

Ancient Economies: The Challenge of Mapping Complexity



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The Present-Day Challenge¹

The ‘challenge’ identified in my title can be simply stated. Those of us whose professional training and research work have focused on the Greco-Roman Mediterranean now have a substantial body of information about the economic activities of the entire pre-Islamic Old World. That information comes from a huge range of literary, epigraphical, papyrological, and archaeological sources in a variety of ancient and modern languages. It is continually being enlarged and updated, and has been used by an international range of scholars for over a century, indeed with ever-increasing intensity and sophistication since the 1950s. Its potential, whether as a database or as a display case, of economic activity within societies is limitless,² and three recent developments make this a good moment to review where we are.

The first is a recognition of the complexity presented by those data. It has become clear from a host of publications that thematically simple portrayals of the economies of that Old World, such as Finley’s influential sketch (Finley 1973) or the more recent preoccupation with detecting aggregate or per capita ‘growth’ (real though it was),³

¹ It is a privilege to have been invited to participate in this Congress, and I thank the organisers for the compliment and for the platform that they have thereby given me. I do so because this Congress provides a valuable opportunity for those of us who are economic historians of antiquity to take stock of where we are, of where we hope to be going, and of how well equipped we are to get there. I thank Zosia Archibald (Liverpool) and David Lewis (Edinburgh) most warmly for encouragement and for specific improvements to the text, and Walter Scheidel (Stanford) for information about reviews of Scheidel et al. (2007). I also thank two anonymous referees for their very helpful comments and suggestions, which I have incorporated as much as space allowed.

² Though the entry by S.L. Engerman for ‘Economic history’ in Kuper and Kuper (2004, 271–274), shows no awareness of that potential.

³ Imprimis Morris (2004), with Bresson (2016, 203–206).

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cannot serve as more than single components of a far more complex portrayal of the economic activities of the world that we study. It was symptomatic that the manifesto for a major conference in 2015 which focused on the Roman economy saw it as a ‘complex adaptive system, consisting of independent agents that interact {with} each other by constantly adapting themselves’.⁴ Such terminology reflects the world of mathematicised applications which the study of complexity has acquired in relation to chaos theory and many-body systems (e.g. in Lewin 1993 or Auyang 1998, or in the work of the Santa Fé Institute), but the challenge remains of linking such applications meaningfully to the data that we have for antiquity.

The second development was the publication of the *Cambridge Economic History of the Greco-Roman World* (Scheidel et al. 2007). A contributor to that volume such as myself should offer comment only with great circumspection and with full and deserved respect for editors and fellow contributors, but the importance of the volume is such that some observations must be made, and enough time has elapsed since its planning and publication for distance to permit some objectivity. One begins inescapably by comparing it with its predecessors in the writing of ancient economic history as a large-range art form, from Rostovtzeff’s two great pioneering compilations (Rostovtzeff 1926, 1941) through Heichelheim (1938) to Vittinghoff (1990). Such comparisons reveal the scale of change, whether in the use of non-written evidence, in the use of intellectual templates derived from economics, or in the degree of emancipation from politically defined spatio-temporal frameworks. True, some critical remarks will follow later in this essay, for the volume remained hybrid, part antiquarian and part social-scientific, but it and its more substantial reviews (especially Bang 2009; Étienne et al. 2011) have provided scholars of the specialism with an invaluable point of reference, both as a systematic assemblage of material and as a shop-window of newer approaches. One awaits with great interest the publication of its competitor, the *Oxford Handbook* (Bresson et al. forthcoming), which on the evidence of Bresson (2016) is likely to take the transformation further. Indeed, it is already being taken further for the Roman world in the various detailed thematic volumes that are now appearing (13 so far) in the ambitious series *Oxford Studies in the Roman economy*, for which Bowman and Wilson (2009) provides a programmatic lead.

Moreover, the *Cambridge Economic History* is now over ten years old. That is not normally a long interval within the discipline of Classical Studies, but in this particular area of scholarship life has moved on with disconcerting speed to generate a third, more radical development. This is the disposition to reach out from economics on its own in order to embrace the analytical terminologies that have been developed by the other social sciences and to employ as many of them as can be used to good effect. The main multi-purpose tool here is now ‘Model’, the adoption of which within the specialism may derive from its use by the geographers⁵ and was already explicit in the title of a conference held at Stanford in 1998, the papers delivered at which were published as Manning and Morris (2005). ‘Model’ has

⁴ Cf. Poblome and Verboven (n.d.).

⁵ Finley (1985, 182–183), citing Chorley and Haggett (1967); add Gregory et al. (2009) s.v. model.

been followed by ‘Agency’, ‘Connectivity’, ‘Game Theory’, ‘Identity’, ‘Mobility’, ‘Network’, ‘Rationality’, and the hybrid ‘Actor-Network Theory’. The most recent addition to the toolbox has been ‘Behaviour’ (cf. Lewis 2018), appropriately so in the light of the recent award of a Nobel Prize in Economics to the High Priest of behavioural economics, Richard Thaler, and of his entertaining exposition of the specialism (Thaler 2015). There remains, inevitably, the last-ditch expedient that is used perforce by all pre-modern economic historians, ‘Proxy Data’, which I have discussed briefly elsewhere (Davies 2018a, 570–571). In the near-total absence from classical antiquity of diachronic datasets that were systematically compiled for communal purposes,⁶ the term reflects the laborious compilation, from the casual survival of physical and written evidence of every kind, of lists of economically pertinent phenomena (sites, objects, commodities, occupations, prices, wages, and so on), their purpose being to shed an indirect but helpful light on an economy, to begin to quantify its activities (detailed initial survey in Bowman and Wilson 2009), and even to detect growth and to calculate a GDP. The evidence of silver production through the centuries as reflected by lead isotope levels from Greenland ice cores⁷ is the classic example of such indirect light: as such evidence expands and profiles of activity become firmer, the sense of building on sand is receding:

All these tools were employed in one or other of the papers that were presented in 2016 (now published as Canevaro et al. 2018) at an Edinburgh conference on the theme ‘Ancient Greek History and Contemporary Social Science’, to which I had the dangerous honour of giving the final address. On that occasion they gave me food for urgent thought, which is set out in Davies (2018a), and they do so again now, because it seems to me that we—the miniscule and under-equipped band of economic historians of the pre-Islamic Old World—now need to make some major decisions with long-term implications. I have space here to sketch only three areas of decision, each exemplifying the complexity that my title reflects.⁸

Area of Decision I: Boundaries in Time and Space

The first is the question of boundaries: how do we define the tracts of time and space within which the activities that are the business of an economic historian of the pre-Islamic Old World take place? The question is real, because at least two distinct answers have been presented, and they reflect an intrinsic tension. When after the First World War historians of Antiquity began to address the challenge of

⁶ The surviving census figures of the Roman Republic (Brunt 1971) come closest. The price-lists of six commodities (barley, dates, mustard, cress, sesame, and wool) that were compiled for Babylonia (with many gaps) from 652 to 60 BCE (Sachs & Hunger 1988), were not used to our knowledge for communal purposes, and the extensive data known from mid-Hellenistic Delos are the fruit of modern compilation (Reger 1994).

⁷ Up-to-date report in McConnell et al. (2019).

⁸ Since this paper is written by a specialist in the economic history of ancient Greece, its citations reflect that bias, but its argument is intended to apply across the pre-Islamic Old World.

economic history as a specific sub-genre, their understandable prime instinct was to follow the pattern set by economists from Smith to Weber and to write in terms of *Nationalökonomie*, i.e. as the study of the ways in which the economic activities of a given polity formed a system that was (or might be) influenced for better or worse by public or royal policy and by public or private institutions. Though that instinct has generated a very wide range of studies, whether of an individual polity,⁹ of a closely linked group of polities,¹⁰ or of sprawling empires, they have shared an underlying assumption that it was legitimate to transmit to the reader an impression of at least a superficial homogeneity—a claim that was strengthened by the degree of complete or prominent overlap that was visible between the circumscriptions and control systems of the polities concerned and the languages in which the dominant genres of primary source material were written. Significantly, though the *Cambridge Economic History of the Greco-Roman World* did its best to distance itself from such polity-based presentations, its very title reflected a conception that was ultimately based on language, and some of the omissions which one of its reviewers in *Topoi* noted (Zurbach 2011)—Tyre and the Phoenician diaspora, the Black Sea, temperate Europe, and Mesopotamia—may also have reflected a subconscious language-based sense of otherness: as a contributor I acknowledge my own complicity.

A second, very different answer took longer to emerge. In essence it looked behind the procedures and pressures that were characteristic of a *Nationalökonomie* towards what was economically and humanly primary, namely the patterns of detectable demand and supply, and therefore also of the activities and movements that they generated. True, such activities might be local or regional, and might therefore be more or less compatible with the descriptive framework that was appropriate for *Nationalökonomie*. However, other activities generated by the processes of satisfying demand might involve triangular trade, or managed exchanges, or the use of force, or—and especially—long-distance transits that crossed linguistic, political, and cultural boundaries.¹¹ In such cases, the historian would be observing or detecting movements (of persons, goods, or services) that ranged far beyond the reach or control of any *Nationalökonomie* and were therefore spatially unbounded and independent of political or fiscal constraints. They might also very well be conducted in whole or in part in languages and within cultures that are not normally accessible to historians whose approach to antiquity lay through Latin and Greek,¹² but they would still

⁹ E.g. of Corinth (Salmon 1984) or Ephesos (Davies 2011).

¹⁰ E.g. of Israel-Judah (Silver 1983), of Delos and the Cyclades Islands (Rauh 1993; Reger 1994; Constantakopoulou 2007; Chankowski, 2008; Constantakopoulou 2017), or of Greek city-states in general (most recently Migeotte 2014 and Bresson 2016).

¹¹ The five studies of ‘Trade beyond the frontiers’ now published in Wilson and Bowman (2018), 443–624 are welcome illustrations, though they unfortunately lack a complementary study of trade across the Rhine-Danube frontier.

¹² For example, I am very conscious that my study of the flows of myrrh and frankincense from Yemen to the Aegean (Davies 2016a) lacked direct access to Aramaic or to the South Arabian languages.

comprise elements of a single complex but very loose web of interaction that might extend 1000 km or more beyond ‘the Greco-Roman world’.¹³

Such traffics require a wholly different genre of description and analysis. They are not adequately reflected by tracing the flows of a single commodity from the place(s) of extraction or production or capture,¹⁴ since their role in that complex web cannot then be fully evaluated. Nor will analysis in terms of institutions be sufficient, for reasons that are spelled out more fully below. Indeed, it is increasingly apparent that the challenge requires the creation of a genre that is capacious enough to encompass the entire range of activities described in the previous paragraph. It has taken several generations to crystallise, for though Rostovtzeff’s study of the Hellenistic world deserves our unqualified respect as a pioneering enterprise, even he found it impossible fully to emancipate his survey from *Nationalökonomie*.¹⁵ Indeed, it is only in the present century that examples of what is now called ‘big history’—heavy-weight volumes which cover large-scale themes in full scholarly detail—have shown what can be done. The books of Michael Jursa and his colleagues (Jursa et al. 2010) and of Anthony (2007), Broodbank (2013), and Cunliffe (2015) make an unassailable case for annihilating the language-based academic boundaries between ‘Archaeology’, ‘Classical Antiquity’, and the ‘Ancient Near East’ (the latter boundary being still exemplified, alas, in the structure of the conference reflected by the present volume), and instead for seeing such flows of goods and services and the institutions which they generate as the basic formative ingredients of a far larger zone of interaction. They thus incorporate but transcend the boundaries of the debate about ‘Mediterraneanism’ that has been rebooted post-Braudel to such effect in the last twenty years by Horden and Purcell (2000), Harris (2005), and Abulafia (2011). They also transcend the sense of familiarity that can be provided by the language of written sources: it cannot be a chance that the books of Anthony, Broodbank, and Cunliffe were written not by historians but by archaeologists who were less constrained by the loom of language.

How large is that zone of interaction? For brevity’s sake I have to state my own view, not argue it: namely, that if it is to embrace the long-distance movement of materials and peoples and technologies that came to transform that world into a single loosely interlocking system, it has **geographically** to embrace a huge tract of AfroEurAsia, a region bounded effectively by the Arctic and the Atlantic to West and North, less effectively to the South by the Sahara and less effectively still to the East by the Russian steppes and the deserts of Iran: and **chronologically** it has to extend at a minimum from the Phoenician-Greek expansion of the Early Iron Age to the

¹³ An extreme exemplar, from a recently excavated grave of the C2-C3 CE, is on display in the Museo Nazionale of Palestrina, in the form of a woman’s diadem with sapphires that may have come from Cambodia or Thailand.

¹⁴ *Imprimis* the distribution-patterns of this or that genre of ceramic products.

¹⁵ My own experience of close engagement with his text a generation ago (Davies 1984), in attempting to characterise the economic behaviour of a multi-lingual, multi-cultural, multi-polity but heavily interactive Hellenistic world, opened my eyes to the importance of the task but also to its extreme procedural intractability.

partial caesura after the Islamic Conquest.¹⁶ That is a formidable prospect. We are all accustomed to thinking locally or regionally, in terms of the ‘Athenian economy’ (e.g. French 1964), the ‘Seleukid royal economy’ (e.g. Aperghis 2004), or the ‘economy of Roman Britain’ (Fulford 1989), and of course practicality will dictate that much work on ancient economies will continue to be carried on in similar local or regional terms. All that it is proper to advocate is that such work bears in mind the continual movements of human beings, animals, materials, ideas, and technologies that can be traced across the entire zone, movements that transgressed all political and physical boundaries and could be controlled by polities only to a very limited degree. That has implications for terminology, since that macro-region of AfroEurAsia never became a single fiscal regime even in the palmy days of the Roman Empire. Even in terms of Nationalökonomie, therefore, the use of the plural ‘economies’ is mandatory—a use that also offers the opportunity to envisage a far more realistic and complex map of shared, geographically overlapping, and weakly interacting fiscal and real economies in continuous slow evolution. It was therefore a pleasure to note the title of Bonn University’s Research Training Group 1878: ‘Archaeology of Pre-modern economies’, and to see it reflected in the theme ‘Archaeology and Economy in the ancient World’ that was enjoined for the XIX Congress of Classical Archaeology in Köln-Bonn in May 2018.

Area of Decision II: Our Relationship with the Social Sciences

The second area of decision concerns our relationship with the social sciences, especially but by no means only with economics. The complexity of that relationship is best sketched by recognising the range of descriptive-analytical modes that are already in use among the economic historians of antiquity, varying not so much by subject-matter as by the level of abstraction. Our normal and predominant mode, for example, is the creation of an array of studies that describe specific areas or aspects of the economic activities of a region, small or large, or focus on a specific institution or aspect of supply. We may call this a ‘first-level’ analysis, because the primary aim of such studies is to collect and organise information while also incorporating a theoretical baseline. Such baselines are, for example, the distribution of a commodity from its origin to its end users, such as salt (e.g. Carusi 2016) or aromatics (e.g. Davies 2016a); or the sources of supply to a single major entrepot, such as the slave trade in the Aegean (Lewis 2016) or the many studies of the grain supply to Athens (Gernet 1909; Moreno 2007; Oliver 2007) or Rome (Rickman 1971, 1980; Garnsey 1988); or a specific corridor and its traffics, such as D’Ercole’s study of the Adriatic (D’Ercole 2002) or Laetitia Graslin-Thomé’s book on the trade-routes across Northern Syria

¹⁶ I do not enter here into the post-Pirenne debate, but that too has now generated two exemplary works of ‘big history’, namely McCormick (2001) and Wickham (2005).

(Graslin-Thomé 2009), not to mention Silk Road studies¹⁷: or the edition of a relevant text or group of documents, such as Simon Swain's edition of Bryson's treatise on the household (Swain 2013), or the work of Clarysse and Thompson (2006) on the salt-tax records in the Ptolemaic Fayum: or persons with economic roles, such as bankers or business managers¹⁸: and of course the economies of specific places,¹⁹ specific polities,²⁰ or specific regions,²¹ and so on.²²

This 'normal and predominant' mode has two limitations. The first is fragmentation, insofar as a focus on one commodity or one institution diverts effort from comprehending the ensemble of interaction. The second is that of specificity itself. That is because, in the absence of widely accepted criteria of assessment, the mode does not lend itself easily to the making of comparisons—a challenge the dimensions of which are well illustrated by the two extended attempts of recent years to compare city-state systems (Griffeth and Thomas 1981; Hansen 2000 with Hansen 2002). There is therefore a case for adopting a second mode, that of engaging not merely with Economics but rather with the Social Sciences in general.

It is not a step to be taken lightly. For most historians of antiquity the narrative-descriptive mode is their comfort zone, not the world of models and Gini coefficients and strategic interactions, still less the world of hypotheses that are stated in order to be verified or falsified in the light of specific categories of data in the way exemplified by Ober (2015). To step outside that zone is to run the extreme risk of making false moves in an unfamiliar environment. My own excursions into social science territory have betrayed amateurishness, and it took me forty years of intermittent engagement with economic history before I had the confidence to use an indifference curve in a published paper (Davies 2016b). Other perils also confront us: that of resorting to a single explanation or interpretative idea—a danger to which I think those wedded to neo-institutionalism have succumbed; that of being infected by the normative language which is endemic in political studies and in some economics; and that of underestimating the degree to which the powers inherent in the predominant form of polity throughout antiquity, a monarchic regime, could cut across conventional economic logic: a theme explored in Paterson (2004). It is sufficient to identify (1) the need to secure legitimacy through magnificence (which might not be an economically productive form of investment), and (2) the shocks to the productive system and to the use of capital that would be created by the selective assassination of economically powerful rivals or by massive confiscation of goods and territory.

¹⁷ E.g. inter multos alios, Haussig (1988) and Frankopan (2015).

¹⁸ E.g. (Cohen 1992; Aubert 1994), or D. Jones (2006).

¹⁹ E.g. Fraser (1972) or Clavel-Lévéque (1974).

²⁰ E.g. Aperghis (2004).

²¹ E.g. Archibald (2013), Migeotte (2014), and Bresson (2016). The same approach is taken throughout by the editors and contributors to Vittinghoff (1990), and very largely also in the *Cambridge Economic History*.

²² This list could be extended, e.g. by citing studies of specific technologies, of institutions such as banks and markets and temples and cults, of physical installations such as stoai and harbours, or of attitudes.

And yet there are strong counter-arguments. The more we accept that the cultures that we study were complex structures, displaying every kind of familiar and unfamiliar human behaviour, the more urgently we need to have at our disposal every descriptive and interpretative tool that we can lay our hands on and learn how to use. Ideas and models taken from economics alone, however broadly based we envisage that discipline as being, cannot easily account for much ritual behaviour, for example, let alone for artistic endeavour. A pertinent example is the economic analysis offered by Ekelund et al. (1996) of the practices developed by the medieval Roman Catholic Church in order to market 'salvation' as a 'credence good'. By treating the Church as a 'firm' and applying theories of the firm to it, they offer a most illuminating model which lends itself for adoption and application to an ancient Greek context.²³ Moreover, it is only by using a wide range of such tools, and their associated vocabulary, confidently and correctly that we can offer our colleagues in the social sciences professionally reliable access to the masses of data that we have assembled, and thereby generate the possibility of deploying that common language and of making firmly based comparisons, across the spectrum of ancient, medieval, early modern, and modern societies.

However, to endorse such counter-arguments is also to accept the need for adaptive behaviour. It can take various forms. The primary form is the requirement either to gain familiarity and confidence in a second discipline or to collaborate with a social scientist. Each expedient carries a risk, the first that of expending much time and effort, the second that of not finding a congenial collaborator,²⁴ but one way or another the risk of amateurishness must be minimised. A second, very uncomfortable form would comprise a collective self-appraisal, for awkward questions pose themselves: while such adaptive behaviour can be seen as an internal drive from within the specialism, it can also be seen as self-preservation on the part of a tiny segment of academic life that is aware of its own marginality and vulnerability. In basic 'economic' terms, who wants such comparisons and hybrid compositions? Is there really a 'demand'? The published version of my closing address at Edinburgh (Davies 2018a) analysed this existential Angst in greater detail, for it is well-based: the ever-increasing emphasis which research funding bodies are laying on STEM subjects (Science, Technology, Engineering, Medicine) is bound to inflict casualties elsewhere. All that can be offered here is the three 'hard truths' with which that address ended: first, that the prospect of any rapid or large-scale convergence between scholarship about the pre-Islamic world and contemporary social science (including economics) is remote: social scientists are not in general disposed to use antiquity either as a database of observed and described behaviour or as a test-bed for their theories and models. The second is that it is unrealistic to expect the initiative in reconnaissance or convergence to be taken by social scientists. The third is that whatever effort is made may yield results that are of peripheral interest but not of

²³ It is however necessary to add the proviso that their straightforwardly 'economic' approach omits all consideration of the aesthetic, communitarian, and cultural roles performed by the 'firm'/Church.

²⁴ But, as I noted (Davies 2018a, 576), the Edinburgh conference displayed five such congenial collaborations.

organic value to one discipline or the other. It is, I fear, a matter of how strongly the community of economic historians of Antiquity wishes to emerge from isolation and to make its data and its voices heard and noticed.

Area of Decision III: Format of Analysis

Moreover, engagement with the social sciences will not in itself reveal which formats of analysis are likely to present the most realistic model of the economic activities of the remote and alien societies which are being studied. Here a third major decision has to be made. On the one hand the search is for a framework within which we can locate all transactions involving a transfer of value—monetary and non-monetary, private and public, legal and illegal, by consent or violence—that took place throughout AfroEurAsia from the advent of the Iron Age at least until the Islamic Conquest: doing so moreover in such a way as not merely to classify such transactions descriptively but also to build into the model a set of modes and directions of energy that are sufficient both to generate all the activities under review and to account for the structural and institutional developments that can be detected. On the other hand, any model must simplify ruthlessly and must subordinate the descriptive mode to the abstract mode, alike in language and in structure. (The distinction envisaged here is analogous to that between cartographic scale map and topological diagram discussed by Waterman 2007, 558.) That is inescapable if we are to go beyond being collectors and organisers of information, but it runs the severe risk of over-simplification.

Echoing a paper currently in press (Archibald and Davies forthcoming), I have therefore to issue a health warning against the over-use of neo-institutionalism. There are three reasons why, for all its wide application and enthusiastic advocacy,²⁵ it is not a panacea. The first is that institutions **channel** and **shape** human action and exchange: they do not **initiate** them. They are pathways, not psycho-physiological drives. Institutions can enable, support, and enhance—that does not admit of denial—but the stimuli that initiate action lie within individuals, singly and collectively, and for practical purposes are best summed up by the various levels of Maslow's hierarchy of needs. The second reason is that, like the proxy data referred to above, recourse to institutional analysis is a way of compensating for the lack of quantitative data: it gives us shape but not volume or trend. The third reason complements the first by being expressed in more specifically economic terms: neo-institutionalism is mostly about the supply side, often helping—but sometimes constraining or manipulating—the practice of exchange and acquisition. Institutions do indeed thereby indirectly influence demand, but they do not provide the basic input of energy which alone comprises and shapes demand.

²⁵ Principal discussion in Scheidel et al. (2007, 113–143) (B.W. Frier and D.P. Kehoe), with Bang (2009), Bresson (2016, 15–27), and Silver (1995) as a forerunner. I thank my referees for both strengthening and challenging my objections.

Personal experience illustrates the point, in the form of the chapter ‘Classical Greece: production’ which I contributed to the *Cambridge Economic History* (Davies 2007). I found that task informative but unsatisfactory, partly because the descriptive mode inevitably dominated, but mainly because the sequence Production–Distribution–Consumption inverted the causal chain: it followed the object or the service from production to consumption, but it followed neither the ‘money’ (in the broad sense of the term) which was expended nor the demand which generated the production in the first place: still less could it dwell on the component drives which comprised that demand, including those such as prestige, magnificence, aggression, display, and legitimation which, Thorstein Veblen notwithstanding, do not figure in the vocabulary of orthodox economics.²⁶ That is all the more severe a criticism when such terms described an essential component of behaviour, not least that of the monarchic regimes which predominated everywhere throughout Antiquity—except of course in those literate and noisy republican areas, the scholarly fascination with which distorts so much ancient economic history. In other words, institutionalist and other conventional approaches do not satisfactorily encompass the entire macro-region. The challenge needs a different register, more abstract but also more inclusive. Broodbank’s magisterial survey comes closer, as does Bresson’s with its stress on the pressures exerted by the environment, but even they do not excavate right down to the bedrock of human nature, to locate the basic springs of action.

It is therefore appropriate to end this presentation by reporting an attempt to perform such an ‘excavation’. A recently published chapter of my own (Davies 2018b) discussed the operative forces of state formation in post-Mycenaean Greece and identified six such forces: that exerted by the exceptional individual; that exerted by population; that exerted by the natural environment; that exerted by ideas of the supernatural; that exerted by the availability of convertible resources; and that exerted by memory, imagination, and a sense of identity. (It is now clear to me that a seventh force should have been added, namely the force exerted by male aggressiveness and violence.) To formulate an analysis in such terms was to focus on inputs of energy as drivers of demand, and thereby to provide a model which was not predicated anachronistically upon ‘the emergence of the city-state’. It could therefore be seen to apply in varying configurations of interaction in each locality of the Archaic Greek world according to local conditions, doing so in such a way as to generate the hundreds of monarchies and polities that are attested therein.

It will be clear that such an analysis is less specifically economic, and much more psychologically based,²⁷ than my first. Whatever the merits and demerits of that second example, I have little doubt that economic terminology on its own is not going to tell us how ancient economies worked: it will be essential to use every intellectual tool that is available.

²⁶ But a referee helpfully calls my attention to Gilady (2018).

²⁷ But not a back-door return to Finley (1973, 1985). Using the work of Granovetter and Williamson, Lewis (2018) sets out the case that ‘Finley was absolutely correct to stress the importance of social values, but he was mistaken in his assessment of what those values comprised’ (ib. 40).

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