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Journal

Cliodynamics, 0(0)

Author

Smith, Michael E.

Publication Date

2022

DOI

10.21237/C7clio0057262

Early Cities in *The Dawn of Everything*: Shoddy Scholarship in Support of Pedestrian Conclusions

Michael E. Smith

Arizona State University

Abstract

Sloppy argumentation occurs throughout *The Dawn of Everything,* making this a highly frustrating book whose interpretations are poorly supported and sometimes in error. I focus on the treatment of early cities and urbanism in the book. While the treatment of the Ukrainian Trypillian sites, Teotihuacan, and other early settlements is adequate, the authors' claims for originality are overblown. Their failure to follow basic scholarly mechanics of documentation and argumentation (e.g., inappropriate use of argument by analogy; failure to cite relevant work; empty citations; and phrasing widely accepted conclusions as if they were radical new ideas) gets in the way of potential contributions to the study of early urbanism.

Two recent books—*Killing Civilization* by Justin Jennings (2016) and *The Dawn of Everything* by David Graeber and David Wengrow (2021)—make the novel claim that in the distant past, cities developed before states. The authors of the two accounts, however, employ radically different evidence and argumentation to arrive at similar conclusions. Jennings uses standard archaeological practices: he defines city and state in social terms, discusses how they are operationalized archaeologically, reviews several well-known case studies, and concludes that cities preceded states in several parts of the world. He shows how some early cities then developed institutions of state-level government, while others did not, leading to their collapse. Graeber and Wengrow start with the Trypillian "mega-sites." They assert that these sites are urban settlements, note that the settlements flourished long before states, and conclude that they have now overthrown tired old models that posit the linked historical origins of cities and states. Problems of evidence and argumentation plague *The Dawn of Everything*, making many of its arguments weak and some of them wrong. I found it a frustrating read because there are quite a few interesting new ideas, but they are poorly supported and not sufficiently contextualized in the literature. The book is full of errors of citation and

argument, which suggests the authors are more interested in persuasion—pushing a set vision of the past—than in scholarly or scientific analysis and argumentation.

Mega-Sites and Faulty Analogies

The so-called "mega-sites" of the Ukrainian Trypillian culture have fascinated archaeologists since they were first excavated decades ago. These early fourth-millennium BCE sites are much larger than most Neolithic settlements, and they show a high degree of planning with an unusual concentric circular arrangement of houses. In an earlier paper, Wengrow (2015) complained that most archaeologists refused to call these sites urban. Although he declined to provide a definition of city or urban, his labeling of the sites as "cities" appears to be based on the claim that they were "the largest contiguous settlements in the world" at that time (Wengrow 2015: 11). Graeber and Wengrow similarly fail to define the terms "city" and "urban," yet confidently label the Trypillian sites as cities (288–94). These sites are important to their overall argument because they are "proof that highly egalitarian organization has been possible on an urban scale" (297).

But what do they mean by "urban scale"? Without explicit definitions of terms such as city and urban in the book, and without quantitative data, this question is hard to answer. Definitions are tools that help scholarship advance. There is no single best definition of city or urban; choice of the most useful definition depends on one's research questions and goals. Most archaeologists have used one of two standard social science definitions of urban: Louis Wirth's (1938) *sociological definition* stresses permanence, population size, density, and social heterogeneity; *functional definitions* (Fox 1977; Trigger 1972) are based on the presence of activities and institutions in a settlement that affect a hinterland; see Smith (2020). The Trypillian sites do not conform to either of these definitions. They lack the social heterogeneity of the sociological definition, and the urban functions of the functional definition (Hofmann et al. 2019; Ohlrau 2020). Graeber and Wengrow are not the only scholars who really want these sites to be considered cities. Gaydarska et al. (2020) resort to a novel definition of city (a settlement larger than others in the region) to conclude that these were urban settlements. But the failure of these sites to conform to a standard definition of urbanism does not mean that they lack importance for understanding the development of settlements and urbanism.

Graeber and Wengrow's discussion of the Trypillian sites has some peculiar features. First, they claim that "in scholarly discussions about the origins of urbanism, these Ukrainian sites almost never come up" (289). In the five years before the Covid-19 pandemic, I attended a number of conferences in Europe and the US on early urbanism, and it seemed to me that I couldn't get away from discussion of Trypillian sites! Perhaps they are referring to the neglect of these

sites by non-specialist authors such as Jared Diamond (1997) and Yuval Harari (2018). In fact, this might explain some of their rhetoric about forging new interpretations: their views do indeed advance beyond the likes of Diamond, Harari, or Steven Pinker, for what it's worth. But they don't really advance beyond the standard scholarship on early cities by archaeologists and historians. In the words of one reviewer of the book, they are "punching down against unworthy foes (Pinker et al.)" (Cos 2022).

A second odd feature is their inappropriate use of ethnographic analogy to support their view of egalitarian organization for the Trypililan sites. Graeber and Wengrow resort to a trick also used by Graeber in his earlier book, *Debt: The First 5,000 Years* (2011). When they want to promote an interpretation that has little or no archaeological evidence, they disparage the archaeological record—"we cannot rely on archaeology alone" (295)—and pull an ethnographic analogy out of a hat to supply the missing information. Graeber did this repeatedly in Chapter 6 of *Debt*. His subject was the early development of debt in "human economies." Because he had no archaeological or historical evidence to back up his evolutionary scheme, he simply described twentieth-century ethnographic cases (the Tiv and Lele of Africa), implying they are representative of what must have happened in the deep past.

Their analogy for the Trypillian sites is a single ethnographic case, from the Basque region of France. Residents of several villages share a cosmological belief that society exists in the form of a circle of households, each of which is equal to all the others. People have worked out a system of cooperative exchanges of goods and labor (Ott 1981). The fact that the Basque villagers are "self-conscious egalitarians" (296) is offered as support for the claim that the Trypillian sites were "highly egalitarian" (297). The Basque villages are dispersed settlements that lack circular plans, however, making the comparison unconvincing.

This "pull-an-analogy-out-of-a-hat" method is not a proper use of ethnographic analogy for archaeological interpretation. There is a literature in archaeology on the topic of analogy. Alison Wylie synthesized this literature in her classic 1985 paper, and she updated and extended her discussion more recently (Chapman and Wylie 2015, 2016) by incorporating Stephen Toulmin's (2003) analysis of warrants and the structure of argumentation. Formal archaeological analogy is a type of inductive logic, and analogical arguments are built and strengthened by increasing the number of source cases, by narrowing the scope, and by attention to the similarities and differences between source cases and the target context; see also Smith (2015, 2017, forthcoming(a)). The use of a single ethnographic case—whose similarities to the target case are not established—to fill in archaeological blanks violates the rules of inductive logic (Copi 1982: 397–400). It is not an acceptable form of scholarly argumentation. Graeber and Wengrow's approach to

analogy fares even worse in light of the methodological literature on case studies, arguments, analogies, and sampling in history (Gordon 2020; Kocka 2003) and the social sciences (Gerring 2012, 2017; Ragin and Amoroso 2011). There is simply no reason to believe that the ethnographic case shares enough characteristics with the archaeological case to illuminate the latter.

None of these problems imply that Graeber and Wengrow's interpretations of the Trypillian sites are incorrect. Their failure to cite Jennings (2016) on the idea that cities preceded states is puzzling, though. Compared to *The Dawn of Everything*, Jennings provides more cases, described in greater detail, and with more citations to the literature. Citing this book would have greatly strengthened their argument. But it also would make it harder to claim that their account is "overturning conventional wisdom on the origins of cities" (p. 288). It's hard to escape the conclusion that the authors have chosen false novelty over rigor.

Size Matters

A more serious example of ignoring prior relevant research is the assertion that population size makes little difference for the number and complexity of social and urban institutions. Graeber and Wengrow want to establish that the decentralized decision-making and social freedoms common in small groups and small-scale societies can also work at an urban scale. Their phrasing is much more convoluted than this, however: "None of which is to say that scale—in the sense of absolute population size—makes no difference at all. What it means is that these things do not necessarily matter in the seemingly common-sense sort of way we tend to assume" (281). The idea that population size and density have strong effects on urban society and organization is not just an assumption or ideological belief, as Graeber and Wengrow suggest. It is, rather, one of the most strongly supported empirical findings of urban research, with much evidence and analysis in sociology (Mayhew and Levinger 1977; Simmel 1898), economics (Casari and Tagliapietra 2018; Glaeser 2010), political science (Tavares and Carr 2012), sociocultural anthropology (Bodley 2003; Carneiro 2000), biology (O'Brien 2011; Uchida et al. 2021), urban science (Bettencourt 2013, 2021), and archaeology (Feinman and Nicholas 2016; Fletcher 1995; Jennings 2016; Kosse 1990; Smith forthcoming(b)).

The Burning Man festival—an annual gathering based on anarchistic principles—illustrates the effect of population size on organizational complexity, an effect that Graeber and Wengrow deny. Burning Man from the start has been organized along anarchistic lines that Graeber and Wengrow would surely approve of; its ten guiding principles are: radical inclusion, gifting, decommodification, radical self-reliance, radical self-expression, communal effort, civic responsibility, leaving no trace, participation, and immediacy.

The festival began in 1986 with 35 people on a beach in San Francisco, and grew to an attendance of nearly 80,000 in 2019.[1] In 1996 a threshold was crossed, with 8,000 people camping at the very high density of 870 persons per hectare (Figure 1); this is more than three times the density of Manhattan. The organizers were forced to introduce innovations in planning and regulation (Harvey 2010). In the words of Larry Harvey, chief organizer of the event, "After '96 it became very plain that unless things were regulated, unless we could create civic order, with the numbers that were coming in, it was unsafe" (Fairs 2015). "We got to a point where I saw people becoming irrationally angry with each other and with the city. It occurred to me that this might be an effect of overpopulation, and that we'd hit some tipping point where people were no longer comfortable" (Berg 2011). In 1996 the festival switched from an informal spatial arrangement to a strictly planned layout, with regulations and teams of enforcers. A clearer example of the way that growing population size and density require innovations in urban social structure and organization is hard to find.
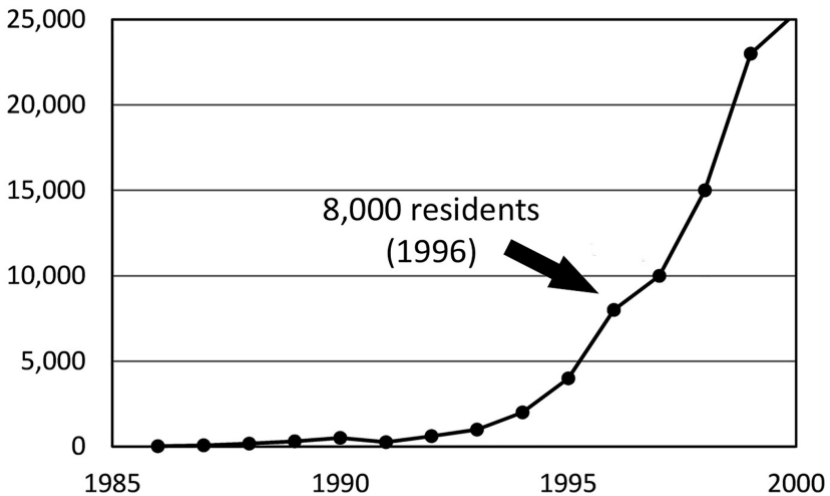

Figure 1. Population threshold for the Burning Man festival. Data from Wikipedia ("Burning Man," Accessed April 7, 2022).

If one wants to establish a point that contradicts such overwhelming empirical and theoretical support, scholarly good practice demands that one acknowledge and respond to the findings of that literature. While the importance of scale for urban organization, society and life is widely accepted, there is certainly room for

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[1] According to Wikipedia ("Burning Man," Accessed April 7, 2022).

qualification, extension, and modification. For example, research on generative processes in cities has become an important theme in urban studies (Bettencourt 2021; Bowles and Gintis 1998; Mehaffy 2008; Moroni et al. 2020; Smith forthcoming(b)), and Graeber and Wengrow's anarchist insights have potential to enrich this body of work. Anarchist scholars Colin Ward and John Turner made a start several decades ago (Turner and Fichter 1972; Ward 1973, 2002), but their contributions have yet to be fully integrated into the larger urban literature. Nevertheless, making bold assertions that fly in the face of established findings, without even citing that literature, is unacceptable scholarship.

Teotihuacan

Graeber and Wengrow's discussion of Teotihuacan (337–45) is not bad. Their conclusions—that Teotihuacan was a large city with a social system more egalitarian and a government more collective than many other early cities—are in accord with the views of many specialists today. Overall, this is a decent analysis, but with some serious deficiencies. First, they describe only one side of a debated issue. Archaeologists who argue for a more autocratic form of rule at Teotihuacan (Angulo Villaseñor 1987; Sugiyama 2005, 2013) are simply ignored. Second, they fail to cite the most important works on Teotihuacan government, where the specialist literature on the two sides of the debate is cited and analyzed. The best current paper (Carballo 2020) may have been published too late for Graeber and Wengrow to cite, but earlier crucial works (Cowgill 2015; Nielsen 2015) are not cited. George Cowgill's 2015 book on Teotihuacan is included in the bibliography, but it is only cited to identify a specific architectural feature, and not for its discussion of government at the city.

A third deficiency of the discussion of Teotihuacan is that a number of their references are empty citations. Empty citations are references to works that do not contain any original data for the phenomenon under consideration. The works are cited merely to lend an aura of support for an argument, when in fact they contain no empirical support. The major scholarly analyses of empty citations are Harzing (2002) and Henige (2006); I discuss this issue with respect to archaeology in Smith (2015, forthcoming(a)). I counted four empty citations in Chapter 9 (notes 9, 16, 60, and 61). I noticed a few in other chapters as well (e.g., note 95 in Chapter 10), but did not check the other chapters systematically. I also noticed that two of the six bibliographic entries for my own works are in error. A 2015 paper I published with seven students is listed as a solo publication, and a solo-author paper from 2017 is listed as "Smith, et al."

One example of an empty citation from *The Dawn of Everything* is found in a passage discussing evidence that the Aztec polity of Tlaxcalla had a collective form of governance. The authors make the following statement: "In the current

intellectual climate, to suggest the Tlaxcalteca were anything but cynics or victims is considered just a tiny bit dangerous: one is opening oneself up to charges of naïve romanticism" (355). This sounded odd to me, so I checked the reference. The statement is referenced with endnote 60 (586), which cites Lockhart (1985). But the paper by Lockhart has nothing at all to say about this subject. It is a paper about how indigenous Nahua concepts were retained into the Spanish colonial period. Casual readers will see that the claim about charges of romanticism has an endnote and assume that it is a valid, documented claim. Those who bother to look at the endnote will see that the citation is to an article by James Lockhart, the preeminent scholar of conquest-period Nahuatl-language documents. If the reader fails to track down this obscure paper, however, he or she will probably assume that the claim in the text is supported. Yet, this is false; the reader is victim of an empty citation. *The Dawn of Everything* is full of empty citations, along with many statements presented as fact with no supporting citations at all. These cast doubt on the scholarly reliability of the authors.

Scholarly Mechanics

Reviewers have pointed out a litany of deficiencies of scholarship and argumentation in *The Dawn of Everything*. Kwame Appiah (2021), for example, singles out what he calls the "bifurcation fallacy" (an artificial claim that the only options are two mutually exclusive alternatives) and what he calls the "fallacy fallacy" (if a poor argument is made in favor of a conclusion, then the conclusion must be false). Other types of deficient scholarly mechanics in the book include straw-person arguments, the practice of interpreting an absence of evidence as evidence for the absence of a phenomenon, the phrasing of widely accepted conclusions as if they were radical new ideas, the failure to acknowledge contrary views, and empty citations. The phrase, "there is every reason to believe," is used often in this book, and it is a sure sign that the authors can present little evidence one way or another on the particular claim.

The scholarly errors, coupled with the emphasis on rhetorical persuasion, bring to mind a criticism of Graeber's book, *Debt.* Mike Beggs (2012) took Graeber to task for trying to answer social science questions about the role of money and debt in societies by appeal to "metaphysical ideas about the true nature of money." This was also Pierre Bourdieu's critique of the work of Michel Foucault, for whom "the philosophical method was used for answering questions that are basically empirical sociological questions" (Callewaert 2006: 92). This criticism also applies to *The Dawn of Everything.*

The authors' deficiencies in argumentation may help explain their disdain for the social sciences in general (22), and for quantitative research in particular. The very first endnote of the book (note 1 on page 7) epitomizes this view: "if one

reduces world history to Gini coefficients, silly things will, necessarily, follow" (527). I have found that some humanities scholars who have little knowledge of science or quantitative reasoning seem to think that the very act of calculating a numerical measure implies that one is trying to explain absolutely everything about a phenomenon from that single quantity. From the perspective of the social or natural sciences, this is an absurd conclusion. But Graeber and Wengrow explicitly distance themselves from both the social and the natural sciences. Perhaps they think this excuses their failure to consider whole bodies of research that go against their ideas.

Apart from the errors in their discussion of settlement size, Graeber and Wengrow's treatment of early cities in *The Dawn of Everything* is rather pedestrian. Their identification of the Trypillian sites as important for comparative archaeology, their views of collective rule at Teotihuacan, and their depictions of other early urban sites largely concur with current views of specialists. Yet the authors' claims that their ideas are novel—such as the notions that cities originated before states or that Teotihuacan had a collective regime—are incorrect, and other conclusions are based on a faulty use of analogy, argumentation and evidence. These difficulties do not make their substantive claims wrong, but they certainly deprive them of the support they could have if properly argued. Such problems also plague most of the other parts of the book.

Perhaps these problems could have been avoided if Graeber and Wengrow had followed the common practice of first publishing their scholarly arguments in detail in peer-reviewed specialist works, and then writing a general-audience work that builds on that foundation. While they did publish a few papers on specific topics covered in *The Dawn of Everything*, many of the key arguments in the book have yet to be vetted thoroughly through peer review. To take just one example, I cite Wengrow (2015) on his interpretation of the urban status of the Trypillian sites, but that paper is the text of a lecture, and has not (to my knowledge) been published in a peer-reviewed journal. As someone trying to convince urban sustainability scientists of the value of archaeological data (Smith et al. 2021), I fear that *The Dawn of Everything* will hinder this effort because of its shoddy mechanics of scholarship and argumentation. The authors claim they want to create "a new science of history" (p. 24). That is certainly a laudable goal, but it would require a more rigorous use of logic, evidence, and argumentation than is evident in *The Dawn of Everything.*

Acknowledgements

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