

**Comments on
Stefan Zimmer, "Modern Necromancy,
or How to Make Mummies Speak"**

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It is regrettable that Stefan Zimmer (hereafter SZ) declined to participate in the International Conference on the Bronze Age and Iron Age Peoples of Eastern Central Asia held at the University of Pennsylvania in Philadelphia from April 19-21, 1996.¹ Had he done so, he surely would have learned a tremendous amount about the ancient populations of the Tarim Basin and surrounding areas, and consequently would probably not have written such an ill-informed and misguided piece as that which he has sardonically entitled "Modern Necromancy." Instead, he will have to obtain a copy of the joint publication of the conference volumes by the University of Pennsylvania Museum of Archaeology and Anthropology and the *Journal of Indo-European Studies* monograph series to read the papers of the distinguished scholars who were in attendance.²

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¹I sent invitation letters to SZ on October 3 and October 19, 1995 from Kyoto. He did respond noncommittally to the first letter, but I received no answer to my second letter encouraging him to come. Later, several sources informed me that SZ had expressed negative feelings about the conference even before he knew who would be participating.

²Contrary to SZ's statement in section III, none of the papers he reviewed were presented at the Philadelphia conference. Among the contributors to the conference volume are Douglas Adams, David Anthony, Elizabeth J. W. Barber, E. Bruce Brooks, Emma Bunker, Chien-wcn Chen, Tzehuey Chiou-Peng, John Colaruso, Jeannine Davis-Kimball, Harold Fleming, Paolo Francalacci, Dru Gladney, Irene Good, Eric Hamp, Han Kangxin, He Dexiu, Fredrik Hiebert, Kenneth Hsü, Juha Janhunen, Karl Jettmar, Dolkun Kamberi, Elena Kuzmina, Katheryn Linduff, Lin Meicun, C. Scott Littleton, Alexander Lubotsky, Victor Mair, James Patrick Mallory, Mei Jianjun, Colin Shell, Asko Parpolo, Ke Peng, Georges-Jean Pinault, Michael Puett, Colin

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When he does so, he will be able to correct the numerous mistakes he has committed in his "Modern Necromancy."

Although much of what I have to say in the following paragraphs will reveal my disappointment with the dismal quality of SZ's review, I wish to preface my remarks by saying that I do appreciate the amount of time that he clearly invested in reading through the entire collection of papers³ guest edited by me and in writing down his ideas about each of them. It is unfortunate, however, that his attitude is so uniformly and one-sidedly negative, for this has caused him to commit a host of horrendous errors. Above all, he has revealed that he does not comprehend the archeological context of the ancient mummies of Eastern Central Asia nor the nature of the interdisciplinary research program devoted to them.

I would much prefer not to have to respond to SZ's diatribe, but it is so laden with inaccuracies and indignities that it is my duty as the Guest Editor of the collection which is the object of his abuse to put things in perspective. I shall not demean myself, the other contributors, and the *Journal* by stooping to the level of unfounded innuendo and aspersive invective engaged in by SZ. Nor shall I take the time to iterate the mountain of fallacies he has perpetrated. Instead, I shall simply point out a few representative errors committed by SZ and make some general comments about the nature of interdisciplinary research on the mummies.

In section IV, SZ raises the ugly specter of a certain "-ism." This is a very serious charge, one that behooves him to demonstrate precisely how any of the contributors to the collection have been guilty of it. If SZ cannot do so, this type of accusation is baseless, malicious mudslinging of the most shameful kind.

SZ repeatedly, snidely satirizes me for my enthusiasm. Perhaps SZ has never organized a large-scale, long-term international research project, so he is not familiar with the levels of energy and effort necessary to sustain such an undertaking. Suffice it to say that, without a considerable amount of excitement and enthusiasm, nothing would get

Renfrew, Donald Ringe (with Tandy Warnow, Ann Taylor, Alexander Michailov, and Libby Levinson), Natalia Shishlina, Shui Tao, Denis Sinor, Kevin Tuite, Wang Penglin, William S-Y. Wang, Werner Winter, and Tongmao Zhao.

³Mair, ed., "Mummified Remains."

done. Therefore, although SZ clearly intended to disparage me for being enthusiastic, I take his denigration as a great compliment. I am not even offended that he sneers at me (he is "more than once ... embarrassed") for showing "unrestrained enthusiasm"! My colleagues, students, and audiences before whom I lecture consistently praise me for my enthusiasm, so I see no reason to be ashamed or to apologize for it. Of course, if my enthusiasm has distorted my scholarship and sensibility, I would be chagrined. Since, however, SZ does not specify any instances where it has adversely affected my scholarly judgement, his objections to my animation are nullified.

SZ is astonished that I do not flaunt my achievements in Chinese studies. I did not think that was necessary in a paper introducing the Bronze Age and Iron Age mummies of the Tarim Basin and surrounding areas. There is no sense padding one's bibliography with one's own writings when they are not relevant to the task at hand.

SZ ridicules (and simultaneously distorts by manipulating and quoting out of context) my allusions to the mummies (desiccated corpses) as being, in a sense, the ancestors of various historic and contemporary populations. If SZ were ever to travel to Eastern Central Asia and observe the preserved human remains found there firsthand, he would realize how lifelike they are. In spite of the fact that the individuals he would see there with his own eyes are undeniably dead now, they once were living and they most assuredly were the ancestors of later populations. Thus it is both pedantic (cf. SZ's footnote 4) and otiose to call attention to the fact that the corpses/mummies are dead bodies. The vast majority of all ancestors who once lived on the face of the earth are now dead.

Since English is not SZ's mother tongue, it would not be fair for me to remark on infelicities in his usage. On the other hand, he should refrain from censoring others, particularly when his command of English is insufficient to grasp the nuances of an idiomatic expression. SZ criticizes Xu Wenkan for asserting that the language of the ancient Tarim Basin Europoids "must have belonged to the Indo-European language family" and even emphasizes (with italics) the word "must." While Xu's assertion is debatable, SZ has misconstrued his intent. Xu did not mean "necessarily/certainly/indubitably belonged to the Indo-European language family" but

“probably/most likely belonged to the Indo-European language family.”⁴

SZ is fond of flippantly labeling the words of others as “bizarre,” “strange,” “weird,” “comic,” and so forth, but what are we to make of his attempt to censor E. J. W. Barber’s clever wording “weaver’s-eye view” (cf. “bird’s-eye view”), his seemingly deliberate misquotation of her generalized reference to “linguistic reconstruction,” objecting to the use of the word “enigma” and then himself employing the word “riddle” to characterize a similar phenomenon, and so forth? Such a crudely supercilious and highhanded manner of dealing with his colleagues’ work is unbecoming a scholar of dignity and worth.

In his discussion of J. P. Mallory’s paper, SZ mentions the famous “Pontische Wanderung” thesis of Robert Heine-Geldern. He dismisses it as being “commonly regarded as unlikely” and “too late for the mummies.” Neither objection, however, is sufficient to disqualify Heine-Geldern’s thesis which was supported by a mass of extremely detailed evidence. In the first place, even as recently as the 17th and 18th centuries, thousands of *trekboeren* in South Africa and settlers in North America covered lengthy distances within one generation in covered wagons and on foot, so we should not automatically rule out the possibility of comparable migrations in earlier times. Secondly, there are many Xinjiang sites with Europoid mummies or skeletal remains that date to the same period (9th-8th cc. BCE) which Heine-Geldern was referring to or even later. (Europoid human remains are found in cemeteries at Subeshi, Sampul, Charchan, Niya, Loulan [Kroraina], etc. dating to the Warring States, Han, Wei-Jin, and later periods, i.e., 5th c. BCE - 5th c. CE.) Indeed, the dating of the two sites which contain textiles and other artifacts that provide the strongest material support for Heine-Geldern’s thesis (Zaghunluq with its pipings and spiral designs and particularly Qaradöwä with its tartans and redheads) is now being revised downward on the basis of new C¹⁴ readings and a critical reanalysis of pottery types. The 8th century for Qaradöwä and the 7th century for Zaghunluq, in fact, are squarely in line with

⁴ *Cambridge International Dictionary of English*, p. 933a.

the dates proposed for these two sites by Wang Bo, Lü Enguo, and other excellent young archeologists working in Ürümchi.⁵

SZ takes Mallory to task for relying on “Pokorny’s clearly dated IEW (1953)! (sic).” Unless he can demonstrate precisely how Pokorny is inadequate for the purposes to which Mallory has put it, SZ’s criticism is both meaningless and nugatory. Of course, there are small problems here and there with Pokorny, but they are of an entirely different order from the monstrous ones, say, in Van Windekens, which even the most eminent Tocharianists are not beneath citing from time to time.⁶ Simply because a book was published in 1953 does not mean that it should be consigned to the trash heap. The splendid Tibetan-English dictionary of Csoma de Körös appeared in 1834, but it is still highly valued. Ditto for the monumental Sanskrit-German *Wörterbuch* of Böhtlingk and Roth (1855-1875), the classic Greek-English lexicon of Liddell and Scott (1843), and so forth. Pokorny’s achievement was enormous and, when used with customary caution, his etymological dictionary of Indo-European is extraordinarily useful. Until someone can produce a dictionary to supersede his *IEW*, they should refrain from taking cheap shots at it.

Speaking of petty pedantry, in footnote 5, SZ feels compelled to correct Xu Wenkan for having referred to one of our colleagues as “an English scholar Colin Renfrew” instead of as “The eminent Scottish archaeologist, Lord Renfrew.” Never mind that Professor Renfrew made it explicitly clear to me that, in academic affairs, he does not wish to be called by his noble title. And let us disregard the fact that Professor Renfrew has studied, taught, and lived in England for decades. All that aside, it is tedious for SZ to waste people’s precious time by dwelling on such insignificant matters.

⁵Like most archeological sites in Xinjiang, the cemeteries discussed here were used for a long time, thus it is natural to expect for each of them a range of dates spanning several centuries or more. Nonetheless, according to the most recent analyses of the artifacts from Qaradōwā and Zaghunluq, we may date the main (or typical) period of their use respectively to the 8th century BCE and the 7th century BCE, not to 1200 BCE and 1000 BCE as had been previously claimed.

⁶It is easy to complain about the scholarship of others but far more difficult to improve upon it. If one is dissatisfied with the available reference tools, one should do something about them. For this reason, I applaud Douglas Adams’ compilation of an etymological dictionary of Tocharian whose publication is eagerly awaited.

In general, SZ displays a cavalier lack of curiosity about what can be gleaned from archeology and physical anthropology, insisting upon the primacy of linguistic data for determining the origins and expansion of the Indo-Europeans. This is evident in his critiques of the papers by Irene Good, Paolo Francalacci, and others. Since the mummies left no record of their language(s), it goes without saying that their identity cannot be determined by linguistics alone.

SZ belittles Douglas Adams and Donald Ringe by declaring that they "are historical linguists working on Tocharian, but without published philological and historical experiences." In light of the fact that Adams and Ringe have authored seminal papers that are universally recognized as two of the most important evaluations on record of the place of Tocharian within the Indo-European language family, this is a remarkable statement for SZ to make. The best light that one can put on it is that he must have been sleeping on the other side of the Atlantic for the last 15 years. Furthermore, having sat in on Professor Ringe's classes on Tocharian at Penn, I can vouch for his knowledge of and respect for the works of Lambert Isabaert and Georges-Jean Pinault. The same holds true of Professor Adams who is far more of a Tocharianist than SZ.

While chiding Adams and Ringe, on the one hand, SZ begins his comments on the paper of Edwin G. Pulleyblank by stating that it is of "quite different weight," implying that it is of heavier substance. It is ironic, then, that SZ proceeds to denigrate almost everything in Pulleyblank's paper. It is doubly ironic that, in doing so, he promotes the idea of a "migration of the Tocharians from Europe to Xinjiang through alien territory" since, as we have seen, he pooh-pooed a shorter migration through less hostile territory in the case of Heine-Geldern's proposed "Pontische Wanderung." It is triply ironic that, in the next section (IX), he chastises Adams for being unaware of Tsung-tung Chang's research on Indo-European loans in Ancient Sinitic, since Pulleyblank is on record as having nothing but contempt for Chang's work.⁷

As for SZ's treatment of James Opie's paper, one may politely style it to be excessive. It would be interesting to learn what SZ thinks of the theories of "that eminent Iranian

⁷"Early Contacts," p. 23b note 4. In the same issue of the journal in which Pulleyblank's unprincipled attack appeared, I defended Chang. See my "Language and Script," p. 39ab.

philologist," W. B. Henning, concerning the Guti and the Tukri, other than that his controversial article on that subject was posthumously published. It would seem that, even in death, Henning's reputation is so awesome that SZ dares not malign him. With poor John Haskins, however, it is another matter altogether. No holds barred! SZ's ruthless derision notwithstanding, Haskin's huge dissertation deserves a more fairminded assessment. It is quite possible that, when evaluated with less mockery, it may yield important ideas and information concerning the identity of the elusive Massagetai and Thyssagetai. (What can SZ tell us about the language of the Getai who, according to him, were Thracians? Is SZ absolutely certain that "the Massagetai are scarcely related to the Getai"? Can we be sure that the Thyssagetai or Thrysagetai really were Scythians, as SZ intimates?) SZ asserts that "The inclusion of such nonsense⁸ in a learned journal is indeed a heavy blow for Mair's reputation." Quite the contrary, my reputation has nothing to fear from such ineffectual, unbalanced taps as those delivered by SZ against James Opie.

One could continue in this vein indefinitely, but it is unnecessary.⁹ At times, SZ's review borders on incoherence; this will be painfully obvious to anyone who reads through it from beginning to end and tries to figure out what his purpose is—aside from the distillation of pure vitriol. From what he has written, it is impossible to tell whether he is opposed to interdisciplinary research on ancient human remains and their associated artifacts or whether he simply does not comprehend it.

A theme that does come up more than once in SZ's review is his angst about "mixing linguistics with biology." Since both

⁸Due to the rambling and ranting nature of his pronouncements, it is somewhat difficult to determine exactly what SZ is condemning as "nonsense." It would seem that he is most exercised about Haskin's raising the possibility of a degree of affinity between the Guti and the Celts. In a forthcoming study of the role of the nomads in cultural transmission across Eurasia, I shall show that the blurring and blending of Celts, Goths, Scythians, and other "barbarians" is not so ridiculous as SZ pretends it to be. These complex matters are touched upon in such standard sources as Wolfram's *History of the Goths* and Hubert's *History of the Celtic People*, and in the brilliant new work by Littleton and Malcor entitled *From Scythia to Camelot*. For those who are dedicated to rational inquiry and are determined to discover what actually happened in history and in prehistory, SZ's intemperate, *ad hominem* assault upon Opie and Haskins is a travesty.

⁹The other contributors will respond to SZ's allegations in due course.

the "eminent" archeologist Professor Renfrew and the preeminent geneticist Luigi Luca Cavalli-Sforza, whom SZ surely must respect because of their sheer **eminence**, both indulge in that dangerous passion, this may enable him to allay the trepidation with which he faces the encounter of the social sciences and the hard sciences. I would also suggest that he read the many judicious, heavily documented articles on this very subject by Robert R. Sokal and his colleagues.¹⁰

Perhaps what SZ really wants is revealed only in the very last paragraph (section XII) of his review: "Indo-European Cultural Studies have to be kept under strict methodological control." Since he cites himself as the authority for that control, we can gain a glimpse of the psychology which motivated him to write such a consistently malicious review. E. J. W. Barber has illuminated an additional facet of SZ's methodology when she describes another paper of his as "unnecessarily negative... in terms of what can be learned about the Indo-Europeans and their neighbors."¹¹

In any event, the only discernible, consistent focus of SZ's comments is condemnation.¹² In a moment of candor, however, SZ shows that he is capable of giving vent to an honest, convincing articulation: "It is true that the presence of the Tocharian language, an Indo-European language of allegedly Western type, in the heart of Central Asia poses many questions deserving research, and even that those mummies offer valuable hints for the further investigation of Central Asia's early history."¹³ This is all that I am asking; it is the whole purpose of the research project that I have organized. I warmly welcome the participation of all diligent, disinterested researchers, SZ included, provided they demonstrate at least a small amount of **enthusiasm** for the subject.

The study of the ancient peoples of Eastern Central Asia and their cultures is a complicated endeavor. To this day, no

¹⁰References will be found in the bibliography to my paper entitled "Priorities" in the mummies conference volumes.

¹¹In her review of *When Worlds Collide*, p. 95.

¹²I would be delighted to hear SZ's thoughts about the possible origins and affiliations of the Bronze Age and Early Iron Age peoples of Eastern Central Asia. It would appear, however, that he is so busy disputing everyone else that he has not allowed himself sufficient time to form an opinion of his own.

¹³I also appreciate the few positive suggestions that SZ makes, such as his discussion of morphological archaisms (in distinction to Adams's isoglosses) and the importance of Iranian loanwords in Tocharian.

firm conclusions concerning their identity have been reached. Several interesting theses have been proposed, such as that some of the mummies may have been the ancestors of the historical Tocharians, that they may have been associated with the Afansievo Culture of the Minusinsk Basin, and that others may have been Epi-Scythians. I expect that it will be many years before we will feel confident that a consensus has been reached. In the meantime, much hard work remains to be done—in archeology, in genetics, in physical anthropology, and in linguistics.

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