just as strong without African data in the sample (and this is confirmed by Albouy's own preferred data series)**.

Our results are also robust when African data are capped at various reasonable levels, for example assuming that Africa had only slightly higher mortality than relatively unhealthy parts of the West or East Indies. Our results are weakened only when the data are altered so as to imply Africa before 1850 was a healthier place for potential European immigrants than was most of the world (including much of Europe!). These edits are at odds with all the available evidence both from contemporary sources and more modern assessments.

[1] MIT and NBER, MIT and NBER, and Harvard and NBER, respectively.

I. Overview

In Acemoglu, Johnson, and Robinson (AJR, 2001), we documented a negative relationship between measures of institutions in countries today (with a particular focus on the risk of expropriation) and the (logarithm of) mortality rates faced by Europeans if they tried to settle in the same place 150 or more years ago. Using "log settler mortality" as an instrument for modern institutions, we argued that institutions have a strong causal effect on GDP per capita today.

We have always stressed that the settler mortality data are not perfect and should be subject to serious robustness checks; in fact, a major part of our original working paper (AJR 2000) was devoted to such checks and some of this also appeared in the published version (AJR 2001). We continue to search for, write about, and welcome from others, any valuable new evidence that addresses the key issue – mortality rates that potential European settlers could reasonably have expected to face during the period when colonial strategies were determined (roughly 1500 to 1850).

Albouy (2004b) leveled a number of criticisms against AJR (2001). Our rebuttal, AJR (2005), responded to all his claims and documented that his critique was without foundation.[2] Albouy (2006) picks up where his previous comment left off and repeats essentially the same critique of our work without responding to the detailed point-by-point refutation in AJR (2005). As with Albouy (2004a and 2004b), Albouy (2006) does not provide any valuable new evidence on the questions at hand. Instead, Albouy (2006), continues to focus on (a) what he calls "eliminating inconsistencies" for some data points in Africa, and (b) a coding of whether the data come from what Albouy regards as "campaigns" (mostly for Africa). All of Albouy's (2006) data editing is based on claiming to understand better and represent more consistently information from two of our original sources: Curtin (1989) and (1998). This Africa-related data editing is the basis for almost all his econometric results, as reported in both the main tables and appendix tables in Albouy (2006).

In this brief note, we reiterate some of the main points already made in AJR (2005), particularly those ignored by Albouy. In the interest of brevity and to avoid

[2] A web link to AJR (2005) is included in our references below.

needless repetition, we refer the reader to AJR (2005) for many of the full details. We also briefly comment here on some new data edits introduced in Albouy (2006).

However, we first need to reemphasize one point. The essence of the disagreement between AJR's original (2001) series for settler mortality and various series constructed by Albouy is Africa. We noted explicitly in the working paper version of our paper, AJR (2000), and in the published version, AJR (2001), that there is some uncertainty about the upper bound on the African data.[3] For this reason, the working paper version, the published version, and our previous response to Albouy documented that our main results are robust, and in fact just as strong statistically, when we do not include any African data in the sample. Albouy does not recognize this point in his text, although, as we show below, it is confirmed by his own preferred data series.

Our results are also robust to capping all African mortality rates at some maximum (such as 250 deaths per 1,000 mean strength), so that African mortality rates are only slightly higher than those in relatively other unhealthy European colonies.[4]

In addition, as we emphasized in AJR (2005), Albouy's results in the past (2004b) and again now (2006) hinge on changing the African data so much as to imply that parts of Africa were quite healthy for Europeans. For example, according to Albouy's latest round of editing, Madagascar and Central Africa were healthier for Europeans than the West Indies, while Sudan and Egypt were healthier for Europeans than much of Europe! This runs counter to everything that is known about the health of Europeans as they traveled around the world before the advent of modern medicine (in the mid/late nineteenth century). If potential Europeans migrants were sure of one thing, it was that sub-Saharan Africa was the "White Man's Grave".[5]

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[3] Specifically, the uncertainty is about the highest estimates, which are very high (over 500 deaths per 1,000 per annum). This was a major motivation for our original use of logs. However, there is no uncertainty – in our reading of the sources – concerning the lower bound of mortality for Europeans in tropical Africa prior to 1850; we have found almost no instances when this was below 250 per 1,000 per annum for newly arriving settlers or soldiers or missionaries or anyone else. This motivated our alternative series, capping mortality at 250 for Africa, in AJR (2005); see also the next footnote.

[4] The specific rate of 250 per 1,000 is from Alexander Tulloch's seminal work on health in West Africa, as explained in AJR (2005), p.13. Albouy (2006) ignores this important information.

[5] They were also sure that Africa was much more unhealthy than other potential destinations. India and the West Indies, for example, were generally seen as places Europeans could survive

This note will be relatively short, since the above two paragraphs contain enough to invalidate Albouy's main thesis and claims (and because we have said all of this before). Nevertheless, since Albouy continues to insist that there are inconsistencies in our original data series, we (again) refute his detailed claims. Note that just because we discuss one of Albouy's points does not mean that point is of any consequence for his econometric results. In fact, Albouy repeatedly places great emphasis on details that do not affect those results, in a way that makes it impossible for the reader to discern which of his multiple selective edits actually drive the results he obtains. We disagree with him on almost all of these details and we would like to make sure the record stands corrected, but in any case these details do not matter.[6]

Section II reviews Albouy's econometric results and shows that they are all due to selective data editing. Section III points out several other inconsistencies, errors and exaggerations in Albouy's approach. Section IV concludes. A short appendix reviews the data on European disease mortality in Sudan.

II. Econometric Results

We start by documenting our main point, that the results in AJR (2001) are robust. In fact, as we showed in AJR (2001), these results are statistically just as strong without Africa. We then go through Albouy's (2006) econometric results in order, following the structure of his tables. There are many smaller points in the paper – typically asserted without explaining to the reader whether they matter for the conclusions – and we will address the more salient of these along the way in this section. Most of the rest we discuss in Section III and for a rebuttal of every detail see AJR (2005). But the heart of the matter lies with the econometric results presented in Albouy's tables.

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for a while, but were not attractive for permanent settlement. Africa was seen as a place to be avoided, apart from brief visits. See Curtin (1964) and the discussion in AJR (2005).

[6] For example, Albouy spends more than a page on the rates we use for the Indian subcontinent (see his note i on p. A3). All of these complaints are inconsequential. In addition, the text and Table 1 in Albouy (2006) devote considerable space to assigning new estimates to Mali, Niger, Angola, Burkina Faso, Cameroon, Gabon and Uganda. We show in Section II that these changes are also of no consequence.

Main Point

In Table 1, Albouy lists his proposed revisions to some of our data points. These changes are for Africa only.

Table 1 shows the original first stage AJR (2001) results in the first column; i.e., we regress a measure of institutions today (risk of expropriation) on log settler mortality. The rows report results with alternative sets of covariates or variations on the countries in the sample. For each specification, we include the standard errors and clustered standard errors in parentheses.

Note that the clustering here is very conservative (and straight from Albouy, 2006), as it ignores the fact AJR (2005) already introduced additional information that confirmed most of the mortality estimates in AJR (2001) from new and different sources. Still, we do not present alternative clustering here because this hardly matters for the overall conclusions.

Column 2 shows the first stages for the same specifications, but now excluding all African data. One point is immediately apparent: **the first-stage results are uniformly stronger without Africa**. For example, for the baseline specification the estimate in column 2 is -1.21 vs. –0.61 in column 1. Despite the reduction in the sample by 27 countries, the estimate in column 2 is only a little less precise than in column 1; the unclustered standard error increases from 0.13 to 0.22, while the clustered standard error increases from 0.17 to 0.18.

The remainder of column 2 shows the robustness of other results from our baseline specifications when we drop all the African data. In all specifications, we find coefficients that are larger than in column 1 and standard errors that are sometimes smaller and sometimes larger than in column 1.

In column 3, we repeat results from AJR (2005; specifically, Table 3A, column 5), in which we used new data, which suggested that capping African mortality rates at 250 per 1,000 would be a conservative robustness check. This is one of our many robustness checks, and we include it here to emphasize the range of plausible variation in our findings. In the first row, the coefficient is –0.70 and the clustered standard error is 0.19; this is very close to the results in the first row of column 1. In all rows, the estimated coefficient in column 3 lies between the estimates for columns 1 and 2, and the standard error (clustered or not) is quite similar to what we find in columns 1 and 2.

In Table 2, columns 1, 2, and 3 report the second stage estimates that correspond to the first stages in columns 1, 2, and 3 of Table 1. We report the Anderson-Rubin (AR) 95% confidence intervals, and the F-statistics from the first stage, both unclustered and clustered for each specification (see AJR, 2005, for more discussion of why it may make sense to look at these intervals). The lower bound of the AR confidence interval is almost always significantly different from zero (typically around 0.4, even for clustered results). The only indication of a weak instrument problem is in the last two rows of column 3, when we include percent of European descent or malaria as covariates (with Albouy's clustering).

The conclusion from this exercise should be clear: editing the African data in order to criticize the findings in AJR (2001) is not a particularly fruitful direction. The results are statistically at least as strong without Africa; they are in no way driven by African observations.

We now turn to a detailed discussion of Albouy's econometric results.

Albouy's Tables 1 and 2

Albouy proposes four sets of changes to our Africa data (see his Table 1 and the layout of his Table 2): (i) Sudan, (ii) Egypt and Madagascar, (iii) "Mali-based countries", and (iv) "laborer mortality" countries. His Table 2 reports results that include cumulative edits to our data, i.e., Panel B just changes Sudan, Panel C changes Sudan plus Egypt and Madagascar, Panel D includes the changes of Panel C plus changes to "Mali-based countries," and Panel E includes the changes of Panel D plus changes for "laborer mortality" countries. However, this presentation of the results masks the singular importance of his change for Sudan, which is responsible for almost all of the significant differences between Albouy (2006) and AJR (2001). We therefore devote some space to its consideration.

For Sudan, we used the earliest available peacetime number, in accordance with our coding rules (see AJR 2005). Albouy (p. A3) both disputes that this is what we did, and also rejects this rule, for reasons that remain unclear.[7] It is striking that, throughout his comment, Albouy points out and complains about instances when, due to data availability, for some countries we had to use data from a small-scale military expedition rather than from peacetime. In general, the tone and content of Albouy's comment agrees with our principle (expressed in AJR 2001 and reiterated in AJR 2005) that, where available, peacetime data are a preferable measure for the mortality rates faced by potential European settlers. But for Sudan, inexplicably, Albouy (2006) then prefers data from a substantial war, even though peacetime data were available.[8] He therefore modifies our data in a way that, according to his own argument, creates an inconsistency.[9]

This single inconsistent modification drives a large part of the results in his Table 2 and biases results heavily towards his position (that there is a weak instrument problem) in Tables 3 through 5. Columns 1-8 in Table 1 show this point in more detail. Since we strongly disagree with Albouy's Sudan edit, our Table 1 examines precisely what happens if we simply change this particular choice that Albouy has made. Note that we also have substantive problems with his other non-Sudan proposed changes (reviewed below), but still it is interesting to see how much of his case rests on the dubious edit for Sudan.

Column 4 reports results using Albouy's series. These match his results in Panel E of his Table 2 and confirm we are using the same data (downloaded from his website).

[7] On p. A3, Albouy (2006) writes, "In my revision the principle of taking the earliest peacetime rate is disregarded..." But this phrase, the larger passage and the entire context are not clear, at least to us.

[8] In Albouy (2004b) the peacetime or "barracks" data for Sudan is set as missing; in Albouy (2006) he has changed his mind and he uses what he previously regarded as "campaign" data in a series of peacetime estimates!

[9] The change is to 10.9 (deaths per 1,000 per annum), which would make Sudan one of the healthiest places in the world for Europeans in the 19th century; healthier, for example, than Canada (16.1 per 1,000) or Australia (14 per 1,000; see AJR 2005, p.33) or Western Europe (the mortality rate for the same regiments in Britain was 15.3 per 1,000, while in France it was 20.17 per 1,000; see Curtin 1989, p.7). In addition to being inconsistent, this change is implausible, yet the magnitude of his adjustment passes without comment in Albouy (2006). We previously pointed out the problem with his approach on p.21 of AJR (2005); Albouy (2006) does not reply to our rebuttal.

Column 5 reports results using a series in which we implement Albouy's changes for Egypt and Madagascar (only).[10] This makes only the smallest difference to the results compared with column 1 – in the first row, for example, the coefficient on log settler mortality falls in absolute value to –0.58 (from –0.61) and the clustered standard error rises from 0.17 to 0.18. A similar pattern is present in the other rows – the coefficient is smaller and the clustered standard error does not change. In any case, the results are essentially the same as in column 1. So the changes for Egypt and Madagascar themselves are not of any consequence. However, it is impossible for the reader to discern this point from Albouy's presentation.

Column 6 keeps the Egypt and Madagascar changes, and now adds the Mali-based changes.[11] In the text Albouy devotes considerable space to these changes and they also feature in Figure 1, but what he proposes again makes essentially no difference.[12] For example, in the first row, the point estimate is now –0.57 (from –0.58 with just the Egypt and Madagascar change) and the clustered standard error is 0.19. The t-statistic remains over 3. The Mali-based changes proposed by Albouy are also of no consequence; again, this point is not clear in his presentation of results.

Column 7 just makes the change to Central African data that Albouy proposes: Kenya and Tanzania are set as missing and a mortality rate of 100 is assigned to Congo and Zaire (this is highly problematic, but we will return to this in the next paragraph).

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[10] For Egypt, as for Sudan, Albouy prefers data from a major campaign, even though peacetime data are available. We pointed this out on p.21 of AJR (2005). Albouy wants us to use data from Nossi-Bé for Madagascar. In fact, Nossi-Bé is an island off the coast of Madagascar (a fact not mentioned by Albouy); it is small (population in 1902 was around 9,000) and was actually run as part of French Mayotte until 1896. Presumably in this case, for reasons not specified, he is willing to waive his "no neighbors" rule. We prefer to use data from Madagascar itself. Typically, small off-coast islands (and coastal forts) in Africa had mortality rates that were lower or much lower than the immediate hinterland. This is why the Europeans kept them as strategic and trading toeholds. The data series in column 4 are therefore the same as in the baseline sample in from AJR (2001) with these two changes proposed by Albouy.

[11] This column therefore uses the data series from AJR (2001) with nine changes. We would point out that Albouy's recoding of Mali itself is not consistent with either our coding rules or his own coding scheme. In addition, in AJR (2005) we presented further evidence that Albouy is ignoring epidemics, whereas these epidemics had a major impact on expected European mortality.

[12] We actually showed very similar results in AJR (2005). Among our robustness checks there, we capped African mortality data at 250 per 1,000 (p. 34 and Table 3A of AJR 2005); this does essentially the same thing as Albouy's "Mali-based changes". Albouy (2006) makes no mention of our results in this context.

These changes again make little difference to the main specifications – the estimate with no covariates is –0.58 (clustered s.e. of 0.17), and with latitude included it is –0.47 (clustered s.e. 0.21). However, the results with malaria are weakened in this case.

For Central African countries, Albouy thinks we are wrong to use the high end of African laborer mortality as a proxy for European mortality. We are sure he is wrong to use the low end – we have found no instance in malaria-prone areas before 1850 where local African mortality was consistently at the same level as European mortality; in fact, in almost all such cases, European mortality was at least twice African mortality (see AJR 2005, p.23, section 4.3.3). We have found no evidence that European mortality on expeditions in this region was any lower than in West Africa (see the discussion of Tuckey's Congo expedition in AJR, 2005, footnote 50); there is also nothing in the nineteenth century or modern medical geography literature that would suggest Central Africa was relatively healthy for Europeans – Albouy's estimates suggest that mortality for Europeans in Central Africa was less than ¼ the mortality for Europeans in West Africa, but there is no hint of any documentary evidence supporting this assertion (e.g., Hirsch, 1883-6, Lancaster, 1990, Kiple, 1993, Lyons, 1993).[13] Surely such a remarkable contrast would have made it into the historical record or drawn the attention of a more contemporary researcher?

As a robustness check, one might also want to assign missing values to all these Central African countries (in the same way that Albouy does for Kenya and Tanzania). This is what we do in column 8 and we report results with Congo and Zaire set as missing (as well as Kenya and Tanzania missing).[14] The results are again very similar to the

[13] Albouy's recoding of the central African data essentially implies that malaria was not an important problem there (or much less than in West Africa, from his own data series). One reality check on this point is to ask: would you (or any competent medical practitioner) recommend that a resident of North America visit Central Africa without appropriate anti-malaria medicine today? The US government's Centers for Disease Control and Prevention are clear on this issue: malaria will kill you in this region unless you take your medicine (post-1850 vintage and preferably a much more recently invented version). See the following web pages and associated links (last checked, August 20, 2006, 6am):
http://www.cdc.gov/travel/cafrica.htm#diseases
http://www.cdc.gov/travel/regionalmalaria/cafrica.htm
There is absolutely no evidence that malaria in this region developed only after 1850; if anything it is likely less a problem than it was historically, because at least some people now have access to appropriate medicine.

[14] This is the AJR data series with 4 changes.

baseline estimates from AJR (2001). Notably, the results are now quite strong when we control for malaria, demonstrating that the weak results with malaria in column 7 were an artifact of Albouy selectively coding some countries as missing but not others.[15]

In summary, a detailed look at the African revisions suggested by Albouy confirms our earlier conclusion that, even though the upper end of mortality for Europeans in Africa before 1850 is subject to some genuine uncertainty, one needs to make untenable assumptions in order to overturn the basic findings in AJR (2001) even when African data are included – essentially, one needs to assume that many parts of Africa were actually quite healthy for Europeans. In any case, as shown above, none of the basic results in AJR (2001) depend on African data.

Albouy's Table 3

In addition, Albouy (2006) repeats some of the claims made in his previous work (Albouy 2004a, 2004b). In those papers he argued that data we took from what he calls "campaigns" are not consistent with data from non-campaigns or what he calls "barracks" rates. Table 3 in Albouy (2006) reiterates this point, although slightly refashioned in the form of a campaign dummy that is included in the regressions.[16]

In our rebuttal to his previous comments, we explained that there are actually three types of sources for mortality data: peacetime, small-scale expeditions (marching, camping, with little or no fighting), and major campaigns or wars (with significant or even very high mortality from enemy action, many fatalities related to injuries of various kinds, etc). Albouy's "campaign" category conflates small-scale expeditions and major wars in a way that misleads the reader.

In our previous rebuttal, we strongly argued against Albouy's view that small-scale expeditions were substantially different from episodes of peace, see, AJR (2005), e.g., p.3 and at many places throughout Section 4. Regrettably, Albouy (2006) does not reply to these criticisms. Just to recap, the key points are the following:

[15] The most prominent results in Albouy (2004b) were also due to selectively recoding some countries while coding others as having missing data. This was most notable in his editing of West African mortality.

[16] Panels B and C conflate this issue with what Albouy calls the "slave labor" observations (for Central Africa); he uses the same unclear cumulative presentation as in his Table 1. We dealt with the Central African data in our discussion of Tables 1 and 2 above.

1. These small-scale expeditions were quite different from major wars; mostly these involved some marching and light fighting.[17]
2. The evidence does not support the idea that soldiers sat in barracks doing nothing in peacetime; peacetime and small-scale expedition activities were very similar.
3. Expeditions sometimes had higher mortality rates than peacetime and sometimes had lower mortality rates. Particularly in the later nineteenth century there was some randomness in the public health measures employed by militaries – sometimes they got it right and sometimes they got it wrong.[18]

In addition, in AJR (2005) we provided additional peacetime evidence on mortality rates, for example for Latin America. These are completely ignored by Albouy – he continues to treat those data as coming from campaigns only.

Albouy (2006) now recognizes that our data for Jamaica are not from a campaign (but there is no mention in his paper that we pointed out this error to him; see p. A10.) More importantly, however, he fails to acknowledge the other substantial additional data we provided on peacetime mortality in the Caribbean (p.32 of AJR 2005).[19] As a result, he does not mention that our estimates from Latin America can be derived entirely from peacetime (a point we made clear in AJR 2005, e.g., pp. 35-36).

He does not dispute that we now have peacetime data for Algeria (Albouy, 2006, p. A8) and that this confirms our original estimate.[20] But, for reasons we do not

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[17] For example, on p. A14, Albouy (2006) brings in mortality data from the U.S civil war. As we argued in AJR (2005, pp.26-27), the idea that the U.S. civil war (a major continent-wide conflict) is comparable with minor colonial expeditions is quite implausible – deaths from disease were 3 or 4 orders of magnitude larger in the U.S. civil war compared with the expeditions in our data.

[18] Before the germ theory of disease was fully established and before the importance of the mosquito in disease transmission was understood, military doctors were employing various heuristic rules, i.e., they were largely guessing and their luck varied.

[19] Albouy wants to make something out of the existence of relatively healthy highlands in Jamaica, Haiti and the Dominican Republic (p. A10), without acknowledging that we pointed out the difference in mortality rates, for example within Jamaica. Given that medical authorities (and others) did not understand the potential health advantages of highlands until quite late in the 19th century (see Geggus 1979, Kiple and Ornelas, 1996, Harrison, 1999), the differential mortality was immaterial – where Europeans chose to live (or tried to live), mortality rates were high.

[20] The rate for settlers is 70 per 1,000; the original AJR rate was 78.2 per 1,000; in logs this is 1.85 vs. 1.89. Albouy's implied criticism of our new estimate is that it is "mentioned in passing" by Tulloch; however, Tulloch was the leading authority on these issues and if he made a point of confirming a particular number, this should be taken seriously. Tulloch and his estimates are cited approvingly by anyone writing about health in the early nineteenth century; e.g., see Nightingale, 1864, Aitken, 1866, volume I, chapter III, and Bewell, 1999.

understand, he persists in coding this observation, and those based on it, as being from a campaign.[21]

He claims that much of our West African data should be regarded as "campaign," even though by his own admission the data are from a period when the troops spent much of their time not fighting (p. A4) – i.e., these were small-scale expeditions.

Column 9 of Table 1 corrects his coding of campaigns but only for Latin America.[22] This minimal correction, just for Latin America, by itself removes almost all the weakening of our results. If we further correct his campaign coding for Algeria and the rest of Africa, there is no weakening.

Albouy's Table 4

Albouy's (2006) Table 4 claims to report results using series that drop data from countries for which we do not have direct observations on mortality (i.e., for which we use information on neighbors). However, this is done in an inconsistent and incomplete manner. For example, Malaysia is kept but not Singapore. This is inconsistent given the evidence that Albouy himself endorses – on page A10, Albouy reports a mortality rate of 20.0 for "a much larger group of soldiers encompassing Penang, Malacca, and Singapore."[23]

Australia is dropped despite the fact that we documented separate data for this country in our response (AJR 2005).[24] Our evidence for the Bahamas, Belize and

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[21] Note also that Curtin (1996, p.100) reports a military mortality estimate for Algeria as 81.54 in 1837-46. He does not regard this an unusual number, and sees the decline as due to (and in line with) improvements in tropical military medical practice more generally.

[22] The fact that he accepts our West Indies data as peacetime implies that Albouy should now recognize our Latin American estimates (in AJR 2005) are peacetime-based, not derived from campaigns, yet his campaign dummy assumes the opposite. We correct his campaign dummy so as to remove this oversight.

[23] Albouy offers only the following odd explanation for dropping Singapore while keeping Malaysia: "However, it should be kept in mind that the Portuguese and Dutch had been in this area for over 300 years." Of course, the Portuguese and Dutch had operated throughout Asia since 1500, with bases almost everywhere (the same is true of West Africa and many other coastal parts of Africa). But Singapore was not settled (by any Europeans) until the British took control in 1819.

[24] On p. A10, Albouy refers to the new data for Australia, but makes it sound like this is for New Zealand. He also says that 14.1 is "considerably different (in logs) from New Zealand's rate of 8.55"; in fact, the difference is 1.15 vs. 0.93 in logs. This difference is hard to pick out on a scatterplot of our data.

Honduras is similarly ignored (see pp.32-33 of AJR 2005). Our additional evidence for Africa and Latin America (from AJR 2005) is also completely neglected. Albouy lists our response (AJR 2005) in his references, but it is hard to see how he has used the pertinent information.

Albouy (2006) also (again) fails to cite our NBER paper (AJR 2000) as having been the first to report results using just a data series without assignment to neighbors.[25]

Albouy's Table 5

Albouy's Table 5 shows the 2SLS results from regressing log GDP per capita in 1995 on the risk of expropriation, instrumented by log settler mortality. We show the most important results in our Table 2, together with Anderson-Rubin confidence intervals and first stage F-statistics, both unclustered and clustered.

Columns 1, 2 and 3 show results with alternative versions of our series. These results, which were discussed above, show our findings are quite robust and not at all weakened by dropping the African data.

Column 4 shows results with Albouy's preferred series. There is no weak instrument problem – the Anderson-Rubin confidence in interval is not disjoint (and is essentially what we find with our series in columns 1, 2, and 3) when (a) there are no covariates, and (b) when the African data is dropped from the sample. Point (b) is important, particularly as Albouy does not mention it. **Even using Albouy's own series, there is no weak instrument problem without the African data.**

Note also that with Albouy's preferred series, there is only one specification in Table 2 where the Anderson-Rubin interval includes zero – this is the last row, with malaria. In all other cases, institutions either have a significant positive effect on income per capita (quite close in magnitude, looking at the lower bound of the positive interval, to what we find with the AJR series in column 1) or they have a large negative effect. Large negative effects from institutions to income per capita are not plausible – there is no serious view arguing that stronger private property rights or better quality institutions have a large negative effect on productivity, reduce capital investment, and make people

[25] This is the third time we have pointed out this particular failure to appropriately cite established findings.

less inclined to invest in their own skills. It is therefore appropriate to ignore the negative portion of the Anderson-Rubin confidence intervals.

Column 5 shows results with a slightly corrected version of Albouy's series. We correct the most major inconsistencies for Sudan, Egypt, and Madagascar. To illustrate the importance of these selective edits, we leave his "Mali-based" changes intact and also set all four controversial Central Africa data points to missing. With this very minimal correction (much less than we would recommend for a sensible alternative series of mortality estimates), what happens to Albouy's results?

Now there are fewer instances of disjoint Anderson-Rubin intervals. The unclustered intervals, in fact, are never disjoint. The unclustered or the clustered intervals never contain a zero effect of institutions on GDP per capita. Moreover, the upper bound (in absolute value) of the negative interval, when this exists for the clustered estimates, becomes much larger. For example, with continent dummies, if institutions do not have a positive effect on income per capita, the Anderson-Rubin intervals indicate that institutions must have an enormous negative effect on income per capita – actually an order of magnitude larger negative effect than we (and others) have found in terms of positive effect. This is simply implausible and again strengthens the view that one should ignore the negative portion of the Anderson-Rubin confidence intervals here. With these negative portions ignored, the confidence include only relatively large effects for institutions on long run income per capita as claimed in AJR (2001) and (2005).

<u>Appendix Table A2</u>

Albouy has four alternative series in this table. The first is a revision of the data for USA and Canada, although it seems from his own words that he does not take this seriously (p. A15). He is right not to take it seriously, because there is overwhelming evidence that much of North America was healthy for Europeans, and this is part of what made this region such a draw for European immigrants. See, for example, the data reviewed in Dobson (1989). Albouy's attempt to depict North America as unhealthy for Europeans does not stand serious scrutiny (see AJR 2005 for more detail).

The second series is called "eliminating inconsistencies and with data from AJR 2005." But the "eliminating inconsistencies" is just the problematic changes in his

Tables 1 and 2, which we already refuted above. Therefore, we fail to see the contribution of this table.

The third series drops countries in which the data are benchmarked using the mortality rate of bishops. He concedes that this does not make much difference to our results (p. A15). In addition, in AJR (2005) we showed that our benchmarking results are much more robust than Albouy claims. He fails to reply to our rebuttal.

The fourth series uses bishop mortality data directly. In other words, Albouy, as in his previous comment, uses the death rates of middle aged or elderly men alongside our usual data, which cover predominantly young men (in the military). AJR (2005) explained in detail why this was not valid. Albouy does not offer a defense for this practice, so we presume that he now agrees with us. But in that case, what is a point of this fourth series (now relegated to an appendix)?

<u>Appendix Table A3</u>

This uses alternative measures for institutions, alongside his series that "eliminates inconsistencies" (i.e., from his Tables 1 and 2). As we showed above, the changes in Tables 1 and 2 actually introduce inconsistencies. Using alternative measures for institutions does not change this fact.

III. Other Significant Issues

A Major Misconception

Albouy argues that just because mortality rates fell over time, we should necessarily use the later data (e.g., in his comments from p. A8 on Algeria). Taking this to an amazing extreme, on p. A2 he provides a list of countries for which he says there was a barracks rate, which he claims we ignore. He fails to mention that most of these data are from Table 1.2 in Curtin (1989), which is "Mortality of European Troops Overseas, *1909-1913*," (emphasis added).[26] This is either a mistake or must have some

[26] He cites as "source of barrack rates" Curtin (1989, p.9 or p.10) for Cameroon, Ghana, Morocco, Tunisia, and Vietnam; these data are all 1909-13. He also cites Curtin (1998, p.9) for Egypt, but these seems to be a typo as that page discusses only the disease background in West Africa; presumably the reference is to Curtin (1989, p.9), where the estimate is 3.97 per 1,000 during 1909-13. He also cites Curtin (1989, p.9) for Haiti, but we cannot find this number on this

other explanation. These data come from experiences after the confirmation of the germ theory of disease, after the discovery of the mosquito as the vector for malaria, and after the breakthrough against yellow fever (all occurring at the end of the 19th century). It is impossible to understand why anyone would prefer them as measures of early (pre-1850) European settler mortality.

The rapid advance in tropical medicine starting in the late 19th century is precisely the reason why we argued in AJR (2001 and 2005) that early data on mortality should be preferred.

Misrepresenting Curtin

While Albouy purports to more accurately represent the data in Curtin (1989 and 1998), in fact he distorts the historical picture painted by those books. Specifically, a major point of *Disease and Empire* is that, during the second half of the nineteenth century, Europeans became much better at managing campaigns in tropical areas so as to lower mortality from disease. The British and the French became more careful about what time of year they would campaign (avoiding malaria), they brought along clean water purified on ships (reducing typhoid), and they used military discipline to enforce prophylaxis (e.g., requiring soldiers to take quinine) and hygiene. As a result, there were instances when expeditions (e.g., against the Ashanti in West Africa and the strike against Magdala in Ethiopia) when post-1850 expeditions had very low mortality from disease – lower than the mortality experienced by troops permanently stationed in the same place before or after the campaign. But even in these low mortality instances, the Europeans understood that they needed to get out – and quickly – to keep mortality low.

Most of the measures used to reduce mortality on campaigns were not available to potential European settlers. In particular, "hit-and-run" is exactly what settlers cannot do – they need to stay. Examining mortality from one or two campaigns late in the nineteenth century, after European militaries became better at managing their campaign mortality, is clearly quite uninformative regarding the mortality for Europeans prior to 1850. Where available, early peacetime data should be preferred. Early small-scale

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page (or anywhere near). Albouy's preferred Indonesia data is apparently for the Dutch East Indies, 1859-1914, and for Pakistan it is British India over the same period (both are from Curtin, 1989, p.82).

expeditions, before the Europeans learned to avoid unhealthy seasons, are also informative about year-round mortality (in fact, the unhealthy Image of Africa was, according to Curtin 1964, based on exactly this kind of experience).

Smaller Issues

Here is a partial list of other, more minor issues:

1. Albouy states: New Zealand was "first settled in the late eighteenth and early nineteenth centuries" (p.7). It was not. European settlements began in the 1840s.
2. Albouy claims (p. A5) that we misread Curtin's data on Nigeria and wrongly annualize the death rate from the 1841 Niger expedition. In fact, Curtin (1996, p.101) confirms that the annualized equivalent rate "would have been more than 2,000 per thousand annual strength"(which is what we used).[27]
3. Albouy asserts that mortality from typhoid is due solely to dirty water, from campaign conditions, and therefore not relevant for settlers (pp. A8-9). While it is true that typhoid is generally spread through contaminated water, until the invention of antibiotics it was a serious health problem in many countries during peacetime (see, for example, Acemoglu and Johnson 2006).
4. Albouy suggests that continent dummies make sense because they may pick up the time of colonization. But in fact European contact with most of the rest of the world occurred in a fairly short period, 1500-1550. Many parts of the world were already profoundly affected by how the Europeans decided to proceed (i.e., their colonization strategy) by 1600. Africa, for example, was deeply affected by the Atlantic slave trade long before formal annexation. Of course, there were later decisions and some impacts took time to unfold, but in any case, the timing of formal colonization is surely endogenous and likely much less relevant than the initial date of European influence.
5. Albouy persists in arguing that epidemics were not representative of early European mortality and should therefore be ignored for data purposes. In fact, the available evidence overwhelmingly indicates that epidemics were key in defining to Europeans what kind of mortality they should expect – perceptions of health

[27] The death rate was 350 per thousand over two months.

risks in the West Indies, West Africa, and India, for example, were very much shaped by epidemics (e.g., Curtin 1964, Geggus 1979 and 1983, Kiple and Ornelas 1996, Bewell, 1999, and Harrison 1999).[28]

6. On p. 15, Albouy says "The Appendix Table A2 contains additional sensitivity checks, showing that the first stage estimate without Africa is not always significant." In fact, that table shows the "without Africa" first stage relationship between log settler mortality and institutions (column 9) is highly significant (panels B, C, and D), even in the face of Albouy's problematic "eliminating inconsistencies" etc. The only specification in which the first stage is not significant is Panel A, in which the US and Canada are assigned mortality rates of 227 and 283 per 1,000 respectively (higher than African mortality!). These estimates are selectively chosen from a couple of famine episodes in which settlers typically did not build proper shelter for the (unexpectedly severe) North American winter. Even Albouy (2006) does not claim these US and Canada numbers as a serious revision – see the second sentence in his section A.III on p. A15.

IV. Conclusion

The main results in AJR (2001) are highly robust to excluding all African data. These results are robust even to a range of questionable changes that Albouy himself makes. Albouy (2006) obtains results that are different from those in AJR (2001) largely by making a very selective edit to the estimate for Sudan. His change to the mortality estimate for Sudan is impossible to defend.

In addition to selecting a Sudanese estimate in a way that is inconsistent, Albouy does not pause to reflect that according to his preferred number, Sudan was one of the healthiest places in the world for Europeans in the 19th century (and healthier than Britain was for the British)! If Albouy is even close to being right, why did the leading British medical authorities view it as a highly unhealthy place that should be evacuated "in the face of heat and disease" (see Appendix I below)?

[28] Nightingale (1864) is quite scathing of people who tried to refute Tulloch's data on India by claiming it was not representative due to epidemics or other reasons. She clearly regarded these data as highly informative on the mortality of potential European settlers.

Appendix I: The Sudan

As we explained in Section II above, the precise mortality estimate for Sudan is of interest. The main issue is which of three mortality numbers we should use.

Albouy (p. 3) states, "One set of inconsistencies involves departures from their rule of always taking the first available rate. An example involves the Sudan: the first, second and third mortality rates Curtin (1998) reports are zero (p. 173), 10.9 (p. 169) and 88.2 (p. 173)."[29] Albouy says we should use the first or the second because they were slightly earlier. We used the third because our actual coding rule was to take the first peacetime number where available (see AJR, 2005, p.7), and we understood from Curtin that the first and second numbers were from campaigns with serious fighting.

At least to us, it remains self-evident from Curtin's (1998) discussion on p. 173 that the first mortality estimate is not a peacetime number, since he says "The expeditionary force fought two sharp actions" and at the end of the same paragraph he reports the rate of casualties "from enemy action." This is confirmed in the official history of the campaigns written by Sir H.E. Colville (1889, Part I, pp. 21-25).

What about the second number? After further checking, we have confirmed that this mortality estimate also came from a campaign with three rather large engagements with the enemy, which were regional tribes associated with the Mahdi and led by Uthman Diqna.

This campaign is discussed in detail in Colville (1889, Part II, Chapter XIII).[30] After arriving in Suakin in March, Sir Gerald Graham marched his forces into the hinterland of Suakin and there were three quite major battles at Hashin (Colville, pp. 197-200), Tofrik (pp. 204-207) and Tamai. All had casualties; for instance Colville records that 55 British soldiers were killed at Tofrik. So these data are definitely from a campaign with a great deal of fighting. Since there was a peacetime number, we were therefore correct in not using the campaign number (i.e., this was consistent with our coding rule).

[29] Suakin, a port on the desert littoral of the Red Sea, was quite close to Egypt. This has a particularly good disease environment (little malaria), which was not typical of Sudan, and the troops could be provided with purified water from ships.
[30] This is the March to May campaign mentioned in Curtin (1998, p. 173).

Was the third mortality rate really from peacetime? In May of 1885 the main force under Graham returned to Egypt and this led to a reduction of the size of the force at Suakin (as discussed by Curtin). The British then maintained a garrison at Suakin. Colville (1889) does not present a detailed account of what happened to this garrison after Graham departed, but the available sources confirm this was a time of peace (i.e., no significant fighting).

For example, after Graham's campaigns, Colville notes (p. 217) "within a few days a body of the tribes made an offer to submit to the British flag ... the political question of the eastern Soudan may be said to have been solved for the time being" and he gives the impression that the threats previously posed to Suakin faded away.

The distinguished British historian of Sudan, P.M. Holt, devotes pages 186-192 of his 1970 book to the Suakin area in the period between the death of Gordon in Khartoum (1885) and the British takeover of Sudan after the battle of Omdurman in 1898. He notes about Suakin (p. 187), "little of importance happened there in 1886 and 1887" and he reports that during this period the British garrison did not mount expeditions into the hinterland. Thus the mortality rate we used was from a garrison that was not engaged in campaigning against enemies or in any armed conflict that ever entered the official record (although the soldiers were likely not confined to barracks).

Just as important, perhaps, what did the British think about health conditions in Sudan? Curtin (1998, p.174) quotes a compelling editorial from the Lancet "recommending evacuation in the face of heat and disease." And what did the British do in the face of these data? They withdrew.

If Albouy is right, the British and other Europeans should have flocked to Sudan – with a mortality of 10 per 1,000 it should have been regarded as a great place for settlers (one of the best in the world). At the very least, it should have been regarded as a potential health spa. Instead, in the mid-1880s, European medical experts and public opinion regarded Sudan as unhealthy.[31]

[31] Curtin, 1998, p.199, suggests that the official death rate for Sudan may be an underestimate. At least during the Omdurman campaign of 1898, a decade later, soldiers who went down with typhoid fever were quickly evacuated to Egypt and Britain. Only some deaths of the evacuees in Egypt were counted in Upper Nile mortality; if soldiers made it out of Egypt, their deaths (even if from the effects of this disease) were not counted in the official mortality statistics for Sudan.

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Table 1
First Stage Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Alternative series for settler mortality								
	Original AJR series	Original AJR series, without Africa	AJR series with robustness data from Tulloch and new alternative estimates Latin America, capped at 250	Albouy's new series	AJR series with just Egypt and Madagascar changes	AJR series, with just Egypt, Madagascar, and Mali-based changes	AJR series, with just Central Africa changes	AJR series, with Central Africa data set as missing	AJR series with slave dummy and minimally corrected campaign dummy
	Dependent variable is protection against risk of expropriation								
No covariates	-0.61	-1.21	-0.70	-0.46	-0.58	-0.57	-0.58	-0.58	-0.52
(standard error)	(0.13)	(0.22)	(0.15)	(0.14)	(0.13)	(0.14)	(0.13)	(0.13)	(0.15)
(clustered standard error)	(0.17)	(0.18)	(0.19)	(0.20)	(0.18)	(0.19)	(0.18)	(0.17)	(0.18)
Number of clusters	36	19	39	33	36	34	33	32	36
Number of observations	64	37	64	62	64	64	62	60	64
With latitude	-0.52	-1.14	-0.59	-0.31	-0.47	-0.46	-0.47	-0.51	-0.46
(standard error)	(0.14)	(0.24)	(0.17)	(0.16)	(0.15)	(0.16)	(0.14)	(0.14)	(0.16)
(clustered standard error)	(0.19)	(0.19)	(0.21)	(0.24)	(0.20)	(0.22)	(0.21)	(0.19)	(0.20)
Number of clusters	36	19	39	33	36	34	33	32	36
Number of observations	64	37	64	62	64	64	62	60	64
Without neo-Europes	-0.40	-0.83	-0.46	-0.20	-0.36	-0.33	-0.35	-0.36	-0.36
(standard error)	(0.13)	(0.29)	(0.15)	(0.14)	(0.14)	(0.14)	(0.14)	(0.13)	(0.14)
(clustered standard error)	(0.17)	(0.27)	(0.15)	(0.19)	(0.17)	(0.18)	(0.18)	(0.18)	(0.17)
Number of clusters	33	16	35	30	33	31	30	29	33
Number of observations	60	33	60	58	60	60	58	56	60
Without Africa	-1.21	n.a.	-0.73	-1.21	-1.21	-1.21	-1.21	-1.21	-1.18
(standard error)	(0.22)		(0.23)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.26)
(clustered standard error)	(0.18)		(0.30)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.22)
Number of clusters	19		22	19	19	19	19	19	19
Number of observations	37		37	37	37	37	37	37	37
With continent dummies	-0.44	-1.30	-0.54	-0.18	-0.38	-0.34	-0.37	-0.44	-0.43
(standard error)	(0.17)	(0.29)	(0.19)	(0.17)	(0.17)	(0.18)	(0.18)	(0.17)	(0.18)
(clustered standard error)	(0.20)	(0.31)	(0.19)	(0.22)	(0.20)	(0.21)	(0.21)	(0.20)	(0.21)
Number of clusters	36	19	39	33	36	34	33	32	36
Number of observations	64	37	64	62	64	64	62	60	64
With percent of European descent in 1975	-0.42	-1.05	-0.44	-0.22	-0.38	-0.36	-0.37	-0.40	-0.34
(standard error)	(0.14)	(0.25)	(0.20)	(0.15)	(0.14)	(0.15)	(0.15)	(0.14)	(0.16)
(clustered standard error)	(0.19)	(0.19)	(0.24)	(0.21)	(0.19)	(0.20)	(0.20)	(0.19)	(0.19)
Number of clusters	36	19	39	33	36	34	33	32	36
Number of observations	64	37	64	62	64	64	62	60	64
With malaria	-0.52	-1.21	-0.60	-0.22	-0.45	-0.43	-0.44	-0.55	-0.46
(standard error)	(0.18)	(0.22)	(0.21)	(0.18)	(0.18)	(0.19)	(0.18)	(0.18)	(0.20)
(clustered standard error)	(0.22)	(0.18)	(0.24)	(0.27)	(0.22)	(0.26)	(0.24)	(0.22)	(0.22)
Number of clusters	35	18	37	32	35	33	32	31	35
Number of observations	62	35	62	60	62	62	60	58	62

OLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Original settler mortality series and covariates are from AJR (2001); Albouy series is from Albouy (2006). Construction of alternative series is explained in the text and summarized here in the column heading.

Table 2
Second Stage Regressions

	(1)	(2)	(3)	(4)	(5)
	Alternative series for settler mortality				
	Original AJR series	Original AJR series, without Africa	AJR series with robustness data from Tulloch and new alternative estimates Latin America, capped at 250	Albouy series	Albouy series, minimally corrected
	Dependent variable is log GDP per capita in 1995				
No covariates	0.93	0.61	0.89	1.09	0.96
AR confidence interval	[0.69,1.40]	[0.41,0.87]	[0.64,1.35]	[0.75,2.24]	[0.70,1.51]
AR confidence interval, clustered	[0.66,1.84]	[0.43,0.89]	[0.63,1.59]	[0.69,8.00]	[0.67,2.34]
F-stat, first stage	23.34	23.34	22.17	10.30	18.95
F-stat, first stage, clustered	12.45	45.98	14.12	5.12	9.83
With latitude	0.96	0.60	0.90	1.43	1.04
AR confidence interval	[0.65,1.78]	[0.37,0.94]	[0.58,1.77]	[0.77,137.3]	[0.69,2.15]
AR confidence interval, clustered	[0.62,2.90]	[0.41,0.88]	[0.58,2.10]	[-∞,-1.23]U[0.69,∞]	[0.64,4.89]
F-stat, first stage	13.48	13.48	12.00	3.96	10.67
F-stat, first stage, clustered	7.30	37.89	7.88	1.68	5.78
Without neo-Europes	1.24	0.77	1.14	1.98	1.35
AR confidence interval	[0.78,3.09]	[0.36,1.94]	[0.70,2.91]	[-∞,-3.31]U[0.97,∞]	[0.83,4.85]
AR confidence interval, clustered	[0.79,7.39]	[0.36,2.25]	[0.70,2.93]	[-∞,-1.30]U[0.83,∞]	[-∞,-12.70]U[0.77,∞]
F-stat, first stage	23.34	23.34	8.92	10.30	18.95
F-stat, first stage, clustered	5.54	9.47	8.98	1.11	3.55
Without Africa	0.61	n.a.	0.66	0.61	0.61
AR confidence interval	[0.41,0.87]		[0.37,1.29]	[0.41,0.87]	[0.41,0.87]
AR confidence interval, clustered	[0.43,0.89]		[0.35,2.22]	[0.43,0.89]	[0.43,0.89]
F-stat, first stage	23.34		10.43	10.30	18.95
F-stat, first stage, clustered	45.98		5.83	43.98	45.98
With continent dummies	0.97	0.72	0.68	1.88	1.17
AR confidence interval	[0.59,3.21]	[0.52,1.10]	[0.35,1.62]	[-∞,-1.15]U[0.82,∞]	[0.69,17.03]
AR confidence interval, clustered	[0.49,10.08]	[0.47,1.32]	[0.16,1.32]	[-∞,-0.47]U[0.67,∞]	[-∞,-8.13]U[0.53,∞]
F-stat, first stage	6.49	11.54	8.06	1.09	4.31
F-stat, first stage, clustered	4.68	17.04	8.25	0.65	3.59
With percent of European descent in 1975	0.92	0.50	0.77	1.38	1.01
AR confidence interval	[0.55,2.31]	[0.27,0.84]	[0.31,7.82]	[-∞,-2.65]U[0.67,∞]	[0.60,3.61]
AR confidence interval, clustered	[0.51,6.91]	[0.18,0.85]	(-∞,∞)	[-∞,-0.55]U[0.58,∞]	[-∞,-7.33]U[0.52,∞]
F-stat, first stage	8.67	8.67	4.63	2.20	6.39
F-stat, first stage, clustered	4.92	30.92	3.28	1.11	3.55
With malaria	0.62	0.55	0.53	1.08	0.76
AR confidence interval	[0.32,1.40]	[0.37,0.82]	[0.20,1.28]	[-∞,-0.49]U[0.41,∞]	[0.42,2.85]
AR confidence interval, clustered	[0.27,2.24]	[0.41,0.78]	[-0.06,1.10]	[-∞,∞]	[0.16,29.48]
F-stat, first stage	8.64	8.64	7.95	1.39	5.82
F-stat, first stage, clustered	5.63	45.65	6.04	0.63	4.23

2SLS regressions, one observation per country. Coefficients and standard errors for covariates, where included, are not reported to save space. Original settler mortality series and covariates are from AJR (2001); Albouy series is from Albouy (2006). The minimally corrected Albouy series corrects his inconsistencies for Sudan, Egypt and Madagascar; the four countries in what Albouy calls "Central Africa" are set equal to missing; his Mali-based changes are accepted. More detaills for construction of alternative series are provided in the text and summarized here in the column heading; corresponding first stages are in Table 1. AR confidence intervals are Anderson-Rubin confidence intervals.