

The Silk Road

A Review Essay on *Empires of the Silk Road: A History of Central Eurasia from the Bronze Age to the Present*. by Christopher I. Beckwith (Princeton University Press, 2009).

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This is a complex, well-argued book, one that every scholar interested in Central Eurasia or in long-term change should read. It is sufficiently encyclopedic to tax any reader's knowledge base. It will be very pleasing in some areas, provocative in others, and informative in still others. Along the way Beckwith drops enough good ideas for research topics to keep a veritable army of doctoral students occupied for decades. One on-line review said, "For iconoclasts who love seeing assumptions torn down, there is plenty of fodder. Lastly, Conan the Barbarian fans may enjoy this sympathetic look at people commonly despised as barbarians" (Gill 2009). (Full disclosure: I am one those fans!).

I begin my review with some general comments about Beckwith's goals then move to chapter summaries. I conclude with an exploration some of the many theoretical implications of Beckwith's arguments. *Empires of the Silk Road* has several interesting organizational features. Two appendices explore the spread of Proto-Indo-Europeans and Central Eurasian Ethnonyms. Each substantive chapter opens with a brief summary of the chapter arguments. These are quite helpful in that the subsequent arguments are at times quite complex. Along with the usual scholarly footnotes, there is an extended set of Endnotes that elaborate on topics discussed in the text. These go beyond the general argument of the chapters, and provide interesting additional deep background.

Beckwith's goal is to elucidate many neglected issues in Central Eurasian history, most notably to argue that "the trade in luxury goods (along the Silk Road) constituted a very significant part of the internal economy within Central Asia" (p. xii). Furthermore this trade along with parallel maritime trade "were all integral parts of what must be considered to have been a single international trade system" (p. xx). The entire social, political, cultural, and economic system not only shaped the history of Central Eurasian states and various other peoples, but also the trajectories of the "peripheral states" that

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abutted Central Asia. It is worth noting that Victor Lieberman calls these peripheral states the “exposed zone” and distinguishes them from “protected rimlands,” that is states at one or more removes from the Steppe, e.g. states in Southeast Asia (Lieberman 2008). All of these have been affected by Central Eurasian history according to Beckwith. He rejects stereotypes of pastoral nomads as warlike, difficult to defeat, poor, and entirely distinctive peoples. He also seeks to refute the accusations of barbarism and the label barbarian, most forcefully in an Epilogue.

In pursuing his goals Beckwith strives to present an “objective history” and eschews all macro theories. He also argues that repetition does not mean that history repeats itself. Rather, that “humans do tend to do the same things” (p. xi). While I applaud an effort to achieve “objective history,” I argue that that goal is impossible. Rather, one can be clear about one’s presuppositions and assumptions, but cannot do without them altogether. I will return to this issue at the end of this essay, focusing primarily on how what he says feeds into world-systems analysis. Beckwith also notes that most of his writing was completed in 2007, so that material that appeared later is not addressed. Finally, his trenchant criticisms of older interpretations are occasionally directed at his own earlier work. His critique of postmodernism is persuasively damning in the preface and in the final chapters.

The Prologue lays out Beckwith’s central approaches. Based on a review of many hero stories, he builds the argument that a cornerstone of the Central Eurasian Culture Complex (CECC) is the *comitatus*. A chief or khan selects men to form “a war band of his friends sworn to defend him to the death” (p. 12). An oath of loyalty trumps all other commitments to clan or nation. Later adoption of world religions undermines the *comitatus* since virtually all of them disapprove of suicide. In return the leader treats the *comitatus* as his family and rewards them for their service. A second major argument that trade was the basis of the Silk Road economy, not raiding which was used only when avenues of trade were closed. Horses were a major item of trade to China. Finally, following Nicolo Di Cosmo’s (2002) he argues that the walls around China were offensive, not defensive. They were built to hold pastureland conquered from pastoral nomads, and to prevent peasants from fleeing from China.

The first substantive chapter explores the chariot wars. He notes that the chariot most likely developed in the mixed steppe-forest zone between southern Urals, northern Caucasus, and the Black Sea. Had he been able to consult David Anthony’s *The Horse, the Wheel, and Language* (2007) he would have been able to strengthen this argument. He does note that Anthony’s book appeared too late for him to make use of it. This was unfortunate not only because much of its evidence would have strengthened his arguments, but also because his comments on it would have been quite interesting.

He also suggests that the idea of writing and bronze may have come to China from the north and west. But these would not have been simple diffusions. When West Asians moved east it was often male warriors who took local brides and shifted, eventually, to local languages. The need for chariots and charioteers to defend against other charioteers made the hiring or enticing of charioteers quite common. Tracing a good deal of linguistic evidence he argues that many of the language divergences occurred quite early, before mounted pastoralism developed. Here, again, Anthony's work supports this argument. For this and all the following substantive chapters, Beckwith's arguments are much more nuanced and better documented than I can discuss in these brief summaries.

The next chapter is devoted to the rise of the first steppe empire, the Scythians in the middle of the first millennium B.C.E. He notes that the Scythians developed trade to support their social structure, and as they rose, so did all the classical cultures: Rome, Indic, China. He notes the puzzle that the classical philosophical works were produced at approximately the same time, might be resolved by the effects of the far reaching Silk Road trade system. Furthermore, "The existence of Central Eurasian philosophers has generally been overlooked" (p. 59). This claim will be disputed by many, but the question of Silk Road transmission of ideas does warrant further exploration.

He goes on to discuss the origins of Iranian states from the interaction with steppe nomads. He argues that Herodotus ignored Scythian and Sarmatian agriculture. The new Iranian culture also influenced the shaping of Hsiung-nu social organization. He emphasized that Central Eurasia was **not** simply a conduit, but "an economy and world of its own" (p. 74). Trade was critical for nomadic pastoralists, but not because of poverty since they were better fed and lived longer. This is why China had to keep peasants at the edges of the empire in: joining the pastoralists was an attractive alternative. This, of course, raises a key question: why the trade? The answer is that the leader needed trade to acquire goods to support himself and his comitatus. This explanation is quite close to what Thomas Barfield (1989) has called the "outer frontier strategy," which I discuss further in my comments on the Epilogue.

Beckwith next turns to the rise of the Roman and Chinese empires. By the time Rome was attempting to conquer the Franks and Germans they had acquired much of the Central Eurasian Cultural Complex, especially the comitatus. In the Eastern Steppes the Hsiung-nu and their successors had major influences on China. Indeed, much of their fighting against the Han was not because they were "natural warriors," but attempts to recapture conquered pastoral lands. He also discusses the complex roles of the Kushan, Tokharians, Hsien-pei, and Proto-Japanese-Koguryoic peoples in Central Eurasian history. The attempts of China and Rome to conquer the Central Eurasians were not sustained, probably because they curtailed trade. The resulting internecine fighting led to a decline in the size of Central Eurasian cities, and probably

contributed to the relative collapses of classical civilizations. One might add that diseases spread through trade might well have been other contributing factors (see McNeill 1976).

These collapses play a role in the “great wandering,” even though its causes are far from clear, its consequences were “revolutionary” (p. 93). The Huns drove Alans and Goths toward Europe, and spread the Central Eurasian Cultural Complex to Japan. Thus, from Britain to Japan the CECC was typical. There were no clear boundaries between Central Eurasia and the peripheral sedentary states and there was considerable movement between urban and rural areas. This general discussion leads to more detailed accounts of the spread of the Huns and the fall of the Western Roman Empire, the early Germanic kingdoms, the growth of the Eastern Roman Empire and rise of the Sassanid Empire, conflicts between China and Hsien-pei, the rise of the Avars and Turks, migrations into Korea, and spread of CECC to Japan. Nations began to be defined by people “bound by oaths” (p. 107). The lack of sharp geographical divisions on the steppe allowed easy movement, whereas among the peripheral agrarians states borders were “macrocosmic reflections of the borders between agricultural fields” (p. 108). Frontier zones often had cultures of mixed steppe and peripheral states. These frontier regions were much more susceptible to effects of economic troubles than more central areas. Beckwith concludes with a reiteration of the strong critiques of Henri Pierrene’s argument that change in Europe was a result of isolation.

The next chapter discusses the rise of Türk Empire, the second regional empire (Scythians were the first). Mid-sixth century saw Persia and Eastern Roman Empire at war. By the end of century the Sui Dynasty had reunited China. The Franks in Western Europe, Arabs, Tibetans, Kazars, and other Turks were marginal empires. Notable in Beckwith’s account is the argument and evidence that the Arabs did not destroy Greek or Persian libraries. After building the Grand Canal the Sui were replaced by the T’ang. Despite flourishing of high culture, especially poetry, the T’ang constantly provoked frontier wars to justify expansion. The account of the Tibetan Empire is a recap of Beckwith’s earlier book (1987/1993). The Second Türk empire was built, and Arabs conquered much of West Asia. China and Arabs combined to defeat Central Eurasians, China motivated by fear of north-south alliance cutting it off from the West. Because each state held its emperor to be ruler of “all under heaven” warfare was nearly constant and expansion brought the separate empires into direct contact and fostered a flourishing of the Silk Road.

The account then turns to the revolution in, and subsequent collapse of the Silk Road. Beckwith notes that in a 13 year period in the eighth century “every empire in Eurasia suffered a major rebellion, revolution, or dynastic change” (p. 140) (Türk dynasty in the eastern steppe replaced by the Uighur, Byzantine rebellion, Abbasid revolution, Carolingian revolution, Tibetan rebellion, and the An-Lushan rebellion in T’ang China). He notes that, most of these “were

led by merchants or people closely connected to merchants and international commerce” (p. 141). World religions spread widely, literacy spreads across Central Asia, and trade shifted further north. He notes the possibility of external influences of Pippin on the Merovingian overthrow, which is generally seen to be due to internal causes. Charlemagne’s conquests spanned territories that contained land routes to Central Asia. The wars that brought Chinese and Arabs together helped transfer new technologies, such as paper from China to the West.

The causality is equally obscure for the correlated spread of world religions; Buddhism to Tibet, Manichaeism to the Uighurs, and Taoism became official for the T’ang. These changes were accompanied by spread of literacy. Again causal connections are unclear. One might speculate that a trade induced need for record keeping drove spread of literacy, and with writing came religious and philosophical ideas. Certainly forms of poetry and music spread.

This all collapsed early in the ninth century. Climate and Chinese massacre of Sogdians are possible causal factors, along with rising xenophobia in China. In short a great deal was happening that was probably interconnected, but how and why remain open questions. The extent and timing of these changes cries out for explanation, but as Beckwith notes, there is none. His comment on the important roles of merchants or leaders closely connected with them suggests complex causal processes. Beckwith’s determination to avoid macro theories handicaps him from exploring causality. Most likely it will be found in the interconnections among empires, and the mergers of erstwhile separated empires. It is unfortunate that he eschews this examination, since his thoughts would surely have been provocative. More on this at the end.

Beckwith then turns to the rise of many small princely states and recovery of the economy. As a consequence the political history becomes quite complex, and defies easy summary. The spread of monastic institutions help to spread literacy, along with increasing orthodoxy. Despite this, the small size of states allowed more movement and intellectual growth. In Central Asia Sufis and other Moslem philosophers prospered, but were eventually curtailed by increasing orthodoxy. Still, Beckwith argues that much of Islamic science and philosophy arose in Central Asia at this time and spread throughout the Islamic world, and later to Europe. The Sung eventually turned inward, but developed printing, paper money, rockets, and precursors of guns. Chinese merchants, though often despised, spread Chinese culture to much of Southeast Asia.

The following chapter focuses on the Mongol Empire which turned most of Eurasia into one commercial zone, spread many of the new Chinese inventions, and helped spread the Black Death and other diseases. The entire empire collapsed in the fourteenth century due to problems of succession, and unprecedented floods and famines, exacerbated by spread of new diseases. The Timurids and Tamerlane mark the only time that urban areas became the

cultural and political center of Eurasia. One of its enduring legacies was lavish patronage of the arts. While the Mongols facilitated trade they did not change the cultural and ethnolinguistic divisions in Eurasia.

The mid fifteenth century saw the rise of maritime states as Russians spread to the coasts, and the Portuguese and Dutch entered the eastern oceans. This was also era of the rise of the Ottoman and Mughal empires. The Safavid rise after the defeat of the Ottomans brought Shi'ism to Persia, sowing the seeds of many later conflicts. Generally, however, this was an era when the littoral states began to surpass the continental states as centers of trade. Beckwith also notes, "Considering the generally overlooked fierceness of their neighbors – the Greeks, Romans, Persians, Arabs, and Chinese, among others – Central Eurasians had to be fierce" (p. 212). But the Europeans needed to exert control over port cities, much as Central Eurasians had done for over two millennia for continental trade cities. China's xenophobia left openings for the Portuguese to exploit. After reviewing the more or less standard histories of various empires Beckwith notes that the Renaissance was Eurasian wide, not just in Europe.

The account then turns to how the Silk Road was closed by the late seventeenth century partitioning of Central Asia. Beckwith notes how only Afghanistan retained much autonomy as buffer between Russia, China, and India. Following an account of the rise of the Manchus, Beckwith turns to discussion of the rise of European domination of Central Eurasian littoral which eventually put the Silk Road out of business. This dominance was completed with the eventual incorporation of Japan into the trading system. The rise of littoral trade was not "imperialistic colonization but international trade" (p. 250). With the collapse of large pastoral nomadic states and associated trading cities, the Silk Road was no longer nourished, and faded away. As a consequence, "Central Eurasia suffered from the most severe, long-lasting economic depression in world history" (p. 261).

The penultimate chapter is concerned with the spread of modernism throughout Eurasia, especially its most central areas, where it was devastating. "*Central Eurasian culture suffered the most of any region of the world from the devastation of Modernism in the twentieth century...*" (p. 288, ital in original). Much of this chapter is a tirade against how modernism destroyed the arts with its cult of the ever new and more shocking. This was especially harmful in Central Eurasia because unlike Europe it did not keep the old along side the new. Here Beckwith seems to go to extremes in desecrating modernism and critiquing "modern" art. Still, as just noted this was especially harmful in Central Asia. Rather than debate the merits of modernism it may be best to follow the old dictum: *de gustibus non disputandum est!*

The closing chapter examines the contemporary era, what Beckwith sees as the fourth regional empire era. After the collapse of the Soviet Union most of Central Eurasia again became independent but had no unifying agency. The

destruction of traditional arts under modernism and the Soviets, left a cultural vacuum, though Central Asian rock music may be beginning to fill that void. Critique of arts notwithstanding, Beckwith does argue that much of modern world culture does come from Central Eurasia, not the agrarian peripheral states of the earliest “civilizations.” When combined with Anthony’s work (2007) the case is strong that the real homeland of Europe is Central Eurasia. While many readers may not be fully convinced, it should be clear that the argument cannot be dismissed out of hand, but should be taken seriously.

Finally, the epilogue on “Barbarians” is extremely valuable. It expands on a conference paper first delivered in 1987, and often cited, typically without permission (another disclaimer: Christopher Chase-Dunn and I did cite it (1997, p. 266, n. 5 to text on p. 158), but with permission and based on other conversations with Christopher Beckwith). As is typical the overall argument is simple, but the details are complex. In short, the term “barbarian” is inappropriate for any Central Eurasian group – and probably for any human group – other than in its original Greek meaning: people who did not speak Greek. Furthermore, the Chinese had no equivalent term, though to be sure they had many terms for foreigners and many of them are pejorative. Beckwith notes in several places that virtually all of the “peripheral states,” all sedentary agriculturalists, often described as “civilized” committed military atrocities that significantly overmatch those committed by any nomadic pastoralists. But there is much more than that the term “barbarian” is politically incorrect. It quite often leads to bad history. Pastoral nomads were not poor. They did have pockets of agriculture, and did not have a dire need for sedentary agricultural goods. They did not wantonly raid for “loot.” Rather, they fought to [re]open gates of trade and to defend or recapture territory previously taken by sedentary states. Indeed, raiding and war were means of last resort to rectify either of these wrongs. If or when they were “warlike” it was because they were forced to be so to defend their livelihoods from encroaching states. These findings vitiate entirely many of the explanations for pastoral nomad fighting and migration. The explanations must be sought elsewhere as Beckwith has done throughout his text.

All this makes his use of Thomas Barfield’s *The Perilous Frontier* (1989) as a strawman upon which to thrust his arguments very puzzling. I see the two of them as being in substantial agreement. Judging from Beckwith’s bibliography I have read far more of Barfield’s work than Beckwith. I can suggest three increasingly serious reasons for Beckwith’s misunderstanding of Barfield. First, and almost trivial, is that Beckwith relies heavily on the works of Nicolo Di Cosmo (1994, 1999, 2002) whose major work Barfield (2003) criticized brutally, but persuasively. Basically Barfield argues that Di Cosmo does not understand how pastoral nomadic societies operated. Where they were similar it was due to similar conditions, **not** inherent “nature,” and that much of their social structure was shaped by their external relations. Second, Barfield, like

many social science writers (including this reviewer) assumes that everyone understands that a structural argument is **not** a claim about nature or culture. Rather, it is an assertion that any humans, under similar conditions will act similarly. This is precisely what Beckwith said about history not repeating itself (p. xi).

The third and most serious problem is that Beckwith fails to grasp one of Barfield's key contributions: the distinction between inner and outer frontier strategies used by pastoral nomads in conflicts with sedentary states, most often with China.

In an outer frontier strategy nomadic pastoralists raided border cities and demanded tribute as guarantees against future raids. Interestingly Beckwith notes that the Chinese word for such tribute actually refers to a form of trade. In this strategy nomads avoided taking Chinese territory and expected that destroyed cities and towns would be rebuilt. Rather, a steppe confederacy leader used trade, tribute, and/or booty from raids to reward followers and to attract more allies (Barfield 1989: 49ff; note the close similarity to Beckwith's description of the *comitatus*). In an inner frontier strategy a weak party in a conflict among steppe khans would ally with a local agrarian leader in China in order to gain support in his conflict. Not infrequently, a local agrarian leader initiated such an alliance (Barfield 1989: 63ff). Both leaders grew strong together. When sufficiently successful the steppe leader would then revert to an outer frontier strategy. The outer frontier strategy only works when the sedentary state is strong. This why steppe confederacies and sedentary states tend to rise and fall in synchrony, and this is one of the mechanism that links East and West Asia. This is Beckwith's argument too, albeit not in these terms. To establish this point would require a painfully long exegesis. This broad overlap in understanding pastoral nomad – sedentary state relations makes Beckwith's use of Barfield as strawman very puzzling, but in no way vitiates his larger point about the disutility of the term and concept "barbarian."

This is a rich and complex book. Implicit in it, despite Beckwith's desire to avoid macro theories, are many theoretical propositions, and empirical data with which to explore explanations of long-term, macro change. I now turn to a brief discussion of some of these.

Assessments and Insights

From the point of view of a social scientists interested in building and evaluating theoretical accounts of change, Beckwith's "just the facts ma'am" approach is frustrating. While one can empathize with his reluctance to enter into myriad theoretical debates, his inability to explain many of the patterns he notes is a result of his eschewing of all theory. So I will turn to some of the theoretical issues that might have been addressed, and try to assess how Beckwith's account sheds light on them.

First, I want to note what I see as the contribution of thick descriptive accounts such as Beckwith's to theoretical issues. They certainly do not prove (indeed, hypotheses are never proved, only corroborated) nor even disprove theoretical propositions. Rather, they are great tools for probing theoretical accounts, finding weak spots, areas in need of further theorizing, and suggesting new avenues of development of both theoretical propositions and kinds of empirical evidence that might be used to evaluate those propositions. Most useful perhaps is the use of thick descriptions to help in theory-building by expanding the topics explained and/or exploring the limits of generality.

In doing this I will draw most heavily on my own work in world-systems analysis (Chase-Dunn and Hall 1997, 2009, forthcoming; Chase-Dunn and Babones 2006; Hall and Chase-Dunn 2006), along with some related approaches (see Denmark et al. 2000; Hornborg and Crumley 2007; Hornborg et al. 2007). Many of these comments apply to other theoretical approaches, *mutatis mutandis*.

First, more studies of how world-systems merge would deepen our understanding of how the modern global system came to be. Here Beckwith's comment that Central Eurasia was "an economy and world of its own" (p. 74) suggest that it might be viewed as another world-system wedged between those in east and west Asia. Equally, or even more, valuable would be insights into what alternative paths there might have been and why the one that did occur was the one that did happen. As always with such studies of the past, we might also gain some insight into both what alternative futures might exist, and possibly insights into how to promote the more desirable futures – presuming of course that some general parameters of just what "desirable" means could be developed.

Second, far less grandiose, but more possible are insights into the overall patterns of world-system expansion and incorporation of new areas, new peoples or both. At which phases of the various world-systemic cycles are forms of resistance more, or less, likely. This is a subset of a wider question: what are the variations of the roles of agency in any sort of social, economic, political, or cultural change?

For instance Chew (2007 and especially 2008) notes how the rise of monasteries in Western Europe were a response to the collapse of the western Roman Empire and the onset of a "dark age" and were a way to slow population growth, build bioregional self-sufficiency, and spread new agricultural techniques. Chew said in a private communication (26 Feb. 2009):

I did not focus on the monks in Asia, primarily because I wanted to draw the parallel between the fall of Rome and the current collapse. There is a periodicity. One of my students did his doctorate on looking at Dark Ages in China and showed much synchronicity but he never did look at monks in Asia.

Does this relate, and if so how, to the Beckwith's arguments about eighth century rebellions and subsequent spread of world religions, rise of monasticism and orthodoxy? It is certainly a tantalizing question. Superficially, at least, it would seem that important world-system processes were a significant part of these changes, given approximate synchronicity of events across Afroeurasia. Beckwith's emphases on external links would also seem to support this sort of interpretation. The point here is **not** to claim this is so – the evidence has yet to be carefully examined. Rather, the point is that this is a question worth pursuing, regardless of the answers eventually uncovered.

In *Rise and Demise* Chase-Dunn and Hall (1997), following the original insights of Frederick Teggart (1939), noted a correlation in the changes in empire and city sizes between East Asia and Southwest Asia, but that South Asia did not seem to move in synch with these changes. The latter point remains a puzzle. Many refinements in data continue to support this finding (reviewed in Hall et al. 2009; Chase-Dunn et al. 2010). This finding also prompted Peter Turchin and Thomas Hall (2003) to use population ecology to explore possible mechanisms for such synchronization. They showed how very low levels of interaction could bring cyclical processes into synch. Among many other things, this underscores the importance of contacts along the Information and Prestige Goods networks. Although by any contemporary standards that level of contact was low, it was sufficient to synchronize cyclical processes. Even where cyclical processes were chaotic and diverged over time, only few cycles of 100, 300, or 600 years, would have had strong effects on historical processes. Finally, the most recent papers on this topic note the very important roles of Central Asian city-states and pastoral nomads in these processes. Beckwith's detailed account of Central Asian history might have addressed some of these speculations (at least the earlier ones, if not the more recent ones). Again, the issue is not the veracity of his claims, but rather that a more refined empirical exploration of them could have been done.

At times Beckwith actually is making implicit theoretical propositions. As noted above his analysis of the workings of the *comitatus* is fully congruent with Thomas Barfield's (1989) outer frontier strategy: a pastoral nomad leader uses wealth gained through trade and/or booty to attract followers, strengthening his power. This explains how and why Central Asian pastoral confederacies could grow and expand so quickly, but also why they were inherently unstable. Charismatic leadership, like that of most khans, tends to disintegrate upon the death of the khan. Thus, the question is **not** why did the Mongol Empire last for such a short time (though it survived longer than the United States has survived thus far). Rather, how did it survive through several successions? Similarly, Beckwith's argument that the *comitatus* form of leadership eventually spread from Japan to Western Europe surely had major

effects on the rise and fall of states, empires, and world-systems. This, too, needs much more elaborate empirical investigation.

The expansion of states and peoples described by Beckwith surely gave rise to many instances of world-system incorporation and to frontier formation. His descriptions suggest a myriad of cases available for comparative study that would allow careful assessment of processes and causes (see Hall 2009 for detailed discussion of methods for doing this). This is important for several reasons. First, as suggested by Ferguson and Whitehead (1992a, 1992b), these processes demonstrate that displacement of nonstate peoples, typically called “barbarians” by members of agrarian states, is an ages old process caused not only by expanding European states in the last several centuries, but for millennia. This strongly suggests that capitalism – though clearly more efficient in doing this – is not the key cause. It seems to be states (and empires and world-systems). Teasing out the nuances of how these instances are similar, and how they are different should yield considerable insight to the patterns of historical change.

The formations, transformations, and disintegrations of frontier zones offer a special, often unique, window into processes of ethnicide, ethnogenesis, amalgamation, and ethnic symbioses (see Hall 1998 for more details). Which groups interact with each other, interbreed, and often intermarry, and which do not do so can shed light on many historical changes. They can also help us to understand linguistic changes especially in instances of gender asymmetry, which seems to be quite common in frontier zones. Beckwith, although it was not his intent, offers an immense catalog of such interactions that could be mined for such studies.

Evaluating *Empires of the Silk Road*, both in general, and for readers of *Clodynamics* is a daunting task. Beckwith’s arguments are persuasive, and backed by considerable empirical evidence. He is scrupulous about noting where the evidence is murky and noting where further research is needed. Beckwith provides an interesting Central Eurasian perspective on world history. Indeed, *Empires of the Silk Road* is Andre Gunder Frank’s *Centrality of Central Asia* (1992) on megadoses of steroids! Even where some scholars might dispute some facts or interpretations, the overall thesis stands up quite well. However, using his insights to plan further research, especially more quantitative research, will require a great deal of interpretation and considerable effort to operationalize many concepts, assuming that any sort of reliable quantitative data can be assembled. Still, his arguments are persuasive that such work will be essential to deeper understanding of human history. Whether one ultimately agrees with or demurs with much that Beckwith says, *Empires of the Silk Road* is work that any scholar who seeks to write about Central Eurasia will need to address closely. It is a benchmark – indeed a high one – for Central Eurasian, and indeed, world history.

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