

It is clear that use of the term ‘anomaly’ here refers to the specific framework of microhistory, and that the authors themselves do not view Athenian citizenship as a standard norm against which all other modes of belonging should be measured. But such a presentation of contrasting examples is a reminder of how deeply ingrained a Greek- and Roman-centric perspective still is. Riva and Grau Mira acknowledge the depth of this challenge themselves – they note, ‘This process of decentring and decolonization . . . has been put in jeopardy by recent Big History studies of long-term Mediterranean trajectories where the grand narrative’s preference for integration is largely for the Graeco-Roman world and the east of the basin’. The implication is that the Greek and Roman worlds remain at the centre, and ‘new additions’ made in the name of decolonization or decentring must be integrated with them, instead of the reverse.

Riva and Grau Mira’s emphasis is quite rightly placed on the critical contributions of microhistorical archaeology; the degree to which Mediterranean archaeology has been colonized by our obsession with Greece and Rome (Dietler 2005), however, means that many of the themes and phenomena explored by a global archaeology will have been established within the same heavily biased context. That is to say, they have been identified *because* of their relevance to Greece and Rome. Without great care, the exercise in one-sided integration seems likely to repeat itself under a slightly different guise. Riva and Grau Mira are, of course, no strangers to this issue either. They note that their analysis of citizenship in south-eastern Iberia is only possible because notions of Athenian citizenship have been dramatically overhauled in recent years. Even so, we are left considering south-eastern Iberian as belonging as part of a much broader, more socially rooted form of ‘citizenship’ instead of discussing Athenian citizenship as one form of collective belonging exhibited more broadly in urbanizing contexts. The difference is subtle, but the implications are great.

I do not mean to suggest that Mediterranean-wide comparison is impossible; on the contrary, it is essential. But perhaps a modified structure would be more fruitful. Instead of comparing seemingly ‘anomalous’ micro-scale examples to sweeping trends, like might be paired with like, and *comparanda* could be limited to equally microscopic case studies, evaluated through a shared bottom-up process. By introducing data from traditionally marginalized regions and contexts into direct conversation with Greek and Roman materials (or even eschewing them altogether), a more balanced knowledge baseline might be established. From that baseline, new themes and phenomena may be identified that hold more equal relevance for all Mediterranean regions. Once such a knowledge landscape has been established – one that is less overtly colonized by its very nature – a decolonized global archaeology of the 1st-millennium Mediterranean may be a realistic goal.

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From the Axial Age to the Fifth Sun. The articulation of the local with the global

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In 1949, Karl Jaspers published his enduring book *Vom Ursprung und Ziel der Geschichte*, a volume which set up one classic model of globalism and the local (Jaspers 1949; 1953). It is a book which posits a global, but causationally disconnected, transformation of the Eurasian world

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of the 1st millennium B.C., when society moved from the archaic to the modern, accompanied by new forms of political production, thought and religion. This was a book by a philosopher rather than a historian, one that deployed generalities rather than details provided by archaeological evidence. The concept of the axial age has had a surprisingly long innings, because of the attraction of a global theory, even though often under attack for the lack of fit both spatially and chronologically when confronted with archaeological data (Spinney 2019). One of its major attractions is that it provides a flexible (perhaps more properly fuzzy), generalized model that can become the testing ground for Big Data. The associated big question is whether archaeological data can meaningfully address such global matters when the zone of operation is often the local region.

In 2019, seventy years later, Camilla Townsend took the local perspective of a global process, a series of events, in a continent neglected by Jaspers. It is a book (Townsend 2019) by a historian that has given voice back to the indigenous inhabitants of Central America, by interrogating the native accounts, written down in the Nahuatl language with the new technologies provided by the conquistadores. This account reveals the agency of the local communities in their interaction with the incomers, an agency that was considerable in spite of the impact of disease and new coercive technologies. The indigenous are revealed as multifaceted political strategists, counting amongst their number 'Phoenician' Chontal Maya, the ruling Mexica and opportunistic Tlaxcalans. The analysis is accompanied by a critique of the narrow-minded use of archaeology and the Spanish sources, posing an alternative history of the local and its many scales, layers and transformations, notably when African slaves were later added to the political mosaic.

These two introductory texts set challenges for archaeologists. One such challenge for archaeology is that textual historians are sometimes inclined to see us as the foot soldiers, rather than the generals of strategy (Garnsey, Hopkins and Whittaker 1981). More recently, non-archaeologists as diverse as Abulafia (2011) and Belich, Darwin and Wickham (2016) have defined global trends that need to be assessed by primary archaeological data. All efforts to counter the impression of the historians (as by Riva and Grau Mira) are to be welcomed and the construction of grand global narratives undertaken by archaeologists is strongly to be encouraged, in the spirit of Broodbank (2013) and Morris (2013) (both nevertheless trained initially as historians). The sceptical perspective of the historians is enhanced by the fact that many of us are fieldworkers with the ability to bivouac and nestle comfortably in our microregions, and are sometimes vulnerable to accusations of apparent lack of concern for how our excavation or field survey fits into a wider pattern. A strong argument for the primacy of our efforts is that our profession has a greater understanding of the data, and more specifically of the formation processes of settlements, cemeteries and land use where the building blocks of material culture find context.

Indeed, one of the major lessons offered by those who sample the evidence is the variability of its quality. In the current pandemic, we have encountered many lessons of data quality, where theoretical modelling based on initially flimsy evidence has driven policy response. The work of the statistician Spiegelhalter has been frank and honest (see www.bbc.co.uk/programmes/m0014644). Archaeologists are similarly well practised in messy, fuzzy data, and for this reason their skills are undervalued. I remember anecdotally a seminar by Andrew Sherratt in around 1992, in front of modern geographers in Oxford, where he asked his audience to identify the key factors when dealing with archaeological data. His answer was that the recovered sample was fragmentary and required knowledge, skill and experience to avoid substantial overinterpretation (cf. Townsend's (2019, 213) discussion of metadiscourse of historical data).

A similarly enduring thread of good archaeological research, particularly that inspired by landscape approaches, has equally been to study the combination of scales, as properly professed here by Riva and Grau Mira. In a thematic review of landscape articles from antiquity, it became clear that a range of scholars who differed substantially in their theoretical outlook shared the integration of spatial scales into one interpretive pattern (Stoddart 2000, 3). The classic article of the Glastonbury Lake Village, way back in 1972, even if subjected to later empirical critique, already

had this sense of the contribution of different scales very much at its heart (Clarke 1972). So the important theme presented by Riva and Grau Mira has a long historiography.

One accessible archaeological element of scale is the settlement, or focus of activity, a central theme already in David Clarke's approach, to which can be added the layers of experience of landscape from other locales. The detailed definition of what is meant locally by settlement is a central route to the comparative approach, and one that I found relatively missing in Riva and Grau Mira's account, which was centred around the cemetery, part of the lived experience, but only a partial proxy of the understanding of scales of experiences, perhaps more cosmological than practical.

Another crucial archaeological element is a focus on temporality (cf. Townsend 2019). Many non-textual global accounts can be frozen methodologically into a *longue durée*. Cemeteries often have greater temporality than settlements, but are largely abodes of memory as much as lived practice. In the 1st millennium B.C., the application of radiocarbon is less habitual than in deeper periods of prehistory, hindered by the Hallstatt plateau in the radiocarbon curve. However, new approaches can begin to tease away at the margins of this plateau to uncover new unsuspected temporalities of demography, matched by climatic studies (Parkinson *et al.* 2021; Palmisano *et al.* 2021).

A major focus of the global is the comparative. For this purpose, a quantitative element, perhaps more easily found in the settlement, needs to be combined with the qualitative, perhaps more easily found in the cemetery and the sanctuary. The modelling of settlement size and density across landscape allows a comparison even beyond the confines of the Mediterranean. The areas covered here are rarely part of the broader global debates about the development of complexity which tend to be dominated by the examples of Greece and Rome, strong examples of qualitative evidence ignoring the important examples of Spain, southern France, central and northern Italy and south-west Germany, where a balance with quantitative evidence can be achieved. What is needed is comparative study of rural settlement and the relationship between major centres and their hinterlands. This allows a proper integration of top-down and bottom-up analysis.

The Riva and Mira Grau essay concentrates on the Mediterranean proper, alluding to the relationship to the east, but focuses on the west. In this immediate context, it is worth adding the extension of the same approach to Gaul, Etruria and relationships with the northern rim and beyond. Recent work on Etruria has shown the diversity of the local by combining scales that include both the large urban centres and the rural landscape (Stoddart 2020; Stoddart *et al.* 2020). Here the powerful tool of surface survey has been energetically engaged. In southern France, the work of Dietler (2010) has addressed this multi-scalar approach, combining all facets of landscape with the distribution of material culture. We can also take other avenues north, following the example of Zamboni (2021). In this region of northern Italy, the sampling strategy has been substantially from state archaeology and we find the large nucleations of population and the cemeteries, but the rural settlement is less developed. One further deep historiographical debate is over the relationship of the Mediterranean to central Europe. More detailed studies of the local scales of these regions are permitting an effective and coherent assessment of old models of core and periphery, and replacing them with subtler post-colonial understandings of the interrelationships, extending the argument of Riva and Grau Mira to other regions.

These examples, and that of Riva and Grau Mira, show that archaeologists can indeed meet the challenge set by philosophers and historians in writing grand narratives that pay equal attention to the global and the local. We nevertheless need to respond to the precise challenges of Jaspers and Townsend. The Jaspers model may be flawed, but it shows ambition in operating above the level of the local. The Townsend methodology may have a certain level of rhetoric, and the accounts of the indigenous in many parts of the Mediterranean may be lost, but we do need to search out the agency of the local to a level that has so far been underdeveloped in archaeological research.

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Balancing macro- and micro-scales in global-context understanding

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In our current era of decolonial reflection, scholarship continues to reckon with the legacy of the Graeco-Roman world. Our recognition of colonialist perspectives by past scholars, who sought parallels for Europe's own empirical activities, has been a substantial driving force for several decades now in changing how we reinterpret the nature and impact of Greek and Roman expansions from our material, visual and literary data sets. We have moved from regarding adoption and adaptation of Greek or Roman sociocultural features as cultural imitation, through a phase of considering variabilities in such alleged emulation as evidence of indigenous agency and hybrid cultural practices, rather than as erroneous practices, to one that seeks to rehabilitate both sets of evidence – those shared practices that were used to support colonial interpretations of aspirational cultural elevation as well as the practice diversities now regarded as evidence of blended, agentic developments.

This is the essence of current globalization theory as applied to our interpretations of the past. Its utility is predicated upon quantitatively substantial and qualitatively robust evidence, which is increasingly available to us, and it is driven by wider recognition of the importance of the past for economic, environmental, cultural and political understandings of and development in our present circumstances. It is one of the reasons why our interpretations pertaining to Mediterranean mobilities and their immediate and long-term impacts have evolved. Nevertheless, Riva and Grau Mira suggest that the Mediterranean of the 1st millennium B.C. is an outright casualty of this trend. They argue that while our consideration of the region during this period has matured to integrate areas marginal to the eastern-centric and Graeco-Roman focus of previous scholarly eras, our ability to understand the relationship of these regions from their perspectives has been put in jeopardy by current Big History studies of long-term Mediterranean trajectories, where the grand narrative's integration preference still favours the Graeco-Roman world, or eastern colonial undertakings (e.g. Phoenician settlement in the wider Mediterranean). They insist that the disparate quantity of material and built-environmental evidence around the Mediterranean basin reinforces the impression that the classical-world record is of higher quality, when in fact it is often more a reflection of long excavation history and investment. As a result, the authors contend there is a risk that global studies may reinforce prevailing Graeco-Roman exceptionalism.

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