




## Debate Article

# Why archaeology's relevance to global challenges has not been recognised

Michael E. Smith\* 

\* School of Human Evolution & Social Change, Arizona State University, USA (✉ [mesmith9@asu.edu](mailto:mesmith9@asu.edu))

Archaeologists are increasingly publishing articles proclaiming the relevance of our field for contemporary global challenges, yet our research has little impact on other disciplines or on policy-making. Here, the author discusses three reasons for this impasse in relevance: archaeologists do not understand how relevance is constructed between fields; too little of our work follows a rigorous scientific epistemology; and we are confused about the target audiences for our messages concerning our discipline's relevance. The author suggests two strategies for moving forward: transdisciplinary collaborative research and the production of quantitative scientific results that will be useful to scientists in disciplines more closely involved in today's global challenges.

Keywords: archaeology, relevance, policy-making, interdisciplinary, transdisciplinarity

## Introduction

A steady stream of publications enthusiastically proclaims the relevance of archaeology for important contemporary global challenges, while at the same time despairing that those outside of the discipline seem indifferent (e.g. Sabloff 2008; Smith 2010a; Chase & Scarborough 2014; Kintigh *et al.* 2014; Kerr 2020; Scarborough & Isendahl 2020; Boivin & Crowther 2021). If our work really does have widely appreciated relevance to broader domains, we would not have to publish such proclamations. We would be busy collaborating with other scholars, stakeholders and policy-makers rather than trying repeatedly to convince ourselves of the relevance of our research. So, what is preventing others from appreciating what we seem to accept on faith—that our work is both relevant and useful? The main hurdle is that we have not produced rigorous, quantitative data that can be understood and analysed by scientists in the relevant domains. I should note here that not all archaeology is or should be relevant to other fields, and I do not want to suggest that relevance be the major criterion for the evaluation of archaeological research.

## The relevance of archaeology

Archaeology can be relevant to people and issues outside of the archaeological community in a number of diverse ways (Table 1). Four types of relevance are arranged in order from a local, place-based focus at the top of the table to abstract and broader perspectives at the bottom.

---

Received: 13 September 2020; Revised: 11 December 2020; Accepted: 18 December 2020

© The Author(s), 2021. Published by Cambridge University Press on behalf of Antiquity Publications Ltd.

Table 1. Types of relevance for archaeology, with examples.

---

<b>1</b>	<b>Heritage or descendant concerns</b> Sacred places or shrines Identity, history and human remains Land claims and resource rights Symbolically important relics
<b>2</b>	<b>Local practical topics</b> Small-scale agricultural methods Small-scale water supply systems
<b>3</b>	<b>Middle-range empirical and conceptual topics</b> Economic systems and institutions Urban neighbourhoods or city size Social inequality Urban sustainability
<b>4</b>	<b>Abstract conceptual topics</b> Resilience theory Fairness and justice Agency and practice

---

- (1) *Heritage or descendant concerns* are usually localised to a specific area (Watkins 2005).
- (2) *Local practical topics* include work—often labelled ‘applied archaeology’ (Isendahl & Stump 2019)—that revives ancient farming systems or other technological features for use today (Erickson 1998; Stump 2013). Much of the impact of these two forms of relevance derives from collaboration between archaeologists and local communities (Little & Shackel 2007). The term for such collaborations in the scientific literature is the ‘coproduction of knowledge’ (Kirchhoff *et al.* 2013; Mach *et al.* 2020). As the above citations show, archaeologists have some experience in these two crucial types of relevance construction, and they are currently both targets of extensive and ongoing research.
- (3) *Middle-range empirical and conceptual issues*—that form the focus of this article—are an area where archaeologists seem to be struggling: eager to contribute to global issues, but unable to comprehend how this might work in practice. Here, I use the term ‘middle-range’ to refer to the sociologist Robert Merton’s (1968) concept of that name, rather than Lewis Binford’s (1983) alternative definition that involves formation processes. Merton’s approach to theory is summarised by Sampson (2010: 72) as “not mindless empiricism and not abstract theory or theory about other theorists. Merton developed theory about how the world works”. The middle-range issues listed in Table 1 are subjects of large bodies of research in the social sciences, to which archaeological contributions are only nascent.
- (4) *Abstract conceptual topics* relate to relevance on a highly abstract, even philosophical, level. In most epistemological hierarchies, abstract

theory and concepts are very general, pertaining to a broad range of phenomena, but are uninformative about the specifics of individual settings or cases (Sampson 2010; Smith 2017). Any discussion of archaeological relevance at this level must, by necessity, be very general and abstract (e.g. Nelson *et al.* 2012).

This article highlights the role of three factors in creating and maintaining our current impasse on middle-range constructions of relevance: a naïve view of how ‘relevance’ works; insufficient emphasis on rigorous, scientific methods; and a confusion about target audiences and how to reach them. My primary message is that articles—such as this one—published in archaeology journals for an audience of archaeologists, will solve none of these problems. Within a discipline, authors, reviewers, editors and readers construct the relevance or usefulness of knowledge jointly, but in moving from one discipline or domain to another, the construction of relevance becomes a very different process. Briefly, one cannot judge one’s own relevance to a different domain. Kieser and Leiner (2011) find that, in the field of management, the nature of relevance differs between researchers and practitioners. Here, it is the users of knowledge—the practitioners—and not the producers of knowledge—the researchers—who decide the relevance of research: “Only practitioners are in a position to authentically attribute relevance to knowledge” (Kieser & Leiner 2011: 897).

Suppose I think that archaeological findings—on ancient urban neighbourhoods, for example (Smith 2010b)—ought to be relevant for understanding urban neighbourhoods in contemporary cities. Nobody working on present-day neighbourhoods cares what archaeologists might think about the possible relevance of their research. But if a sociologist were to interact with archaeologists and learn a little about ancient neighbourhoods, then he or she might decide that archaeological findings do have relevance to some domain of knowledge on modern cities. If so, they might cite the relevant archaeological publications and use archaeological results in their own research. Only they can judge the relevance; the archaeologists of ancient cities cannot.

Consider the opposite situation: is research on contemporary Chicago neighbourhoods relevant to understanding ancient cities? If a sociologist were to claim some archaeological relevance for these Chicago neighbourhoods, most of us would just shrug our shoulders—what does a sociologist know about the distant past? But if I were to read modern neighbourhood research, and find it relevant and useful for my own studies of early cities, then such relevance can make a difference: it might motivate a new type of analysis or interpretation. Put simply, one cannot judge the relevance of one’s own research to other domains. The relevance of our work as archaeologists can only be determined from the non-archaeological domains to which such relevance may apply.

The creation of relevance relies on the communication of scientific or scholarly findings across disciplines; this is a well-studied process. Fischhoff (2013: 14033–34), for example, has written that “Because science communication seeks to inform decision making, it must begin by listening to its audience, to identify the decisions that its members face—and, therefore, the information that they need”. Hence, archaeologists need to stop seeking to convince one another that our work is relevant to vague, unspecified, external domains, and start doing

what needs to be done to inform others about our research, and to allow them to appreciate the existence and nature of any relevance.

If archaeologists are going to help establish our relevance for confronting global challenges (Kintigh *et al.* 2014), we must communicate with scientists working in the target areas, and/or with policy-makers. In either case, the first requirement is that archaeologists conduct rigorous research that is recognised as valid by other scientists. While approaches that lack a scientific epistemology are acceptable for many archaeologists, scientists in other fields and policy-making establishments are looking for reliable results, backed up by rigorous methods and an explicitly scientific research design (Kohler & Rockman 2020). Those external parties do not care about, for example, currently fashionable post-humanist theory in archaeology.

I have previously argued that archaeologists should look to the social sciences for models of how to become more rigorous and scientific (Smith 2017). In contrast to the cartoon-like caricature of science promoted by some archaeological theoreticians—in which science consists of experiments and covering laws (Smith 2017: 521–22)—the social sciences have scientific methods and epistemologies designed for disciplines studying human society. Social science methodologist John Gerring cautions against using unscientific methods when the goal is to understand and change society:

*The purpose of social science, let us say, is to help citizens and policymakers better understand the world, with an eye to changing that world. Social science ought to provide useful answers to useful questions [...] The wilful avoidance of scientific methodology has doleful long-term consequences for social science, and for those who would see social science playing a role in the transformation of society.* (Gerring 2012: 396 & 399)

In the field of sustainability science, the type of knowledge needed to effect change is referred to as ‘usable knowledge’ (Clark *et al.* 2016) or ‘actionable knowledge’ (Kirchhoff *et al.* 2013). Current consensus is that such knowledge must be credible, salient and legitimate (Cash *et al.* 2003; see also Koyama 2015; Kohler & Rockman 2020). Do we, as archaeologists, produce usable knowledge relevant to the ‘grand challenges’ of today? And, can we produce such knowledge in a manner that is both ethical (Hakenbeck 2019) and rigorous?

## Moving forward

If we assume that archaeologists can produce rigorous scientific results that researchers and practitioners in other disciplines are likely to accept as reliable, how can such results be communicated to those other fields? How will urban sociologists learn about archaeological work on ancient neighbourhoods? How will ecologists or climate scientists find out about archaeological research on specific environments? One promising approach is through transdisciplinary research. As this term has multiple definitions, and as archaeologists have yet to agree on a common meaning, it merits brief discussion. The term ‘interdisciplinary’ usually refers to bringing knowledge from one discipline into another. When I consult with specialists, for example, about the ancient vegetation or rocks at my archaeological site, this is considered ‘interdisciplinary’ research. ‘Transdisciplinary’, however, refers to research for which individuals collaborate deeply and learn elements of one another’s discipline. Erich Jantsch (1972:

104) describes transdisciplinary work as “a mutual interpenetration of disciplinary epistemologies”. In contrast to my site-based interdisciplinary consultations, my transdisciplinary research has required many months—even years—of discussion and analysis in order to achieve the kind of common understanding that can lead to conclusions not possible in the constituent disciplines (York *et al.* 2011; Lobo *et al.* 2020).

Transdisciplinary research is crucial, because many serious and challenging issues—from climate change adaptation to growing wealth inequality—cannot be answered by individual disciplines: “many, if not all, of the traditional approaches, as well as many heterodox tactics, fail to answer the most pressing issues plaguing the world” (Polimeni 2006: 2). I have observed two types of confusion about transdisciplinary research among archaeologists. First, a minority of authors employ the term transdisciplinary to indicate research that is relevant to society and that involves collaboration with stakeholders (e.g. Richer *et al.* 2019; see also Pohl *et al.* 2017). The appropriate term for such research in the sciences, however, is the ‘coproduction’ of knowledge (Kirchhoff *et al.* 2013). Second, many archaeologists confuse transdisciplinary and interdisciplinary research, claiming, for example, that as most archaeological projects are interdisciplinary this will somehow help archaeologists to achieve transdisciplinary results with scholars in other fields (e.g. Kerr 2020). If that were really the case, however, why have we not seen more examples of cross-disciplinary collaboration related to global challenges? The answer is that transdisciplinary research is difficult and time-consuming. In my own experience, interdisciplinary collaborations on fieldwork projects provided almost no help in initiating or conducting transdisciplinary research projects.

Transdisciplinarity requires archaeologists to learn what is going on in other fields. We need to read the journals, go to the conferences and—most importantly—talk to the social or natural scientists in the target field. If scientists from different disciplines want to collaborate ‘deeply’ with one another, they must learn what the others consider important and essential about a phenomenon of joint interest. Furthermore, they need to agree both on a question and on what would constitute an adequate answer. A lesson learned from decades’ of transdisciplinary research at the Santa Fe Institute is that this kind of collaboration—and the breakthroughs that can result (e.g. West *et al.* 1997)—requires researchers in divergent fields to use a common language. For most of the questions in complexity science, for example, that language is mathematics. Economic historian Mark Koyama (2015: 586) takes the authors of *The history manifesto* (Guldi & Armitage 2014) to task for making outdated and erroneous claims about other fields: “If historians are to play an important role in shaping attitudes towards inequality and climate change they must come to grips with what other social scientists say on these issues”. Archaeologists must also educate scholars in other disciplines about our work, and a productive way of doing this is to publish in their journals; this is now a growing trend that should pay dividends (e.g. Barthel & Isendahl 2013; Jackson *et al.* 2018; Lobo *et al.* 2020; Riede *et al.* 2020).

What if an archaeologist thinks that his or her results might be relevant to policy-making? Unfortunately, many archaeologists adhere to what van Langenhove (2011) calls the ‘limestone model’ of knowledge transfer. If we just publish our results in a readable fashion, they will eventually penetrate society, like water seeping through limestone; someone, somewhere, may find them and put them to work (e.g. Chase & Scarborough 2014). Research and practice relating to social-science policy, however, indicate that the road from scientific results

to social policy-making is long and complicated. In the words of policy expert Vivian Tseng (2012: 3), “Without a strong understanding of the worlds ‘on the other side’ of the gaps [between research and policy], however, scholars’ efforts to make research more useful and to communicate it more effectively run the risk of missing the mark”.

‘Evidence-based policy’ is one of the major approaches to the research-policy interface (Stoker & Evans 2016). Followers of this approach strive to incorporate research results into the policy-making process. Although the idea that policy should be based on research may sound unexceptional to scholars, research results rarely play a large role in the formulation and implementation of social policy. Critics of evidence-based policy do not denigrate the role of research, but instead point out that research is only one factor among many in the creation of policy. Nick Black (2001: 276), for example, has written that “Clearly, research has only a limited role because governance policies are driven by ideology, value judgments, financial stringency, economic theory, political expediency, and intellectual fashion”. Given this situation, policy experts such as Tseng (2012) and van Langenhove (2011) suggest a more active role for researchers in the policy process. Tseng (2012: 5) identifies the following policy-oriented users of social science research:

- Federal policy-makers.
- Frontline practitioners.
- State and local agencies.
- Mid-level administrators and programme managers.
- Advocacy groups and think tanks.

These users rarely obtain evidence and ideas directly from researchers. Instead, they source their information from intermediaries or translators. Researchers rarely have the skills or inclination—much less, the resources or professional rewards—to present their results in a format that can be interpreted and used by policy-makers. In the words of Tseng (2012: 12):

*Thus, we do not simply face a communications problem of better conveying research; nor is it merely a dissemination problem of better distributing research. Translation is critical, and we should reflect more intentionally on who makes for the best translators and how to create productive contexts for translation.*

Unfortunately, archaeology lacks the relevant intermediaries who can translate our research for the types of users listed above. Professional associations, such as the Society for American Archaeology or the European Association of Archaeologists, do not perform this service. While university public-relations offices and journalists translate archaeological results for non-professional audiences, they tend to emphasise sensationalist issues; they are not equipped to translate archaeology for policy-makers.

For archaeology, a potentially productive approach is to focus on the scientists in the target domains, rather than the policy-makers. This makes sense for both policy and science. Policy-makers on social issues are not going to pay attention to archaeologists. A local politician looking to design policy on, for example, urban neighbourhoods will look at work by sociologists and economists. If archaeologists can convince scholars in these fields that our data are

relevant and useful, we can play a small, indirect role in policy-making. Furthermore, the intellectual benefits of collaborating with scientists rather than policy-makers can be tremendous. This strategy can lead to transdisciplinary advances.

For archaeology, this approach—targeting scientists in other domains for collaboration in order to advance questions based on global challenges—has advanced furthest in the field of climate change and global warming. Kohler and Rockman (2020) detail the nature of involvement of archaeologists with the Intergovernmental Panel on Climate Change (see also Jackson *et al.* 2018; Rockman & Hritz 2020). While archaeology has made some contribution, its role is smaller than many archaeologists would like. A key observation made by Kohler and Rockman (2020) is that archaeologists must improve methodologically to be taken seriously by climate scientists in other disciplines. Minimally, we must contribute rigorous data—usually quantitative—based on adequate sampling and analysis.

## Conclusion

A growing number of archaeologists now believe that our work has relevance for issues and disciplines beyond archaeology (e.g. Smith 2010a; Kintigh *et al.* 2014; Ortman 2019; Kerr 2020), but putting this belief into action requires hard work (Stump 2010; Kohler & Rockman 2020). We must convince scientists and scholars in other disciplines that our data and models are useful and relevant to broader issues, and we need to be able to collaborate in transdisciplinary research from an appropriate scientific baseline. We cannot be content to keep telling ourselves, in journals only read by other archaeologists, that our results are relevant to global challenges. Our data *do* have relevance for a variety of contemporary global challenges, but that relevance will not be realised until we do the hard work of producing scientific results—including transdisciplinary research—and making sure that they reach the relevant social and natural scientists.

## Acknowledgements

Earlier versions of this article were presented at the 2017 Annual Meeting of the Society for American Archaeology and at a 2014 session of the iHOPE-Maya group. I thank the following colleagues for comments on an earlier draft: Caitlin Drummond, Nicolas Gauthier, Christian Isendahl, Keith Kintigh, Tim Kohler, José Lobo, Christopher Morehart and Scott Ortman. The comments of two anonymous reviewers contributed greatly to the clarification and strengthening the argument; such helpful reviewers are much appreciated.

## Funding statement

This research received no specific grant from any funding agency or from commercial and not-for-profit sectors.

## References

- BARTHEL, S. & C. ISENDAHL. 2013. Urban gardens, agriculture, and water management: sources of resilience for long-term food security in cities. *Ecological Economics* 86: 224–34. <https://doi.org/10.1016/j.ecolecon.2012.06.018>
- BINFORD, L.R. 1983. *In pursuit of the past: decoding the archaeological record*. New York: Thames & Hudson.
- BLACK, N. 2001. Evidence based policy: proceed with care. *British Medical Journal* 323: 275–78. <https://doi.org/10.1136/bmj.323.7307.275>

- BOIVIN, N. & A. CROWTHER. 2021. Mobilizing the past to shape a better Anthropocene. *Nature Ecology and Evolution* 5: 273–84.  
<https://doi.org/10.1038/s41559-020-01361-4>
- CASH, D.W., W.C. CLARK, F. ALCOCK, N.M. DICKSON, N. ECKLEY, D.H. GUSTON, J. JÄGER & R.B. MITCHELL. 2003. Knowledge systems for sustainable development. *Proceedings of the National Academy of Sciences of the USA* 100: 8086–91.  
<https://doi.org/10.1073/pnas.1231332100>
- CHASE, A.F. & V.L. SCARBOROUGH (ed.). 2014. *The resilience and vulnerability of ancient landscapes: transforming Maya archaeology through iHOPE*. Washington, D.C.: American Anthropological Association.
- CLARK, W.C., L. VAN KERKHOFF, L. LEBEL & G.C. GALLOPIN. 2016. Crafting usable knowledge for sustainable development. *Proceedings of the National Academy of Sciences of the USA* 113: 4570–78.  
<https://doi.org/10.1073/pnas.1601266113>
- ERICKSON, C.L. 1998. Applied archaeology and rural development: archaeology's potential contribution to the future, in M. Whiteford & S. Whiteform (ed.) *Crossing currents: continuity and change in Latin America*: 34–45. Upper Saddle River (NJ): Prentice-Hall.
- FISCHHOFF, B. 2013. The sciences of science communication. *Proceedings of the National Academy of Sciences of the USA* 110: 14033–39.  
<https://doi.org/10.1073/pnas.1213273110>
- GERRING, J. 2012. *Social science methodology: a unified framework*. New York: Cambridge University Press.  
<https://doi.org/10.1017/CBO9781139022224>
- GULDI, J. & D. ARMITAGE. 2014. *The history manifesto*. New York: Cambridge University Press. <https://doi.org/10.1017/9781139923880>
- HAKENBECK, S.E. 2019. Genetics, archaeology and the far right: an unholy trinity. *World Archaeology* 51: 517–27.  
<https://doi.org/10.1080/00438243.2019.1617189>
- ISENDAHL, C. & D. STUMP (ed.). 2019. *The Oxford handbook of historical ecology and applied archaeology*. Oxford: Oxford University Press.
- JACKSON, R.C., A.J. DUGMORE & F. RIEDE. 2018. Rediscovering lessons of adaptation from the past. *Global Environmental Change* 52: 58–65.  
<https://doi.org/10.1016/j.gloenvcha.2018.05.006>
- JANTSCH, E. 1972. Towards interdisciplinarity and transdisciplinarity in education and innovation, in L. Apostel, G. Berger, A. Briggs & G. Michaud (ed.) *Interdisciplinarity: problems of teaching and research in universities*: 970121. Paris: Centre for Educational Research and Innovation.
- KERR, S. 2020. The future of archaeology, interdisciplinarity and global challenges. *Antiquity* 94: 1337–48.  
<https://doi.org/10.15184/aqy.2020.138>
- KIESER, A. & L. LEINER. 2011. On the social construction of relevance: a rejoinder. *Journal of Management Studies* 48: 891–98.  
<https://doi.org/10.1111/j.1467-6486.2009.00886.x>
- KINTIGH, K.W. et al. 2014. Grand challenges for archaeology. *Proceedings of the National Academy of Sciences of the USA* 122: 879–80.  
<https://doi.org/10.1073/pnas.1324000111>
- KIRCHHOFF, C.J., M. CARMEN LEMOS & S. DESSAI. 2013. Actionable knowledge for environmental decision-making: broadening the usability of climate science. *Annual Review of Environment and Resources* 38: 393–414.  
<https://doi.org/10.1146/annurev-environ-022112-112828>
- KOHLER, T.A. & M. ROCKMAN. 2020. The IPCC: a primer for archaeologists. *American Antiquity* 85: 627–51. <https://doi.org/10.1017/aaq.2020.68>
- KOYAMA, M. 2015. *Review of The history manifesto, by Jo Guldi and David Armitage*. *Journal of Economic History* 75: 584–87.  
<https://doi.org/10.1017/S0022050715000741>
- VAN LANGENHOVE, L. 2011. Social sciences and policy impact: the case for a participatory approach, in G. Papanagnou (ed.) *Social science and policy challenges: democracy, values and capacities*: 95–111. Paris: UNESCO.
- LITTLE, B.J. & P.A. SHACKEL (ed.). 2007. *Archaeology as a tool of civic engagement*. Lanham (MD): AltaMira.
- LOBO, J., L.M.A. BETTENCOURT, S.G. ORTMAN & M.E. SMITH. 2020. Settlement scaling theory: bridging the study of ancient and contemporary urban systems. *Urban Studies* 57: 731–47.  
<https://doi.org/10.1177/0042098019873796>
- MACH, K.J. et al. 2020. Actionable knowledge and the art of engagement. *Current Opinion in Environmental Sustainability* 42: 30–37.  
<https://doi.org/10.1016/j.cosust.2020.01.002>



- MERTON, R.K. 1968. *Social theory and social structure*. New York: Free Press.
- NELSON, M.C. *et al.* 2012. Long-term vulnerability and resilience: three examples from archaeological study in the southwestern United States and northern Mexico, in J. Cooper & P. Sheets (ed.) *Surviving sudden environmental change*: 197–221. Boulder: University Press of Colorado.  
<https://doi.org/10.2307/j.ctt1wn0rbs.13>
- ORTMAN, S.G. 2019. A new kind of relevance for archaeology. *Frontiers in Digital Humanities* 6: 16. <https://doi.org/10.3389/fdigh.2019.00016>
- POHL, C., P. KRÜTLI & M. STAUFFACHER. 2017. Ten reflective steps for rendering research societally relevant. *GAIA—Ecological Perspectives for Science and Society* 26: 43–51.  
<https://doi.org/10.14512/gaia.26.1.10>
- POLIMENI, J.M. 2006. Transdisciplinary research: moving forward. *International Journal of Transdisciplinary Research* 1: 1–3.
- RICHER, S., D. STUMP & R. MARCHANT. 2019. Archaeology has no relevance. *Internet Archaeology* 53. <https://doi.org/10.11141/ia.53.2>
- RIEDE, F., G.L. BARNES, M.D. ELSON, G.A. OETELAAR, K.G. HOLMBERG & P. SHEETS. 2020. Prospects and pitfalls in integrating volcanology and archaeology: a review. *Journal of Volcanology and Geothermal Research* 401: 106977.  
<https://doi.org/10.1016/j.jvolgeores.2020.106977>
- ROCKMAN, M. & C. HRITZ. 2020. Expanding use of archaeology in climate change response by changing its social environment. *Proceedings of the National Academy of Sciences of the USA* 117: 8295–302.  
<https://doi.org/10.1073/pnas.1914213117>
- SABLOFF, J.A. 2008. *Archaeology matters: action archaeology in the modern world*. Walnut Creek (CA): Left Coast.
- SAMPSON, R.J. 2010. Eliding the theory/research and basic/applied divides: implications of Merton's 'Middle Range', in C. Calhoun (ed.) *Robert K. Merton: sociology of science and sociology as science*: 63–78. New York: Columbia University Press.  
<https://doi.org/10.7312/calh15112-003>
- SCARBOROUGH, V.L. & C. ISENDAHL. 2020. Distributed urban network systems in the tropical archaeological record: toward a model for urban sustainability in the era of climate change. *The Anthropocene Review* 7: 208–30.  
<https://doi.org/10.1177/2053019620919242>
- SMITH, M.E. 2010a. Sprawl, squatters, and sustainable cities: can archaeological data shed light on modern urban issues? *Cambridge Archaeological Journal* 20: 229–53.  
<https://doi.org/10.1017/S0959774310000259>
- 2010b. The archaeological study of neighborhoods and districts in ancient cities. *Journal of Anthropological Archaeology* 29: 137–54.  
<https://doi.org/10.1016/j.jaa.2010.01.001>
- 2017. Social science and archaeological inquiry. *Antiquity* 91: 520–28.  
<https://doi.org/10.15184/aqy.2017.19>
- STOKER, G. & M. EVANS (ed.). 2016. *Evidence-based policy-making in the social sciences: methods that matter*. London: Policy.  
<https://doi.org/10.1332/policypress/9781447329367.001.0001>
- STUMP, D. 2010. Ancient and backward or long-lived and sustainable: the role of the past in debates concerning rural livelihoods and resource conservation in eastern Africa. *World Development* 38: 1251–62.  
<https://doi.org/10.1016/j.worlddev.2010.02.007>
- 2013. On applied archaeology, Indigenous knowledge, and the usable past. *Current Anthropology* 54: 268–98.  
<https://doi.org/10.1086/670330>
- TSENG, V. 2012. The uses of research in policy and practice. *Social Policy Report* 26: 1–22.  
<https://doi.org/10.1002/j.2379-3988.2012.tb00071.x>
- WATKINS, J. 2005. Through wary eyes: Indigenous perspectives on archaeology. *Annual Review of Anthropology* 34: 429–49.  
<https://doi.org/10.1146/annurev.anthro.34.081804.120540>
- WEST, G.B., J.H. BROWN & B.J. ENQUIST. 1997. A general model for the origin of allometric scaling laws in biology. *Science* 276: 122–26.  
<https://doi.org/10.1126/science.276.5309.122>
- YORK, A.M., M.E. SMITH, B. STANLEY, B.L. STARK, J. NOVIC, S.L. HARLAN, G.L. COWGILL & C. BOONE. 2011. Ethnic and class clustering through the ages: a transdisciplinary approach to urban social patterns. *Urban Studies* 48: 2399–415.  
<https://doi.org/10.1177/0042098010384517>