Debate Article

Enhancing archaeology's role in addressing grand challenges needs more reflection on known unknowns

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Identification of the relevance of archaeology has been a recurrent concern in the discipline since at least the 1930s (Clark 1939), and which has often played out in the pages of *Antiquity*. In British contexts, the shifts in fashion and emphasis, much like the changing interpretations of Stonehenge, undoubtedly reflect wider concerns of the time. These extend from the role that archaeological knowledge might play in countering the rise of fascism in Europe, and the racist and antisemitic ideologies that underpinned it (Childe 1933), to the more recent interest in the discipline's contributions to understanding rapid climate change (Mitchell 2008).

Public anxieties over the existential threat posed by climate change, reinforced by the dystopian futures promoted by the entertainment industry, contemporary fiction and popular culture (Trexler 2015; Bulfin 2017), and coupled with the rise in funding opportunities, has led to a proliferation of reflections on how archaeology and cognate historical disciplines might contribute to addressing these grand challenges (e.g. van der Leeuw *et al.* 2011; Kintigh *et al.* 2014; Armstrong *et al.* 2017; Pétursdóttir 2017; González-Ruibal 2018; Jackson *et al.* 2018; Fisher 2020). In this regard, Smith's (2021) commentary provides a timely reminder of just how challenging it is to communicate these messages to scholars trained in other fields, and especially those working in applied and future-oriented disciplines—let alone seeing archaeologically informed perspectives entering policy and mainstream scientific discourse on the future of our planet. Recognising that efforts to demonstrate the relevance of archaeology to addressing these grand challenges range from those with "a local, place-based focus" to ones that offer "abstract and broader perspectives", Smith (2021: 1061) is particularly interested in approaches that address mid-range empirical and conceptual issues. He identifies three broad impediments that limit the incorporation of archaeological insights into policy. There is a naïve view among archaeologists of how 'relevance' works; insufficient emphasis on rigorous, scientific methods to generate empirical findings; and a confusion about target audiences and how to reach them. Citing a recent article on the shifting interest in archaeological insights and the weight given to them by the Intergovernmental Panel on Climate Change since its establishment in 1988 (Kohler & Rockman 2020) in support of his argument, Smith argues for better and more consistent communication across disciplines. In particular, he argues for the need to communicate directly with "scientists working in the target areas" (Smith 2021: 1064) rather than with the policymakers themselves, on the grounds that the latter are unlikely to listen to archaeologists. He further argues for the generation of archaeological insights that can be shown

to be "credible, salient and legitimate" (Smith 2021: 1064), and for greater engagement on the part of archaeologists in collaborative, transdisciplinary research.

On one level I am in broad agreement with much of what Smith has to say. I have argued elsewhere (Lane 2015) that while the deep-time perspective of archaeology has much to offer to collective efforts aimed at creating more equitable, environmentally sustainable futures, there is also a need to recognise the limitations of our field (and expertise) in this regard. There is also a growing corpus of examples of good practice and research philosophies (Kaufman *et al.* 2018) to draw on—and here I would single out several of the longer-term projects, such as those on Iceland and in Mesoamerica, that have developed under the umbrella of the Integrated History and future of People on Earth (IHOPE) consortium of researchers (https://ihopenet.org).

Yet, in some respects, the bigger challenge may be not so much that of finding ways to encourage climate modellers, development economists, conservation ecologists, urban planners and others whose work receives more prominence in policy formulation to adopt longer-term perspectives on the problems they are seeking to address—important though this is. Rather, what is needed if archaeologists are genuinely to help shape the content and direction of policy is greater attention to the many 'known unknowns' embedded in existing policy, along with careful and self-critical consideration of whether archaeological knowledge can help elucidate them. Research on evidence-based policy, or 'evidence-informed policy', as is increasingly preferred in recognition of the many other factors that contribute to policy formulation and implementation (Nevo & Slonim-Nevo 2010; Pearce *et al.* 2014; Pallett 2020; Smith-Merry 2020), highlights both the importance of knowledge brokerage in the policy process (Bandola-Gill & Lyall 2017) and the extent to which such brokerage is based on multiple uncertainties concerning the evidence that might be mobilised to design a policy intervention (Pawson *et al.* 2011). Although the case study that Pawson and colleagues outline provides an in-depth analysis of the uncertainties relating to a public health issue, as the authors note, the nature of the predicament is the same for any evidence-based process of policy formulation.

What this implies for archaeologists hoping that their research can contribute to addressing some of the grand challenges is the importance of identifying which of the associated 'known unknowns' might be transformed into 'known knowns' in light of archaeological evidence. Sadly, for the most part, archaeologists have yet to identify many such known unknowns that may be amenable to conversion into known knowns through their endeavours. One example that seems to achieve this is the work of the LandUse6K research group that aims to generate knowledge of the changing proportions of different kinds of land use at different points in time over the last 6000 years, in a manner that makes the data readily usable by climate modellers (Morrison *et al.* 2021). For many of the other grand challenges, it seems that we are still a long way from identifying in any precise way how our archaeological knowledge might be both usable and useful. While Smith's (2021) contribution certainly helps us to recognise some of the reasons for this, we still have more work to do if we are not to become mired in a swamp of 'unknown unknowns' and, instead, to produce meaningful and effective answers to those who ask, rhetorically or not, 'what is to be done?'.

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