



Review Article

Ancient inequality and economic growth

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ADAM S. GREEN, TOBY C. WILKINSON, DARRYL WILKINSON, NANCY HIGHCOCK & THOMAS P. LEPPARD. 2024. *Cities and citadels: an archaeology of inequality and economic growth*. Abingdon: Routledge; 978-1-032-02484-4 hardback £135 ebook OpenAccess <https://doi.org/10.4324/9781003183563>

The authors of this book are archaeologists who want to create a field they describe as ‘critical paleoeconomics’. Their quest is promising in several ways. For example, they are not averse to grand narratives and believe modern economic theory can offer insights into various features of ancient economies, including markets, trade, money and debt.

Adam Green *et al.* pose a central question: “What is it that Old Kingdom Egypt had that the Upper Paleolithic did not?” (p.25). Their answer is capital. The authors emphasise that capital does not consist of mere non-human objects such as gold or houses. Rather, “Capital is a *claim on something produced through someone else’s labor*” (p.26). They assert that “labor is the source of value” and that those who collect returns from capital are capturing labour they themselves did not perform (p.26). Their view that capital is inherently exploitative places the authors on the periphery of contemporary economics. I will comment more on this subject later, but for now it suffices to say that one can reject their labour theory of value while accepting the value of their archaeological contributions.

After an introduction describing their general approach, Green *et al.* argue in Chapter 2 that egalitarianism was the norm in early cities. They support this claim by pointing to the lack of palaces or monumental tombs and what they view as limited evidence for stratification during the early urbanisation of the Indus valley, Mesopotamia, the Sahel, the Andes and Mesoamerica. Green *et al.* emphasise Thomas Piketty’s (2014) finding that, over recent centuries, economic growth rates have been inversely correlated with inequality and hypothesise that early cities were egalitarian because they had high rates of economic growth.

To use Piketty’s ideas, the authors need to show not just that early cities had concentrated populations with large investments in housing and infrastructure, they must also show that such cities were places where output per person was rising (the conventional meaning of economic growth). This is hard to establish archaeologically. But it does make theoretical sense that an increase in the scale and diversity of craft manufacturing, perhaps together with long-distance trade, could have led to large urban settlements with rising living standards (at least in the short run; see the comments on population dynamics toward the end of this review).

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Chapter 3 introduces the concept of ‘citadels’, which contrast with early cities in several ways. They display clear stratification and are typically organised around elite residences; have monumental architecture; have relatively small populations in relation to land area; and possess large storage facilities to which access is tightly controlled. Green *et al.* provide archaeological examples from the Mediterranean, East Asia, Central Asia, southern Africa and the Americas. Most of the cases have two other interesting features in common: fortification and locations that are viable but marginal for the Neolithic package of domesticated plants and animals. The authors regard cities and citadels as distinct social developments, not stages in a neo-evolutionary scheme. In contrast to early cities, citadels were stratified because they had low rates of economic growth.

The alleged causal role of economic growth, again based on the work of Piketty (2014), leads to various problems and puzzles. First, Green *et al.* treat low agricultural productivity as an indicator of slow economic growth. But the former concept is a level while the latter is a rate of change. What the authors need is a positive linkage between the level of agricultural productivity and the rate of economic growth, but they do not give any theoretical justification for such a relationship. Second, slow economic growth cannot be a sufficient condition for stratification. If it were, most societies of the Upper Palaeolithic would have been stratified. Something more is required.

Green *et al.* do not clearly describe how elites accumulate wealth. They suggest that in some cases elites may collect tribute from commoners in (low productivity?) hinterlands, while in other cases elites may control craft manufacturing. Presumably these processes have coercive elements, but the nature of the coercion is not discussed. Given the prominence of fortifications and defensible locations, some citadels may have arisen through conflicts over land rent among rival elites but, again, no details are provided.

The closest the authors come to a general theory is to say that when economic growth is slow relative to returns on capital, wealth inequality tends to increase because stochastic events amplify existing wealth differentials. This is a reasonable starting point but requires a good deal of elaboration on the theory. For instance, what determines the rate of return on capital in the societies the authors discuss? The direction of causality can also be debated: stratification could cause slow growth rather than vice versa.

Chapter 4 covers a range of topics including money, debt, trade and taxation. Perhaps the most interesting hypothesis is that clay tokens in Neolithic West Asia could have recorded labour contributions to major collective projects in an egalitarian context, been redeemable for grain or other rewards, and circulated much like negotiable promissory notes.

Chapter 5 addresses the role of merchants in Bronze Age West Asia. Through a series of fascinating case studies (Ur III, Old Babylonia, Old Assyria, Ugarit), the authors make the point that merchants whose trading activities crossed political borders were deeply enmeshed with the state (i.e. the palace and/or temple). Merchants and states often exchanged goods and had credit arrangements. Some merchants even collected taxes or imposed capital punishment. While the authors stress the lack of sharp boundaries between the public and private sectors, they conclude that in these societies merchants always acted “within the power structures of any given urban community” (pp.180–81).

Chapter 6 documents that inequality, measured by ratios of land ownership or financial wealth between the very rich and everyone else, was much greater in classical Rome and

China than in the Bronze Age. The authors refer to this as the development of oligarchy, meaning the existence of a few private individuals with economic resources rivalling those of the state. They suggest two main reasons for this development: expansion in the size of cities and the spread of coinage. These factors facilitated speculation on urban land markets, which led to more wealth inequality than was possible through agricultural landlordism.

This is an intriguing hypothesis, and it could be generalised as follows. The evolution of asset markets enabled the rich to speculate on real estate, outcomes of warfare and technological innovations, and the statistical nature of such processes led to highly skewed wealth distributions where the very lucky did very well. For example, with Gibrat's Law that the rate of proportional growth is independent of absolute size, the system converges to a log-normal wealth distribution.

The authors close in Chapter 7 with broad remarks on potential future research directions. I limit my comments here to one point. The authors criticise what they call the "highly relativist view" of Graeber and Wengrow (2021), who argue that "[t]he only limits on the shape of human society stem from our own imagination" (p.219). Green *et al.* are deeply dismayed by this retreat from "any concern with materiality" (p.220) and cite climate change as a case of material constraints on human systems. I could not agree more. Having said that, I turn to a few areas of disagreement.

A central feature of the book is that it adopts a naive labour theory of value and it treats all capital as exploitative. This view is problematic. Suppose one workers' co-operative constructs machines by hand. Another workers' co-op buys one of these machines at a price that reflects the labour time of the first co-op, where the wealth used to buy the machine comes from savings out of previous labour income. The second co-op uses the machine to produce widgets, which are sold to consumers. Green *et al.* would either have to say that the second co-op exploits the first or deny that the machine is a form of capital. Neither interpretation is analytically useful.

Furthermore, labour values can be linked to prices in a coherent way only when there is a single non-produced input (labour). With two or more non-produced inputs, such as labour and land, it is no longer possible to translate labour values into the prices at which people buy and sell goods. But useful things are always created by applying human labour to natural resources. Because the labour theory of value ignores nature, it precludes any theoretical role for climate, geography or ecosystems in explanations of early inequality.

Another problem is that the authors disregard the endogeneity of population. Older archaeological literature often treated 'population pressure' as an exogenous variable that caused the development of agriculture, inequality, cities or states. But, over archaeological time scales, improvements in climate or technology raise food per capita and therefore raise fertility, leading to population growth. Conversely, deterioration in climate or technology lowers food per capita and therefore fertility, resulting in population decline. These dynamics are well-documented for societies ranging from the Upper Palaeolithic to the early stages of the Industrial Revolution (see Dow & Reed 2022, chapters 2 and 3).

In such societies food per capita (or more broadly, income per capita) tends to be roughly constant in the long run. Elites may have high incomes while commoners have low incomes, but income distribution is a zero-sum game, and technological progress usually makes commoners poorer in the long run (Dow & Reed 2023). Green *et al.* say very little about how population dynamics influenced early inequality. This is unfortunate because economic

growth is defined by a rising ratio of total income or output to population size. The crucial role of population in the denominator cannot be overlooked.

Finally, readers are cautioned that Green *et al.* paint a distorted picture of what they call ‘mainstream economics’. They exaggerate the influence of ‘neoliberalism’, which they define as the belief that governments should not intervene in markets, while underestimating the value of mainstream economics as a source of insights into prehistory. Economists have accumulated a large amount of literature on the origins of agriculture, inequality and states. Some of the work along these lines has a critical flavour: for example, models where elites exploit commoners by extracting land rent without contributing anything of social value (Dow & Reed 2023). This literature is not mentioned in the book by Green *et al.*, but discussions and citations are available in Gregory Dow and Clyde Reed (2022). Archaeologists who have the patience to dig through the mainstream economic literature could unearth some valuable artefacts.

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