

Josephine Quinn: How the World Made the West: A 4000-Year History

This book has met with instant acclaim. One comment on the cover describes it as “eye-popping, mind-blowing, path-breaking”. The present review will not reach that level of celebration; but credit should be given where credit is due, and before going on to express basic disagreement, the genuine merits of the book will be duly noted. It contains fascinating accounts of intercultural encounters and entanglements; of processes that shape historical regions and can also be seen as steps on the way to globalization; and of cultural worlds emerging in time and space (although the author insists, as will be seen, on rejecting the most adequate way to conceptualize the last-named topic). One of its main strengths is a vast knowledge and effective use of archaeological evidence; but this also means that its best parts are those most dependent on that kind of background, whereas the more problematic aspects of the argument come to the fore when issues of historical sociology are directly involved.

The reliance on archaeology makes it easier to trace the interactive dynamics of trade than those of conquest or the spread of religions, and there are epochs as well as regions where this focus is notably rewarding. The most valuable parts of the book are the chapters dealing with the Bronze Age and early Iron Age Mediterranean; the sections on Ugarit, on the interaction of indigenous and transmarine cultures in the Iberian peninsula, and on Assyrian connections to the Mediterranean deserve particular mention. Quinn’s comments on the complex and recently much-discussed crisis of the late Bronze Age are mostly convincing (for some reservations, see below); she portrays this series of events as a multi-causal process with a regionally diversified impact and great uncertainty about the key actors involved.

Unfortunately, the historical and archaeological expertise brought to bear in this book is mixed up with a misguided and weakly grounded ideological exercise. Professor Quinn wants to purge civilizations from history. “Civilizational thinking” is under attack throughout the book, but Quinn’s understanding of it never gets beyond caricatural versions. To clarify the issues at stake, a closer look at presuppositions and misconceptions is needed. Quinn’s rejection of the civilizational approach is backed up by a thesis formulated in very general terms at the beginning of the book: “It is not *peoples* that make history, but *people* and the connections they create with one other” [30]. But at the end of the book, peoples seem to be back in favour: “The new Atlantic West was like all that came before it the product of long contact with other peoples, ideas and networks that had drawn Europeans into a wider world” [411]. Even if we disregard the self-contradiction, the first statement is at best a half-truth, and a corrective is implicit in its second half: the reference to connections should remind us of the multiple patterns that they assume. Cultures, states, nations and

empires are obvious examples, and the case for a civilizational dimension (on which more below) has to do with a specific type of connections.

Another general premise of Quinn's argument is that "the past does not act on the future; people choose to interpret, develop or adapt what they find there" [30]. She seems unfamiliar with the sociological concept of *habitus* (pioneered by Norbert Elias and more recently popularized by Pierre Bourdieu). A habitus is a link between past and future; it does not exclude choice or interpretation, but constitutes an orienting framework for such responses. And if the concept of habitus refers to the level of action, the corresponding and closely related concept of legacy denotes the same kind of connection at the level of culture; it has been a key category of civilizational analysis.

The introduction to the book contains a very brief summary of the history of "a phenomenon I call civilisational thinking" [3]; it begins with eighteenth-century thinkers and ends with Samuel Huntington, who is credited with giving "a new lease of life" to civilizational thinking" [8]. This is a bit like saying that Leonid Brezhnev gave a new life to Marxism; Huntington's disastrously impoverished notion of civilizations as the "ultimate tribes" has done more to discredit the concept than to revitalize it. But even his deservedly much-criticized description of clashing civilizations is not quite as simplistic as Quinn suggests; the analysis of interrelations between Western and non-Western civilizations (from Westernizing inroads to indigenous backlash) clearly implies that we are not dealing with closed worlds in isolation or collision. Apart from that, the most noteworthy feature of Quinn's summary is that two key chapters of the story are missing. The first is the contribution of classical sociology; the definition of civilizations as "families of societies", proposed by Durkheim and Mauss, put them on the agenda of comparative inquiry and avoided any connotations of closure; Max Weber used the term "cultural world", rather than "civilization", but the equivalence of the two concepts is well established. The second chapter, even more important, is the revival of civilizational analysis by historical sociologists in the last decades of the twentieth century; Benjamin Nelson and Shmuel Eisenstadt are the key figures, but Quinn has clearly not engaged with their work. Both of them rejected the ideas of civilizations as closed worlds; Nelson was especially interested in what he called "intercivilizational encounters". As for Eisenstadt, indisputably the most seminal theorist of civilizations, Quinn could have avoided two basic mistakes if she had taken note of his views. In the first place, it is not the case that a civilizational approach to history necessarily entails a belief in "Western civilization" as a distinct entity; Eisenstadt's alternative to that was the idea of modernity as a new civilization, developed in multiple versions in different historical settings.¹ Secondly, he did not assume that civilizations map "on to specific

¹ Quinn makes much of a supposedly fashionable focus on "Western civilization" in higher education, but the only example she mentions is the "Ramsay Centres for Western civilisation that have opened at three major Australian universities since 2020" [9]. This claim is misleading and deserves a brief comment. There are some 40 universities in Australia, and gaining a foothold at three of them is hardly a sign of great strength; moreover, only one of the three – the University of Queensland – can conceivably be described as a major university. The other two, the University of Wollongong and the Australian Catholic University, are certainly not in that category. Some other universities (notably the University of Sydney, Australia's oldest) have rejected proposals from the Ramsay Foundation, and there has been vocal and widespread criticism of the centres; they are supported by some of the most conservative politicians in Australia, but definitely not part of the academic mainstream. To round off this discussion, it may be added that the present reviewer identifies as a civilizationist, but has never used and will never use the term "Western civilization"; it presupposes a unity and a continuity that are not borne out by historical evidence.

geographies that emerge and develop in isolation from each other” [415]. Civilizational analysis, properly understood, treats the connection between regions and civilizations as a variable, not a constant feature; some regions are more civilizational than others and some civilizations are more regional than others (modernity least of those so far recorded, although it has a stronger genealogical connection with some regions like Western Europe and East Asia than others).

A third author who should be cited in this connection is William McNeill; in fact, Quinn mentions him, but only in a dismissive endnote [419 n. 23], where it is admitted that he “did make the case for communication and diffusion between civilizations, but he focused on the diffusion of ideas from ‘civilisations’ to ‘aliens’”. This comment misses the most interesting innovation in later editions of McNeill’s *Rise of the West* [most recent edition 1991]: the growing emphasis on “ecumenes”, defined as zones where diverse civilizations enter into complex and lasting interactive relations. As McNeill saw it, the first historically documented ecumene took shape in the Near East and the Eastern Mediterranean during the later Bronze Age, prior to the great crisis that engulfed these regions towards the end of the second millennium BCE. Quinn has, as noted above much to say about that time and place, and most of her story would fit nicely into McNeill’s account of the ecumene. This is not the only possible and plausible civilizationist corrective to a line of argument constructed against such views. The case for a civilizational difference between Mycenaean Greece and Minoan Crete is stronger than Quinn would have it (she dismisses it as a modern invention), and many scholars accept it. The defence can begin with a basic fact: the difference between the respective centres that have been excavated. There is no doubt about the character of the Mycenaean ones: they were palaces of rulers with at least some real capacities of bureaucratic control (although I do not think that anybody has ever argued that they controlled “every aspect of their subjects’ lives” [96]). By contrast, the Cretan “palace” of Knossos would seem to have combined religious, political and economic roles in a way unparalleled elsewhere. Another example is the “change of priorities” [98] after the collapse of the Bronze Age palatial cultures. Changes of overall socio-cultural orientations are among the central themes of civilizational theory, properly understood. In this connection, the work of Mario Liverani deserves special mention. He is one of the most authoritative scholars on the history of the ancient Near East and its neighbours; his most extensive coverage of this topic [*Liverani 2013*] throws much light on the change of orientations after the late Bronze Age collapse. Quinn only mentions him in a completely different context, related to the archaeology of the Saharian kingdom of the Garamantes [508 n. 3].

More serious and specific problems emerge when Quinn’s narrative reaches classical antiquity, especially the Greco-Persian wars of the fifth century BCE. There is nothing wrong with trying to reconstruct a Persian view of these momentous events; it has been attempted before, and a convincing version would get us closer to a balanced picture of global history in the making. But Quinn’s anti-Hellenic bias is so blatant that the result cannot be taken seriously. When discussing the rebellion of Greek cities in Asia Minor (a prelude to the Persian war with Athens and Sparta), she does not fail to mention the extremely brutal Persian response. The exemplary punishment of the leading city, Miletos – “much of the population either killed or deported” [199] – was obviously meant to terrorize the others into lasting obedience; but a few pages later, the same events are

revisited, with a concluding remark about measures “suggesting ... considerable Persian faith in popular support for their own hegemony” [203]; readers can only wonder whether this was written with a straight face. When it comes to the wars on Greek home ground, Quinn is at pains to minimize the weight and significance of Athenian and Spartan resistance to the Persian onslaught. She begins with the claim that “most Greek speakers allied” with the Persians, and that “in the end”, the conflict was a “success for Persia” [198]. I will return to the second part of this statement, but the first one deserves a closer look. If the defeated Greeks of Asia Minor were enlisted in the Persian war machine, that can hardly be described as an alliance; they had no choice. As for Greeks outside the empire, Quinn notes on the next page that “most of northern and central Greece either capitulated or agreed to remain neutral, as did most of the islands” [199]. Neither capitulation nor neutrality can be equated with alliance. What remains of the sweeping claim just cited is the never-contested fact that the successful resistance to Persian aggression was not a pan-Hellenic action; but the decisive factor was the ability of the two key Greek states, Athens and Sparta, to ally with each other and mobilize further allies.

Quinn argues that defeat at Salamis and Plataea left the Persians unimpressed: they were “uninterested in holding territory in distant Europe” [211]; but previously she had noted that expansion into Europe – the conquest of Thrace and the submission of Macedonia – secured “access to the valuable metals of the southern Balkans and continental Europe” [193]. It seems a plausible assumption that control of mainland Greece would at least have served to round off these prior gains, and in addition, this had become a matter of punishing ostentatious defiance. The demand for recognition was a part of great power politics, then as now. In any case, Quinn’s own emphasis on the keen interest of Persian rulers in the Peloponnesian war shows that Greek affairs did matter to them. She concludes: “The Persian wars may have been a draw, but the Peloponnesian War was won by the King of Kings” [208]. Both assertions are perfectly absurd. It is true that the Spartans lost the victory after winning the war, but if we look for a longer-term victor benefiting from Spartan failure, it is not the Persian King of Kings. It is the Macedonian kingdom that had been waiting in the wings but then embarked on a successful bid for hegemony in mainland Greece and went on to destroy the Persian empire.

That brings us to Alexander the Great, his exploits and his legacy. Quinn makes a very strong statement on that subject: Alexander “would lead the Greeks east on a mission that collapsed the distinction between Asia and Europe completely” [218]. If this claim could be substantiated, we would be looking at a decisive landmark of global history. But the image of Alexander as a cultural missionary and unifier of peoples or even continents belongs to obsolete historiographical traditions of a kind that Quinn is otherwise keen to dismiss. The debunking of Alexander, initiated by Ernst Badian [2012, a collection of older papers going back to 1958] but now widely accepted, has made short shrift of such notions. The great conqueror now appears as a “ruthless killer of his rivals and those who disagreed with him, a mass murderer in his conquests, and perhaps even an incompetent imperialist” [prefatory remark in Badian 2012: I; the only thing left intact is Alexander’s reputation as an exceptional military genius]. Far from unifying regions east and west of the Aegean, Alexander’s brief rule set the scene for a new round of long-term geopolitical divergence. An inchoate empire gave way to multiple kingdoms in permanent rivalry; but there were significant differences between the Hellenistic monarchies on the European and

Afro-Asian sides (assuming that we can use the continental labels, which are – as Quinn notes – products of the imagination and at this stage still devoid of cultural and political content). For one thing, the Macedonian kings had to fight wars with coalitions of Greek cities, whereas the Seleucids and the Ptolemies never had such problems (despite the fact that the former dynasty's realm included cities with a certain autonomy). For another, the rulers of territories inherited from the Persian empire drew much more decisively on Near Eastern traditions of kingship. There were, in other words, divergent modes of re-stabilizing monarchic rule after an unhinged intermezzo.

The Hellenistic world of warring states was vulnerable on two fronts. On the western side, it proved incapable of resisting the Roman onslaught. In the east, an Iranian backlash beginning with the Parthians and continuing – long after the Roman takeover – with the Sasanians shifted the boundary of Hellenism westwards, though still within “Asian” limits. The long-drawn-out Roman-Iranian confrontation ended with what one historian calls the “last great war of antiquity” [Howard-Johnston 2021]; the massive mutual exhaustion caused by that conflict opened the way for Islamic expansion and thus for a new kind of polarization. In brief, Alexander's comet-like intrusion into history did not so much obliterate boundaries as set the scene for a new round of geopolitical and (*pace* Quinn) civilizational dynamics and conflicts.

The chapters on the Roman Empire and the “rise of the barbarians” do not invite the same kind of criticism as the treatment of Greece and Persia; their best part is probably the discussion of Rome's relations with the world beyond its borders (not least on the African side, where the otherwise rarely mentioned kingdom of the Garamantes is given its due; another impressive section is the portrait of Palmyra). However, the focus on this very aspect throws doubt on the claim that we should think of the empire “not as a collection of lands but a series of coasts” [307]. It is of course an important fact that this was the only pan-Mediterranean empire ever established. But its conquest of Gaul and expansion into Central Europe (although brought to a halt at a certain point) were no less crucial. They broadened the basis of imperial power and enlarged the historical arena of its impact; they also brought the empire into contact with the neighbours who in the long run proved most capable of challenging it on its most vulnerable terrain.

Apart from that, the main problem with Quinn's approach to the Roman Empire is that she bypasses four fundamental questions raised in recent scholarship, all important for global and comparative perspectives of the kind relevant to the “making” of the West. The first has to do with its Greco-Roman character, most convincingly stressed by Paul Veyne [20]; neither the cultural and political profile of the empire, nor its historical legacy, can be understood without proper regard to this dual character. That aspect calls for a civilizational perspective (which Quinn abhors). A closely related issue is the question of the relationship between empire and Christianity. As Quinn puts it, Emperor Constantine “propelled to power an Asian deity, the Christian god” [311]. It is hard to know what to make of this. If the reference is to the original notions of what some historians now call the Jesus movement, the label “Asian” seems meaningless; this was a local heterodoxy in a marginal province on the border between Hellenistic and indigenous milieus, without any meaningful connection to a broader world that might be called Asian (if there is a case for speaking of Asian religiosity in the Roman empire, the term is more applicable to Mithraism than to Christianity). More importantly, Quinn's statement makes short shrift of the

work historians have done on the transformations of Christianity. To make headway in the empire, it first had to be Hellenized (beginning with Paul of Tarsus); and then its alliance with imperial power, beginning with Constantinian turn, was at the same time an opening to Romanization (Peter Heather's recent and excellent study [2022] is a convincing account of this process and its long-term consequences). If Constantine "propelled" Christianity to power, he also – by the same token – changed its character. That transformation was, in turn, closely linked to a more comprehensive change. The understanding of late antiquity as a distinctive epoch, most appropriately dated from the late third to the early seventh century, is one of the most significant recent developments in scholarship on world history, and it went hand in hand with a new interpretation of the empire's final phase: the traditional narrative of decline and fall gave way to a new paradigm, focused on a triple transformation of the Roman world. That geopolitical and geocultural mutation resulted in the rise of multiple "barbarian" kingdoms in the west, the perpetuation of a retrenched empire in the east and the emergence of Islamic power on conquered territory east and south of Christendom. This major revision of long-established perspectives does not enter into Quinn's version; some examples of work related to late antiquity; but there is no trace of this theme in the main text, and when Quinn writes that "scholars have agreed since the sixth century that the final 'fall of Rome' to the barbarians should be dated to 476 CE, and that it was a catastrophe" [316], she seems to ignore a widely accepted turn against that consensus. Finally, the question of Rome's long-term impact on European history, recently activated by Walter Scheidel's work [2021] is absent from the book. What was most important: the political and cultural legacy of the empire, or – as Scheidel argues – the escape from it? Or was it the fact that it was partially restored in the shape of two competing centres, the papal monarchy and the Holy Roman Empire?

The last question brings us to the Middle Ages and the final sections of the book. Given the wealth of sources and the complexity of problems raised by relevant scholarship, this part of Quinn's narrative is more perfunctory than those dealing with earlier times, and it cannot be said to do justice to the developments that have led generations of scholars to reflect on medieval origins of modernity. The great transformation often described as a twelfth-century renaissance is not given its due. In fact, Quinn suggests that this very time saw a "trend towards cultural isolation in Europe" [373]. But not much later, she seems to stress the very opposite characteristic. The thirteenth-century Mongol empire is credited with creating "an economic ecosystem across three continents where curious observers from Europe were learning fast" [384].

One particular offshoot of medieval European history calls for more detailed comment. Quinn's excursus on the Nordic countries is a bit on the imprecise side. She writes: "although the English had converted in the seventh century, Christianity reached Denmark only in the tenth and came later still to Norway and Sweden" [344]. It depends what we mean by "reach". In fact, Ansgar's mission to Denmark (sponsored by Carolingian rulers) began in 826; it had a significant impact and was favourably received by rulers, although they did not convert; his attempts further north were less successful. The first Danish king to convert, and credited with Christianizing the whole country, came to power around 960; the first Christian king in Norway, who ruled at least over the central part of the country, was killed in battle at roughly the same time. A violent Christianizing campaign was launched by another Norwegian king at the end of the tenth century, and the first Swedish

ruler to be baptized was his contemporary. It is true that a pagan backlash lasted longer in Sweden than elsewhere in the region.

The comments on medieval Iceland are more off-target. The developmental path of Icelandic society was very different from the Scandinavian kingdoms, but it was not a “republic of farmers” [345]; this is a romantic idealization, now unanimously rejected by Icelandic historians. Its political regime was an oligarchy of chieftains who monopolized legislative power. The Icelandic parliament is not “the world’s longest surviving one” [344]; this is a nationalist myth, now recycled for the benefit of tourists. The medieval *Althing* was not a representative institution in the modern sense; it was an assembly of chieftains; farmers did attend and were called on for jury service, but took no part in legislative procedures. Under Norwegian and later Danish rule, this assembly lost its original legislative power, but retained judicial functions; even that remnant was abolished late in the eighteenth century. The Icelandic parliament established in the 1840s was a new foundation, part of the constitutionalizing process then taking off in the Danish kingdom.

What did set medieval Iceland apart from continental Europe was a vastly lesser social distance between chieftains and farmers, compared to the relationship between lords and peasants in feudal societies, and the absence of monarchic rule. These features were crucially important for cultural life. But Quinn’s remarks on that subject reflect outdated views. No scholars will now accept that the Icelandic sagas are “family stories ... passed down for generations before they were committed to writing in the thirteenth and fourteenth centuries” [345 *n.*]. The sagas are written compositions, drawing on oral traditions, but in ways so various that the relationship between sources and authorial creation – to the extent that it can at all be clarified – is best seen as specific to each individual case.

The final chapter of the book (apart from a very brief and declamatory postscript on “a new world”) deals with the fourteenth and fifteenth centuries. The disastrous impact of the fourteenth-century plague is aptly described and disagreement about its long-term consequences duly noted, but otherwise the main emphasis falls on “a monoculture ... emerging – and not from ancient roots, but out of ethnic cleansing and imperial conquest” [404]. The symptoms of such a development are chiefly seen in declining commercial and cultural contacts with Eurasian lands previously united under Mongol rule, as well as in expulsions of Jews and Muslims. But a couple of pages later, Quinn stresses the active interest in contacts with the rising Ottoman empire after its final victory over Byzantium; and at the end of the abovementioned postscript, medieval and early modern signs of “confrontation between Europe and Asia” are reduced to “intermittent glimpses” [414]; the real closure and self-canonization of the “West” now seems to be a matter of the nineteenth century, and that point is accompanied by a final blast at “civilisational thinking”, no better aimed than the preceding ones. In this connection, it is worth noting that the best available discussion of Eurocentrism in relation to Asia [Osterhammel 1998; English translation 2018] is not mentioned anywhere in the book.

The vagueness of Quinn’s claims about the timing of Western closure is not unrelated to a more serious flaw of her argument. If we are to think of Europe (and, in due course, its overseas extensions) as the modern embodiment of the “West”, we must pay attention to its internal dynamics as well as to the conditioning global processes. That means – not least – placing due emphasis on the irreversible division of Western Christendom, unfolding from the Reformation to the Thirty Years’ War. It is true that the main landmarks of this process

belong to a time after the closing date of Quinn's narrative. But there were fifteenth-century antecedents: the schism of the Catholic Church, the council trying to put an end to it, and – most importantly – the Hussite revolution in Bohemia, not mentioned in the book, which can be seen as the first major episode of the Reformation.

After this inevitably rapid tour through the book, it is tempting to add a brief comment on the author's lapses into the kind of "civilizational thinking" which she otherwise wants to consign to oblivion. I will limit myself to two cases. Quinn is appropriately critical of modern misconceptions about homosexuality in classical Athens and sums up the issue in the following terms: "What we do know is that although modern Western ideas about human sexuality often assume that people have a consistent 'sexual orientation' towards one or more genders, Athenians conceptualized sexual behaviour and even desire very differently, with reference instead to age, status and stages of life" [202]. At the beginning of the book, she had blamed "civilizational thinking" for assuming "enduring and meaningful difference between human societies" [9]; if the abovementioned difference between Athenians and moderns is not meaningful, it is hard to see what would count as such. Moreover, the kind of difference posited by Quinn corresponds exactly to a pattern foreshadowed in the civilizational adumbrations of classical sociology; in the last chapter of his work on the elementary forms of religious life, Durkheim referred to civilizations as different ways of conceptualizing basic aspects of the human condition. The other example is the (also very appropriate) description of Palmyra as "a borderland between worlds" [289]. Which worlds? Obviously the Greco-Roman and Iranian ones; and they can surely be described as cultural worlds, which is – as we have seen – an alternative label for civilizations.

There is more to be said on various details and implications of Quinn's 4,000 year history, but no space here for further discussion; it seems best to conclude on the note that this book deserves to be widely read, but should nevertheless be taken with a load rather than a grain of salt.

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