REVIEW ARTICLE

The Ancient Economy and New Institutional Economics*

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For Erik Christiansen, teacher and friend

W. SCHEIDEL, I. MORRIS and R. SALLER (EDS), *THE CAMBRIDGE ECONOMIC HISTORY OF THE GRECO-ROMAN WORLD*. Cambridge: Cambridge University Press, 2007. Pp. xiv + 942, illus. ISBN 0-521-78053-5/9-780-52178-0537. £120.00/US$225.00.

> Robert Solow, in discussing the fundamental assumptions of economic theory, characterised such views as follows: 'My impression is that the best and the brightest in the profession proceed as if economics is the physics of society. There is a single universally valid model of the world. It only needs to be applied.' To an economic historian surveying the ten millennia of human history from the onset of the Neolithic revolution, however, the ergodic hypothesis is a-historical.
>
> (Douglass North, *Understanding the Process of Economic Change* (2005), 19)

I

The Cambridge Economic History of the Greco-Roman World is a monument. The result of the undoubtedly Herculean labour of the three editors — Walter Scheidel, Ian Morris and Richard Saller — this mighty and awe-inspiring tome unites the efforts of more than a score of historians to present an economic interpretation of Greek and Roman history, from Archaic times till the beginning of Late Antiquity. With twenty-eight chapters, generally well-written and clearly laid out, and more than 950 pages, the volume represents the culmination of a generation of vigorous scholarship and intense debate; but more importantly, it also marks a new, inspiring beginning.

Several such attempts have been made during the last decade. *The Corrupting Sea* (2000) of Peregrine Horden and Nicolas Purcell used an ecological view of the Mediterranean to identify a common baseline for ancient and medieval history in the region. To clarify, broaden and nuance our understanding of the preconditions for the rise of feudalism, Chris Wickham's *Framing the Early Middle Ages* (2005) developed a socio-economic comparison spanning four centuries and comprising a great diversity of societies from the

* I am much obliged to Professors Andrea Giardina and Elio Lo Cascio for having included me in what proved to be a very enjoyable and inspiring one-day seminar debating the *Cambridge Economic History of the Greco-Roman World* on 22 April at the Istituto Italiano per la Storia Antica in Rome. This gave me a welcome opportunity to present some of the views expressed here and benefit from the succeeding discussions. Peter Garnsey read the first version and offered his usual trenchant and penetrating remarks which much improved this essay. As an enthusiastic correspondent, Daniel Thompkins has generously shared his views on the ancient economy and new institutional economics with me. Carl Hampus Lyttkens has allowed me to benefit from an economist's point of view on several occasions. It is now seventeen years since I first met Erik Christiansen, my teacher in Aarhus, who with his life-long dedication to the study of the Roman coinage of Egypt taught me the value of straying outside the beaten track. Now at his retirement, I offer this piece to him in humble gratitude.

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Caliphate of the Umayyads to Late Iron Age Scandinavia. The current volume is perhaps less adventurous and transgressive than either of these books in terms of its chronological and geo-cultural conception; however, only a little. While the emphasis is placed on the 'classical' periods of Greek and Roman history, chapters are also devoted to the Persian Near East and Early Iron Age Western Europe. Most surprising is the near omission of Late Antiquity (treated by Andrea Giardina in the epilogue, ch. 28) from the volume. Pragmatic concerns paired with some institutional inertia seem to be the root cause. The Late Roman world was already part of the *Cambridge Economic History of Europe* and full inclusion would have made an already long volume even longer.

If what might be termed the traditional, political narrative of Greco-Roman civilization still structures the volume, there are plenty of other innovative features. Part 1, 'Determinants of Economic Performance', consists of five essays setting out the basic parameters which shaped the ancient economy in *la longue durée*: ecology, demography, household and gender relations, law and economic institutions, as well as technology. More broadly speaking, the underlying ambition of this landmark economic history has been to approach the Greco-Roman experience through the methods and concepts of the so-called New Institutional Economics (NIE). Taking their cue from the work of Nobel-laureate economist Douglass North, the editors define the purpose of the project as an attempt to understand both the structure and performance of the ancient economy (12). To this end, economic analysis must be combined with close attention to social and political institutions and developments. By retaining a foothold in the 'traditional' political history of the Greco-Roman world, the authors have been able to integrate into their view of the economy the crucial aspect of state-formation, which was largely and, one could argue, fatally missing from the seemingly more radical *Corrupting Sea*. The Oxford Roman Economy project has set about systematically collecting and organizing our steadily expanding store of data produced, in particular, by archaeology (cf. http://www.romaneconomy.ox.ac.uk; Bowman and Wilson (eds), *Quantifying the Roman Economy* (2009)). But if historical research is to reap the benefits of such efforts, conceptual innovation is required to match the growing amount of evidence. At the moment, new institutional economics seems to be the body of theory with the greatest promise. Certainly, it provides a solid set of organizing concepts for this volume. The many individual chapters which in Parts 2–8 trace the economic history of the Greek and Roman world as it unfolds, chronologically and geographically, succeed in addressing a shared set of questions. This is no mean feat and the editors must be congratulated on their achievement.

II

New institutional economics would, in many respects, seem congenial to the interests of Greco-Roman historians. It is perhaps even a little surprising that dialogue between the two disciplines has not developed much earlier and more forcefully. When Finley launched the modern debate on the ancient economy in 1973, he famously rejected the models of mainstream economics. Modernizing interpretations of the Greco-Roman world, in the style of Rostovtzeff, then came fully equipped with capitalist entrepreneurs, a strong merchant bourgeoisie, and the commercial wars familiar from the much later age of mercantilism. These, Finley insisted, were anachronistic notions and he called for an image of classical antiquity more firmly grounded in its particular historical context. Anthropology, development economics, Marxism and the historical sociology of Weber were all mobilized to develop an alternative view of the ancient world which stressed its specificity, its structure, in other words, and its agrarian base. What these different strands of thought all had in common was the denial that capitalism was somehow universal and immutable. Weber spent much of his career pursuing the question why modern capitalism had failed to germinate in many civilizations even though they were no less complex societies than in early modern Europe. By seizing on Protestantism, Weber came to see capitalist economic activity as a cultural anomaly and as an exception to the historical rule (cf. *Gesammelte Aufsätze zur Religionssoziologie* (1920), Vol. 1, Vorwort). Our image of complex agrarian economies with widespread urbanization and substantial trade, like the Greek and Roman economies, had to be historicized.

As ancient historians increasingly turned away from the language of capitalism and market-centred economics, some economists began to move from the other side of the spectrum and make their way into history. A seminal contribution was made by Ronald Coase who drew attention to the shortcomings of classical economic theory (for the essentials cf. 'The Institutional Structure of Production', http://nobelprize.org/nobel_prizes/economics/laureates/1991/coase-lecture.html, further *The Firm, the Market and the Law* (1980); *Essays on Economics and Economists* (1994)). Markets if left to themselves do not necessarily function in the same way, let alone produce the beneficial results predicted by much mainstream economic theory. The missing link is provided by the concept of transaction costs. Many things prevent agents from pursuing their economic interests unhampered, even in a 'free' market. This is because it is costly to engage in exchange activities. Goods have to be transported to market; information about price, quality and other relevant things needs to be obtained. All this and much more burdens market transactions with considerable costs. One crucial implication of transaction cost analysis is that frequently it is too expensive to operate through the market and alternative forms of organization prove more efficient in allocating economic resources. This, for instance, explains why so much activity even in a modern competitive market economy takes place inside social organizations such as firms ('The nature of the firm', *Economica* 4:16 (1937), 386–405). Another implication, which follows closely from the first, is that markets and economies come in many different shapes and sizes, depending on the precise make up of the institutional fabric which structures economic activity (cf. 'The problem of Social Cost', *Journal of Law and Economics* 3:1 (1960), 1–44). Economists, Coase argues, need to pay much more attention to the concrete and varying ways economic activity is organized.

These are ideas that should resonate well with ancient historians. In previous generations we have learned from Karl Bücher and Polanyi to see the economy as an 'instituted' process characterized by historically varying patterns and forms of organization (*Die Entstehung der Volkswirtschaft* (first of numerous editions 1893); K. Polanyi, *Trade and Markets in the Early Empires* (1957)). Among the neo-institutional economists, it is Douglass North, in particular, who has successfully advanced an agenda for economics as the study of differing historical institutions (*Institutions, Institutional Change and Economic Performance* (1990)). But there are some significant differences too. Polanyi, for instance, taught us to think of pre-capitalist economies as socially and culturally embedded. To North and his followers, it is important to emphasize that a modern market economy is no less a product of social and cultural institutions — all economies are embedded. North, therefore, also tends to conceptualize historical difference less in terms of the prevalence of discrete types or forms of exchange such as Polanyi's triad of gift-exchange, redistribution and market, but rather as variation among complex institutional configurations. This brings his work much closer to the tradition of Weber and within classics, to Finley and the whole Cambridge school of economic history (cf. *Understanding the Process of Economic Change* (2005), 57 for approving remarks on Weber, and also Lo Cascio, 'Crescita e declino: l'economia romana in prospettiva storica', *Rivista di Storia Economica* 23 (2007)).

North's project, however, started off as an attempt to rescue neoclassical economic analysis; its universalizing models based on the assumption of the rational pursuit of self-interest had to be complemented by the addition of transaction costs and institutions. Initially, this constituted a stumbling block of sorts between the two areas. *Structure and Change in Economic History*, from 1981, took in Finley's work, but only for a brief hearing. North reacted negatively to the 'anti-market economics' aspect of Finley's argument and saw it mainly in terms of opposition to his own project. '. . . historians and other social scientists examining the ancient world have maintained that the motive forces underlying classical and neoclassical economics are not amenable to analysing the ancient world,' as he observed wryly (*Structure and Change*, 120). But this conflict of opinion was mostly a clash of competing rhetorics, rather than a reflection of an unbridgeable rift. For, according to North, in order to work, traditional economic theory had to be modified substantially by taking account of culture and ideology, precisely the linchpins of the anthropologically informed analyses of *The Ancient Economy*.

As North has developed and sharpened his theoretical work during the last decades, the socio-cultural and cognitive dimension has come to play an increasingly dominant role in his analysis (cf. *Institutions, Institutional Change and Economic Performance* (1990), chs 3–6; *Understanding the Process of Economic Change* (2005), chs 3–5). Today he hardly stands behind the anthropologically inspired analyses of the 1970s in throwing the occasional aside at the expense of the so-called market-fundamentalists. 'Economists of a libertarian persuasion have for some time labored under the delusion that there is something called laissez faire. . .', this scornful remark is only one of several that jump off the pages of North's recent book, *Understanding the Process of Economic Change* ((2005), 122). Classical economic models frequently get it wrong, North maintains, because agents only have a limited understanding of the choices open to them. Human models of reality are bounded and imperfect; and we cannot predict the things we will find out tomorrow that may seriously affect the wisdom of the decisions we make today. Moreover, economic choices and incentives are shaped by culture and social organization. In other words, the broad institutional weave of society defines 'the rules of the game' that individual economic actors play. Institutions, therefore, must be at the centre of economic analysis. If anything, North's objections to purely market-centred economics now go moderately against the majority trend in Roman economic history. A strong tendency has emerged to re-emphasize the scale of economic activity, the significance of markets and trade, and in close tandem with these phenomena, to shift focus to the issue of performance rather than the particular structure of the Roman economy (e.g. in this journal Temin (2001); Wilson (2002); Harris (2006)). This makes the new institutional economics ideally suited to help ancient historians transcend some of the well-worn debates that under various labels have characterized the field. Primitivists and modernists is only the most famous pair of antagonistically opposed concepts which have lent their name to the recurrent clashes among ancient historians about whether in our portrayal of the Roman economy we should privilege structure or scale and performance, as the editors write in their introduction. New institutional economics, however, combines a clear focus on the particular institutional configuration of economies with a strong interest in performance.

If neo-institutional economics can be seen as an attempt to bring history to the attention of economists, it may equally well be a way to make economic theory serve the interests of historians; it is situated at the crossroads of the two disciplines. But quite apart from the obvious analytical benefits which flow from intellectual cross-fertilization, there is another dimension no less important. Both Coase and North received the Nobel Prize for economics during the 1990s. They have been in the vanguard of an increasingly influential and successful theoretical movement. More prizes have followed in the twenty-first century, for instance with recognition of Joseph Stiglitz (e.g. 'Markets, market failures and development', *American Economic Review* 79:1 (1989), 197–210; *Globalization and its Discontents* (2002)) and George Akerlof (http://nobelprize.org/nobel_prizes/economics/laureates/2001/akerlof-lecture.pdf). The continued attraction of Classics is closely connected with its ability to address current concerns and inform the central debates of the present. With Classics always under threat from the relentless and almost ritually recurring calls for modernization of educational curricula, the attempt to inscribe the Greco-Roman experience within a growing and influential new discourse is an initiative which deserves to be applauded — for its clearsightedness no less than its intellectual interest.

The efforts of the New Institutional Economists have mainly been targeted at revising the classic narrative and explanation of the rise of the modern economy in Europe and the Atlantic, from the Middle Ages to its present extension across the globe. *The Rise of the*

Western World, a New Economic History (1973) — the result of a collaboration between North and Robert Paul Thomas — sought to explain how gradually a property rights structure had evolved in European societies that successfully merged the free pursuit of individual interests with socially productive results. The firm belief that 'Self-Love and Social be the same', which since the Enlightenment had served as the foundational principle of economic thought, had to be historicized. Only under a very specific institutional regime did the predictions of Adam Smith and his followers hold true. As the authors write on the first page: 'Efficient organization entails the *establishment* [emphasis added] of institutional arrangements and property rights that create an incentive to channel individual economic effort into activities that bring the private rate of return close to the social rate of return.'

Medieval history has been quite well served by NIE. Karl Gunnar Persson also made it the backbone of his *Pre-industrial Growth* (1988) — a study brought to the attention of classicists by Paul Millett ('Productive to some purpose?' in Mattingly and Salmon, *Economies Beyond Agriculture in the Classical World* (2001), 17–48). More recently Avner Greif has developed an extensive comparison of Maghribi and Genoese medieval merchants and the social institutions they created to structure their activities. Greif's study combines an interest in elaborating interpretations of European institutions with the second main analytical trend within this branch of economic thought, globalizing and cross-cultural comparisons of historical economies ('Cultural beliefs and the organization of society', *The Journal of Political Economy* 102:5 (1994), 912–50; *Institutions and the Path to the Modern Economy* (2006)). The most prominent representative of this approach must be E. L. Jones with his *The European Miracle* (1981) and *Growth Recurring* (1988). A new off-shoot in this direction was provided by Findlay and O'Rourke in their *Power and Plenty* (2007). This is an attempt to bring the history of world trade during the last millennium to the attention of economists by describing its development, not merely as an ever wider expansion of markets, but as the result of a dynamic interaction of military power and market behaviour.

So far, Greco-Roman history has not been a central concern of this movement. But various observations on the ancient world can be found scattered around in the writings of its practitioners. Roman law has inspired several brief comments from North. The whole Finley debate spilled over onto the pages of Jones' discussion of technological innovation in the pre-industrial world. While his conclusions repeat several, but not all, of Finley' views, Jones nevertheless criticizes him for underestimating the potential of slow, incremental technological development (*Growth Recurring* (1988), ch. 3). Traditional societies were not stagnant, but gradually expanding. Improvements in technology helped societies manage and sustain such change. Most of the time, however, growth remained extensive, that is the economy expanded in tandem with population growth; but occasionally brief spurts of rising per capita incomes were also experienced. Again, it is North who is best able to accommodate this type of consideration within a general sketch of human economic development. Not without justification, the terms of economic history writing have been dominated by capitalism and the industrial revolution. Nonetheless the focus on the dramatic increases in productivity brought about by this rupture has put in the shade a much prior, but equally significant economic revolution. This first economic revolution, as North calls it, is the transition of human societies to sedentary agriculture. In the wake of this followed crucial developments such as metallurgy, writing, and urbanization. None of these enabled man to 'escape from hunger', but they significantly increased the capacities of human society. Most of pre-industrial history, including Greco-Roman, concerns the growth and expansion of these kinds of agrarian, civilized societies (*Structure and Change in Economic History* (1981), ch. 8). For a field which has been vigorously engaged in discussing the modernity of Greek and Roman economic life, this shift in perspective completely changes the terms of the debate. Economic developments need not be fitted analytically into a context of modernity of which they generally fall far short when questioned more deeply (cf. in addition North, Wallis and Weingast, *Violence and Social*

Orders (2009), 48–9, on Rome and the absence of a further evolutionary drive towards modern conditions). Instead Roman developments can be relocated in a context which allows a more realistic assessment of their 'merits'. In this perspective, the Roman Empire emerges as the first culmination of the long-term expansion of urbanized agricultural societies. Eventually the rising burden of taxation combined with a decline in military superiority over its neighbours undermined the advantages enjoyed by the Roman Empire. There were few economies of scale left to large scale organization, as North sees it, and the whole edifice broke up (*Structure and Change* (1981), ch. 9).

But even if North has actually attempted to incorporate the Greco-Roman experience within a wider macro-historical framework, these observations do little more than scratch the surface; his is only a brief sketch, an intimation more than a finished analysis. A key tenet of NIE is that detailed historical study of institutions is necessary to explain economic developments. This work still mostly remains to be done, or rather that has been the case. During the last few years a small number of studies have begun to fill in the picture (Manning, *Land and Power in Ptolemaic Egypt* (2003); Erdkamp, *The Roman Grain Market* (2005); Kehoe, *Law and the Rural Economy in the Roman Empire* (2007); Bang, *The Roman Bazaar* (2008). See also pioneering contributions in the *Journal of Institutional and Theoretical Economics* by Lyttkens in 153:3 (1997), 462–84 and by Morris, Weingast, Scheidel, Manning and Quilin in 160:4 (2004), 702–85). *The Cambridge Economic History of the Greco-Roman World (CEHGR)* now makes a huge stride forward to bring this type of enquiry out of its infancy and represents an important effort to flesh out the impressionistic outline offered by NIE of classical historical development.

III

What picture of the Greco-Roman economy then emerges out of this project? The following sections will discuss in what ways *CEHGR* manages to confirm, modify, or contradict and to what extent it succeeds in mobilizing the basic analytical framework offered by the new institutional economists. Focus will be on questions of long-term historical development and the Roman Empire, while Greek history receives less attention — a set of priorities which hopefully will be found to reflect the interests of the readership of this journal. If the 'first economic revolution' combined agriculture with the rise of states and cities, then the Greco-Roman experience can be fitted into this process as the second wave. 'The Greco-Roman world generated a distinct Iron Age economics', Ian Morris and the other editors write in the introduction, 'involving much larger movements of staples through markets, concentrations of people in cities, extensive monetization, and investment in the stock of knowledge. Puny as these developments were compared with what happened since the eighteenth century, they were unprecedented' (10). The central theme in the economic history of classical antiquity may then be identified as the story of how this more powerful way of life evolved and expanded eventually to take in the entire Mediterranean world.

At its most basic, this is a history about numbers of people. Unsurprisingly it is Walter Scheidel who, in what must be one of the cornerstones of the book, provides a basic model of the demographic history of the ancient world (ch. 3). He reminds us of the need to differentiate our concept of economic growth. This is not merely a question of rising per capita incomes. No less important when we deal with agrarian societies where many people live close to subsistence levels is growing population. The growth of the ancient economy is to a very significant degree a story about rising population, some of it achieved through conquest and inclusion of peoples previously outside the system, lateral growth as Scheidel calls it. For per capita growth, however, expanding population is a mixed blessing. Malthus long ago pointed out how in agrarian societies gains in productivity tended to be undermined by population growth, in effect more mouths to feed. During the 1960s Boserup turned this view up-side-down by demonstrating how increasing population often pressed peasants to intensify production by taking more land under plough, shortening fallow periods, or otherwise adopting more labour intensive regimes of cultivation. Economic development was a product of population growth, not its enemy (*The Conditions of Agricultural Growth* (1965)). However, the pressure on incomes produced by the need to divide agricultural resources between a growing number of people was a necessary inducement since the solution generally required peasants to increase their work load. A rise in productivity was bought at the price of increased drudgery. Scheidel combines these two approaches in a model where demographic growth in the long run tends to outstrip rises in per capita incomes, but is offset temporarily by periods where intensification moderately raises productivity. Such periods of relative affluence should not therefore be written off as ephemeral, but neither should they be written into an explanatory framework seeing the economy approaching the early stages of modernization. Far more important, from a long-term perspective, is that they raised the level of population which could be sustained in a given territory. When Malthusian pressures after a time reasserted themselves, the economy would not simply fall back to its old equilibrium level; it would be a bigger social organism that found a new balance. Greco-Roman economic history has to be constructed around such a series of moving, or homeostatic as Scheidel prefers, equilibria that gradually enabled the expansion of ancient society. The lasting achievement of this process was to push urban forms of social organization into North and Central Europe, where a string of Roman foundations still today lends shape to the urban geography along the Rhine and Danube (85).

Schneider's chapter (6) on technology complements the picture offered by Scheidel of gradual expansion by demonstrating a slow, but nevertheless significant accumulation of technological improvements through Greco-Roman history. The water mill, the screw press and larger kilns, among other examples given, bespeak gains in the technological capacity of ancient society. In pulling the threads together Schneider has been able to build on the many advances in our understanding made particularly within Roman archaeology during the last decades. One of the advantages of a book such as *CEHGR* is its ability to provide a *longue durée* perspective on the ancient economy. After all, there is little reason to expect economic trends to conform neatly to the chronological rhythms of political history. An observation that emerges with particular force from the chronological chapters is the significance of productive developments during the earlier phases of both Greek and Roman history (cf. chs 8, 10, 18 and 19 by Morris, Osborne, Morel and Harris). If we are to speak of per capita growth, these periods seem by far the best candidates. On the other hand, it is well known that in terms of material, and hence archaeological remains, culmination happens only relatively late and often coincides with perceived population maxima. Most of Schneider's prime examples are drawn from the Principate. This discrepancy may seem paradoxical, even counter intuitive. But the contradiction is only apparent. Archaeology reflects activity; it does not offer an uninflected mirror image of income levels. Experience from much better documented historical societies would teach us to expect an inverse relationship between population density and income levels, as Scheidel observes (59, 65–6). This actually means that in an economy such as the Roman we would expect Malthusian pressures to make themselves felt on per capita incomes at least during the first and second centuries A.D. when population and therefore aggregate economic activity were climbing towards their pre-Antonine plague maximum. This interpretation will seem controversial among some historians. Jongman's contribution (ch. 22) to the volume on 'Consumption' in the early Roman Empire, for instance, argues for a peak in per capita incomes during the first and second centuries A.D. with the Antonine era before the plague of the 160s marking a high point of affluence in the history of pre-industrial populations. Jongman bolsters his case with various quantitative graphs, starting with Hopkins' tabulation of dated shipwrecks per century and ending with a similar diagram recording mammal bones from excavations. Quite apart from the very considerable difficulties involved in using such graphs (discussed by Whittaker, *Land, City and Trade in the Roman Empire* (1993), ch. 14 and Wilson in his contribution to *Quantifying the*

Roman Economy (2009)), the issue is not likely to be resolved by the archaeological data alone. Both views predict the increased density of material culture that archaeology documents. The question, therefore, is as much about competing interpretations. At the moment, the Malthusian version seems to me to offer the theoretically most economical explanation. The claim that the early to high Empire combined unprecedented population densities with a peak in per capita income places a considerable burden on the rival, 'optimistic' school; it needs to find ways, both logically consistent and theoretically satisfying, to explain how the Roman agrarian economy somehow managed to escape Malthusian constraints. The other chapters in the *CEHGR* offer little help here. Both Schneider and Kehoe, the latter in his chapter (20) on production in the early Roman Empire, see moderate expansion in productive capacities. But this may not be enough.

IV

The *CEHGR* is not a doctrinaire work. Within the overall framework of moderate long-term expansion and limited growth, the editors have wisely refrained from imposing a party-line on the Greco-Roman economy and giving a semblance of unity and certainty of interpretation where none exists. This makes it a much more lively and interesting work to engage with as one notices tensions, disagreements and conflicts in the way the material is handled by different authors. Such incongruities may help us to identify open questions and challenges that await us.

A good example is the contrasting chapters on Egypt under the Ptolemies and the Romans offered by Joe Manning and Dominic Rathbone. 'I will argue for significant aggregate and per capita growth in the first two centuries, attributable to the institutional, commercial, and behavioural impact of integration into the Roman world; then, following the Antonine plague, some aggregate decline in production, but renewed, if more differentiated, per capita growth attributable to internal socioeconomic changes' (698). So Rathbone. Manning views the Ptolemaic achievement in rather more modest and cautious terms: '. . . on the whole Rostovtzeff's view that we are dealing not so much with "a radical change" in the economy as with "its partial improvement and its systematic organization" is sound' (459). Yet the rule of the Ptolemies saw the emergence of Alexandria as one of the biggest cities and markets in the Mediterranean, the development of the Fayum, and the introduction of coinage. Egypt may well have advanced under the Romans, but it is difficult to compile a similarly impressive list. The contrast in assessment is, of course, a reflection of the perennial frustration of ancient economic history: we lack reliable statistics. But we may seek to ground our (impressionistic) analyses more firmly by imposing tighter analytical controls on our assessments, for instance through more rigorous historical contextualization. One of the great virtues of the volume is precisely to remind us of the importance of co-ordinating and fitting our specialized analyses of shorter periods into a longer historical sequence.

Contextualization and sequencing is one aspect of the broader phenomenon of historical comparison. Given the character of current debates about the ancient economy, it is perhaps a little surprising how sparingly individual authors draw on comparative examples to inform their discussions — for refining our sense of the historically plausible, and for alternative analytical models. The institutional theoretical framework, however, serves as a substitute and also offers plenty of novelty. There is, after all, a limit to just how experimental such a volume should become. The task of synthesizing the political economy of the late Republic (ch. 19) and early Empire (ch. 23) has fallen to William Harris and Elio Lo Cascio respectively. These are key chapters of the book. Here the whole armoury of institutional analysis is brought into play to provide a general explanation of Roman economic history. Again, juxtaposing the results raises interesting questions and problems. Harris's last century of the Roman Republic is one of rapidly accelerating trade, growth of markets, accumulation of capital, increasing mastery over nature, and development of law and administrative skills, particularly in the hands of the *publicani*. There were also some institutional impediments, of course. Low levels of literacy, poor information, and household organization of business are some of those mentioned. But overall 'the potential for modest economic growth was there, and liquid capital seems to have been abundant'. In the end, however, war prevented the Roman economy from reaping the full rewards of these developments. Plunder and disruption, particularly following in the wake of the civil wars, 'makes it impossible to suppose that there was much ultimate economic growth'. The scale of 'destruction of fixed capital — and of people' was too large, Harris concludes (539). Real growth therefore had to await the stabilization of Roman hegemony under the Empire.

In many respects, the early Roman Empire seems to conform to an almost ideal scenario from the point of view of the New Institutional Economics. As Elio Lo Cascio observes, 'the creation of more peaceful and safer conditions translated to a marked decrease in transaction costs. The suppression of piracy in the final decades of the Republic, the diffusion of a "technology of measurement" and of common metrological systems, and above all the creation of a unitary monetary zone and of common legal rules, especially in the field of commercial law, were all quite remarkable contributing factors in this reduction of transaction costs'. Under the Principate, the Roman state proved to be a light burden; it provided plenty of services, most importantly 'the imperial state could define and enforce the fundamental "rules of the game", in particular exclusive property rights' (626). The Principate would seem to have produced conditions resembling those described by North and Thomas for European history where a stronger property rights regime reduced transaction costs and spawned trade, division of labour and growth. Yet there are unresolved questions here. In European history, stronger property rights and reduced transaction costs facilitating exchange developed in tandem with a steady build up of taxation and state institutions. The countries that fared best, like the Netherlands and United Kingdom, were some of the most heavily taxed in pre-industrial history (De Vries and Woude, *The First Modern Economy* (1997), 100–13; Brewer, *The Sinews of Power* (1988), ch. 4). Producing a strong property rights regime required a strong state and a lot of resources. However, the apparatus of the Roman imperial government was extremely rudimentary and the Roman state commanded a very small share of the economy; at least this is what is widely believed — and certainly by Lo Cascio (622–5). At a minimum, we need a theoretical explanation of why the Roman state could perform so well and at the same time make do with so much less.

But perhaps a more radical rethinking of the implications of institutional economics for Roman history may turn out to be necessary. It is thought-provoking that the phase of most explosive commercial development is not the peaceful Principate, but the war-ridden late Republic. This earlier period saw the formation of the two largest markets in the Mediterranean, the enormous cities of Rome and Alexandria. The unprecedented expansion of commercial activity is also reflected in the rise of the slave villa and the extensive export of Italian wine to Gaul, both inside and outside the province (Tchernia, *Le vin de l'Italie romaine* (1986)). To this could be added the emergence of Delos as a commercial centre and the establishment of a sizeable Roman merchant community on the island (Rauh, *The Sacred Bonds of Commerce* (1993)). Even the data on atmospheric pollution recovered from the Greenland ice cores which seem to register increased industrial, particularly mining, activity under the Romans, date the beginning of this phenomenon several centuries before our era and actually experience a peak under the Republic (cf. 548). Compared to these developments, the Principate gives much more the impression of a phase of consolidation and growth along the margins. As Morley observes in his chapter on the patterns of exchange during the early Empire: 'There is no evidence of the sort of year-on-year expansion of economic activity ... the great expansion had already taken place under the Republic' (589). There is something missing from our theoretical equation. That element might well be the effect of war and violence on the economy. This should not simply be seen as a negative influence. It is well-understood from later European history how warfare in the long term promoted economic development (McNeill, *The Pursuit of Power* (1982)). Integrating this aspect into our institutional model is one of the challenges which emerge from the *CEHGR* (as recognized in passing by Harris, 539).

V

Taken as a whole, there is a slight tension between the first part of the volume, 'The Determinants of Economic Performance' and the succeeding historical sections. The former chapters all offer very guarded statements about the potential for economic growth. Besides Scheidel's Malthusian low-level equilibrium trap and Schneider's limited technological developments, Sallares points out the possibilities as well as the constraints of the Mediterranean ecology and Saller shows the limited development in household strategies for property investment and utilization of female and child labour. These themes all come together in Kehoe and Frier's chapter on 'Law and economic institutions' which in effect serves as an introduction to the most influential strands of neo-institutional economic theory. Apart from North we meet, among others, Akerlof's work on information uncertainties and market imperfections as well as Coase's theory of the firm. The latter two economists have been intensely interested in the limitations of market institutions. It is not by coincidence that neither of the two economists plays a very prominent role in the historical chapters. These rather tend to focus on growth and the creation of effective institutions to lower transaction costs. Law, for instance, is almost invariably seen as increasing efficiency. But few institutions have only beneficial effects, as North reminds us; generally the arrangements they promote always favour some over others (e.g. *Understanding the Process of Economic Change* (2005), 51, 61–3, 67). More firmly defined property rights may for instance benefit landlords, while peasants find themselves squeezed out of the commons. When working with historical examples, however, our data are, more often than not, insufficient to measure the impact of a particular institution. Here, the contribution of institutional theory is to provide us with a more differentiated and systematic understanding of the various factors involved in the operation of a set of institutions. Indeed, it is one of the strengths of institutional theory that it allows us to perceive, as Kehoe and Frier rightly note, 'that many curious economic institutions of the Greek and Roman world were, at least in their origins, the consequence of individuals struggling rationally (within their lights) to maximize their personal gain' (143).

But strength sometimes turns to weakness. It is a small step from the attempt to discover the rationality of a given arrangement to the conclusion that it was also efficient and generated growth. This is a common fallacy in much institutional analysis. Recently Sheilagh Ogilvie discussed the problem in *Economic History Review* (60:4 (2007), 649–84) in an article with the ironic title, a verse taken from Pope's *Essay on Man*, 'whatever is, is right'. Taken as a whole, the historical chapters do not quite steer clear of the temptation generally to view institutions as productive; and the result is a tendency to conclude analyses in favour of growth. Seen in isolation each incident may be quite plausible, but the cumulative effect seems to give a slightly exaggerated impression of growth in antiquity, particularly compared with the framework sketched out in the introductory chapters. In that limited sense, one could perhaps argue that the institutional agenda has been most fully implemented in Part 1. But this observation is not intended as a critique. The task of Part 1 was to identify a limited set of macro-economic parameters. During the subsequent historical chapters the analytical programme had to be fleshed out in micro-economic detail. It goes almost without saying that the latter task is much more complex and potentially more fraught with problems of interpretation. One of the great achievements of *CEHGR* is to have begun this work in earnest, opened the debate about how to implement NIE in studies of the ancient economy and placed it on very firm foundations with a strong series of historical chapters.

It has been a fundamental tenet of NIE that institutions very rarely produce ideal results (North, *Institutions, Institutional Change and Economic Performance* (1990), 7–9). Consequently institutional economists have explored the effects of such deviations from the traditional market model and put alternative forms of economic organization high on the theoretical agenda. In many chapters of the volume, this aspect of institutional economics leads a rather muted existence. The main challenge that the historical sections of *CEHGR* have left to ancient economic history now is to integrate this dimension more firmly. By way of conclusion I want to single out three themes which could help solve some of the issues presented by the pattern of Roman economic history: (1) market imperfections, (2) hierarchical organization, and (3) protection costs and rent-seeking. But we start from the general principle that economies are dynamic and subject to change; one institutional matrix may prove efficient at one point in time and later turn into a stumbling block.

Now on the first point, economists such as George Akerlof have noted that frequently transaction costs are too high to make it possible to integrate markets. Risk and uncertainty, thin, unreliable and asymmetrically distributed information as well as transport costs often cause markets to stay separate. This is a phenomenon which is particularly pertinent to any discussion of pre-industrial trade ('The market for "lemons"', *Quarterly Journal of Economics* 84 (1970), 488–500; cf. Bang, 'Imperial Bazaar', in Bang, Ikeguchi and Ziche, *Ancient Economies, Modern Methodologies* (2006), 51–88). The constraints of transport and information technology ensured that market integration remained thin and fragmented. Peasants would even normally keep a substantial part of their produce outside the forces of the market; it was too risky for families to rely for basic subsistence on selling and buying alone. We therefore have to imagine the Mediterranean in which Roman expansion took place as a fragmented, only loosely integrated commercial environment (cf. Erdkamp, *Roman Grain Market* (2005)). Although rarely articulated, this view is in fact implicit in most accounts of Roman economic history. Otherwise it is difficult to see how one could fit in a notion of tightening integration under the emperors.

This brings us to the second point. If transaction costs are high, the market may not be the most efficient means of co-ordinating the allocation of economic resources. Coase made this observation the basis of his explanation of the function performed by firms in a market economy ('The nature of the firm', *Economica* 4 (1937), 386–405). If there were no costs involved in using the market to allocate economic resources, then it would in fact be impossible to understand why there were firms. Firms perform this function when markets operate too expensively, for instance when information is difficult to obtain or risk is high. Inside firms, economic resources are not allocated by the market mechanism, but on the basis of command and hierarchy. Economic integration within the firm is vertical and supersedes the price-mechanism: 'If a workman moves from department Y to department X, he does not go because of a change in relative prices, but because he is ordered to do so.' Firms are 'islands of conscious power' in the sea of spontaneous economic organization orchestrated by the 'hidden hand' (ibid., 387–8). Firms, however, are not the only kind of social organization to allocate economic resources on the basis of command and hierarchy. Empire and conquest was the single most important mechanism for reallocating economic resources in the Mediterranean under the Republic. We can now understand why the formation of empire did not only wreak disruption and destruction on the world of the Mediterranean. If markets were fragile and fragmented, then imperial command may have been a more efficient means of mobilizing economic resources. This could happen in several ways. Plunder brought immobilized wealth, such as temple treasures, into circulation again. Mass enslavement forced victims to work harder at below market rates of wage labour. Most importantly the imposition of an imperial tribute served to increase pressure on peasants (even in the eastern parts, as Alcock notes, 676). Just like population growth, the need to defray imperial taxes forced peasants to intensify cultivation. Empire turned out to be a much more forceful mechanism than the market to mobilize the economic resources of the Mediterranean. The growth of Rome into perhaps the biggest centre of consumption in European pre-industrial history on the basis of imperial tribute and rent remains testimony to the enormous economic power unleashed in this process. This mechanism may explain why the biggest expansion in commercial activity pre-dated the peaceful conditions of the early Empire.

Finally we arrive at the third point, the issue of protection costs and rent-seeking. Now, if the early Empire perhaps surprisingly presents us with less dramatic economic developments than the preceding period, we may be able to seek the answer in the area of transaction costs. If the establishment of the imperial peace, more unified coinage and legal integration did not usher in quite as much economic development as we might expect, the cause may lie in the existence of some transaction costs that have been left out of the equation so far. Likening the Empire to a firm is not as far-fetched as it might seem on a first impression. Lo Cascio, in his contribution, makes the important point that the Roman state did not restrict itself to laying down the rules of economic life. As the biggest landowner in the realm, the Roman emperor was also an important economic actor in the free market (631). From this angle, the Roman state could well be conceptualized as an economic enterprise. However, when we apply this perspective, institutional economics normally focus on another aspect of state activity. The most important good sold by the imperial government was not the produce of its estates, but protection (Bang, 'Trade and Empire', *Past and Present* (2007); cf. Lane, *Venice and History* (1966); Steensgaard, 'Violence and the rise of capitalism: Frederic C. Lane's theory of Protection and Tribute', *Review* V, 2 (1981), 247–73). Thinking about the Roman emperor as a seller of protection (in return for taxes) raises all sorts of interesting questions. The establishment of the monarchy saw the expansion of Roman power and consolidation of hegemony on a continental scale. On the fringes of the Empire there were monarchs and tribes strong enough to resist submission and halt the further advance of the legions; but there were no rivals left able to equal the might of the Roman ruler. It would be an anachronism to think that the emperor was in possession of a monopoly on the exercise of legitimate violence (cf. Brunt, *Imperial Themes* (1990), ch. 11) — Weber's distinguishing mark of a modern state — but from an economic point of view, he certainly held a very commanding, near monopolistic position in the market for protection. Monopolies normally abuse their power to charge a monopoly rent. Interpretations, therefore, that emphasize the benign institutional conditions produced for economic life by the Roman state, need to develop an explanation why the imperial government did not abuse its monopolistic position.

On the other hand, there are several indications that the Empire did not represent an exception to the historical rule. Impressionistic as our evidence is, the emperors expanded their share of landed property inside the realm (cf. 642–5); and they were far from foreign to predatory behaviour in relation to the estates of the political élite. What the emperors did, the landed aristocracy emulated on a smaller scale. Through its commanding positions in government and city-councils, the political élite was able to exploit its power and privilege to steadily expand and deepen its hold on the agricultural wealth of society (e.g. Bang, *The Roman Bazaar* (2008), ch. 2). Rent seeking of the Roman government and the landed aristocracy may be a very important part of the explanation why the Empire, and not only during Late Antiquity (*pace* Jongman, 597), was unable to derive full advantage from the blessings of peace. The reproduction of a top-heavy, or as Scheidel writes 'primate urban hierarchy' (78–9), under the early Empire is at one and the same time an index of the initial power of the Empire, through tribute evaluation, to mobilize economic resources and of its limits. The lop-sided concentration of demand in a small number of very large cities was uneconomic; it depended on political preference rather than economic concerns and saddled the economy with an extra burden. Imperial hegemony could, in other words, be seen as imposing a protection rent on the economy which tempered the productive effects of unification (cf. the general remarks of North, *Institutions, Institutional Change and Economic Performance* (1990), 14, 67; *Understanding the Process of Economic Change* (2005), 114). Rather than fitting the scenario described by North (and Thomas) for early modern Europe (see Section II above), the Roman case may come closer to his interpretation of Latin American societies where the prevailing conditions eventually slowed down growth: 'Under the Royal system, rights were granted to individuals and groups based on personal ties to the Crown. The result was huge land grants to wealthy individuals and the church; rights and privileges to the military ... In Latin America it produced neither economic collapse nor stagnation, but continuing instability, extensive rent seeking, political authoritarianism, adverse income distribution, and an inefficient provision of public goods, with slow economic growth' (2005, 113).

Where precisely to locate the Roman economy between these two poles, is not likely to see consensus emerge anytime soon. The great value of institutional economics to ancient history, however, is that it can accommodate these disagreements within a common conceptual framework. But even more significant, the outcome may very well turn out in favour of the unorthodox and unexpected. The biggest asset of institutional economics is precisely that it forces us to confront our habitual perceptions and rethink the way we organize the argument and construct explanations. Scheidel, Morris, Saller and their team of historians are to be congratulated on having launched ancient economic history on such a fascinating and fruitful intellectual journey.

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