Reply to The Revised (2008) 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 is now on the fourth distinct version of his "comments" on Acemoglu, Johnson, and Robinson (2001). Like the previous three efforts, Albouy (2008) is based on a misunderstanding of the underlying data and what appears to be a recoding designed with a particular purpose in mind - to undermine our results. We assess his claims here and find all the substantive concerns to be misplaced. Even though we disagree that one can draw a systematic and meaningful distinction between "campaign" and "barrack" mortality rates, we also show that a minimal correction to Albouy's "campaign dummy" implies that using this variable has no effect on our results.

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*This is the September 2009 version of a note originally written in October 2008.

1 Introduction

David Albouy (2008)'s latest comment on Acemoglu, Johnson, and Robinson (2001) is not a significant improvement over Albouy (2004) and (2006).[1] While this comment differs from previous versions in crucial respects, the conclusions remain the same. Albouy again claims: "However 36 of the 64 countries in their sample are assigned mortality rates from other countries, typically based on mistaken or conflicting evidence. Also, incomparable mortality rates from populations of laborers, bishops, and soldiers – often on campaign – are combined in a manner favoring their hypothesis. When these data issues are controlled for, the relationship between mortality and expropriation risk lacks robustness, and instrumental-variable estimates become unreliable, often with infinite confidence intervals" (from his abstract).

How does he reach this conclusion? Perhaps in his strangest attempt so far to dispute our findings, he drops more than half of the original sample with various - unconvincing and unreasonable - arguments and ends up with just 28 data points. Using this much smaller sample and specifications with additional covariates, Albouy concludes that our results are not robust. However, throwing out more than half of our sample has no justification whatsoever, and the arguments that Albouy makes to support this action are simply false.

He also attempts to bolster his case using a "campaign" dummy. We have argued repeatedly that Albouy's claimed distinction between campaigns and barracks is at odds with the historical record (e.g., AJR 2005, 2006). Moreover, given the available evidence - even from his own sources - Albouy appears to code the campaign dummy so as to overturn our results. His key claims about what Curtin says in this regard are incorrect, and in some important instances Albouy chooses to ignore explicit statements by Curtin (our original source, which he still relies on extensively).

Finally, he also recodes the data for Mali, again without any compelling justification and makes a number of other "corrections". But these are inconsequential.

[1] Our replies to those earlier comments are Acemoglu, Johnson, and Robinson (2005) and (2006). We also rebutted informally and privately an even earlier comment by Albouy. The content has varied across these four generations of Albouy comments - and he has dropped a lot of points along the way. But he always comes to the same conclusion.

2 Main Points

David Albouy's strategy is: (1) to arbitrarily drop data, including all of Latin America, an approach for which there is no justification; and (2) to insist on the distinction between barracks and campaign data, which is not supported by the historical evidence. Moreover, in direct contradiction to what Albouy states explicitly, such a systematic distinction is not in Curtin (1989 and 1998). In some cases, Albouy's distinction between barracks and campaigns is not only not supported by the historical evidence, but seems to be coded in contradiction to what is explicitly written in Curtin; in all such cases, this miscoding helps Albouy in attempting to overturn our results.

2.1 The Central Issue in Albouy's Table 1

Almost the entirety of Albouy's paper is in his Table 1. In fact, the rest of his paper plays almost no role whatsoever, since if the reader were to accept his Table 1, of course nothing would be robust – since there would be only 28 observations left from our original data set. And these 28 observations appear to have been chosen and recoded by Albouy to make our original results look as weak as possible. Thus the rest of the 50 page paper (including the appendix) is really a distraction.

What does Table 1 do? Essentially, instead of trying to recode all of our data (as he did in previous comments), Albouy simply drops most of it on the grounds that it is inadmissible or too faulty to be considered. In his second attempt to overturn our findings, four years ago, Albouy wrote (2004, p. 4) there are "only 36 distinct mortality rates". Now apparently, he decides that there are only 28 data points. We fear that as this process continues, our dataset may end up consisting of one observation.

How did Albouy decide that there are only 28 valid data points and how they should be recoded? His principle (p.9) is "to drop countries with conjectured mortality rates that originate from outside their borders – including the "benchmarked" Latin American data…"

The least appealing part of this (not at all justifiable) strategy is to drop all the Latin American data. Albouy's rationale for this radical action is that our source, Gutierrez (1986), does not mention specific Latin American countries. He states categorically: "Gutierrez does not provide mortality rates by country… It is AJR who assign the countries to the three regions," (p.5). This is simply not true. As we made clear in our 2005 response to his paper, in fact Gutierrez mentions many cities in Latin America and assigns them clearly to his mortality zones (and this is quite parallel to the nature of the data available from Curtin and other sources). See pp.34-35 of Gutierrez, where he goes in detail by region, for example saying that "Merida, Managua and Panama in Central America... Santiago-de-Cuba, Santo-Domingo and San-Juan in the Antilles... Cartagena, Cayenne, Belem, Sao-Luis and Olinda in tropical South America" are in his high mortality region; and the middle mortality region includes, "Durango, Guadalajara, Mexico and Guatemala" in North and Central America, as well as Bogota, Quito, Cuzco, Arequipa, La Paz, and Sucre on the plateau of the Andes, Trujillo and Lima by the Pacific, Sao Paolo and Curitiba on the Atlantic (all in his tropical South America category), and in temperate South America, Santiago, and Concepcion (Pacific side), and Tucuman, Corboda, and Buenes Aires on the Atlantic side).

For other cases which are unclear, it is in fact straightforward to classify different countries based on the temperature regions that Gutierrez provides. In Appendix 1 of AJR (2005) we refuted in detail Albouy's previous attempt to recode the Latin American data. We assume now he agrees he made a mistake, including in the way he calculated the mortality rates (as we showed in our 2005 paper) or at least he agrees that proper treatment of the Latin America data does not serve to overturn our results. Since his first strategy failed, he has a new one – drop all the data.

We have no problem with using heteroscedastic-clustered standard errors as a robustness check or even as the main specification – given that countries' estimates (including for some places outside of Latin America) are to some degree based on overlapping or pooled data. But there is no remotely acceptable reason for dropping the Latin American data and therefore all information about the region from our dataset. The results in Albouy's paper that drop these data are invalid and should be disregarded.

In total, Albouy drops 36 countries (to go from 64 to 28 observations). Remember that he claims he is dropping countries with conjectured mortality. In doing so, he ignores the substantial evidence that we have repeatedly provided on the similarity of diseases in some instances where countries are close neighbors – our approach follows the completely standard view in the medical and demographic literature (both modern and historical). In addition, in response to his earlier requests, we provided substantial documentation and additional sources for mortality in the Caribbean (e.g., in AJR 2005), but this is apparently ignored.

There is also a lot of (suspect) judgment in the way Albouy chooses which countries are in and which are out. He drops Singapore, but keeps Malaysia, which is based on the same source in our original work. He drops Australia, even though we have provided new data that supported our original estimate (in AJR 2005). He drops Pakistan, even though it is based on Indian data from Curtin, and Bangladesh and Sri Lanka – based on the same data – are not dropped. He drops some African data, following a precise pattern that is hard to discern. At best, a justification for the judgment in the details of his approach is not completely clear. He has also not at all responded to the point-by-point substance of our previous rejoinders (and the supporting evidence we provided to support our estimates); it is as if he is substantially ignoring AJR (2005) and AJR (2006).

Absent any other plausible explanation, the broad pattern of data editing evident here (again) suggests that data points are being dropped strategically (and certainly without any compelling justification). Again, a much more reasonable approach would be to use heteroscedastic-clustered standard errors. In this case, as we show in Table 1 here (and mentioned in our original paper), our main results are not substantially affected.

Overall, much of what Albouy does in his new 50-page comment relies on reducing the sample to 28 observations. This has no justification, and we do not find it surprising that after throwing out more than half of our sample strategically Albouy is able to weaken some of our results. There is nothing that merits serious consideration here.

2.2 The Remainder of Albouy's Table 1

Albouy's next tactic, which we have repeatedly argued against, is based on the notion that there is a significant distinction between "barracks" and "campaign" rates in reality or in Curtin. He states (p. 6) that "Curtin carefully distinguishes between what he terms "barracks rates" and "campaign rates". This is completely untrue and Curtin does no such thing.

In fact, Albouy is himself aware that this statement is untrue. He quickly weakens it to the following: "Curtin usually discusses whether a mortality rate is from a campaign or not." (p. 7). But this is equally untrue.

Consider Curtin's 1989 book, Chapter 1, whose Table 1.1 provided the first set of numbers we used (i.e., the "first mortality estimate" in Appendix Table A2 of AJR 2000). Any reader of this chapter will see that there is simply no basis here for making the distinction between "barracks" and "campaign" rates that Albouy wants to make.

As we have argued repeatedly before and we show again in the Appendix here, many instances that Albouy codes as "barracks" actually involved a considerable amount of campaigning. Curtin mentions some campaigns as part of his historical discussion, but he does not make a systematic barracks vs. campaign distinction, presumably because he does not view this distinction as so important. Equally significant, Albouy is extremely selective in his reading of Curtin. An objective reader cannot but conclude that Albouy's campaign dummy is simply coded on the endogenous variable (institutions today) in a way that clearly serves Albouy's purpose of trying to weaken our results.

Let us provide two examples here (others discussed in the Appendix below illustrate both that Curtin in general makes no such distinction and that Albouy systematically miscodes what ever Curtin says). First and probably most egregiously, Albouy decides that New Zealand (and on this basis Australia) is a barracks number even though Curtin discusses at the top of p. 13 losses from battles in New Zealand – British troops were campaigning in New Zealand against Maori tribes. As we have pointed out repeatedly before, troops were in the colonies in order to engage in certain amount of fighting in expeditions. Of course they were engaged in "campaigns". On the basis of this, is the number we used for New Zealand, 8.55, a barracks or campaign number? There is no basis in Curtin for determining this and he certainly makes no such distinction. If one insisted in making this artificial (and as we have argued essentially meaningless) distinction, one would be forced to code New Zealand as that "campaign" rate on the basis of campaigning against Maori tribes. In fact, 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."

So why does Albouy decide this is a barracks number? The answer is clear. If the New Zealand number is taken to be a campaign number, then this will not serve Albouy's purpose of overturning our results.

Another particularly egregious case is Hong Kong, for which the data point comes the China Field Force. Albouy knows this well (and has discussed in writing, including on p.A11 of this paper!). As the name suggests, the China Field Force was a field force engaged in fighting (and in this instance, Curtin actually says so – see Table A8.2, p.239, in Disease and Empire!). But Albouy chooses to code this as a "barracks" rate because this seems to be consistent with this objective.

These miscodings are not inconsequential. Even a minimal amount of correction to Albouy's campaign dummy changes his results significantly. The Appendix reviews the details of his campaign dummy for other countries. While we cannot ascertain the presence or absence of every campaign in every data point (and as we have emphasized there was some amount of campaigning in all instances), it is apparent that in many instances, such as the New Zealand and Hong Kong examples above, data coded as "barracks" by Albouy is actually from an episode involving heavy campaigning. Even a minimal correction for this has major implications. This is shown in our Table 1.

Panel A of this table replicates Albouy's Panel C, with the only difference that we show the coefficients and standard errors for the campaign and laborer dummies (which interestingly Albouy omits). These are never individually or jointly significant. Panel B of our Table 1 shows the results with a minimal correction of Albouy's campaign dummy, only recoding the obvious cases of New Zealand and Hong Kong discussed above as campaigns. It shows that just correcting these two egregious cases gives results that are very different from Albouy's Panel C. Correcting for heteroscedasticity and clustering (as preferred by Albouy), settler mortality is now statistically significant in all of the specifications, apart from column 7.

As we discuss in more detail in the Appendix, we have gone through the historical record for all our observations, to determine if there were significant campaign experiences in the sample period. This is a difficult exercise (for which Curtin is not authoritative, as it was not his focus) and we are not of the opinion that systematic comparable evidence really exists on this point. Nevertheless, we find that Albouy missed or ignored a great deal of relevant detail. In particular, there is strong evidence from standard sources that – in addition to New Zealand and Hong Kong – campaign affected mortality from disease in our estimates for Jamaica, Sri Lanka, Malaysia, Senegal, Singapore, Trinidad and Tobago, USA and South Africa; in addition, the Australia estimate is affected because it is based on New Zealand. We accordingly propose an "extended" recoding of Albouy's campaign dummy, and Panel C of our Table 1 shows the results when these corrections. (We have also looked at recoding only some of these observations; the general result is something between what we show in Panel B and what is in Panel C.)

In Panel C, log mortality is always significant. We do not find evidence that a reasonable coding of the campaign dummy affects the robustness of our original results.

This discussion shows two things very clearly. (1) There is no basis for a systematic coding of campaigns versus barracks, and in contrast to what Albouy claims, Curtin makes no such consistent distinction. (2) Albouy consistently codes the campaign dummy in order to obtain the results that he wishes, in particular, to weaken the first stage relationship from our 2001 paper – there is no other plausible explanation for his handling of New Zealand and Hong Kong. As with Albouy's attempt to reduce our sample to only 28 points, there is nothing worthy of serious debate here.

3 Other Points

Albouy also has an extensive discussion of recoding the data number for Mali. This has no justification, but also has almost no effect on our results. It is largely a smokescreen. In our original paper and all our responses we made it perfectly clear what our coding rule was. Albouy still makes his own judgment as to what is "reasonable" in Mali (p. 4). To us, there seems to be no reason to reject our coding rule on the basis of subjective assessments of what is Albouy finds "reasonable," particularly since it seems to us that his criterion for reasonableness appears to be to minimize the t-statistic of the first stage. The number for Mali was high because of a yellow fever outbreak, but the threat of yellow fever was certainly an important disincentive to European colonization of the "white man's graveyard." As we pointed out in the data appendix of the 2000 working paper version of our paper (AJR 2000, p. 33) other data on earlier expeditions in this region had mortality rates of the same size and during Mungo Park's Second Expedition down the Niger River almost every European died of tropical diseases. There is nothing unreasonable about the number we used which we took from Curtin on the basis of our well defined and clearly explained coding rule. Therefore, his strategy of recoding our data again for his objective of weakening and "overturning" our results is entirely unacceptable.

It should be emphasized that the effect of changing Mali's mortality estimate to 280 per 1,000, which is what Albouy now does, has no effect on our results in the full sample – in fact, it barely changes the value of the second decimal place in our point estimates. This is not surprising, as in and around Table 1B of our 2005 reply, we already showed that capping Mali (and all other mortality rates) at 250 per 1,000 – which would be very much on the low side for high mortality areas during the period under consideration – does not affect the robustness of our results. His recoding of Mali also hardly makes a difference even in the sample of 28 countries presented in his latest comment. With no covariates, using his preferred estimate of 280 per 1,000, the coefficient on settler mortality is -0.59 and the heteroscedastic-clustered standard error is 0.24. With our original Mali mortality estimate, the coefficient is also -0.59 and the standard error is only slightly lower, at 0.19.

In addition, Albouy includes a laborer dummy in his regressions (together with the campaign dummy discussed above). The labor dummy is almost entirely inconsequential. We showed in the working paper version of our paper (AJR 2000) that none of our basic findings changed when we dropped the data for which Albouy codes a laborer dummy.

Let us end by making a few additional points, essentially repeating issues we raised before, which are still being ignored or misrepresented by Albouy. The current version of his comment still attempts to sow continual confusion for the reader. For example on page 2, he criticizes our previous demonstration that his arbitrary recoding of African data has no impact on the real findings which hold even if you drop Africa. His response to this is to drop all of the Latin American data and argue that we only have 13 data points after he drops Latin America! Therefore, we conclude that Albouy's response to our statement that our results are robust to dropping Africa is: But they are not robust if almost everything else is dropped so that in total we have 13 data points!

The new version continues to make the same incorrect, and frankly quite insulting, accusations of the previous versions such as "six assignments are based upon AJR's misunderstanding of former names of countries in Africa." We understand very well the historical geography of Africa, and we suspect that we understand it much better than Albouy does.

On page 3 he states again "when more than one rate is available, they take the earliest rate". As we have explained in every response over the past 4 years this is not what we did. We took the first peacetime rate if it was possible to discern this. But Albouy continues to repeat what is not true. We really don't understand what the issue is here. Albouy also continues to argue as if it were significant.

"Second, AJR's mortality rates never come from actual European settlers" (Albouy, 2008, p. 2). This is true, but we never claimed otherwise. This is why always referred to our variable as "potential settler mortality". We never wanted to measure the actual mortality rates of settlers, but the mortality rates of comparable people had they decided to migrate. Our original paper is absolutely clear about this. Our argument is that the rates we used from Curtin were representative of the mortality rates that would have faced European settlers.

Albouy also again repeats accusations that we have contradicted in great detail before.

For instance on page 8 he argues that we violated our coding rules for "Sudan, Egypt and Madagascar." We did not, as we have explained at length. In our last response (AJR 2006) we quoted from the official military history of the Sudan campaign on this.

4 Conclusion

Albouy has come up with an entirely new comment, with an entirely new set of arguments in order to try to "overturn" our results. As with his previous attempts, the points he raises have no scientific merit and misrepresent Curtin and the known historical and medical record. In brief, Albouy's new strategy is: (1) to arbitrarily drop data, particularly Latin America, (2) to create an arbitrary and often incorrect distinction between barracks and campaign data, and then code a dummy specifically designed to weaken the first stage relationship in our 2001 paper.

Appendix

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

USA - this is for American troops 1829-1838. But US soldiers were obviously fighting Indian wars in this period. Again, Curtin does not discuss this number, but this period includes a number of Indian wars: the Second Seminole War in Florida, 1835-1842; The Black Hawk War 1832; and the Creek War of 1836.

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

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

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

4.1 Summary

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

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Table 1: First Stage Estimates, Dependent Variable is Expropriation Risk

Correcting Albouy's Campaign Dummy

	No controls (1)	Latitude (2)	Without neo-Europes (3)	Continent Dummies (4)	Continent Dummies & Latitude (5)	Mean Temp & Min Rain (6)	Percent European 1975 (7)	Malaria in 1994 (8)
Panel A: Albouy coding, with campaign and forced labor dummy coefficients reported								
Log mortality	-0.45	-0.39	-0.31	-0.37	-0.30	-0.12	-0.27	-0.26
	(0.18)	(0.20)	(0.17)	(0.22)	(0.23)	(0.21)	(0.19)	(0.24)
Campaign Dummy	-0.72	-0.71	-0.43	-0.61	-0.59	-0.74	-0.72	-0.85
	(0.46)	(0.46)	(0.42)	(0.49)	(0.49)	(0.43)	(0.43)	(0.54)
Forced Labor Dummy	-1.61	-1.39	-1.34	-1.45	-1.20	-1.62	-1.44	-1.41
	(0.89)	(0.91)	(0.89)	(0.93)	(0.95)	(0.80)	(0.92)	(0.98)
Latitude		1.47			1.49			
		(1.31)			(1.43)			
Percent European, 1975							0.014	
							(0.006)	
Malaria in 1994								-0.73
								(0.44)
Panel B: Correcting campaign dummy								
Log mortality	-0.60	-0.53	-0.39	-0.46	-0.38	-0.27	-0.42	-0.46
	(0.18)	(0.19)	(0.17)	(0.21)	(0.21)	(0.19)	(0.19)	(0.25)

All standard errors are heteroscedastic-clustered. The dependent variable is expropriation risk. The independent variables are log mortality (our original series), a dummy for whether the data come from a "campaign" experience and a dummy for whether the data come from a "forced labor" experience. Panel A replicates Panel C from Albouy, but reports the coefficient on the campaign and forced labor dummies using his data and specification. Panel B makes our correction to his campaign dummy. Panel B includes the campaign and forced labor dummies, but these are not reported to save space (and as they are not significant).