

UTOPIAS AND MYTHS IN TIME

Review of *The dawn of everything: A new history of humanity* by David Graeber and David Wengrow. New York: Farrar, Straus & Giroux, 2021. Pp. 704. ISBN 9780374157357 (paper).

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During my time in college, our anthropology professor used to tell us that world history was an unattainable goal. She was critical of any comprehensive synthesis of world history and disregarded all attempts to this end. Since then, I have often remembered this remark anytime I encounter ambitious syntheses that claim to provide a thorough account of the critical developments in humanity's history. My work in archaeology has taught me that the sheer diversity of human adaptations makes fitting the evidence in a singular narrative more an act of faith than proof.

The book by D. Graeber and D. Wengrow, with the flashy but catching title *The dawn of everything: A new history of humanity*, attempts to do precisely that. The title hints at the *Theory of everything*, recently popular in Physics and Philosophy, a totalizing theoretical compound of nature and abstract thought, setting the stakes very high. The question is, can we construct a single set of premises that would explain all things social and cover from the remote past of the *Homo sapiens* up to modern social life? In other words, can anthropology and archaeology find their own Holy Grail?

Several serious challenges need to be surmounted in such a synthesis. First, approaches to the social past are necessarily hampered by our inability to objectively analyze that which is integrally associated with us. Contemporary archaeological science asserts its ability to do so, but this often comes at the expense of the social and cultural context in which the archaeological material refers. This is not the way to reconstruct social and cultural history! Second, with such a rich spectrum of cultural adaptations, it is impossible to abstain from picking the most indicative of one's point of view. In this case, the synthesis of world history comes as close as it gets to politics. Without the support of evidence, it risks ending up as wishful thinking.

Broad, expansive synthesis was a mark of the nineteenth century. At that time, cherry-picking was impossible because so little anthropological evidence was available. Instead, in the spirit of the times, the discussion was based on universal concepts considered active on a deeper level, underneath the phenomenology of cultural occurrences. Engels' and Marx's theory of class struggle, Morgan's stages of social evolution, and Childe's study of developing production forces and relations were all of this ilk. At the height of modernism and following the paradigm of the scientific method, discovering the underlying cause of social phenomena was paramount, together with recognizing the timeless principles of human matters.

This type of causative single story is now rejected as obsolete and misleading. Social evolution is discredited as an imperial synthesis aiming at putting the white Western man on the dominating top step of the social evolution ladder. The second half of the twentieth century saw the challenge to modernity and the demolition of grand narratives such as evolutionism, Enlightenment, and Marxism. The style of this fierce criticism is worth noting; starting from the content, it soon extended to the method of modernity. As the discredited generalizing principles of modernity were replaced by the particulars of specific cases, the evolutionary stages and what actually comes with them fall to the same pervasive critique.

In this context, the book by Graeber and Wengrow makes its contribution. It sets out to dismantle the grand narrative of the stages of social evolution that ostensibly led to the emergence of the present-day state. Social evolution of this type naturalizes political formations and presents them as a natural conclusion of a long process. This is the core of the book. The authors want to demonstrate that this determinism associated with the stages of social evolution is erroneous and that things could have gone differently. Hunter-gatherers are not necessarily egalitarian; egalitarianism does not end with the emergence of agriculture, and hierarchical societies need not lead to the establishment of the state; the people of the urban centers could very well manage without the intervention of a ruling bureaucratic elite. In short, the argument is as political as it can be: if it did work in the past, it could very well work now. As the authors put it, it is an attempt to evaluate the 'possibilities for social change today.'

However, do the authors exaggerate here? Frankly, I don't know many scholars who would seriously maintain today that all these steps resulted from an inevitable necessity and followed a predetermined trajectory. This would be akin to the obdurate Soviet Marxism of the 1930s, whose application imposed so many human costs. Nowadays, social evolutionists are an almost extinct breed. On the contrary, the critical concept of contingency, already vastly discussed in history and archaeology, is well-established. It rejects the sequential process from step to step and accommodates indeterminacy in human decisions and -political- actions. So, why this book?

Indeed, old ideas die hard. There is a public that does not know about contingency or care about the different shades of esoteric academic debates. On reflection, why should they? This book is mainly addressed to this broader audience. The problem is that to do

that, it has to stretch from the Paleolithic origins of modern humans as foragers to the ruling elites and sovereigns in cities and complex states. Evidence is gathered for every corner of the Earth. This is where several problems start.

Such a vast expanse of space and time negates any contextualization. Simply, a book does not have enough space to include it. The evidence thus remains somehow hanging, although admittedly, the detailed and valuable bibliographic references at the book's end might help someone willing to delve deeper into it. The broad audience, however, may not realize the complexity of the evidence, especially for the specialized fields and the expert knowledge required to make sense of them.

For a book claiming – as its title indicates – to challenge in seven hundred pages everything that has been said in the macrohistory scholarship, it is strange that the implicit argument in the first five chapters is based on the confidence that the modern 'primitive' groups are 'fossils' of past societies, a rather nineteenth-century idea. Why else would the Northwestern Californian hierarchical hunter-gatherers Kroeber studied in the early twentieth century have anything to tell us about hunter-gatherers some tens of thousands of years ago? It may or may not, but this is pure speculation, not evidence. The only thing that follows is that hunter-gatherer societies and hierarchical structures are not mutually incompatible. Still, as previously said, this may be only significant to fend off nineteenth-century style social evolutionists if there are still any left.

More concerns are raised for the book's first part (Chapters 1 to 5), which mainly addresses the origins of human inequality. This is a fundamental issue. Noticeable is the absence of recent scholarship on human evolution, which has taken great strides in comparing primate and human adaptation. It is a serious omission that tallies with the absence of the role of gender, which is closely connected to the *Homo sapiens* making. The main argument that follows from the hierarchy in modern hunter-gatherers in Northwestern California is that hunter-gatherers generally possessed political capabilities and could form different political realities. The evidence presented is sparse and ambiguous and by no means, as described, overwhelming, unless someone forgets that it is merely a speculative interpretation. We fly from Krakow to Kyiv, Dolni Vestonice and Kostenki, Göbekli in Anatolia, sites in France, Italy and Brazil, and dash from the Upper Paleolithic to the present. I will leave with no comment the Central and South American cases discussed in later chapters, as they are far beyond my expertise.

This is vertiginous as it is, but it is also confusing. Placing many cases in one category varying in geography and time and under a blanket explanation weakens the argument. For instance, Göbekli can be a meeting place for hunter-gatherer groups to reinstate their communal solidarity, a political act for sure, but still within the egalitarian ethos. Alternatively, the impressive monuments could be a sign of conflict within or between communities. Such meeting places are still familiar in the Neolithic and are not affected by the alleged emergence of inequality in that period. Why was this monumental structure not the result of a collective decision? Furthermore, I doubt that Göbekli can be compared

to twentieth century Nambikawara groups in Brazil, which show seasonal, temporal administrative leadership. And I still don't see why the people of Göbekli would need a hierarchy to persuade them to work harder than the idyllic life pictured by Sahlins, Harrari, and Diamond.

I can't see how this evidence supports the view of 'self-conscious political actors capable of embracing a wide range of social arrangements.' We get no help from the authors, who avoid answering explicitly what they think these rare expressions of monumentality might represent. The only sure thing I can see in these examples is that in the Paleolithic, as in later periods, there was a vast variety of human adaptations to the social and natural environment, which remains to be discovered and studied. The fact that groups of hunter-gatherers were egalitarian does not mean they were insensitive to their social environment. This variability becomes invisible only to scholars who insist on squeezing social life into anthropology's predetermined stages. The hunter-gatherer's regime and the adaptation to social reality are two things that are not opposing and incompatible. If that was the reason for rambling in the four corners of the world, it would be easier to refer to the literature that has already noticed these oscillations in the prehistoric ways of life, for example, the book by Flannery and Marcus of 2012. Otherwise, it's dangerously close to preaching to the converted.

I need to add a small remark about the political dimension of the hunter-gatherer groups: everything is political. In fact, I would think that the split between the non-political and the political had not yet occurred. Even flint knapping, setting up a hut, or wearing a shell ring are political and require communication and cooperation. Small daily events build the political, not necessarily leaders, with capacities akin to politicians of the post-industrial era or judgments made by a council of executives. Politics in hunter-gatherer tribes would most likely be bottom-up rather than top-down. Finally, one comment on the argument that Paleolithic humans had, as sapiens, the same mental and cognitive capabilities as modern humans have. I am not at all proficient in cognitive science. Still, I find it difficult to accept that Paleolithic *Homo sapiens*' abilities are defined by an alleged mental roof set by the modern brain. Apart from the epigenetic effects on the DNA, which are environmentally induced, it is well-known that the deployment of cognitive capacities also depends on the social environment. Is it unreasonable to expect that hunter-gatherers' cognitive abilities may have varied significantly from modern humans, perhaps even outperforming them in some aspects? The tolerance of eccentricity mentioned in the book is one possible example but not the only one.

Chapter 6 is about the emergence of agriculture. This is a crucial part of the book's argument about the flexibility inherent in primitive societies. The point is to argue against the cataclysmic and far-reaching change conventionally attributed to agriculture's inception. The authors correctly point out that this was a slow process with many setbacks and retreats. However, instead of focusing on sites that really portray this change, like Abu Hureyra, they concentrate on Çatalhöyük, which does not represent the period of trans-

formation, as it is much later. I think the authors were carried away by the sheer amount of analytical work done there.

The thing is that Çatalhöyük is a unique case, unique in the size of the settlement and the symbolic expression inside the houses and in many other respects. Except for the monumental sculpture of Nevalı Çori, further to the East, there is no other Neolithic site displaying comparable art to Çatalhöyük. Anything that happens at Çatalhöyük is hardly representative of the entire Neolithic period.

Whether we call it a town or an – unusually – expanded village is perhaps immaterial. The real question is how a population of that unprecedented size (calculated at around 5000 people) manages to stay together without splitting apart. Something in this community's structure and organization must be accountable for this Neolithic oddity, but this review is not the place to go into it. Only one thing Çatalhöyük is not: a community practicing 'flood retreat' agriculture. According to recent detailed paleo-hydrology research and modelling at Çatalhöyük, agriculture was not of the flood-retreat type but rather an intensive gardening, heavily manured with dung for the plots, decreasing with the distance from the settlement. As recent specialized archaeobotanical research shows, intensive gardening was the usual practice for the Early Neolithic in the eastern Mediterranean, so there was nothing special in this respect there. The same holds for Abu Hureyra, where flood retreat agriculture has not been detected. By the way, research shows that intensive gardening farming was not at all 'play agriculture.' It required a lot of communal labor.

The presentation of agriculture's emergence has some noteworthy points. The chapter is well documented, with a wealth of knowledge reflecting Wengrow's expertise. The synthesis is inventive and enticing but occasionally stumbles on inadequate evidence. The divide of the 'fertile crescent' in the north uplands and the south lowlands is well established in the relevant literature since Braidwood's time. However, the clear separation proposed between the aggressive hierarchical highlanders with monumental architecture and the passive egalitarian lowlanders is too much of a generalization to be accommodated.

First, although later, the example of the Jericho Tower in the southern Levant is equally spectacular regarding monumentality and mobilized labor. Second, the aggressive character of the highlanders, based on the skulls of Göbekli and Nevalı Çori, is purely hypothetical. The skull ritual is also found in the southern lowlands, such as Jericho and Tell Aswad, and it stretches in time, as seen at Çatalhöyük much later. The skulls at that site, in fact, belonged to deceased members of the household, as evidenced by the digging of a pit to extract the skull of a woman buried beneath the house's bench. Why wouldn't the 'violent' highland people's collection of skulls imply a similar ritual? I don't see why the plastering of skulls in Jericho makes much of a difference or that it makes skulls unfit for trophies, as argued. To argue this, the authors have first to prove that the skulls of Göbekli or Nevali Çori were indeed trophies. It is well known that the manipulation of dead

bodies in the Neolithic, removing bones from skeletons, etc., is a widespread practice. In that case, the skull stash at Nevalı Çori and elsewhere might have been a sort of mortuary taxonomy. I believe the authors overstretch their interesting hypothesis of connections between North and South to fit with their Californian ethnographic model of Kwakiutl and Yurok. However, the fundamental obstacle to accepting fully the North-South hypothesis is the lack of a well-established archaeological record for the Late Paleolithic foragers in SE Anatolia and around the Göbekli site, which would give us some idea of the communities' features active in that region.

The model of 'trade' mobility (I would use the term 'exchange' instead) is fascinating with its geographic parameters, overcoming the stiffness of the conventional archaeological approach based on the style of artifacts and hypothetical migratory movements. It remains to identify the raw materials or artifacts that keep these bilateral exchanges running. The section on the relation of property with the field is another much older legacy of Jean-Jacques Rousseau. Only, it's likely that it's not the field that produces ownership rights but rather the labor and the eventual product. As the agricultural process involves cooperation, it commands a kind of reciprocity ties which sets up the social fabric.

The book's final part discusses the rise of urban centers and the social structures that theoretically go with them. I will not delve into that part, as my archaeological knowledge is not sufficient, especially, as said before, for the empires of Central and South Americas. Here, the authors extend their argument for the administration of every social formation free of any determining compulsion or even coercion coming from the social domain. As we have hunter-gatherers who can be hierarchical and agriculturalists who insist on egalitarianism, we have urban centers in Asia with no trace of administrative elites. We have agriculturalists who abandon agriculture and go back to hunting and gathering (though I still don't know how that could be confirmed). The message is that people were always free to experiment and apply a structure different from what the mythology of the past predicted, even revolting in some cases and going back to self-administration. Of course, there will always be exceptions in social formation. Whether those would be anything more than exceptions is another question. In any case, the book's political message is clearly and openly anarchic to the point of occasionally being a manifesto of social emancipation.

Come to think of it more closely, the connection of the past with politics represents a bizarre twist. Politics addresses the future, while archaeology aims at the past. Nevertheless, from Thucydides to Rousseau and Hobbes, and from there to Marx, all political thinkers exploited the benefit of hindsight and approached the origins as the conceptual incubator of political thought. Anthropologists know that in pre-modern societies, this role was played by mythology; modernity has set up its own myths based either on archaeology, or anthropology, or history, in any combination.

All political discourse aims to construct a narrative that naturalizes and legitimizes its content. It makes it look not like ideas conceived sometime in the present but as some-

thing prepared a long time ago and built from actions and decisions taken in the past, occasionally, as in the case of *The Dawn* from the remote past. This is the myth that is presented by archaeology with its power to poke the depths of the career of *Homo sapiens* on Earth, and anthropology with its thick description of alterity. It is as if *Homo sapiens* is unable to think outside the historical guidelines, as if homo is doomed to repeat the past, not in an eternal cycle but in a spiral, hopefully upwards.

The book *The Dawn of Everything* does just that. It presents a political argument based on a long and difficult trail of archaeological and anthropological evidence. The reunification of anthropology and archaeology is an old dream come true since their split at the beginning of the twentieth century, but it also shows its limits. However, the book is aimed at the general audience, not the scholars, and it couldn't be as exact and pedantic as the latter would like; it would take volumes. Scholars can learn a lot by reading it, even if, as I do, remain skeptical or even in disbelief for some of the oscillations between past and present or the speculative arguments and the occasional cavalier attitude towards evidence. Even with these flaws, the book poses intriguing questions in a captivating style, provoking a dialogue we rarely see within the discipline.

What I found brilliant, however, especially for the general audience the book aims at, is the effort to approach peoples of the past not as the dry abstractions we got used to in scholarly literature but as real people with real lives doing things with which we are familiar, despite the distance of time. This is the real fascination of the past. We are grateful to both authors for pointing that emphatically, particularly to the late Graeber, who is regrettably not with us anymore, to continue the dialogue.