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Debate Article

Archaeology, relevance and science

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I thank the four respondents for engaging with my remarks (Smith 2021) and for providing excellent insights into the question of relevance in archaeology. I find the diversity of examples refreshing and invigorating, and have already benefitted from reading some of the work cited in the responses. Here, I will provide brief remarks on the major themes. Kathleen Morrison's palaeoenvironmental research in India, and its intersection with the LandCover6k working group (Morrison *et al.* 2021), is exciting and aligns well with the middle-range concepts that I emphasise. Morrison (2021: 1071), however, seems dismissive of my emphasis on a scientific approach, suggesting that "it is time to retire this worn-out trope", and urges archaeologists to "leave the science wars behind us and get on with the job" (2021: 1072). The other three authors join Morrison in expressing dissatisfaction with my strong emphasis on a scientific epistemology for research that can tie in with contemporary concerns. I do not, however, claim that science is the only way to address questions of archaeological relevance. When Shadreck Chirikure (2021) and Karina Croucher (2021) take me to task with respect to science, they are dealing more with the kinds of activities that I include under relevance types one (heritage) and two (local practical topics). I am less familiar with work on these topics, and I have no desire to argue that colleagues working in these areas should avoid research that is not science.

Morrison's impatience with my scientific stance seems distinct, however, and I wonder whether it derives from differences between the environmental sciences and the social sciences. It is difficult to imagine an ecologist or earth scientist claiming that past environments are irrelevant to the concerns of today because they were so different. Yet this is the dominant view in the social sciences. Ancient and non-Western societies are seen as so radically different that they can generate no insights into contemporary patterns. Given the growing dominance of quantification in the social sciences, it seems clear to me that any actionable insights from archaeology will need to be quantitative and scientific in nature.

Yet many archaeologists working on social-science-related topics do not feel obliged to pursue routinely the level of quantification that Morrison probably uses. Some archaeologists, for example, promote the concept of 'low-density urbanism', but they do not actually calculate any densities. That such work is published in our major journals (e.g. Gaydarska *et al.* 2020) suggests that non-quantified density claims are acceptable in archaeology. Certainly, there are aspects of urbanism whose study do not require quantified data. But, I argue that if one is going to discuss a topic such as urban density—a theme of great theoretical and empirical importance (Angel *et al.* 2021)—one needs to do some measurement and mathematics.

I like Paul Lane's (2021: 1079) suggestion that archaeological data could be well-suited to identify some of the "known unknowns" in general scholarship. For an example of this, my

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colleagues and I proposed that archaeological data may provide a level of clarity concerning the boundaries of cities that is lacking in research on modern cities (Lobo *et al.* 2020). Unfortunately, we have yet to see the flood of attention from urban scholars that we think our findings merit!

Chirikure's (2021) discussion on archaeological relevance in Africa adds a valuable perspective, and he makes a number of useful observations. I must disagree, however, with his claim that scientific rigour is a luxury that will not help to reduce global inequalities and exploitation. Without a broad scientific understanding of wealth inequality (to select just one example), it is hard to imagine how inequities can be adequately addressed. Indeed, one of the most explicitly scientific and quantitative approaches I have been involved with—settlement scaling (Lobo *et al.* 2020)—is being applied to improve living conditions in slums in the developing world (Brelsford *et al.* 2018). This work is being co-produced with slum dwellers (Bettencourt *et al.* 2018). While archaeological inputs do not feed directly into the slum component, they do provide quantitative evidence for the near-universality of certain scaling relationships within settlement systems (and thus contest vague, data-free claims of a deep gulf between ancient and modern cities).

Croucher, in her Continuing Bonds project, approaches relevance from a transdisciplinary perspective anchored more in emotion and meaning than in economic and social structure—a valuable contribution to my first two types of relevance. I must, however, question her use of the term "pigeonholing" (Croucher 2021: 1081) to characterise my typology of relevance. To pigeonhole is to make a bad typology, or to carry out a poor classification. I included the table in order to show types of potential relevance for archaeological knowledge that I deliberately excluded from my article, not to reveal deep truths about the nature of reality. I am sure this typology can be improved, and I would welcome such a development.

The construction of relevance for archaeological data is an ongoing task. These four respondents, like many of us, are engaging with the actual and potential uses of our data that go far beyond our specific archaeological findings from individual places. Explicit discussion and debate such as this are important as we all work to move forward such agendas.

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