Resetting History's Dial? A Critique of David Graeber and David Wengrow, *The Dawn of Everything: A New History of Humanity*

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Graeber and Wengrow's sprawling new history of freedom (Graeber and Wengrow 2021) has considerable strengths: its emphasis on formative processes that unfolded before literate civilizations appeared, its global reach, and its skepticism about the connection between state power and civilization. But it also suffers from serious shortcomings: the authors' commitment to an excessively idealist view of historical dynamics, their use of rhetorical strategies that misguide their audience, and their resultant inability to account for broad trajectories of human development.

Introduction

Graeber and Wengrow set out to revise our understanding of the early history of our species: not of our earliest evolutionary beginnings, but—mostly—of the thousands of years between the beginning of the Holocene and the spread of increasingly powerful states that produced the current world system. What are their principal claims? Unlike their present-day remnants, ancestral foragers were not necessarily confined to small bands; farming matured very slowly and hybrid foraging-farming arrangements endured for millennia; crude stage models of social evolution fail to do justice to the complexities of the historical experience; early cities did not immediately spawn autocracies or even avoided them altogether; early polities were much more limited in scope than modern states; one to two dozen generations ago, some Indigenous North Americans chose to turn away from farming and unequal arrangements and developed a political philosophy that inspired European Enlightenment thinkers; the richness of the historical experience revealed by this book suggests meaningful alternatives to our current way of life and can therefore support social activism today.

The authors present these positions as more or less novel or at least unfamiliar beyond narrow circles of specialists who fail to communicate them more widely or coherently, and frequently as conflicting with prevailing academic opinion. Their

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key question, repeated throughout the book (9, 76–77, 115, 119, 121, 133, 248, 503–4, 519, though I may well have missed further references), is simple: how did we, as a species, "get stuck" in a single mode of hierarchical submission to political and other authorities?

I address these points in turn. I frequently quote directly from their work, to make sure to let the authors speak in their own voice. As I hope to show, this literalness is often essential for clarifying their line of reasoning. I largely refrain from taking issue with points of detail: not because they are all unobjectionable (which is hardly imaginable for any book of such extraordinary length, breadth and verve), but because they are so varied and so frequently presented without reference to competing views that a diverse team of experts would be required to probe them. Such scrutiny, while well worth undertaking, might distract from more fundamental issues of method and presentation, on which I focus here.[1] This approach exposes a wide range of serious flaws in what is otherwise a timely and stimulating book.

Foraging

First stop, foragers. Graeber and Wengrow forcefully reject the notion that ancestral foragers generally lived in small and resolutely egalitarian bands that accomplished nothing of note (83–85). They argue that tiny groups of utterly impoverished hunter-gatherers that we currently encounter in marginal terrains of little appeal to farmers, such as the Kalahari, cannot be viewed as representative of Paleolithic foragers who had the whole world to choose from, enjoying unfettered access to much better lands and abundant natural resources, especially along coasts and rivers (153ff). Ecological constraints on the scale of their social arrangements would therefore have been much less severe. Yet much relevant evidence has long been lost to rising sea levels (154–55). Thus we can only guess at the degree of real-life variation: most likely some groups were relatively small and poor while others were not (140).

A handful of Pleistocene and early Holocene burials with hard-to-produce grave goods indicate what was possible. This potential grew even further in the climatically more stable Holocene: Graeber and Wengrow's exhibits range from the impressive stone pillars erected by foragers at Göbekli Tepe (near the Turkish-Syrian border) beginning around 9000 BCE to monumental construction in many parts of the world from eastern Europe to North America, most notably the massive Poverty Point earthworks in Louisiana of the second millennium BCE and archaeological remains from Jomon-era Japan. As far as we can tell, these endeavors do not seem to have required chiefs, ranked societies or state

[1] By the same token, I rarely refer to other work, or add footnotes.

structures, and not even farming (92, 103). Thus, the authors stress, monumentality—and the degree of social cooperation it implies—cannot be reduced to a corollary of food production (147).

None of this is new in a formal sense; nor would Graeber and Wengrow suggest it is. They are primarily concerned with the visibility of these developments in the scholarly and public imagination. To be sure, their question of why sites such as Poverty Point are not more widely known (144) is worth raising, even though we must wonder which standard to apply. Short of polling the reading public, it is hard to tell who knows what. Notable sites such as Göbekli Tepe and Poverty Point now routinely appear in college-level world history textbooks and can hardly count as arcana accessible only to ivory-towered specialists. And is Stonehenge more famous—as it unquestioningly is—because it was erected by early herders or because of its British location? That said, we can all agree that ambitious forager sites could, and ought to, occupy a more prominent position in our historical imagination.

Graeber and Wengrow make a case for seasonal patterns of aggregation and dispersal, which would have allowed mobile foragers temporarily to cooperate on a large scale (104–5). In some settings, they hold, such practices may have caused recurrent oscillations between small bands and much larger groups with some state-like attributes (110). To them, this reveals an enviable institutional flexibility, an ability to "step out of the boundaries of any given structure and reflect" (111). This is an attractive model, even if it is quite a leap from documenting or inferring such patterns in specific cases to boldly assuming, as they proceed to do, that humans spent "most of the last 40,000 or so years moving back and forth between different forms of social organization" (112)—which is not impossible, but surely impossible to know.

Nor does this kind of seasonal flexibility mean that we "can't speak of an evolution from band to tribe to chiefdom to state if your starting points are groups that move fluidly between them as a matter of habit" (111). It simply means that we may imagine transitional processes, if and when they did unfold, to have been gradual rather than sudden. Moreover, the intermittent presence of attributes commonly associated with different stages of social development reflects the considerable potential for social evolution toward more stable larger entities over the long run. If early Holocene foragers were already willing to engage in coordinated activities even as they had clear exit options, the break between that state of affairs and later modes of more routinized submission becomes less pronounced. Early seasonal aggregation makes humans seem *more* primed for lives under hierarchical control, which is the quite opposite of what Graeber and Wengrow would like them to be.

The authors elect not to explore how much this seasonal flexibility depended on particular ecological conditions and acquisitive strategies. These matter, not least because if history is to serve as an inspiration for the present, the contingency of such arrangements on features we have utterly lost (such as the feasibility of foraging and the resultant mobility) is crucial (see my final section). Instead, they inveigh against stage models of social development, rejecting not only basic taxonomies but also sub-categories such as "complex," "affluent" or "delayed-return" foragers (150–53). Yet if documenting the richness of the historical experience is their goal—and they themselves stress variation among foragers (140)—it is hard to see why. Contrary to what the authors imply, such calibrations may but need not make such groups look anomalous.

Their opposition to placing complex forager societies "on the cusp" of some transition to farming, chiefdoms and the like (164) reflects their preferred mode of black-and-white reasoning that runs like a red thread through their entire book. While not all such groups would have been on some sort of "cusp," some would and even must have been. After all, if none of them had ever been, there would never have been any agricultural societies at all. A simple thought experiment reveals the underlying fallacy: following Graeber and Wengrow's premise, if you went x years back in time in the history of a given farming community and encountered complex foragers, you would not be allowed to identify them as approaching a transition even as they were. In the aggregate, directional transitions and even "cusps" must have been very common, especially since—as the authors note in their discussion of farming—such transitions took a very long time. Yet without knowing the future, we cannot tell how close any one group was to such a "cusp."

Farming

Graeber and Wengrow emphasize the slow pace and tentative nature of the process whereby foragers shifted from experimental crop domestication to full-fledged food production. Simply put, there was no "agricultural revolution." For millennia, farming was just one element in diverse portfolios used to sustain early sedentary communities, alongside hunting, foraging and fishing.[2] Such communities retained the ability to move in and out of farming without fully committing to food production as the backbone of their existence (260), perhaps even "remaining wedded to the cultural values of hunting and foraging" (272). The authors mention transition times of 3,000 years in northern China, 3,000 or 4,000 years in the Middle East, and 5,000 years in Mexico (234, 271). Farming expanded

[2] I am less than sympathetic to the term "play farming" they repeatedly apply to this mixed mode of subsistence (e.g., 248): putting food on the table (or at least on the ground) day after day must have been serious business no matter how it was obtained.

slowly across space (254–55), and reversals occurred (254, 257–58, 261–62). Farming therefore represented much less of a rupture than one might imagine (244). Given the extensive overlap between them, it would be misleading to portray foragers and farmers as polar opposites (247). All of this is true and important.

Just as before, I cannot properly gauge how novel these observations will seem to the reading public. What *is* however clear is that the different views they argue against do not reflect the current state of scholarship. Thus, their claim—referencing as it does their academic peers—that "most scholars assume, as a matter of course," that "farming [was] from the very beginning about the serious business of producing more food to supply growing populations," is simply untrue (211). More strawpersons enter the stage in the guise of "[h]istorians painting with a broad brush [who] sometimes write as if" food production had presented itself to foragers "as an obviously beneficial thing" (252).

More importantly, does their account change our understanding of the relationship between farming and social and political development over the long run? Not really. As so often in the book, they belittle the significance of well-documented secular trends. For instance, their observation that "while agriculture allowed for the *possibility* of more unequal concentrations of wealth, in most cases this only began to happen millennia after its inception" (248) is not as remarkable as they seem to think: after all, if the transition to full-blown food production likewise took several millennia, it is not obvious why economic inequality should have emerged any more rapidly. And even if agriculture did not mark the origin of social rank, inequality and private property (248), it acted as a catalyst and accelerant.[3]

Graeber and Wengrow curtly dismiss the notion that large-scale societies emerged where domestication first appeared: "Archaeological science changed all this." How so? By identifying some 15 to 20 zones around the globe where domestication commenced independently, yet with none of them evincing "a linear trajectory from food production to state formation" (251–52). But a look at their own map (253) shows that the earliest major large-scale societies did indeed appear in areas where domestication had begun early on, in Mesoamerica, western South America, the Fertile Crescent, South Asia and northern China (and much later in the western Sudan). As a matter of fact, *none* of them appeared anywhere else.

What is more, in the initial domestication zones that did not spawn large-scale societies, the local crops were either poorly storable or hard to appropriate (notably banana, yam and taro in New Guinea, or manioc in Amazonia) or

[3] Borgerhoff Mulder et al. 2009; Bogaard et al. 2019 offer pertinent theory and data.

otherwise not terribly productive (the native crops of eastern North America).[4] Finally, once we account for the huge lag times that separated cultivation, domestication, urbanization and state formation around the world, there are not any obvious outliers at all—if you look for areas where useful storable crops were domesticated but those later developmental features did not materialize within a few millennia, you will come up short. The authors' "linear trajectory" is a red herring: trajectories can be both linear and very long.

Even so, Graeber and Wengrow condemn "conventional narratives of world history, which present the planting of a single seed as a point of no return" (260). Yet as hyperbolic as this claim is made out to be, it is, in the end, true. Even they think so: asking "why does it all matter?," they grant that it is "reasonable" to ask whether it is not the case that "what matters in the wider scheme of things are not the first faltering steps toward agriculture, but its long-term effects"—given that farming had managed to spread almost everywhere 2,500 years ago and that it unlocked terrestrial carrying capacity in ways that allowed our species to grow in number by three orders of magnitude between the end of the Holocene and today (273–74).

Their caveat is forceful: "You can't simply jump from the beginning of the story to the end, and then just assume you know what happened in the middle" (274). That is a fair point: directing the spotlight at hybrid forager-famers and at developmental lags, detours and hiatuses is a worthy goal, and bound to enhance our understanding of historical dynamics. Yet it also more modest an ambition than they suggest near the end, when they seek to mobilize this narrative for contemporary political purposes (see my final section). Even a trap that was slow in closing was, in the end, a trap.

The authors' discursive style makes it hard to gauge if, how and especially how systematically ecology, demographic dynamics and warfare shaped the emergence and expansion of farming. That is unfortunate: population growth would have helped trap farmers by rendering alternatives less feasible, and random environmental shocks could have had a similar effect.[5] Sedentary and reliant on fixed buildings and weighty equipment, seed grain and food stocks, full-blown farmers became more exposed to organized predation, a predicament that ought to have encouraged commensurate responses.[6] I already mentioned the authors'

[4] As Graeber and Wengrow acknowledge elsewhere, if only in passing, storability mattered (271). See most recently Mayshar et al. 2022.
[5] Their reference to climate change in northern China precipitating cultivation by depleting wild crops (271) remains an exception.
[6] The authors aver that warfare was not very important in early farming societies, whereas trade was (249). Of course, ancient trade in durables is also more visible now, unlike warfare that fell short of fancy fortifications and wholesale destruction of sites.

neglect of the role of crop attributes in mediating development, a neglect echoed in their contrast between "serious" farming expansions (in Europe or the Nile Valley) and "play farming" as practiced in Amazonia for millennia (266–73): how much did the superabundance of rainforest resources sustain the latter, and the—increasingly extreme, as the Sahara dried out—ecological circumscription of the Nile Valley the former?

On a rare occasion when Graeber and Wengrow invoke environmental factors with approval—Jared Diamond's well-known point that better east-west connectivity in Eurasia facilitated crop diffusion compared to fewer north-south exchanges in the Americas (255–56)—they immediately backtrack by asking, "To what extent can such observations help make sense of human history on a larger scale? How far can geography go in explaining history, rather than simply informing it?" I have trouble understanding the distinction between "explain" and "inform": doesn't the latter simply imply a meaningful contribution to (multifactorial) explanation? This rhetorical contrast only makes sense if "explanation" is defined as exclusively monocausal. The actual question here is how to weigh the relative importance of geography and other factors, yet the authors have no interest in pursuing this line of inquiry. The net outcome is a tendency to assimilate consideration of environmental factors to the bugbear of "determinism," which is of course a familiar ploy (see my penultimate section).

Graeber and Wengrow's approach encourages an apotheosis of proximate causes, without which broad patterns are deemed unintelligible and irrelevant. Even as they concede that only cereal farming and storage made bureaucratic empires possible, they consider that truth to be "so broad as to have very little explanatory power" (127). But all this is an unwillingness to differentiate between higher- and lower-order explanations. "Now it is undoubtedly true that, over the broad sweep of history, we find ever larger and more settled populations, ever more powerful forces of production, ever larger material surpluses, and people spending ever more of their time under someone else's command. It seems reasonable to conclude there is some sort of connection between these trends." Reasonable indeed, one would think, but sadly, it turns out, to no avail: after all, they hold, the nature of this connection and the underlying mechanisms are "entirely unclear" (133).

Perhaps their sense of "entirely" diverges from standard dictionary definitions: but even if it did, why would (inevitable) uncertainty about how exactly various links made up a chain (complex and varied as these linkages must have been around the globe) invalidate observations about general trends? Does the (unattainable) perfect explanation really have to be the mortal enemy of the good (enough)? For Graeber and Wengrow, if a particular outcome cannot be linked to a particular condition by means of an iron chain, any connection between condition and outcome does not seem to matter at all. Little wonder that they object so strenuously to the efforts of social scientists: by denying meaning to even the most powerful correlations, the authors' mindset, were it universally applied, would drive entire academic fields out of business.

Cities

In their discussion of early cities, Graeber and Wengrow continue to foreground their favorite themes of hybrid beginnings, lag times and occasional reversals. Urban concentrations did not immediately give rise to kings, bureaucrats and top-down government; sometimes centuries passed without signs of palaces or temples, sometimes they never showed up at all, and at other times they did but then faded from view (277). In the authors' view, this matters not least because it "suggests a much less pessimistic assessment of human possibilities" of relevance to the urbanized world of today (278)—a nexus that is asserted rather than argued. How exactly does a richer vision of early urbanism inform social activism today? I return to this question at the end.

For now, let us stick to early history. They note that the more foragers and early farmers intermittently interacted on larger scales, the less alien urban concentrations might have seemed to them (281): that is worth considering, even though urbanism did away precisely with what Graeber and Wengrow consider a defining feature of earlier lifestyles, the seasonal ability to step in and out. Only from their idealist perspective could the city be "a structure raised primarily in the human imagination" (282): the demands of food supply and waste disposal would have brought that imagination quickly down to earth.[7]

But what matters most to them is the lack of sharp ruptures, of a mechanical, law-like nexus between early urbanism and autocratic forms of social control. They readily concede that the evidence for early conditions is generally poor—the lowest layers of sites that sprang up in floodplains and wetlands are particularly hard to investigate (287–88)—but they nevertheless deem it good enough to "upend the conventional narrative" (283–84).

What, in their telling, sustained these early agglomerations of people? Environmental factors make sporadic appearances: the authors note that more stable flood regimes made river basins more useful for habitation after about 5000 BCE (286–87) and that black earth soil formation in Ukraine supported the rise of the "mega-sites" of the fourth millennium BCE (290). Even so, Graeber and Wengrow are quick to put ecology and technology in their place with the unsupported assertion that "In point of fact, the largest early cities, those with the greatest populations, did not appear in Eurasia—with its many technical and

[7] Contrast Smith 2020 for an incisive survey of definitions of "urban" and "city."

logistical advantages—but in Mesoamerica, which had no wheeled vehicles or sailing ships, no animal-power traction or transport, and much less in the way of metallurgy or literate bureaucracy" (285). But this is impossible to reconcile with the historical record: in terms of absolute chronology, mega-cities such as Babylon predated Teotihuacan by a wide margin, as did large urban sites in South and East Asia. Moreover, in terms of actual urban population size, Teotihuacan appears to have been a solitary outlier in the Americas until Tenochtitlan rose in the fifteenth century. That matters because the ecological and technological preconditions for urbanism differed so much between the Old and the New Worlds.

Compared to their discussion of the origins of farming, much more strenuous mental gymnastics are required to fashion early urban settlements into initially peaceful, cooperative enterprises. Poor evidence is never allowed to deter us from this conclusion: the poorer the evidence, the bolder the claims. Thus, the authors advance the "speculative" claim that certain structures on the late fourth-millennium BCE acropolis of Uruk may have been assembly halls, which were subsequently razed and replaced with gated courts and ziggurats that appear more consistent with the exercise of priestly and then royal power (306). Yet this explicitly "speculative" take swiftly morphs into fact, turning into "at least seven centuries of collective self-rule" at Uruk (380).

For the Indus civilization of the third millennium BCE, Graeber and Wengrow fill the absence of evidence for kings or warrior elites with the model of a caste system that maintained order—a model inferred from later customs and the presence of a citadel with monumental purification facilities in the city of Mohenjo-daro (317–18). Piling conjecture upon conjecture, they maintain that although the system they envision implies "a clear hierarchy between groups," "this doesn't necessarily mean that the groups themselves were hierarchical in their internal organization," or, for that matter, that the higher caste called the shots in "matters of day-to-day governance" (319). While this last point is indeed impossible to disprove, "necessarily" does a lot of heavy lifting here; yet readers suspected of harboring doubts are promptly chided for their lack of imagination (319). True, there are no iron laws of history, "no necessary correspondence between overarching concepts of social hierarchy and the practical mechanics of local governance" (321), but there are affinities, patterns and trends.

In a case that is even less well understood, that of oblong Trypillian "mega-sites" in what is now western Ukraine, Graeber and Wengrow haul in contemporary Basque rural communities because these "also imagine their communities in circular form … as a way of emphasizing the ideal equality of households" (295). An instant hedge—"obviously, the social arrangements of these existing communities are unlikely to be quite the same as those of ancient Ukraine" (295)—

jars with the "also" that confidently conjoins Basque and Trypillian mentalities. We are left to marvel at reasoning as perfectly circular as Basque communal ideals.

But there is more. The authors fail to mention that the Basque settlements look nothing like those ancient mega-sites: unlike Talianki or Nebelivka, they are not physically round, and not even coherent sites, merely social communities loosely spread out over miles of scenic countryside. The leap from (spatially) circular sites 6,000 years ago to the (mental) "circular template" (295) of Basque villages is breathtaking. Yet although "we must admit that much remains unknown" (297), the former nonetheless offer "proof that highly egalitarian organization has been possible on an urban scale" (297). "Proof"—their word, not mine.

In the end, all we end up with is a single solid case of a large city without clear signs of highly centralized authority, Teotihuacan in the Valley of Mexico. A metropolis of around 100,000 residents in the first half of the first millennium CE, it has not left written records but lacks iconographic evidence of royalty, even as men hailing from there occupied positions of power in Mayan Tikal (331–36). Conventional beginnings—ritual mass killings, ambitious temple projects that must have required vast labor inputs—were derailed around 300 CE (or perhaps rather earlier). Pyramid construction ceased, the fanciest temple was desecrated, and high-quality stone-built multi-household apartment compounds were erected to house the urban masses, arranged around 20-odd local temple complexes that may have provided local coordination. This situation prevailed for several centuries before things started falling apart, culminating in the abandonment of much of the site (341–45).

Nobody knows who ran the city during the second phase: some layer of overarching control, for instance by religious authorities, is an option, as is more dispersed authority centered on the local temple complexes. The relationship between these precinct installations and the general population is likewise unknown. While the city's grid design implies strong governance, this does not allow us to infer whether it was centralized or collective in nature: both options are empirically attested elsewhere. What *is* known is that in terms of its design, Teotihuacan was unique in the sense that it was without precedent and successor among Mesoamerican cities (Smith 2017). Palaces in particular were common and readily identifiable in other Mesoamerican cities from at least the early first millennium CE onward, whereas Teotihuacan may well have lacked them entirely, just as it lacked the otherwise ubiquitous ball courts. Conversely, other cities did not feature comparable multi-household compounds. Something unusual was clearly going on, and a non-royal form of governance seems more likely than not. It is unclear how far beyond this we can push the data.

Graeber and Wengrow infer from all that a "surprisingly common pattern" of scaling-up "with no resulting concentration of wealth or power in the hands of ruling elites" (322). Leaving aside the meaning of "common," it is difficult to reconcile this with the evidence they themselves present. By their own account, Teotihuacan appears to have started out in more authoritarian ways and even later may well have been maintained by elite groups, and Mohenjo-daro is thought to have been controlled by a higher caste. Their speculative scenario of a "social revolution" in the previously stratified city of Taosi in Shanxi, China, around 2000 BCE is likewise predicated on several centuries of initial urban formation characterized by "rigid segregation between commoner and elite quarters" and the presence of a palace (324–26).

Nobody can tell if there were religious elites in Uruk well before there were kings, and even less is known about the Ukrainian mega-sites. Moreover (and as Graeber and Wengrow themselves note: 323), other than Uruk, where monarchs become visible by the early third millennium BCE, all of these sites failed spectacularly, not to be replaced by anything comparable. Taken together, all this hardly amounts to a formidable challenge to the standard paradigm that links urbanization to hierarchy and centralized control, nor does it in any way "upend the conventional narrative" (284). We are left wondering—how much revision do prevailing interpretations of the emergence of urbanism actually require?

States

Graeber and Wengrow's extensive discussion of "why the state has no origin" is even more disappointing. Their claim that the "state" has no "origin" turns out to be exceedingly narrow: they argue for varied origins rather than a single one everywhere and every time. I cannot tell who is supposed to disagree with that. From the outset, they commit to a maximalist definition of the "state" rooted in nineteenth-century German theorizing that revolves about concepts of sovereignty and the monopoly on the legitimate use of violence within a given territory (359). Yet scholars do not generally apply such concepts to the kind of pre-modern states covered in this book. In fact, political scientists are inclined to reserve them for European states of the last few centuries, most notably following the Westphalian settlement of 1648.

Readers, however, have no chance to find out as they are deftly steered away from the huge literature on the nature of pre-modern states, produced by scholars in different disciplines who have long labored to develop more inclusive definitions that capture essential elements of statehood. Graeber and Wengrow dismiss these efforts with a flick of the wrist by contrasting their own modernizing maximalist concept of the state with unreferenced alternatives so broad "as to be absolutely meaningless" (359). This binary gives a lay audience no sense of just how much daylight lies between those extremes.[8]

In as much as the authors mention competing approaches at all, they do so in the most cavalier fashion. Witness their casual reference to managerial theories of state formation (360–61)—which consider the state a means of coordinating larger societies and their activities and resources—while similarly prominent conflict theories—which seek the origins of state structures in the pressures of group-level conflict—are passed over in silence. In stark contrast to the relevant literature, warfare barely features in their treatment of scaling-up processes. (Warfare is mostly invoked as a source of captives to be ritually slaughtered rather than as a plausible driver of centralized coordination). Nor is there one word about hybrid models, such as Robert Carneiro's famous circumscription theory that combines conflict, coordination and environmental factors.

Graeber and Wengrow's aggressive lack of interest in any of this shines through when they conclude their scene-setting pages (359–62, to my mind the most misleading bit of the entire book, dashing as it does any hope of reasoned engagement with existing scholarship on state formation) with the rhetorical question of why we should even care if something is defined as a state or not (361–62). While this is fair enough in principle, it seems an odd concern in an eighty-page survey that pits a highly anachronistic vision of the state against earlier forms of sociopolitical organization.

Their vaguely Weberian model of three different bases of social power—control of violence, control of information, and charismatic politics (365 and *passim*)—is a perfectly workable template for the study of early state formation. Yet this approach is not as novel as unwary readers might be led to believe: Michael Mann's well-established quartet of ideological, economic, military and economic power (IEMP) is simply ignored.[9] Given that such different sources would not always and everywhere have been configured in the same way, significant variation in the dynamics of state formation was well-nigh inevitable (367): but that truly is old news.

Instead of acknowledging existing frameworks, Graeber and Wengrow prefer to invoke the most extraordinary strawpeople who are said to "assume that there

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[8] While it may be bad form to reference one's own work, I do so anyway because Scheidel 2013 surveys relevant debates and literature in considerable breadth. Space restrictions prevent even a brief summary here.

[9] At least Mann 1986 is cited in their bibliography. Timothy Earle, a leading authority on the emergence of social complexity, leadership and inequality, is consigned to outright *damnatio memoriae*, which obviates the need to acknowledge his similar tripartite scheme regarding the economic, military and ideological sources of emergent leadership (Earle 1997).

is only one possible end point (…): that these various types of domination were somehow bound to come together, sooner or later, in something like the particular form taken by the modern nation states in America and France at the end of the eighteenth century" (369). Considering how strongly the study of state formation tends to be infused with notions of European or Western *exceptionalism*, nothing could be farther from the truth. Thus, what Graeber and Wengrow present as the "only possible end point" is commonly regarded as a highly unusual and/or even unique outcome restricted to just one small part of the world, which in that regard is viewed as being quite different from all the others.

In a re-run of this specious line of reasoning, Graeber and Wengrow caution us that the ancient Chinese Shang regime of the second millennium BCE "is not the 'birth of the state' in the sense of the emergence, in embryonic form, of a new and unprecedented institution that would grow and evolve into modern forms of government" (413). Again, why would it have been? It was the first iteration of a series of state building exercises that over time resulted in more durable and better adapted systems of governance. The authors' reminder that in dealing with ancient archaeological remains, "we'd be wise to resist projecting some image of the modern nation state on their bare surfaces" (440) is even wider off the mark. Who does that?

As the authors survey historical cases, their insistence on a maximalist concept of the state forces them into the already familiar position of all or nothing: if an early polity did not match narrowly defined standards, it was probably not much of a state at all. For example, in a claim that there is "no compelling evidence that ancient Mesopotamian cities, even when ruled by royal dynasties, achieved any measure of real territorial sovereignty" (368), the modifier "real" does a lot of work. State formation was a very drawn-out process, just as the spread of farming, say, or the creation of durable cities that did not fail after a few centuries, and one that is in fact still ongoing. Today's US looks quite different from that of just a century ago.

Graeber and Wengrow seem either unaware of or uninterested in this: they imply that state formation somehow precedes actual state-ness, a perspective reflected in their comment that if the Inca empire "is to be considered a state, it was still very much a state in formation" (372). Yet all states were and still are. Moreover, state formation includes what we might call state deformation—the abatement of the concentration of coordinative capacity. Detours, even collapses, cannot be used to invalidate the story: they are simply part of it. By the same token, the authors' metaphor of "play kingdoms" that became more substantial over time just as "play farming" morphed into "more serious agriculture" (429) is perfectly compatible with conventional notions of state formation. All of these processes could be, and often were, very drawn out.

Word games add nothing of substance. It genuinely does not matter that "it never occurred to Spanish conquistadors to ask whether or not they were dealing with 'states' since the concept didn't really exist at the time. The language they used, of kingdoms, empires and republics, serves just as well, and in many ways rather better" (427; also 370). After all, the world has moved on over the last 500 years, and we have come up with plenty of novel analytical concepts to apply to past and present. For some reason, Graeber and Wengrow are greatly exercised by this trivial fact, juxtaposing "historians," who commendably still speak of kingdoms, empires and republics, with "social scientists," who prefer to talk about states and state formation (427–28). None of this matters at all unless we were to define kingdoms, empires, republics and states as four completely different categories, which we have no reason to do unless we subscribe to maximalist concepts of the latter.

Further strawpersons appear in the form of their claim that "it is often simply assumed that states begin when certain key functions of government—military, administrative and judicial—pass into the hands of full-time specialists" (428). How many scholars believe that? It is well known that pre-modern officials were often members of the propertied elite who performed their duties on the side, in order to bolster their status and income when they were not occupied with managing their estates and suchlike affairs. Standing armies were rare throughout history (and note that the most plausible candidates for early full-time specialists, namely scribes, go unmentioned). It will be news to the much-vilified "social scientists" that they have somehow contrived to remain unaware that there was a "gap between what elites claim they can do and what they are actually able to do" (430)—which was inevitably true in the past just as it is today.

Graeber and Wengrow confuse space and people. Thus, their observation—intended to de-center state formation as a key feature of human history—that for "most" of the last 5,000 years, "cities, empires and kingdoms" were "exceptional islands of political hierarchy, surrounded by much larger territories whose inhabitants (…) systematically avoided fixed, overarching systems of authority" (382) is technically correct yet wildly misleading by prioritizing territory over population number. As far as we can guesstimate, the majority of our species has been claimed by polities with entrenched political hierarchies for several thousand years. Around the beginning of the Common Era, up to three quarters of all people on earth lived in just four Eurasian empires.

The authors' equation of the absence of monarchy with democratic politics likewise merits comment. Their chosen example is early sixteenth-century Tlaxcala, a quadripartite republic in east-central Mexico, whose form of government, in Hernan Cortés' own words, "is almost like that of Venice, or Genoa, or Pisa, because there is no one supreme ruler. There are many lords all living in this city, and the people who are tillers of the soil are their vassals, though each one has his lands to himself, some more than others. In undertaking wars, they all gather together, and thus assembled they decide and plan them." Graeber and Wengrow cite only the first of these sentences (347): the elision of lordly power makes it easier to turn this system—governed by a council of some 50 to 100 mostly but not exclusively hereditary nobles that was coordinated by four principal leaders[10]—into a "democracy" (354; also 357–58) with a "mature urban parliament" (353). They instead choose to emphasize a later account of how those wishing to serve on the council had to undergo self-abasing and painful preparation (356), yet without mentioning that public bloodletting rituals had likewise been common among powerful Maya elites, hardly paragons of democratic governance.

In 1519, we are told, the Tlaxcalan council deliberated whether to support the Spanish. Graeber and Wengrow make much of elaborate speeches proffered by the Spanish scholar Cervantes de Salazar some 40 years later: they take them at face value, despite the give-away inclusion of a remark by one of the speakers that such an alliance "would make ourselves into slaves" (353)—they even note guilelessly that he "was quite right about all this" (585 n.55). This literalistic embrace of the tradition is meant to document "the facility of its politicians in reasoned debate" in the "democracy of Tlaxcala" (354).

Yet no matter how much faith we put in the finer points of this third-hand narrative, competitive rhetorical performances by oligarchs do not *eo ipso* translate to "democracy." Just as aristocratic politics tend to be a key element of formally monarchical systems, aristocrats cutting out kings to sort things out among themselves simply practice a more overt form of elite rule. That can involve more or less democratic processes—the interminable debate over how democratic the Roman Republic was is a case in point—but the specifics need to be established rather than assumed. By themselves, the absence of kings and the presence of councils and even assemblies are a poor guide to political power dynamics.

Much as in their discussion of agriculture, Graeber and Wengrow eventually revert to concessions. "Overall, one might be forgiven for thinking that history was progressing uniformly in an authoritarian direction. And in the long run it was; at least, by the time we have written histories, lords and kings and would-be world emperors have popped up almost everywhere" (323). "There is no doubt that, in most of the areas that saw the rise of cities, powerful kingdoms and empires also eventually emerged" (362). James Scott's "compelling description of how this agricultural trap works"—his model linking state formation and grain farming (Scott 2017)—receives a sympathetic hearing (444–45). At one point, the authors

[10] Stasavage 2020: 41–42, who also provides the Cortés quote.

even appear to make their peace with "state formation," which "can in fact mean a bewildering number of very different things. … But … the range of possibilities is far from limitless" (439). Every word of this is true, none of it new. It is a simply a brief summary of what students of early states have been saying for many decades.

Underneath Graeber and Wengrow's combative rhetoric lies a survey of different modes of state formation in different parts of the globe, mostly in Mesopotamia, Egypt, Mesoamerica and the Andean region, framed by stimulating observations about the specific configurations of the main bases of social power. Two elements invite further exploration. One is the authors' reconstruction of first-order and second-order versions of political structures, where second-order systems combine two of the three bases to increase their capacity for control and collective action (413). This is a promising template, even as it sits uneasily with their own verdict that "seeking the origins of the state is little more than chasing a phantasm" (427). Seeking out such origins is exactly what their model is designed to do.

The second point worthy of note is their rejection of a nexus between state power and "civilization" (431–33), and more specifically their critique of deeply entrenched metaphorical language of "pre," "post" and "intermediate" periods that in practice tend to value state power and the fine arts above all else (378–83). "Intermediate" periods, when strong states failed and freedoms expanded, might well have offered a better deal to the masses (378, 382). Yet even in that respect, by their own admission, Graeber and Wengrow are not as "radical" as they might have been, for a "truly radical" account would focus on precisely those in-between times and places, whereas "for the most part, we are telling the same old story" (382), merely shorn of what they perceive as the teleology of rigid stage models.

But much of that teleology has already gone out of fashion (447), and through their own concessions the authors effectively reaffirm the validity of overall trends. In the end, just how different is their account of early state formation not only from the dominant positions of recent scholarship—which of course always benefit from synthesis and popularization—but also—at least in broad outlines—from what we have always thought?

Alternatives

One could be forgiven for wondering if Graeber and Wengrow share these doubts. After all, just as they wrap up their discussion of state formation, they decide to move the goalposts. Ostensibly playing devil's advocate in noting overall trends and outcomes that are perfectly consistent with the standard narrative—"surely what matters is how things ended up"—what they really do is narrow their focus to contingency: "all you are saying is that the inevitable took a little longer to happen. That doesn't make it less inevitable" (443). Couldn't we say, they muse, that we are dealing with foregone conclusions: just as in the case of farming, state building took a long time but became a big success: once grain states grew, so did their populations, and they outcompeted other forms of organization (443–44).

That, of course, is exactly what happened. But did it *absolutely have to be* that way? "How inevitable, really, were the type of governments we have today, with their particular fusion of territorial sovereignty, intense administration and competitive politics? Was this really the necessary culmination of human history?" (446). The very moment Graeber and Wengrow concede that ultimate outcomes do matter, they retreat to ponder their "inevitability." Taken literally, the notion of inevitability sets a pretty high bar, much harder to clear than documenting patterns and trends.[11] Is that their final stand, their last chance to prevail on a technicality?

How can we even hope to establish a lack of inevitability? Counterfactual reasoning might be an option, but that they dismiss as "at best an idle game" (449). Where are real-life alternatives to be found? Graeber and Wengrow come perilously close to conceding that the Old World was a lost cause all along: the emergence of grain farming and more capable states in multiple locations together with growing supra-regional interconnectivities made the triumph of the state hard to avoid (450).

The Americas, preserved as a fully separate world system up to 1492, are "the one truly independent point of comparison" (451). For this reason, "[i]n the case of the Americas, we can actually pose questions such as: was the rise of monarchy … inevitable? Is cereal agriculture really a trap …?" Will others follow the example of precocious autocrats? "Judging by the history of pre-Columbian North America, at least, the answer to all these questions is a resounding 'no'" (451).

This sets the scene for the final showdown. Without saying so in so many words, their use of "actually" and "at least" obliquely concedes most of the world to deterministic social evolutionism. An awful lot of weight comes to rest on North America as the only real option even in the New World, once the emergence of the

[11] There is some slippage in their earlier observation that "the course of human history may be less set in stone, and more full of playful possibilities, than we tend to assume" (25). The latter they strive to document at length; yet the former does not logically follow: variation along the way is compatible with predictable or convergent outcomes. Cf. the final conclusion of the careful and detailed review by Paul 2022 that "in Hinblick auf die Leugnung historischer Großtrends, struktureller Irreversibilitäten und, wenn nicht rundheraus überzeugender, so doch in mehr als einem Fall diskutabler Modellierungen derselben, liegen die Autoren schlichtweg falsch." ["with respect to the denial of historical macro-trends and structural irreversibilities—and of models that even if they are not completely compelling are in more than one case worth discussing—the authors are simply wrong"].

Aztec and Inca autocracies from more diverse beginnings effectively put Central and South America out of play as well (370–78).

Graeber and Wengrow sketch a familiar trajectory for south- and central-eastern North America. In the first half of the first millennium CE, the Hopewell culture managed to produce substantial earthworks despite its limited investment in farming (457–60). Following its decline, maize cultivation and warfare became more frequent (464). The next step was the creation, in the eleventh century, at Cahokia in southern Illinois, of a large site of maybe 15,000 residents, marked by monumental construction, social hierarchies, mass killings, and elite control over the city and its hinterland, plus extensive cultural influence elsewhere—in other words, the emergence of an incipient grain state. Yet Cahokia was abandoned in the fourteenth century, and smaller successor polities also failed eventually (452, 464–68).

Graeber and Wengrow make much of these failures and an attendant move away from grain farming in eastern North America. However, none of this seems particularly remarkable when viewed in the context of their own book. A much mightier and even more influential polity, Teotihuacan, had likewise failed in spectacular fashion, as had other earlier Mesoamerican and Andean centers. Earlier in their book, they refer to a hiatus in cereal cultivation in central and northern Europe after 4500 BCE (261–62) and relate how the inhabitants of Britain abandoned cereal farming around 3300 BCE to collect hazelnuts while keeping pigs and cattle (105). Across the Atlantic, Amazonians went back and forth between more and less farming for thousands of years (268).

As for North America, they helpfully note that "populations were relatively sparse" (469, also 472), a condition essential in creating affordable exit options—dispersal away from farming-based centers and reversal to foraging. In addition, the absence of horses constrained the capacity of aspirants to project power, thereby curtailing inequality and scaling-up by means of organized violence.[12] Both of these factors sustained an unusual lack of circumscription that maximized flexibility.

In the absence of strong push factors in favor of state formation or a firm commitment to food production, there simply may not be that much to explain here. Collapses and hiatuses were common around the world, even in places where the deck was not as stacked against early states as it was for Cahokia and Cahokianism. Moreover, within a couple of centuries, the impact of the European arrival began to make itself felt, amplifying existing disincentives to farming and state formation. As Graeber and Wengrow note, in Central and South America some 50 million hectares of cultivated land were lost and reverted to wilderness in the

[12] See Turchin et al. 2013; Kohler et al. 2017; Bennett 2022 for assessments of this factor, never mentioned in the book.

wake of European conquest and disease (258). In North America, they also note, the fact that "petty kingdoms" lingered into the sixteenth and seventeenth centuries but failed by the eighteenth is a process "historians seem inclined to see … as in large part a reaction to the shock of war, slavery, conquest and disease introduced by European settlers" (471).

In view of all this—the familiar template of concentration and abatement, the lack of circumscription, the growing impact of European-induced attrition—it is not quite obvious how developments among Indigenous groups could represent a genuine alternative to conventional trends. Yet Graeber and Wengrow, bereft of other candidates elsewhere, need it to be. The result is a narrative in which what transpired after the fourteenth century was not driven by these interlocking factors but instead resulted from a "backlash" against the Cahokian experience—a backlash that "was so severe that it set forth repercussions we are still feeling today" (482). Still felt today, that is, because they believe this backlash to have informed the development of a political philosophy among the Iroquois (physically far removed, by the authors' own admission, from Cahokia: 482) which subsequently inspired European Enlightenment thinking through the intermediation of Indigenous interlocutors (the main theme of chapter 2).

Some might see a tangle of conjectures, others a scenario worth thinking with. Graeber and Wengrow are more confident: "Certainly, the overall direction, in the wake of Cahokia, was a broad movement away from overlords of any sort and towards constitutional structures carefully worked out to distribute power in such a way that they would never return" (491). And it was that "backlash" that allowed indigenous North Americans to "almost entirely sidestep the evolutionary trap that we assume must always lead, eventually, from agriculture to the rise of some all-powerful state or empire; but in doing so they developed political sensibilities that were ultimately to have a deep influence on Enlightenment thinkers and, through them, are still with us today" (492).

As much as the second half of this thesis invites critical engagement by intellectual historians,[13] the first part—the weight attributed to a conscious anti-

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[13] I pass over their extensive discussion of how Indigenous reasoning inspired European Enlightenment thinkers and their ideals of individual liberty and political equality, not because it is not novel or potentially important but because it is best addressed by actual experts. Suffice it to say that their contention that in pre-seventeenth-century Europe, social (in)equality "simply did not exist as a concept" because in medieval literature the Latin terms *aequalitas* and *inaequalitas* or their vernacular cognates were never used to describe social relations (32) does not inspire a lot of confidence in their approach to the history of ideas. This example also reflects their fixation on words over substance: recall the spurious significance they assign to sixteenth-century Spanish not conceptualizing New World polities as "states," noted above.

Cahokia backlash—would seem to make an even softer target, and not only because of its dependence on serial conjecture. More importantly, the special significance Graeber and Wengrow accord to post-Cahokian developments does not mesh well with central premises of their own work. Throughout their book, the authors are at pains to emphasize—and rightly so—the slow pace and gradual nature and frequent detours of big processes such as the transition to full-fledged food production and hierarchical state structures. I have quoted some of their statements to that effect.

Comparatively speaking, North America was not an obvious outlier. Very roughly 7,000 years separated the earliest known traces of crop cultivation and the appearance of archaic states in Central America and the Andes. Lag times were similar in the Middle East and only somewhat shorter in East Asia, the Sahel and southern Africa. In North America, domestication of local crops commenced in the fifth millennium BCE and maize was introduced only much later. In view of timelines elsewhere, the failure of sustainable state formation to catch on in North America prior to the European takeover can hardly count as anomalous. If the anti-Cahokia backlash scenario is the best Graeber and Wengrow can come up with to demonstrate alternative trajectories of social evolution, determinists can rest easy after all.

Diagnosis

The Dawn of Everything has formidable strengths. Its authors' desire and ability to refresh, augment and rebalance conventional narratives by rescuing neglected millennia and muted experiences is impressive and commendable. They succeed in showing "what happens if we accord significance to the 5,000 years in which cereal domestication did *not* lead to the emergence of pampered aristocracies, standing armies or debt peonage, rather than just the 5,000 years in which it did" (523). Graeber and Wengrow are right to remind us of the risk that in more streamlined accounts, "huge swathes of the human past disappear from the purview of history, or remain effectively invisible (except to a tiny number of researchers)" who do not usually reach a wider audience (442). Their own project seeks to center rather than marginalize these experiences (524).[14] In that regard, their book renders us all a big service.

[14] In an important sense, however, those experiences had always been marginal: the authors' claim that "[i]f you bracket the Eurasian Iron Age, … free or relatively free societies … represent the vast majority of human social experience" (523) is true in terms of space and time but not in terms of lives lived (which collectively make up "human social experience"): state-level agrarian societies had disproportionate demographic heft.

A related virtue lies in their critique of classicisms that fetishize state power, stability and the fine arts over freedoms and experimentation, a theme that—as they themselves suggest (382)—would have warranted further elaboration. Their resolutely global perspective—essential for the task they set themselves—is another strength, and especially their detailed engagement with North America. It is true that sub-Saharan Africa comes up short, even as sites such as Jenne-jeno would have supported their argument, but the book is already very long (and sequels might well have followed).

Those qualities, in my view at least, are what makes this book worth reading. At the same time, a number of shortcomings and idiosyncrasies undermine their efforts. Graeber and Wengrow commit to an idealist approach with blinding ardor. In their view, ideas, reasoned deliberation and free choice are the crucial determinants of historical outcomes: material conditions and environmental or technological incentives and constraints pale by comparison. Even as they invoke ecological factors if its suits them, the specter of "environmental determinists" is never far away (204). Yet "determinism" and their occasional admission that ecology and technology made certain developments "possible" are merely extremes that bound a wide spectrum of more balanced explanatory approaches. At the very least, any plausible account of our early history must give due weight to the influence of natural endowments and technological change. More often than not, however, Graeber and Wengrow give short shrift to geography, ecology, demography and technology, and generally steer clear of materialist arguments and explanations: at best, these are brought up just for explicit rejection (e.g., 197).

The authors explain their stance with commendable candor. Yes, they are aware that the "intersection of environment and technology does make a difference, often a huge difference" (205). And so, they concede, environmental and technological explanations (or cultural ones, for that matter) are not necessarily bad. Yet they consider them problematic for a very specific reason—such explanations "presume that we are already, effectively, stuck. This is why we ourselves place so much emphasis on the notion of self-determination" (205).

This may well be the single most important statement in the entire book. The implications are clear. If we pay too much attention to such factors, we might be led to conclude that our own sociopolitical arrangements are too hard to change because they are too heavily constrained by current technology and culture. For Graeber and Wengrow, it does not seem to matter whether that is true or not: what counts the most is that it is ideologically unappealing. And they admit that it is for that reason that they sideline these factors—out of an ideological commitment to the feasibility of change through collective action today.

Lest anyone think that such firm priors might corrupt their readings of our early history, they tell us not to worry—for "precisely where one wishes to set the dial between freedom and determinism is largely a matter of taste." This premise allows them to "set the dial a bit further to the left than usual" (206; hence the title of my essay).

But is it true that the balance of freedom and determinism in shaping the course of history is "largely a matter of taste"? One would think that it is something else entirely, namely one of the greatest intellectual challenges of all: to find ways to gauge, as best we can, where that balance lies. That it might never be possible to accomplish this to everyone's satisfaction is a far cry from giving up in advance and declaring it ("largely") a matter of personal preference.

Establishing a proper balance is hard work. More specifically, it requires precisely the kind of scholarship Graeber and Wengrow shy away from: large-scale mapping and data processing and coding in support of statistical investigation of correlations, probabilities and significance that help us understand the strength and limitations of particular patterns and trends and to weigh the impact of specific factors (e.g., Turchin et al. 2018; Currie et al. 2020). Without such inputs, the exercise of setting the dial is indeed reduced to a mere matter of taste. But it doesn't have to be, and—among inquisitive academics at least—it shouldn't be.[15]

Other problems of substance and style likewise catch the eye. In their thoughtful review, Lindisfarne and Neale 2021 indict Graeber and Wengrow for their neglect of class and class conflict, which strikes me as a legitimate charge. The most interesting question is not so much whether there was a king or a bureaucracy or how powerful they were, but rather in which ways and to what extent elite groups wielded power and enjoyed structural privilege. After all, tax and tribute rendered to rulers and rent collected by those in control of the means of subsistence were merely two sides of the same coin, reflecting the struggle among the few over the resources generated by the many. I have already alluded to the book's relative neglect of warfare as a force in societal development.

I have likewise referred to examples of Graeber and Wengrow's readiness to devise all-or-nothing scenarios in which deviations from simplifying templates are taken to invalidate the templates as such. Their mode of engagement with their peers is also a cause for concern. *Ars longa vita brevis*—whoever paints on a canvas as wide as theirs must needs be selective. There is no doubt that a popularizing account of the dawn of "everything" cannot adhere to the exacting standards of a dissertation literature review. Then again, Graeber and Wengrow err on the side of parsimony, disavowing the conventional approach of setting out different interpretations and explaining their own preference for a particular version by

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[15] Their "anything goes" approach is emblematic of a more fundamental problem, the absence of any discernible method that might guide their discussion: see Morris in this issue, and in greater detail Morris forthcoming.

noting that this would have overburdened the reader (514–55). Not quite: it could easily have overburdened the text. But that's what endnotes are for.

It can be hard to decide what grates the most: the tacit sidelining of scholars who already espoused ideas Graeber and Wengrow present as shiny and new; the tacit sidelining of literature that prioritizes the impact of factors such as geography, ecology, technology, or warfare; the parading of choice quotations by non-specialist writers who espouse outmoded positions as though they represented common failings of contemporary scholarship; or the unreferenced indictment of imaginary positions. Instead of taking issue with fellow historians, archaeologists and anthropologists over substantive points, they prefer to lambast eclecticists such as the physiologist Jared Diamond, the psychologist Steven Pinker, or the primatologist Robin Dunbar.[16] Mercifully, outright insults along the lines of their dismissal of several fellow scholars with the words that "at some point, you have to take the toys back from the children" (529 n.12) remain rare.

Activism

In the beginning, Graeber and Wengrow state three goals: the quest for truth; the desire to make the past less "needlessly dull" by showcasing variety and flexibility; and to avoid the "dire political implications" of standard accounts (3). Bravo to truth: postmodernism has had its day. Also to animation: streamlined narratives can indeed come across as desiccating and reductive (21). Reductionism, to be sure, is the great bugbear of the academic humanities, often branded one of the gravest of intellectual sins. To their credit, Graeber and Wengrow claim to appreciate its value—"one must simplify the world to discover something new about it"—and are only wary of overdoing it—"the problem comes when, long after the discovery has been made, people continue to simplify" (21). Put that way, one can hardly disagree; but they consistently beg the question of where to draw the line. The authors have no doubt who the oversimplifiers are: the (usually unnamed) "social scientists" (22). We can see how Graeber and Wengrow tick all the familiar boxes: the Rankean search for "*wie es eigentlich gewesen*," the fashionable urge to complicate narratives, and the fratricidal animosity of a history and an anthropology that have shifted from the social sciences to the humanities.

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[16] Graeber and Wengrow deal with the only credentialed historian in this cast of villains, Yuval Harari, by twisting his words. He is charged with likening Paleolithic foragers to apes (93) when the quote shows that this was meant to be a metaphor, and is chided for proposing to reconsider the expansion of cereal cultivation from the wheat's perspective (which turned humans into its servants, just as cartoon cats habitually do with their owners), a clever pedagogical gambit that they pretend to take literally (230–31)—and even follow up with their own observation that "[i]t's also undoubtedly true that, over the long term, ours is a species that has become enslaved to its crops" (230)!

But what of the third objective, political purpose? Graeber and Wengrow are certain that we are "stuck"—witness their repeated question of how this came to pass. They are equally certain that something went "terribly wrong in human history," because "given the current state of the world, it's hard to deny that something did" (502). But just how badly have things gone wrong, and in which ways? Is it late capitalism, racist colonialist legacies, the growing threat of environmental degradation, or all of the above? Conveniently, and perhaps by design rather than by chance, readers are left free to choose: a promising strategy given that everyone is likely to be upset about some aspect of human affairs.

Only once does their charge against the present become concrete: "There is no doubt that something has gone terribly wrong with the world. A very small percentage of its population do control the fates of almost everyone else, and they are doing it in an increasingly disastrous fashion" (76). How this notion squares with the fact that the proportion of humankind living in liberal or electoral democracies has increased from next to nothing a couple centuries ago to about a third today is left unexplained. Nor does it account for the concurrent growth in prosperity, health, longevity and knowledge. No matter: while Graeber and Wengrow concede early on that it is hard to argue with the statistics of progress, they query whether "'Western civilization' really made life better for everyone" (18)—truly a high bar if taken literally.

However we define the flaws of the present, the authors seek redress. That, in the end, is what the book is for: an improved understanding of the past will help us improve our own future. To do so, we must first "rediscover the freedoms that make us human" (8); rediscover them, that is, in the historical record. Graeber and Wengrow are aware that their account might be seen as even more tragic than foreshortened teleological versions precisely because it highlights alternatives that once existed but are long gone. "But on the other hand it also suggests that, even now, the possibilities for human intervention are far greater than we're inclined to think" (524). In the context of their own narrative, this "even now" comes out of the blue, even as they present it as axiomatic, as self-evident. We are not told why it is true: it is simply assumed to be. This is not nitpicking: Graeber and Wengrow are proudly wary of anything that smacks of an unquestioned assumption, and eager to challenge it. Yet their own defining axiom is just that.

Is it at least plausible? As a particular way of life became dominant, earlier alternatives slowly but surely lost their relevance, both in terms of their legacy—their impact on our own world—and in terms of inspiration—what they can make us do today. As a result, we are more profoundly shaped by agrarian customs—a fact which Graeber and Wengrow themselves evocatively illustrate (245, 307)—than by the more distant habits of ancestral foragers and "play farmers." This does not justify ignoring or slighting the latter. But it forces us to confront a basic question: how much do these faded traditions have to offer to us today, how can they teach us to make different choices in the present? After all, while it long remained possible to evade the grip of the state, "[w]e no longer live in that world" (446).

Just as in their reading of the human past, Graeber and Wengrow rely on idealistic actuation to bridge that chasm: "If something did go terribly wrong in human history … then perhaps it began to go wrong precisely when people started losing that freedom to imagine and enact other forms of social existence" (502). Change flows from the imagination, both then and now. This, they feel, makes it imperative to revive an earlier, freer imagination. But is it enough simply to remind us that it once existed? Do they who control the past really control the future?

Their idealist purism traps Graeber and Wengrow in a cage of their own making. Acknowledgment of materialist perspectives would have helped them draw more meaningful connections between past and present. If it was their mobile lifestyle and hybrid mode of subsistence that made it easier for Holocene foragers to step in and out of different forms of cooperation than it was for full-blown farmers who found themselves tied to their lands and crops, how do *we* compare? Do service economies, digital tools and globalization hold out the promise of a new dawn? Materialism is not the enemy of historical understanding: it is essential to it. Nor is it the enemy of social activism. It might even be its best friend.[17]

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[17] Or, as Lindisfarne and Neale 2021 put it much more strongly, "any politics of equality or human survival must be profoundly materialist."

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