EJW
ECON JOURNAL WATCH
Scholarly Comments on Academic Economics

ECON JOURNAL WATCH 19(1)
March 2022: 85–108

Long-Run Determinants of Economic Growth: Putterman and Weil Revisited

Jason Briggeman[1]

LINK TO ABSTRACT

Some researchers have tried to investigate whether there is a statistical relationship between people's ancestries and the economic performance of their countries. Genealogical research mostly ceases to be reliable past 200 years ago, but these researchers who surmise that economic development is partially owed to 'deep roots' tend to presume that today's ethnic groups correspond to the geographical location of ancestors.

Louis Putterman and David Weil (hereafter PW) published in 2010 a "World Migration Matrix" data set in which for the most part they infer from a person's year-2000 ethnicity the year-1500 location of the person's ancestors. PW (2010) then treat the forms of government that prevailed between the years 1 CE and 1500 within the boundaries of a year-2000 country as a personal characteristic that was passed from the people who lived within those boundaries in the year 1500 to their descendants in the year 2000. PW announced in the *Quarterly Journal of Economics* a finding that this supposed characteristic has a positive relationship with current income, sharing regression results indicating a strong linear relationship for the year 2000: countries whose populations were low in "ancestry-adjusted state history" purportedly had lower GDP per capita than did countries whose populations were high in the characteristic.

PW (2010) is widely cited and the "World Migration Matrix" has been used

1. Austin Community College, Austin, TX 78752. Errors are mine. I thank Nathaniel Bechhofer, Adam Gurri, and Mark Koyama for their kind help.

in other research. The paper is acknowledged as important among those papers that offer similar claims about possible effects of 'deep roots.' Its publication in a top-five economics journal has no doubt contributed to some belief in its central finding.

Less known is an article coauthored by Putterman that was published years later in the *Journal of Economic Growth*. Oana Borcan, Ola Olsson, and Putterman (hereafter BOP) conducted a similar analysis, but treated the forms of government that prevailed between the years *3500 BCE* and 1500 CE as the personal characteristic that was passed down from year-1500 persons to year-2000 persons. BOP announced a finding that once we assume 5,000 years of history shaped our year-1500 ancestors rather than merely 1,500, "the current level of economic development across countries has a hump-shaped relationship with accumulated state history" (2018, 1).

Our situation then seems to be that the reader of PW (2010) alone would presume that ancestry-adjusted state history has a linear relationship with current development, and the reader of BOP (2018) would have the more nuanced understanding that there is a linear relationship when only 1,500 years of history is used while there is a hump-shaped relationship when 5,000 years of history is used.

Neither is true. I show here that *there is a hump-shaped relationship when only 1,500 years of ancestry-adjusted state history is used*. Because of data errors, this hump-shaped relationship would not have been found by PW (2010) had they looked for it, which they did not; meanwhile, BOP (2018) only showed that there is not a hump-shaped relationship when looking at 1,500 years of state history *unadjusted for ancestry*; BOP neglected to provide regression results using the crucial ancestry adjustment. Furthermore, I show here, at length and in various ways, that *the linear relationship PW (2010) claimed to find is much less substantial than their presentation indicates*.

The findings here are important because in so many ways the hump-shaped relationship is less compelling, its interpretation is less clear, and its implications less obvious than those of a linear relationship. When he launched his research into 'deep roots,' Putterman hypothesized a linear effect.[2] I argue here that the PW (2010) evidence for this hypothesis is not strong. BOP (2018) characterize themselves as putting forward a hypothesis of a hump-shaped effect and then

2. "An index of state antiquity was developed by Brown University Professor of Economics Louis Putterman and then Brown University undergraduate Valerie Bockstette circa 1999–2000 to test the proposition that present-day countries that had been the site of nation-states, kingdoms or empires over longer spans of history have achieved more rapid economic development in recent decades. This proposition or conjecture was suggested to us by the observation that countries in East Asia, with long histories of nationhood, have done much better economically in the late 20th century than have countries in sub-Saharan Africa on which the nation-state was imposed by 19th century colonization" (Putterman 2017, 1).

confirming that hypothesis.[3] I think we should disregard BOP's claim that they tested "a theoretical framework" with data.

Literature on persistence

Morgan Kelly (2019; 2020) assesses studies of "persistence" including PW (2010). Kelly notes the often very large regression coefficients found in such studies:

> A substantial literature on deep origins or persistence finds that many modern outcomes such as income or social attitudes strongly reflect the characteristics of the same places in the more or less distant past, often centuries or millennia previously. …
>
> Naturally, such findings are open to various charges of *p* hacking, of publication bias, of answers in search of questions, of scepticism about mono-causal and largely atheoretical explanations of complex phenomena, about the mechanisms driving persistence, and so on. However, all of these objections crumble into irrelevance in the face of one blunt fact: the unusual explanatory power of these persistence variables. While a judicious choice of variables or time periods might coax a *t* statistic past 1.96, there would appear to be no way that the *t* statistics of three, four, or even larger that appear routinely in this literature could be the result of massaging one's regressions, no matter how assiduously. (Kelly 2020, 2)

Kelly proceeds, however, to demonstrate that the inevitable presence of spatial autocorrelation could have caused many of these very high t-statistics, and thus the effect of persistence variables is likely greatly overstated throughout the literature. Kelly (2019) carried out simulations of PW (2010) to find whether ancestry-adjusted spatial noise would 'explain' year-2000 income. In Kelly's preferred set of simulations, not only was the ancestry-adjusted noise significant at the .001 level in over half of the simulations, but the noise outperformed the actual ancestry-adjusted state history variable 35 percent of the time (Kelly 2019, 21, 35).

I find Kelly's efforts sufficient to cast some doubt on 'persistence' results generally and those of PW in particular. But I also would deny Kelly's notion that the "various charges" that economists normally raise against weak empirical

3. "We outline a theoretical framework where accumulated state experience increases aggregate productivity in individual countries but where newer or relatively inexperienced states can reach a higher productivity maximum by learning from the experience of older states. The predicted pattern of comparative development is tested in an empirical analysis where we introduce our extended state history variable" (BOP 2018, 1).

research must "crumble into irrelevance" whenever one beholds a t-statistic of "three, four, or even larger." After all, Kelly himself first calls for the use of…geographic control variables (2020, 3ff.). Yes, innocent misspecification can result in the generation of huge t-statistics, and run-of-the-mill "objections" regarding omitted variable bias (yawn!) therefore can be sufficient to undermine findings that rest upon those whopper t-stats.

And PW (2010) *did* carry out regressions with geographic controls. Kelly (2019) does show that the *uncontrolled* regression result of PW has a good chance of being simple noise, but Kelly (2020) in general advocates controls of a form similar to those PW use. Since the key controlled regression in PW chalks up a t-statistic of nearly three, quite possibly our post-Kelly view of PW (2010) should be more skeptical than before but not (yet?) to the point of dismissal. And if that's true of PW (2010), perhaps it is true of the other persistence studies. In short, I think while Kelly has brought the question into serious doubt, there is further work to do before we can feel strongly one way or the other about persistence findings.

The contribution of the current paper then can be considered to be doing that dull work of raising the usual objections to which economists resort when they doubt certain findings. I locate particular problems that undermine particular findings in particular studies, and in that way this study merely takes a place among those studies. I don't purport here to level the entire field of persistence studies. But I find it intriguing that what I've found in replicating and assessing PW (2010) aligns with what Kelly (2019; 2020) has to say about persistence even though most of the work reported on below was done prior to my seeing Kelly's papers.

The organization of the remainder of the current paper is as follows. Section 3 describes the data errors in PW (2010) that (as will be shown in Section 5) would have prevented them from finding the hump-shaped relationship had they looked for it. Section 4 shows that robustness checks, including ones advocated in other work by Putterman, indicate a much less substantial linear relationship than PW (2010) claimed. Section 5 shows that (with the data corrections presented in Section 3) the hump-shaped relationship is present using 1,500 years of ancestry-adjusted state history as opposed to only with 5,000 years as claimed by BOP (2018). Section 6 assesses the central claim of PW (2010)—that the linear relationship they found is "surprisingly" strong. Section 7 concludes.

Corrections and updates to Putterman and Weil's data

Putterman and Weil's data set has substantial errors, including needlessly missing values that caused many countries to be dropped from regressions. For the key regression with PW's geographic controls presented in Table 1 below, the corrected and updated[4] data includes 38 countries that were excluded by PW because of missing values for the latitude, climate, landlocked, state history, or GDP variables, and these values simply should not have been missing.[5] Among the countries erroneously dropped by PW were all of the former Soviet republics except Latvia and Georgia, plus several other countries previously under communist rule, which perhaps accounts for some of the substantive differences in the results. The corrections and updates cause the t-ratio on the ancestry-adjusted state history variable to fall from 5.24 to 5.05 in the simple regression and from 2.93 to 2.75 in the regression with PW's geographic controls. R-squared falls from 0.22 to 0.19 in the simple regression and from 0.59 to 0.50 in the regression with PW's geographic controls.[6]

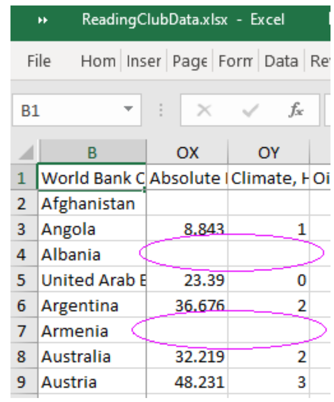


Figure 1. Albania and Armenia are two of the 38 countries that would have been included in Putterman and Weil's key controlled regression were it not for needlessly missing values, seen here in the original data file shared by Putterman.

4. The 'updates,' as opposed to the 'corrections,' primarily consist of my using Putterman's latest revisions to his state history data (see BOP 2018) in the construction of the ancestry-adjusted state history variable. A second sense in which the data might be considered 'updated' is that since I newly sourced the GDP data for the year 2000 from the World Bank, any improvements that have been made to that data since 2010 would be reflected in my results but not in PW's.

5. Box A1 lists these mistakenly excluded countries (as distinct from countries that are simply absent from PW's data set, some of which are listed in Box A2). The reader may note that Table 1's corrected and updated regression with controls has 37 more cases than does PW's regression, not 38. The discrepancy is because one case included by PW is excluded from my regressions because that country (Syria) has a missing value in my source for GDP data. I could not locate a source with the same year-2000 GDP per capita figures that PW use. The correlation between their dependent variable and mine, for the countries included in PW's Table II column (2) regression, is 0.961 (n=135). The correlation between their dependent variable and Angus Maddison's year-2000 GDP per capita variable, for the same set of countries, is 0.966 (n=132), and that between my dependent variable and Maddison's variable is 0.951 (n=134).

6. It must be noted that Putterman and Weil present two different results for a simple regression of year-2000 GDP per capita on ancestry-adjusted state history. The first is their Table II col. (2), which I replicated and reproduced in my Table 1, with n=136, and those are the results described above in the text that have a t-ratio for ancestry-adjusted state history of 5.24 and an r-squared of 0.22. The second is their Table IV col. (1), which they present alongside their controlled regression results. This second simple regression has n=111, just like their controlled regression does, i.e., it excludes the countries with missing values for the latitude, climate, and landlocked variables. In that second simple regression, which I also exactly replicated but do not reproduce in a table here, the t-ratio for ancestry-adjusted state history is 5.92 and the r-squared is 0.29.

TABLE 1. Putterman and Weil's original OLS results vs. OLS results with corrected and updated data

	Dependent variable: ln(GDP per capita 2000)			
	PW Table II col. (2)	PW Table IV Panel A col. (6)	Corrected and updated data	
	(1)	(2)	(3)	(4)
Ancestry-adjusted state history	2.01*** (0.38)	1.24*** (0.42)	2.01*** (0.40)	1.01*** (0.37)
Absolute latitude		0.0337*** (0.0084)		0.0318*** (0.0060)
Landlocked		−0.558*** (0.172)		−0.750*** (0.196)
Eurasia		−0.327 (0.247)		−0.169 (0.218)
Climate		0.235* (0.121)		0.191* (0.113)
constant	7.61*** (0.17)	6.99*** (0.20)	7.92*** (0.17)	7.47*** (0.20)
N	136	111	148	148
R squared	0.219	0.593	0.194	0.496

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Robustness checks of the Putterman and Weil findings

Today's division of the world into countries, with regard both to borders and to the sheer number of countries, is endogenous to history, 'deep' or otherwise, and can have potentially profound effects on regression results when country is used as the unit of observation, as BOP (2018, 3) note. Just to illustrate such effects, I'll use regions—also endogenous and contingent, but a different division of the world—rather than countries as the unit of observation. PW (2010, 1637–1639) define 11 regions of the world and provide some data and analysis by region, but they do not assess whether their main finding holds when using those regions as the unit of observation. Computing GDP per capita and the ancestry-adjusted state history for those regions is straightforward—each is equal to the population-weighted average across countries—so it is easy to show that their finding does not hold up at all. Even in a simple regression the t-ratio is only 0.55, and it is possible to flip the sign of the coefficient by removing just one observation, that for sub-Saharan Africa (Table 2).

Figure 2. Regions scatterplot

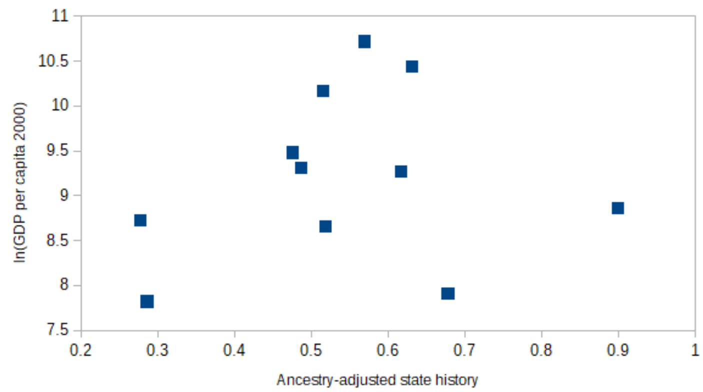


TABLE 2. OLS results with corrected and updated data, for the 11 regions defined by Putterman and Weil

	Dependent variable: ln(GDP per capita 2000)	
	all regions	all regions except sub-Saharan Africa
	(1)	(2)
Ancestry-adjusted state history	0.983 (1.791)	−0.390 (1.926)
constant	8.68*** (1.01)	9.57*** (1.13)
N	11	10
R squared	0.032	0.005

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Weighting countries by population is another obvious and easy method to address endogeneity or contingency of country borders, as Bryan Caplan (2016a) notes. Caplan (2016b) finds when that weighting by population is applied using PW's uncorrected data the coefficient on ancestry-adjusted state history becomes very small. Using the corrected and updated data plus PW's controls I find that the sign on that coefficient actually *flips*, at the same time that *every one of PW's geographic controls is statistically significant at the 1-percent level* (Table 3, column 2), in notable contrast to the unweighted regression where only two of the four controls are even significant at the 5-percent level.

One might object to Caplan that year-2000 population is itself endogenous to migration and thus population weighting is not an ideal way to address endogeneity of borders. BOP (2018, 3), in fact, assert that the best unit of observation would be not countries but rather "equal-sized grid cells" of land; they say however that undertaking such an analysis would be very difficult. Here again though there is

TABLE 3. WLS results with corrected and updated data

	Dependent variable: ln(GDP per capita 2000)					
	Weighted by population 2000		Weighted by land area 2000		Weighted by arable land area 2000	
	(1)	(2)	(3)	(4)	(5)	(6)
Ancestry-adjusted state history	0.0350 (0.3475)	−0.167 (0.303)	0.921** (0.363)	0.950*** (0.281)	0.0692 (0.3948)	0.617** (0.275)
Absolute latitude		0.0313*** (0.0065)		0.0388*** (0.0047)		0.0438*** (0.0051)
Landlocked		−0.887*** (0.292)		−0.916*** (0.249)		−0.636*** (0.235)
Eurasia		−0.863*** (0.182)		−0.867*** (0.154)		−1.28*** (0.13)
Climate		0.405*** (0.118)		0.126 (0.110)		0.309*** (0.104)
constant	8.62*** (0.24)	7.92*** (0.21)	8.73*** (0.20)	7.78*** (0.22)	9.04*** (0.23)	7.60*** (0.22)
N	148	148	147	147	146	146
R squared	0.000	0.452	0.043	0.518	0.000	0.628

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

an obvious and easy method to address the concern: we can weight the country observations by land area. When applying land-area weights (Table 3, columns 3–4), the coefficient on the ancestry-adjusted state history variable is much smaller in the simple regression (t-ratio of 2.54, about half as with the unweighted data), but is about the same in the regression with PW's geographic controls (t-ratio of 3.38).

But it makes little sense to attribute vast spaces of unoccupied, untouched, unbothered-with land to the nominal rule of year-1500 governments. Putterman and his coauthors of a 2014 *American Economic Journal: Macroeconomics* article said so, when they questioned Daron Acemoglu, Simon Johnson, and James A. Robinson (2002) regarding the "appropriateness" of year-1500 population density estimates: "The major conceptual problem is that in most countries, the large majority of the people are found in a small subset of the territory, often including river valleys, coastlines, and fertile plains, and the ratio of largely uninhabited to inhabited territory varies among countries as defined by their modern borders in a fashion that may reflect less on the level of development of the society than on geographic happenstance" (Chanda, Cook, and Putterman 2014, 5 n.4). Therefore a likely better alternative to weighting by land area would be to weight by only *arable* land area, since that represents so much of the space that human societies occupy or live adjacent to.[7] Using arable land area to weight the data (Table 3, columns 5–6),

7. While there could be some endogeneity concern regarding the particular areas that are cultivated or not in the year 2000 as compared to 1500, I suspect that concern should be judged minor.

the coefficient on ancestry-adjusted state history almost disappears in the simple regression (t-ratio of 0.18) and is significant but smaller in the regression with PW's geographic controls (t-ratio of 2.24); meanwhile, *all four of PW's geographic controls are significant at the 1-percent level* (absolute t-ratios ranging from 2.70 to 9.50).

I now provide two further robustness checks of PW (2010)—checks that are suggested in published work by Putterman and his colleagues, but that PW did not provide themselves. First, Putterman and his coauthor Matthias Cinyabuguma were in a 2011 paper "concerned with what accounts for the poor economic performance of many countries in Africa," and they argued: "If factors alleged to account for the African difference in global samples perform quite differently within Africa, their relevance to African and other development policy makers would be called into question" (Cinyabuguma and Putterman 2011, 219). As the analysis of regions above suggests, and as Table 4 below confirms, sub-Saharan Africa drives the linear relationship between ancestry-adjusted state history and year-2000 GDP per capita found by PW.[8] And therefore the Cinyabuguma and Putterman (2011) argument applies: Before we declare the PW results to have relevance for "development policy makers," we had better examine whether ancestry-adjusted state history is linearly related to year-2000 GDP per capita *within* sub-Saharan Africa.[9] It isn't, as shown in Table 4.

The second robustness check suggested in Putterman's work that I pursue here is to examine whether there is a relationship between ancestry-adjusted state history and year-1960 GDP per capita, rather than year-2000 GDP per capita. This analysis is readily conducted thanks to Putterman's 2014 *American Economics Journal: Macroeconomics* article coauthored with Areendam Chanda and C. Justin Cook, because it provides a "World Migration Matrix" for the years 1500–1960.[10] The reason to conduct this robustness check is simply that if 'deep' history matters a lot, then there seems little reason to think it would affect specifically the year 2000 but not other recent years. And in fact Chanda, Cook, and Putterman's purpose

8. If we were to drop the sub-Saharan African countries from the unweighted simple regression (Table 1, column 3) we would cause the coefficient on ancestry-adjusted state history to drop from 2.01 to 0.83, its t-ratio to fall from 5.05 to 1.66, and R-squared to collapse from 0.19 to 0.03 (as shown in Table 4, column 3).

9. Readers may wonder if there is substantial variation in ancestry-adjusted state history across the sub-Saharan African countries. There is; the mean value is 0.19 and the standard deviation 0.24 (n=44). For all other countries the mean is 0.54 and the standard deviation 0.21 (n=110). One in four countries in sub-Saharan Africa has an ancestry-adjusted state history value above 0.3; over half of all other countries have a value between 0.4 and 0.7.

10. Chanda, Cook, and Putterman perhaps should not have bothered, because their 1500–1960 matrix is hardly different from PW's 1500–2000 matrix. For countries in my data set I find that the correlation between state history as adjusted by the two matrices is 0.9997 (n=154). For more on this see note 19 below.

in creating a 1500–1960 Migration Matrix was exactly that—"to check whether there is anything unusual about the year 1995 as a representation of recent incomes"—and when they found similar results using data for the year 1960 as they had for the year 1995 they took it as a demonstration of robustness (Chanda, Cook, and Putterman 2014, 16).[11] For my regressions I used Angus Maddison's GDP per capita figures (**link**) as the dependent variable, both for 1960 and 2000.[12] The results with unweighted data are shown in Table 5, and all told the relationship between ancestry-adjusted state history and GDP per capita in 1960 appears to be about half as strong it was in 2000—while the relationships between PW's geographic controls and GDP per capita are hardly different. The t-ratio on the ancestry-adjusted state history variable in the simple regression on year-2000 GDP per capita is 6.67, but for that on year-1960 GDP per capita it is only 4.41, and the corresponding t-ratios for the regressions with PW's geographic controls are 4.25 and 2.10.

When weighting the data by arable land area there is no relationship at all between ancestry-adjusted state history and GDP per capita in 1960, while *every one of PW's geographic controls is significant at the 1-percent level*, as shown in the first two

11. Putterman (2000), meanwhile, argued the exact opposite: that state antiquity *only* matters for modernization "under late 20th Century conditions" (Putterman 2017), when state antiquity would have helped developing countries cope with the imposition of large-scale institutions by colonizers: "While traditions of bureaucratic and state-level organization pre-date colonial encounters in such countries as India, China, or Japan, such traditions were typically absent or present only on smaller social scales in most parts of Africa" (Putterman 2000, 8). Putterman's (2000) empirical strategy was to use developing countries' 1960 levels of "population density, farmers per cultivated hectare, and the prevalence of irrigation" as his measure of their "pre-modern development," and to see if there was a relationship between that and their GDP growth 1960–1990. He controlled for per capita GDP in 1960, but labeled that as "initial" per capita GDP, not "pre-modern" per capita GDP. The coefficient he found on 1960 GDP per capita was negative ("suggesting that poorer countries grew more rapidly, *ceteris paribus*," p. 14), while those on 1960 population density, 1960 farmers per cultivated hectare, and 1960 prevalence of irrigation turned up positive ("Each indicator has the predicted sign, and their addition to the equation adds to the explanatory power of the regression," p. 16). He acknowledged that these results are, to say the least, open to interpretation: "A more general problem with these variables is that while their use to proxy for levels of pre-modern development is consistent with the frameworks of Boserup and other evolutionary theorists, it is not yet possible to rule out alternative interpretations of the correlations between them and economic growth that are reported below. Thus, agricultural intensification could be argued to be conducive to later economic development simply because it generates reserves of labour and other resources that can be drawn upon for the industrialisation process. Population density may encourage growth due to scale economies that bear no relation to the 'broad human capital' conception offered above. With a variable set too generic to exclude alternative hypotheses, only further research can determine whether the arguments of this article in fact account for the phenomena observed" (Putterman 2000, 13).

12. Maddison's year-2000 GDP per capita variable is for whatever reason more favorable to the PW thesis: There is, both with unweighted data and when weighting by arable land area, a stronger relationship when regressing ancestry-adjusted state history on Maddison's year-2000 figure than when regressing it on my year-2000 figure from the World Bank.

columns of Table 6. The relationship for ancestry-adjusted state history remains intact for the year 2000, as shown in the third and fourth columns. So there may be something substantially unusual about the year 2000 as a representation of recent incomes.

TABLE 4. OLS results with corrected and updated data, for sub-Saharan Africa and for all other countries

	Dependent variable: ln(GDP per capita 2000)			
	sub-Saharan Africa only		all other countries	
	(1)	(2)	(3)	(4)
Ancestry-adjusted state history	0.145 (0.642)	0.0149 (0.5537)	0.825^{*} (0.497)	0.812^{*} (0.432)
Absolute latitude		0.0341^{*} (0.0191)		0.0274^{***} (0.0063)
Landlocked		-0.617^{**} (0.232)		-0.685^{**} (0.265)
Eurasia				-0.528^{***} (0.190)
Climate		0.104 (0.187)		0.168 (0.138)
constant	7.61^{***} (0.17)	7.36^{***} (0.42)	8.81^{***} (0.25)	8.13^{***} (0.23)
N	43	43	105	105
R squared	0.002	0.191	0.029	0.331
Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.				

TABLE 5. OLS results with corrected and updated data, Maddison's GDP per capita data for 1960 and 2000

Dependent variable:	ln(Maddison's GDP per capita 1960)		ln(Maddison's GDP per capita 2000)	
	(1)	(2)	(3)	(4)
Ancestry-adjusted state history			2.18^{***} (0.33)	1.48^{***} (0.35)
1960 ancestry-adjusted state history	1.17^{***} (0.26)	0.611^{**} (0.290)		
Absolute latitude		0.0318^{***} (0.0051)		0.0357^{***} (0.0063)
Landlocked		-0.616^{***} (0.186)		-0.544^{***} (0.158)
Eurasia		-0.630^{***} (0.224)		-0.450^{**} (0.214)
Climate		0.159 (0.135)		0.150^{*} (0.085)
constant	6.97^{***} (0.12)	6.62^{***} (0.19)	7.19^{***} (0.15)	6.66^{***} (0.19)
N	127	127	148	148
R squared	0.124	0.463	0.245	0.512
Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.				

TABLE 6. WLS results with corrected and updated data, Maddison's GDP per capita data for 1960 and 2000

Dependent variable:	ln(Maddison's GDP per capita 1960)		ln(Maddison's GDP per capita 2000)	
	Weighted by arable land area 2000[13]			
	(1)	(2)	(3)	(4)
Ancestry-adjusted state history			0.843^{**} (0.382)	1.56^{***} (0.27)
1960 ancestry-adjusted state history	−0.373 (0.479)	0.00290 (0.36251)		
Absolute latitude		0.0369^{***} (0.0060)		0.0456^{***} (0.0048)
Landlocked		-0.684^{***} (0.249)		−0.375 (0.228)
Eurasia		-1.46^{***} (0.14)		-1.35^{***} (0.13)
Climate		0.329^{***} (0.099)		0.205^{**} (0.098)
constant	7.89^{***} (0.30)	6.85^{***} (0.20)	8.06^{***} (0.22)	6.67^{***} (0.21)
N	125	125	145	145
R squared	0.005	0.729	0.033	0.644

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The hump-shaped relationship

BOP (2018, 2) claim there is a quadratic relationship between deep history and present GDP per capita, and they say they discovered the quadratic relationship only because their new study looked 3,500 years further back in time: "We show that the relationship between state history and current income per capita across countries is hump-shaped rather than linear, and that this is due to the inclusion of state experience before the Common Era." But they in fact showed that only for state history that is *not adjusted for ancestry*:

> Taken together, our estimation results so far [that is, using the unadjusted *Statehist*] are consistent with our predicted pattern [that is, concavity of per capita GDP with respect to state history]. Moreover, this becomes evident only when we employ the new extended *Statehist* index. Are the estimates

13. The World Bank, my source for arable land area, does not have data for 1960. It does have data for 1970 (for example), but there are numerous missing values. The correlation between the data for 1970 and for 2000 on the percentage of land that is arable, for countries included in the simple regression for 1960, is 0.951 (n=124).

> improved by accounting for the state histories of the ancestors of present-day populations, instead of the state histories of places? To investigate this, we estimate the model for per capita GDP above using the ancestry-adjusted *Statehist* index. The results are displayed in Table 6, where we use the *Statehist* index in 1500 CE adjusted by the migration matrix (as in previous studies, but for the first time including full state history before 1 CE). (BOP 2018, 32–33)

In other words, BOP (1) showed a quadratic relationship between per capita GDP and unadjusted *Statehist*; (2) showed that that quadratic relationship was only present when including pre-CE history; and (3) then "improved" the estimates by using ancestry-adjusted *Statehist*, with those estimates shown in their Table 6 (BOP 2018, 30) and the fitted quadratic regression curve from the uncontrolled regression (their Table 6 Column 2) is displayed in my Figure 3. But what they did not show is whether the quadratic relationship between per capita GDP and ancestry-adjusted *Statehist* is only present when including pre-CE history. Here in my Table 7, I provide the estimates showing that a quadratic relationship exists in the corrected and updated PW data, which is to say that a quadratic relationship exists even without the pre-CE history, and I display the fitted quadratic regression curve from the uncontrolled regression in Figure 4. Tables 7 and 8 together confirm that there is a "hump-shaped" relationship per the standard applied by BOP (2018, 21), that of there being a negative- or downward-sloping portion of the fitted quadratic curve (Table 7) and that there are positive and negative linear relationships holding respectively below and above the optimal value of the independent variable (Table 8). And so the claim by BOP (2018) regarding the supposed effect of state history before the Common Era does not hold when using ancestry-adjusted state history—and of course they affirm that the ancestry adjustment is vital.[14]

Table 9 presents the quadratic relationship using data weighted by arable land area. The absolute coefficients on ancestry-adjusted state history and its squared term are large, but the combined effect size is what yields readily to interpretation. Table 10 thus presents combined effect sizes for different values, which are similar in size to the effects found in the linear regressions. Beyond that, the implications of the quadratic relationship are far different than those of a linear relationship. For example, as shown in Table 10 the quadratic relationship indicates that having the highest possible value of ancestry-adjusted state history is actually worse for GDP per capita in 2000 than having the lowest possible value would be. Not only that, but at the weighted first-quartile value of ancestry-adjusted state history (0.375) the combined effect size is 1.83, while at the weighted third-quartile value (0.687)

14. "Population flows after 1500, when the era of colonization began, are instrumental in mapping the impact of historical events to today's economic performance" (BOP 2018, 22).

the combined effect size is 1.54—so having the third-quartile value of ancestry-adjusted state history is worse for GDP per capita in 2000 than having the first-quartile value would be.

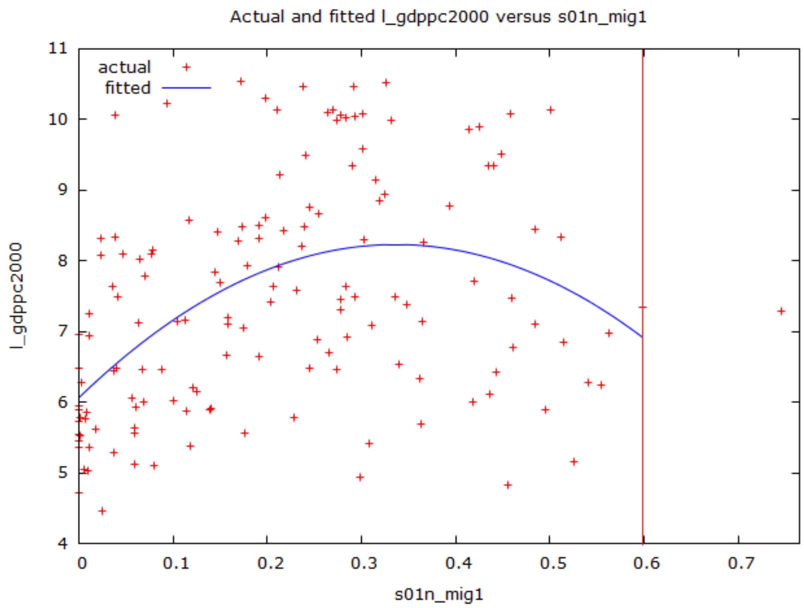
TABLE 7. OLS results for quadratic relationship

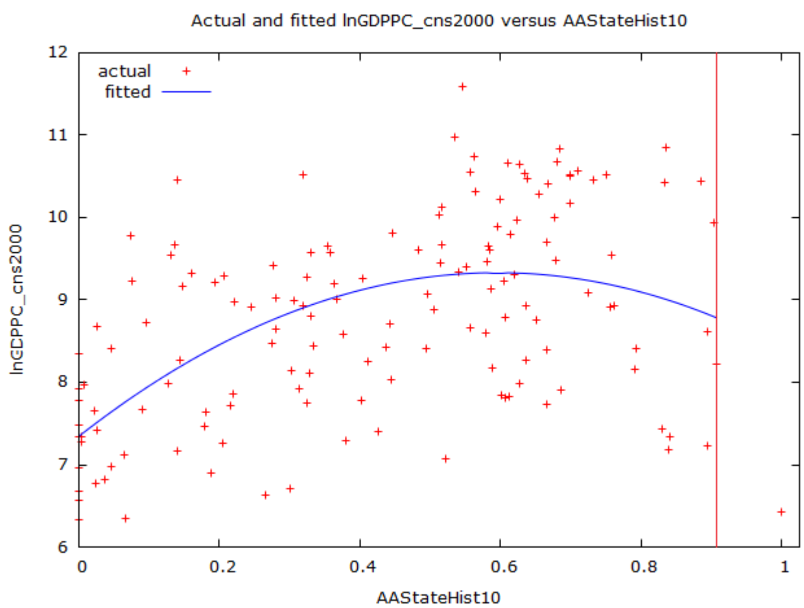
	Dependent variable: ln(GDP per capita 2000)			
	PW original data		Corrected and updated data	
	(1)	(2)	(3)	(4)
Ancestry-adjusted state history	5.24*** (1.12)	2.51** (1.03)	6.68*** (1.11)	3.68*** (0.95)
Ancestry-adjusted state history squared	−3.86*** (1.43)	−1.52 (1.23)	−5.61*** (1.43)	−3.17*** (1.15)
Absolute latitude		0.0310*** (0.0087)		0.0266*** (0.0060)
Landlocked		−0.502*** (0.164)		−0.686*** (0.186)
Eurasia		−0.263 (0.256)		−0.0994 (0.2148)
Climate		0.243* (0.125)		0.202* (0.109)
constant	7.20*** (0.19)	6.85*** (0.23)	7.34*** (0.17)	7.20*** (0.21)
N	136	111	148	148
R squared	0.273	0.600	0.291	0.524
Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.				

TABLE 8. OLS results for a 'linear segment check of inverse-u relationship'[15] with corrected and updated data

	Dependent variable: ln(GDP per capita 2000)	
	Ancestry-adjusted state history *below* optimal value for ln(GDP per capita 2000)	Ancestry-adjusted state history *above* optimal value for ln(GDP per capita 2000)
	(1)	(2)
Ancestry-adjusted state history	3.36*** (0.47)	−3.39* (1.91)
constant	7.59*** (0.16)	11.7*** (1.3)
N	98	50
R squared	0.331	0.082
Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.		

15. The comparable check by BOP 2018 is their Table D8 (page 21 of their online appendix).





Figures 3 and 4. BOP's fitted quadratic regression curve (top) and mine (bottom), showing year-2000 GDP per capita's 'hump-shaped' relationship with ancestry-adjusted state history 3500 BCE–1500 CE and 1 CE–1500 CE, respectively. I have taken the liberty of erasing the portion of the curves to the right of the second-highest value of ancestry-adjusted state history to prevent visual distraction by the disparate behavior of the two curves over values that are all but out of sample.

TABLE 9. WLS results for quadratic relationship with corrected and updated data, dropping cases with missing ancestry-adjusted state history

	Dependent variable: ln(GDP per capita 2000)		
	Weighted by arable land area 2000		
	(1)	(2)	(3)
Ancestry-adjusted state history		8.08*** (1.29)	6.70*** (0.79)
Ancestry-adjusted state history squared		−8.51*** (1.32)	−6.55*** (0.82)
Absolute latitude	0.0397*** (0.0048)		0.0418*** (0.0042)
Landlocked	−0.695** (0.237)		−0.417** (0.198)
Eurasia	−1.20*** (0.13)		−0.972*** (0.118)
Climate	0.373*** (0.102)		0.329*** (0.087)
constant	7.91*** (0.17)	7.71*** (0.29)	6.46*** (0.23)
N	146	146	146
R squared	0.614	0.227	0.745
Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.			

TABLE 10. Effect size at different values of x when $y = B_0 + 8.08x - 8.51x^2$

Value of x	Effect on y
0	0.00
0.2	1.28
0.4	1.87
0.6	1.78
0.8	1.02
1	−0.43

What findings are surprising?

Putterman and Weil say that “the ability of these historical measures to predict income today is surprisingly high” (2010, 1651). They therefore suggest we should be surprised by the strength of the correlation between their ancestry-adjusted state history variable, which combines information on year-1500 governance and year-2000 ethnicities, and year-2000 GDP per capita.

I show in Table 11 that year-1500 population[16] and year-2000 population together have an effect on year-2000 GDP per capita—which, personally, I do not find to be surprising—and that the linear relationship between ancestry-adjusted state history and year-2000 GDP per capita is relatively *weaker*. Ancestry-adjusted state history adds 0.03 to the R-squared achieved solely by PW's controls, but year-1500 population and year-2000 population add 0.06.

TABLE 11. OLS results with corrected and updated data, dropping cases with missing ancestry-adjusted state history

	Dependent variable: ln(GDP per capita 2000)			
	(1)	(2)	(3)	(4)
Ancestry-adjusted state history			1.01^{***} (0.37)	1.02^{***} (0.34)
ln(Population 2000)		0.135 (0.084)		0.0576 (0.0861)
ln(Population 1500)		-0.226^{***} (0.068)		-0.187^{***} (0.069)
Absolute latitude	0.0304^{***} (0.0064)	0.0278^{***} (0.0057)	0.0318^{***} (0.0060)	0.0290^{***} (0.0055)
Landlocked	-0.815^{***} (0.197)	-0.718^{***} (0.198)	-0.750^{***} (0.196)	-0.692^{***} (0.194)
Eurasia	0.107 (0.240)	0.351 (0.228)	−0.169 (0.218)	0.0654 (0.2145)
Climate	0.231^{**} (0.111)	0.274^{**} (0.108)	0.191^{*} (0.113)	0.231^{**} (0.108)
constant	7.74^{***} (0.19)	8.36^{***} (0.95)	7.47^{***} (0.20)	8.86^{***} (0.95)
N	148	148	148	148
R squared	0.463	0.523	0.496	0.553
Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.				

16. Putterman and Weil (2010, 1637) say that in their Table I they "confine [their] analysis to looking at 11 large regions" because "population data for 1500 are very noisy, particularly at the country level." However, Putterman and Weil actually don't use the region-level data from their source, a book by Colin McEvedy and Richard Jones (1978). What Putterman and Weil use as the year-1500 populations for the regions is McEvedy and Jones's data for individual countries, summed up—but Putterman and Weil's data set doesn't include every country. For example, McEvedy and Jones's year-1500 estimate for the entire Caribbean region is 0.3m (1978, 299), but PW don't use that figure; instead PW sum up the populations of the seven Caribbean countries in their data set, arriving at 0.186m (though even that is incorrect, as it happens). Like Putterman and Weil, then, I use McEvedy and Jones's country-level estimates for 1500, and I see no reason to judge that data any more noisy than the data in Putterman and Weil's World Migration Matrix, which after all is also country-level data on where people were living in the year 1500.

Conclusion

The notion of a strong relationship between year-2000 GDP per capita and ancestry-adjusted state history is not robust to weighting by population or arable land area—such weights being sensible methods to address the historical contingency of today's country borders—but the strong relationships between year-2000 GDP per capita and distance from the equator, being landlocked, and climate *are* robust to those weights. Furthermore, sub-Saharan Africa drives the relationship observed in the PW (2010) regressions, yet the effect of ancestry-adjusted state history among sub-Saharan African countries is zero. And taking into account state history between 3500 BCE and 1 BCE does not transform whatever relationship may appear to exist between early development and current incomes.

Reflect for a moment on the historical contingency of today's country borders. Why does the PW (2010) finding fail the robustness checks? It's not only because the robustness checks address spatial autocorrelation *a la* Kelly (2019), but also because the checks treat recent historical accidents such as the partition of Africa as accidents, while in PW (2010) those accidents drive the results. If there were a few countries in sub-Saharan Africa rather than a few dozen, PW's regressions would have found nothing—that huge t-ratio on the simple regression crumbling down to where p isn't even below .05. And the number of countries in Africa is hardly a result of anything that happened before 1880, let alone before 1500 or 1 CE.

Further reason to refrain from accepting the conclusions of PW (2010) is the nature of the "World Migration Matrix"—the table used by PW (2010) to adjust state history data for population ancestry. Its builders' frequent assumptions that year-2000 ethnicities reveal the year-1500 location of ancestors are sometimes unwarranted and are not justified in principle.[17] It neglects, and as a result often

17. Present ethnic categorizations are at best only weakly suggestive regarding migrations of 80, 150, or 400 years ago. "The nations in which we claim citizenship are no more than two hundred years old, and the ethnic groups with which we identify, while sometimes older, have been remarkably changeable," notes the scholar of migration Patrick Manning (2005, 4). PW (2010) purport to overcome the weak suggestiveness of ethnicity by making "heroic assumptions" (Putterman 2016). By diving into the PW (2010) appendices the immense role of such assumptions can be appreciated. The "World Migration Matrix" often assumes that if a year-2000 person can be classified into an ethnic identity, then there was a corresponding social group in the year 1500 that contained 100 percent of that person's ancestors. Only somewhat less often, it incorporates the assumption that that year-1500 social group lived entirely within the year-2000 borders of a single country. In combination these assumptions lead PW into some silly statements, e.g., "The majority of Austria's population (95.2%) is ethnic Austrian" (Appendix-Europe, page 1), by which they mean, incredibly: Almost everyone living in Austria today is descended entirely from 'Austrians,' a people who in the year 1500 lived entirely within the borders of present-day Austria. And according to the World Migration Matrix the only other countries where descendants of Austria's year-1500 population live today

conflicts with, other public data sets about migration.[18] Its sources, primarily American and British encyclopedias, exhibit incompleteness regarding past migrations across present-day country borders, an incompleteness that is surely systematically biased.[19] And as its data is specifically about the locations of people in the year 1500, it would seem that justification is needed for any direct application to data from other time periods, and yet without supplying such justification PW applied the Matrix not to a variable for state development as of the year 1500 but to

---

are Australia, Canada, Italy, Slovenia, Switzerland, and the United States. Yes, it says *Germany* has no such persons, and further that *none* of the year-1500 ancestors of today's population of Germany lived in the territory of *any* country that borders Germany. It also says that none of the year-1500 ancestors of today's population of Hungary lived in Austria. It says that South Koreans have zero North Korean ancestry, and vice versa! Such absurdities are all over the Matrix.

18. World Bank data on foreign-born populations (**link**) shows that 45 of 165 countries had more foreign-*born* population in the year 2000 than, per the World Migration Matrix, their year-2000 population had 'foreign' *year-1500* ancestors (see Box A3). Needless to say, that likely should not be the case for *any* country. Notably, that same World Bank data set was used by Chanda, Cook, and Putterman (2014), who treated its differences between year-2000 and year-1960 foreign-born populations as a measurement of migration and then applied that measurement to the "World Migration Matrix" to produce a "1500–1960 Migration Matrix." I was unable to successfully replicate what Chanda, Cook, and Putterman did to produce that 1500–1960 Migration Matrix (which I nonetheless use in Section 4 above), and the process they used to produce it seems to me flawed. One example of such a flaw is that their process assumes, without acknowledgment, zero differences in natural rate of population change 1960–2000 among persons of different ancestry. A more dramatic example is that in many cases the World Bank data indicated an *increase* in foreign-born population between 1960 and 2000 in a country where the World Migration Matrix had said there was *zero* 'foreign' ancestry *in 2000*, meaning that any straightforward calculation by Chanda, Cook, and Putterman should have led them in many cases to an absurd conclusion that there was negative foreign ancestry in 1960, but of course the 1500–1960 Matrix contains no negative values.

19. Dramatic mass movements may be recorded in an encyclopedia paragraph or two, but encyclopedias do not recount, in any detail or with any precision, regional patterns of reproduction and relocation over the several centuries after 1500 during which the borders of today's countries were often nonexistent, not meaningful, or both. Consider that of the 138 countries in the World Migration Matrix that have two or more "immediate neighbors" (those sharing a land boundary or separated by less than 24 miles of water), the Matrix says 91 of them have *zero* ancestry from *over half* of their immediate neighbors. And of the 155 countries with at least one immediate neighbor, the Matrix says 61 have zero ancestry from *all* of their immediate neighbors. Early in their paper, PW (2010, 1634–1637) devote a few pages to examining the data as it would be if countries that are immediate neighbors were lumped together. There they write that lumping of immediate neighbors is needed because while "[t]he principal diagonal of the matrix provides a quick indication of differences in the degree to which countries are now populated by the ancestors of their historical populations…in some cases, the diagonal entry may give a misleading impression" because, they say, migration that is from neighboring countries should be considered at least "near-indigenous" (PW 2010, 1635). However, subsequent to those few pages, PW do *not* lump immediate neighbors in any of their analyses. And it is easy to find other researchers who have used the World Migration Matrix but not bothered to do any such lumping: Shrira et al. (2018, 45) create a "historical heterogeneity index" variable that simply "counted the number of source countries" and take that variable to be measuring "the historical likelihood of encountering unfamiliar cultural outgroups." Klasing (2013, 453) constructs a variable using "the share of its current population originating from another country." Spolaore and Wacziarg (2013, 333 n.11) "define whether a country's population today is composed of more than 50 percent of descendents of its 1500 population."

a variable for state history over the entire period 1 CE to 1500.[20]

BOP (2018) purport to offer and confirm a hypothesis that early development has a bit of a "hump-shaped" relationship with year-2000 GDP per capita: too much early development, like in eastern Asia, is bad; too little, like in sub-Saharan Africa, is also bad; northwestern Europe had just the right amount. Since 2000, though, the countries of eastern Asia and sub-Saharan Africa have seen faster economic growth than have the countries of northwestern Europe.

We should now have a new understanding with regard to the data provided in PW (2010) and BOP (2018). The message of a strong linear relationship, conveyed in PW (2010), is wrong; the message conveyed in BOP (2018), that there is a hump-shaped relationship *because of pre-Common Era history*, is also wrong. So what is the effect of history, 'deep' or otherwise, as far as the evidence here goes? One could point to Table 9 and say: If you weight countries by arable land area and regress year-2000 GDP per capita on basic geographic controls you'll get an R-squared of 0.61. By adding information on world history up through the year 1500 plus information about presumed connections between present-day ethnicities and year-1500 locations of ancestors, and allowing that information to have nonlinear effects, your R-squared increases to 0.74.[21] If you were to add further information on history more recent than the year 1500, obviously your R-squared would go far higher.

20. The PW (2010) empirical strategy considers people alive in the year 1500 to inherit or exhibit their land's pre-1500 history, as if (among other presumptions) there was zero migration across year-2000 borders prior to 1500. They offer nothing like the reservations that Comin, Easterly, and Gong (2010, 83) express about the application of the World Migration Matrix to pre-1500 data: "This [applying the Matrix] is straightforward for the 1500 AD technology measure. It is more problematic for the 1000 BC and 0 AD exercise, since we have no data on migrations before 1500 AD. It still seems of interest to correct the 1000 BC and 0 AD measures by the post-1500 migration matrix to test a peoples-rather-than-places technology persistence view. The post-1500 migrations are arguably the most consequential, since the discovery of the New World and the technological advances in oceangoing transport made wholesale replacement of low-technology people by high-technology people more likely than in earlier eras. We could assume either that pre-1500 migrations were random and orthogonal to the error term, or that they also tended to direct high-technology peoples to low-technology places (because of the ease of conquest and the high returns from applying more advanced technology to a previously underdeveloped land area). In the first case, the coefficient on 0 AD and 1000 BC would be unbiased. In the second case, the coefficient would be biased downward, making persistence look lower than it really was."

21. For comparison's sake, by adding variables for year-1500 population and year-2000 population to the basic geographic controls, the R-squared increases from 0.61 to 0.70.

Appendix

Box A1. Countries excluded from Putterman and Weil's key regression (their Table IV Panel A col. (6)) because of mistakenly missing values, in descending order by population

- Russia
- Vietnam
- Ukraine
- Myanmar
- Tanzania
- Uzbekistan
- Iraq
- Saudi Arabia
- Kazakhstan
- Cambodia
- Belarus
- Serbia
- Azerbaijan
- Tajikistan
- Libya
- Kyrgyzstan
- Turkmenistan
- Croatia
- Bosnia and Herzegovina
- Moldova
- Lithuania
- Eritrea
- Lebanon
- Albania
- Armenia
- Liberia
- North Macedonia
- Slovenia
- Namibia
- Estonia
- Eswatini
- Cyprus
- Fiji
- Guyana
- Equatorial Guinea
- Qatar
- Comoros
- Cabo Verde

Box A2. Countries excluded from all of Putterman and Weil's regressions, or even from their data set entirely, but whose year-2000 population is greater than that of Cabo Verde (the smallest country in their data set for which there should not have been any missing values), in descending order by population

- North Korea
- Afghanistan
- Cuba
- Somalia
- Puerto Rico
- United Arab Emirates
- Oman
- Kuwait
- Guinea-Bissau
- Timor-Leste
- Djibouti
- Bahrain
- Bhutan
- Suriname
- Luxembourg

Box A3. Countries that, per the World Bank, had a percentage of foreign-*born* population in the year 2000 that was higher than, per the World Migration Matrix, the percentage of their year-2000 population's *year-1500 ancestors* that were foreign, in descending order by the product of the percentage-point difference and the year-2000 population

- Germany
- Russia
- Saudi Arabia
- United Arab Emirates
- Japan
- United Kingdom
- Spain
- Greece
- Netherlands

- Austria
- Nepal
- Burkina Faso
- South Korea
- Switzerland
- Ethiopia
- Belgium
- Sweden
- Uganda
- Portugal
- Mozambique
- Ireland
- Philippines
- Norway
- Poland
- Tanzania
- Denmark
- Armenia
- China
- Gambia
- Bahrain
- Finland
- Tajikistan
- Luxembourg
- Algeria
- Mauritania
- Slovenia
- Bosnia and Herzegovina
- Czech Republic
- Rwanda
- Tunisia
- Senegal
- Congo, Rep.
- Papua New Guinea
- Lesotho
- Oman

Box A4. Countries that have at least one immediate neighbor and, per the World Migration Matrix, have *zero* year-1500 ancestry from *all* of their immediate neighbors, in descending order by number of immediate neighbors

- China
- Germany
- Brazil
- Tanzania
- Algeria
- Burkina Faso
- Mozambique
- South Africa
- Argentina
- Bolivia
- Colombia
- Congo, Rep.
- Peru
- Senegal
- Uganda
- Uzbekistan
- Angola
- Ethiopia
- Finland
- Greece
- Guatemala
- Indonesia
- Mauritania
- Nigeria
- Tajikistan
- Thailand
- Turkmenistan
- Venezuela
- Chile
- El Salvador
- Eritrea
- Ghana
- Honduras
- Malawi
- Mexico
- Nicaragua
- Norway
- United Arab Emirates
- Bhutan
- Bosnia and Herzegovina
- Costa Rica
- Ecuador
- Guyana
- Nepal
- Netherlands
- Panama
- Papua New Guinea
- Somalia
- Tunisia
- Uruguay
- Australia
- Canada
- Dominican Republic
- Gambia
- Haiti
- Japan
- South Korea
- Lesotho
- Philippines
- Portugal
- Trinidad and Tobago

Data and code

Data and code used in this research is available from the journal website (**link**).

References

Acemoglu, Daron, Simon Johnson, and James A. Robinson. 2002. Reversal of Fortune: Geography and Institutions in the Making of the Modern World Income Distribution. *Quarterly Journal of Economics* 117(4): 1231–1294.

Borcan, Oana, Ola Olsson, and Louis Putterman (BOP). 2018. State History and Economic Development: Evidence from Six Millennia. *Journal of Economic Growth* 23: 1–40.

Caplan, Bryan. 2016a. Cross-Country Regressions and Population Weighting. *EconLog, Library of Economics and Liberty*, June 20. **Link**

Caplan, Bryan. 2016b. Ancestry, Long-Run Growth, and Population-Weighting. *EconLog, Library of Economics and Liberty*, June 23. **Link**

Chanda, Areendam, C. Justin Cook, and Louis Putterman. 2014. Persistence of Fortune: Accounting for Population Movements, There Was No Post-Columbian Reversal. *American Economic Journal: Macroeconomics* 6(3): 1–28.

Cinyabuguma, Matthias M., and Louis Putterman. 2011. Sub-Saharan Growth Surprises: Being Heterogeneous, Inland and Close to the Equator Does Not Slow Growth Within Africa. *Journal of African Economies* 20(2): 217–262.

Comin, Diego, William Easterly, and Erick Gong. 2010. Was the Wealth of Nations Determined in 1000 BC? *American Economic Journal: Macroeconomics* 2(July): 65–97.

Kelly, Morgan. 2019. The Standard Errors of Persistence. CEPR Discussion Paper 13783. Centre for Economic Policy Research (London). **Link**

Kelly, Morgan. 2020. Understanding Persistence. CEPR Discussion Paper 15246. Centre for Economic Policy Research (London). **Link**

Klasing, Mariko J. 2013. Cultural Dimensions, Collective Values and Their Importance for Institutions. *Journal of Comparative Economics* 41: 447–467.

Manning, Patrick. 2005. *Migration in World History*. New York: Routledge.

McEvedy, Colin, and Richard Jones. 1978. *Atlas of World Population History*. Harmondsworth, UK: Penguin.

Putterman, Louis. 2000. Can an Evolutionary Approach to Development Predict Post-War Economic Growth? *Journal of Development Studies* 36(3): 1–30.

Putterman, Louis. 2016. Correspondence with Bryan Caplan. *EconLog, Library of Economics and Liberty*, February 2. **Link**

Putterman, Louis. 2017. State Antiquity Index ("Statehist") Version 3.1 and Updates. November 10. **Link**

Putterman, Louis, and David N. Weil (PW). 2010. Post-1500 Population Flows and the Long-Run Determinants of Economic Growth and Inequality. *Quarterly Journal of Economics* 125(4): 1627–1682.

Shrira, Ilan, Arnaud Wisman, and Kenji Noguchi. 2018. Diversity of Historical Ancestry and Personality Traits Across 56 Cultures. *Personality and Individual Differences* 128: 44–48.

Spolaore, Enrico, and Romain Wacziarg. 2013. How Deep Are the Roots of Economic Development? *Journal of Economic Literature* 51(2): 325–369.

About the Author

Jason Briggeman is the managing editor of *Econ Journal Watch*, an adjunct associate professor of economics at Austin Community College, an associate editor for *Liberal Currents*, and an adjunct fellow of the Niskanen Center. His email address is jason@briggeman.org.

Go to archive of Comments section
Go to March 2022 issue

Discuss this article at Journaltalk:
https://journaltalk.net/articles/6047