Review Article

Central Eurasians Everywhere

Christopher I. Beckwith, *Empires of the Silk Road: A History of Central Eurasia from the Bronze Age to the Present.* xxv, 472 pp. Princeton and Oxford: Princeton University Press, 2009. ISBN 9780691135892. $35.00.

In the words of Peter Golden quoted on the dust jacket, Christopher I. Beckwith's idiosyncratic new book, "ambitious, provocative, and bristling with new ideas...is quite unlike any other." Indeed, both reader and reviewer may find it a bit intimidating, given the breadth of the undertaking, the manner of exposition and the author's distinction and specialized research interests. A professor of Central Eurasian Studies at Indiana University, he is the recipient of many honors, including both MacArthur Foundation and Guggenheim fellowships. His first book, a pioneering work on the history of the first Tibetan Empire, quickly established itself as the authoritative study in a field which requires the mastery of several difficult languages.[1] A good deal of Beckwith's research since has focused on what he himself would characterize as the "arcane" subject of the early history of various East Asian languages. He traces the origin of his work on this new book back to a paper he delivered in 1987; in important ways one can see some of its themes in an even earlier article on "slave soldiers" in the Islamic world and in his first book, one of whose significant conclusions concerned the major place Tibet occupied in the history of early Eurasia, a fact that had remained largely unrecognized.

Before analyzing the main argument of his new book, I should comment on one major challenge which it will present for readers expecting to find in it a clearly articulated and scholarly account. The author states (p. xii) that his intent is "to write a realistic, objective view of the history of Central Eurasia and Central Eurasians," but I am not persuaded that is the way much in the book reads. It is an angry and polemical work, whose tone often cannot but raise questions in the reader's mind about its basic objectivity. To a considerable degree, the Central-Eurasian specific material is framed and inspired by a sweeping diatribe against Modernism and Postmodernism (pp. ix-xii, chs. 11-12). It is not my purpose here to argue with Beckwith in his judgments on such issues (in fact I tend to agree with many of his opinions), but I do have to wonder how much of that material really belongs in this book. The anti-modernist stance obviously serves certain purposes. Most importantly, it informs us why Beckwith is such a passionate advocate for the Central Eurasians, who not only have been victimized by developments in the modern world but whose historic importance and very value system, as he argues, have been distorted by the way modern authors have dealt with Central Eurasia (if they even pay any attention to it whatsoever). These are certainly valid points, but to drag into the condemnation of Modernism everyone from T. S. Eliot and Pablo Picasso to Arnold Schoenberg and Frank Zappa at best tells us about the author's personal tastes, at worst is simply irrelevant and distracting. For Beckwith "Modernism" seems to encompass anything he happens to dislike; so readers should not be surprised to find lumped under that epithet such strange bedfellows as modern "democracy" (always in quotes) and Islamic fundamentalism. As far as culture is concerned, he laments the passing of royal courts where "the aristocrats...represented an ideal...demanded perfection...[and] so hired the best artists and artisans to produce it..." (p. 300). Consistent with this stance, Beckwith cavalierly dismisses modern and postmodern theoretical works in favor of what he claims will be "straightforward presentation and analysis of what I consider to be the most relevant data known to me" (p. ix), even though, as we shall see, the way he treats existing scholarship is in many ways problematic.

Beckwith is to be praised for at least attempting to find in the otherwise confusing and amorphous history of Central Eurasia some abiding themes, although this effort then courts the danger of finding continuities and similarities where they do not exist or can at best only be hypothesized. One is tempted to suggest that there might alternatively be some merit to admitting "the irrelevance of the distant past," as one recent author has provocatively put it, which is, of course, not the same as saying we should not study it.[2] Interestingly, Beckwith's Central Eurasia is not a fixed geographical region but one whose "boundaries change along with human cultural and political change" (p. xix). At its greatest extent, in the period from High Antiquity to Julius Caesar's conquests and again "from the fall of the Roman Empire to the end of the Early Middle Ages," it encompassed

much of Europe north of the Mediterranean and extended all across Asia in the region south of the Arctic and north of the "warmer peripheral regions." In other periods the territory shrank (in Europe, for example, retreating to east of the Danube), due to expansion on the non-Central Eurasian periphery; that shrinkage continued significantly in modern times. The territory of Central Eurasia is to be distinguished though from territory in which Central Eurasian peoples might be found, since often they moved outside of the region or others moved into it. Beckwith's book then is not a history of a geographical region but is rather a history of the Central Eurasians, who "made fundamental, crucial contributions to the formation of world civilization" (p. xx). To understand what these are requires that he look both at the center and the periphery, since the two always interacted. In particular his focus is on what he terms "traditional Central Eurasia," which after the early Middle Ages "was coterminous with the ancient continental internal economy and international trade system misleadingly conceptualized and labeled as the Silk Road."

The whole point of this narrative and analysis is to dispel the negative views he claims most modern writers have regarding Central Eurasians, opinions based on the largely biased and inaccurate sources written in the peripheral societies. In particular these biased accounts have focused on nomads, who are in fact only "one segment of Central Eurasian society" and one which never was isolated from sedentary groups there. The stereotypical Central Eurasian nomads were warlike, poor, predatory and dangerous. On the contrary, Beckwith asserts, the nomads were no more warlike and dangerous than the sedentary peoples around them. First and foremost they were interested in peaceful trade and as a result were anything but poor. They were not the barbarians depicted in so many of the secondary sources but rather were "dynamic, creative people ... Central Eurasia in the Middle Ages was the economic, cultural and intellectual center of the world" (p. xxiv). The ills of the modern world though are largely the consequence of the Central Eurasians ultimately having "lost almost everything" to the encroaching states and peoples of the periphery, a history that parallels the fate of the Native Americans in North America.

What defines the Central Eurasians over time and space is what Beckwith calls the "Central Eurasian Culture Complex" (which I shall abbreviate CECC), "a collection of cultural elements shared by the peoples of pre-modern Central Eurasia that goes back to the Proto-Indo-Europeans" (p. 12). Its components include shared myths of origins (translated or summarized here in the Prologue), in which an oppressed people comes together under the leadership of a hero to overthrow their oppressors and gain their freedom. More concretely and most importantly, the CECC involved "the sociopolitical-religious ideal of the heroic lord and his *comitatus*, a war band of his friends sworn to defend him to the death" (p. 12). "The lord and his comitatus formed the heart of every newborn Central Eurasian nation" (p. 14) at least from Scythian times down through the rise of the Mongol Empire. The list of those for whom this is "directly or indirectly" attested is a long one, including Hittites, early medieval Germanic peoples, Koguryo, the early dynastic Japanese, for a while even Byzantines and Chinese, and "especially the Arabs." "By contrast, the true comitatus is unknown among non-Central Eurasian peoples..." (p. 17).

Now as Beckwith himself clearly indicates, the comitatus is not in fact equally well documented for all peoples at all times (his "indirectly" is important here). Thus he is quite comfortable, as others have been before him in discussing Central Eurasian political formations, to extrapolate from the relatively well documented case of the Mongols to other peoples about whom we know little. Moreover, even if in its original form the comitatus involved an oath to die for one's leader, the reality seems to be that far from all comitatus members did so. Perhaps the whole concept of the comitatus is as fluid as is the geography of what constitutes "Central Eurasia." The Chinese, as we learn, did not have the comitatus, although Central Eurasians in their service did. In the case of the Hittites, "it is likely" that the elite personal bodyguard of the king was his comitatus (p. 38). In the case of the Sogdians, the fact that Chinese travelers found among them "huge numbers of warriors" is sufficient for Beckwith characterize the Sogdians as having "a Central Eurasian warrior ethos with a pervasive comitatus tradition" and to conclude that "the comitatus was evidently more widespread among the Sogdians and other settled Central Asians than among any other Central Eurasian people." Thus, they were as warlike in their dealings with neighbors as were the nomads (p. 77). "There is no reason to think the situation was any different in Antiquity." But is that sufficient to suggest that it was the same? I wonder. As far as the Koguryo were concerned, the "elite warriors referred to in the sources were probably the king's comitatus; unfortunately, the sources are unclear on this point" (p. 91 n. 37). The "only known motivations" for the rise of the medieval Tibetan Empire "are the sociopolitical features of a culture with the Central Eurasian Culture Complex" (p. 127). While that statement may be unexceptional if dealing with kings and their armies, the

argument seems a bit forced when it comes to explaining that the devotion to a spiritual teacher in the form of Buddhism Tibetans adopted "was little different from that of the comitatus members to their lord." That is, the comitatus "at least to a certain extent was transmuted into a monastic form" where Tibetan monasteries were inhabited by warrior monks (pp. 151-52). Moving on, the Safavids "eventually ensured their success" thanks to a "comitatus-like dedication to their leader" (pp. 208-9).

Granted that something "comitatus-like" may be found all over the map if one starts by assuming it should be there, we are still left with the question of whether it is really distinctive to early Central Eurasians and how it turned up in so many different places. It is not at all clear that Beckwith has done a systematic examination of non-Central Eurasian cultures (nor should we expect he would have). Not the least of the problems in all this involves the endlessly controversial issue of the early Indo-Europeans and their migrations: where they were when, whether one or another people should be identified as Indo-European and, more importantly, what the cultural implications of that identity may be. It is easy to understand the encomiums in Victor Mair's dust jacket blurb for this book, since he is a strong proponent of the idea that so much is owed the Indo-Europeans almost anywhere one might look in early Eurasia. However, even some of the advocates of this viewpoint may find Beckwith's views on, for example, migration lacking in nuance and conviction.[3] And Beckwith is likely to find himself in a very lonely place when it comes to such speculative suggestions as that concerning possible Indo-European influence on the early Chinese language and development of its writing system (pp. 45-48; endnotes 43-47), thanks perhaps to the presence in China of Indo-European "specialists" in new technologies.

Beckwith states the obvious, that to confirm a good many of his hypotheses a lot more work is needed, including additional archaeological work on elite burial complexes. An intriguing test case would be the burials of the Xiongnu, who certainly are among the primary examples of Central Eurasians who are supposed to exhibit the features of the CECC. The history of the Xiongnu is one where traditional perceptions have been shaped by the Chinese annals, since the Xiongnu did not have a writing system of their own. Even though, as Beckwith emphatically points out, the Chinese sources are biased and should not be trusted, he himself has to use them as sources for the Xiongnu version of the "First Story" account of the origins of their polity and for other kinds of details concerning their culture and economy.[4] Multi-ethnic as the Xiongnu probably were, it is anyone's guess where he finds sufficient evidence though to assert "it is probable that they [the Xiongnu] either learned the Iranian nomadic model by serving for a time as subjects of an Iranian steppe zone people, as in the First Story model...or they included an Iranian component when they started out...." (pp. 72-73).

The other main source we have for Xiongnu history is archaeological evidence, where to date much of the emphasis (as so often is the case with excavation of Central Eurasian sites) is on elite burials. The Chinese sources delight in depicting a "barbaric" Xiongnu elite culture with human sacrifice accompanying the burial of a leader. If such really was practiced, then it might reasonably be interpreted as the ritual death of members of a comitatus. Indeed, many Xiongnu elite tombs are accompanied by "satellite" burials which some have interpreted as "sacrificial" ones, in keeping with what the Chinese sources indicate.[5] Even if that were the case (the matter is debated), those satellite burials cannot simply be the burials of a warrior comitatus, if for no reason other than the fact that among the buried are young children.[6] Furthermore, there are yet to be answered questions as to whether "satellite burials" occurred at the same time as the main ones or perhaps only later. Apart from the excavation of both the elite tombs and "ordinary" graves, one of the most important tasks for Xiongnu and more broadly "steppe" archaeology is to learn more about settlement sites, some of them being fortified enclosures of substantial size.[7] It is very likely that such exploration will flesh out the growing consensus that "nomadic" polities in fact were mixed socio-economic entities. So archaeology may well support certain of Beckwith's assertions but not necessarily produce the evidence needed to support all of his many hypotheses.

Ever since a pioneering article of his published in 1984, Beckwith's comitatus hypothesis relating to the military elites in the early period of the Abbasid Caliphate has attracted considerable attention.[8] Indeed, there is extensive evidence for the rewriting of the early history of the institution of so-called "slave soldiers," analyzed more fully now by other major scholars, notably Peter Golden and Étienne de la Vaissière, whose works he cites.[9] La Vaissière's recent book is of particular interest here for its careful delineation of the different forms in which the Central Asian presence in the Abbasid armies appeared and the change over time which eventually resulted in the creation of mamluk units in the form that then exercised profound influence in the subsequent political history of the Middle East.[10] His is a serious effort to write social history which goes well

beyond a concern over institutional form and ethnic origins. While there seems to be general agreement that some of the features of such elite guards can be traced further back in Central Eurasian history, for the most part Golden and La Vaissière make no effort to find in these institutions and cultural practices a kind of overarching theme to inform us of millennial-long features of Central Eurasian culture. The question we are left with then is whether there is in fact a documentable larger significance to the military presence of "Central Eurasians" in the *longue durée* across Eurasia. Should this somehow reshape our basic understanding of how rulers and their regimes operated (where any ruler is going to need some kind of guard corps of loyal retainers), or is this not merely another kind of modern or post-modern identification of roots which are too far separated in time and space and too diverse to have much meaning?

Indeed it is not quite that simple in Beckwith's telling, as we learn when we examine other parts of what constitutes his CECC. For what is involved here is not merely political organization and ethos but economic goals and rewards. The Central Eurasians acquired a great deal of wealth, most of which came allegedly not from seizing booty, but from systematic cultivation of trade. These are not just warriors, but "warrior-merchants" whose main goal was to be able to trade peacefully and who went to war against neighboring polities only when provoked to do so by the fact that those neighbors had erected barriers to peaceful trade. Such a picture is a salutary antidote to what indeed (but far from as uniformly as Beckwith insists) has been a depiction of the Central Eurasians as predatory barbarians. However, there is a real danger here of substituting one untenable generalization for another, where the truth may lie somewhere in between. As Beckwith himself chronicles often with numbing detail, there are plenty of conflicts both among Central Eurasians and between them and the states on the periphery. In a situation where the very nature of the political system (the leader and his comitatus) requires that there be rich rewards for the military elite, the leaders (or their at times disloyal subordinates) often resorted to intimidation, raiding, etc. There simply is not sufficient evidence to assert flatly that they always preferred a peaceful settlement whose main goal was to keep open markets. And even if we had evidence that the Central Eurasians occupied the moral high ground in such matters, the fact is still there that intimidation and any other tactics which might produce the desired result were being used. Apart from whatever is specific here to Central Eurasia, this subject is one which might well be approached from a different standpoint within the context of debates which go way back historically regarding what constitutes a just war and whether war for any reason whatsoever can be considered justifiable.

Beckwith is, of course, quite right, as others before him have pointed out, that in such matters the Central Eurasians were not necessarily much different from those ruling the peripheral states. For some time now, there have been cogent arguments as to why we should discard the traditional perception of the Great Wall's having been erected to keep the nomad barbarians at bay.[11] If we are wanting to point the finger at the Chinese rulers for their aggressive incursions into the territories occupied by their northern neighbors, we need not confine ourselves to the events of two millennia ago but indeed should also remember the tragedy that ended with the massacre of the Kalmyks in the middle of the eighteenth century.[12] However, such readily documented examples do not place the onus entirely on the peripheral states or leave the Central Eurasians holding the high ground even as they were being pushed at times to near extinction. In making the case for them, Beckwith cannot really resolve the contradiction between reinterpreting them and their goals as essentially good (yes, they were not barbarians) and at the same time saying that all parties were more or less the same. Indeed it seems odd to find him equating the methods used by the Europeans to establish their dominance in Asian ports beginning in the late 15th century with the methods used by the Central Eurasians ("They sometimes found it necessary to force the Asian rulers to follow the rules of peaceful diplomatic and commercial relations" [p. 217]). It appears that this was just fine if those being forced to submit were corrupt or obstructionist.

For me the most interesting issue here is whether we can learn something new about Central Eurasian economies and in particular the overland trade and the relationship between it and the maritime trade between East and West. While he undoubtedly overstates the comparison, there is something to be said for his insistence that "the socioeconomic structure of Central Eurasian empires was not significantly different from that of peripheral cultures, which had three main components: urban, rural suburban ...and rural..."(p. 342). In his view "pastoral nomads are essentially just farmers of 'crops on the hoof'" with, however, a "different ethnolinguistic identity" from that of the urbanites and proximal farmers. Hence, "there was no fundamental distinction between the economic and political structure of the empires ruled by people belonging to pastoral nomadic ethnic groups on the one hand and empires ruled by people belong to agricultural ethnic groups on the other."[13] Yet we might well raise

questions about how useful this sweeping "structural" characterization is, given the substantial differences in the relative weight of the indicated components and the wide variety of local conditions that may be contingent on local geographic and political considerations. As with the comitatus, this surely is a moving target, where ultimately the exceptions may vitiate the analytical value of the concept. At the very least, typologies of the various kinds of settlements, cities, etc. across Eurasia still need to be explored in detail.

On the face of it, Beckwith's ringing reaffirmation of the importance of the overland "Silk Road" trade may seem less controversial. There is, however, no serious effort in the book to try to understand the mechanisms of pre-modern economic exchange, analyze who the merchants were and how they operated, or to say enough about the maritime trade to clarify the relationship between it and the overland trade. However tempting it is to try to quantify which was more important, even if only in a relative sense and not with exact numbers, the evidence simply is not there. Nor do we have any basis to assert confidently that much more of the wealth of Central Eurasian elites came from trade than it did from booty, logical arguments in favor of that idea notwithstanding. We may never have enough data to enable us to write a serious economic history of Central Eurasia, even though ongoing discoveries by archaeologists in places such as the Turfan oasis are turning up revealing documents about economic life.[14]

There has for some time been a substantial literature on many aspects of the Indian Ocean trade, the economies of the Islamic states, the numismatic history of Eurasia, and so on. Yet Beckwith's book provides little evidence that much of that literature fell within his purview.[15] This does not mean, of course, that all his hypotheses are wrong—I have long felt as he does that Inner Asian regimes were indeed concerned very much about commerce. Certain sources such as Ata-Malik Juvayni's account about why the Mongols invaded Central Asia (which he cites) in fact support very nicely what Beckwith is arguing. He could, however, have done a much more convincing job of supporting his assertions. Moreover, he undermines our confidence in anything he says about littoral states anywhere when he proclaims *ex cathedra* that "although the Littoral routes had existed for some two millennia before the Europeans set out across the open ocean to Africa, Asia and the Americas, they were *politically* and *culturally* unimportant, and therefore barely noticed. It was only when the Europeans established trading posts and began reaping huge profits from international trade that the Littoral zone became truly significant" (p. 255, emphasis his).

In many ways the least well developed (and hence least satisfying) aspect of his CECC discussed by Beckwith concerns the aspects of culture which include "the arts" broadly speaking and "intellectual life." What he offers here really goes little beyond re-labeling certain rather well known figures or works as "Central Eurasian," some of it is of questionable accuracy, and he simply has not read enough about, e.g., art, to be passing judgments on whether or not other scholars have effectively discussed such subjects. Granted, it may seem offensive when writers or scholars of the Islamic period who happen to have been born in Central Eurasia are not labeled "Central Eurasian," but as we know, even though many of them wrote in places such as Bukhara or Ghazna, many left Central Asia and pursued their studies in Baghdad or other locations in the Islamic world. Their "Central Eurasian-ness" here is probably of no greater relevance than their "Persian-ness" or whatever label has been applied to them by other scholars. What is important is the languages individuals such as the mathematician Muhammad ibn Musa al-Khwarizmi knew, the books they read, wrote or translated, and their ideas which transcended any scheme imposed by later generations regarding ethnic or linguistic boundaries. No one has hidden the al-Khwarizmis under a bushel, even if there are not as many dissertations being written on him annually as there are on, heaven forbid, T. S. Eliot. On the face of it, Beckwith's singling out al-Khwarizmi might seem a bit odd, where there is not a word in the book about Mahmud al-Kashgari and Yusuf Khass Hajib, both from Kashgaria, who made contributions that arguably are of even greater importance for our understanding of Central Eurasian culture than is al-Khwarizmi's work. It is, however, hard to claim for them any formative influence on European culture. In that regard, al-Khwarizmi better suits Beckwith's rhetorical purpose. To insist that certain eminent figures of early Islamic learning must be touted as representatives of the CECC is more of a "modernist" distortion than would be to insist simply that they be treated as a part of Islamic culture.

Moreover, while modern nationalism or religious prejudices can certainly be blamed for many distortions of this history, Beckwith's sweeping indictment of Islamicists for their supposed "strong opposition...to any suggestion of Central Asian or Indian influence on Islamic civilization during the formative period" is quite simply wrong and very misleading where he never clarifies what exactly "the formative period" is. There is ample evidence that the Indian

contributions have been recognized for late 8th-century Baghdad, even though, as serious scholars of Islamic learning then indicate, they were largely displaced by Greek learning subsequently.[16]

Another example of his cavalier dismissal of scholars who to his mind should have written something different on the arts and intellectual life of Central Eurasians reveals at best that Beckwith is shooting at the wrong target. He takes Tibetologists to task (p. 231 n. 85) for never having paid any attention to the stunning painted thangkas of Tibet; yet, other than an Indiana University M. A. thesis relating to them, he seems not to have consulted any major works on Eurasian art history, many of which in fact include significant appreciations of the visual arts of Tibet. Do art historians fall into the category of "academic dilettantes and charlatans" whom, in response to his critics of his earlier book on the Tibetan empire, he condemned as not worthy of mention by name for fear that their views might thereby be given credence?[17] I think the real issue is a much broader one involving the narrowness of scholarly disciplines, not any conscious effort to diminish Central Eurasian artistic achievements. If "Tibetologists" who focus on "other things" such as language and text do not write seriously about Tibetan art, that should hardly come as a surprise. There is little evidence here that Beckwith, who is, inter alia, a Tibetanist, is an exception to that generalization, even if, unlike some of his colleagues, he professes an interest in aesthetics (not the same, of course, as art history).

Despite a recognition in his introduction that modern scholars have in fact contributed a lot to correcting misconceptions about Central Eurasians, on page after page throughout the book the author insists that practically no one else has gotten the facts or the larger story right. Even where he uses extensively other scholarly work (apropriately, among his sources are works by Thomas Allsen, Nicola Di Cosmo and Peter Perdue), he cannot resist diminishing it with niggling criticisms, not all of which are justified. Time and again we get the impression that he brings up certain issues not because they have any particular importance for his larger argument, but simply because he wants to seize the opportunity to criticize someone else who has written about them. One may hope he is right, but perhaps what we are seeing here is one of the excesses of the Modernism he condemns and which in his definition "means...continuous rejection of the traditional or immediately preceding political, social, artistic and intellectual order" (p. 289).

Now he has erected some defenses, a kind of Small Wall if you wish, to deflect criticisms of the selectivity of his citations and his penchant for digressions. We should certainly agree that anyone tackling a subject like his cannot be expected to read everything. My problems are not so much with the inevitable gaps in the bibliography (a few of which I have noted) or in some instances his reliance on now very dated scholarship—one might always wish for some different choices—but with his penchant for launching bold opinions that seem to be based less on any reading at all and more on pre-conceived opinions. Citation and "proof" are often uneven and/or self-referential[18]—a "generally accepted view" may be in the first instance an argument in buried in another of Beckwith's own works.

Furthermore, even though he is perfectly frank about the very real practical considerations involved, especially those parts of the book which deal with Central Eurasia in early modern and modern times rely largely on a few general surveys, encyclopedia articles, textbooks or chronological handbooks. The result then is by and large not misinformation, but a kind of summary cataloguing that is of little interest to read and bears at most a tenuous relationship to the threads of the argument being presented in the earlier portions of the book. The real problem here, I think, is that he elected to try to say a bit about "everything" in these sections of the book but with the predictable result being he says very little about anything, and none of it is very well connected. The book would have been better had he stopped before modern times, but of course for his polemical purposes, he was obliged to cobble together something on the modern world.

As far as the digressions go, he himself admits with disarming frankness that while he had intended to write a book with minimal footnoting, he got somewhat carried away: "Habits are difficult to repress, and apparently I *like* notes that go into detail on interesting topics" (p. viii, emphasis his). Indeed, he is quite self-indulgent in this regard. The result is a book with two substantial appendices and both footnotes and endnotes. While he professes that the endnotes are for matters of interest only to specialists, in fact there is no clear distinction between the density or importance of material in each set, or, for that matter, between the notes and some parts of the main text. Some of the endnotes, often very interesting mini-essays, arguably deserve a place in the main text, some of the footnotes would better be relegated to the end along with some of the main text. In the circumstances, a single set of notes certainly should have sufficed, and a good many of the "interesting topics," some merely trivial and distracting, would better have been passed over in silence.

The editors at Princeton certainly did not earn their pay when this book crossed their desks, even if the version we have in print is substantially different from that which they first received. Moreover, it is too bad that they did not see fit to insist on more maps than the two (Premodern Central Eurasia and Modern Eurasia) which occupy the endpapers. It should have been possible to devise more effective graphic means to illustrate Central Eurasia as a cultural concept and how its borders changed. These purely geographic representations in the two maps each cover the same territory, even if political borders and place names have changed. Hence, no "Central Eurasia" in the modern world, the designation there of Xinjiang as "East Turkistan (occupied)," and similar indications for Tibet and Inner Mongolia. For a book that professes to have an interest in the wonderful accomplishments of Central Eurasians in, among other things, the visual arts, it comes as a surprise that there is not a single illustration, nor can we see here any of the artifacts of material culture which would support arguments about the complexity of the socio-economic structures.

One of the mystifications of this book is what the author and publisher imagined might be its audience. Until I perused some internet blogs, I assumed there was no way it would attract a general audience, given the arcane nature of a lot of the detail, the welter of unfamiliar names and the organizational problems. Well, I was wrong. There do seem to be readers, although how many and whether they are part of the "general public" or rather individuals with specialized knowledge on at least some parts of the material is not clear. Some readers seem to be following the good suggestion by Victor Mair in his dust jacket blurb that the Epilogue entitled "The Barbarians" merits particular attention; others have decided to skip the diatribes on Modernism. Whether this means the book is going to have the kind of impact the author hopes is another matter. Some of the blog traffic is already wandering off into arguments about whether it is appropriate to characterize Beckwith's tastes in music as "prejudices."

It is just possible that readers come to the book for reasons that would, understandably, appall the author. Perhaps they find in this work, written with ostensibly serious intent and invoking rare erudition, the kind of provocative and self-indulgent pontificating that is characteristic of modern talk radio, which in its turn provokes listeners to respond with equally self-indulgent pontificating. It would be ironic if another possible audience of those who might applaud at least some aspects of the book (and take pleasure in throwing bricks at others) turned out to be the adherents of the post-colonial theories which the author so despises that he does not even read them. Arguably there is much here which could be subsumed under any labels such as postmodernist, postcolonialist or whatever, even if what is important is never the labels but the substance of what is being said.

Apart from the bloggers and potential politically motivated commentators, there is at least some hope serious scholars and the graduate students they are training will slog through this volume, weigh in with responses to Beckwith and perhaps even take up some of his challenges to explore areas in which we unquestionably need more research. His laudable accomplishment is to provoke us to re-think a lot, even if far from all of what he suggests is quite as new as he would have it. It would be a shame if the book's flaws diverted attention away from the forcefulness of his argument that Central Eurasians, their history, and culture deserve our serious attention, this whether or not we would agree with the inflated claims he makes for them in the conclusion to the main part of his book:

> The earliest of the great civilizations known from archaeology—the Nile, Mesopotamian, Indus, and Yellow River valley cultures—were born in the fertile agricultural periphery of Eurasia. But modern world culture does not derive from them. It comes from the challenging marginal lands of Central Eurasia.
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> The dynamic, restless Proto-Indo-Europeans whose culture was born there migrated across and "discovered" the Old World, mixing with the local peoples and founding the Classical civilizations of the Greeks and Romans, Iranians, Indians, and Chinese. In the Middle Ages and Renaissance their descendants and other Central Eurasian peoples conquered, discovered, investigated, and explored some more, creating new world systems, the high arts, and the advanced sciences. Central Eurasians—not the Egyptians, Sumerians, and so on—are our ancestors. Central Eurasia is our homeland, the place where our civilization started [pp. 318-19].

This paean to the Central Eurasians is of a kind with the image chosen for the dust jacket of the book, a highly romanticized painting (courtesy of National Geographic) of what we must assume are two of the "Scythian" or Saka elite. Somewhat improbably one might think, since their purpose seems to be overseeing the herding of their horses, they have donned their burial suits of golden armor. They have Caucasoid features with Hollywood looks: the man on a white horse resembling, say, an Errol Flynn and the woman perhaps a Julia Roberts. Ironically, there is nothing in that image to suggest that Central Eurasians were more than nomads who had expensive tastes.

To quote Peter Golden again, this is indeed a book "quite unlike any other."

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NOTES

[1] Christopher I. Beckwith, *The Tibetan Empire in Central Asia: A History of the Struggle for Great Power among Tibetans, Turks, Arabs, and Chinese during the Early Middle Ages* (Princeton: Princeton University Press, 1993; first ed. 1987).

[2] Walter Goffart, *Barbarian Tides: The Migration Age and the Later Roman Empire* (Philadephia: University of Pennsylvania Press, 2006), 221. Goffart's point here is specific to his trenchant critique of the way undifferentiated "Germanic" migrations have been invoked by German nationalists as a means of extending their roots into remote antiquity.

[3] Here I have in mind, for example, a book which Beckwith notes he received too late to use, David W. Anthony, *The Horse, the Wheel and Language: How Bronze Age Riders from the Eurasian Steppes Shaped the Modern World* (Princeton and Oxford: Princeton University Press, 2007). Anthony provides in Ch. 6 a lucid discussion of the changing views of archaeologists regarding migration. The way the subject is now treated is quite different from the views of those who traditionally have written about the Great Migration. I cannot know, of course, what Anthony thinks about the book under review here. A stimulating revision of ideas about migration and the mechanisms of cultural interaction amongst the early Central Eurasians is in a book Beckwith could not have seen, Michael Frachetti, *Pastoralist Landscapes and Social Interactions in Bronze Age Eurasia* (Berkeley etc.: University of California Press, 2008), which builds on other studies of pastoralist societies in early Eurasia going back at least a decade. At very least, when it comes to the spread of something like metallurgy or chariot technology, we need to consider carefully whether this may not best be explained by mechanisms of diffusion other than migrations or any substantial movement of "experts." Goffart states rather bluntly in his text-based revision of ideas about early Germanic migrations: "The appeal to 'migration' as an actor in history masks our ignorance, bridges gaps in our knowledge, and imparts ostensibly scientific seriousness to empty guesses" (*Barbarian Tides*, 117).

[4] Note, however, that he does use the excellent work of Sophia-Karin Psarras published in several long articles in *Monumenta Serica*, and cites frequently Nicola di Cosmo, *Ancient China and its Enemies: The Rise of Nomadic Power in East Asian History* (Cambridge: Cambridge University Press, 2002), both of whom rely extensively on archaeological evidence.

[5] S. S. Miniaev and L. M. Sakharovskaia, "Soprovoditel'nye zakhoroneniia 'tsarskogo' kompleksa No. 7 v mogil'nike Tsaram," *Arkheologicheskie vesti* (St. Petersburg) 9 (2002): 86-118.

[6] Bryan K. Miller, Francis Allard, Diimaajav Erdenebaatar and Christine Lee, "A Xiongnu Tomb Complex: Excavations at Gol Mod 2 cemetery, Mongolia (2002-2005)," *Mongolian Journal of Anthropology, Archaeology and Ethnology*

(Ulaanbaatar) 2, 2 (2006): 1-21. See also Jessica Jones and Veronica Joseph, "Excavation of a Xiongnu Satellite Burial," *The Silk Road* 5, 2 (2008): 36-41.

[7] Beckwith cites an important case of Gelonus, a city at the edge of the steppe in the Scythian areas north of the Black Sea. One of Di Cosmo's emphases is on the mixed nature of the socio-economic formations on the northern borders of China. At least one Xiongnu settlement, with fixed houses and a fortified wall, has been thoroughly investigated, and some work has begun on others. The Mongol capital Karakorum is receiving increasing attention from archaeologists, but the impressive Uighur city of Kharbalgas just to the north of it in the Orkhon River valley is still very much in need of study despite some recent excavation and survey work. For some of the Xiongnu examples, see A. V. Davydova, "The Ivolga Gorodishche (A Monument of the Hsiung-nu Culture in the Trans-Baikal Region)," *Acta Archaeologica Academiae Scientiarum Hungaricae* 20 (1988): 209-245 (this excavation was first reported in 1956; she has also published a fuller report as a separate book in Russian); A.V. Davydova and S. S. Miniaev, *Kompleks arkheologicheskikh pamiatnikov u sela Dureny* (St. Petersburg: Fond "AziatIKA," 2003); David E. Purcell and Kimberly C. Spurr, "Archaeological Investigations of Xiongnu Sites in the Tamir River Valley: Results of the 2005 Joint American-Mongolian Expedition to *Tamiryn Ulaan Khoshuu*, Ogii nuur, Arkhangai aimag, Mongolia," *The Silk Road* 4, 1 (2006), esp. 27-31. For a survey of settlement sites from various periods in Mongolia, see J. D. Rogers et al., "Urban centres and the emergence of empires in Eastern Inner Asia," *Antiquity* 79 (306) (2005): 801-18. The pioneering volume by S. V. Kiselev et al., *Drevnemongol'skie goroda* (Moscow: Nauka, 1965) retains its importance even if the archaeological methods employed in the excavations were sloppy. We look forward to the appearance of the first major volume of reports on the ongoing Mongolian-German excavations at Karakorum, Jan Bemmann et al., eds., *Mongolian-German Karakorum Expedition Volume 1: Excavations in the Craftsmen Quarter at the Main Road* (= Forschungen zur Archäologie Aussereuropäischer Kulturen, Bd. 8) (Wiesbaden: Reichert Verlag, 2009). There is a good overview of a range of topics reflecting recent study of Karakorum and its environs in *Dschingis Khan und seine Erben: Das Weltreich der Mongolen* (München: Hiermer Verlag, 2005), 128-95, with illustration and details about many of the artifacts found there.

[8] The article is "Aspects of the Early History of the Central Asian Guard Corps in Islam," *Archivum Eurasiae Medii Aevi* 4 (1984): 29-43.

[9] Golden's articles include "Some Notes on the Comitatus in Medieval Eurasia with Special Reference to the Khazars," *Russian History/Histoire Russe* 28 (2001): 153-70; "Khazar Turkic ghulams in Caliphal Service," *Journal Asiatique* 292, 1-2 (2004): 279-309.

[10] *Samarcande et Samarra. Élites d'Asie Centrale dans l'empire Abbasside* (= Studia Iranica Cahier 35) (Paris: Association pour l'avancement des études iraniennes, 2007).

[11] Notably, in the books by Arthur Waldron, *The Great Wall of China: From History to Myth* (Cambridge: Cambridge University Press, 1990) and Nicola di Cosmo, *Ancient China*. For a good summary of Great Wall history and its interpretation, see the latter's "The Origins of the Great Wall," *The Silk Road* 4, 1 (2006): 14-19.

[12] This is now best explored in Peter C. Perdue's impressive book *China Marches West: The Qing Conquest of Central Eurasia* (Cambridge, Mass., and London: Harvard University Press, 2005), on which Beckwith relies extensively.

[13] He gives credit for these views to the prominent specialist on the Ghaznavids C. E. Bosworth, writing in the *Cambridge History of Iran* in 1968. That archaeologists and anthropologists working on mobile pastoralists (nomads) have for some time recognized social and economic complexity can be seen from the stimulating essays in Hans Barnard and Willeke Wendrich, eds., *The Archaeology of Mobility. Old World and New World Nomadism* (University of California, Los Angeles: Cotsen Institute of Archaeology, 2008).

[14] Presumably we will have a substantial account of this material when Valerie Hansen's long-awaited book, *A New History of the Silk Road*, finally appears. But short of that, there are a good many articles by her and others which have been out long enough to have been consulted by Beckwith. One can access some of her work and find guides to other material at her website http://www.yale.edu/history/faculty/hansen.html.

[15] This makes the more puzzling such comments as his sweeping assertion that economic historians have "dismissed" the trade in luxury goods as a way of arguing that the overland trade was less important than that of the littoral states (p. 216 and endnote 87). Regarding the literature relevant to the subject of trade, of course there is much that Beckwith did use, but the list seems quite uneven and it is not always clear that his interest in the works cited is for what they say about economic issues. Thus, for example, we do find Thomas Noonan's work on East European dirham hoards and on the Khazar economy, Rudolph Matthee on the Safavid silk trade, important work by Scott Levi on Indian-Central Asian trade and several articles in a very recent collection of essays Levi edited. It seems odd that Beckwith made no use of the several interesting articles by Morris Rossabi on the Inner Asian trade under the Ming.

[16] Here see his bizarre endnote 76, where he cites a phrase "the Indian Half-Century of Islam" from a source he cannot remember (and specialists he has consulted cannot identify) to suggest that one scholar did get it right but his views have been deliberately deep-sixed by all the other "Islamicists." For a succinct summary recognizing the importance of the Indian influences at least in certain of the sciences, see Kim Plofker, "Mathematics in India," in *Mathematics in Egypt, Mesopotamia, China, India and Islam*, ed. Victor Katz (Princeton: Princeton University Press, 2007), 434-35. Additional discussion of the matter of Indian and in particular possible Buddhist influences on the Islamic world is in Beckwith's endnotes 77 and 78, where one might insert a small corrective to his assertion that the Arabs had "invaded and subjugated much of Central Asia by the late seventh century." Invaded yes, but in fact subjugation took substantially longer.

[17] Beckwith, *Tibetan Empire*, 215.

[18] An example is in endnote 79 (p. 414) regarding the wrong ideas some have about Bon being a pre-Buddhist Tibetan religion. We do not need to wait for "Beckwith (forthcoming-c)," when we can readily find an intelligent and presumably accurate discussion in Donald S. Lopez, Jr. in his introduction to his edited volume *Religions of Tibet in Practice* (Princeton: Princeton University Press, 1997), 28-30. The potential dangers of self-referential "evidence" are apparent if it turns out that some of his own work to which Beckwith sends his

readers is problematic. Others will need to judge whether his frequently cited work dealing with historical linguistics has merit. See the critical comments in reviews by Thomas Pellard, in *Korean Studies* 29 (2006): 167-70, and by Scott DeLancey in *Himalayan Linguistics Review* 5 (2008), esp. 3-5, where among the points raised are Beckwith's penchant for strings of "dismissive statements with no actual facts" and his opaqueness about his methodologies, which differ from those accepted by others in the field.