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The Uniqueness of the West Reinforced A Reply to Beckwith, Goldstone, and Turchin

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This paper defends the ‘Kurgan hypothesis,’ the uniqueness of the epic heroic poetry of Indo-Europeans, and the uniqueness of Western civilization generally. The term ‘uniqueness’ is defined and associated with cultural creativity rather than with global economic and military dominance only.

I am pleased for this opportunity to clarify my arguments on the aristocratic culture of the Indo-Europeans, and the uniqueness of Western civilization generally. Three worthwhile criticisms have been offered in response to my article *Indo-Europeans Were the Most Historically Significant Nomads of the Steppes*. Since the three criticisms are very different from each other, I will reply to each author separately.

Beckwith

Beckwith first questions my claim that the Indo-Europeans (IEs) initiated the riding of horses and the invention of wheeled vehicles in the fourth millennium BC, arguing that “the earliest actual hard evidence for the existence of early IE speakers anywhere” consists of Assyrian documents from the 19th century BCE, including other “remains” dating to the same period from probable proto-Tokharian-speaking people. From this point, Beckwith asserts that the “old” gradual model of change over millennia, “according to which Proto-Indo-European (PIE) evolved very slowly into the attested IE branch-languages, has been resoundingly rejected in recent scholarship on language contact and change.”

This claim, worded as it is, goes against the entire existing scholarship on the IE question. Indo-Europeanists speak of the existence of a Proto-Indo-European language (PIE) reconstructed as the common ancestor of the Indo-European branch-languages. The actual competing hypotheses about when and where PIE was originally spoken propose dates ranging from the fourth to the seventh millennium BC. In my book, I defended the most widely accepted, judicious and empirically consistent hypothesis, which is known as the Kurgan hypothesis, which says that PIE was spoken in the fourth millennium in the

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Pontic steppes of what is today known as Ukraine and southwest Russia. It strikes me as extremely unreasonable, on Beckwith's part, to suggest that this hypothesis, with its gradual model of IE dispersal from this region, has been "resoundingly" refuted. David Anthony's 2007 book, *The Horse, the Wheel, and Language*, is widely recognized as the most exhaustive study to date on this question, combined with J.P. Mallory's *In Search of the Indo-Europeans* (1989), as well as *The Oxford Introduction to Proto-Indo-European and the Indo-European World* (2006), by Mallory and D.Q. Adams.

Of course, what Beckwith wishes to say (in a rather offhand way) is that there is no direct evidence of PIE *written* language. Everyone knows that all PIE words are reconstructed from later IE languages, but they have been so reconstructed using the well attested and respected techniques of historical linguistics (which include the [comparative method](#) and the method of [internal reconstruction](#)). As Martin West expressed it in *Indo-European Poetry and Myth* (2007), judged by reviewers as one of the definitive books on the IE language and religion: "The assumption of a single parent language as the historical source of all known IE languages...is still a hypothesis, not an observable fact, but it is an inescapable hypothesis."

To back his claim, Beckwith cites a paper he authored and his book, *Empires of the Silk Road*, together with two book chapters by Andrew Garrett (1999, 2006). Yet none of these writings supports his claim or challenge anything Anthony wrote or the Kurgan hypothesis generally. To the contrary, they are in line with the argument I expressed in the article and in my book! Garrett's chapter, "Convergence in the Formation of Indo-European Subgroups: Phylogeny and Chronology" (2006) instead questions Renfrew's Anatolian hypothesis that the first IE was spoken in 7000 BC "as in the first agriculturalist framework" (that is, the claim that the IE language was introduced into Europe by Neolithic farmers from Anatolia). It suggests that PIE was spoken and IE language diffusion began in the fourth millennium BC. He writes:

I take it that PIE was spoken c. 3500 BC, perhaps somewhat earlier, in part of what Mallory calls the 'circum-Pontic interaction sphere'.

PIE was spoken and IE language dispersal began in the 4th millennium (144–6).

In other words, he agrees with the arguments I endorsed. In fact, what Beckwith actually says in his own book is consistent with the Kurgan (or Pontic) view on the existence of a PIE common language existing in 4000BC:

[T]he view of the early Indo-Europeanists, who suggested a period around four millennia ago, is *supported by the available data*, including

typology, and also corresponds to the younger end of the dating ranges suggested by several proposals of Indo-Europeanist scholars (31).

This sentence is footnoted with a reference to Mallory and Adams (2006), which is supportive of the Kurgan hypothesis. Similar statements can be found in Beckwith's book. Therefore, one cannot but conclude that Beckwith is being disingenuous, since he distinctly accepts the scholarship on the reconstruction of a PIE mother tongue and the Pontic hypothesis.

It is very odd, moreover, that he later cites Garrett to argue that ancient Greek contains "a very high percentage of non-IE lexical forms," since Garrett also concludes that the Greek spoken during and after the Mycenaean period was "linguistically closer to IE than has been supposed" (147). Garrett cannot be so easily associated with a theory of creolization.¹

The argument has never been that the IE speakers who Indo-Europeanized Europe did not integrate non-IE language forms. The argument is about why, when, and how they superimposed their language as the *dominant* form, which is why they are today called IE languages. The field of "Indo-European Studies" has been driven by the effort to understand why this language family became the world's most widespread language, spoken today by almost three billion native speakers, the largest number by far for any known language family.

By the same token, Beckwith says that Anatolia remained as Indo-Europeanized as Europe (until the 20th century CE). The objective seems to be: marginalize the impact of Indo-Europeans in Europe, while exaggerating their impact in lands (Turkey) that are not even designated today as Indo-European. However, there is nothing in his reply, or books, which challenges my claim (in *Uniqueness*) that the IEs who invaded Anatolia were eventually assimilated to the non-IE cultures, languages, and ethnic groups of this region. When IE groups entered this region in high numbers during the second millennium this was already a complex, urbanized, and well-populated civilization, unlike the Neolithic world encountered by IEs in much of Europe. Obviously, additional scholarship on this matter is required rather than a quick exchange with Beckwith; suffice it to say that my claims are consistent with Trevor Bryce's authoritative account, [The Kingdom of the Hittites](#).

Beckwith further writes that "the very solid evidence for their [PIE] farming rules out nomadism." This is an incorrect dichotomy; no IE expert, as I made clear in the book, argues that PIE speakers were complete nomadic

¹ Neither publication cited by Beckwith from Garrett can be read as a challenge, never mind a 'resounding' refutation, of anything I said, or as supporting any of Beckwith's claims here, including Garrett (1999).

pastoralists; they were semi-nomadic, practicing some farming *but* dedicating themselves mostly to herding. Beckwith adds that “there is absolutely no historical evidence for the riding of horses, let alone from fighting from horseback, before the early first millennium BCE”. He says that Anthony’s bit-wear evidence is contested by scholars, and, in this vein, cites a book by Drews published in 2004. But this book could not be seen as a refutation of Anthony’s book which was published a few years later in 2007. Beckwith offers no sources against Anthony, who has offered considerable evidence showing that, before the onset of cavalry warfare in the first millennium, horses were used for “raiding” with minimal riding equipment by the middle of the fourth millennium. Pita Kelekna, in his recent book, *The Horse in Human History*, agrees with Anthony that some Botai horses were “bitted and likely ridden for hours” (2009, 38). In 2011, Anthony (Anthony and Dorcas 2011) offered additional arguments and evidence according to which:

Current evidence indicates that horses were domesticated in the steppes of Kazakhstan and Russia, certainly by 3500 BC and possibly by 4500 BC. Tribal raiding on horseback could be almost that old, but organized cavalry appeared only after 1000 BC. Riding might initially have been more important for increasing the productivity and efficiency of sheep and cattle pastoralism in the western Eurasian steppes. The earliest (so far) direct evidence for riding consists of pathologies on the teeth and jaw associated with biting, found at Botai and Kozhai 1. Recent developments and debates in the study of bit-related pathologies are reviewed and the reliability of bit wear as a diagnostic indicator of riding and driving is defended.

Beckwith says that the PIEs “did not invent the wheel or the wagon,” but “borrowed them from the Near East,” although he admits that PIE does have “a solidly reconstructed word for ‘wheel; vehicle.’” Apparently, Beckwith does believe in the existence of a PIE language *reconstructed* from IE branches. I argued in *Uniqueness* that they were ‘co-inventors’ of wheeled vehicles. The historical linguist, Asya Pereltsvaig, has maintained (2012) that reconstructions of PIE include the word ‘wheel,’ with solid evidence [favoring Anthony’s argument](#) that wheels and wagons became widespread between 3400 and 3000 BCE, and that the vocabulary for wagons and wheels was not imported from outside the PIE speech community.

Beckwith draws attention to the “equally wonderful epic literature of the ancient, medieval, and modern Central Eurasians.” My book—and I don’t expect Beckwith to have a read it on a short notice to comment on my article—does neglect this epic literature from Central Eurasia; however there is no reason to assume robotically, without reflection and comparative analysis, that

these two epic traditions were “equally wonderful.” Having read two substantial articles, “[Mongolian-Turkic Epics: Typological Formation and Development](#)” (2001), and “[Mongolian Oral Epic Poetry: An Overview](#)” (1997), I am confident in making the following distinctions, which I can only address in point form due to lack of space:

First, the IE epic and heroic tradition precedes any other tradition by some thousands of years, not just the Homeric and the Sanskrit epics but, as we now know with some certainty from such major books as West’s *Indo-European Poetry and Myth*, and Watkins’s *How to Kill a Dragon: Aspects of IE Poetics* (1995), going back to a prehistoric oral tradition. Second, IE poetry exhibits a keener grasp and rendition of the fundamentally tragic character of life, an aristocratic confidence in the face of destiny, the inevitability of human hardship and hubris, without bitterness, but with a deep joy (Gunther 2001; Gurevich 1995, 19-61).

Third, IE poetry contains a richer repertoire of motifs and narrative stories, and a higher aesthetic level of achievement. The most basic theme of Mongolian and Turkic poetry is the search for a wife and children and fight with a demon, battle over horses, slaves, ransacking property, and clan feuds. Heroic deeds consist of overcoming natural obstacles and the evil designs of competitors en route to winning a future wife as well as fighting demons and other heroes. Similar themes can be found in IE poetry, but many of these tales are richer in motifs, in the performance of greater, more adventurous and worldly deeds. The Vinland Sagas, for example, chronicle the adventures of Eirik the Red and his son, Leif Eirikson, who explored North America 500 years before Columbus, providing the first-ever descriptions of North America, recounting the Icelandic settlement of Shetland, Orkney, the Hebrides, parts of Scotland and Ireland, the Faroe Islands, Iceland, Greenland and Newfoundland. The *Iliad*, like the *Odyssey*, widely acknowledged through the centuries as two of the greatest works of literature, is a world of powerful kings living in vast, wealthy palaces, and in charge of huge armies; they are superb stories far richer in character, with heroes exhibiting complex inner contradictions, regrets, and self-criticism (Gill 1996).

Fourth, IE epics show both collective and individual inspiration, unlike non-IE epics which show characters functioning only as collective representations of their communities. Moreover, and this is a very important contrast, further illuminating IE individualism, in some IE sagas there is a clear author’s stance, unlike the anonymous non-IE sages. The individuality, the rights of authorship, the poet’s awareness of himself as creator, is acknowledged in many ancient and medieval sagas (Gurevich, 61-75). Fifth, Beckwith says the Central Asian epic tradition continued to the 20th century while the Greek tradition ceased. Sure, it remained relatively stagnant in Central Asia while Homer’s writings set the basis for Pindar, Aeschylus,

Sophocles, Euripides, Aristophanes, and their invention of nearly all the literary patterns we use today: tragedy and comedy, epic and romance, and many more, which the Romans eagerly assimilated, adding Virgil's *The Aeneid*, the satires of Horace and Juvenal, Ovid's *Metamorphoses*, not to mention the heroic epic of the Middle Ages all the way to Richard Wagner who is seen as the artist principally responsible for keeping the European mythological tradition alive in the modern world.

Beckwith says that Murray's statistical assessment of human accomplishments across cultures could not but reflect a Western bias; and he asks: "did Murray read though the many massive literatures of non-European languages". Well, if Beckwith had consulted Murray's book, and as I elaborated in my book, he would have learned that Murray definitely used sources from a variety of countries (as much as these countries have in fact produced encyclopedias and reference works). Murray also constructed separate inventories for non-Europeans in the Arts so as to avoid applying one standard of artistic excellence. Beckwith questions my claim that Turkic warriors were transformed into mamluks lacking nobility, but instead of engaging with the source I cited in support of my argument, he repeats that the *comitatus* was aristocratic, which is not under dispute.

Finally, he hastily asserts that "Europeans borrowed science as a completely developed tradition from Classical Arabic civilization during the Crusades, citing his latest book, *Warriors of the Cloisters* (2012). But as Toby Huff, well-known authority on the comparative history of European and Islamic science, argues in [his review in AHR](#) (2013), Beckwith's book is full of "dubious and highly misleading claims". He confuses, for example, madrasas with the original European creation of universities, fails to realize that madrasas were lacking faculties, a formal curriculum, and offered no 'degrees' and no collective evaluation (examination of students).

Goldstone

For Goldstone the uniqueness of the West is all about the 'rise' of this civilization to economic, "military, and colonial domination." But as Hewson clearly explained in his review, I argue that multiple divergences, successive revolutions, and continuous creativity are the basic peculiarities of the West. The statement that, before the revisionists came along, "it was really all black and white or West and non-West" is a false opposition which cannot be attributed to the Eurocentric literature at large. When one examines the scholarship on the civilizations of the world, produced primarily by Western-educated scholars, one meets numerous sources exhibiting balance, respect, and commitment to the non-West. I relied on many of these works in *Uniqueness*. It is the revisionists who have ignored huge components on the

scholarship and histories of the West. As I argued in my book: (i) revisionists have invariably relied on outdated sources, (ii) reduced the debate to mostly comparative economic history, (iii) misread, and sometimes willfully misinterpreted numerous classic authors including Weber and contemporary authors. The claim that Weber contrasted a rational West to an irrational East is a standard assertion by revisionists without scholarly merit. In fact, revisionists have had to rely on histories written by standard Eurocentric scholars on Asia, Africa, and the Americas in order to formulate their own ideologically oriented views. By ideological, I mean the feeling that history must be written in conformity with the multicultural principle that all cultures are equal or similar in value and achievement.

Goldstone's reply is replete with vague generalizations. I can only bring up a few examples. He says that Asian societies had a "clear lead...in exploration, production, manufacturing, seafaring and navigation, experimental science, pluralism and toleration, lasting well into the 17th and in some respects the 18th century". This is totally inconsistent with masses of books, articles and quantifiable data. How can one say this when it was the Europeans who had rounded Africa, set up trading ports throughout the Indian Ocean, [mapped every region of the earth](#) (Buisseret 2003), carried successive [shipping and navigational improvements](#) from the mid-1400s onwards (Unger 1981), promoted a [printing revolution](#) (Eisenstein 1980) and then a book/journal/newspaper culture, developed an experimental science with cumulative insights, established all the disciplines in science and social science we teach today, set up capitalist firms around the world, colonized the Americas and Africa, instituted the supremacy of Parliament in England and representative institutions in Europe, introduced religious tolerance, produced one continuous great philosopher after another, invented [the novel](#) (Watt 2001), every single [classical composer](#) in modern times (Schomberg 2006), a restless sequence of artistic styles, analytical geometry and [algebraic logic](#) (Gabbay and Woods)? The Asians were leaders essentially in the production of large populations and labor intensive farming techniques.

Here's another confounding statement: "[T]he flood of new empirical knowledge after the discovery of the New World and the inventions of telescopes, microscopes, vacuum chambers and other scientific instruments *forced* Europeans to confront inadequacies in their classical inherited philosophy. Other societies *absorb* these discoveries without feeling those inadequacies for they were more cosmopolitan and syncretic." Nothing could be further from the truth. How exactly were Europeans *forced* to confront their inadequacies if they were the inventors of telescopes, microscopes, vacuum chambers and numerous scientific instruments? The research says the opposite of what Goldstone wishes had happened: Huff's book, *Intellectual Curiosity and the Scientific Revolution* shows that European efforts to

encourage interest in telescopes and microscopes in China, the Ottoman Empire, and Mughal India “did not bear much fruit.” “The telescope that set Europeans on fire with enthusiasm and curiosity, failed to ignite the same spark elsewhere. That led to a great divergence that was to last all the way to the end of the twentieth century” (2011, 5).

Uniqueness does not offer a “single factor explanation,” as should be clear from its reading; tracing the first cultural component (the aristocratic ethos of Indo-Europeans) in Europe’s multiple divergences does not mean that this is the only factor; it is the first one, and it undergoes considerable changes, together with the emergence of other factors and cultural novelties that are examined each in their own right. Goldstone cites some lines from Quataert (2005) to the effect that Turkish steppe nomads of the 14th century were as aristocratic and heroic as the Indo-Europeans, but when one investigates Quataert’s book, it contains nothing else about this topic except these lines. Goldstone also brings up the epic of *Gilgamesh* as if it were a demonstration that there were aristocratic tales elsewhere. Therefrom he announces: “so much for the uniqueness of Indo-European nomad epic and social organization!”

My book contains a section entitled “the Epic of Gilgamesh is not a Tragedy.” As explained in my [reply to Elvin’s review](#), in this section I relied on 24 cited sources, examined arguments for and against, reaching the conclusion that a true heroic epic presupposes the existence of peers who can compete for heroic status. The king Agamemnon in the *Iliad*, for example, is surrounded by free, prideful aristocrats always deliberating and competing with their peers. Agamemnon is “first among equals”; Achilles even refuses to accept all of his commands, and everyone else in the epic enjoys the opportunity to perform glorious deeds. In all the epics and sagas of the Western world we meet individually named characters, rather than one singular ruler, as Gilgamesh, bragging endlessly and demanding adoration and servility even from the upper classes.

It is not clear why Goldstone brings the Song of Solomon into this question, but his point seems to be that the Hebrew Old Testament had a “rather more profound impact on later Western and European thought” than the aristocratic epic tradition. I disagree, and will call attention to my essay, “Christianity is a Hellenistic Religion, and Western Civilization is Christian” (2006), and add that the Hebrew Bible *on its own* is not Western, but develops into Judaism, wherein the Talmud becomes the central text (Solomon, 2000), at the same time that Christianity undergoes further theological and administrative/legal development in a Hellenic and Roman manner, as well as [Germanization](#) (Russell), which is not to say that the Hebrew Testament does not remain a central component.

Turchin

Turchin is the author of five books (1998, 2003a, 2003b, 2006, 2009), all of which have been highly praised by the academic community. His commentary on my paper is really an article at 7,500 words. Unlike Beckwith and Goldstone, Turchin brings up my book more than a few times, rather than the article.

About half of Turchin's commentary consists of a stimulating section arguing that China, not Europe, was unique geographically. I have no current objections to this argument, but I do wish to make some corrections and clarifications about his understanding of my book in this regard. He says that "Duchesne devotes a *substantial* amount of discussion in his book to the observations by Cunliffe in *Europe between the Oceans*" (Cunliffe's argument that Europe was well-connected by land and sea). Actually, I devote no more than one page (311-12) to this particular argument by Cunliffe. Moreover, I don't rely on Cunliffe alone but on Landes, Hegel, Harold and Dorn, Spengler and others. Turchin writes that "Duchesne uses this observation [Cunliffe's view about Europe's connectivity] to argue that the European topology and landscape were [he quotes me] 'a crucial geographic component in the formation of Europe's uniquely restless culture.'"

But this is a truncated quote which creates the false impression that I view Cunliffe's geographical argument as crucial to my understanding of Europe's uniqueness. What I wrote, after describing Cunliffe's views on topography and the Middle European Corridor, was: "I will argue in the next chapter that this corridor and its link to the steppes, *with its pastoral, horse-riding way of life*, was a crucial geographical component in the formation of Europe's uniquely restless culture" (312). I criticized Cunliffe for writing of Europe's "restless culture" in purely geographical terms and disconnecting the IE aristocratic warlike culture from this geography.

Turchin also implies that I accepted Cunliffe's argument without coming to terms with Jared Diamond's opposing view about Europe's geographical disunity. I understand that reading a 500+ page book (with almost 900 different cited sources) is quite demanding in the context of a debate with time and spatial boundaries, but I have to clarify that I devoted more space to Diamond (44-6, 216-17, 308); and also criticized the way he explains Europe's political disunity in terms of Europe's geographical division into isolated peninsulas. I criticized the commonly held idea (Eric Jones, Daniel Chirot, and David Christian) that Europe was different and more innovative because it was divided into states pressured to compete and innovate against each other.

Now, Turchin claims that "the first step to explaining this divergence is to realize that there is nothing particularly unique about European political disunity." What's really unique is China's unity. But here Turchin misses

another basic component in my argument, which goes beyond geography and the mere division of Europe into competing states. I don't explain Europe's political disunity merely in terms of its geography. I argue that Europe's political disunity involved more than lack of imperial unity. Europe was uniquely shaped internally by a division of powers, the presence of a unique civil society, which worked to counter the central powers of the nation state, with roots going back to the libertarian ethos and republican character of Europe's aristocratic culture, relative separation of religious and secular powers, autonomous cities and universities with corporate privileges based on a unique legal system with roots in Rome, the rise of representative institutions, and more (this argument comes up at various points in the book, including in pages 226-229; 269-284). I also bring up Greece's division into city-states, and connect the emergence of the polis to Greece's aristocratic foundation. I argue that the Greek (and Roman) republican character (both in its early, strictly aristocratic, and its citizen-oriented forms) cannot be understood without a prior appreciation of the culture of IE aristocratic values. The same is true of the institution of medieval feudalism (312-15; 390-418; 460-88). I don't examine Europe's geography in isolation and then argue for uniqueness.

Turchin's challenge against the uniqueness of the "Western way of war" is marred from the beginning by his supposition that Hanson's thesis amounts to the simplistic claim that the ancient Greeks invented a type of decisive infantry warfare that became the basis for Western superiority right into modern times. He relies essentially on one book by Hanson, *The Western Way of War: Infantry Battle in Classical Greece* (1989), which is focused on the invention of hoplite warfare, while ignoring Hanson's other publications, including *Carnage and Culture: Landmark Battles in the Rise of Western Power* (2001), which do not restrict the Western way to infantry battle. Turchin mentions the names of Parker and Keegan, and *The Cambridge History of Warfare* edited by Parker (2005), but, despite the fact that this book clearly shows that the Western way evolved and underwent organizational, technological, strategic and tactical changes, he still reduces this thesis to the claim that "the heavily armed infantrymen of Greece" was singularly, on its own, the path which led to Western dominance. Turchin concludes this section stating: "The recipe for 'western dominance of the globe' in the early-modern period has nothing to do with Greek warfare." But why suppose that such learned scholars as Keegan, Parker, and Hanson made such an obviously crude argument?

Likewise, in *Uniqueness* I devoted three sections on the Western way of war and military revolutions in Europe (209-221), showing that Greek hoplite warfare was only the *beginning* of the Western way. As it is, Turchin's summation of Hanson's hoplite style is incomplete, and leaves out three other traits related to culture, citizenship, and military theorization (see *Uniqueness*,

p. 219). Turchin says that “the Macedonians, who learned the Game of Empire very well, used a different style of warfare from that described by Hanson”. But I used two chapters by Hanson in *The Cambridge* on the Macedonians and Romans, and explained:

The Macedonians, Romans, and later Europeans revised and added new tactics, weapons, and strategies, but they did not depart from the fundamental principles of the ‘Western Way.’ Thus, the Macedonians, under Phillip II, added the ‘companion cavalry,’ an elite body of aristocratic horsemen, integrating horse and infantrymen, and they also lengthened the spear from eight to nearly fourteen feet. The Romans, through a long period of evolution lasting nearly a millennium, created a more fluid and open order of legionnaires, armed with throwing-javelins and short double-edged swords. They also raised discipline to new levels of professionalism, and backed their marching armies with a superb infrastructure consisting of roads, camps, hospitals, armour, pensions, and salaries.

The Macedonians and Romans, after all, were Western. Turchin also mentions English medieval archers, and the modern military changes as challenges to Hanson’s thesis, but, again, these changes were by Europeans and consistent with the Western way, which included more than face to face infantry battle. Moreover, I distinctly argued in *Uniqueness* that “the essentials of Hanson’s thesis can be effectively defended in the revised manner Parker has in his ‘Introduction’ and ‘Epilogue’” to *The Cambridge*. Parker lists five traits making up the Western way, which I described (220-21), adding that Parker had brought together his ideas on the “Military Revolution” with certain aspects of Hanson’s thesis. I also covered the debate over this military revolution (209-14).

Turchin adds that my emphasis on the libertarian, heroic and prestige-seeking lifestyle of barbarian aristocratic warriors has a “curious dissonance” with the habit of discipline and obedience expected from soldiers. He mentions the unflattering views the disciplined Romans had of the barbarian Gauls. I addressed this on various occasions, starting with the observation that the early Greeks and early Romans were themselves barbarians who fought in the berserker style, and that once they developed their disciplined armies they “contrasted their self-restraint and reasonableness (as well as their courage in staying in rank in the face of the enemy without giving way to fear and panic” (370). But one of the contributions I have made is that all Indo-Europeans, as barbarians, followed a uniquely berserker style, and that this ethnic and cultural group invented, as IEs developed city-states and civilizations, the Western way of warfare. I added that “Greek hoplites, Macedonian phalanxes,

and Roman legionnaires did not eradicate the state of mind of the berserker as much as sublimate its excessive, disorganized and 'barbaric' impulses into a far more effective, disciplined style of warfare that would make Westerners 'the most deadly soldiers in the history of civilization'" (370). There is a section, "Arête and the Education of the Greeks" (445-456), where I explained how early Greek barbarians transformed themselves into civilized but still agonistic members of city-states, and how this competitive and individualistic spirit found expression in the high culture of the Greeks, their invention of Olympics, poetic contests, a dialogical style of reasoning, and creativity.

In the opening pages, Turchin maintains that my position on the uniqueness of Western civilization is not "neutral" but "ideological" in that I show a preference for Western civilization. Turchin believes that all cultures are unique and that my argument is biased in assigning uniqueness only to the West. This is a misunderstanding; clearly my argument is not that other cultures lacked uniqueness (which is almost self-evident since they are not the same in customs, language, beliefs and history); it is that the West was unique in exhibiting the greatest cultural creativity and dynamism. This is not an ideological position *per se*—even if I show admiration for this creativity—as long as I employ the basic protocols of academic scholarship, proper use of sources, careful reading and understanding of opposition views, proper calibration of the evidence and the available sources, and a willingness to engage with falsifying or counter-arguments in a serious, fair-minded way. Over two hundred pages of *Uniqueness* are dedicated to the revisionist counter-arguments.

Turchin objects to the passing of 'good' and 'bad' judgments on the achievements of cultures, but then admits that even scientists like him who wish to construct mathematical models of history to anticipate the future are motivated by judgments about what is good and worthy of admiration. Why would someone want to predict future crises if not to anticipate them, avoid them, and promote a *better* future? So, in the end, his point is that we should avoid allowing our cultural preferences to interfere with our arguments and evidence. And he implies that the goal of my book is not to seek the truth but "to pass a value judgment on the achievement, creativity, significance, vigor, excellence, etc. of the Western Civilization." Turchin marshals the following points in support of this judgment. He says "the main thrust of *The Uniqueness of Western Civilization* is to argue against [citing my words] 'the devaluation of Western culture that swept the academic world starting around the 1960s,'" No, this is the main goal of chapter one; Turchin does not refer to the contents of this chapter, which actually trace the ideological origins of the revisionist/multicultural approach to history. He detects "two serious problems" with my work; one is that my ideological agenda "leads to predictable results", by which he means that I ignore the achievements of non-

Europeans, giving as an example “a dismissive sentence” I wrote on the explorations of Polynesians. (Here he endorses Beckwith’s critique, which he read before writing his comment; he did not have my reply to Beckwith at hand, so readers will have to judge.) First, for clarification, what I wrote about the Polynesians is contained in an article I wrote after *Uniqueness*, “A Civilization of Explorers,” an argument barely developed in the book. I wrote:

The Polynesians navigated across millions of square miles the Pacific, but as gifted as they were in practical and experiential matters, they did not cultivate a body of geographical knowledge. The Phoenicians left no geographical documents of their colonizing activities.

These are two sentences. Turchin’s own description of the Polynesian voyages is consistent with my first sentence except that he uses more expressive language; however, he does not question my view that they left no geographical documents. This is a fact rather than a subjective judgment. To this day there are no major studies on these anonymous explorations, and it is interesting that Turchin has to rely on one paper on the spread of the sweet potato to hypothesize these explorations; a paper written by Western scholars. Turchin, by describing the Polynesian explorations as “remarkable,” as “fearless seafarers,” as “truly heroic feats,” is making a value-judgment. But that does not mean that he is wrong on this point *per se*. Either way, how can an article showing that about 95 percent of all explorations (combined with the development of geography as a science, superior navigational techniques, and a cartographic revolution) be deemed as more “ideological” than an article saying that all cultures were equally exploratory, or simply because my article did not pay enough attention to the Polynesians?

Turchin’s second major methodological objection seems to be that the very endeavor to account for something peculiar in a culture carries inherent ideological pitfalls, in that “any world region, or any human society has a multitude of peculiarities that distinguish it from other regions, or societies,” and so, the researcher, in trying to explain these peculiarities, will tend to emphasize those which suit his ideological inclinations, cherry-picking facts that confirm the peculiarity he prefers. This is true, but it is no less true of those inclined to see common, regular patterns across cultures. They too will be tempted to cherry-pick those facts that suit the common patterns and similarities they wish to highlight. Every research project carries pitfalls, particularly when we are dealing with human and cultural phenomena.

I think the main reason Turchin is sceptical of the idea of uniqueness is that it runs counters to his own inclinations to construct general laws of history. Turchin’s field of expertise includes population dynamics from a comparative historical perspective (emphasizing as well social structure and state strength)

and its effects on political stability. He believes that using such tools as nonlinear mathematics and computer simulations (which can analyze vast amounts of data and model the interactions of millions of individuals) will allow historians to construct general laws of history to predict future outcomes. His historical books have been about understanding common patterns and outcomes of instability across Europe and Asia. The idea of uniqueness is anathema to this endeavor, since one of the underlying premises of cliodynamics is that patterns of human behaviour hold across cultures and can be predicted based on an analysis of repeating population, economic, social, and political trends. But history is filled with political upheavals, rebellions and revolutions that do not fit this emphasis on population dynamics and social and state structures. Certainly the continuous creativity of the West cannot be so explained. Moreover, my argument on the aristocratic/individualist psychology of Europeans precludes a uniform conception of human nature and the notion that all humans irrespective of culture and ethnicity perceive, reason, feel in the same way. As the scientific research of Richard E. Nisbett has shown, "Asians and Westerners think differently" (2003). However, for all these differences, I don't believe we should mistrust the scholarly intentions of cliodynamics by calling it ideological.

Perhaps we can agree that we need a clearer awareness of what it means to say 'common patterns' and 'unique traits.' But I do believe we have underestimated the uniqueness of the West, partly because this debate has been preoccupied almost entirely with the rise of the West to economic and military hegemony in modern times. The cultural uniqueness of the West has been narrowly evaluated in the degree to which it encouraged or discouraged economic and scientific innovation. Few have addressed the West's creativity in its own right; its original sources and its identifying cultural character. My book has received a high number of heartening reviews, including eight review essays thus far; unfortunately, no revisionist had paid any attention to it, notwithstanding the very detailed, critical effort this book committed to the revisionist arguments. If I may close with the following words from David Northup's review (2012) in the *Journal of World History*:

Although *The Uniqueness of Western Civilization* may well upset or infuriate world historians, they have much to gain from reading it, since it presents summaries and critiques of a great many works in comparative world, European, and Asian history. In all he cites nearly nine hundred works on subjects ranging across the full chronological spectrum of history and including major works in sociology, archaeology, philosophy, and history. Following Duchesne's example,

serious historians cannot afford to read only authors belonging to their own favored school.

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