HISTORY AND PROBABILITY

LAVAN (M.), JEW (D.), DANON (B.) (edd.) *The Uncertain Past. Probability in Ancient History*. Pp. xiv + 307, figs, maps. Cambridge: Cambridge University Press, 2023. Cased, £90, US$120. ISBN: 978-1-009-10065-6.
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This is an important book, its edited volume format uniquely justified. No individual could have produced the range of research essential to the book's central claim: that, when data is incomplete (and when is it not?), ancient historians should learn how to extract probabilistic information through the application of 'Monte Carlo simulations'. For this, the computations may become complicated, but the core idea is simple. We are provided with information (whether from first principles or based on documentary evidence) for a range of possibilities of certain variables. While it is difficult to calculate all those variations taken together, basic computing makes it possible to go through a great number of combinations of random values assigned to each variable and to simulate the results. With a sufficient number of simulations made, their aggregate becomes effectively as good as a precise calculation of the overall distribution of probabilities. We can thus find – given certain historical assumptions – the range of probabilities of the various outcomes.

This method is carefully and clearly explained by Jew and Lavan in the first, introductory chapter. The following papers are rigorous, compelling contributions, each adding substantially not only to the methodological claim but also to our understanding of ancient history (all the papers fall broadly within the area of economic history, which is to be expected; the method, however, is more widely applicable). In what follows I will present the main argument of the separate papers and raise a few queries, some methodological, some historical.

E. Mackil demonstrates that classical estates faced a non-trivial risk of confiscation through exile or foreign conquest. The paper appears entirely valid (perhaps because it is not entirely surprising). My query has to do with Mackil's framing of the significance of the claim. It is pointed out that there is a recent consensus that there was a classical Greek efflorescence and that it was related to the more-egalitarian institutions of the Greek *polis*. Mackil then claims that the causal link between the institutions of the *polis* and its efflorescence is in its guarantee of property rights – so that the observed risk of confiscation is a problem that the new consensus should somehow resolve. It seems to me, first, that the causal link between the *polis* and the efflorescence is that of growing human capital – are property rights really the distinguishing mark of the *polis*? –, and, second, that the implications of catastrophic risk are perhaps somewhat different from those of a general weakness of property rights.

Danon shows that there ought to have been a few households of senatorial wealth in Pompeii. This is a case where the dense graphs and calculations mask a simple argument. To the extent that we believe that even modest members of the elite had a property of 100,000 HS and above (and for this, Danon's strongest evidence comes from traditional political and economic history), the assumption that all households in Pompeii had a non-senatorial wealth of below 1,000,000 HS forces us to 'bunch' the distribution of elite household wealth in an improbably close cluster. Since no Pompeian senators are attested, Danon's argument adds probability to the independently plausible claim that the one million sesterces were a necessary rather than a sufficient condition for senatorial status.

G. Bransbourg makes two claims concerning Roman money supply. First, that it could not have been as heavily gold-based as implied by R. Duncan-Jones; second, that its high overall rate suggests that the ancient economy ought to have been bigger than usually assumed. The first claim I find persuasive, the second less so. This brings me to a general methodological point. The subtitle of the book refers to 'probability in ancient history', but the focus, as noted, is narrower, largely concerning the applicability of 'Monte Carlo simulations'. The editors allude to other probabilistic considerations, of which the most important must be Bayesian inference. Simply put, an assumption has an initial probability; an added argument changes that probability. When making claims, one needs to pay attention not only to the consequences of new arguments but also to how they interact with prior probabilities – 'Bayesianism' being, in effect, the statistician's way of saying that one needs to perform reality checks and to apply common sense. Mackil's claim is plausible – because she argues for an initially plausible claim (many ancient estates got confiscated). Danon's claim is plausible – because the consequence of this claim, in turn, is plausible on its own (many super-rich people were not senators). Or another example: as Bransbourg notes in the course of the argument (p. 143), 'it is inherently implausible that gold should have approximately the same weight in hoards and stray finds' –, so that, Bransbourg explains, the fact that Duncan-Jones found that his analysis pointed him to that inherently implausible conclusion should have alerted him to the likelihood that his analysis was wrong; a consideration that then applies, more generally, to Duncan-Jones's implausible high estimate of the fraction of gold in imperial-era coinage.

This is all well explained by Bransbourg, and I am surprised that he does not pay more heed to this type of argument as he reaches his other main conclusion. This goes as follows: the central value, computed by Bransbourg's method, for money supply during the High Empire is roughly the same as the central value usually assumed for the size of the economy as a whole (both values at about 15 billion). Since comparative evidence suggests that pre-modern money supply was unlikely to be more than 50% the size of the economy, Bransbourg concludes that the economy was bigger than usually assumed. Bayesian considerations, however, should remind us that our faith in the evidence for money supply should be weaker than our faith in the evidence for the size of the economy as a whole (which is much more directly based on strict demography). The overall result for Bransbourg's claims is both: a *strong* argument for taking the real size of money supply to be on the lower side of Bransbourg's distribution of probabilities; a *weak* argument for taking the real size of the economy to be on the higher side of the standardly assumed distribution of probabilities. (The methodology advocated in this volume is based on the consideration of the distribution of probabilities instead of just central values; it is somewhat strange that, at this point of the argument, Bransbourg seems to ignore this distribution, arguing as if the central value was all he had found.)

While framed as a critique of Bransbourg's argument, my account implies that he has made an important observation concerning the way in which the size of the ancient economy interacts with its money supply, allowing us to constrain further both estimates.

In general, probabilistic arguments work best when they help us think through the interaction of variables. This would be my only critique of P.V. Kelly's paper. This argues that economically ruined peasant families in Roman Egypt fairly frequently had to abandon their children. I do not doubt the grim realities plotted by Kelly's figures, but their interpretation seems to rely on an assumption concerning the *independence* of variables, mentioned in passing in p. 227: 'no linkage has been assumed between quality of harvest / prosperity and fertility or mortality'. Perhaps I misunderstand the claim, but it seems to me that many children projected by Kelly's model to have been abandoned could have died at home instead.

Kelly's argument has little to work with as far as standard documentary evidence is concerned, relying instead mostly on general assumptions concerning the viability of agriculture in the Nile under ancient conditions. Those assumptions, however, are well supported. N. Solonakis et al., studying the financial sustainability of grain funds, once again have little evidence to work with but, in their case, the assumptions are not as safe: the inputs into such funds, in the form of benefactions and civic contributions, are only lightly constrained by the epigraphic evidence. This article, then, is mostly a pure study of the interaction of variables. To be clear, this is a valuable exercise, and what Solonakis et al. show is that – contrary to views expressed in the literature – a considerable fraction of such funds *could* have been sustainable, given a certain threshold of funding, a threshold that the authors usefully quantify. This paper, stronger on the study of the interaction of financial variables, weaker (by necessity) on historical evidence, is the most 'economic' of the papers in the collection, and it is noticeable that it tends – my one critique – to marshal equations where ordinary prose could do just fine. (On the whole, this collection has many graphs and calculations, and yet it remains lucid and readable.)

The final paper, by J.W. Hanson, does not depend as essentially on 'Monte Carlo simulations', largely because the evidence from first principles is in this case quite robust. It is in broad outline a generalisation of M.H. Hansen's shotgun population estimates, to the Roman Mediterranean as a whole, finding a very high rate of urbanism (about 25%, depending on the definition of a 'city'; comparable to Europe in the eighteenth century). This rate is also found to be extremely stable throughout the Roman era. An implication left unmentioned by Hanson (who explicitly avoids discussions of wider chronological horizons) is that there ought to have been a rapid process of urbanisation in some unspecified pre-imperial era. The picture as a whole – rapid urbanisation, which then settles at a very high rate – is surprising, but in this case the evidence, as noted, is robust, somewhat overwhelming our Bayesian qualms. We are called to question many variables. Does Hansen overestimate the size of the Greek *polis*? Does Hanson err in the further application of the method? Could it even be that, overall, the population of the Mediterranean was somewhat on the higher side of the distribution of the probabilities we currently employ (so that the many city-dwellers found by Hanson should in fact be diluted by more rural inhabitants than usually assumed)? Whatever approach we take, Hanson's paper alerts us to the possibility of a startlingly modern-looking ancient Mediterranean. It is interesting that many of the papers in the collection – otherwise, defined by method rather than by an allegiance to any camp of ancient economic history – tend to cohere around this fairly optimistic view of antiquity.

Methodologically, too, the collection is largely speaking optimistic, as it should be. The emphasis is usually not on the claim that probabilistic estimates should replace central value estimates (so that instead of asserting absolute claims, we should stick with more prudent probabilities) but rather on the claim that probabilistic estimates should replace mere interval estimates – those that merely state upper and lower bounds.

Classicists are used to those extra-cautious claims that, say, 'Athens' population's size was somewhere between a few tens of thousands to a few hundreds of thousands'. Such wide intervals tell us nothing, a vacuity in which many past, positivist historians took perverse delight. Once we see the interval as possessing a well-defined structure – a distribution of probabilities –, it is no longer meaningless: it becomes, instead, eloquent in its own language. Lavan, Jew and Danon are right, and Classicists should learn this language of probabilities.

Stanford University REVIEL NETZ
revielnetz@gmail.com

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