RESPONSE TO A CRITIC

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At the invitation of the current Editor of *Early China* I have accepted to write a response to Sophia-Karin Psarras's review of my book, published in 2002, *Ancient China and Its Enemies, The Rise of Nomadic Power in East Asian History* (*Early China* 31, 2007: 227–259, published in June 2010).

As a frequent book reviewer myself and former book review editor, I do believe that reviews have an important role in academia, but it is a role sometime abused, and something needs to be said when the credibility of a book and its author is treated in a manner that, by any standard, can only be regarded as prosecutorial. A point-by-point rebuttal would be a tedious and unnecessary exercise, and therefore I shall address only a few illustrative issues.

On p. 231 I am accused of appropriating Lin Yun's language and of having taken terms from him without proper attribution (implying some kind of plagiarism). But in the passage that immediately precedes the incriminating excerpts, I say: "The standard typology of the Northern Zone's complex metal inventory, *provided by Lin Yun* [my emphasis] includes daggers, axes with short sockets, axes with tubular sockets, mirrors, and 'bow shaped' objects." Did the reviewer miss this?

On p. 255 the reviewer claims that I "add" a sentence on "traditional Inner Asian society" to another sentence on "the 'state' among the Hsiung-nu," and the resulting argument apparently confuses her. But in my book the two sentences come in an order exactly inverse to what she alleges. Readers of the book who won't engage in constructing non-existing sentences and meanings hopefully will not be similarly confused.

As for the accusation that I do not provide references (pp. 255–256), one may hope that a better (or simply unbiased) reader will have the patience to reach the end of the sentence, where, as customary, references are placed (in this case note 32).

The review is replete with bizarre or simply uninformed claims. For instance, I am faulted for not providing "casualty statistics" from the Han-Xiongnu wars; and yet, as anyone familiar with pre-modern sources knows, taking the figures that appear in Han texts as "casualty statistics" at face value would be evidence of a jejune approach to historical analysis.

Still on p. 255 the statement according to which my point that "kin bonds, reliance on customary law, and segmentation of political power among the clans and tribes of pastoral societies prevented the emergence

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of any absolute, indivisible, and legally recognized authority that would be the expression of a given political community in its entirety" could "apply to China with equal validity" just goes to show the reviewer's profound lack of understanding of the fundamental differences between the Inner Asian and Chinese political systems.

On p. 257 I am faulted for having "truncated" the history of Xiongnu-Han relations, but one does not need to go very far beyond the title to discover that my book is about "the rise of nomadic power in East Asia," not the full history of Han-Xiongnu relations. Since the reviewer never explains what the book is actually about, this leaves her free to condemn it for not being what it was never intended to be.

On p. 254 the reviewer says that "virtually none of the discussion of the structure of the Xiongnu state (pp. 177–186) has anything to do with the Xiongnu" but neglects to say that the section to which she refers (actually pp. 178–186) is titled "The Formation of the Hsiung-nu State in Historical Perspective" (and, thus, historical precedents of nomadic states) and is followed by another (pp. 186–190) called "State Formation among the Hsiung-nu" which has everything to do with the Xiongnu.

Such spotty reading is coupled with extremely convoluted passages. On p. 235 the reviewer accuses Chernykh (or is it me?) of "lack of familiarity with Shang artifacts" (p. 235) in a way that makes it unclear whom she is arguing against. While at it, the reviewer can't help criticizing Grjaznov as well (same page, note 20).

To misconstructions, selective reading, and fanciful representations of my arguments one must add a consistent failure to check her facts properly, as when she attacks my statement about the existence of a connection between the Seima-Turbino phenomenon and the Andronovo people (p. 234). Some time has passed since the publication of my book and today even more scholarship is available for an informed judgment. In the recently published *The Horse, The Wheel, and Language* David Anthony states: "by the beginning of the Andronovo period some Seima-Turbino metal types . . . became widely popular in Andronovo communities" (pp. 447–448), supporting my point.

Attempting to demolish what I write about the Han use of cavalry, the reviewer finds contradictions in my argument: "How, then—thunders she on p. 237—can the Han use of cavalry against the Xiongnu be attributed to Chao Cuo when his memorial [of 169 BC] post-dates such use?" (which I posit as 178 BC). But on the same page, right after the sentence she quotes, I say that Chao Cuo's 169 BC memorial "suggests that, even if the building of a cavalry was underway, it had not yet been sufficiently developed." Later on (p. 238) she continues to insist that, "as with the discussion of Chao Cuo," I "fail to see the conflict created by those dates."

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But such a conflict was entirely dreamed up by the reviewer. The additional conflict I failed to note, by the way, points to yet another mistake by the reviewer, who seems to "misunderstand" quotations and dates.

But aside from all the fabrications and misconstructions, it is even more worrying that the reviewer seems not only unconcerned about conveying what the book is about, but simply unable to grasp certain basic issues, such as what is a "theory," or the difference between an archaeological culture and a historical phenomenon.

Whenever the reviewer is unable to appreciate or follow a deductive argument or a logical association it is claimed that I make up theories, which to her are the same as "inventions." On p. 253 she impugns my reasoning based on her own interpretation of a passage, and then says that my "theory" does not "serve" the "historic text." At best, whatever the "theory" might be, it would only serve her subjective interpretation of the text; in any case, theories normally do not "serve" historic texts.

Finally, a few words ought to be spent on the question of "Xiongnu culture," a topic that appears to be especially aggravating to the reviewer. Her meandering around the issue of what I "imply" or say or mean when I speak of Xiongnu culture (pp. 247–250) could be easily resolved by an unbiased reading of what I write. Leaving aside the fact that what I say on pp. 80–81 is as usual distorted through a creative mixing of passages from p. 81 and p. 151, I state there that "the term 'Hsiung-nu culture' can be used in archaeology only as an umbrella term synonymous with 'early nomadic culture,' and not in reference to a particular ethnic group or political community" (pp. 81–82). In other words, I do not believe that ethnicity and material culture are necessarily correlated, nor that it is possible to attribute discrete archaeological cultures to specific peoples mentioned in the Chinese sources, and then derive historical arguments from that association. By such a logic, if an iron object were found in a tomb attributed to a "Xiongnu culture," thus making iron metallurgy a "Xiongnu feature," that very "feature" could then be used to explain some aspect of Xiongnu history, such as their political and military hegemony. And yet, there would be nothing to link that iron object to the historical Xiongnu other than someone's arbitrary decision to associate an artificially created "Xiongnu culture" with an historically documented "Xiongnu people." One may or may not agree with me, but when Psarras says that I "seek to eliminate Xiongnu culture conceptually" but have "no substitute to offer" (p. 250), she just shows her inability to grasp the very nature of the problem.

The reviewer is "curious" (p. 250) to know how I would classify the finds from Noin Ula. I would then recommend reading the entry in the Grove Dictionary of Art that I wrote twenty years ago. This can be easily

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accessed by searching for "Noin Ula" on the website of Oxford Art Online. As I state there, those are Xiongnu *aristocratic* tombs. As such, they cannot be indicative of whatever "Xiongnu culture" one may imagine to have existed: they are the expression of the prestige and power of the upper echelons of a nomadic society, and many precious artifacts are, not surprisingly, of Chinese origin. All of this has since been amply confirmed by recent archaeological excavations in the republic of Mongolia. Lacking an appreciation of the formation of the Xiongnu as a historical phenomenon, of its socio-political evolution, and of the problematic relationship between ethnicity and material culture, Psarras's insistence on finding a "Xiongnu culture" at Noin Ula is simply meaningless.

In the end, beyond Psarras's ill-advised aspersions, there are some serious questions to raise: what are the merits of reviews that do not engage or even begin to address the main theses and arguments of a book? What is the use of a "review" whose only goal is to confound and mislead the reader? I am not the one to answer these questions, but the final impression, as far as this review is concerned, is one of embarrassing futility.

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