David Graeber and David Wengrow: The Dawn of Everything: A New History of Humanity

The merits of this book are considerable; so are its shortcomings. The grandiloquence is awesome: "In this book we will not only be presenting a new history of humankind, but inviting the reader into a new science of history, one that restores our ancestors to their full humanity" (p. 24). This may be contrasted with a much more cautious statement at the beginning of the book: "The task is immense, and the issues so important, that it will take years of research and debate even to begin to understand the real implications of the picture we're starting to see" (p. 4). That sounds more acceptable; and a review of the book may take the mismatch between the intellectual *hubris* of the first-quoted statement and the laudable modesty of the other as a guiding thread for further discussion. The authors make some very good and solidly grounded points, but in between, they succumb to temptations of shortcut and speculation.

This is not to advocate a retreat to uncommitted agnosticism. Such a position can never achieve complete detachment from presuppositions, and in any case, this essay is written from a viewpoint agreeing with the authors on some basic issues. The common ground is the critique of evolutionism. To clarify the implications of this shared stance, it will be useful to spell out what an anti-evolutionist vision of history excludes and does not exclude. Most obviously, there will be no assumption of a predetermined or prefigured history, neither in the strong sense of historical necessity nor on the more cautious note of a developmental logic. By the same token, we reject the idea of uniform progression on a global scale, albeit more rapid in some places than others; emphasis will be placed on multiple histories with distinctive trajectories and divergent outcomes. That said, I cannot agree with the claim that the theory of social evolution, inseparable from the doctrine (or, if we prefer, the myth) of progress was simply a conservative project, "developed to counter" the "indigenous critique" (p. 69) of European societies, and that the later left-wing appropriation of it can be dismissed as an aberration. There will be more to say on the "indigenous critique"; but at this point, let us note that the history of ideas is less streamlined than Graeber and Wengrow often seem to think (they are not as sensitive to nuances and complications on this level as they are when it comes to the cultural diversity of pre-literate societies), and the idea of progress is a case in point. It is one of the defining but disputed cultural themes of modern civilization; it draws on multiple sources and allows for conflicting interpretations as well as recurrent expressions of scepticism. Monocausal explanations of its origins are as unconvincing as they are in every other field worth serious inquiry. Hans Blumenberg, whom the authors have obviously not read, has made a strong case for the early phase of the scientific revolution as a key factor in the formation of a secular idea of progress. The experience of early modern European societies, including the "long road to the industrial revolution" [*Van Zanden 2012*] as well as new

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developments in state formation, was an additional factor. As for the spectrum of interpretations, there was neither a pristine conservative version nor one decisive (Rousseauian) misappropriation from the left; there were crossroads of importance for both sides to the conflict, and restarts of the debate with new terms of reference. The authors rightly stress Turgot's argument on "the primacy of technological progress as a driver for overall social improvement" (p. 60); but in the context of the Enlightenment, Turgot was a moderate reformist, and to grasp the broader picture, it is not enough to counterpose him to Rousseau. The most emphatic and multi-thematic Enlightenment conception of progress is to be found in the work of Condorcet, who is nowhere mentioned in the book. And Turgot's intellectual posterity includes the whole tradition of historical materialism, from Marx's writings of the 1840s to Habermas's terminal reconstruction [*Habermas 1976*]; this intellectual current was marked by an effort to retain and refine the insights into technological progress, while at the same time adding necessary complements and correctives.

These remarks are not to be read as a defence of evolutionary theory. But it serves no good purpose to simplify the case against it, and reliance on that kind of criticism carries the risk that rejected ideas might return through the back door. As we shall see, Graeber and Wengrow are not entirely above such reproach (the promise of a "new science of history" is already a reason to suspect affinities with strong notions of progress). However, there are points to note and questions to consider before coming to a final view on that, and first of all, more must be said on corollaries of the anti-evolutionist position. It does not exclude long-term processes with a specific direction, such as state formation, provided they are contextualized within history and not converted into metahistorical dynamics. On the level of concrete analyses, Graeber and Wengrow do recognize such processes, but they are somewhat reluctant to confirm that in theoretical terms (on p. 399 and in the following section, they seem to dismiss the concept of state formation – put in quotation marks – along with the notion of the state as an unequivocally defined stable structure; this is as incompatible with the actual message of the book as it is with historical evidence).

But there is another side to this example, and it brings us to one of the strengths of the book. Drawing on the work of Pierre Clastres and James Scott (Michael Mann would also deserve a mention in this context), but adding a rich survey of anthropological evidence as well as more speculative lessons from archaeology, the authors show that state formation has, time and again in highly varied settings, provoked a countertrend. There are cultures, strategies and long-drawn-out processes of resistance to state formation, and the fact that in the very long run they have been unsuccessful should invite reflection rather than simple equation with a verdict of history.

One more implication of the anti-evolutionist research programme deserves comment. If the idea of a lawlike or at least globally intelligible history is abandoned, doubt will also be cast on notions of coherent overall patterns, emerging in the course of ongoing progress. Graeber and Wengrow apply this revisionist view to traditional opinions about the origin of civilization, commonly identified with the combination of statehood, urbanism and writing: "Where we once assumed 'civilization' and 'state' to be conjoined entities that came down to us as a historical package (take it or leave it, forever), what history now demonstrates is that these terms actually refer to complex amalgams of elements that have entirely different origins and which are currently in the process of drifting apart" (p. 431). Here we need not discuss the concluding hint at a diagnosis of our times, except to note the

affinity with political philosophers (notably Raymond Geuss) who question the coherence and signal an incipient break-up of liberal democracy. The claims regarding disparate origins and contingent amalgamation are backed up by plausible analyses of fragmentary but by no means inconclusive evidence. Recent archaeological discoveries suggest that settlements on an urban scale are older than previously assumed, and may even have preceded the definitive shift to agriculture. The experiments with recording systems that may be seen as a prehistory of writing were not linked to the administration of state-size groupings. Finally, there is no datable beginning of the state and no single factor responsible for its formation.

In general terms, there is no reason to disagree with the emphasis on historical combination of diverse sources, rather than a structural unity of co-belonging elements. The authors would probably agree that the exaggeration of unity and simultaneity has to do with a particularly impressive and influential case: the archaeological discovery of ancient Near Eastern civilizations and the decipherment of their written legacies. That was a prime example of statehood, urbanism and writing coming together and reinforcing each other. But it can also serve to justify a relativizing step beyond the argument developed by Graeber and Wengrow. Was amalgamation the whole story, or can it be shown that it gave rise to an emergent order and a new kind of tradition? There are, in my view, good reasons to stress the latter aspect. In Mesopotamia and Egypt, the convergence of separate strands did result in a synthesis with new horizons and possibilities of articulation, from imperial ambitions to literary expressions of self-reflection. The novelty and (for better and worse) epoch-making reach of this breakthrough is most evident from its impact on later periods. On one hand, the homelands of the innovation went through cycles of breakdown, revival and adaptation, but in a different overall fashion. In Egypt, a sequence of crises and restorations on the same core territory includes a phase of imperial expansion and a fascinating episode of failed religious revolution from above, attempted by Ikhnaton in the fourteenth century BCE. Here we should note in passing that Graeber and Wengrow draw attention to positive features of the crises, at least in the sense of more local autonomy; that is a valid point, and not to be confused with now forgotten Marxist attempts to dignify the downfall of the Old Kingdom as the first social revolution in history. In the Mesopotamian area, the most pronounced feature of the long-term trajectory was a succession of imperial ventures from different quarters, but sustained and cumulative enough to exemplify a "prehistory of imperialism" [*Liverani 2017*].

On the other hand, Egyptian and Mesopotamian patterns of civilization became "significant others", models of reference for other cultures, in both a positive and a negative sense. Two cases stand out as particularly momentous. Ancient Greece and ancient Israel drew on skills and examples developed in the older civilizational centres, but at the same time, they defined their most distinctive cultural orientations in opposition to the dominant ones of the Near Eastern environment. The outcomes of these double-edged interrelations were to have a decisive impact on world history.

So far, I have focused on the overall framework of Graeber and Wengrow's argument and underlined a far-reaching but not at all unqualified agreement. It remains to take a closer look at the reasonings and narratives that serve to spell out the message of the book. This part of the review must begin with comments on understated presuppositions, complementary to the critique of evolutionism but much less elaborated. The most crucial

point is briefly touched upon when the role of the imagination in the formation of social units has to be noted (p. 276; see also the references to human beings as "fundamentally imaginative creatures", p. 121). The authors then cite Elias Canetti as an authority on this matter. For their project, which is "mainly about freedom" (p. 206), it is essential to admit the "extreme variability" of social-historical forms, "raised primarily in the imagination", and involving "conscious experimentation" (p. 282, 283). They tend to emphasize the positive side of this imaginary reach; a mention of its role in the creation of large-scale structures is followed by the observation that this enables "amicable relations" (p. 282) with people whom the creators have never met. It is true that references to humans as "creatures of excess" and therefore "the most creative, and the most destructive of all species" (p. 128) can be found elsewhere in the book; but there is next to nothing on the interconnections of the two aspects, except for a brief discussion of cultures imagining and identifying themselves in contrast to extravagances practiced elsewhere (the example in question has to do with native American societies on the northeast coast of the continent). That point is valid and instructive, as far as it goes; Graeber and Wengrow give an interesting twist to Bateson's concept of schismogenesis, by applying it to intercultural (and, more tentatively, intercivilizational) contacts and relations. But it is not a substitute for general reflections on the ambiguities of the imagination; and here we need to bring in a source left unnoticed by the authors but eminently relevant to the questions they are asking.

The present writer would be the last to deny that there are lessons to be learnt from Canetti; but as far as the imagination is concerned, his contribution pales in comparison with the theorist who did most to focus the attention of social theory on this subject. Cornelius Castoriadis may be described as the great absence in Graeber and Wengrow's book. In his writings, and especially in his main work, *The Imaginary Institution of Society* [*Castoriadis 1987*], we find a thoroughgoing analysis of imaginary components at work in the constitution of the social-historical. He emphasizes the ambiguity of the imagination, in a double sense and with consequences that have a significant bearing on themes discussed in the reviewed book. On one hand, the human creations that make history include monstrosities as well as meaningful works, structures of oppression as well as networks of communication and spaces for reflection; and the different types can intertwine in ways that make it difficult to envisage historical alternatives. On the other hand, it is a prominent feature of human history that the social imaginary projects its own creative powers onto meta-social forces or principles – gods, ancestors or cosmic orders. The result is what Castoriadis calls the institution of heteronomy. There is no doubt that he exaggerated the contrast between heteronomous and autonomous societies (the latter being defined by their ability to consciously grasp and implement their own capacity for self-determination), and the picture of history (apart from Greek and modern breakthroughs) as an unrelieved sequence of variations on heteronomy is overdrawn. As will be seen, Graeber and Wengrow have things to say that can help to improve on these matters. It is nevertheless the case that heteronomy in the sense theorized by Castoriadis represents a key historical phenomenon, and the book does not give it due consideration. Its reiterated focus on freedom is spelt out in terms of a tripartite division: "the freedom to move, the freedom to disobey and the freedom to create or transform social relationships" (p. 426; these "primordial freedoms" are repeatedly defined in much the same words elsewhere in the book). Two comments seem in order. In the first place, the freedom to "create or

transform" presupposes the ability to question. This is the most fundamental meaning of the autonomy that – according to Castoriadis – has to be asserted against a more frequent and longer-established heteronomy. It explains his strong insistence on the link between philosophical reflection and political invention, exemplified by ancient Greek developments. Secondly, it is a question for comparative historical research whether – or to what extent – the heteronomous order translates into a concentration of power in the hands of elites (that is, for Graeber and Wengrow, the main obstacle to freedom). Such power can be of different kinds, and situations where the impersonal domination of a traditional and/or transcendent order sets limits to visible power are at least conceivable. That pattern – or something close to it – may even be a plausible hypothesis in some of the cases discussed in the book.

Having outlined critical as well as concurring perspectives, we should now turn to closer engagement with the narrative that serves to contextualize the keystones of Graeber and Wengrow's argument. They begin with a chapter on the origins of an "ambivalent story of civilization" and a subtitle referring to "the indigenous critique and the myth of progress" (p. 27). The ambivalent story is the idea that inequalities and oppressive power structures, however regrettable in themselves, are a necessary price to pay for the benefits of civilization; the indigenous critique is a supposedly non-European input into the debates of the Enlightenment, and more specifically a set of ideas traced back to the responses of native Americans to the encounter with European intruders. This latter idea is of major importance for the whole thrust of the book, and merits careful examination.

The main piece of textual evidence for an indigenous critique is a report on conversations with a native American of high standing among his people, written by a French nobleman in exile from his country (pp. 48–61). As related in this source, the indigenous conversation partner takes the position of a "rational sceptic", condemning the unfreedom and inequality that the Europeans have imposed on themselves through "a form of social organization that encourages selfish and acquisitive behaviour" (p. 51). Earlier commentators on the text often assumed that the native American discourse was a fiction, akin to stereotypes of the noble savage; on the basis of convincing circumstantial evidence, Graeber and Wengrow reject this view, but then they seem to conclude that the only other option is to take the report at face value. After a brief survey of recorded reactions to the dialogue, they go on to summarize: "as we've seen, the indigenous American critique of European society had an enormous impact on European thought" (p. 61). That kind of verdict would need more extensive grounding than the book provides; and there are, moreover, reasons to question the whole construction. Since the invocation of an indigenous critique sets the tone for much of what follows (the book might be understood as an attempt to revive an early modern indigenous view with the help of contemporary anthropology and archaeology), the treatment of the primary evidence is worth closer inspection.

The choice is not between dismissing the native interlocutor as a fiction and accepting him as a direct presence. A third option is to admit the composite character of the text in question. Given the background, education and experience of the European author, it is extremely unlikely that he could – or would have wanted to – be reduced to a mere transmitter. The authors allude to Greek influence (p. 50), and it is improbable that such connections could have been limited to style. As for the broader setting, the shift from an oral to a literate culture must have mattered for the final version of the message. Most

importantly, though, there is a historical context that enables – and demands – a complex approach to the issue. Graeber and Wengrow are not unfamiliar with the work of Lévi-Strauss, and their bibliography includes some of his writings; but there is no trace of the short yet very important text on "three humanisms" [*Lévi-Strauss 1973*] and their significance for anthropology. It outlines a global perspective on European encounters with non-European worlds, beginning during the early phase of modernity but with consequences of more lasting importance and still relevant as sources of anthropological reflection. The three humanisms are new perspectives on the human condition and its social framework, opened up by the rediscovery of classical antiquity in the course of the Renaissance, the first contacts with the great civilizations of South and East Asia, and the confrontation – however destructive – with indigenous societies of the Americas. For a historical approach to the Enlightenment, the most obvious implication is that we must allow for multiple global sources, interpreted and intertwined in various ways, and it will not be easy to isolate a single one in anything like a pure form. As is well known, there have been exaggerated accounts of direct Chinese influence, and it is not unreasonable to assume that something similar can happen to the search for native American inputs; they are more likely to be found as ingredients in mixtures, rather than as intact and self-contained discourses.

The main stepping-stones from the "indigenous critique" to contemporary thought are the writings of Jean-Jacques Rousseau, who is credited with founding "the left as an intellectual project" (p. 69), but criticized for portraying his egalitarian savages as devoid of imaginative powers, and thus doomed to fall into the trap of progress accompanied by oppression; and the twentieth-century anthropologist Pierre Clastres, who pioneered the idea of societies actively resisting the formation of the state and inventing protective arrangements against it [*Clastres 1990*]. Graeber and Wengrow have reservations about the over-generalized claims made by Clastres, but applaud him for "insisting that the people studied by anthropologists are just as self-conscious, just as imaginative as the anthropologists themselves" (p. 73). This lesson becomes a guiding thread for the whole argument of the book, and one of the key steps towards broader application is a venture into archaeology. The chapter on "unfreezing the Ice Age" (pp. 78–119) re-evaluates the archaeological evidence on hunter-gatherer societies and concludes that it suggests much more diversity than earlier scholarship – especially its evolutionist offshoots – was willing to admit. Recent discoveries have raised some difficult questions about the possible extent of variations within this early and at first sight resource-poor phase of human history. Some researchers have argued that the "extraordinary burials and monumental architecture" (p. 104) of some hunter-gatherer societies must reflect a complex social hierarchy, but we can agree with Graeber and Wengrow's doubts on this matter. The inference is not compelling. On the other hand, an interpretation which they think is plausible and backed up by anthropological knowledge points to problems not quite adequately confronted in the book.

Their hypothesis is that seasonal variation can account for the apparent anomalies; on that view, the "public works" so out of tune with the poverty and primitive technology of the societies in question are ceremonial sites, only visited or inhabited during a part of the year or on special occasions. This conjecture is then linked to anthropological analyses of seasonal variation, from Lévi-Strauss's well-known account of the Nambikwara back

to an older generation of scholars, including members of the Durkheim school. What is not mentioned is Durkheim's own very instructive work on this theme. In the *Elementary Forms of the Religious Life,* he analyzed seasonal patterns in the life of the Australian aborigines; it is true that his approach is biased by the over-integrated and over-encompassing conception of society that leads him to regard only the phases of close co-presence as authentically and effectively social. But there is another side to the picture. The whole seasonal pattern – the alternation of dispersal and congregating – is subsumed under the collectively accepted myth of an ancestral order, originally imposed by quasi-divine beings. In that regard, it exemplifies the situation that Castoriadis theorized as heteronomy. Graeber and Wengrow take the widespread seasonal pattern to show that "human beings were self-consciously experimenting with different social possibilities" (p. 107), and suggest that this may have extended to early episodes of agriculture. This view is not far removed from Lévi-Strauss's claim that a phase of experimentation must have preceded the neolithic revolution; and a qualified acceptance is still compatible with the idea of a mythically anchored order that sets strict limits to innovation. Such a conception of stateless societies is proposed in Marcel Gauchet's "political history of religion" [*Gauchet* 1999]; we may regard it as over-generalized, but the evidence is drawn from a wide variety of cases, and it must certainly be recognized as one of the most relevant contenders in its field. It is not given its due in Graeber and Wengrow's book.

The following chapters move closer to the supposedly key question of the book: "how we got stuck" in the traps of hierarchy, statehood and private property. Since the authors reject the idea of a grand historical necessity presiding over the course of events, as well as the assumption that a persistently dominant pattern must be rooted in a functional imperative, they do not aim to show that the loss of primordial freedoms had to happen, but an argument as to how it could happen is developed. It is noteworthy that the break-up of large cultural areas into smaller units appears as an important prelude to the transformations of social power. The narrowing of cultural borders facilitates the imposition of controls and commands. The authors do not explain the fragmentation of cultural worlds at any length; their reference to "schismogenesis", a term coined by Gregory Bateson to describe divisions generated by contact, is interesting and suggests that this concept is applicable to intercultural relations, but this idea is not taken far; and although cultures are indeed "structures of refusal" (p. 174), as Graeber and Wengrow note with reference to Mauss, it should not be forgotten that they are also structures of communication, transmission, borrowing and adaptation. More detailed connections are made when it comes to the fatal transition as such. A discussion of ceremonial sites discovered by archaeologists leads to the following conclusion: "What we can suggest … is that such carefully co-ordinated ritual theatres, often laid out with geometrical precision, were exactly the kinds of places where exclusive claims to rights over property – together with strict demands for unquestioning obedience – were likely to be made, among otherwise free people. If private property has an 'origin', it is as old as the idea of the sacred, which is likely as old as humanity itself. The pertinent question to ask is not so much when this happened, as how it eventually came to order so many other aspects of human affairs" (p. 163).

This summary calls for a few remarks. In the first place, the pluralistic approach to the genealogy of power, property and privileged knowledge – emphasizing the role of sacred centres and their expert administrators, but allowing for links to political and economic

domination – is a promising one. It is a good deal more convincing than the suggestion (made without any compelling arguments) that "domination first appears on the most intimate, domestic level" (p. 209). A caveat must, however, be entered in regard to private property. The analogy with the sacred, based on the observation that the object of property rights is "set apart, fenced about by invisible or visible barriers" (p. 159), does not carry conviction; ownership is not central to cultural worldmaking, but the sacred is. A further consideration, concerning the argument as a whole, is that the idea of humans as "creatures of excess" could have been used in a more systematic way. Not that it could serve to restore the belief in historical necessity; but if humans are prone to excess, new possibilities of that kind will not remain unused, and that applies to the accumulation of wealth and power as well as to knowledge claims. Finally, it must be said that the book does little to answer the question indicated in the last sentence of the quote. The way to clarify the joint impact of sacrality, power and property on "other aspects of human affairs" would be to trace the development of archaic civilizations, from early stages of statehood, temple-centred religion and class division to later ones. As will be seen, the discussion of this crucial historical phase is less informed and less instructive than one would expect from a book promising a new history of humanity. Comments on the earliest examples and episodes incline to cast doubt on the presence of full-fledged state and/or class structures, and the arguments are often of the one-sidedly speculative type. The key cases merit closer consideration. But first we should pause to note some undeniably fruitful approaches to the background.

There is no doubt that Graeber and Wengrow's analysis of foraging societies (chapters 5 to 7, pp. 164–275) adds some major insights to the evolving picture of the pre-agricultural world; the settlements, ceremonial sites and complex social patterns of these cultures reflect a much higher level of variety and creativity than the long-dominant simplifying theories of social evolution would allow for. The account of the transition to agriculture as a long-drawn-out process with interruptions, countercurrents and uncertain outcomes is similarly persuasive. But the view that "the first farmers were reluctant farmers who seem to have understood the logistical implications of agriculture and avoided any major commitment to it" (p. 249) seems more doubtful, and may not be quite compatible with the idea that "the whole process was a fairly leisurely, even playful one" (p. 241). I do not think that the authors have convincing grounds to claim that "it was all carried out in ways that made radical inequality an extremely unlikely outcome" (p. 241), especially not if the abovementioned point about creatures of excess is kept in mind.

Doubts increase when it comes to the early agricultural civilizations. In some of the relevant cases, the archaeological evidence is so ambiguous that there is considerable scope for speculation. Two much-discussed examples stand out, the Indus civilization and the city of Teotihuacan in Mexico; both of them tempt Graeber and Wengrow to rather adventurous interpretations. Their comments on the Indus civilization stress the "likelihood" that it had developed "institutions of communal self-governance" (p. 321); they do not reject the possibility of an early caste system with a strong hieratic summit but without the multi-level hierarchy of the later one (that is, as far as the present writer can judge, the most plausible view emerging from scholarly debate) but argue that practical governance may have been of a more egalitarian kind. The point is, of course, that practical governance is not all local, as the "perfect counter-example" (p. 321) of Bali is meant to suggest; egalitarian participation in everyday tasks can coexist with permanent elite struggle at higher

levels of power (as in Bali), or with an encompassing hierocracy (as was, most probably, the case in Mohenjo-daro). The authors take a stronger view on Teotihuacan, described as a culture that "changed its course away from monarchy and aristocracy" (p. 343) towards an emphasis on general well-being and – probably – a division of authority "among local assemblies, perhaps answerable to a governing council" (p. 344). Outsiders should not try to settle this issue, but it is worth mentioning that there exists a very different – and very detailed – interpretation of Teotihuacan [*Headrick 2008*], listed in the bibliography but not discussed in the book; that work stresses the rivalry of different oligarchic groupings. Such a scenario does not exclude broader social conflicts, but that is still a far cry from popular rule and social housing.

If the comments on Teotihuacan are freewheeling, things get a good deal worse when it comes to Mesopotamia. Graeber and Wengrow cite Thorkild Jacobsen's thesis about "primitive democracy" in the Mesopotamian city-states, claim that "scholars in that field have extended his idea even further" (p. 301, no reference), and that "popular councils and citizen assemblies … were stable features of government" (p. 301). They admit in passing that we "know very little" (p. 301) about these institutions, but then they revert to a more assertive mode, quoting C.L.R. James on every cook being able to govern in fifth-century Athens, and adding: "In Mesopotamia, or at least in many parts of it, it seems this was literally true" (p. 302). They can hardly have ignored where James got this from. The metaphor of the governing cook is borrowed from Lenin's *State and Revolution,* and Lenin took this possibility – the simplification of state affairs to a point where every cook could handle them – to be an achievement of capitalism. A leap from Bolshevik mythology to ancient Mesopotamia does not inspire confidence, and the whole story falls apart on closer examination. There is no mention of the fact that Jacobsen's thesis was a highly speculative elaboration of tenuous evidence, and reactions of later scholars have much more often been critical than supportive. Marc van de Mieroop, certainly one of the most authoritative scholars in the field, is quoted as if he agreed with Jacobsen, but in fact his view is very different. He is quite dismissive about the idea of "primitive democracy" and takes it to be an unwarranted extrapolation from late literary evidence [*Mieroop 1997: 133–135*]. He argues that self-government of cities became more important in later stages of Mesopotamian history, especially under the Assyrian empire; but this was a selective development, and more precisely a privilege granted by imperialist rulers to key cities in their heartland. The citizens thus involved in administration were not governing a state; they were subaltern actors in the service of an expansionist power.

Getting the Mesopotamian case straight is of some importance for broader historical perspectives. The Jacobsen thesis has been a hobby-horse of those who wanted – mostly for ideological reasons – to downgrade the Greek experience (it should be noted in passing that the present reviewer happily confesses to a Hellenocentric outlook; Ancient Greece was, as Christian Meier puts it, the needle's eye of world history). Greek history is not a major concern of Graeber and Wengrow's book, but they are obviously inclined to dismiss both the originality of Athenian politics and its affinity with modern democracy. More generally speaking, the last millennium BCE in the Eurasian world is not discussed at a length comparable to the chapters on Neolithical and Bronze Age developments; there is, however, a brief reference to a crucial phase that should not be left uncommented. The paragraph on the Axial Age (p. 450) packs a lot of confusion into a few lines. The claim that

Karl Jaspers believed "all the major schools of philosophy we know today" to have originated during this period is vastly overstated; Jaspers described Kant as "the philosopher pure and simple" (*der Philosoph schlechthin*), and he certainly did not think that Kantian conception of philosophy went back to the Axial Age. The authors then add that Jaspers's periodization "has been extended by others to include the period that saw the birth of all today's world religions, stretching from the Persian prophet Zoroaster (c. 800 BC) to the coming of Islam (c. AD 600)". This is very misleading. The only author who argued for including Christianity and Islam among axial religions was S. N. Eisenstadt, and his approach changed the character of the concept; it was no longer a chronological term for a certain period, but a typological category covering a distinctive group of civilizations. This view did not find favour with other participants in the discussion, and various other revisions of Jaspers's narrative were proposed. The debate is documented in several volumes, none of which is listed in Graeber and Wengrow's bibliography.

As for Zoroaster, there is no consensus about his date; but the main positions now in debate put him either long before or considerably later than 800 BC. Another problematic claim is that the "core period of Jaspers's Axial Age" saw "the spread of chattel slavery across Eurasia". Can this be documented? I doubt it. The historical evidence is limited and fragmentary; but if I am not mistaken, the dominant view among historians of China – for example – is now that chattel slavery was in retreat during the period of the Warring States, i.e. exactly at the time mentioned by Graeber and Wengrow, gained ground under the short-lived Qin dynasty and declined again under the Han.

To conclude, it is probably true that the developments of the Axial Age were "just … what happened in Eurasia" (p. 450); but what happened in Eurasia had very massive long-term consequences for the later history of the whole world. That is, of course, outside the purview of Graeber and Wengrow's book. But it remains to consider a part of their argument that relates more directly to later history than any of the others; it is a revisit of the question already raised about the origins of domination, and this time with a more explicit emphasis on the state.

It is easy to agree with the claim that "the state has no origin" (p. 359) – if we mean that there is no single causal factor to be identified as responsible for its rise, no precise dating of its beginnings, and no simple definition of statehood that would enable us to draw a clear dividing line between stateless and state-centred societies. And it is at least arguable that "modern states are, in fact, an amalgam of elements that happen to have come together at a certain point in human history" (p. 368); that view seems closer to truth than the idea of a coherent system of structures and functions. But if we want to grasp the formation and transformation of states as changing outcomes of historical processes, it will nevertheless be necessary to define basic components of the picture. Graeber and Wengrow do so in a somewhat abrupt fashion. They identify "control of violence, control of information, and individual charisma" as "three possible bases of social power" (p. 365). Since this tripartite division is proposed after a brief brush with Max Weber's theory of the state, a closer look at implicit Weberian connections may help to put the seemingly disjointed bases into perspective. As will be seen, they turn out to be separate aspects of a more complex problematic, and a reformulation would bring that background to light.

"Control of violence" corresponds to Weber's remarks on violence as the ultimate foundation of statehood; but the role of violence in the history of state formation is not

simply a matter of control. A comprehensive view will have to take in the efforts to achieve a monopoly on the means of violence, as well as on the decision to use them; the ways of displaying and transfiguring it, and of unleashing it in exceptional ways; last but not least, the involvement in unpredictable explosions of violence, due to interstate competition and revolutionary upheavals. "Control of information" belongs to what Weber called the level of administration, and more specifically to its bureaucratic version. But bureaucracy presupposes more than governance based on information; it also involves a division of labour and a hierarchy of authority predicated on competence; as the historical variety of bureaucratic regimes and traditions shows, cultural definitions enter into every aspect of this pattern. Moreover, bureaucratic forms of administration often mix with other ones, not primarily based on information, such as the type described by Weber as patrimonial; that phenomenon appears in many guises, including modern ones. A more specifically modern development is the interference of ideology with administration, often in collision with the informational basis. Finally, charisma is a notoriously Weberian concept, although it has had a very chequered later career; Graeber and Wengrow's use of it is the least adequate part of their tripartite model (and the preposterous choice of Kim Kardashian as an example does not help). They refer only to individual charisma and link it closely to competitive politics; their view of modern democracy seems to be "a game of winners and losers played out among larger-than-life individuals" (p. 367), which is a rather dubious generalization (think of the contest for the succession to Boris Johnson – not a larger-than-life individual in sight).

There is no reference to the very extensive discussion that has developed around Weber's analyses of charisma. It is not being claimed that a classic is necessarily a final authority; but an unexplained narrowing of classical perspectives calls for comment. Although Weber had much to say about charismatic individuals, he did not reduce charisma to that particular type. Not only was it, in his view, transferable to institutions and ideas (he even refers to a "charisma of reason" in connection with the Enlightenment and the French revolution); he also refers to charisma as a revolutionary historical force. It seems fair to say that he failed to settle the question of the relationship between personal and impersonal aspects, but at least he pushed the concept of charisma in directions that invite further reflection on basic issues. In the context of Weber's sociological project, it belongs to the level of domination (*Herrschaft*), identified as power with legitimation, but not thematized as such by Graeber and Wengrow. Domination is, for Weber, the mitigating middle between violence and administration: it ensures the linkage of command and obedience, without direct coercion. There is, however, an under-theorized background. The modes of legitimation presuppose cultural definitions of power, which Weber does not explicitly discuss; but the concept of charisma, understood in light of its broadest ramifications, is at least indicative of the cultural and historical creativity that remains a crucial theme for attempts to think with Weber beyond Weber.

To sum up, the above criticisms are not directed against the idea of analyzing historical trajectories of the state in terms of different constellations, made up of elements that are neither intrinsically co-constituted nor invariably associated. Rather, the complaint is that the conceptual scheme used by Graeber and Wengrow to develop this argument is an arbitrary and oversimplified selection from a more complex problematic. An impressive version of that more complex approach can be found in the work of Max Weber, but it awaits further development.

For reasons of space, these remarks will have to be the final point for now. A selective discussion cannot do justice to all parts of an exceptionally wide-ranging work; but to conclude with a balanced though provisional judgment, the opening comment should be re-emphasized. Nothwithstanding occasional lapses into ill-advised bombast, some biased arguments and ideological flights of fancy, this is a hugely interesting book and deserves a wide readership.

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DOI: 10.14712/23363525.2022.22

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