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## **Toward a New Universal History?**

A Review Essay on *The Great Holocene Transformation* by Peter Turchin (Beresta Books, Chaplin, CT USA, 2025)

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This essay relates Peter Turchin's *The Great Holocene Transformation* (2025) (GHT) to the development of "universal history" as a genre of scholarship. It identifies a paradox: while unprecedented datasets and computational methods now make systematic planetary history possible, academic historians remain disengaged from complexity science approaches. Citation analysis documents detachment rooted in methodological divides. This mutual isolation deprives historians of much-needed theoretical frameworks and limits complexity science's influence on historical scholarship. Meanwhile, it may finally have become possible to develop a new form of "universal history" that draws on standardized datasets and computational analysis. In order to succeed, this would require the lowering of barriers to engagement across fields, a process in which AI may have an important role to play.

### **Lineages of universal history<sup>1</sup>**

Attempts to grasp the full sweep of human history have a long pedigree, rooted in myth doubling as history: witness the accounts in the ancient Hebrew tradition of how Noah's descendants repopulated the earth after the Flood. Early historians such as Herodotus and Sima Qian extended the scope of their narratives beyond their respective homelands – the Greek Aegean and Han China –, as did more elaborate Hellenistic treatises that have been lost. Globalizing history subsequently flourished in the medieval Islamic ecumene. While al-Mas'udi's *Meadows of Gold* and Rashid al-Din's *Compendium of Chronicles* privileged narration, Ibn Khaldun's *Prolegomena* – which has not coincidentally played a

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<sup>1</sup> I offer these observations as a historian of the premodern world with a long-standing interest in the kind of work represented by the book under review. Rather than commenting on the substance of Turchin 2025 I focus on how it relates to broader issues regarding scholars' efforts to understand human development over time.

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prominent role in Peter Turchin's own thinking – put greater emphasis on theory and explanation.

European expansion finally widened the historiographical vision to the entire planet, allowing Georg Horn in the 1660s to cover not only much of Eurasia but also northern Africa and parts of the Americas (Hornius 1666). Much relevant work was encyclopedic in style, an approach that culminated in the massive multi-authored *Universal History*, from the *Earliest Account of Time to the Present* that was published in Britain between 1736 and 1766 in 51 volumes (and in 64 volumes by the time of the third edition of 1779-84). Data collection and regurgitation dominated.

Enlightenment scholars were slow to move beyond these limitations, mostly in aspiration if not execution. At the University of Göttingen that had been founded to promote Enlightenment causes and innovative research, Johann Christoph Gatterer spent much of this career trying to work out how to structure universal history: by period via synchronized accounts, by connection and interaction, by theme, or (ideally) all at once – the latter an undertaking literally impossible within the two dimensions of the printed book.<sup>2</sup>

Those problems have lost none of their relevance: contemporary world historians continue to grapple with them with no consensus in sight. For present purposes, what makes this episode in early modern scholarship all the more interesting is that its driving ambitions align with those of the study of cultural macroevolution. As Gatterer's local colleague-competitor August Ludwig Schlözer explained in his own introduction to universal history in 1772, "[w]e want to survey the revolutions of (...) the human species, to which we belong, in their entirety, to understand the causes of its current condition" (Schlözer 1772: 1). Yet the sheer extent of change over time (and space) turned this into a daunting challenge. Whereas "[a]ll humans are creatures of the same kind," (4), they are all so different now. "Humanity has undergone revolutions. It is not anymore the way it used to be (...)" (4). "What circle of changes did the inhabitants of this world have to go through before they reached their specific stage of culture!" (7). In his view, such changes had been caused by what we would now call adaptation to environmental conditions in the broadest sense of the term: "A human being is nothing by nature, and can become anything by means of conjunctures" (6). The universal historian's job is to track how this came about: "We still lack a shared vision that encompasses the whole: this powerful vision converts aggregation [of evidence] into a system, reduces all states of the world to a single unit, that of humanity, and assesses peoples only in their relation to the great revolutions of the world" (18-19). Yet Schlözer not unreasonably wondered whether that was feasible, given not just the breadth of necessary coverage but also massive

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<sup>2</sup> His efforts spanned three decades from Gatterer 1761 to Gatterer 1792. Gierl 2012 discusses his projects.

imbalances in documentation (39). A quarter of a century later, both the goals – to understand the drivers of change (or “revolutions”) over time and variation in outcomes – and the challenges – how to manipulate the data – are thoroughly familiar to those invested in the study of macrosocial evolution and the logics of world history.

Back in the day, none of this work had any prospect of delivering an adequate understanding of causal factors and big regularities – that is, of universalizing historical inquiry. But that was not the main reason why nothing came of these ambitious strivings. That was also the time when academic professionalization spawned specialization and disciplinary fragmentation. Proper historians were supposed to be experts, with all the progressive narrowing of horizons that entailed. World history was increasingly left to philosophers (such as Wilhelm Hegel), popularizers (such as H. G. Wells) and assorted outsiders (most notably Karl Marx and Oswald Spengler). Credentialed historians who tried their hand at this – such as Arnold Toynbee – were rarities. For all their ambitious efforts to identify and track supposed regularities across space and time, and despite the fact that they reached and influenced vastly more people than any professional experts, the latter carried the day.

Anthropology went down the same path as the initial quest for all-encompassing “laws” and taxonomies (Tylor 1871, Morgan 1877) eventually went out of fashion. Some of this was for the best, having been tied up with “scientific” racism that was taking off just as Enlightenment history failed. But as so often the babies went out with the bathwater. By the mid-twentieth century, the globalizing impulse had withered. Databases such as the ones Turchin references (Turchin 2025: 323, omitting the sprawling Human Relations Area Files) linger as legacies of a bygone age, largely left behind by the profession – Anthropology – that birthed them even as they continue to be put to use by social scientists across the board. Among historians, academic global history recovered slowly and cautiously, as scholars such as William McNeill filled a gaping void.<sup>3</sup>

Judging by the usual trappings of having arrived, global or world history are now well established: there are courses, textbooks and surveys, dedicated journals and professional associations. For the most part, however, work under these headings takes the form of case studies that are more or less broadly contextualized (discussed by Crossley 2008, Hunt 2014, Olstein 2015, Conrad 2016). Truly planetary sweeps are mostly reserved for didactic materials, often written by committee and with predictable consequences. “Big History,” which seeks to elucidate dynamics that occur at vastly different scales of space and time, from the cosmic to the human, remains very much a fringe pursuit (Christian 2004,

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<sup>3</sup> McNeill 1963 and subsequent work, including with his son John: McNeill & McNeill 2003.

Brown 2007, Christian 2018, Hesketh 2023). As for contemporary Anthropology's (lack of) interest in those kinds of issues, the less said the better.

The appeal of methods that are most suitable for large-scale analysis has faded within the historical profession. Quantitative work that used to be more popular among historians a couple of generations ago has long since been eclipsed by various cultural and 'post-' turns, allowing practitioners of more quant-friendly social sciences to capture a growing amount of history's terrain (Klein 2018). And leaving numbers aside, it is emblematic of the spirit of the field of History that there does not seem to be a single introduction to comparative history (at least not in English): Lange 2013, the most detailed discussion of comparative historical methods, is not really designed for historians.

## **Modes of disengagement**

All this has produced a striking paradox. At a time of unprecedented globalization in the real world (and to some extent even within academia, notwithstanding enduring national idiosyncrasies), of unprecedented data accumulation (above all thanks to archaeology), and of unprecedented means of data analysis (thanks to advances in information technology), History as an academic field is now singularly ill-equipped to draw on all these to explore the dynamics of history on a global scale – or rather would be singularly ill-equipped to do so if it wished to, which in practice is rarely the case.

This paradox is compounded by ignorance and indifference. One would not know from consuming recent historical scholarship that work along the lines of GHT has been done or might even be doable. Similarly, one could not tell from the work of Peter Turchin and his associates just how detached their endeavors are from what historians usually consider their remit. By all accounts, this is not a matter of great concern to either side: certainly not to historians, who seem perfectly content to soldier on even as they like to fret over falling enrollments and loss of public standing, and arguably not all that much either to cliodynamicists, cultural macroevolutionists and complexity scientists, judging by their lack of outreach to the historians' community and their focus on a combination of technical papers in science journals (scaled up to book form in GHT) and popular books for a lay audience (both now conveniently corralled into the bibliography of Turchin 2025).

It is easy to illustrate the degree of historians' (and anthropologists') lack of engagement with this kind of work, even as a more comprehensive survey is beyond the scope of this intervention. At the time of writing, the sources of the 20 most recent citations in Google Scholar to Turchin 2003 include one paper on Byzantine history while all the others consist of a mix of articles and preprints from social science, evolutionary theory and the natural sciences. The sources of the most recent 20 citations of Turchin & Nefedov 2009 include no conventional

historical scholarship, only the maverick Journal of Big History and a mix of economic, political science and natural sciences papers. Much the same is true of Turchin 2006 (one ancient history conference contribution, otherwise more of the same), Turchin 2016a (a lot from economics and cognate areas), Turchin 2016b (one archaeology paper, otherwise the usual mix), and Turchin 2023.

Articles do a little better but not by much. The sources of the 20 most recent citations of Turchin et al. 2013 include a history dissertation on historiography and an archaeology paper, otherwise the same spread as before. Turchin et al. 2018 caught the attention of three archaeology papers, the aforementioned dissertation, and otherwise of economics, political science and complexity science papers. Two of the 10 most recent citations of Whitehouse et al. 2022 originate from history or area studies papers. Finally, Turchin 2009, which is quite conventional in style, features in a lot of social science papers concerned with historical questions.

What about publications expressly designed to build bridges? When I was one of the editors of a well-established ancient history journal, *Historia*, I invited Daniel Hoyer and Joseph Manning to contribute a survey that gave ample space to Turchin's work (Hoyer & Manning 2018). Of the 16 citations it has garnered since its publication, one each are by Hoyer and Manning, three are by Turchin, one occurs in a Whitehouse et al. paper, and another three appear in publications in this journal (*Cliodynamics*) (including a preceding working paper) – a closed circle barely penetrated by a smattering of “outsiders.”

All this is most unfortunate. This mutual lack of communication deprives historians of vital context that could help frame and inform their own, usually smaller-scale, more granular and more particularistic work, as well as of potential ammunition in their struggle for attention that might translate to enrolments, jobs and clout. But it also robs the cliodynamicists of their chance to influence those who study and teach history for a living.

Turchin himself acknowledges a relevant divide on the opening page of the preface (Turchin 2025: XIII) when he cites Randall Collins for the distinction between low-consensus fields – that are happy to chew over ideas from long ago – and rapid-discovery, high-consensus fields. He is optimistic that Cultural Macroevolution, as a branch of “historical social science” – and thus, one might add, at least in terms of subject matter a cognate of historical study more generally – might develop into a rapid-discovery science. “Slaying bad theories” (Turchin 2025: XV) is deemed central to this endeavor, which is something from which historians of all stripes stand to benefit, whether or not they are actively invested in any of those theories or merely keep them at the back of their minds as they pursue their own projects. After all, genuinely theory-free work does not really exist, at least not beyond the most sterile antiquarianism.

Apart from that, Turchin casually alludes to a “more extreme position, broadly shared by humanities scholars using less quantitative approaches, (...) that

different societies have unique histories and cannot be meaningfully compared in this way” (i.e., through complexity metrics) (Turchin 2025: 4). This observation bears emphasizing because it lies at the heart of the lack of engagement with GHT-style scholarship.<sup>4</sup>

Turchin defends the “assumption that, although every society is unique in its own ways, this doesn’t preclude the possibility that common features can be shared by multiple societies” (Turchin 2025: 5). The likely response of many historians or anthropologists would be, even if that were true, I don’t care because it’s not germane to what I want to study and how I want to study it. Which would be fair enough, were it not for the fact that “that’s not what I (as an individual) want to study” all too often morphs into “that’s not what we (as a profession) do (these days).” I have lost count of how many times I have encountered baffled interjections along the lines of “but we don’t do this” or “that’s not how we do things anymore” even in response to far less ambitious generalizations than those proffered in GHT. Personal preference shapes group norms and vice versa, through processes of self-selection into academic fields based on their perceived preferences in method and outlook, reinforced as needed by disciplining during the training and credentialing process and then through hiring and promotion.<sup>5</sup>

At this stage of divergent cultural evolution – perhaps not an entirely frivolous term in this context – between humanistic historians and anthropologists on the one hand and quantitative complexity scientists on the other, it is a challenge to imagine the reaction of the former if they were to be confronted with key findings of GHT such as the observation in chapter 7 that it is now possible to document “a low-dimensional manifold within the multidimensional phase space of social complexity components, where most past societies would be located” that allows 79 percent of variety to be captured by the first principal component in a survey covering 536 polities across space and time (Turchin 2025: 153-5). The same would be true if they became aware of the success of James Bennett’s simulation in replicating patterns in state formation across all of Afroeurasia from 1500 BCE to 1500 CE with considerable fidelity based on an extremely parsimonious selection of parameters that does not seek to account for cultural specifics (beyond the agrarian/pastoralist divide), let alone for input from Great Men (or Women) (Bennett 2022).

According to current humanities orthodoxy none of this should be possible. Yet as my brief sampling of citations underscores, humanistic historians and

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<sup>4</sup> Graeber & Wengrow 2021 – very reasonably omitted entirely from Turchin 2025 – represents an extreme version of this position: see Scheidel 2022: 22 for their refusal not just to do or engage with that kind of work but even to acknowledge its existence.

<sup>5</sup> Turchin 2025: 293 n.11 – a PhD student in Mesoamerican archaeology being dissuaded by advisors from conducting a study informed by Turchin’s theories – provides a depressing if unsurprising illustration of the role of reinforcement.

anthropologists may safely carry on unperturbed because the odds of such encounters are small, indeed minute. Instruction of the next generation – whether at the professional or general education levels – is therefore bound to remain untouched by such heterodox inputs. As a result, history seems condemned to forever remain “one damned thing after the other” – a big mess only haphazardly leavened by consideration of proximate causes and opportunistic reference to pet theories and premises.

Such deficits almost dwarf other undesirable consequences of this failure to engage. One of them is that it robs historians’ work not only of overarching framing but also of an incentive to focus on issues that call for application of their traditional skills: as Turchin notes, the GHT project misses out on proximate factors that operate on shorter timescales but nevertheless contributed meaningfully to overall outcomes (Turchin 2025: 166-8). It also deflects attention from historically prominent cases that are difficult to explain within the GHT framework, for example the Inca expansion (Turchin 2025: 296): reference to llamas does little to account for the emergence of an extraordinarily extensive and diverse mega-empire (a problem already clear from but papered over in Turchin et al. 2022: 8). This is precisely where a functional interface between conventional history and complexity science is badly needed but is sadly absent.

## **Odds of revival**

Yet returning to what I said at the beginning, the current state of affairs is unfortunate not least because it denies us the opportunity to revive and rebuild a version of universal history that is appropriate to our age and capabilities. No longer limited to tedious digests or wild conjectures, it could for the first time ever be grounded in curated datasets (such as those provided by Seshat) and rigorous methods (such as computational analysis of coded data or the application of agent-based modeling) that would lift it out of the imbalanced, banal and conjectural to a higher level of genuinely species-scale, explanatory and testable investigation that inevitably remained well outside the reach of previous generations.

That such a new universal history would look nothing like previous versions should not be a cause for concern but for hope. It would be able to include vast amounts of historical information without getting bogged down in endless presentation. It would do so by coding that information, minimizing expansive verbal exposition. It would by computational means establish multidimensional connections that were unfeasible to trace before (verbally), and also weigh their relative impact. It would fashion all this into a coherent whole that nevertheless gives due weight to variation and outliers, and thereby privileges causal explanation of processes and outcomes.

This new format holds out the promise of a universal history that is “universal” because it covers all of humanity, apprehends regularities and variation in a

systematic manner, and boldly transcends the regional or cultural units that tend to structure historiographical discourse. Far from replacing other, deeply entrenched forms of engagement, it would complement them by offering a superordinate framework to inform and inspire them. GHT is concerned with multi-level theory: a new universal history would help create a multi-level model of historical research and understanding that moves back and forth between different modes of inquiry that tend to be associated with different scales.

The viability of a multi-level approach depends on our ability to bridge the existing divergence between academic cultures. This ability cannot be taken for granted: in fact, it accounts for the question mark in the title of this paper. What may now in theory be within our reach may not be feasible in practice. Where there was once a will (to “universalize”), there were not the right resources. We are now entering a phase where the inverse is becoming ever more true.

It remains to be seen whether a more severe crisis of the academic humanities – in terms of student interest, funding and public support – will help precipitate experimentation and openness: after all, the idea of “crisis” is baked into the very concept of the academic humanities (Reitter & Wellmon 2021). This suggests that something quite out of the ordinary might be required to upset long-enduring equilibria. Indeed, crisis might just as likely encourage more doubling-down on the status quo. Well-endowed institutions that account for a disproportionate share of research and training the next generation of scholars are in any case well placed to weather storms. This in turn casts doubt on the idea that shifts away from what I like to call the “heroic individualism” (Scheidel 2025: 190-2) that governs the humanities and toward the skills-pooling collaborative work that is a hallmark of the sciences will gain traction in academic training and assessment.

In the very long run, consilient convergence upon a particular way of studying why the world has turned out the way it has may well prove unstoppable. Then again, the long run has an unfortunate tendency to outrun human lifespans. A potential game changer has burst onto the scene: we may soon find out to what extent AI can support scaled-up projects and lower barriers to engagement for the less quantitatively inclined or trained. Application of this technology may turn out to be our best chance yet to increase the reach, accessibility and profile of GHT-style work enough to make it harder or even impossible to ignore. It is only then that a new universal history has a real chance of developing on foundations that GHT – and the wide range of collaborative work it draws on – has managed to broaden and strengthen

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