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QUANTIFYING THE GRECO-ROMAN ECONOMY AND BEYOND

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QUANTIFYING ROMAN ECONOMIC PERFORMANCE BY MEANS OF PROXIES: PITFALLS AND POTENTIAL

Introduction

Many histories of economic growth over the *longue durée* usually start around AD 1000, tracing a gradual growth in the early middle ages with the weakening of feudalism and the development of urbanism, markets and economic institutions. [1] Long-run models and graphs of world economic performance tend to retroject the period before this as a more or less steady state, contributing to a view in which economic progress in preindustrial societies is minimal or gradual, but without engaging with the possibility of either sustained growth or serious economic collapse. [2]

Building on recent work in archaeology and ancient history, however, we can extend the perspective back beyond the AD 1000 watershed, and open up some very different views. Recent research has highlighted mounting evidence for the occurrence of both aggregate and per capita growth in classical antiquity, especially in the Roman period, followed by economic contraction or even outright collapse in the Western Mediterranean and NW Europe in the fifth to seventh centuries AD. [3] This picture challenges the idea of a steady state or minimal growth; moreover, there is some evidence of simultaneous per capita and population growth being sustained for perhaps two centuries between c. 50 BC and AD 150. Such evidence suggests that the Malthusian ceiling was capable of being reset to a higher level under the influence of certain technological shocks.

While there is an emerging consensus among ancient economic historians that the Roman period saw some (limited) economic growth, the focus of debate has

[1] North and Thomas 1973; Greif 2006. Allen 2011 starts around AD 1500.

[2] Saller 2002 = 2005; Maddison 2007.

[3] Temin 2006; Jongman 2007b; 2007b; Bowman and Wilson 2009a; Scheidel 2012b.

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now moved to whether that growth was simply population growth, or whether there was per capita growth as well; when such growth might have occurred, and what drove it and ended it. [4] Answers to these questions would affect the answers we would give to the question of whether such growth was a one-off, unrepeatable effect of the integration of the Mediterranean under Rome, or a process sustained over perhaps two centuries until terminated by exogenous shocks such as the Antonine Plague. [5]

Increasingly, historians and archaeologists are attempting to grapple with these questions by using proxy data that may be thought to bear some relation to certain sectors of the economy, or to overall performance. Richard Duncan-Jones, François de Callataÿ, Wim Jongman, Walter Scheidel, and my colleagues and I myself in the Oxford Roman Economy Project have all tried this approach in various ways. [6] Most of the proxies used have turned out to be problematic in one or another aspect, but this does not invalidate the exercise; probing the reasons for data bias leads to a better understanding of what the evidence actually does show. [7] In this paper I examine criteria for proxy construction and presentation, to reduce the misleading effects of graphing often imprecise data. I examine a number of commonly used proxies (shipwrecks, stature, lead and copper pollution, animal bone consumption), looking at their strengths and weaknessess. I also present some early attempts at constructing new proxies, some of which might hold greater promise but which currently either suffer from small sample sizes (fish-salting capacity, water-mills) or regionally uneven collection policies (building inscriptions), neither of which is an insuperable problem. The different pictures presented by archaeological, literary and documentary data for the same phenomena are compared, and the importance of regional disaggregation stressed. Finally, the paper tackles attempts to compare the trends suggested by several proxies.

1. *How (not) to use proxies*

We would like to be able to track economic performance of the Roman economy over time and in comparison with other economies, and GDP per capita would ideally be the best index to do so. Fig. 1, by Richard Saller based on research by G. Lucas, purports to do this. But it has no evidential value whatsoever,

[4] E.g. Jongman 2007a; Scheidel 2009; Wilson 2009b; and papers in Scheidel, Morris, and Saller 2007; Bowman and Wilson 2009b; Scheidel 2012b.

[5] E.g. Scheidel 2009; Temin 2012, 62-9.

[6] Duncan-Jones 1974; 1990; de Callataÿ 2005; Jongman 2007a; 2007b; Scheidel 2009; Wilson 2009b; Wilson 2011b; Bowman and Wilson 2009b; 2011; 2013.

[7] Cf. Wilson 2009a; 2009b.

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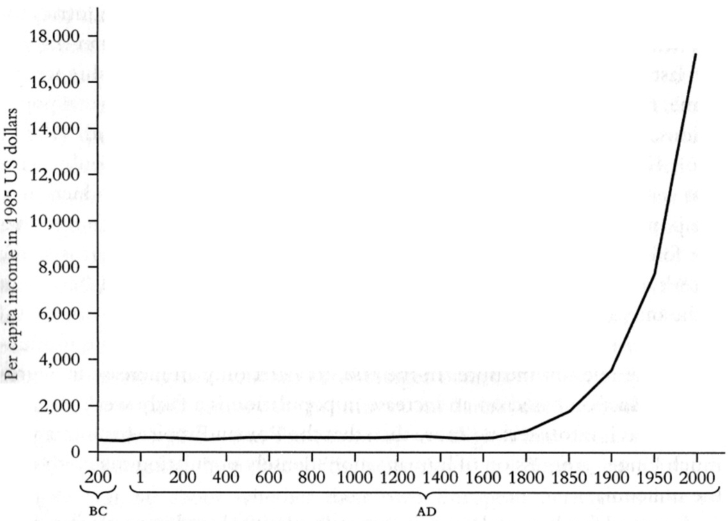

Fig 1. - Estimate of GDP per capita income in leading economies, 200 BC to AD 2000 (Saller 2002, fig. 12.1, after Lucas).

since the entire left-hand half of the graph, for the period before AD 1200 where we lack good time-series data, is based entirely on guesswork and assumptions. I remain unconvinced by attempts to calculate per capita GDP for the Roman period. We are still arguing about the size of the Roman population (55 million or 100,000 million at the end of the first century AD?), and we do not know GDP and rely on a series of assumptions to estimate it in wheat equivalent (itself a largely misleading and probably pointless exercise); the idea that we can come to any meaningful expression of per capita GDP therefore strikes me as wholly impossible. All attempts to calculate Roman GDP per capita have relied on a battery of different figures as inputs to the calculation of which almost none are actually known. [8] What you get out of such a calculation is entirely a function of the assumptions you put in.

In the case of Fig. 1, these assumptions included the constraint that in pre-industrial economies an average income of twice subsistence was a maximum limit. [9] This was based on the rather naïve assumption on the part of Lucas that the failure of Herodotos and Marco Polo to remark on massive income disparities in the societies they described was in any way meaningful, and the inaccurate assumption that Adam Smith did not mention major disparities in standards of living (he did, noting that the accommodation of British peasants far exceeded that of sub-Saha-

[8] Hopkins 1980, 117-20; Goldsmith 1984; Maddison 2007; Scheidel and Friesen 2009; Temin 2013, 243-61.

[9] Lucas 2004.

ran African kings).[10] But Jongman refers to instances of pre-industrial economies with average incomes of three times subsistence;[11] and in the case of the Roman economy, it is precisely the exploration of the limits of the possible that is at stake – how well was it possible for pre-industrial economies to do? One does not get anywhere with the question by imposing at the outset an assumption that constrains precisely the thing which one is trying to measure. Graphs of assumptions are unhelpful; if we are going to quantify, we need data, and our task is to select meaningful proxy indicators for economic performance, and to present them in a way that both reveals their weaknesses and highlights their utility.

This point is important: we like to show quantification visually, and the consequence is that other scholars then use our presentation of the data – our graphs and charts – to support larger arguments that they are making, often without examining the underlying data. Before I go further, I want to make some remarks about methods of graphing data.

A key example is the graph of Mediterranean shipwrecks published by Parker and widely used by others.[12] Parker's graph of the chronological distribution of 1,189 Mediterranean shipwrecks dated before AD 1500 has been used by several ancient historians to illustrate a supposed peak in maritime trade in the early Roman empire. The dataset contains a large number of poorly dated wrecks, and the shape of the graph is strongly affected by how one chooses to treat them – whether by using the mid-point of the date range assigned to each wreck, as Parker did (fig. 2), or spreading the probability that the ship sank in any particular year in that date range (fig. 3).[13] The former method inflates the number assigned to the second century AD, since the mid-point of the date range for all generically 'Roman' wrecks falls in that century; the second method by contrast shows a sharp drop from the first to second centuries AD. That is unexpected, and does not accord with the evidence for maritime trade recovered from terrestrial sites; the explanation must lie in a combination of biases in the evidence, including some or all of: increasing use of the archaeologically less visible barrel as a transport container, instead of the amphora; the under-representation of shipwrecks along the North African coast;[14] and possibly the construction of better harbour facilities that reduced the risk of wreck.[15]

[10] Smith 1775, Book 1 Chapter 1 (page 17 of the 1811 edition): 'and yet it may be true, perhaps, that the accommodation of an European prince does not always so much exceed that of an industrious and frugal peasant, as the accommodation of the latter exceeds that of an African king, the absolute master of the lives and liberties of ten thousand naked savages.'

[11] Jongman 2007a, 185.

[12] Parker 1992, fig. 3.

[13] This issue is discussed at length in Wilson 2009a; 2011b, 33-9.

[14] Wilson 2009a; 2011b, 33-9.

[15] For this latter point, Robinson, Rice and Schörle forthcoming.

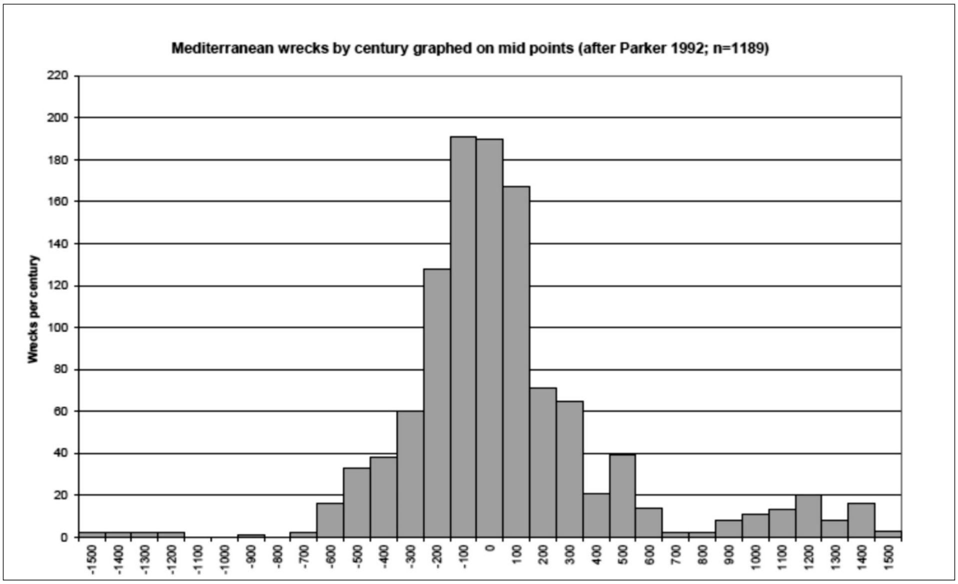

Fig. 2. - Chronological distribution of Mediterranean shipwrecks recorded by Parker (n = 1189), graphed on the midpoint of the date range (after Parker 1992, fig. 3).

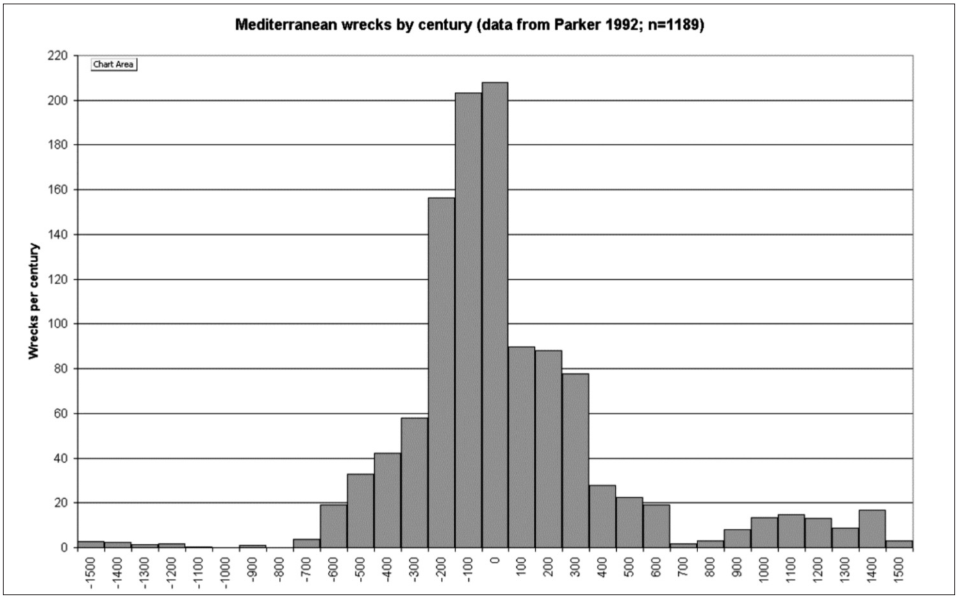

Fig. 3. - Chronological distribution of Mediterranean shipwrecks recorded by Parker (n = 1189), graphed according to an equal probability of sinking in any year within their date range.

The original shipwreck graph was presented in century-long slices; but the data can in principle be graphed at a finer resolution – figs 4-7 show updated data, on a total of 1,646 wrecks, graphed by century, half-century, quarter-century and 20-

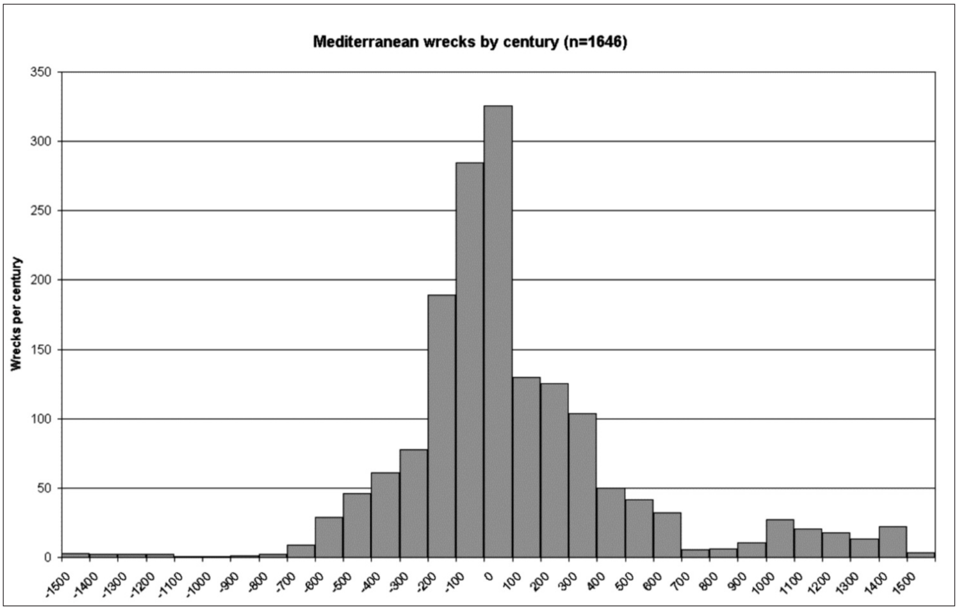

Fig. 4. - Chronological distribution of Mediterranean shipwrecks (n = 1,646) graphed by probability, by century. (Data collected by Julia Strauss).

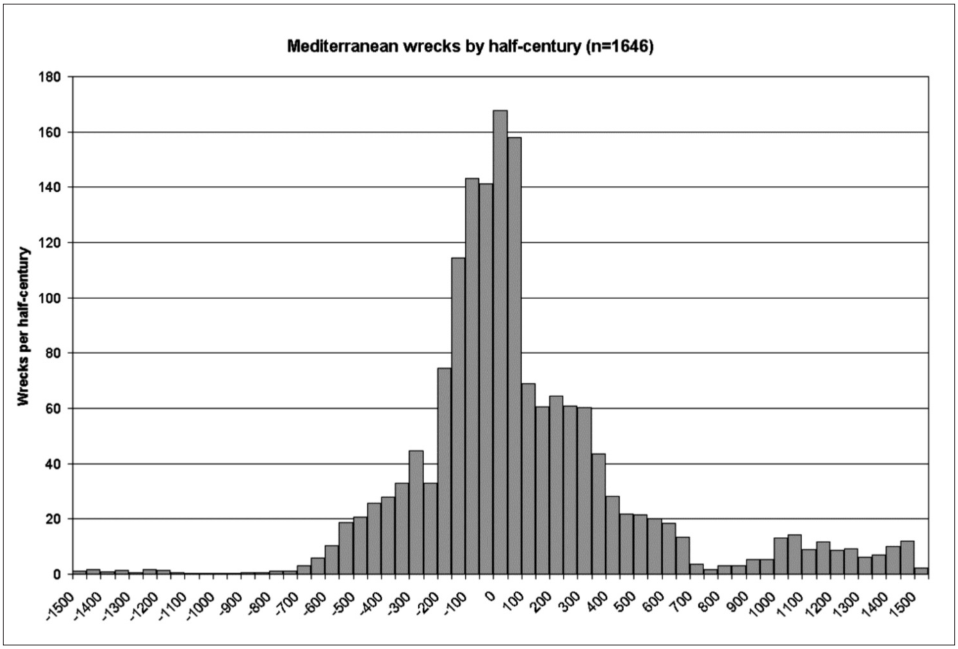

Fig. 5. - Chronological distribution of Mediterranean shipwrecks (n = 1,646) graphed by probability, by half-century. (Data collected by Julia Strauss).

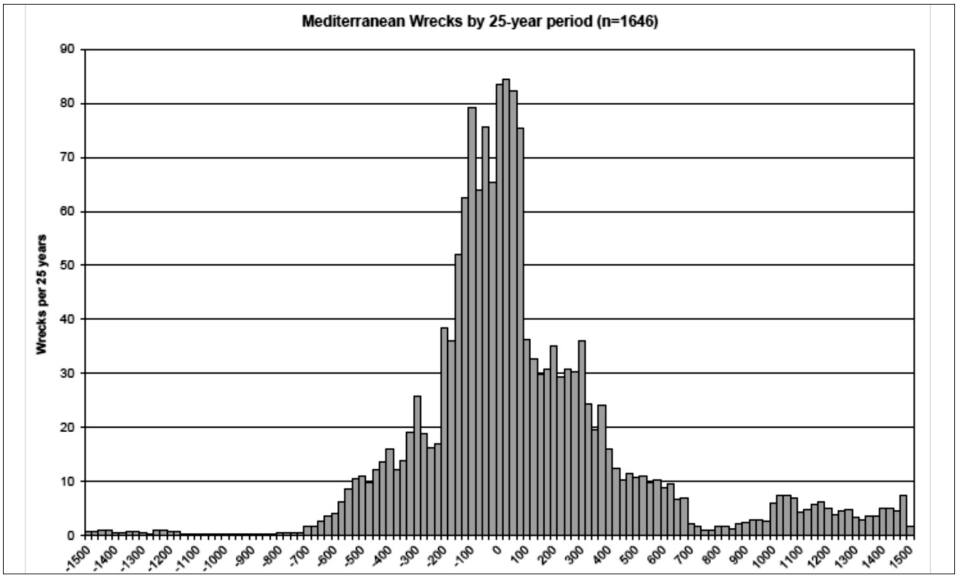

Fig. 6. - Chronological distribution of Mediterranean shipwrecks (n = 1,646) graphed by probability, by quarter-century. (Data collected by Julia Strauss).

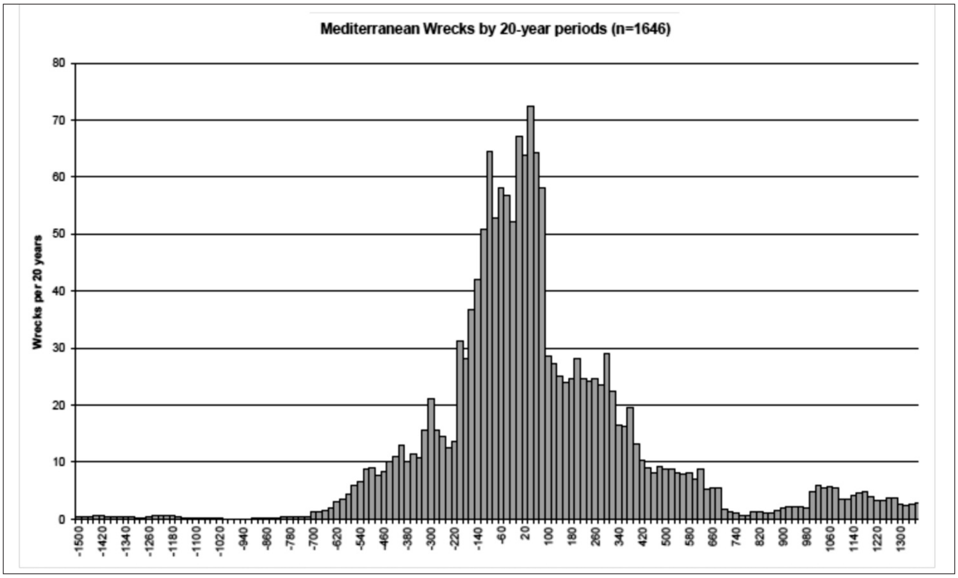

Fig. 7. - Chronological distribution of Mediterranean shipwrecks (n = 1,646) graphed by probability, by 20-year periods. (Data collected by Julia Strauss).

year periods. The overall patterns for the century and half-century data look very similar, but considerable differences in the timing and nature of peaks and some troughs when we slice the data by 25 or 20-year periods suggest that at this resolution we are pushing the limits of our data and generating a spurious impression of accuracy. It seems better to stick with 50-year periods or centuries, or to use only well-dated wrecks, whose range is no greater than the time periods by which the data are being grouped, thus wrecks dated within a 100-year range for a graph showing wrecks by century, and within a 50-year range for graphs showing wrecks by half-century.

In the quest to produce graphs capable of direct correlation with historical events, there is a temptation to produce a smoothed trendline through data points. Rather than break the data up into chronological blocks that may reflect something about the chronological uncertainty of the underlying data, a smoothed trendline appears to offer the possibility of seeing the situation in any year. This is almost always misleading.[16] An example is the graph of aggregate meat weight in Italy presented by Scheidel in 2009 – several features of the graph should ring alarm bells:[17] the standardised index; smoothed line; and the lines starting and ending in mid century, so we probably have only one data point per line per century if that, and I suspect it would be possible to generate this smoothed trend line without a data point at all in the fourth and second centuries BC. One should certainly not infer any chronological precision from this graph.

The choices made in the method of graphing imprecise chronological data (midpoints of ranges, or probability distribution), and of the manner of displaying them (histogram, bar chart, or smoothed line) have a significant effect on what inferences readers (and indeed the author of the chart) are likely to draw from such charts, and both authors and readers need to be aware of this. Failure to do so is likely to lead to false interpretations and conclusions. In 2009 Walter Scheidel made a bold attempt to do what we would all like to do – compare a series of four proxy indicators, plotted with a curved trendline standardised so that 1 on the Y-axis represented the highest point of each data series. He used this comparison to argue for a spurt of economic growth in the late Republic, followed by a period of stagnation, and argued that this pattern was due to Mediterranean unification and state formation under Rome. Unfortunately the attempt was flawed in several ways. Two of his four indicators were merely Parker's way and my way of graphing the same data on ancient shipwrecks, and cannot be read as independent data series that support each other.[18] Moreover, the shipwreck lines are interpolated through data points in each century, and would look rather different if one did this by 50-year or 25- year periods. Likewise, the chronology of a crucial part of the lead pollution line is interpolated from a mere two radiocarbon dates from a Spanish peat bog sampled in 5-cm units (and

[16] E.g. Prag 2002, 21-22; Scheidel 2009, fig. 1.
[17] Scheidel 2009, fig. 51.
[18] A point acknowledged by Scheidel (2009, 47 n.7).

which dates themselves are merely expressions of probability, crudely treated as midpoints of their possible error ranges); these smoothed trend-lines are entirely misleading. Beyond the issues of presentation, there are serious questions about what these various indicators – shipwrecks, animal bones, lead pollution as measured from a single peat bog – actually mean in terms of wider economic trends. [19]

I have mentioned above and discussed at length elsewhere the problems of intepreting the shipwreck graphs. Problems of interpretation also apply, in a slightly different way, to graphs showing quantities over time of archaeological finds, or dated wood remains, or animal bones, or aggregate meat weight extrapolated from animal bone data. [20] In so far as they attempt to give some quantifiable substance to Hopkins' impression that archaeological sites of the Roman period simply have far more "stuff" than preceding or following periods, [21] – an impression which I, and I think most if not all field archaeologists working on the period, strongly share – they represent a valiant attempt. But no archaeologist would dream of comparing absolute numbers of potsherds found from different sites or phases to express any meaningful economic trend, not least because of uncertainties over whether similar volumes of earth have been excavated in different sites, and the problem is exactly the same here. Although these proxies are *consistent with* the idea of both absolute and per capita economic growth under the Roman empire, uncertainty about their value means that these data series can hardly be used to *argue for* either form of growth.

2. *More promising proxies*

The biological standard of living

Human stature is a function of nutrition in childhood and adolescence and can thus, potentially, tell us quite a lot about economic wellbeing. The existing studies for antiquity, though, have come to opposed conclusions. Attempts to extrapolate stature from skeletal data may use one of several different formulae, which give different results and are not always directly comparable. Geerte Klein-Goldewijk's focus on femur length, as reported by Wim Jongman, elegantly sidesteps the problems with different formulae for extrapolating stature from skeletal data; in so far as femur length tends to bear a fairly consistent relationship to stature, by focusing on femur length they cut out some stages of uncertainty. [22] The problem with the published graph – which we should remember is explicitly work in progress at an early stage of the re-

[19] See Wilson 2009b for discussion.
[20] Jongman 2007a, 190-2, Graphs 3-6; Scheidel 2009, 51 fig. 3.
[21] Hopkins 1978, 71.
[22] Jongman 2007a, 193-5, Graph 7.

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search –, is that the overall sample size was still relatively small for the period concerned – 1,000 bones over a millennium – and there is no regional disaggregation; Klein-Goldewijk's study will, it seems, eventually include over 10,000 bones.[23]

Nicola Köpke and Jorg Baten's study of mean body height, by contrast, has a larger sample size and does present regionally disaggregated data, which show strong differences between the Mediterranean and North-West Europe; but it has been criticised for sample aggregation in different ways.[24] The two studies come to diametrically opposed conclusions that underscore the need for further data collection and regionally disaggregated studies, preferably using femur length as a common denominator. Essentially, while Jongman and Klein-Goldewijk's graph emphasises a rise in stature in the Roman period and a drastic drop in late antiquity, the sharp rise in the first century AD may be due to the incorporation of Northern European and especially British datasets in this period, previously excluded as prior to this period these areas were not "Roman". In these northern areas there was a much more pastoral and less urbanised economy, and we may expect the concomitantly greater contribution of milk and meat protein to diet, resulting in taller people. This suspicion is strongly borne out by Köpke and Baten's regional disaggregation. Indeed, this point highlights a major methodological weakness in the attempts to use stature to track economic performance. Pastoral groups like the Masai are extremely tall, for these reasons of milk and meat consumption; but that does not reflect the complexity of their economy or GDP per capita in a straightforward relationship to an urbanised industrialised economy. Stature in pre-industrial societies may be strongly determined by Malthusian pressures, and is likely to be inversely related to urbanisation which, on another view, is a proxy measure of per capita performance, since a higher per capita output by agricultural workers is required to support a greater share of the population living in cities and not engaged in primary food production.[25] Studies of stature, and the biological standard of living, are indeed useful in helping us understand important aspects of the economy of past societies, but we should not expect them to bear a straightforward relationship with, e.g. per capita surplus and economic growth, and they need to be assessed against other variables such as urbanisation and population pressure in order properly to understand what they do tell us.[26]

Lead and copper pollution records

In the mid 1990s a series of studies on lead and copper in Greenland ice caused a stir by demonstrating levels of lead and copper pollution in the Roman pe-

[23] Scheidel 2012a, 325.
[24] Koepke and Baten 2005. Critique: Jongman 2007a.
[25] Lo Cascio 2009; Wilson 2011a, despite Morley 2011.
[26] Cf. Scheidel 2012a.

riod that exceeded anything till the Industrial Revolution, suggesting that the scale of lead/silver mining and copper mining and smelting from which the pollution derived was massive, and polluted the atmosphere on perhaps a hemisphere-wide scale. Versions of the graph of lead pollution in Greenland ice cores, first published by Hong *et al.*, have been used by de Callataÿ, Jongman and myself, to draw attention to these apparent peaks in smelting, and thus mining, activity in the Roman period; moreover, there is likely to a be a link with silver production as much silver occurs combined with lead and the desilvering process. Graphs of lead pollution (and also copper pollution) should therefore be informative about trends in mining activity and metal production, and, at one greater remove, coin production. However, these were based on a handful of samples over 1500 years, from a single core in Greenland, and the paucity of data points (just 22 between 1000 BC and AD 1500) gives a very spiky result, meaning we cannot draw detailed historical conclusions from the data. Moreover, we cannot fully exclude the possibility of some short-term spikes from other causes (volcanic eruptions or asteroid impact). There is some uncertainty also as to what extent the Greenland pollution represents homogenised global levels, or is strongly influenced by atmospheric pollution closer to Greenland, notably from Europe.

This is about to change. Ice core studies have developed phenomenally over the last 20 years and are now making real contributions to the understanding of past climate and atmospheric pollution, enriching understanding of the past and helping to inform debate on contemporary climate change by providing a long-term perspective (how much variation in the past might have been natural, and how much anthropogenic?). Current projects, not yet published, offer the prospect in the near future of long-term lead pollution data-series at the annual and even sub-annual level, from ice cores from various points in the Arctic circle and from a glacier at Colle Gnifetti on the Swiss-Italian border near Zermatt. [27] Not only should the resolution allow us to exclude one-off volcanic events, and to explore possible correlations with known historical activity, but the variety of spatial sampling should allow us to approach the question of whether there is strong variation between ice cores in different regions, thus potentially allowing discrimination between pollution caused by the Roman empire and Han dynasty China, or whether they are capturing a wider global atmospheric signal.

Fish salting vats

So far, I have mainly been criticising other people's use of proxies. It thus seems only fair to present two of my own that are also open to criticism, this time for small sample sizes.

[27] Colle Gnifetti: http://projects.iq.harvard.edu/shp/blog/press-release-new-ice-core-drilled-swiss-alps ; http://climatechange.umaine.edu/colle_gnifetti_2013_.

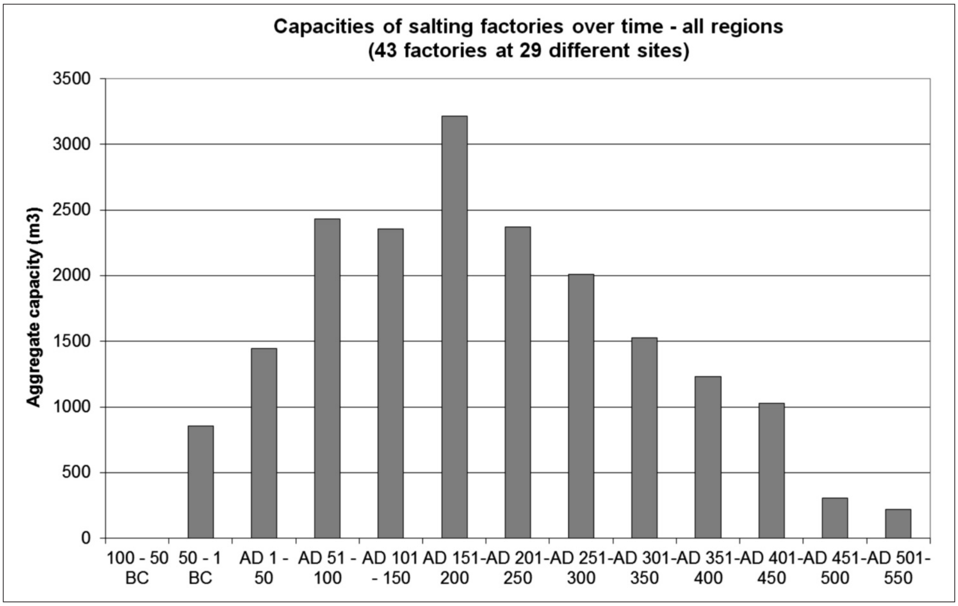

Fig. 8. - Aggregated capacities of Roman fish-salting factories – all regions.

In 2006 I published a graph showing the aggregated vat capacity of 34 fish-salting factories from 26 different sites, based on publications which reported both the vat capacity and the dating of construction and abandonment. [28] The potential attraction of this proxy is that it would seem to provide an approximation for trends in the capacity of the fish-salting industry, although one should note some caveats – the possibility that not all the capacity of a factory was simultaneously used each year, the likelihood of seasonal variation, and the fact that some salting vats are known to have been used for salting joints of meat (outside the fish-salting season, in addition to or instead of salting fish?). [29] The sample was not particularly large, [30] limited as it was by the availability of published information that combined both statements of vat capacity and dating. Even for sites with reported data, estimates are sometimes minima because of incomplete excavation or the partial destruction of some vats. Nevetheless, comparison of trends across time is possible. Since 2006, some more data have become available, although it is still striking how rarely publications of fish-salting sites report vat sizes and thus factory capacity, or relate such information clearly to the explicit dating of phasings. [31] It is now possible to present updated graphs, showing aggregated vat capacities over time for 43

[28] Wilson 2006.
[29] See Wilson 2006, 536-7.
[30] As acknowledged at the time (Wilson 2006, 536).
[31] Important new data in various papers in Lagóstena, Bernal, and Arévalo 2007.

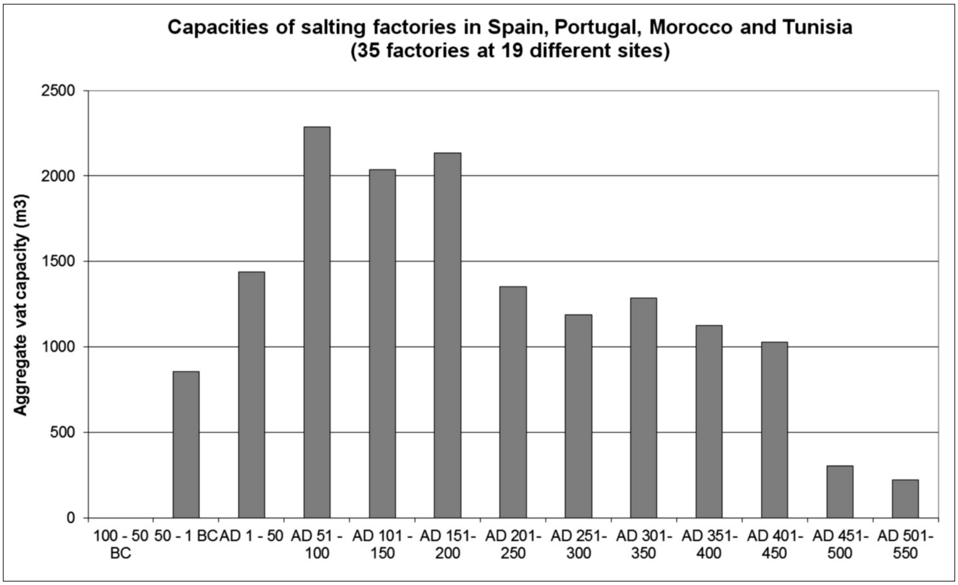

Fig. 9. - Aggregated capacities of Roman fish-salting factories in Lusitania, Baetica, Mauretania Tingitana and Africa Proconsularis.

factories at 29 different sites across the empire (fig. 8), including (fig. 9) 35 factories from 26 different sites in North Africa and the southern Iberian peninsula. The overall data show a steady increase in total capacity from the later first century BC to the end of the first century AD, a levelling-off in the first half of the second century AD and then a rise to an absolute peak in the second half of the second century. There is then a steady reduction in overall capacity until the mid fifth century, followed by a rather sharper-drop-off thereafter.

This overall trend is however principally the result of the superimposition in a single graph of two different regional trends. Fish-salting factories of the Mediterranean and Atlantic coasts of the Iberian peninsula and North Africa (fig. 9) show a rise to a peak in the first century AD, followed by a slight drop in the early second, and then a sharper drop in the early third century, after which things stay broadly stable with a slight declining trend until the mid-fifth century, and then reduce sharply again in the later sixth. In Brittany, by contrast (fig. 10), construction of fish-salting factories starts later, in the second half of the first century AD, and the second half of the second century sees a massive expansion in total capacity (with the construction of the largest single fish-salting factory yet published, at Plomarc'h in the bay of Douarnenez). Many of these factories remained in operation through the third century, although with some decline in capacity in the second half, followed by a sharp drop in the fourth, tailing off to nothing in the fifth.

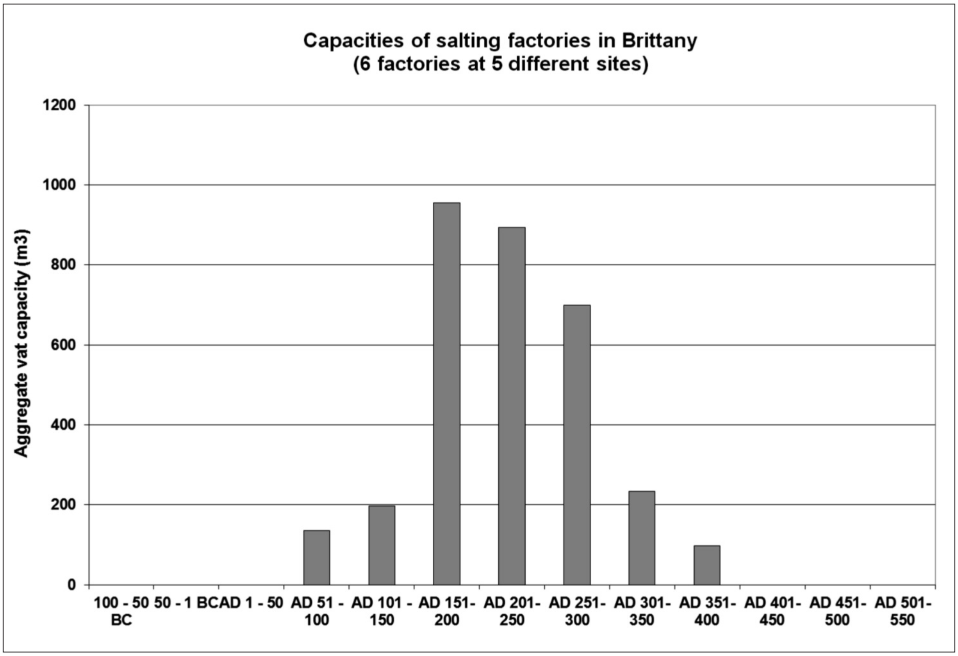

Fig. 10. - Aggregated capacities of Roman fish-salting factories in Armorica. (Wilson 2006, fig. 4).

Given the relatively limited sample sizes, it would be unwise to try to push the interpretation of the graphs too far by analysing the finer detail – the variations between AD 51 and 200 in the Iberia/North Africa graph are relatively small and might be evened out or reversed by the publication of another factory or two. But the larger trends are likely to be robust: the fish-salting vat capacity data show a boom in investment in fish-salting factories in the Mediterranean in the first century AD, and a peak in the total number and volume of factories in use in the mid-second century. Mediterranean techniques for fish-salting and their large-scale application in veritable factories were transferred to Brittany in the late first century AD, and especially in the later second through third centuries. But the really significant point is this: before the first century BC and after the sixth century AD people made salted fish products on a relatively small scale, using dolia, pithoi or other large jars. During the Roman period this small-scale production of salted fish produce continued, but it was only between the first century BC and the fifth century AD that production was on such a large scale to require the establishment of built infrastructure – factories with batteries of large concrete vats – for this purpose. Irrespective of arguments about the precise timing and scale of changes in vat capacity, and of the size of the sample available to us to track this, this fundamental point remains unchanged. The Roman Mediterranean saw production and trade of salt-fish products on a scale unmatched until the early modern period or even perhaps the nineteenth century.

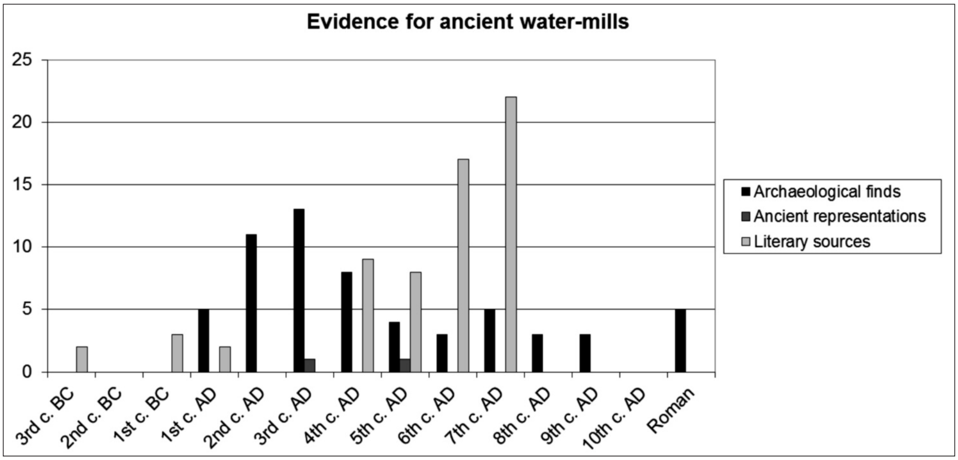

Fig. 11. - Evidence for ancient water-mills.

Evidence for ancient water-mills

Fig. 11 plots the archaeological evidence of the physical remains of ancient water-mills over time against the evidence for their representation in art, and their mentions in written sources. [32] This exercise, as Walter Scheidel has pointed out, again suffers from what he understandably calls "feeble" sample size; [33] but it makes the important methodological point that we do need to compare written and archaeological data. If much of the foregoing discussion has sounded like a litany of complaints about how difficult it is to use the archaeological data, that is not meant to discourage us from using archaeological proxies, or to say that they are useless and we cannot do anything with them. Rather, it is intended to stress that it is time quantification studies lost their innocence and faced up to the very real difficulties and complexities of dealing with proxy data whose relationship to what we would really like to know is imperfect, and which frequently suffers from a set of biases in preservation, selection, collection and often presentation. In the case of the water-mill, comparison of the archaeological indicators and the documentary indicators is instructive. There is a clear mismatch between them. Why?

Scholars such as Marc Bloch had argued that although the water-mill was known in classical antiquity, it did not become widespread until the early middle ages, a view based on the numbers of references to water-mills in texts of different periods. [34] In 1984, however, Örjan Wikander pointed out that the chronological di-

[32] Wilson 2007; Bowman and Wilson 2009a, 33-36.
[33] Scheidel 2009, 56 and n. 43.
[34] Bloch 1935; Finley 1959; 1965; White 1980.

stribution of references to ancient water-mills was entirely a function of literary genres. [35] The increase in mentions of water-mills in the fourth and fifth centuries AD was the result of the introduction of new genres of documents – hagiography, lawcodes, and monastic charters – all of which were more likely to mention such everyday items than were epic poetry or grand narrative history. By contrast, the largest numbers of archaeologically attested water-mills are from the second and third centuries AD, when no surviving literary sources refer to water-mills at all, reflecting the fact that few texts overall have survived from this period.

It is clear that the documentary sources alone do not give a good picture of the use of water-power over time; and moreover, that because the very large numbers of documentary references to water-mills in the early medieval period are not matched by a concomitantly high number of archaeological finds, the archaeological evidence should represent only a small proportion of the original total of ancient mills. [36] The physical evidence may however provide the best available means of assessing the relative use of water power over time, provided that there is a roughly equal chance of survival for the archaeological evidence for water-mills from different periods. Water-mills are especially vulnerable to erosion by changing water-courses, reducing the chances of survival – or recognition – of older sites. The survival of Roman sites may therefore have a greater relative value than the survival of medieval sites. Construction materials may also have a bearing on survival or identification rates – has the the Roman propensity for brick or stone construction resulted in a correspondingly greater identification of Roman sites than wooden early medieval ones? But numerous Roman water-mills in northern Europe are also made of wood, and in any case, mills may also be identified on the basis of finds of mill-stones. On balance, it seems unlikely that Roman mills are overrepresented in the archaeological record, and the peak in the Roman period should therefore be considered real. Indeed, it is actually more pronounced than fig. 11 at first sight suggests, as the six sites in the right-hand column, dated simply to the 'Roman' period, should be distributed across the columns for the first–fourth centuries AD. If they were distributed evenly, they would further accentuate the Roman-period peak, but all the more so for the second and third centuries if they were distributed in proportion to the pattern already suggested by the graph, in a near-normal distribution.

A similar comparison of the archaeological and papyrological evidence for different kinds of ancient water-lifting devices raises similar issues and highlights the need to compare archaeological and documentary sources where at all possible. [37]

[35] Wikander 1984.
[36] The following dicussion first appeared in Wilson 2007a.
[37] Malouta and Wilson 2013.

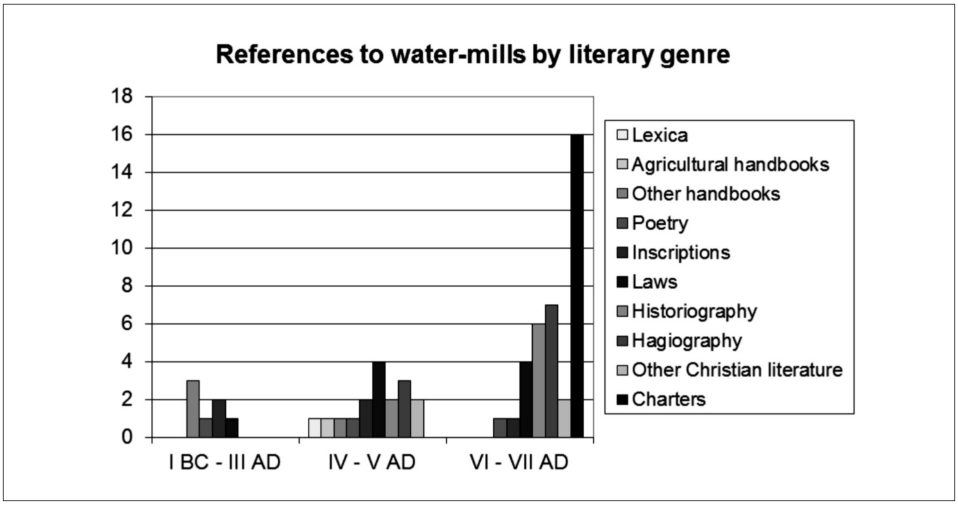

Fig. 12. - Water-mills by literary genre.

3. *Potential for future work*

The examples just discussed, of fish-salting vats and water-mills, illustrate some of the potential for the use of proxies, both to identify trends but also to make us think about various biases inherent in the data were are using, and how we might overcome them. The relatively small sample sizes should make us somewhat cautious in the conclusions we can draw, but this is a limitation that could be overcome by future work – and part of the reason for discussing them here is to call attention to the need for excavators of such sites to publish them, and in the case of fish-salting installations, to publish explicit statements on vat capacities, and the dating of construction and abandonment of individual vats where possible, to enable trends to be tracked in greater detail in the future.

Another very promising source of proxy data is inscriptions. Well over 450,000 Latin inscriptions are known from the Roman world, published partly in a monumental project started in the 19th century, the *Corpus Inscriptionum Latinarum*, partly in regional compendia, and partly in annual updates in *L'année épigraphique.* Several different projects have begun to put these datasets online, but while there is now a vast amount of data available, most of these databases are currently not well organised for answering big historical questions. Ultimately, most of these projects appear to have been designed by epigraphers primarily to facilitate the location of a particular text, or to search for words or phrases, rather than to extract larger phenomena from the data. The widest coverage of Latin inscriptions is to be found in the Epigraphik Datenbank Clauss-Slaby, which as of July 2014 contains over 463,000 inscriptions, a truly enormous quantity, but the data are largely unstructured; one can query by words or place, but not by inscription type or date.[38] This means that one cannot track any data series over time without a lot of time-consuming manual intervention and reordering of the data. The Epigraphische Datenbank Heidelberg (*EDH*) is better organised for such purposes. As of July 2014, it contains over 67,000 inscriptions entered,[39] as highly structured data which can be queried on a variety of criteria, including inscription type, and can return numerical date information. The disadvantages are that regional coverage is patchy: good for the Balkans, as the regional corpora have been fully entered for the provinces of Achaia, Dacia, Dalmatia, Epirus, Macedonia, Moesia Inferior, Moesia Superior, Thracia; fairly good for Spain; patchy for Italy; and very poor for Africa, Asia and Gaul.

Greek inscriptions are at the moment poorly served by online search tools. The Packhard Humanities Institute (PHI) Greek Inscriptions website allows access to the digitised texts of inscriptions from numerous corpora, but its search interface is again limited to searching for words or phrases.[40] The Trismegistos website offers a search capability that includes cross-platform searching of several epigraphic and papyrological collections on several different criteria.[41] Currently, however, it does not allow searching by type of text, since that field is either missing or entries are so poorly standardised in the underlying databases that results would be meaningless.

Nevertheless, the potential here is enormous, for analysing trends in the construction and dedication of public buildings, or indeed other phenomena. Here we encounter the vexed question of what Ramsay MacMullen has called "the epigraphic habit", which he argued, rather vaguely, was formed by a collocation of social attitudes and not easily related to political or economic events.[42] His article made forays into quantified analysis but his main samples were localised (Rome, and seven cities in North Africa), and his rejection of political and economic factors was premature and probably misguided, although it is certainly true that there are also other factors at work in determining the decision to inscribe on stone.[43] Nevertheless, I would argue that one can get some distance with at least the building inscriptions. The question is how far do these inscriptions reflect the wider picture of public building activity, and how far do they just reflect the propensity to in-

[38] http://www.manfredclauss.de/.

[39] http://edh-www.adw.uni-heidelberg.de/home.

[40] http://epigraphy.packhum.org/inscriptions/main.

[41] http://www.trismegistos.org/.

[42] MacMullen 1982, 245.

[43] Cf. Meyer 1990, for a not wholly convincing argument that epigraphic commemoration declined after Caracalla's grant of citizenship in AD 212 levelled status distinctions between people; contrast Smith 2008.

scribe donations or dedications of building activity? I think it is certainly fair to say that a purely mechanistic reading of the epigraphic data would understate the degree of public building activity in e.g. the Roman Republic, before the habit of widespread commemoration in inscriptions really takes off. Particularly from the Augustan period onward there is a much greater tendency to inscribe building dedications, and for the next few centuries we have entered a period where local political competition between elites generates the impetus for this epigraphic practice. Within the Roman imperial period therefore things may be broadly comparable across chronological divisions. Undoubtedly the extraction and analysis of sets of inscriptions even from the better structured databases such as *EDH* is not just a matter of a computer algorithm, and will involve substantial human intervention in making decisions on how to classify, date and interpret building or honorific inscriptions, but the exercise would certainly be worth the effort – especially if the data were disaggregated both by region and by funding source (imperial, municipal, private).

Conclusion

The foregoing is not meant to sound as negative as perhaps it does. It is intended to warn against the naïve use of quantification, e.g. the simplistic assumption that graphs of shipwrecks bear a straightforward relationship to the volume of maritime trade, or that certain categories of evidence have necessarily had the same chances of survival across time. Nonetheless I do believe that quantification offers a very productive and informative way forward for the economic history of the ancient world, so long as care is taken when thinking about *what* to quantify, and how to do it. And, importantly, those whose work involves the generation of data about the ancient world – archaeologists and epigraphers especially – need to be alive to the potential of large quantification studies to address big historical questions, and to ensure that their own data are published in a way that will enable their incorporation within such studies.

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