

for non-canonical rhyming, or even simply trying to *sound* non-canonical? The vagaries of interpreting rhyming evidence should have been discussed at the outset of a work like this, so that plausible counter-theories could be decisively disposed of. Which is unclear—the author’s writing or his scholarship? I fear it may be the latter.

The chatty tone of the book does not conceal the fact that the author is uncomfortable with the technical aspects of his material. I find that tone, along with the many contractions used (and abbreviations such as “2x” for “twice”), distracting in a scholarly work; I grant that these are small matters but they make it harder to follow an argument whose logic is less plain than it ought to be.

In short, this book should not have been published in its present form. It deals with an important question but is confusing and awkwardly composed. In its favor, however, I can say that it is a great improvement over the author’s earlier work on this subject, “Pursuing Zhuangzi as Rhymester: A Snark-hunt in Eight Fits” (*Sino-Platonic Papers*, no. 67, April, 1995), which examines the philosophies within the rhyming passages of *Zhuāngzǐ* but has no foundation in either phonology or statistics. This new essay is more coherent than the earlier one was, but its rambling and hesitant presentation makes me wonder about the soundness of its findings.

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*Empires of the Silk Road: A History of Central Eurasia from the Bronze Age to the Present.* By CHRISTOPHER I. BECKWITH. Princeton: PRINCETON UNIVERSITY PRESS, 2009. Pp. xxv + 472. \$35.

The many years Beckwith has dedicated to the study and teaching of the history of Central Eurasia shine through in this intellectually wide-ranging book. Not many scholars are comfortable working with materials in Chinese, Old Tibetan, Arabic, Old Turkic, and a range of other languages. And not many books discuss the Koguryo, whose realm bordered on the Pacific, and the Franks, whose territory touched the Atlantic, on the same page (135). However, the strong points in this book are often overshadowed by glaring flaws. It probably should not be used as a textbook unless the instructor knows the subject well and can help students focus on the book’s strengths and avoid its flaws.

The historical core of the book, twelve chapters long (pp. 29–319), begins with “The Chariot Warriors” and the Proto-Indo-Europeans in the late third millennium B.C. and ends with “Central Asia Reborn” and the twenty-first century. The most important historical points are well covered. The lengthy bibliography (pp. 427–55) and comprehensive index (pp. 457–72) magnify the usefulness of the volume. A book covering such enormous historical geography as this one should contain many detailed maps. Instead, there are only two maps, one inside the front cover on premodern Central Eurasia and a corresponding one inside the back with modern political details. The book would be significantly more useful with more maps, especially if it is intended for use as a textbook.

The author almost seems to seek out and relish the role of iconoclast, frequently and with vigor challenging traditional views. In some cases the traditional ideas deserve to be overthrown and here the author does a fine service. He amply succeeds in showing that Central Eurasia is central to world history rather than peripheral to it, and that Central Eurasia was no more made up of barbarians than any other part of the world. But in other cases the author goes too far with his proposed intellectual revolutions. The idea that Avestan, the Old Iranian liturgical language of Zoroastrianism, was a “phonologically Iranized Indic language” (p. 368) will probably not win converts among Iranian historical linguists. And he wishes to cast away the idea that civilization might come from Sumeria, Egypt, or China: “Central Eurasia is our homeland, the place where our civilization started” (p. 319). The author is also prone to expounding idiosyncratic theories and is often highly opinionated. For instance, in chapter 12 he asks, “Why have Modern artists failed to produce much real art after an entire century of revolution and experimentation?” (p. 314). This is in a section several pages long decrying what he

sees as the failure of an artistic movement, and culminating in a call to arms: “It is time for artists to reject the death grip of Modernism . . .” (p. 318). Perhaps he is correct, but most readers would agree that such a rant is out of place in a work on history.

In the first paragraph of the preface the author describes his original intention “of writing professionally informed but readable essays for an educated general audience, with minimal annotation” (p. vii). But besides the core of the work, there is a preface, an introduction, a prologue, an epilogue, two long appendices (all of the above heavily footnoted), and forty-one pages with 111 endnotes in compact type. In order to explore this book at its deepest level, one must face the heavy chore of jumping about among all of these parts. The book would work better if it were structured with a more sequential narrative. Some extraneous ideas, like the diatribe on modern art (see below on chapters 11 and 12), could be eliminated, and other good ideas, like the exploration of the concept of “barbarian” (see below on the epilogue), could be condensed and included in the core of the work.

The prologue, “The Hero and His Friends,” sketches the author’s thesis that there are common cultural currents, specifically an origin myth and the *comitatus*, which he names the Central Eurasian Culture Complex, that spread from the earliest Indo-Europeans down to the Mongols and likely beyond. While there are indeed interesting parallels in the myths, they do not seem to prove much. As for the *comitatus*, I doubt that there is anything uniquely Central Eurasian about it. All military organizations have an inner circle: they cannot function without concentric circles of authority. This is probably true of all large organizations. One could describe both the Knights of the Round Table of King Arthur and the lieutenants of a mafia don as a *comitatus*. The epilogue, “The Barbarians,” is more convincing if a little long (pp. 320–62). He shows that Central Eurasians have often undeservedly received a bad reputation as brutish, deceitful, and vicious. This is partly because histories were written by the peoples on the periphery of Central Eurasia who were often in conflict with them. Although there were brutal massacres performed by Central Eurasians, they probably did so no more than other, supposedly more civilized peoples.

Chapter 1, “The Chariot Warriors,” covers the period of the dispersal of the Indo-Europeans to the borders of China, the Near East, India, and Greece and the role of the chariot. While his grouping of Indo-European dialects and peoples is idiosyncratic (see below on appendix A) and likely incorrect, there are some points where his iconoclasm is productive. For instance, he argues that the chariot-riding Indo-Europeans had a greater role in the history of China than is usually assumed, and he notes that the language of those people may have been “an otherwise unknown branch of Indo-European” (p. 45), rather than Tocharian as is usually assumed. He suggests that because “Hittite does not seem to preserve the Proto-Indo-European words for ‘wagon’ and so forth,” the Anatolians acquired the chariot after leaving the homeland (p. 38 n. 38). But the wagon and the chariot are two very different things, invented more than a thousand years apart. No matter what the evidence of vocabulary, for general historical reasons the Hittites likely left the homeland with wagons and much later adopted chariots from foreigners. In this case his conclusion is correct but the road there is rocky.

Chapter 2, “The Royal Scythians,” deals with the steppe hegemony of the horse-riding Iranians, Scythians, Cimmerians, and Sarmatians, and the later Xiongnu whose ethnic identity is not clear. He describes the feinting and withdrawing of the Scythians as “the classic Central Eurasian guerilla warfare technique” (p. 68). This description reveals his Central Eurasian-centric posture, since feinting and withdrawing is a common military tactic—a common solution to a common situation—and there is nothing particularly Central Eurasian about it. But he usefully points out that the nomadic cultures were not impoverished as often thought but rather that “Nomads were in general much better fed and led much easier, longer lives, than the inhabitants of the large agricultural states” (p. 76).

Chapter 3, “Between Roman and Chinese Legions,” discusses Romans, Franks, Sarmatians, Alans, Huns, Goths, Parthians, Sakas, Tokharians, Wusun, Kushans, Xiongnu, Chinese, Xianbei, the Koguryo, and several others. He equates the Tokharians with the Yuezhi, and the Wusun with the Aśvins, as if these are established facts, and refers to his arguments in appendix B. But these identifications remain controversial, rather than established, for most scholars. Chapter 4, “The Age of Attila the Hun,” discusses the Huns in Europe, the Western and Eastern Roman Empires, the early Germanic kingdoms, the Sasanids, the Hephthalites, the fall of the Han Chinese, the rise of Jin and Toba Wei, Puyo and

Koguryo, and Japan. Beckwith has also worked with the ethnolinguistic history of Korea and Japan and in that field also as an iconoclast. In note 59 he criticizes the approach of other scholars in this area, but in this book his approach to that topic is relatively conservative.

Chapter 5, "The Türk Empire," also covers the Roman-Persian wars, the rise of the Arabs, Chinese reunification under Sui and Tang, the Tibetan Empire, the end of Koguryo, and the rise of the Franks. Beckwith describes the rise of the Turks as "following the dynamics of the Central Eurasian Culture Complex myth" (p. 112). He changes three historical names: he alters the name for the Xianbei (Hsienpei) used by the Turks from Tabghaç to Taghbaç (p. 113), referring to his explanatory publication in a footnote (note 2). Either he should clearly justify changing the name known to generations of scholars, or he should keep the traditional nomenclature and sketch his theory in a note. The name Bumin of the founder of the Turk Empire he revises to Tumin (p. 114), suggesting in note 14 that the Bumin of the Old Turkic inscriptions is an error. It would be odd indeed for official inscriptions to misspell such an important and well-known name. He also alters Taspar Kaghan to Tatpar (p. 117) with references in note 16. All three changes will be controversial, if not widely rejected. Beckwith asserts that "it is generally believed that there were no major linguistic divisions in the early Old Turkic period," and that the Bulgar language diverged dramatically soon after that period (p. 118). I think the general view is somewhat different. From a still poorly understood Proto-Turkic there evolved distinct Western and Eastern Old Turkic branches. The only modern survivor of Western Old Turkic is Chuvash, a descendant of Bulgar. All other living Turkic languages are from the Eastern branch. The spread of Turkic peoples in the pre- and early historic periods is still not well understood. Beckwith also opines that there is something special about the frequent warfare in medieval Central Eurasia due to shared ideology and a long history of war (p. 137). But frequent warfare was likely the norm everywhere on the planet until recently.

Chapter 6, "The Silk Road, Revolution, and Collapse," covers mid-eighth-century upheavals in Central Eurasia and its periphery, from the Carolingians to the borders of Korea. It is not clear to me that there was more upheaval in this period than in the preceding or following centuries; it is simply possible that we just have better sources for this time. Beckwith notes that this is the period when urbanizing steppe polities adopted world religions, the Uyghurs Manicheism in 763 (p. 148) and the Khazars Judaism in 740 (p. 149), although in note 75 he notes that Peter Golden prefers the early ninth century for the latter conversion. There is the suggestion that the Sogdian community along the Silk Road secretly planned and led both the An Lushan rebellion in Tang China, 755–757, as well as the Abbasid revolution in the Caliphate, 747–750 (p. 146), which elevates the historical profile of Sogdia, in itself a good thing, but carries the flavor of a conspiracy theory. While outlining the importance of the rise in literacy in this period, mostly associated with world religions, he mentions that Prakrit texts were translated into Old Turkic (p. 156), but I do not think there are any examples of this.

Chapter 7, "The Vikings and Cathay," again has a rich description covering a long list of peoples and polities. For the non-initiated, the detail will be overwhelming. Beckwith does a good job of showing the key role of Central Asian intellectuals in the flowering of Islamic science, mathematics, and philosophy, and demonstrating how medieval Western European intellectualism, and hence "Western civilization" depended on contact with this movement (pp. 176–80). Chapter 8, "Chinggis Khan and the Mongol Conquests," covers the rise of the Chinggisids to the fall of the Timurids (twelfth to fifteenth centuries). Under the Pax Mongolica the Silk Road was likely more open and safe than at any other time. Chapter 9, "Central Eurasians Ride to a European Sea," deals with the rise of the Ottoman, Safavid (Persia), and Mughal (eastern Central Asia and northwestern India) empires in the fifteenth century, the Russian in the sixteenth, and the Manchu and Junghar in the seventeenth. Important also was the landing of the Portuguese explorer Vasco da Gama on the Malabar coast of India in 1498, and the development of littoral commerce which would soon make the Silk Road less important for world trade.

Chapter 10, "The Road is Closed," describes the conquest and partition of Central Eurasia by the peripheral powers, Russia and Manchu-China, working together. This is the beginning of the economic and cultural decline of the region, which might better be described as a plunge into poverty and darkness. At the same time, there is "The Great Urban Shift to the Littoral" (pp. 245–51), well described by Beckwith, in which coastal cities in Asia and Russia rise dramatically to prominence while interior

cities decline in importance. Chapter 11, “Eurasia without a Center,” is the longest chapter but may have the least focus on Central Eurasia. We find sections on China, the First World War, Russia, Germany, Turkey, less than a page on modern Central Eurasia before the Second World War (p. 272), the Soviet Union, the Great Depression, the Second World War, the Chinese communist revolution, and the human and cultural devastation of Tibet, East Turkistan, and Inner Mongolia, the Iranian revolutions, and the Cold War. All of this is permeated by Beckwith’s opinion that Modernism is the root of most evil in the modern world. He defines this as “what is modern—new and fashionable—is *better* than what it replaces” (p. 288). There follows a fourteen-page rant on this evil trend. The section “Radical Modernism in Central Eurasia” (pp. 287–90) has very little on that geographic area, but it is noted in italics: “Central Eurasian culture suffered the most of any region of the world from the devastation of Modernism in the twentieth century” (p. 288). The final section, “Modernism and the Destruction of the Arts” (pp. 291–300), pays still less attention to Central Eurasia. A sample of iconoclasm is this statement about modern democracies: “In fact, none of them are true democracies, and most are not even true republics, but dictatorships or, at best, oligarchies” (p. 289 n. 70). Another: “The institution of compulsory national education in all modern republics brought with it the indoctrination of children in the ideology of ‘democracy’ so they would not oppose the programs of those who held actual political power but would instead unwittingly support them” (p. 266).

Chapter 12, “Central Eurasia Reborn,” begins towards the end of the twentieth century. As usual it contains much that is accurate and useful but that is often overshadowed by opinionated pronouncements. For instance, he repeats his view that no country in the world is a democracy (p. 305), and maintains that India’s economic rise at almost the speed of China was “little noted” (306). Both of those opinions would be debated by most scholars, and as they do not serve to support any of the author’s points, they could have been omitted. There is another decrying of Modernism for over five pages (pp. 313–18). The final flourish at the end of the core of the book is the comment, noted above, that our civilization started in Central Eurasia (p. 319).

Appendix A is pure iconoclasm. Beckwith proposes that the typological unlikelihood of the traditionally reconstructed Proto-Indo-European three-way contrast among the stops—voiceless ~ voiced ~ voiced aspirate—is a sign that the reconstruction is incorrect (p. 364). He proposes that the voiced and voiced aspirated stops are in complementary distribution in the proto language and later became distinctive in a “convergent” group of languages for which a three-way opposition is reconstructible (p. 364). This proposal alone would mark a revolution in Indo-European studies, but there are two more revolutionary ideas. He seems to propose three migratory waves of languages from the Proto-Indo-European homeland: wave A with one set of stop consonants (Tocharian, Anatolian), wave B with three (Germanic, Italic, Greek, Indic, Armenian), and wave C with two (Celtic, Slavic, Baltic, Albanian, Iranian) (p. 365). Although there are several schools of thought among scholars about the relative temporal and genetic relationships among the languages, Beckwith’s is outside all of them. He is able to separate the traditional Indo-Iranian family into Indic (B) and Iranian (C) waves because of his third proposed revolution—that Avestan is not an Iranian language (pp. 365–69). It is unlikely, to say the least, that any of these three ideas will enter the mainstream. All linguistic reconstructions are abstractions that can give only a partial picture, since some information is always lost, and the greater the time depth of a reconstruction, the more information lost. It is not in any sense wrong that the traditional reconstruction for the stops is typologically unnatural. It is an abstraction. Beckwith does not offer any evidence here for the complementary distribution of the voiced and voiced aspirates, instead referring in a note to an article of his published elsewhere (364 n. 5). But this evidence is crucial to the three revolutions. Either the arguments should have been at least summarized here in a convincing way or this entire appendix should have been omitted. His three waves imply that the language in the homeland had one set of stops when wave A left, then evolved to three sets of stops before B left, then devolved to two sets for wave C. This is far less natural than the traditional view. Finally, even if Avestan had not survived, the Indo-Iranian family is on solid ground with rich comparative evidence on the Iranian side from Old Persian and a wide range of middle and modern languages.

Appendix B, “Ancient Central Eurasian Ethnonyms,” is mostly philological speculation. In each section his arguments partly rely on idiosyncratic forms which he claims to have established in other places, so that their plausibility is hard to verify. While I do share with Beckwith the probably still

unorthodox view that chariot-riding Indo-Europeans played a key role in early Chinese civilization, his philological evidence for this is not sound. He tries to equate Qiāng, the name of the main enemy of the Shang Chinese, with Tokharian *klānk-* (Beckwith has *n* in place of *ñ*) ‘to ride, go by wagon’, suggesting that the name means ‘charioteer’ (p. 375). One problem with this is that languages generally borrow nouns rather than verbs. His comparison would perhaps be better with A *klānk*, B *klenke* ‘vehicle’, the noun from which *klānk-* appears to be derived, but this word seems also to refer to a mountable animal. The main word for wheeled vehicle is A *kukäl*, B *kokale* ‘cart, wagon, chariot’. It is also a real stretch to equate Chinese Wusun, name of a mounted nomadic people, with Sanskrit *aśvin* which Beckwith gives as “‘the horse men’, the name of twin equestrian gods” (p. 376). It would be preferable to use the basic meaning ‘possessed of horses’ here and leave the deities aside. He employs much speculative philology to get to this equation, then treats it as established. In discussing the Middle Iranian Saka~Sogdian~Scythian ethnonymic group, he equates the Chinese name for Yarkand, which he apparently analyzes as \**Saylā* in another work, with \**Saklai*, his analysis of the name of the ancestral nation of the Puyo-Koguryoic people (p. 378). Even if neither analysis is flawed, this still looks more like coincidence than common origin. There is a final foray into the Yuezhi~Tokharian~Toḥaroi debate, where Beckwith refers to another of his articles to help show that “in one of the highly archaic border dialects of Old Chinese in Antiquity the word 月 [yuè] ‘moon’ would have been pronounced \*tokwar or \*togwar” (p. 382).

The endnotes continue the balance between usefulness and iconoclasm. For example, note 93 correctly dismisses as folk etymology the notion that the name Istanbul is anything other than a shortening of Greek Constantinopolis, while note 94 maintains that the conspiracy theory about US leadership knowing beforehand about the attack on Pearl Harbor could not be correct since the leadership could not be that clever. Rather, the disaster was “due mainly to the stupidity, ignorance, and arrogance of the U.S. leadership” (p. 420).

A final criticism is that the author has a penchant for using outdated transcriptional or transliterational systems. In the beginning consistently we find the Wade-Giles transcription system for Chinese. For at least a generation this has been supplanted in scholarly works using Chinese by the Hanyu pinyin, partly because it has official status in China, but also because it is a superior tool. The ROC (Taiwan) did persist with Wade Giles for many years, and perhaps Beckwith persists in sympathy with that practice, but the most recent government in the ROC made Hanyu pinyin official on January 1, 2009. While the use of Wade-Giles makes it easy to compare names in older works, it will make it difficult for non-specialists in future to decipher whom or what Beckwith is referring to. It is not obvious to the uninitiated that Hsüan Tsang is Xuan Zang, the seventh-century Buddhist traveler, or that the Ch’ing are the Qing. Later in the book Beckwith does use pinyin when examining Chinese words. For instance, in the epilogue Chinese words appear in characters and pinyin (albeit in italics), but not Wade-Giles: 野蠻人 *yěmánrén* ‘wild person’ (p. 358), 草 *cǎo* ‘grass’ (p. 359). But at the same time Chinese names are in Wade-Giles: “Hsiung-nu” (p. 330), “Chang Ch’ien” (p. 354), “T’ang” (p. 359). Nonspecialists will find this mixing of systems puzzling. An idiosyncrasy in his pinyin is that carat is used instead of macron for the first or high tone: “番 *fān* itself has no negative connotations in T’ang texts” (p. 359), “Ch’iang 羌 NMan *qiāng*” (p. 375). This idiosyncrasy is disconcerting, as he does elsewhere use the macron, such as in Tocharian *klānk-* on the same page as *qiāng* in the last example (p. 375). This is in keeping with a general inconsistency in the use of carat and macron. For instance, though Indic and Iranian specialists have marked long vowels with macrons for generations, Beckwith uses a carat in Persian and Sanskrit: “*Nawbahâr* is the Persianized form of Sanskrit *Nava Vihâra* ‘the new *vihâra*’” (p. 25 n. 104), but a macron in Avestan *miθrəm yazāi zaθrābyō* and Vedic *mitrám yajāi hótṛābhyah* (p. 368).

In summary, this volume exhibits impressively vast learning and an immense chronological and geographical range. However it contains a great number of idiosyncratic views, many of which will likely be rejected by mainstream scholarship, and it should be read skeptically.

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