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# Reviews VIII

by
Peter Daniels, Daniel Boucher, and various authors

Victor H. Mair, Editor
*Sino-Platonic Papers*
Department of East Asian Languages and Civilizations
University of Pennsylvania
Philadelphia, PA 19104-6305 USA
vmair@sas.upenn.edu
www.sino-platonic.org

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William RYAN and Walter PITMAN. *Noah's Flood: The New Scientific Discoveries about the Event that Changed History*. New York: Simon & Schuster, 1998. Pp. 319. $25.00. ISBN 0-684-81052-2.

Reviewed by Peter T. Daniels, New York City

This may be the worst title a generally worthy book has ever received. The authors are marine geologists, senior scientists at the Lamont-Doherty Earth Observatory of Columbia University, and they are largely responsible for discovering that the Black Sea as presently known was created just about 5600 B.C.E. when a natural dam at the Bosporus, which held back the sea at a level some 400 feet higher than where the Black Sea was at the time, burst. Ryan, moreover, had previously discovered that the Mediterranean, too, had been dry in the not too distant past as well—maybe six million years ago.

The bulk of the book (part 2, 51–161) is a narrative of how those two discoveries were made, fully endnoted with references to the technical literature (263–302). It is definitely not a case of "Just the facts, ma'am," but a highly personal history of events at most of which the authors were present; thankfully, the narrative is well written even though no journalist or novelist co-author is credited. It reads quickly, and it seems to be a clear and authoritative presentation of the evidence and its interpretation—an interpretation, we are given to believe, that has never been in the slightest controversial. About the only hint of conflict involved some KGB surveillance of oceanographic surveying in the Black Sea by a joint Soviet–American team!

Unfortunately—and perhaps it was at the publisher's insistence—the hydrogeological part is preceded (part 1, 21–57) by a conflated account of the decipherment of cuneiform (which manages to completely miss out Georg Friedrich Grotefend, Edward Hincks, and everyone else involved except H. C. Rawlinson) and George Smith's discovery and initial interpretation of the Flood tablet from the Mesopotamian epic of Gilgamesh. Well, I suppose it's adequate history for a pair of marine geologists, but it certainly isn't close to an accurate representation of the events.

The last part of the book relates the Black Sea flood event to the human population of the area (part 3, 165–225). Knowing nothing of Near Eastern archeology, and less of European archeology, I must accept the authors' claims that the epoch of the flood coincides with the appearance all across Europe of new cultures: Vinča, *Linearbandkeramik*, Danilo-Hvar, Hamangians. To the south, they also connect "the Ubaids"—this and other transmogrifications of terms for archeological sites into names for peoples like "the Halafs" and "the Kurgans" suggest even to this tyro that the authors have scarcely more than skimmed the relevant literature and did not have this portion read by appropriate commentators. I have never heard anyone who knew her refer to Dame Kathleen Kenyon as "Kay"; having that be the *only* name she is granted seems inappropriately cheeky, to say the least.

I am on much surer ground (as it were) in the chapter concerning the spread of languages. It is possible that the flood relates to the dispersal of Indo-European; connecting it with the Tocharian manuscripts of nearly 7000 years later, or with the possibly proto-Tocharian mummies from several millennia earlier than that, seems a stretch. Deriving Semitic-speakers from a population on the southern shore of the Black Sea that fled across the Taurus into Mesopotamia in two branches (Akkadian- and West Semitic–speaking, presumably) strikes me as much less defensible, especially since on the map (p. 194)—but nowhere in the text—"pre-dynastic Egyptians" are said to come from the far northwestern coast of Anatolia. (In this connection we note an acknowledgment to Colin Renfrew, in whose *Scientific American* article about a decade ago [Oct. 1989] there is no evidence of awareness that Egyptian and Semitic are part of the Afroasiatic phylum, most of whose languages are firmly established in Africa.) The brief discussion of language history and comparison (208ff.) is hopelessly muddled, and repeated mentions of "Kartvalian" further erode confidence (the Georgian language belongs to the Kartvelian family). Just about the only sources cited in this area are the well-known books by the archeologists Renfrew and J. P. Mallory (identified as a respected linguist!) and a *Scientific American* article by T. V. Gamkrelidze and V. V. Ivanov (March 1990). The finding by Don

Ringe et al.'s computer analysis that Anatolian was the first family to branch off from Indo-European is presented as if it were a surprise (212).

The next chapter describes Parry and Lord's discovery of the bardic tradition, in hopes of explaining how tales of a catastrophic flood could have survived several thousand years to appear in various mythologies. Finally, there is a telling of the Babylonian flood story pieced together from many published translations (part 4, 229–59).

The prologue of the book (13–18) is an especially vivid picture of various communities fleeing rising waters that they cannot understand. It is a far more valid representation of the importance of the event than the amateurish reconstructions of linguistic history found later on.

The immense value of this book lies in its pellucid presentation of the evidence and argumentation for a Black Sea flood at a particular moment in recent prehistory. What its consequences on the local populations were might better have been left to those better suited to offer interpretations—but this task is made much harder by the omission of one crucial datum: there is no map of the extent of the proto–Black Sea just before the flood event. We are given no idea of how much territory was affected—even though there are maps of several other stages, including the largest extent of the New Euxine Lake, swollen with glacial runoff, that with the Aral and Caspian Lakes formed a great chain emptying into the sea, around 12,500 B.C.E.

The implications for human prehistory have barely begun to be explored.

ADDENDUM: See the accompanying review of *The Bronze Age and Early Iron Age Peoples of Eastern Central Asia* for additional comments.

MAIR, VICTOR H., ed. *The Bronze Age and Early Iron Age Peoples of Eastern Central Asia.* 2 vols. *Journal of Indo-European Studies* Monograph 26. Washington, D.C.: Institute for the Study of Man Inc.; Philadelphia: University of Pennsylvania Museum Publications, 1998. Pp. [xviii] + 534, [ii] + 535–899. ISBN 0-941694-63-1.

Reviewed by Peter T. Daniels, New York City

I approach this collection as an outsider, as a linguist making no claim to special knowledge of Inner Asian archeology, history, or languages. For the linguist, Mair's collection and related recent work raises several important questions regarding the history of language use in Inner Asia and the relationships among the Indo-European languages (both Iranian and Tocharian), Turkic languages, and Chinese. In particular, correlating archeological and linguistic data is, as always, a problematic enterprise, and it is made more difficult by persistent attempts to align in erroneous ways language, culture, and "race." Recent attempts to enlist the computer in broad studies of language affiliation provide no panacea for long-standing problems.[1]

Mair's book comprises the publication of a major conference held at the University of Pennsylvania, in Philadelphia, 19–21 April 1996.[2] From this point of view, it has the one great failing of absence of an introductory overview of the many problems to be considered and the data to be evaluated. This lacuna is now largely filled by E. J. W. Barber's *Mummies of Ürümchi* (1999), which, however, did not become available until many months after the publication of the volumes under review. The other main defect of the enterprise is the use of a malfunctioning printer for producing camera-ready copy, which left the dots off most of the *i*'s in the size of type used for references and footnotes, lending authors and titles an oddly Turkish look. (The names of ancient sites on the master map, vol. 1, p. v, are rendered in a small bitmapped font tilted some 30°

[1]This summary paragraph was contributed by M. O'Connor, whose advice has been valuable throughout this review as well and is gratefully acknowledged. I would also like to thank Victor Mair for much help with both the substance and the form of this review article, well beyond the call of duty of either book or journal editor.

[2]Seven of the articles in the book—those by Jettmar, Tzehuey Chiou-Peng, Adams, W. Wang, Linduff, Hsü, and Gladney—were not conference presentations, but were added later; all but Adams's and Hsü's deal with matters peripheral to the main focus. Eleven papers intended for but not presented at the conference and not received in time for publication are briefly described by the editor (18–20).

to the left and are thus virtually unreadable.)

It turns out that "The Mummies of Ürümchi" is a misleading title—as though a book about thunder-lizards were called "The Dinosaurs of New York" because it was based on the collections of the American Museum of Natural History. Barber is a rare specialist in ancient textiles, making her an excellent guide to the mummies housed in the museum at Ürümchi, since almost nothing but their clothing seems to have been recovered from their interments.[3] Barber's book is not particularly easy to use for reference, but it is a good read, since it interweaves accounts of her own process of discovery, her findings, and tales of the adventures and misadventures of many previous explorers of Central Asia.

Barber describes desiccated corpses (not "mummies" in the Egyptian sense) from three different sites around the Tarim Basin associated with some of the paths of the Silk Road connecting West and East, representing three quite distinct eras and cultures. Most of her attention is given to the latest group of individuals, those from Cherchen, ca. 500 B.C.E. Their weaving techniques appear to be a development of those of the earliest group, from Loulan/Qäwrighul, ca. 2000–1800 B.C.E. The latter were buried with sprigs of ephedra, a shrub containing hallucinogens that has convincingly been argued to be the source of the mystic beverage *haoma* [Iranian]/*soma* [Indic], so it is a plausible supposition that they spoke an early form of Iranian, or perhaps even an undifferentiated Indo-Iranian language. Barber's third group, from Hami/Qumul, ca. 1200 (or 770? [this volume, 651, 661]) B.C.E., are apparently the most sensational: their textiles are

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[3]Remarks by both Barber and Mair make it clear that the museum's authorities have provided less than full cooperation to Western researchers: items on display were not removed from cases, items in storage were brought out individually at the whim of the curators rather than after an overview of the collection or even on the basis of an inventory. One may surmise that the inordinate attention to the "race" of the desiccated corpses in the Western press, their supposed "Caucasoid" appearance, may be in some measure obnoxious to the Chinese authorities whose relations with "non-Han minorities" could be exacerbated by emphasis on the "alienness" of early occupants of territory they consider their own (cf. n. 25). Similarly, an Appendix by Mair and Dolkun Kamberi (857–64) listing variant forms of names and recommending usage standards seems to go out of its way to favor non-pinyin spellings, even Sinkiang rather than the Xinjiang prescribed by Beijing.

remarkably similar in technique to those associated with the Hallstatt culture of Europe, securely associated with speakers of Celtic. The Indo-European language family attested nearly two thousand years later from documents discovered along the Silk Road, known as Tocharian,[4] is very similar to Celtic among the branches of Indo-European, so this might be taken as some evidence for the period of arrival of the Tocharian languages in Central Asia, so far from the supposed core of Indo-European (165–67).[5]

Of course, no amount of material culture can be probative of the identity of the languages spoken by a long-dead people; much can change over several millennia. And (to a linguist, at any rate) Barber's pages on the languages are the weakest in her book: she relies on questionable articles by nonlinguists (Finkelberg 1997, Mair 1990)[6] to maintain the outmoded view of an Indo-

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[4]"Tocharian" (like "Indian" for Native Americans) is a misnomer (Adams 1988: 1–4): the people known as *Tocharoi* in Classical Greek sources did not speak either of the languages to which that name was prematurely applied. The languages are usually called simply "Tocharian A" and "Tocharian B."

[5]Hami lies east of Ürümchi, on the northern route of the Silk Road, westward on which were the findspots of the Tocharian materials, such as Kucha and Turfan. Loulan and Cherchen are on the southern route.

[6]Finkelberg proposes that the pre-Greek, Indo-European substratum detectable particularly in Greek place names comprised languages belonging to the Anatolian family, but she is unaware of extensive work by V. I. Georgiev and E. P. Hamp showing that the substrate "Prehellenic" language (so called in preference to the much abused "Pelasgian") relates most closely to Germanic (convenient characterization in Hamp 1994: 1665–66). Since Georgiev's major work is not widely available, I append the relevant passage:

> The pre-Greek language "Pelasgian", though it was related to Hittite-Luwian, was a separate IE language. This is clear from the toponymy. "Pelasgian" place names ending in –νθ– and -σ(σ)- are undoubtedly related to the Hittite-Luwian toponyms ending in *-nt/d-* and *-ss-* but, while the "Pelasgian" place names end in –ινθ–, –ανθ–, –υνθ– and -ᾱσ(σ)–/–ησ(σ)–, –ῐσ(σ)–, –ωσ(σ)-, the Hittite-Luwian ones only have the forms *-ant/d-* and *-assa-*, rarely *-issa-*. This difference is quite old, as is evident from the place names in the Mycenaean texts: *korito* = Κόρινθος, *aminiso* = 'Αμνισός, *turiso* = Τυλισός, *konoso* = Κνωσός. This can be explained by the different phonematic changes which cannot be reduced to a common Hittite-Luwian form, cf. IE *-ent-* > Pel. –ινθ– but Hitt.-Luw. *-ant/d-*, IE *-n̥t-* > Pel. –υνθ– but Hitt.-Luw. *-ant/d-*, IE *-ont-* > Pel. –ανθ–, Hitt.-Luw. *-ant/d-*. Thus "Pelasgian", though closely related to Hittite-Luwian, is a separate IE language. (1981: 106)

In his summary, he places Thracian and Pelasgian in the Southern group V and Hittite-Luwian in the Southeastern

Hittite in which Anatolian is only remotely connected to Indo-European[7] and for evidence of early Iranian–Chinese contact. For reasons that are not entirely clear, she introduces (188) a "Uralo-Altaic" language phylum—the English name for this, a century ago when it was in vogue, was "Ural-Altaic"[8]—but not even the most ardent "lumpers" of language families, who combine modern families into ever larger, ever earlier groups, assign any closer connection between the Uralic phylum and the Altaic phylum than between any other phyla within the postulated "Nostratic" supposed to be ancestral to virtually all the languages of Eurasia.

With this introduction to the field of study, we can turn to the volumes under review. Two parts in the second volume deal with questions of material culture. That of the excavated mummies is considered only in Barber's own article, "Bronze Age Cloth and Clothing of the Tarim Basin: The Krorän (Loulan) and Qumul (Hami) Evidence" (647–55), and in "Bronze Age Cloth and Clothing of the Tarim Basin: The Chärchän Evidence" by Irene Good (656–68), which constitute the part "Textiles." (They represent a much compressed version of the information presented more accessibly to the lay reader in the opening chapters of Barber 1999.)

Under "Metallurgy," Ke Peng describes "The Andronovo Bronze Artifacts Discovered in Toquztara County in Ili, Xinjiang" (573–80), an area just inside the border, far to the east of any other Andronovo sites (map, p. 575) except a "Shamsha hoard" found in Kyrgyzstan. They are not among the "Copper and Bronze Metallurgy in Late Prehistoric Xinjiang" discussed by Jianjun Mei and Colin Shell (581–603), who report a copper-smelting site has been found at Nurasay, Nilqa, far to the southwest of the region, dating around the beginning of the first millennium B.C.E.;

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group VI (361ff.).

[7]The notion of an "Indo-Hittite" comprising Anatolian versus everything else Indo-European gained some currency in the U.S. (but never in Europe) in the 1930s and 1940s and was thoroughly refuted a generation ago (Puhvel 1966).

[8]Pedersen ([1924] 1931: 248), in a work cited by Barber (1999: 224 ad loc.!), explicitly denies there is proof of genetic relation between what he (or rather, his translator) calls "Finno-Ugrian and Turkish (between Uralian and Altaic)."

copper and bronze are not found in Xinjiang before 2000. Emma C. Bunker, "Cultural Diversity in the Tarim Basin Vicinity and Its Impact on Ancient Chinese Culture" (604–18), discusses the use of gold (and other precious metals) in ancient China. Five cultures are included in "The Emergence and Demise of Bronze-producing Cultures outside the Central Plain of China" by Katheryn M. Linduff (619–43). Three of them flourished simultaneously in north(east) China, in the first half of the second millennium; one, farther west, was earlier, and one, farther south, is found from the second quarter of that millennium.

The dubious, even offensive, search for "racial" affiliations is prosecuted in the part called "Genetics and Physical Anthropology." Paolo Francalacci offers "DNA Analysis on Ancient Chinese Desiccated Corpses from Xinjiang (China): Further Results" (537–57).[9] He explains that the molecules studied are delicate and the possibility of contamination of samples is high; moreover, the samples were taken from "several individuals naturally mummified, dated 3,200 BP, from the graveyard at Qizilchoa near Qumul (Hami) ... and from the Museum of Archeology in Ürümchi. All together, 25 specimens from 11 individuals were collected, but up to now only 5 samples belonging to 2 individuals are available for analysis" (540). However, the two individuals are nowhere identified! Were they from Hami (thus perhaps Tocharian-speakers), or from the museum? If from the museum, from the Hami group, or another (thus perhaps [Indo-]Iranian-speakers)? The "preliminary results are in agreement with a possible European origin of the ancient Xinjiang corpses" (544); but in view of the wide range in time and culture of "the corpses," apparently unknown to the researcher, what does this tell us? In the next article, Tongmao Zhao considers "The Uyghurs, a Mongoloid–Caucasoid Mixed Population: Genetic Evidence and Estimates of Caucasian Admixture in the Peoples Living in Northwest China" (548–57). In "The Physical Anthropology of the Ancient Populations of the Tarim Basin and Surrounding Areas" (558–70), HAN Kangxin measures skulls.

The part entitled "Geography and Climatology" encompasses two articles, one outlandishly

[9]This article appears on superficial examination to reproduce the author's in *JIES* 23 (1995): 371–98.

speculative, the other extrapolatory. Harold C. Fleming, "At the Vortex of Central Asia: Mummies as Testimony to Prehistory" (671–96), commits the cardinal fallacy of equating language families with ethnic groups, even doing so at the highest levels—"Tibeto-Burmans," "Altaic hunters"—and the most conjectural—"Dene-Caucasian dispersions" (all 671). He attempts to portray the (pre)history of "High Tartary" (which apparently excludes "Chinese Turkistan," whatever that may be, but is defined by enumeration as comprising Turkistan, W.Siberia, Dzungana, Altai, Tarimia, Tibet, and Kansu—so the headers of seven arrays within the text [674–76] that are reprinted as table 1 [676–78]). The claim is that "each of the labels can be filled in with ethnographic, biological, economic and linguistic data, including political relationships" (674) for seven periods over five millennia; in the event, only language (family) names appear, along with an entry on horses in some cells and "herders, hunters, farmers" at the earliest layer. The only reference to the "mummies" is in the sentence "Chariot Europoids refers to the 'mummy people' themselves who most likely used chariots ..." (680): this appears to be an error: horses, yes (Barber 1999: 35); chariots, no evidence. And again "mummy people" fails to distinguish among the several populations. Later in the same paragraph, "Linguistic evidence presented in other papers suggests that Tocharian was probably an earlier arrival in Turkistan than Iranian [I find no such presentation—PTD] and is a more likely source of Indo-European loanwords in Sinitic than is Iranian [true—PTD]." The suggestion by some (but not all or even many) linguists (see below) that Tocharian was one of the first families to diverge from core Indo-European does not of course mean that Tocharian-speakers arrived in any specific region earlier than speakers of some other variety of Indo-European.

Kenneth J. Hsü, "Did the Xinjiang Indo-Europeans Leave Their Home because of Global Cooling?" (683–96) gives worldwide evidence for a 1200-year cycle of global warming and cooling, with minimum temperatures around 2000 B.C.E., 800 B.C.E., 400 C.E., and 1600 C.E. (691)[10]—the first two of these might correlate with the entry of Iranian- and (according to Barber's

[10]The repetition of these numbers on p. 695 with "C.E." and "B.C.E." reversed seems to be an error.

suggestion) Tocharian-speakers respectively into the (at the time) more hospitable Tarim region; Ryan and Pitman (1998: 215–16) describe an immense lake filling the Tarim Basin after the last Ice Age that began to dry up ca. 12,000 B.P. and remnants of which were described in Marco Polo's report.

The part called "History" again includes two articles. Michael Puett, "China in Early Eurasian History: A Brief Review of Recent Scholarship on the Issue" (699–715), limns the pendulum swing between "diffusionism" and what might be called "autochthonism" in the historiography of China, noting that consensus appears to have been reached on an "interactionist" model, wherein outside influence is neither assumed nor excluded. It seems clear that metallurgy and chariotry, at any rate, arrived from the west (ca. 2000 and ca. 1200 respectively). E. Bruce Brooks, in an article rendered virtually unreadable by idiosyncratic methods of indicating dates and references and by a bizarre scheme for transliterating Chinese whose key is unpublished, considers "Textual Evidence for 04c [scil. 4th c. B.C.E., or, using Needham's system, –4th c.] Sino-Bactrian Contact" (716–26). A millennium later than the "mummies" ostensibly responsible for the conference at hand, these materials would not seem particularly relevant.

The next and last part, "Mythology and Ethnology," includes, surprisingly, the only contributions in the 900 pages by specialists in Altaic and Iranian languages.[11] Denis Sinor (Altaicist), "The Myth of Languages and Language of Myth" (729–45), begins with the important warning that cannot be repeated too often or stressed too highly:

> It is usually assumed that over the centuries if not millennia, the geographical distribution of the two language groups—Sino-Tibetan and Altaic—has not undergone major changes, and it is also at least tacitly assumed that some correlation exists between geographic races and language families. Hence a

[11]Surprisingly, because the present-day inhabitants of Xinjiang mostly speak the Turkic language Uyghur, and Turkic is one of the three traditional components of what may or may not be the linguistic unity Altaic, and the prehistory of Altaic ought to be considered along with that of other relevant language families; and because Iranian languages are attested from the same areas as Tocharian (which is well represented in the volume) but rather earlier, and Mair (1990) had adduced evidence of Iranian loanwords into Chinese at a very early period (but see below).

> population with Europoid ethnic features would presumably speak an Indo-European language.
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> This belief is just plain wrong. Genetic and linguistic relationships are not correlative. The fact that languages $L^1$, $L^2$, $L^3$, $L^4$ derive from a primary *L does not prove that the peoples $P^1$, $P^2$, $P^3$, $P^4$ have the same *P ancestry. Racial features provide no reliable linguistic information. (729)

Sinor continues by implying that Iranian is a more likely candidate than Tocharian for the local Indo-European language and by surveying in a single page the succession of cultures of the region, before turning to his principal theme, which is the tracing of a myth of cranes and pygmies from Homer to China to North Asia—could the sizable non–Indo-European element in Greek, he wonders, reflect remnants of far-prehistoric connections (of some sort) with a primeval Eurasian unity? The article is marred by many miswritings of "CE" where "BCE" is obviously intended.

Alone among contributors to this collection, C. Scott Littleton offers citations in support of the consensus that "the mummies" represent Tocharian-speakers. The opening sentences of "Were Some of the Xinjiang Mummies 'Epi-Scythians'? An Excursus in Trans-Eurasian Folklore and Mythology" (746–66) are:

> It should be emphasized at the outset of this paper that I concur wholeheartedly with Pulleyblank (1995:415), Mair (1995a:299), Xu (1995:364), Adams (1995:410), and others who suspect that the majority of the Xinjiang mummies—especially the ones that date from the second millennium BCE or earlier (cf. Mair 1995a, 1995b)—were most likely Indo-European-speaking Tocharians of one sort or another. The evidence adduced in support of this identification, although still largely circumstantial, is extremely convincing.

This statement offers the opportunity to consider the earlier collection of articles assembled by Victor Mair, a special issue of the *Journal of Indo-European Studies*, taking them in the order they appear there:[12] Is the evidence convincing?

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[12]Articles by E. J. W. Barber, I. Good, and P. Barber are excluded, having been superseded by Barber 1999; for Francalacci's, see n. 9.

Mair 1995b[13] clarifies that the recent finds of "Caucasoid" persons from the Tarim region should not be considered surprising, since similar individuals have been known for close to a century; he makes the mistake, however, of assuming that they are "almost certainly the most easterly representatives of the Indo-European family" (282). He admits that "we must be prepared to accept that they may not constitute an utterly homogeneous group" (289), but the inference certainly is that they do. A further fallacy is the statement "I leave it to the historical linguists to explain how Tocharian, with its apparent Celtic and Germanic affinities and its archaic features, became separated by Indo-Iranian and Balto-Slavic languages from the languages which it more closely resembles" (300). This, of course, is the task not of the linguist, but of the (pre)historian and the archeologist; the linguist identifies the relationships among the languages (on which see below) but has nothing to say about the peoples who spoke the languages. Besides the "Tocharians," another group constituting a subsidiary leitmotif in the volumes under consideration is the Yuezhi. Here, Mair says,

> we know from historical sources that during the second century BCE the Greater Yuezhi themselves moved all the way from northwest China[14] to Ferghana and Bactria ..., then from there south across the Hindu Kush into Afghanistan and the northern part of the Indian subcontinent where they founded the mightly Kushan empire. The latter, in turn, extended its power back into the Tarim Basin and with it spread Buddhism, which eventually reached China. (300)

Xu Wenkan (1995) cites a considerable number of Chinese sources but uses such terms as "White (Caucasian) race" (358), "white people" (359), and "Europeans of a somewhat primitive shape" (360), the latter referring to: "In addition to the obvious European features such as long and

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[13]For convenience I have retained Littleton's designations of the references; I have not seen Mair 1995a.

[14]Since Xinjiang was not even brought "within the orbit of Chinese control" until after 1850 (Mair 1995b: 301), and so cannot be the "northwest China" mentioned here, and thus presumably even at the relatively late 2nd c. B.C.E. was not Yuezhi territory, one wonders how they could be relevant to the study of the recently discovered peoples many centuries earlier than the historically attested Tocharian-speakers, who seem fairly well connected with the Yuezhi (see below on CHEN Chien-wen's article).

narrow shape, highly prominent brow ridge and nasal bone ..., these skulls still have some primitive characteristics such as backward sloping forehead, low and wide face, deep eye sockets, wide nose, etc." (ibid.). "These skulls" date to 3800 B.P., raising some question as to the meaning of "primitive" here. (Of course it may be Xu's translator who is at fault.) But what of Littleton's "extremely convincing" evidence? Even more baldly than Mair, Xu states: "Most of the ancient residents in Xinjiang were white people, and their languages must have belonged to the Indo-European family" (360). Before the Yuezhi, Xu turns first to the Wusun, described in the *History of the Later Han* [Han dynasty: 202 B.C.E. – 220 C.E.] as green-eyed and red-headed (361); later he notes that the Yuezhi, too, "can be related with Tocharian people and language" (365). Unfortunately, in between he credulously accepts the most extreme proposals of long-range linguistic connections and the widely publicized but highly questionable associations of human linguistic and genetic relations.

Mallory 1995 is a typically lucid introduction to the archeology and linguistics of Central Asia. Again focusing principally on "the Tocharians," he identifies three archeological horizons that could be associated with their "migration" to the area: Afanasievo (third millennium, or perhaps as early as 3500 B.C.E., replaced ca. 2200 by Okunevo); Andronovo (ca. 2000 or 1800 to 1500, usually associated with some form of Indo-Iranian language); and Hallstatt (9th–8th c.; in China relating to Dongson). The earliest of these prompts Mallory to offer linguistic evidence showing that "it is difficult to see how Proto-Tocharian could have separated from the other IE stocks earlier than about the 4th millennium BC and possibly later" (381), which accords with conventional linguistic notions of the placement of Tocharian within IE, but makes it difficult to understand the claim that "dates in the range of *c* 3500 — 2500 BC may tip the scales toward a Tocharian (or anonymous IE) identification rather than Indo-Iranian" (382) (there are no signs yet, though, of human presence in the eastern Tarim Basin before ca. 2000). Mallory's comment on Andronovo indicates that he has interpreted the evidence correctly but feels pressure to acknowledge the dominant point of view:

> Whether the entire Andronovo phenomenon belonged to Indo-Iranians in the larger

> sense or whether it included Proto-Tocharians is impossible to say from the direct evidence but the former is more likely than the latter. If Tocharians are also to be included here, there is no evidence for a distinctive culture, or better, the type of cultural and presumably linguistic isolation that might indicate independence from the general continuum of Indo-Iranian languages. (378)

Mallory states that evidence for the latest of the three possible "migrations" is weaker than when it was hypothesized in the 1930s but acknowledges that it fits best with the linguistic evidence for the "western" affinities of Tocharian.

The linguistic evidence is summarized by Adams (1995). The author of a grammar of Tocharian (1988), Adams first offers some evidence that the Yuezhi may have been Iranian-speaking, including Iranian place names and loanwords in the Tocharian areas and languages respectively. After an attempt to refute some of the suggested etymologies, he asserts concerning the Loulan mummies dated 2200–1900 B.P. [*sic*] that, "given both date and location it would be surprising if, when living, they had not spoken some form of Tocharian (presumably 'Tocharian C')" (403). The Loulan group, however, according to Barber 1999, is dated ca. 2000 *B.C.E.* and is associated via ephedra with (Indo-)Iranian. Adams's other remarks on dates and Tocharian vs. Iranian areas seem consistent with Barber's statements. He goes on to consider the relationship between Tocharian and the rest of Indo-European. The evidence (404–7), while sparse, is of the sort that is virtually irrefutable, and it clearly indicates closer affinity to western stocks than, in particular, to Indo-Iranian, and also indicates that models showing Tocharian as the second language to lose contact with core Indo-European are, at best, simplistic. There are in addition four groups of Iranian loanwords in Tocharian; all this indicates to this reviewer that Tocharian arrived relatively late in regions where (Indo-)Iranian previously prevailed.

For Edwin Pulleyblank, a leading Sinologist, it is the Kirghiz rather than Xu's Wusun who are green-eyed and red-headed (1995: 415, citing the *Tang Huiyao*, a work many centuries later than Xu's Han annal), suggesting that the monstrous appearance of foreigners is a topos in Chinese historiography rather than a description to be taken literally. He discusses the evidence in Chinese sources and the Chinese language for the presence of the Tocharian languages, and is able to trace them back to the 2nd c. B.C.E., but no earlier. He seems far more certain than Mallory is that Mallory connects the Tocharian language with the Afanasievo culture. He takes up the Yuezhi, arguing that "if one wants to see them as Iranian, it implies a migration at some unspecified earlier time from western Central Asia to the borders of China, either bypassing the Tocharian settlements in the oases of the Tarim on the north, ... or along the southern silk road from or through the known Iranian settlements in Kashgar and Khotan" (421–22); but this is only a problem if it is insisted that Tocharian-speakers were there before any Iranian-speakers. Pulleyblank last questions (though with no particular relation to the "mummies") "how Gandhari Prakrit came to be used as an administrative language in Shanshan. ... The most plausible explanation is that it was the legacy of a Kushanian occupation of Xinjiang of which there is no mention in contemporary Chinese sources but which is referred to in Buddhist legends of the exploits of Kanishka, greatest of the Kushanian rulers" (422). Is this "explanation" really more plausible than the suggestion that, as so often, a written language was brought to the region along with Buddhist missionaries, as has happened so often throughout the history of proselytization?

Donald Ringe, whose own historical phonology of Tocharian (1996) is published in the same series as Adams's historical grammar, simply presents a baby introduction to Indo-European linguistics (1995), mentioning the four layers of Iranian loanwords, and refers (440) to an earlier article (which he variously references as 1990 and 1991; the journal issue is dated "1988–90" and was received at the Columbia University library on 14 September 1992) for the assertion that "Tocharian shares virtually no diagnostic innovations with any other branch of IE." Over more than 60 pages there, Ringe's main counter to the evidence proffered by other specialists seems to be "I don't believe it." Adams (1995: 405–6) lists nine "putative shared innovations" of Tocharian, of which he especially stresses three: (3), (4), and (9). Because the terminology and approach of the two specialists differ immensely, I as an outsider cannot be certain that Ringe has even addressed most of Adams's points; but here is the clearest one:

> 9) Characterization of (thematic) dual oblique by **-ois-* (shared with Greek)
> (Adams 1995: 406, citing Adams 1984: 398 as well as a later article on the dual not

> available to Ringe)
>
> **5.9.** I also prefer to be cautious concerning the derivation of *-ais-* in T[ocharian ]B gen. du. *-aisäñ/-aisi* from (quasi-)PIE … *-oysi (Adams 1984: 398), as the fate of PIE nonfinal *oy in noninitial syllables remains unclear. … Moreover, Olav Hackstein (p.c.) points out that TB *-ai-* appears to be a dual marker in a number of verb endings …; if that is its function in nominal endings as well, it is unlikely to reflect any PIE morpheme directly. (Ringe 1988–90: 102)

Neither of Ringe's observations vitiates the comparison with Greek! The *absence* of a form in PIE, and its *presence* in two (or more) families, is what suggests those families experienced a period of common development and constitute a subgroup. It seems to be very important to Ringe that Tocharian *not* prove to be a "western" Indo-European language, though why this is so remains a mystery.

We can now return to Littleton's article in the volume under review. I think it is clear that I find "the evidence adduced in support of this identification [of 'the Xinjiang mummies' as Tocharian]" far from "extremely convincing." But he suspects "that at least some of these Central Asian Europoids were what [he has] chosen to call 'Epi-Scythians,' that is, eastern ethnic cousins of the ancient Scythians" (746)—despite his dating that "in the latter part of the first millennium BCE (or possibly as early as 800 BCE …) several of these Northeast-Iranian-speaking peoples appear to have migrated to the Tarim Basin and perhaps even farther east" (ibid.). His demonstration of shared folkloric motifs among the British Arthur and Lancelot, the Ossetic Batraz, and the Japanese Yamato-Takeru, all well within the Common Era, thus appears to be anachronistic and not demonstrably relevant to the populations under consideration.

CHEN Chien-wen's "Further Studies on the Racial, Cultural, and Ethnic Affinities of the Yuezhi" (767–87), after considering Turkic and Qiang (or Proto-Tibetan) solutions, comes to the by now unsurprising conclusion that they "Must Have Been Indo-Europeans" (774, heading of sec. 4)—speaking either Iranian or Tocharian (779). CHEN's presentation is valuable because the literature survey covers almost entirely Chinese authors, many of whom opt for the Turkic or Proto-Tibetan solution.

The last two articles in the book do not relate specifically to the "mummies." Dolkun Kamberi, "Discovery of the Täklimakanian Civilization during a Century of Tarim Archeological Exploration (ca. 1886–1996)" (785–811), chronicles that archeological work, most of which has covered historical periods, and also describes his discovery of the Cherchen burials. The term "Täklimakanians" may be a bit misleading; it is characterized as "Uyghurs" (787), "a place where 'If you go in, you will never come out'" in modern Uyghur (789), and "'Vineyard'" in ancient Uyghur (ibid.)—but one thing that is clear from all that is presented in this volume is that the (Turkic-speaking) Uyghurs are (in the overall picture) quite recent arrivals in Xinjiang. This is stressed by Dru C. Gladney, whose "Ethnogenesis and Ethnic Identity in China: Considering the Uyghurs and Kazaks"[15] (812–34) is concerned to dispel folk notions of these peoples' own authochthony in the regions they currently occupy.

Thus far, we have surveyed the eighteen articles in the second volume under review. Of greater interest to this reviewer are the twenty-six linguistic and archeological studies (including a section on migration) that make up the first volume. Let us begin with the linguistic articles, at least two of which—those of Hamp and of Ringe et al.—are immensely important, and others of which add significant details in support of the view of the "mummies" being developed here.

Don Ringe, Tandy Warnow, Ann Taylor, Alexander Michailov, and Libby Levison, "Computational Cladistics and the Position of Tocharian" (391–414), appears to be the official publication of results that have previously had to be cited from a report in the *New York Times*, 2 Jan. 1996, B15, by G. Johnson, which I have not seen (reference from Ryan and Pitman 1998: 298 ad 211). Their goal has been one of the goals of Indo-European studies for a couple of centuries now: to establish the family tree of Indo-European—the "first-order subgrouping" of the twelve "major subgroups" (399). They correctly assume that the only sure indication that some languages (A, B, C) are more closely related than others (D, E, F) is that A, B, and C share some innovation that does not appear in D, E, or F. They also recognize that just any shared innovation

[15]The correct spelling "Kazakh" is used within the text of the article.

won't do, because some kinds of linguistic change tend to occur frequently in all sorts of languages and so might have happened independently, giving the spurious impression of shared innovation.[16] Moreover, they say that no individual researcher could possibly inspect all the possible pairwise comparisons that would be necessary for a rigorous evaluation of all possible trees of relationship among languages. Hence a computer program was designed to take care of, at least, the latter problem. The data input to this program were 229 "characters" as expressed in twelve languages: for each group, "the most archaic language in the group that is well attested" (399), except that Old English was used instead of Gothic.[17] The total of 229 includes "the 207-word Swadesh list of Tischler 1973," with five of its items—"day" and each non–third person pronoun—split in two ('daylight'/'24 hours' and nominative/oblique); plus "four regular sound changes; two unexplained phonological peculiarities; [and] eleven morphological characters" (400). The 212 words are not listed, but the 17 other items, with their assignments, are given in the appendix (409–11). Some examples:

2. augment (1. present; 2. &c. absent)
10. superlative suffix (1. *-isto-; 2. *-ism̥o-; 3. &c. other or none)
11. full satem development (*kw, *k > k; *k̑ > affricate (1. absent; 2. present)
12. "ruki"-retraction (1. absent; 2. present)
13. obl. du./pl. case endings (1. *-b$^h$-; 2. *-m-; 3. &c. no similar endings)

| | Hi | Ar | Gk | Al | TB | Ve | Av | OCS | Li | OE | OI | La | PIE |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 2) | 2 | 1 | 1 | 3 | 4 | 1 | 1 | 5 | 6 | 7 | 8 | 9 | 10 |
| 10) | 3 | 4 | 1 | 5 | 6 | 1 | 1 | 7 | 8 | 1 | 2 | 2 | 9 |
| 11) | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 | 1 | 1 | 1 | 1 |
| 12) | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 | 1 | 1 | 1 | 1 |
| 13) | 3 | 1 | 1 | 4 | 5 | 1 | 1 | 2 | 2 | 2 | 1 | 1 | 6 |

The initially puzzling assignment of (say) ten different states in (2) is a device to keep the computer

[16]It is not clear why they claim that "the naturalness of a sound change *ti > *si* seems far from obvious" (392), taking the opportunity to cite four unrelated examples; surely such lenition is commonplace, and assibilation via palatalization before a high front vowel is not surprising at all (cf. Georgiev 1981: 272f.).

[17]*Why* this was done is not stated, only that the substitution did not affect the results.

from taking absence of an innovated feature as itself a feature indicating subgrouping (that is, the nine languages that do not exhibit the augment [otherwise unidentified] get nine different indices). Of the seventeen criteria, Vedic and Avestan differ in only one; Old Church Slavonic differs from Lithuanian in two. Items (11) and (12) have identical assignments in every language considered—that is, all *satem* languages also exhibit the RUKI phenomenon[18]—and it is not explained why both are included in the input.

The output of the computer program is, ideally, a binary branching tree, and the following is presented (402):

[ Hi [ TB [ [ La OI ] [ [Ar Gk ] [ [ Al OE ] [ [ Li OCS ] [ Av Ve ] ] ] ] ] ] ]

apparently confirming Italo-Celtic, Helleno-Armenian, Balto-Slavic, and Indo-Iranian—but evaluated as "quite poor" on topological grounds (404). The following arrangement is said to be more satisfactory (403):

[ Hi [ TB { [ [ La OI ] OE ] Al [ [Ar Gk ] [ [ Li OCS ] [ Av Ve ] ] ] } ] ]

with Albanian made coordinate with Germano–Italo-Celtic and with the *satem* group (but binarism is given up). A still "better" result is achieved by removing(!) Old English and allowing Albanian to float anywhere after the divergence of Tocharian B. But the final chart in the chapter (408) virtually abandons binarism entirely. The phrase "Dialect continuum" replaces some nodes; a swoopy curve connects "Germanic" with "Italic" and "Celtic" (for now the individual languages have been replaced by their family names) by way of Armenian and Greek.

Thus are many of the weaknesses of the methodology explicitly exposed. Foremost is the

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[18]A footnote explains why Armenian is not counted as a *satem* language. "RUKI" refers to the fact that "a peculiarity of the Eastern languages (Aryan, Slavic, and in part Baltic) is a change of *s* to *š* after *i u r k*" (Szemerényi 1996 § 4.6, cf. Beekes 1995 § 11.5; details in Georgiev 1981: 277f.). According to Nichols 1998: 262 n. 7, "the *satem* shift applies inconsistently in Balto-Slavic," with examples. Considerable detail on *centum/satem* in Georgiev 1981 § 2.

initial desire for a binary tree of relationships. For more than a century it has been recognized that idioms—closely related or not!—occupying contiguous or even overlapping territories will share features among themselves, with novel ways of speaking welling out from one or another center of innovation, sometimes spreading throughout an extended speech community, sometimes not. A second weakness is the choice of a single, well-documented language to represent each (assumed) family. Szemerényi (see n. 18) states that RUKI operates in *part* of Baltic. Puhvel's discussion of "Indo-Hittite" (see n. 7) involves characteristics of the *non*-Hittite Anatolian languages. Hamp's essays on classification (see below) reflect the most minute study of obscure epigraphic and dialectal data. A third weakness is inevitable in any investigation of language classification: the choice of characteristics to be evaluated. In so general a work as the *Encyclopædia Britannica*, dative plural *-m-* (reflected in character [13] above) is mentioned as indicating a special connection between Balto-Slavic and Germanic (Maziulis n.d., cf. Georgiev 1981 § 8.2), but this special connection is not manifested in Ringe et al.'s binary or quasi-binary trees. A puzzle on the empirical level is the finding of the very early branching of Tocharian, a result at odds with the most careful investigation of Indo-European subgrouping (e.g. Adams 1984).

Results as unsatisfactory (in Ringe et al.'s eyes) as these were not the necessary outcome of the application of computational methods to the problem of subgrouping Indo-European, as is shown by a project published a few years earlier: Dyen, Kruskal, and Black 1992 (not mentioned by Ringe et al.). The purpose of this study was not the generation of a binary-branching tree, but to check some techniques of lexicostatistics[19] against data that had been well studied by conventional techniques of historical linguistics. The 200 (ideally) words of the Swadesh list were compiled for 84 languages (only modern languages were used, so as to make the process evaluated relevant directly to the data available for most of the families to which it can potentially be applied;

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[19]Lexicostatistics in general must not be confused with a discredited application of it, glottochronology, where the age of divergence of two related languages was supposed to be directly related to the percentage of retention of vocabulary shared between them.

thus the Anatolian and Tocharian branches are completely excluded) and the entries compared for cognacy. The computer emitted both an 84 × 84 matrix of cognacy percentages and a graphic representation of closeness of relationship on which boxes are drawn in accordance with explicit algorithms. Not surprisingly, a vocabulary-based classification does not yield a binary tree of relationships. (Neither Indo-Iranian nor Italo-Celtic emerges as a unit, but a "Mesoeuropeic" does, comprising Italic, Germanic, and Baltoslavic.) The first-order branchings of Indo-European are Celtic, Mesoeuropeic, Indoaryan, Greek, Armenian, Iranian, and Albanian. In his discussion of the results, Dyen repeatedly makes the point that some indisputable subgroupings—such as Indo-Iranian—have simply become unrecoverably obscured by the passage of the millennia (48). The lesson of Dyen's experiment is that lexicostatistics produces reasonable results for low-level groupings, but the groupings themselves may not be groupable on the basis of purely lexical data. It should be added that in each project, the judgments of cognacy were not performed by the computer, but by Taylor and Ringe and by Dyen respectively.

The Ringe et al. results may be compared with those in Eric P. Hamp, "Whose [*sic*] Were the Tocharians? Linguistic Subgrouping and Diagnostic Idiosyncrasy" (307–46), which is as dense and allusive as any of the two- or three-page *Einzeluntersuchungen* for which he is so well known (and dozens of which are cited here). The article is certainly difficult to read,[20] but full comprehension of it might amount to a complete education in Indo-European linguistics. Its prime lesson is that subgrouping can be certified only through shared innovations—as set forth in the extensive quotation of a passage Hamp wrote fifty years ago. To it he will "propose now a further constraint":

A relation of proximity in subgrouping can be made plausible only by

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[20]It must be mentioned that a considerable number of typographical errors are found in the text, particularly in the English prose, which must be attributed to the malfunctioning computer available to Hamp's amanuensis, David Testen, who assembled, typed, and retyped the material from handwritten faxes received piecemeal during the author's world travels. I am especially grateful to my friend Dr. Testen for assistance in interpreting this chapter; note that he confirms the enigmatic title as how Hamp intends it.

> demonstrating a principled shared chronology of interesting historical depth; this can be best done by identifying traits whose development presupposes other shared features resting upon common innovation. ... Such a method, it is claimed, improves upon the simple criterion of shared innovation by structural replacement or addition; this last criterion is necessary but not sufficient. (309)

After a pair of illustrative examples showing that Venetic is "either Italic or close kin to Italic," and a long list of forms derived from IE **bherĝh-* 'rise, raise' that seems to suggest highly promiscuous commingling of derivation, innovation, and semantic development, Hamp proceeds through twelve numbered sections that successively mark off subgroups that can be securely established by very subtle indications that can simply not be found by means of surface inspection or by comparing (however extensive) lists of basic vocabulary. Section (1) establishes, not surprisingly, "significant diagnostic innovations not shared by Anatolian. Tocharian participated in these innovations, and therefore is not to be grouped with Anatolian" (315). "Non-Asiatic IE" is covered in section (2). Indo-Iranian (including Nuristani) is clearly a subgroup, but it could have separated from the rest at any time; therefore, it is necessary to find shared innovations in the rest of IE to establish that it was the next one to become detached: for instance *milk* 'wipe off, smear on' in II, but 'to milk' everywhere else; 'tongue' and 'fingernail' show contrasting formations; and "clearly, N-A IE is an integral entity" (316). Section (3) establishes "Pontic" or Helleno-Armenian. Section (4) discusses Northwest IE, which seems more likely an areal grouping than a genetic one, since overall shared innovations cannot be discovered; but Tocharian participates in a number of them and "might show the fine-grained divergences ... by having lived on the margins of NWIE. It is unlikely that the observed result came from a rapid or early termination of the exposure to that conglomerate (= a hurried move East), for that would then cut off the more specific memberships that we are about to consider" (319). Section (5) shows that Albanian is close to Balto-Slavic. Section (6) combines Phrygian with Italo-Celtic—beginning with "*ad-* as a preverb. This feature alone classes Phrygian with Italic, Celtic, Germanic, Prehellenic, and apparently Tocharian" (322, references omitted). The etymology of King Midas's name, too, figures in this long section, which concludes, "My claim of Phrygian's eastward move from the 'West' makes the journey of the

Tocharians not surprising" (326). (This "west," of course, must not be identified with the present-day remnant of Celtic on the farthest fringe of islands off Europe.) Section (7) establishes Italo-Celtic (on the basis of extensive new materials on both ancient and dialectal Celtic that were not available to the doubters of the 1960s). Section (8) briefly considers "'North Europe', an ancient Sprachbund" (cf. Hamp 1990), showing that Tocharian perhaps existed on its periphery but did not participate in it fully. Section (9) mentions the languages Prehellenic ("Pelasgian") and Cimmerian, discoverable only as otherwise unexplained loans in Greek and Slavic respectively. A side trip in section (10) takes us to Thracian and its likewise scantily attested neighbors and their relations to Albanian; section (11) shows that *centum/satem* is not diagnostic of anything in particular; and section (12) turns to "Individual Tocharian correspondences." From a plethora of these, we may choose: "Tocharian *lip-* 'übrigbleiben' [be left over], *lyipär* 'Rest' [remnant] seems to be a peculiarity shared with Germanic 'leave' (e*lev*en, twe*lve*, etc.), against Latin *linquo*, Lithuanian *liekù*, etc." (338);[21] or, from a long list of derivations from **dheg$^w$h-* 'burn', only Latin, Irish, and Tocharian B have senses of 'bodily malaise', presumably by way of 'fever'.

Hamp's article concludes with a complicated diagram of relationships—both genetic and areal, and incorporating population movements—of Non-Asiatic Indo-European. It is in effect a combination of two figures in Hamp 1994: 1664, a family tree and a schema of diffusions; note that the former differs from an earlier avatar in Hamp 1990: 302, and in turn needs to be modified in light of the present article. It would seem impossible after the present demonstration to imagine that Tocharian is an outlier of Indo-European, or that it branched off nearly as early as Anatolian; it would also seem that computerized classifications of languages have a long way to go in subtlety before they are able to produce results remotely related to reality.

Hamp's article is followed (and Ringe et al.'s is preceded) by a number of *Einzeluntersuchungen* by specialists in Tocharian. I am in no position to comment on the details, but their salient points may be registered here. Werner Winter takes up "Lexical Archaisms in the

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[21] The point would seem to be the labial in the former versus the labiovelar in the latter.

Tocharian Languages" (347–57). The article is framed as a response to a brief article by K. T. Schmidt (1992), which claims that the survival of archaic features would "set these two idioms apart from most of the other members of the Indo-European group of languages" (347); Winter rejects three of Schmidt's points (b, c, f), accepts one (a), sets aside two (e, f), and discusses one (g): two alleged lexical archaisms, Toch. B A *yäp-* 'enter' and Toch. B *kwīpe*, A *kip* 'shame'. The first is said to preserve the original meaning of a root that elsewhere in IE changed to 'have intercourse (said of the male)'; the second is supposed to reflect the etymon of otherwise unexplained Germanic **wiβa* 'woman' > English *wife* (Winter is dubious). In either case, the notorious semantic slipperiness of words in taboo realms ought to render any etymological speculations suspect. Winter also adduces the Toch. equivalent of *lox*, which means 'fish' in general rather than 'salmon', and 'stone', which elsewhere is 'millstone', claiming that a word could never change from specific to general in meaning (thus the other IE languages have innovated). But we can belie this by looking no further than Modern English, where *aspirin*, *fridge*, and *xerox* have been generalized. Lastly, Winter cites Mallory (1989: 56–63, 226) in support of a very ancient separation of Tocharian from Indo-European; but this is painfully circular, since Mallory presents the arguments placing Tocharian squarely within core Indo-European, but is forced by some maverick linguists to cast about for a possible very early archeological counterpart to a supposed very early Tocharian. Referring to another item not summarized here, Winter writes: "It seems more reasonable to view this loss as a shared innovation of the non-Tocharian idioms rather than as a development that took place independently in at least four subgroups of Indo-European" (352–53). Winter seems largely innocent of theoretical concerns, but Hamp writes: "I insist further that we must additionally discriminate one more category of consideration, innovation by loss, which is labelled B.2.a [in the reprinted passage written in 1949], and which is sometimes mistakenly invoked. This fallacy is really a special case of the well known *argumentum ē silentiō*" (308), his example being—*centum/satem*.[22]

[22]It is perhaps necessary to point out that what both Dyen et al. (1992) and Ringe et al. (this volume) are

Finally, even if all of Winter's "archaic" features really are archaic, this means nothing for the subgrouping of Indo-European; for is not Lithuanian conventionally said to be the "most archaic" IE language (whatever that means; e.g. Maziulis n.d., Georgiev 1981: 265), while Balto-Slavic stands at the very heart of "core Indo-European"?

Georges-Jean Pinault, "Tocharian Languages and Pre-Buddhist Culture" (358–71), discusses what can be gleaned of "bits and pieces of the culture of the Tocharians before their conversion to Buddhism" (358). He determines that they were culturally close to other, Altaic-speaking, steppe peoples. In the process he provides the first publication (in transcription and translation only) of a brief Toch. B text, a land sale contract (364) that describes the boundaries of the plot in question. Douglas Q. Adams, "On the History and Significance of some Tocharian B Agricultural Terms" (372–78), seizes on this text to discover in it three clear Iranian loanwords dealing with irrigation—which virtually proves that Tocharian arrived on the scene *after* irrigation agriculture had already been established by Iranian-speakers (375).

Two articles deal with Tocharian lexical stock in Sinitic: "Tocharian Loan Words in Old Chinese: Chariots, Chariot Gear, and Town Building," by Alexander Lubotsky (379–90), and "Qilian and Kunlun—The Earliest Tokharian Loan-words in Ancient Chinese," by LIN Meicun (476–82). The latter suggests (on the dubious basis that Tocharian [A or B?] should have a word *kaelum* 'sky' because Latin has *caelum*) that *qilian*, said in Chinese sources to be the Hunnic word for 'sky', must actually reflect Tocharian B *klyomo*, A *klyom*. More soberly, Lubotsky considers a baker's dozen words from the indicated semantic fields that seem to be of Indo-European origin, about half of which can be specifically connected to Tocharian.

Staying in this semantic realm, Juha Janhunen, "The Horse in East Asia: Reviewing the Linguistic Evidence" (415–30), expands the discussion beyond Indo-European to consider the word for 'horse' throughout East Asia. Indo-European, the several branches of Uralic

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investigating is shared innovation by loss—for there is no reason to expect that related languages should all replace the *same* lexical items over time.

(Samoyedic, and the separate components of Finno-Ugric), Turkic, Mongolic, and Bodic (Tibetan) each has an individual term, indicating age-old familiarity with the animal, whereas Tungusic, Chinese, Korean, and probably Japanese all have words that seem to be borrowed from a Mongolic language, "clearly show[ing] that the horse was introduced to East Asia in a rather rapid wave of cultural influence, originally radiating from a single horse-breeding population in Eastern Central Asia. ... Their linguistic and cultural relationship with the Bronze and Iron Age Europoid Mummies of Xinjiang remains a tantalizing but scientifically unverifiable possibility" (426)—excluded, of course, for those who insist on identity of language and "race." We may say, in any event, that while horses may have brought Indo-Europeans, it wasn't always Indo-Europeans who brought horses.

While we must note once again the absence of any articles by Iranologists that might illuminate the proto-historical situation in our region, there follow two articles considering wider "Caucasoid" connections: Caucasian languages themselves. John Colarusso, a specialist in languages of the Caucasus, after a rambling introduction to "Languages of the Dead" (431–47) etymologizing various proper names, eventually gets down to his topic of whether, as has been suggested, the Xinjiang corpses might have been Caucasian- rather than Indo-European–speaking. His presentation of Caucasian toponyms leads him to answer this question negatively; and he concludes with a speculative consideration of the successive stages of Indo-European that might be represented by the corpses (leading all the way back to and before Proto-Indo-European itself). Colarusso's article cannot be considered more than suggestive, but suggestive it is.

Kevin J. Tuite, an authority on Kartvelian (South Caucasian) language and culture, offers "Evidence for Prehistoric Links between the Caucasus and Central Asia: The Case of the Burushos" (448–75). He eruditely finds mythographic and linguistic evidence of contact (but not familial connection) between the language isolate Burushaski (found in northern Pakistan) and the Nakh-Daghestanian (Northeast Caucasian) language family, and between Burusho and (more general) Caucasian myth, suggesting that "the Proto-Burushos were historically linked to the Caucasus region, and very likely migrated to their present homeland from there" (449). Any connection with the Tarim Basin, however, would consist of possible "influx" from the little-studied south (467).

Penglin Wang's "A Linguistic Approach to Inner Asian Ethnonyms" (483–507) ranges widely through Indo-European, Altaic, and Chinese vocabulary, collecting a vast range of materials relevant especially to mutual Tokharian–Altaic influence. William S.-Y. Wang's "Three Windows on the Past" (508–34)—specifically of China—are archeology (510–15), genetics (515–22), and linguistics (523–28). The discussion remains quite general, with the necessary caution against equating language and ethnicity, leading to a puzzling description of a computer program apparently designed to perform glottochronology, using data from Chinese languages, from Sino-Tibetan, and from Indo-European (the last, in fact, borrowed from the unpublished Dyen et al. 1992 database). Since neither the input data, nor the assumptions, nor (except via references) the methodology is described, one has little idea what to make of the resulting dendrograms. Some maps of cultural areas of China at two-millennium intervals (9000, 7000, 5000 B.P.) are then reproduced as somehow relevant to the split between Sinitic and Tibeto-Burman, estimated to have taken place around the last date. With this puzzling paper, the linguistic portion of the book (and volume 1) comes to an end.

Since I am not at home with archeological materials, I will pass quickly over the more data-oriented articles, and then turn to the syntheses by a, or perhaps the, half-dozen leading authorities on the prehistory of Eurasia. In first place are a pair of survey articles: AN Zhimin, "Cultural Complexes of the Bronze Age in the Tarim Basin and Surrounding Areas" (45–62), and E. E. Kuzmina, "Cultural Connections of the Tarim Basin People and Pastoralists of the Asian Steppes in the Bronze Age" (63–93). AN lays out the ten regions of Bronze Age Xinjiang, whose artifacts are dated between about 2000 and 400 B.C.E., falling into three periodizations. The heart of the article is a typology of the bronze objects.[23] But the various "mummies" are not associated with their places and dates—a correlation that might have been of great assistance to other participants in

[23]There is an awkward insertion by the editor (54f.) bringing in Celtic and Scythian comparisons.

the conference in preparing their papers for publication. Kuzmina discusses scholarship surrounding the Silk Road, horses, and appurtenance of the new Xinjiang materials to such horizons as Afanasievo and Andronovo, and presents considerable comparative material.[24] Karl Jettmar's brief contribution, "Early Migrations in Central Asia" (215–21), may be mentioned here as a summary of the issues as they stood before the Philadelphia conference.

HE Dexiu, "A Brief Report on the Mummies from the Zaghunluq Site in Chärchän County," translated by Jidong Yang and Victor Mair (169–74), provides the only discussion in the volume of (some of) the dessicated corpses themselves; the treatment is superseded by that in Barber 1999.[25] SHUI Tao, "On the Relationship between the Tarim and Ferghana Basins in the Bronze Age" (162–68), considers two Tarim cultures of the middle to late first millennium B.C.E., one from southern and western Xinjiang, the other represented only by the Charwighul cemetery in the northwest Tarim, finding that the latter relates to earlier materials from the Turfan Basin and to even earlier ones from the Ferghana Basin. Tzehuey Chiou-Peng, "Western Yunnan and Its Steppe Affinities" (280–304), seems out of place here; it summarizes the archeology of southwestern China.

From the point of view of economics, Natalia I. Shishlina and Fredrik T. Hiebert discuss "The Steppe and the Sown: Interaction between Bronze Age Eurasian Nomads and Agriculturalists" (222–37). They deny that the venerable titular dichotomy is viable. They trace the developments of both pastoralism and agriculture as series of stages, manifested differently in different regions, and driven, ultimately, by environmental and ecological changes. Their elaborate

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[24]An amusing gaffe is found on p. 81. A Russian-language map is provided with an English legend, but the symbols are labeled with cyrillic letters in the key and their explanations with roman letters in the legend—a for *а*, b for *б*, c for *в*, d for *г*, and e for *д*—which will mystify those who can't read the Russian letters and confuse those who can.

[25]Here welcome, if embarrassing, editorial invention (173, 174) corrects the author's baseless claims that the individuals were buried alive, and that "they were probably a branch of the [Tibeto-Burman] Western Qiang people."

map (227) is reproduced too small for the key to be legible.

Jeannine Davis-Kimball, "Tribal Interaction between the Early Iron Age Nomads of the Southern Ural Steppes, Semirechiye, and Xinjiang" (238–63) considers interactions among these regions at the subsequent period and also adds modern ethnographic observation of Chinese Kazakhs. She describes an archeological survey, first of southern Kazakhstan, then of Xinjiang; she has excavated Sauromatian and Sarmatian kurgans of the 6th–2nd centuries B.C.E. in Kazakhstan (and takes the welcome opportunity to display ritual objects and an assortment of Scythian animal representations). She stresses the importance of the Semirechiye (Seven Rivers) area of southern Kazakhstan north of the Tien Shan (thus north of the Tarim Basin)—it "may have been the primary fulcrum for dissemination of trade and cultural systems" throughout Kazakhstan, Xinjiang, and Mongolia (258).

Claudia Chang and Perry A. Tourtellotte, "The Role of Agro-pastoralism in the Evolution of Steppe Culture in the Semirechiye Area of Southern Kazakhstan during the Saka/Wusun Period (600 BCE-400 CE)" (264–79) cover the same area at a later period, with an environmental emphasis. They find both agriculture and herding, once again finding not steppe *versus* sown, but steppe *and* sown. They report 1994–95 excavations at Tuzusai, which appears to have been occupied year-round—and "the Saka/Wusun nomadic confederacies must have relied heavily on the ability to extract surpluses from agro-pastoral settlements like Tuzusai" (275). With this article, we have arrived firmly in the historical period.

Our consideration of the general archeological articles may begin with Colin Renfrew, "The Tarim Basin, Tocharian, and Indo-European Origins" (202–12). In numerous publications, Renfrew has set forth a view of the spread of Indo-European languages based on his interpretation of archeological data. He here presents it as four "phases or episodes": 1. Farming dispersal;[26] 2.

[26]Renfrew 1997 presents more detail on "farming dispersal" and, as here, rightly deprecates population-genetic studies like those of L.-L. Cavalli-Sforza, whose Philadelphia conference presentation was not submitted for publication. Cf. n. 31.

Development of pastoral nomadism; 3. Social Hierarchy and Chariots; 4. Mounted Warriors. This picture is effectively refuted by Mallory (see below) from an archeological point of view; the linguistic objections are far less subtle, and even though Lord Renfrew appears to be oblivious to them, we may first repeat Jasanoff's (1988) single devastating example:

> PIE **(h₁)é̂kwos*, the name of the horse ..., is a word that plays little part in his discussions, presumably because the early Indo-Europeans, at the remote date of 6500 B.C. [Renfrew now pushes this back to 10,000 B.P. (204)], could not yet have had horses. ... PIE **(h₁)é̂kwos* is almost certainly connected with the PIE adjective **(h₁)ŏ̂ku-* 'swift' ...; and since the derivational processes that relate the two forms were no longer productive in late IE, the creation of **(h₁)é̂kwos* must have taken place within the proto-language itself.

Additional objections may be made to the present article. Renfrew states (204), "I have proposed ... that the earliest Proto-Indo-European ... homeland was in Anatolia, some 10,000 years ago.[27] This view has been taken on linguistic grounds ... by Gamkrelidze and Ivanov (1984" [1995]). These authors' view, however, is that the Indo-European homeland occupied the Caucasus region, and not Anatolia (as can more conveniently be seen in their *Scientific American* article 1990: 112, or in sketch maps displaying nine homeland proposals in Baldi 1999: 40). Renfrew states (208), "In harmony with the view of Dolgopolsky, and of Gamkrelidze and Ivanov, and following Sturtevant (1962), I suggest that the basic division in the early Indo-European languages is between the Anatolian languages on the one hand and all the other members of the Indo-European family on the other." Sturtevant "1962," however, is a verbatim reprint of an obscure 1939 article,

[27] Actually, Jasanoff is correct; Renfrew (1987: 207) says "the original separation would have taken place by 6500 BC"; and also (ibid. 266), "before about 6000 BC there were, in the eastern part of Anatolia, and perhaps in some adjacent lands to the east and southeast, and probably nowhere else, people speaking languages ancestral to all the Indo-European languages of today." Of course Ryan and Pitman's (1998) Black Sea flood of ca. 5600 B.C.E. was unknown to Renfrew or Jasanoff at their writing, but it would seem to add an additional difficulty to the proposition of a (Proto-)Indo-European language that remained virtually uniform for some thousand (or two thousand four hundred) years until the flood's disruption.

and Anatolian's outlier status has always been recognized.[28] With the following sequence:

> Let us accept, for the moment, the inviting assumption that these 'western-looking' and hence 'caucasoid' people, living in the Tarim Basin around 2000 BCE, were indeed ancestral to the population there some 2500 years later who at that time were speaking the Tocharian language. (203)
>
> It now seems possible that the ancestors of the Tocharians were in the Tarim depression by at least 2000 BCE, and I predict that further evidence will be found going back to c. 3000 BCE. (208)
>
> I am led to suggest, therefore, that at an early date around 3000 BCE one should think of a Proto-Indo-Iranian-Tocharian sub-family. (ibid.)
>
> Of course it is over-bold to posit a possible linguistic relationship on the basis of arguments which are mainly archeological. Rather one should perhaps ask whether there is any linguistic basis for suggesting that the proto-Tocharian language some four thousand or more years ago might have had such affinities with early Indo-Iranian and also with the distant ancestor of Scythian, that all three groups of languages (with descendents [*sic*]) could be considered closely related. (209)
>
> The linguistic relationships are predicted from the model of Indo-European origins advocated, namely that of farming dispersal from Anatolia. It remains to see how far the real linguistic relationships—that is, those based upon a close study of the languages in question—correspond with these predictions. (209–10)

Renfrew reveals himself (besides being unable to count: the "3000 BCE" of p. 208 becomes "four thousand or more years ago" on p. 209) utterly uninterested in linguistic evidence. The long list of proposals for Tocharian's affinities (Adams 1984: 395, dating between 1913 and 1970) has *never* included Indo-Iranian. (And "Scythian" is uncontroversially an Iranian language, its surviving descendant being Ossetic [Schmitt 1989 §§ 2.3.7 (Schmitt), 3.2.4.2.1 (Bielmeier), 4.2.5.1.3 (Thorardson)].)[29] He even claims that Ringe et al.'s results are consonant with these predictions,

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[28]Finkelberg 1997: 10 lists three dissenting views but without dates or references; Reference to Rosenkranz 1978 shows that they belong to the early days of research.

[29]Apparently he has forgotten his earlier admission "Certainly I came to see more clearly some of the difficulties in the linguistic field for the alternative thesis which I have been propounding" (1990: 15, quoted in Krell

which, of course, is not the case in any of their versions!

For a return to sobriety, we may consult J. P. Mallory's "A European Perspective on Indo-Europeans in Asia" (175–201), which as in the earlier collection presents a dense, comprehensive, but comprehensible overview of the relationship between cultures discovered by archeology and speech communities. After a brief history of Indo-European homeland proposals, the refutation of Renfrew's specific ones comes first (177–78), leading to the observation that any *single* model of language dispersal will not account for the spread of Indo-European in both Europe and Asia. These two realms are conceived in terms of four "fault lines," one conceptual—the notion of "steppe versus sown," which (as we have seen with Shishlina and Hiebert) is now recognized as oversimple—and three geographic. The Dniester–Dnieper Line separates largely agricultural land from the steppe; the Ural Line marks off the Andronovo culture; and the Central Asian Line divides the steppe tribes to the north from the historically attested locations of Indic- and Iranian-speaking peoples to the south. (Ironically, it is this latest frontier that is hardest to interpret.) East of the first line lies the Yamna(ya) or Pit Grave culture, which seems to be identifiable with the Proto-Indo-European speech community; east of the Ural River, Andronovo seems to relate to Proto-Indo-Iranian. Mallory is still greatly, and rightly, troubled (189) by the supposed link of Tocharian with the earlier, and farthest to the east, Afanasievo culture, and concludes somewhat hopefully that "the Tocharians may have come from the west but at a date later than that envisaged by all of the previous discussion, viz., at some time in the first millennium BCE" (191), noting that Barber's textile evidence is the only tangible evidence currently available. Mallory's "not entirely facetious" proposal for the last of his fault lines is a metaphor, the "*Kulturkugel*," or "culture-bullet" (192), in which "social organization" (here "steppe") and "language" (here Indo-Iranian) impel "material culture" (here Andronovo) through (what?), and the result is the (recently recognized) Bactrian–Margiana Archeological Complex (BMAC), found at the southern reaches of the steppes, near the northwestern portals to the Indian subcontinent and north of Iran. Such a metaphor is admittedly a

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1998: 280).

throwing up of hands—presumably further investigation will make firmer conclusions possible.

These are exactly the questions addressed by Fredrik T. Hiebert, "Central Asians on the Iranian Plateau: A Model for Indo-Iranian Expansions" (148–61), writing from the point of view of Iranian archeology; he welcomes the new availability of Central Asian materials, and traces BMAC finds throughout Iran.

The development of Indo-Iranian (which he quaintly calles Aryan) has long been a concern of Asko Parpola, and "Aryan Languages, Archeological Cultures, and Sinkiang: Where Did Proto-Iranian Come into Being, and How Did It Spread?" (114–47) is offered as "yet another update" of his model. To his considerable archeological detail, he adds the evidence of loanwords between Indo-European and Uralic languages. The Proto-Uralic and Proto–Finno-Ugric speech communities are widely accepted to have occupied the forests on either side of the Ural Mountains, necessitating an Indo-European (115)—and subsequently Indo-Iranian (119)—presence fairly far to the north. Parpola unfortunately is misled by the *centum/satem* question into requiring an early separation of Tocharian from Indo-European, and association with Afanasievo, but this is now seen to originate from a Khvalynsk (rather than Yamnaya/Pit Grave) culture, and he writes himself into knots (119) trying to reconcile this with the facts of Tocharian and Indo-European. As regards the development of Indo-Iranian and its successors, Parpola's statements (cast largely in terms of successions of cultures) seem compatible with Mallory's version. Further detail on the BMAC (124–26) would present the material background underlying the *Kulturkugel* metaphor. Other topics discussed include the presence of Indic words in cuneiform documents from Mitanni, a Hurrian realm, ca. 1600 B.C.E. (128), the "problem" of *soma* (126–27), and the significance of the horse in Indo-European expansion (passim).

The history of the horse, though, is the specialty of David W. Anthony, and in "The Opening of the Eurasian Steppe at 2000 BCE" (94–113) he lays out the evidence of its importance, coming to the same conclusion as the other authors that the Yamnaya culture most likely represents Proto-Indo-European-speakers, and expressing appropriate doubts (105) about the assignment of Tocharian to Afanasievo. One may be a bit troubled by his reliance on a single bit-worn tooth as evidence of extremely early horse use (contrary to, for instance, Kuzmina's position); a laboratory dating of the specimen was not yet available in 1998 (101f. n. 6).

All that remain at this point are an assessment of the editor's introduction ("Priorities," 4–41) and conclusion ("*Die Sprachamöbe*: An Archeolinguistic Parable," 835–55), and the presentation of my own synthesis of the remarkably coherent presentations contained in this work.

Victor H. Mair begins "Priorities" with an attempt to impress the reader with the number and diversity of studies that have been brought to bear on the textiles of Eastern Central Asia (ECA). But he soon lapses into geneticism, devoting several pages (8–10) to a large number of articles by Robert R. Sokal and colleagues. He summarizes his understanding of the work with the following scattered sentences (picked out in the text in bold type):

> [1] Language differences themselves act as barriers to free gene flow and hence enhance genetic differentiation. [2] Speakers of different language groups in Europe do differ genetically and this difference remains even after geographic differentiation is allowed for. [3] Empirical evidence supports the conclusion that there is a correspondence between linguistic and genetic information, one aspect of which is rapid gene change across language boundaries. [4] There is a significant correlation between genetic and linguistic distances.

Even without a single consultation of any of Sokal's work, some of these assertions are instantly suspect from the point of view of linguistics. Taking them in reverse order, (4) there could only be a correlation between genetic and linguistic distances if there were some reliable metric for linguistic distance. Even the discussion above of Hamp, Ringe, and Winter should show that any such metric is at best a distant desideratum. (3) While there sometimes, or even often, may be a correspondence between linguistic and genetic information, there is no *necessary* correspondence.[30] The quotation above from Sinor is an assertion; the proof is simply that any human infant will learn whatever language(s) it hears spoken around it, regardless of genetic

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[30]Chen, Sokal, and Ruhlen (1995: 610), quoted by Blench (1997: 14), have apparently backed away from their earlier assertions: "The consensus between language trees and genetic trees is low ... so low as to make the trees incomparable."

background. (2) The linguistic situation of Europe, with many "national" boundaries that reflect linguistic and cultural boundaries, is quite exceptional in worldwide terms. In most of the world there are in fact very few linguistic boundaries; there are intercommunicating chains or fields of dialects covering vast areas. Nichols 1992 and Dixon 1997 may be consulted for technical and polemical explorations of this phenomenon respectively. (1) Is it language differences, specifically, or is it many other cultural phenomena that correlate with language that are barriers to free gene flow, i.e. intermarriage, i.e. exogamy? Bosnians, Croats, and Serbs speak the same language but rarely consort together. Intermarriage between Italian-Americans, Irish-Americans, Polish-Americans, etc., is practically a topos of American popular culture, where Roman Catholicism transcends ethnic identification; while intermarriage between Catholics and Protestants may be far less common.[31]

I have, in fact, been able to consult only a handful of Sokal's fourteen articles cited by Mair (and cannot even be sure which they were).[32] What I found was a gridding of Europe that was far too coarse to reveal anything useful about the linguistic composition of the European subcontinent[33] and the sort of general disregard for the nature of language and its variation and the

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[31]I am grateful to Mr. W. C. M. Vaughan for a set of abstracts from the *American Journal of Human Genetics*. That for Hurles et al. 1999 concludes: "This study thus provides evidence for direct or indirect gene flow over the substantial linguistic barrier between the Indo-European and non-Indo-European-speaking populations of the Catalans and the Basques, during the past few thousand years." On the other hand, that for Poloni et al. 1997 states that "human population structure inferred from the Y chromosome corresponds broadly to language families ..., in agreement with autosomal and mitochondrial data. ... Variability ... is also significantly correlated with the geographic location of the populations ..., reflecting the fact that distinct linguistic groups generally also occupy distinct geographic areas." Without information on the linguistic groupings compared, however, this result is highly untrustworthy.

[32]Prof. R. R. Sokal has kindly provided me with a selection of offprints at the request of Victor Mair. A detailed consideration of his work must be postponed to another occasion.

[33]Mair reports the cells are 225 miles square (10). 225 square miles—or cells 15 miles on a side—might have produced interesting results, but 225 miles (360 km) is the distance from Milan to Strasbourg or Budapest to Belgrade—spans covering considerable linguistic diversity. Sokal excludes the Caucasus from his purview.

facts of language distribution that seems to be endemic to social scientists.[34]

Mair next turns to the spread of Sinitic (his penchant for coining otiose neologisms leads him to propose "Hanic") languages. There is one curious observation here: "The vast majority of these languages has never been written down (indeed, many of their morphemes are unwritable in Chinese characters—despite the fact that they are said to be Sinitic [Hanic] languages)" (12). I am at a loss to imagine what writability in Chinese characters has to do with the familial connection of Sinitic languages. Mair also opines that there is at least as much variation within Sinitic as within Non-Asiatic Indo-European—even though the former have been diversifying for about two thousand years and the latter for 5200 (to quote a chart [838] presented but wisely withdrawn by Ringe et al., but of which Mair is enamored—see below); and this even with reference to W. Wang's dendrograms (530), which—however arrived at—show Sinitic about the age of Germanic, and Sino-Tibetan overall somewhat younger than Indo-European.

Another linguistic *faux pas* is found in n. 4, where Mair writes:

> After having observed thousands of bilingual speakers over a period of three decades, I have formulated what I sometimes jokingly call "Mair's Law of Second Language Acquisition". According to this law, most individuals under the age of approximately 11.5 years can move to a different linguistic environment and readily become essentially native speakers of their new tongue, usually correspondingly losing full fluency in the language of their birth. After that age, while there are, of course, rare exceptions, it becomes increasingly difficult for an individual to acquire true native fluency in a secondarily acquired language. I suspect this is so because the neurological configurations required for the processing of language become less malleable (they become "hardened", as it were) as one approaches puberty. (15)

Only incredible naïveté could lead someone to present the most basic, obvious, and familiar fact about language acquisition as his very own "Law."

Despite these *péchés linguistiques*, Mair stresses and exemplifies the important point that

[34]Mair even notes, in presenting Sokal's claims about population movements, that they are contradicted by the facts of Central Asian linguistic history (10).

rarely do conquering invaders impose their languages on subject peoples. Such language shift requires a massive population influx; "elite dominance" normally yields a plethora of loanwords into the local language, and at best "only an ultra-thin veneer on the surface of a sea" of local languages (12).

In the brief succeeding section, "Linguistics, Chronology, and Geology" (16f.), Mair confesses to pressing (dare I say browbeating?) Ringe et al. into putting dates onto their binary-branching tree (and it differs yet again from any of the trees presented in their article); since they disavow it, there is no hope of learning how the dates of branching were arrived at. But since this tree bears no more relation to the branchings discovered by more reliable techniques than any of the others, it is moot anyway.

Next comes Mair's description of the papers presented at the conference that are not published here. He then offers no fewer than twenty-two desiderata for further study of ECA, and then raises half a dozen questions concerning its place in the wider Eurasian picture. Two of these offer the opportunity to discuss Mair 1990. Item (5) concerns the widespread distribution of spiral decorations—mentioning Maori art of New Zealand. The suggestion that spiral patterns could be evidence of cultural contact or diffusion overlooks the fact that the spiral is one of the most simple and obvious designs that must have been invented by every artist—by every child!—anywhere on earth.[35] The same holds for the "cross potent," the symbol Mair finds all across Eurasia. In particular, he finds it incised on the head of a figurine "with unmistakably Caucasoid or Europoid features," so it must be the Archaic Chinese character **m$^y$ag* (> *wu*) 'mage'. Missing from the argument is any evidence that the design on the figurine is in fact a character—no examples are given, for instance, of similar figurines with this or any other character on them. As for the word itself, the attested Old Persian form, *maguš*, is tolerably similar to the reconstructed Chinese

[35]Bill Vaughan notes, "Spiral designs as body decorations are present on Paleolithic so-called 'goddess' figures, whose dates, such as are known at least, so far predate the languages being discussed as to render Mair's argument quite meaningless" (pers.comm.).

form—but the Iranian item is from at least a millennium later than when any borrowing would have had to take place, and the reconstructed Proto-Iranian form **magh-u* (1990: 46) is more relevant. Further examples of Iranian loanwords in Sinitic would go a long way toward validating this one; the other example discussed by Mair, *ch'e* (< *$kl^{y}ag$) 'chariot, wheeled vehicle', compared with IE *$k^{w}\acute{e}k^{w}lo$- 'wheel', is analyzed entirely differently by Lubotsky (385), who parallels it with the synonym *jū*, glossing them 'wagon, vehicle', and derives them both from Chinese verbs meaning 'abide, stay'.

Under item (6), Mair refers to this discussion and to Mair 1996 for "Indo-European" loans into Chinese (1996: 37 discusses 'wheat' but with no indication of which IE language might be the source of the loan). But that's not good enough: since the putative locations of Proto-Indo-European and—at the epoch of PIE—what? Proto-Sino-Tibetan? are far apart, any such loans must be from some Indo-European daughter language, and that daughter language should be identifiable (if only as a coherent substrate, as with Prehellenic in Greek or Cimmerian in Slavic).

Mair begins "*Die Sprachamöbe*" by insisting that the notion of "Indo-European peoples" and the search for their homeland is legitimate. But it isn't: what is legitimate is the search for where Indo-European was originally spoken—which is not the same question. He then makes bizarre claims about the origin of Sinitic—denying Sino-Tibetan, asserting "kinship" with Austronesian and "influence" from Austroasiatic and Altaic "during the past two millennia" (836), all without evidence, argument, or references. Somehow this legitimates the similar statement: "PIE may have arisen as the result of a concatenation or convergence of elements from Uralic, Old European [what?], Caucasian, Semitic, and other languages" (836). But what does this do to the just-asserted notion of "Indo-European people"? And why are Sinitic and Indo-European—just the two phyla Mair happens to be interested in—so privileged? Why do Uralic et al. not get to be such *Mischsprachen*?

Avoiding all such questions, and unfortunately stimulated by Mallory's *Kulturkugel*, Mair invents the "*Sprachamöbe*" or "language amoeba" (why in German?), and in an extended metaphor that is considerably more uninterpretable than Plato's metaphor of the cave, writes of pseudopods, cannibalism, a nucleus,[36] verboplasm, fission, and astonishingly self-aware entities. Since the only referents of the metaphor are either languages or speech communities, this last property is the most amazing.

Be that as it may, the essay ends with a series of nine maps of the spread of Indo-European across Eurasia, dated 4200, 3700, 3200, 3000, 2500, 2000, 1500, 1000, and 100. It is unfortunate that they follow Ringe et al.'s rather than Hamp's conclusions, so that some very bizarre suggestions appear in the intermediate maps, because they incorporate one very important feature that I would like to leave unmentioned until the appropriate place in my own synthesis of the presentations in this work.

What, then, can the linguist extract from this massive compilation? A fairly clear picture of the spread of Indo-European into and across Asia, it seems to me. (The prehistory of the other principal phyla of the region, Uralic,[37] Sino-Tibetan,[38] and Altaic, has been neglected here.) The

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[36]A more sober version of the notion of a language's nucleus—she calls it "locus," saying that innovations radiate therefrom—is Nichols 1997. For her, the Indo-European locus is far to the east, in southern Kazakhstan, Kyrgysztan, and Tajikistan (map, 135). No archeological correlates for this speech community are offered; yet "the locus of the IE spread is a theoretical point representing a linguistic epicentre, not a literal place of ethnic or linguistic origin, so the ultimate origin of PIE need not be in the same place as the locus" (138), and Yamnaya/Pit Grave is accepted as that place of origin. Nichols (1998) attempts to detail the spread of Indo-European using a number of new concepts. Since she gives archeological correlates for some of the moves she proposes, it is safe to assume that where she doesn't, there aren't any; so the reason behind the basic assumption that Indo-European spread from the far eastern steppe seems to be no more than that Iranian, Turkic, and Mongolian did so subsequently. Mallory (this volume, 195) offers the very cogent objection that before Andronovo (ca. 2000 B.C.E.), "we have little evidence of the existence of these steppe or desert dwellers"; cf. Christian 1998 ch. 5. Her view of the familial structure of Indo-European, too, seem badly outdated—Hamp 1990 is cited in one context, but ignored overall; her claims of lack of subdivisions would not survive consultation of, e.g., Hamp 1994.

[37]Uralic is touched on by Ashikhmina 1997 and Koryakova 1998.

[38]From van Driem 1998 and Higham 1998 one receives the impression that East Asian archeology has lagged far behind North Asian archeology, and attempts at correlation of linguistic and archeological history are rudimentary at best. Van Driem's maps are devoid of physical features, so it is very difficult to relate his large ovals demarcating cultural horizons to the more detailed maps usual in such articles; also, he espouses a deviant view of

scenario might as well begin with the Black Sea flood event, whose absolute dating to ca. 5600 B.C.E. is determined by several corroborative means. It did not inundate an enormous area—the two northern bays of the present Black Sea, amounting to the extent of the state of Florida.[39] It correlates, though, with the appearance of new cultures along many of the river valleys of Europe, and one might expect a similar effect to the east as well. This date, however, is too early for the initial dispersion of Indo-European (Mallory 1997: 109–10)[40]—but perhaps the present-day Sea of Azov was the homeland for a late stage of Nostratic, for a possible common ancestor of Indo-European and Uralic.[41]

I have objected strenuously to the assumption of a necessary correspondence between language and "race." It is another matter, though, to recognize the close connection between language and culture; Fischer (1989) shows such connections in great detail between the linguistic

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the branching of Sino-Tibetan making it unclear where he thinks the earliest form of Chinese might have been spoken.

Higham, an archeologist, suggests that Austroasiatic radiated southeastward and southwestward from Yunnan/Sichuan (associated with rice-growing cultures), but he has a naïve view of long-range linguistic proposals: Schmidt in 1906 suggested a relation between Austroasiatic and Austronesian. Benedict in 1942 suggested a relation between Austronesian, Miao-Yao, and Tai-Kadai, "which would *ipso facto* link Miao-Yao and Tai-Kadai with Austroasiatic" (1998: 107). Such proposals, however, are not additive! Austroasiatic does not figure in any present-day Southeast Asian macrofamilies (Matisoff 1983, 1991), so Higham's further tracing of Austroasiatic back to the Yangtze valley does not find linguistic support.

Matisoff places the original location of Sino-Tibetan "somewhere on the Himalayan Plateau" (1991: 470) but offers no suggestion of the subsequent travels of the Sinitic branch. It needs to be in contact with Austroasiatic, though, because of a group of loanwords in Chinese, including at least some of those figuring in the "twelve earthly branches and ten heavenly stems" of the Chinese time-cycle (Higham 1998: 109–10, citing Norman and others).

[39]William Ryan, lecture at the University of Pennsylvania, 14 October 1999.

[40]Mallory 1997 beautifully presents criteria for evaluating proposals for correlating archeological and linguistic speculations on the homeland and dispersal of Indo-European.

[41]The possibility of familial connection between these two phyla is now recognized by Bernard Comrie (1998); while he does not admit Altaic at the same level of relationship, he now seems to accept the latter as a legitimate unit rather than merely a typological association.

and cultural characteristics of the four principal dialect/culture regions of the United States and the counties of England from which their original settlers came; and these distinctions remain powerfully evident in modern America.

Stepping now into the pages of this volume, Kuzmina, Anthony, Mallory, and Parpola all agree that the Yamnaya/Pit Grave culture corresponds to the Proto-Indo-European–speaking community.[42] Attempts are made to connect Tocharian with Afanasievo, but this does great violence to the linguistic evidence of the relationship of Tocharian with the European branches of Indo-European, and certainly not with the other families that ended up in the east, Anatolian or Indo-Iranian. The solution for Afanasievo, it seems to me, is the one hinted at in Mair's closing essay: on maps II–VII (848–53), dated 3700–2000, he plots "pseudopoda" of Indo-European reaching to the west, labeled XYZ, defined as "A series of cultures such as Andronovo and Afanasievo whose languages cannot readily be determined." Since the consensus is that Andronovo represents (Indo-)Iranian speakers, and since the rest of Mair's plots derive from Ringe et al.'s defective tree and datings, his maps cannot be taken seriously overall. But

> Dozens of languages must have been spoken in the ancient Near East [and, *a fortiori*, elsewhere], and most will never be heard again. Each distinct population would have had its own tongue; a population could be as small as a single village, or as large as a plain that allowed regular communication across its full extent. Some of these languages were similar to languages still used today: their speakers or speakers from neighboring communities who spoke slightly different varieties of the same language left descendants whose communities underwent no major upheavals through history, or who preserved an older language in a sacred or scholarly tradition. Some—most—of the ancient tongues must have left no living relatives, and nearly all will have disappeared without a trace. (Daniels 1995: 81)

There is no reason to believe that every branch of Indo-European has survived to the present. I

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[42]Krell 1998 provides useful cautions against taking Marija Gimbutas's equation of "Kurgan culture" with that of Proto-Indo-European-speakers too seriously, but Yamnaya/Pit Grave is only a small part of what Gimbutas assigned to Kurgan (e.g. 1970: 156) and is rather later than what her hypothesis requires.

would thus prefer to assign Afanasievo to speakers of an Indo-European—or other—language that simply fell out of use, for any of the reasons that languages go out of use.

The spread of Andronovo correlates with a period of global cooling around 2000 B.C.E., as described by Hsü. The next period of cooling, then, would be the stimulus for Tocharian to arrive on the scene much later—around 800 B.C.E., in accordance with the redating of some of the material reported by Barber and Good—consistent with its picking up loanwords from Iranian languages in four stages, consistent with the textiles made by its speakers resembling closely those of contemporary or slightly earlier Europe, and consistent with its appearance as the probable language of the Yuezhi. As far as I can tell from the articles in this volume, no information is available on the Hami/Qumul site that might enable archeologists to associate it with whatever cultures were found in the Eurasian steppes around 800 B.C.E.

As for the "mummies," the association of the earliest group with ephedra makes the link with (Indo-)Iranian virtually incontrovertible. There is as yet no evidence that the Tarim Basin was inhabited much before the era of the earliest corpses; it may not yet be clear how extensive at successive periods the lake was that filled the basin—perhaps there was no land there to be occupied. Placed in the context of the occupation of Inner Eurasia as a whole, the Tarim Basin is quite the backwater: pastoralists arriving from the west perhaps came upon this territory that would suit their needs, occasionally to be encountered by people from the east. The "mummies" were known to the author of the latest historical survey of the region, who accords them a single paragraph (Christian 1998: 105; he had seen the *JIES* articles, but not this volume): they are simply the easternmost exemplars known so far of the eastern steppe pastoralists. Christian is sensitive to environmental considerations, but barely mentions language, and I have been able to find no archeological speculations about the prehistory of Altaic[43] and little on Sino-Tibetan.

[43]"These languages [are] originally restricted to Siberia. ... The Turkic- and Mongolian-speaking peoples became mobile and started expanding westwards only after they had adopted horsemanship from Iranian-speaking nomads in the first millennium BC" (Parpola 1994: 126).

Thanks to the present volume, and Victor Mair's other efforts in stimulating the study of "the mummies of Ürümchi," the same cannot be said about the eastern representatives of Indo-European. He is to be warmly thanked for making accessible new Chinese work pertaining to the topic, and overall for the wealth of information and speculation brought together here.

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Reviewed by Peter T. Daniels
New York City

This review was occasioned by the publication of an English translation of Ouaknin's meditations on the alphabet, but it provides the opportunity for an assessment of a number of recent books (all now with paperback reprints) on writing intended for a general readership.

Ouaknin's is just the latest of a genre of art books containing pretty pictures and an unfortunate text. In this case, the pretty pictures are limited to a few drawings of ancient inscriptions, frequently repeated, often with the relevant letter highlighted in color, as well as some fairly dubious examples of modern calligraphy. (The book is an awkward shape and bound in a way that resists the reader.) After a short history of writing (taking far fewer words than its extent, pp. 18–73, would suggest; the list of Thai letters, p. 71, is upside down), come a brief attempt at script typology (which misses the essentially phonographic nature of Chinese characters), a look at "aleph," and a stab at the difference between the Phoenician and Greek scripts.

The heart of the book, though (113–338), is a series of 23 chapters (most of them ten pages) arranged by roman letter that give, in order: a giant Bodoni capital and the supposed pictographic origin of the letter, along with an epigraph that generally seems to have nothing to do with the letter at hand; a description and illustration of the development of its shape from "Proto-Sinaitic" to modern Hebrew, Greek, and Roman; a "Summary Table of [e.g.] 'M' Derived from *Mem*," which is a melange of historical data, folk etymology, and kabbalistic mysticism; and finally a spread containing extremely shaky calligraphy of (left) the modern Hebrew cursive and (right) the roman capital forms.

This is followed by a brief chapter making the mystical claim that the pictographic origin of a letter somehow imbues the words it is used in with some of its essence.

Rabbi Ouaknin has sometimes been betrayed by his translator, who seems to have chosen to guess rather than to check: there is a chart of "Arabic of the Margrave" on p. 58 (for "Maghreb"); "Hebraic," used as the adjective for "Hebrew," throughout; but "Mohajo Dao, Haroppa" (p. 30) can't be explained by passage through French.

An earlier example of a book with similar problems (but much nicer pictures, and much nicer to hold) is Robinson's *Story of Writing*. This has all the hallmarks of a made-for-TV tie-in product (author a BBC producer, every topic confined to a single two-page spread, broad but superficial acquaintance with the subject), but apparently no such companion program was produced. It is not always clear what principle the author has followed in sequencing topics; some, such as "Sign Language" (38) seem quite at sea. The account of the decipherment of Egyptian hieroglyphics (20–35), though simplified, is not inaccurate; but that of the decipherment of Mesopotamian cuneiform (70–77, followed by much irrelevance on the cuneiform civilizations) is deeply flawed, which is disturbing, since one of Robinson's consultants is the British Museum's Irving Finkel, who years before had pointed me in the direction of Edward Hincks's achievements in that realm and surely directed him to the relevant publications.

The problem with Robinson's book is that he has learned widely, but not deeply. He rightly observes that (among phonographic scripts) "English spelling represents English speech sound by sound more accurately than Chinese characters represent Mandarin speech; but Finnish spelling represents the Finnish language better than either of them" (14); but he does not inquire why this is so: he does not note that English has been diverging from its orthographic model (through ordinary sound change) far longer than Finnish has been diverging from its. "Ideally, alphabetic spelling should represent the phonemes of a language" (40). This is often asserted—but is it true? Would the English-speaking world be better off if each locality wrote its own version of English so that there were as many written dialects as there are spoken dialects? What condition would English literature be in if all writing were as difficult for outsiders to interpret as the poems of Burns, the tales of Dickens, or the novels of Twain? "If the Egyptians had an alphabet nearly 5000 years ago, why did they need all the other signs in the hieroglyphic script?" (97). But they didn't: they had 200 or so phonetic signs, denoting 3, 2, or 1 consonant. The 24 uniconsonantal signs *never* constituted an "alphabetic subsystem" of phonetic hieroglyphs. The account of the decipherment of Linear B (114) fails to note Michael Ventris's crucial observation: that certain sign sequences occurred only at specific sites—so that place names could perhaps be correlated with specific writings. The same insight was essential in the decipherment of Maya glyphs and Luvian hieroglyphs as well. Nearly every assertion about Hebrew and Aramaic language and script (172f.) is wrong.

Robinson's book could only be used for instruction by a teacher who can take the class through it page by page, correcting the errors while posing the questions the author has hinted at but has perhaps not known how to ask.

My last example of this genre is Jean's *Writing*. Like all the volumes in the compact series of Abrams Discoveries, it comprises 128 pages of pretty pictures, among which the text seems fairly incidental, and 80 pages of "Documents," here devoted almost entirely to calligraphy of various kinds. The text, to be kind, is execrable. Confining our examples to the chapter on "The Alphabet Revolution," we may note: "Aramaic writing and language were to have a major impact on history, since it was in this language that several books of the Old Testament were written" (53f.) [small portions of two books are written in Aramaic]; "The [Arabic] alphabet consists of eighteen letters, which, when combined with their various marks and accents (used to indicate the vowels), comprise twenty-nine" (58) [there are 28 letters, several of which have the same line-shape and are distinguished by dots; these have nothing to do with the vowel markings]; "All writing systems derived more or less directly from Phoenician script transcribe consonants only. ... The Greeks therefore had the idea of borrowing certain signs from the Aramaic alphabet to transcribe their vowel sounds, choosing those signs that represented consonants that did not exist in the Greek language" (60) [while the details remain unclear, it is certain that "the Greeks" did not add Aramaic vowels to a Phoenician-based script; nor is it likely that the process was the result of a deliberate decision]; "This [Greek] alphabet could be written either in uppercase ... or lowercase letters" (61) [not until well over a millennium after the period under consideration].

"Indian writing first appeared in the 3rd century B.C., when the edicts of the great ruler Asoka (272–231) were committed to stone. Following these inscriptions, two principal writing systems appeared in the Indian subcontinent: Kharosthi and Brahmi" (67) [Brahmi *is* the script of the Asoka inscriptions, and Kharosthi predates it slightly]; "A totally alphabetic system, Brahmi script contains both consonants and vowels. This has led scholars to conclude that these scripts did not originate locally but can ultimately be traced back to the Phoenician alphabet" (67) [this misses the special nature of Indian scripts with respect to consonants and vowels, and the reason given has nothing to do with why a Semitic (more likely Aramaic) origin for Brahmi is sought]; "Panini ... was able to describe the exact functions of the consonants and vowels in Sanskrit, the Indian 'writing of the gods.' This is not so surprising since Indian scripts are integrally alphabetic and show a highly structured phonetic system" (69) [Sanskrit is not "writing" of any sort, but a language, and Panini's work was done long before any script for Sanskrit was introduced]; "The main languages of India (which are usually read from right to left)" (69) [wrong!]; "Words are normally arranged around a "power," a form of large horizontal bar that links all the letters to each other above an imaginary line" (69) [in only a handful of the Indian scripts]; "The scripts used in present-day Tibet and other southeast Asian countries" (69) [!].

Execrability of quite a different sort is exhibited by Shlain's *The Alphabet versus the Goddess*. Shlain puts forward the thesis that literacy (a "left-brain" endeavor) is overall a Bad Thing for humanity, adducing dubious observations from a wide range of pop psychology and such. The book has received serious attention from at least two reviewers (M. O'Connor, *Written Language and Literacy* 3 [2000], to appear, and Simon Goldhill, *Times Literary Supplement* 27 Aug. 1999: 32); a couple of sentences may be quoted from the latter regarding the book as a whole: "Its sweeping central thesis combines wilful fantasy, *folie de grandeur* and cloying sentimentality, and the whole edifice is buttressed by a pretence of scholarship at times ludicrous, at times merely trivial and ill-informed. ... Deep ignorance about the ancient world, about problems of historiography and about the work of anthropologists and historians on literacy, allow massive over-simplification as well as simple mistakes. This ignorance is both endemic to and necessary for the argument."

In contrast, upon reading the book I found myself wondering whether the author would maintain that his thesis remained valid even if the basic errors were corrected. I posted the following message at the book's website:

Dear Dr. Shlain,

> Don't you think that when you wrote a book about the supposed effects of literacy on culture and civilization, you ought to have checked your assertions about writing and its history for accuracy?
>
> Not one of the persons listed in your acknowledgments, nor any of the references listed in your bibliography, is a recognized authority on the history and nature of writing systems. Quite aside from my own edited book, you have not cited any of the standard works, such as those by Diringer, Jensen, Driver, or Naveh (to mention only books in English).

Virtually nothing you say about cuneiform, about Egyptian hieroglyphs, about Chinese language or writing, or about the history of the alphabet itself is factually correct.

Would these inaccuracies have any consequences for the credibility of your thesis?

Yours,

Peter T. Daniels co-editor (and principal contributor),

*The World's Writing Systems* (Oxford University Press, 1996)

This message appeared the same day:

Dear Mr. Daniels,

Your criticism of my book leaves me puzzled. You state that I made "factual errors" in my chapters on cuneiform, hieroglyphics, and Chinese writing and you pose the question as to whether these could have affected my thesis. Since you never mention what these "factual errors" are it is impossible to respond. I am reminded of Nietzsche's statement that "there are no facts only differing perspectives on facts." Surely you would grant me the benefit of the doubt that I indeed did check with recognized authorities in each field, several times, to make sure that I eliminated all possible factual errors. Alas, when someone who is not an expert in a particular field of endeavor wanders into that constricted circle inevitably there will be errors. However, I did not achieve the position of chairman of my division of surgery and associate professor of surgery at the medical school by not checking and rechecking my "facts."

You failed to comment as to whether you felt my thesis had any value, was original, or was worthy of possible consideration by those guardians of the "facts" of whom I presume you consider yourself to be among. You fail to mention whether you learned anything new or whether you found it interesting to combine recent advances in neurosciences with ancient historical enigmas. Do I understand you to say that someone who is attempting a synthesis of many different fields of knowledge should be inhibited from doing so because they are not a "recognized expert" in each field?

As for the bibliography, I am familiar with the authorities you mentioned and had several books in the bibliography before the major editing. I then decided to include only those that I mentioned in the text rather than expand the bibliography to include everyone influential in the numerous academic disciplines I covered.

There was a young patent official in Bern who at the age of 26 submitted a paper to a prestigious academic journal. His bibliography contained not a single name of the "recognized authorities" in the field. Einstein's Special Theory of Relativity changed our perceptions yet he did not cite authorities. Should he have not written his article? While I am not comparing myself to Einstein, I think academics that criticize others for venturing into their field of expertise would have to exclude a great number of significant advances in the arts and sciences. Copernicus did not have his "facts" correct but he still was right. Kepler did not have his "facts" right but he still made momentous discoveries. I would ask you to cut me a little slack concerning the "facts" and look beyond to whether the thesis as a whole have validity.

My reply:

Clearly this is not the venue in which to post a page-by-page listing of the factual errors concerning writing systems that appear throughout the book. It's hard to imagine what sort of response would be possible other than, "Sorry, I'll correct it in the paperback reprint."

Would you mind naming the Assyriologists, Egyptologists, and Semitists who found nothing objectionable in your formulations of statements regarding cuneiform, hieroglyphs, and Semitic scripts? None appear in your Acknowledgments.

I'll take only the simplest misrepresentation, the description of Egyptian writing. (The two pages on Chinese language and writing, 181-82, contain the most concentrated collection I've ever seen of every myth that's ever been put out about Chinese. John DeFrancis refuted every one of them in his 1985 book *The Chinese Language: Fact and Fantasy*, but actually Peter Stephen Duponceau had already done so in 1838.) On p. 53, you say "Each picture/glyph served three functions: (1) to represent the image of a thing or action, (2) to stand for the sound of a syllable, (3) to clarify the precise meaning of adjoining glyphs.... Hieroglyphs were a surprisingly expressive writing system."

(1) is simply false. Hieroglyphs do not represent images; they are images. They represent things; they sometimes stand for actions by metonymy.

(2) is false. The phonetic signs (a subset of the entire system) do not at all stand for the sound of syllables; they stand for the sound of consonants -- one, two, or three consonants. This was true from the earliest examples we have of hieroglyphic writing, most notably the Narmer palette, but even in a few items that are older than that.

(3) could perhaps be said of the subset of signs that are used as phonetic or semantic determinatives, but "specify the reading" would be more accurate than "clarify the meaning." What must always be recalled--for Chinese as well as for Egyptian -- is that the signs do *not* directly represent meanings; they represent language, the sounds of language. They stand, some of them, for words, not for ideas. (And I don't see what's "surprising" about the fact that the writing system can express any and all words, and hence all possible utterances, in the language. That, after all, is why it's writing and not something else.)

At the bottom of p. 53, "While hieroglyphs were able to express most ideas" is a misunderstanding of what they do. They don't "express ideas" at all; they record language. They are not "a language based on pictures." It is not the case that (p. 54) "the Egyptians invented twenty-five icons to represent each of their language's spoken consonants" -- there are a few hundred signs used phonetically (for the sound of their consonants alone), most of which denote two consonants, many of which denote three, and some of which denote one. At no time in their over 3,000 years of use were the monoconsonantal signs ever used alone, as a system, or as a subsystem of hieroglyphs. The Egyptians did not have the principle of the alphabet. They did not develop the first rudimentary alphabet.

Bottom of 57: The account of hieratic is incorrect. It did not (begin to) supplant hieroglyphs. Hieratic writing is nothing but a cursive equivalent of hieroglyphs; any hieratic text can be transposed, sign for sign, into hieroglyphs (and vice versa). At no time did "Egyptian writing [pass] from icons based on images to symbols based on abstraction" (p. 58).

The description of Coptic writing (p. 259) is also completely incorrect. "They" (who?) did not "merge elements of demotic with alphabetic principles to form Coptic, an Egyptian original"; rather, Egyptian Christians began writing their language with the Greek alphabet, to which they added seven letters (their shapes taken from Demotic characters) for consonants that occur in Egyptian but not in Greek. It's just the same principle that a number of other Christian communities followed in devising their own alphabets about the same time. The Coptic alphabet did not supplant the native Egyptian scripts "almost overnight"; the pagans continued to use their scripts alongside (or, presumably, while ignoring) the Christians who used theirs.

Egyptian isn't my area of specialization; when you draw near to Aramaic, the assertions are sometimes more amusing. I certainly get the impression from chapter 9 that you are asserting that the alphabet did not develop out of any existing writing system, but that it was a gift from God on Sinai (e.g. 78-79). I'm not sure what date you assign to this event, but surely it's later than the pre-1450 you seem to claim there were alphabetically literate "Aryans" in India (p. 161)? (It's especially amusing that you would have "Aryans" conquering the "Sanskrits" of the Indus Valley civilization ... in view of the fact that Sanskrit is the classical version of the (Indo-)Aryan family of languages. Most competent scholars converge on the Dravidian family as the most likely known language family to represent the language of the Indus seals.) Indologists are generally agreed that the Brahmi script was invented just about the time of its earliest attestations, ca. 250 B.C. The great grammatical works of Panini were created some centuries earlier, by a scholar who *did not know writing*.

I assume you're better at surgery than at philology.

As for the "advances in neurosciences," I can only say that Prof. Lise Menn of the University of Colorado (perhaps America's leading neurolinguist) was less than impressed with your pop-psychology version of "left-brain/right-brain" and was utterly bewildered by your claims about the functions of rods and cones.

[snipping ritual mention of Einstein, Copernicus, Kepler]

What I'm asking is whether the thesis holds even if what you have to say about the history and nature of writing systems is pretty much entirely wrong. I don't see any argumentation: at the end of each chapter, I see a statement along the lines of "I posit that it was the existence of alphabetic literacy that caused [this tragic event] to take place." But I see no consideration of literate societies where tragedies didn't happen, or of tragedies that happened in the absence of literacy.

I also find no acknowledgment of the existence of a considerable literature on the actual phenomenon of literacy: books have been written that attempt to quantify the percentage of literate persons in the general population at a number of times and places, and how it correlated with age, class, gender, etc. Perhaps if such data were taken into account, something specific could be said as to whether this or that cluster of literacy was related to this or that tragic event.

In short, I find nothing in your book to persuade me of your thesis. You may have noticed some correlations of increasing literacy and tragic events; but until you have considered the details of who was literate and how much so, you will not have shown even that there are patterns to be accounted for by your thesis.

Once you can show such patterns, you will then need to show (in detail, and with accuracy) how literacy, and specifically alphabetic rather than syllabic or logosyllabic literacy, is the cause of the patterns.

Thank you for your attention.

As best I can determine, there was no response.

5/3/99

Having now read *The Alphabet versus the Goddess* all the way to the end, I am now in a position to identify an example where misstatement of fact has a profound effect on the viability of the conclusion drawn. I am not referring to elementary boners that would not have been made by anyone familiar with the history of modern inventions, such as

"Lithography, the reproduction of images by means of engraving, was perfected in the 1820s virtually at the very moment photography superseded it in importance" (p. 383) [lithography was successful precisely because it is *not* engraving; it was discovered in 1796 and began to be used by artists for printmaking in the 1820s; Daguerre's process of fixing images was announced to the public in 1839]

"Then, in 1873, an American, Philo Remington, invented the typewriter" (p. 391) [Remington was a manufacturer of guns and sewing machines and other such intricate machinery, who set aside a sizable portion of his factory to produce the typewriter invented by Christopher Scholes]

"In 1887, Thomas Edison's prolific laboratory developed a technology that combined electromagnetism and photography. An electric motion picture projector unspooled a series of negatives past an intense light, which shone through them" (p. 392) [early movie projectors were hand-cranked, and the projected film did not bear negative images]

Nor at this point am I concerned with specifically linguistic questions, such as the rather bizarre assertions regarding grammatical gender on p. 387, or the origin of Esperanto on p. 427. Refutations and counterexamples are too easy to gather to bother with here. Rather, the astonishing error I am referring to is found on p. 421: "By the middle of the nineteenth century, Christian missionaries had adapted a form of Indian script to Southeast Asian vernaculars." This is sheer fantasy. The indigenous scripts of Southeast Asia were the result of hundreds of years of development from the Indic scripts. Small objects bearing inscriptions in Indian scripts have been found in Southeast Asian sites dating to the 2nd-5th c. C.E., and Sanskrit inscriptions go back to the 3rd century in southern Vietnam. Around 400, an inscription was written in Cham (an Austronesian language). The distinctive Southeast Asian scripts go back to at least the 6th century (Khmer), Burmese writing dates to the 12th century, and Thai to the 13th. (While the details are not widely published, the outline of the development of these scripts is readily available in books like those by Diringer, Jensen, and Daniels & Bright.)

Thus the claim (p. 422) that "after each of the other Southeast Asian nations adopted their new alphabetic language, they became haunted by extremes in human behavior" is utterly absurd -- the rises of Pol Pot in

Cambodia, of Thai prostitution, and of Burmese economic decay have nothing to do with the adoption of a writing system of any kind.

The assertion (p. 423) "I submit that the essential character of the twentieth-century Southeast Asian was utterly transmuted by the rapid spread of alphabet literacy in the nineteenth century" is thus completely void.

5/25/99

Dear Mr Daniels,

You, sir. have too much time on your hands. I don't think that your comments about engraving, lithographs, or hand cranked early movies do anything to discredit my thesis which I notice you have never addressed. The question you must ask yourself is why you have such an emotional investment in proving me wrong. Why is the language you use so hyperbolic? Sort of like those religious attacks I write about that were so destructive. As to your comments about the great literate tradition of Southeast Asia. Could you name a poet, writer, painter, philosopher, or religious leader that originated in these cultures of equal status to the top ten of the traditions of the literate cultures of China, Japan, or Korea? I stand by what I wrote. These SE cultures did have an early literate tradition but then passed into a long dark age and by the early 18th century had a very high degree of illiteracy.

After this particularly dubious example of cultural supremacism, it is a pleasure to turn to the one book in our small survey that constitutes an exception to the trend. Drucker's *The Alphabetic Labyrinth* is not a history of writing, but an account of the *uses* of writing beyond simple literacy—mysticism, philosophy, calligraphy, advertising, display, and much more—in the Western world, as well as its encounters with alien writings of the mysterious Orient. It is cultural history of the best sort, not, perhaps, to be read straight through, but worth dipping into at any point.

Richard Salomon, *Ancient Buddhist Scrolls from Gandhāra. The British Library Kharoṣṭhī Fragments.* With Contributions by Raymond Allchin and Mark Barnard. Forward by His Holiness the Dalai Lama. Seattle: University of Washington Press, 1999. pbk. $40.00.

Reviewed by Daniel Boucher
Cornell University

There are occasionally in Buddhist studies those happy conjunctions in which new discoveries are met by the right scholars. One thinks immediately of the Dunhuang manuscripts, and for us, the fortunate circumstances that led Paul Pelliot to obtain and begin studying them. We can be likewise encouraged that the recent acquisition of a number of very early *kharoṣṭhī* scrolls by the British Library has found its way into the capable hands of Richard Salomon. It is difficult to imagine a better guide to the problems and promises of these ancient Buddhist manuscripts -- a supposition more than confirmed by this inaugural volume to a planned series of integral studies.

The British Library *kharoṣṭhī* manuscripts, acquired in 1994 through the assistance of an anonymous benefactor, are in many ways comparable to the finds of the Dead Sea scrolls and the Nag Hammadi corpus. In all three cases these new manuscripts filled out our picture of local religious communities, and in the process, challenged old misconceptions of the great traditions. However, Salomon's care in making these manuscripts promptly available to the public assures us that the British Library scrolls will not become mired in the problems of discovery, acquisition, and scholarly study suffered by the Nag Hammadi corpus.[1]

The British Library manuscripts consist of 29 fragmentary scrolls in *kharoṣṭhī* script / Gāndhārī Prakrit, together with 5 clay pots and 26 inscribed potsherds. Also among the finds is one manuscript fragment with *brāhmī* script as well as one *brāhmī*-inscribed potsherd. Salomon provides a tentative catalogue of the manuscripts on pp. 42-53 and a survey of the pots and potsherds in an appendix (183-247). Although the *in situ* context of the scrolls is not known, a number of internal and external clues point to them as originating from the vicinity of northwest Pakistan or eastern Afghanistan. It is almost certain that these manuscripts were deposited together as a unit -- very probably within pot D which was acquired with the manuscripts. Their interment has the appearance, in Salomon's opinion, of a Buddhist genizah, a ritual burial of "dead" manuscripts that could not be casually discarded. The insertion of the interlinear notation *likhidago* ("copied") on five of the manuscripts all but proves that once the scribes had recopied worn out manuscripts, they so designated the old ones for disposal.

A comparison with other materials in the same script -- the extant *kharoṣṭhī* inscriptions and coin legends, the Khotan *Dharmapada*, and the Niya documents -- leads Salomon to date these manuscripts paleographically to the first century of the Common Era, a claim buttressed by other historical and linguistic data within the texts. What we have then in this collection is an essentially random selection of discarded texts from a first-century Gandhāran Buddhist monastery. But the real value of this find is that it once again requires that we ask difficult questions about canons and canonicity in early Buddhism. The modern printed *tripiṭaka* in Pāli, and to a lesser extent, the collections in Chinese and Tibetan, have exerted an almost oppressive hold on the scholarly conception of Buddhist textual history. Steven Collins, writing in reference to the ideological use of a closed list of texts in Theravāda Buddhism, has argued for a different approach:

> If we wish to delineate the actual 'canon' or 'canons' of scripture (in the wider sense) in use at different times and places of the Theravāda world, we need empirical research into each individual case, not a simple deduction from the existence of the closed *tipiṭaka* produced by the Mahāvihāra. We need more research, for example, historical and ethnographic, on the actual possession and use of texts, in monastery libraries and elsewhere, and on the content of sermons and festival presentations to laity, to establish more clearly than we currently can just what role has been played by the works included in the canonical list.[2]

The British Library scrolls give us an example of such an actual canon: a collection of texts that were used often enough to require recopying and ritual burial. The selection of texts, of which we must have only a very small fraction from this monastic library, is then of some intrinsic interest. What we find is a combination of "canonical" sūtras, drawn largely from a collection paralleling the Pāli *Khuddaka-nikāya*, which were translated--or as some prefer--transposed from another Middle Indo-Aryan language into Gāndhārī, and local productions, such as *avadānas* featuring a contemporary Saka king as the main

[1] On the tangled history of the discovery of these Coptic Gnostic texts, see James M. Robinson, ed., *The Nag Hammadi Library* (San Francisco: Harper & Row, 1978), 21-24.

[2] Steven Collins, "On the Very Idea of the Pali Canon," *Journal of the Pali Text Society* 15 (1990): 104.

character.[3] Among the texts included are those known to be popular at other Buddhist sites in Central Asia from later periods: verses from the *Sutta-nipāta*, especially the *Aṭṭhakavagga* and *Pārāyaṇavagga*, selections from a version of the *Dharmapada*, *avadānas*, and the *Sthaviragāthā* (=Pāli *Theragāthā*). Noticeably absent are any remains of a vinaya recension, also uncommon in our finds from Central Asia.[4] Salomon makes a good case for the possibility that the vinaya was still transmitted via a living *bhāṇaka* (reciter / preacher) tradition and may not have been committed to writing by the first century C.E., at least in Gandhāra.[5]

Also noticeably absent from the British Library fragments is any hint of the movement we call the Mahāyāna. Again, this generally parallels our Central Asian manuscript finds, with a few noteable exceptions, particularly from the vicinity of Khotan. And while Salomon is almost certainly correct that at best this presents a kind of indirect, negative evidence for the hotly contested location or locations of the early Mahāyāna, a number of caveats must be quickly brought to the fore. First, precious though they are, the British Library scrolls are a rather small collection of texts. We have no way to determine, and thus no reason to believe, that they are representative of the full holdings of the monastic library from which they were discarded. Secondly, recent work on early Chinese translations of Mahāyāna sūtras has pointed to the strong possibility, indeed the likelihood, that a number of the Indic source texts for these translations were in *kharoṣṭhī* script and

[3] See pp. 35-39 and 141-151. Salomon refers to the Sakas throughout his book as Indo-Scythians. While common in scholarly writing, this ethnonym is also often used to refer to the Yuezhi and to the Kushans, two groups who only partially overlap. But more to the point, there is nothing "Indo-" about the Sakas other than the fact that they were forced into Indian territories as a result of Yuezhi expansion in the second century B.C.E., and there appears to be no convincing reason to link them to the Scythian peoples who inhabited the steppes north of the Black Sea from the seventh to the third centuries B.C.E and who are referred to at length by the Greek historian Herodotus. Thus, to avoid confusion, I would recommend that in the future this group simply be referred to by their own self-appellation, i.e., Sakas.

[4] See Lore Sander, "The Earliest Manuscripts from Central Asia and the Sarvāstivāda Mission," in *Corolla Iranica. Papers in Honour of Prof. Dr. David Neil MacKenzie on the Occasion of his 65th Birthday on April 8th, 1991*, ed. by Ronald E. Emmerick and Dieter Weber (Frankfurt am Main: Peter Lang, 1991), 133-50.

[5] Recently, Gregory Schopen has suggested that the preoccupation with the writing down of the Buddhist canon as a singular, momentous event in Buddhist history may well ignore a much more casual attitude in Indian monastic law toward such a form of textual preservation. See his "If You Can't Remember, How to Make It Up: Some Monastic Rules for Redacting Canonical Texts," in *Bauddhavidyāsudhākaraḥ. Studies in Honour of Heinz Bechert on the Occasion of His 65th Birthday*, ed. by Petra Kieffer-Pülz and Jens-Uwe Hartmann (Swisttal-Odendorf, 1997), 571-82.

possibly Gāndhārī Prakrit.[6] Indeed, our *kharoṣṭhī* corpus could be expanded greatly as more studies of these early translations become available.

Although intended for the general reader, *Ancient Buddhist Scrolls from Gandhāra* includes several more technical sections, particularly chapters five and six dealing with the material construction of the scrolls and their linguistic and paleographic features. And here again Salomon displays the same acumen that has made him one of the world's leading authorities of Indian epigraphy generally and Gāndhārī studies specifically.[7] Nevertheless, the difficulty of the task before him could hardly be exaggerated: twenty-one scribal hands appear to be represented in the British Library manuscripts in various states of preservation. Only a scholar with Salomon's formidable skills would dare tackle the problems that await.

The material of these cigar-shaped scrolls is birch bark, the nearly universal writing medium of texts in ancient northwest India, eastern Afghanistan, and parts of Central Asia. Salomon speculates that the scroll format may have been inspired by Greek papyri scrolls that could have been a well-known import in the Greco-Bactrian kingdoms of the last few centuries before the Common Era. These new scrolls reveal much about the birch bark manuscript construction process that had previously been guesswork, since now we can see more clearly the seams at which strips were glued together into long scrolls. Salomon

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[6] Here I must respectfully disagree with Professor Salomon that "what most effectively sets off Gāndhārī from all other Indo-Aryan and other Indian languages is the fact that it was written in the Kharoṣṭhī script, whereas all the others have been written, from the earliest times, in the Brāhmī script or its several local variants and derivatives" (3-4). It may be little more than a historical and geographic coincidence that a northwestern Prakrit was typically written in a script derived from Aramaic, once the lingua franca of a region that encompassed Gandhāra. I am reminded here of the remarks on this matter by Gérard Fussman: "Bien que la totalité des textes jusqu'ici qualifiés de gandh. soient écrits en khar[oṣṭhī], il n'y a aucun lien nécessaire entre cette langue et cette écriture. Rien en principe n'empêche de noter en khar. d'autres parlers que la gandh. Il existe des textes khar. en sanskrit, plus ou moins correct. Inversement rien n'interdit de supposer l'existence de textes gandh. en brāhmī: lorsque la khar. cessa d'être en usage au Gandhāra, la gandh. ne cessa pas d'y être parlée et d'y être écrite" *Dialectes dans les littératures indo-aryennes*, ed. by Colette Caillat (Paris: Collège de France, 1989), 439. It is becoming clearer, in part on the basis of evidence Salomon himself has collected, that the *kharoṣṭhī* script was used to write Sanskrit and hybrid Sanskrit in Gandhāra and Central Asia; Niya documents 510, 511, and 523 are among the most obvious examples. As work continues on the early Chinese translations of Mahāyāna sūtras, we may be well advised to hold open the possibility that many of their source texts could have been composed in a version of Buddhist Hybrid Sanskrit that was transcribed in *kharoṣṭhī* script. For an example of one such possibility, see Daniel Boucher, "Gāndhārī and the Early Chinese Buddhist Translations Reconsidered: The Case of the *Saddharmapuṇḍarīkasūtra*," *Journal of the American Oriental Society* 118.4 (1998): 471-506.

[7] The scholarly reader can find an extended discussion of the issues related to the history of writing in India and the languages of Indic inscriptions in Salomon's recent *Indian Epigraphy. A Guide to the Study of*

sees the later shift from the scroll format to the *poṭhī*-style books on birch bark, as found, for example, in Kucha and Gilgit, to reflect a "dissolution of the distinctive features of the Gandhāran linguistic and literary tradition and their replacement by the mainstream classical traditions of India" (107). This shift, not surprisingly, coincides with the adoption of Sanskrit and *brāhmī* script in these texts as well. Here again, Salomon is almost certainly correct. But there is at least one piece of evidence that I know of from a fairly early date that may suggest a slight qualification of this.

In a preface to the Chinese translation of the *Saddharmapuṇḍarīkasūtra* by Jñānagupta and Dharmagupta (601 C.E.), the anonymous writer, presumably a translation assistant to the two Indian masters, describes their reexamination of the earlier translations by Dharmarakṣa (286 C.E.) and Kumārajīva (406 C.E.).[8] The preface writer also tells us that the Indic originals of these two translations were still available to them in a sūtra repository and that they collated these with the translations as well. The most interesting piece of information for our purposes is his description of Dharmarakṣa's Indic manuscript as *duoluo zhi ye* 多羅之葉, *tālapattra* or palm leaf manuscript. I have tried to make the case elsewhere that there are good reasons for supposing that Dharmarakṣa's Indic manuscript of the Lotus Sūtra was written in *kharoṣṭhī* script (though not necessarily Gāndhārī Prakrit).[9] If this proves to be the case, then this may suggest that palm leaves, which are not native to regions north of the Deccan plateau, were in fact imported into *kharosthi*-using regions to serve as Buddhist manuscripts. But it still may be true, as Salomon suggested, that such manuscripts were composed in Sanskrit or hybrid Sanskrit.[10]

The phonological features of the language in these scrolls for the most part follow the same basic patterns we would expect on the basis of our knowledge of other Gandhari documents. Nevertheless, there are interesting distinctions within the corpus between the

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*Inscriptions in Sanskrit, Prakrit, and Other Indo-Aryan Languages* (New York: Oxford University Press, 1998). See especially pp. 42-56 on the *kharoṣṭhī* script and pp. 72-86 on epigraphical Prakrits.

[8] *Tianpin miaofa lianhua jing xu* 添品妙法蓮華經 (T 264), 9:234b-c.

[9] See Boucher, "Gāndhārī and the Early Chinese Buddhist Translations Reconsidered," esp. 499 ff. and Boucher, "On *Hu* and *Fan* Again: The Transmission of 'Barbarian' Manuscripts to China," *Journal of the International Association of Buddhist Studies* 23 (forthcoming).

[10] There is other evidence of palm leaf manuscripts in birch-bark-using territories. Jens-Uwe Hartmann has recently described a palm leaf text of about 30 leaves found among the Gilgit manuscripts, all the others of which are written on birch bark. See his "Studies on the Gilgit Texts: The *Sarvadharmaguṇa-vyūharājasūtra*," *Dharmadūta. Mélanges offerts au Vénérable Thích Huyền-Vi à l'occasion de son soixante-dixième anniversaire*, ed. Bhikkhu Tampalawela Dhammaratana and Bhikkhu Pāsādika (Paris: Editions You-feng, 1997), 135-140.

translationese of the canonical texts (e.g., the *Anavataptagāthā* and the *Khaḍgaviṣāṇasūtra*) and the more colloquial style of the locally composed *avadānas*. This difference is of considerable importance. As Salomon notes, "this Gāndhārī translationese contains numerous grammatical formations and syntactical constructions that are unlikely to have been natural in colloquial Gāndhārī" (139). Such vernacular forms afford us practically our only specimens of the living Gāndhārī language and provide an important benchmark for comparison with texts that were transferred from central Middle Indo-Aryan languages to Gāndhārī and eventually to Sanskrit.[11]

The issue perhaps most related to Buddhist studies proper is the question of the sectarian affiliation of the British Library manuscripts, discussed at length by Salomon (Chapter 8.2, 167-78). The fact that the scrolls were found in a clay pot with a dedicatory inscription to the Dharmaguptakas makes it all but certain that these texts came from a monastery of that school. Since this school has long been presumed to have had a significant role in Gandhāran Buddhism, early Chinese Buddhism, and the transmission of Buddhism along the ancient Silk Routes, it may behoove us to reexamine the evidence in some detail.

First of all, Salomon calls our attention to the fact that even by the time Lamotte published his magisterial *L'Histoire du bouddhisme indien* in 1958, his claim that "[a]ucune inscription ne les mentionne comme secte, Dharmagupta étant toujours un nom propre appliqué tantôt à des laïcs" (582) was already incorrect. The Dharmaguptakas were attested in Mathurā in a *brāhmī* inscription published by Lüders. Until now, only two *kharoṣṭhī* inscriptions mentioning the Dharmaguptaka school had been published: the Jamālgaṛhī stone inscription (also published by Lüders) and the Qunduz vase inscription from ancient Bactria. With the addition of the British Library collection, we now have the inscription on pot D and at least three and possibly a fourth potsherd with inscriptions dedicated to the Dharmaguptakas (Salomon, 175-76 and potsherds nos. 8, 11, 17, and possibly 26). This, together with other recently published finds, greatly bolsters the role of the Dharmaguptakas in ancient northwest India and eastern Afghanistan. But we should

[11] See pp. 138-39. Salomon calls our attention to a small controversy related to the nature of the linguistic transfer between Middle Indo-Aryan languages, namely, whether, as K. R. Norman contends, this was an act of translation between mutually incomprehensible languages, or, as Heinz Bechert argues, a transposition between mutually understood dialects. The matter is not easily resolved, as one can point to instances both of miscomprehended translations and entirely mechanical substitutions between different sound systems. Salomon himself seems to remain uncommitted, as he uses the words "language" and "dialect" interchangeably, in marked contrast to the typical linguistic distinction (see, e.g., p. 110: "the language has been referred to as 'Northwestern Prakrit'," "Gāndhārī is one of the regional dialects of the Prakrit", "as do other MIA languages", "from all other MIA dialects", etc.).

not forget that they are still one among several schools mentioned in our extant inscriptions; others include the Sarvāstivādins, Mahīśāsakas, Mahāsāṃghikas, and Kāśyapīyas.

In order to buttress this claim concerning the prominence of the Dharmaguptakas in Gandhāra, Salomon amasses data from other scholars attesting to the role of this school in the spread of Buddhism to Central Asia and China. Here the evidence is far less certain. First, Salomon follows Franz Bernhard in claiming that the Dharmaguptakas were established in the ancient kingdom of Shanshan: "Probably the strongest single piece of evidence cited by Bernhard (p. 59) is the fact that, according to him, one of the central Asian *kharoṣṭhī* documents from Niya (no. 510) contains six verses which correspond to the concluding verses of the Prātimokṣa-sūtra in the Dharmaguptaka version, implying that its writer belonged to this sect" (167). Unfortunately, Bernhard gives no reference as to his source of such a claim.[12] It has been pointed out several times, including by Bernhard himself, that these verses are parallelled in known versions of the *Dharmapada* and *Udānavarga*.[13] More recently, HASUIKE Toshitaka has shown that these verses are also found at the end of a number of *prātimokṣa-sūtras*, including those of the Mahāsāṃghikas, the Sarvāstivādins, the Mūlasarvāstivādins, as well as the Dharmaguptakas.[14] Moreover, in the opinion of Hasuike, the verses in Niya 510 agree more completely, at least in sequence, with the *prātimokṣa-sūtra* of the Mūlasarvāstivādins. However, on the basis of Bernhard's unsupported claim, Salomon was willing to state:

> At the least, this would show that Dharmaguptaka monks were present in the Buddhist communities of the Shan-shan Kingdom in and around the third century A.D., and since there is no direct evidence there for the presence of any other particular sect at this relatively early period, it is

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[12] "Gāndhārī and the Buddhist Mission in Central Asia," in J. Tilakasiri, ed., *Añjali. Felicitation Volume Presented to Oliver Hector de Alewis Wijesekera on his Sixtieth Birthday* (Peradeniya, 1970), 59.

[13] See A. M. Boyer, E. J. Rapson, and E. Senart, eds., *Kharoṣṭhī Inscriptions Discovered by Sir Aurel Stein in Chinese Turkestan*, Part II (Oxford: Clarendon Press, 1927), 185; J. Brough, *Gāndhārī Dharmapada* (London: Oxford University Press, 1962), 259 (n. 271) and 266 (n. 292); F. Bernhard, *Udānavarga*, Band I (Göttingen: Vandenhoeck & Ruprecht, 1965), 128 (Niya no. 510, a4-5), 241 (510, a3-4), 317 (510, a.1), 353 (510, a1), 357 (510, a1-2), 426 (510, a2-3).

[14] HASUIKE Toshitaka 蓮池利隆, "Shinkyō Niya iseki shutsudo no bukkyō bunken ni tsuite (2)" 新疆ニヤ遺跡出土の仏教文献について (2) [On the Buddhist Literature Excavated from Niya, Xinjiang], *Indogaku bukkyōgaku kenkyū* 45.2 (1997): (183)-(187).

> reasonable to hypothesize that the Dharmaguptakas were the dominant school there (167-68).

Obviously we will need more evidence with greater precision before we can accept such a claim about the Buddhist monastic communities at Shanshan, which in many ways exhibit traits much unlike what we'd expect from Buddhist clerics.[15]

It has also been claimed by Bernhard -- and here Salomon follows him again -- that the role of the Dharmaguptakas in spreading Gandhari Prakrit through Central Asia is supported by their early prominence in China. But once again, the evidence is less than convincing. First, Bernhard has suggested that a third-century Chinese translation of the *Karmavācanā* was rendered from a Gāndhārī original and was affiliated with the Dharmaguptakas. But as Hisashi Matsumura has rightly pointed out, "it is very dubious that Kang Seng-kai translated the *Karmavācanā*. Once it has become clear that the extant two Chinese *Karmavācanā* texts of the Dharmaguptakas were compiled in China, it is entirely meaningless to discuss what the original language of the *Karmavācanā* of this school was."[16]

Secondly, Salomon follows Lamotte and Bareau in asserting that the Dharmaguptaka-vinaya enjoyed the widest acceptance in early Chinese Buddhist monasticism (167). Once more we need to introduce important qualifications. Jacques Gernet has reminded us that "[s]izeable Buddhist communities began to take form only under the Eastern Chin (317-420)," and it was not until the first quarter of the fifth century

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[15] For example, among the documents from the ancient Shanshan kingdom found at Niya, we find injunctions that impose fines on monks who arrive at the *uposatha* ceremony in householder's garb (document no. 489), who give their daughters away in marriage to other monks (no. 418), who owned slaves and kept servants (no. 506) -- in short, monks who in very many respects led lives within the household and not in segregated communities. For a translation of these documents, see T. Burrow, *A Translation of the Kharoṣṭhī Documents from Chinese Turkestan* (London: Royal Asiatic Society, 1940) and also the discussion by C. Atwood, "Life in Third-Fourth Century Cadh'ota: A Survey of the Information Gathered from the Prakrit Documents Found North of Minfeng (Niyä)," *Central Asiatic Journal* 35 (1991), esp. 173-175. Whether such activities of Buddhist "professionals" were common outside of Cadh'ota (Niya) is impossible to determine

[16] Hisashi Matsumura, "Miscellaneous Notes on the Upālipariprcchā and Related Texts," *Acta Orientalia* (Copenhagen) 51 (1990): 69. Bernhard's article has done a disservice to Gāndhārī studies and to Bernhard himself. It is filled with half-baked conjectures, unsupported hypotheses, and outright inaccuracies. Matsumura is almost certainly correct that had a scholar of Bernhard's caliber lived to see this article into publication, he would surely have corrected many of the problems. As it stands -- and this all the more so given the plethora of citations to it -- it fundamentally misleads scholars who may not be able to check his references (when they are provided). Now that our knowledge of Gāndhārī has increased dramatically since 1970, scholars in related fields would do well to cite more authoritative sources (and this most definitely includes the volume under review by Salomon).

that the large vinaya collections were translated into Chinese.[17] Moreover, the greatest monastic law authority in China, Daoxuan, who in the early seventh century founded a school of vinaya studies and wrote extensive commentaries on the Dharmaguptaka-vinaya, tells us that the most influential vinayas in the early phase of Chinese monasticism were those of the Mahāsāṃghikas and Sarvāstivādins.[18] There is some corroborating evidence for this from the travel account of Faxian, who journeyed to India in the early fifth century in search of the vinaya. We find the following record in chapter 36 of his biography:

> From Vārāṇasī they travelled east, returning to Pāṭaliputra. Faxian was originally looking for the vinaya, but in the kingdoms of north India, all of the masters transmitted them orally. There was no book he could copy. Therefore, he travelled afar to central India, and at a Mahāyāna monastery, he obtained a copy of the Mahāsāṃghika-vinaya. When the Buddha was in the world, this was the one observed by the first great assembly, which transmitted the original in the Jetavana-vihāra. The other eighteen schools each have their own teachers. Their general purport is not different; they are dissimilar only in trivial matters. For example, one says "open," another "shut." But this one is the most extensively annotated and complete. He also obtained a vinaya copy of about 7,000 verses; this is the Sarvāstivāda-vinaya, *the very one that is followed by the monks in China.*[19]

The Dharmaguptaka-vinaya does indeed become central to Chinese monastics, but only with Daoxuan in the early Tang dynasty. One suspects that this would have been too late for ongoing Gandharan influence.[20]

Without a doubt the best known argument for a connection between the Dharmaguptaka school, Gāndhārī Prakrit, and the early Chinese translations is the

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[17] *Buddhism in Chinese Society. An Economic History from the Fifth to the Tenth Centuries*. Trans. by Franciscus Verellen (New York: Columbia University Press, 1995), 65.

[18] Gernet, *Buddhism in Chinese Society*, 66.

[19] *Faxian zhuan*, my translation from J. Legge's edition, *A Record of Buddhistic Kingdoms* (New York: Dover Publications, Inc., 1965), ch. 36.

[20] In fact Waldschmidt published a small fragment of a Dharmaguptaka Prātimokṣa-sūtra (*Sanskrithandschriften aus den Turfanfunden* I [Göttingen, 1965], no. 656) in Turkestan *brāhmī* of the fifth or sixth century, and this fragment is clearly Sanskrit, despite Bernhard's attempt to see under it a Gāndhārī original. See Waldschmidt's discussion in "Central Asian Sūtra Fragments and their Relation to the

translation of the *Dīrghāgama* by Buddhayaśas and Zhu Fonian in 413 C.E. Two separate arguments have been made, one attempting to demonstrate that the underlying Indic text of the *Chang ahan jing* (*Taishō* 1) was in Gāndhārī Prakrit, the other linking this text with the Dharmaguptaka school. Early studies by Weller and Waldschmidt, and later by Brough, have attempted to show that the reconstructed pronunciation of the Chinese transcriptions of Indian proper names and Buddhist technical terms that occur in this translation derive from a Prakrit source text that has much in common with, and may be identical to, what we today call Gāndhārī. I have suggested elsewhere that these conclusions have been drawn from a very small body of data.[21] The comprehensive examination of the transcriptions in the Chinese *Dīrghāgama* by KARASHIMA Seishi makes clear that the situation is more complex than generally supposed:

> As we have seen above, the original language of the *Chang ahan jing* is not something that can be simply decided upon as Gāndhārī. When one looks at the particulars, complex aspects emerge in which elements of Sanskritization, Prakrits, and local dialects were harmonized in addition to specific features of the Northwest dialect. We may still be able to call this dialect Gāndhārī in a broad sense, with the necessary proviso that it differs considerably from the Gāndhārī language as reflected in the Northwest inscriptions.[22]

At the very least, the connection between the *Chang ahan jing* and Gāndhārī Prakrit can no longer be asserted without the necessary qualifications, and, in all likelihood, further research.

The second argument, that the Chinese *Dīrghāgama* is associated with the Dharmaguptaka school, has been long championed by Japanese scholars, who have expended huge amounts of energy in determining the school affiliation of the various

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Chinese Āgamas," in *Die Sprache der ältesten buddhistischen Überlieferung / The Language of the Earliest Buddhist Tradition*, ed. by H. Bechert (Göttingen: Vandenhoeck & Ruprecht, 1980), 164-69.

[21] Boucher, "Gāndhārī and the Early Chinese Buddhist Translations Reconsidered," 472-74.

[22] KARASHIMA Seishi, *Chōagonkyō no gengo no kenkyū -- onshago bunseki o chūshin to shite* 長阿含経の原語の研究一音写語分析を中心として [ *Study of the Original Language of the Chang ahan jing -- Focusing on an Analysis of the Transcriptions*] (Tokyo: Hirakawa Shuppansha, 1994), 51-52.

*āgama*s, and, in the West, most notably by André Bareau.[23] Despite its virtually universal acceptance, this position, like others we have examined, is founded on surprisingly little data. Bareau, whose argument is representative of this thesis, takes as "la preuve décisive de l'origine dharmaguptaka du *Dīrgha-āgama*" the hypothesis that the similarities between the narrative of the gift of the mango grove in the Chinese *Dīrghāgama* (the *Mahāparinirvāṇasūtra*) and the account of Bimbisāra's gift of the bamboo grove in the Dharmaguptaka-vinaya are greater than the parallel accounts in the vinaya of other schools (the Theravādins and Mahīśāsakas). Although Bareau notes a few of the structural idiosyncrasies of the respective versions, the crux of his argument is that the Dharmaguptaka accounts agree in making the offering to the Buddha *and* the saṅgha while the Theravādin and Mahīśāsaka versions direct the donation to the saṅgha with the Buddha as its head. This, in Bareau's opinion, reflects "sous une forme condensée leurs idées propres sur les valeurs respectives du don fait au Buddha et du don fait au Saṃgha" (54). While it is entirely possible that something important has escaped me here, I fail to see a significant doctrinal difference in these two statements, especially one that depends on precise language discerned from Chinese translation.[24] Given the widespread acceptance of the Dharmaguptaka affiliation of the Chinese *Dīrghāgama* among scholars of Buddhism, it seems to me that this question deserves another, more thorough look.

We have seen then that the presumed connection between the Dharmaguptakas and both the Gāndhārī language and the Chinese *Dīrghāgama* is not without problems. Ironically, Salomon's new evidence from the British Library collection -- their provenance in a pot dedicated to a Dharmaguptaka monastery and parallels with the Chinese *Dīrghāgama* version of the *Saṅgīti-sūtra* -- does more to link the Dharmaguptakas,

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[23]For a survey of Japanese scholarly opinions on the schools of the Chinese *āgama*s, see Egaku Mayeda, "Japanese Studies on the Schools of the Chinese Āgamas," *Zur Schulzugehörigkeit von Werken der Hīnayāna-Literatur*, Erster Teil, ed. H. Bechert (Göttingen: Vandenhoeck & Ruprecht, 1985), 94-103 and Fumio Enomoto, "On the Formation of the Original Texts of the Chinese Āgamas," *Buddhist Studies Review* 3.1 (1986): 19-30; see also A. Bareau, "L'Origine du Dīrghāgama traduit en chinois par Buddhayasas," *Essays Offered to G. H. Luce by His Colleagues and Friends in Honour of His Seventy-fifth Birthday*, ed. by B. Shin, J. Boisselier, and A. B. Griswold (Ascona: Artibus Asiae Publishers, 1966), 49-58

[24]It has also been noted that an additional connection between the Chinese *Dīrghāgama* and the Dharmaguptaka-vinaya is the fact that both were translated by Buddhayaśas, a Gandhāran monk who came to China in the early fifth century. However, Sengzhao's preface to the Chinese *Dīrghāgama* as well as Sengyou's early sixth-century catalogue make clear that the actual translation, i.e., the rendering of Buddhayaśas' recitation of the Indic text into Chinese, was carried out by the Liangzhou monk Zhu Fonian; see *Chu sanzang ji ji*, T 2145, 55:11b and 63c. Moreover, it should not strike us as remarkable that parallel passages of different texts rendered by the same translators would often appear to be nearly identical.

Gandhara, and the Chinese translations than any of the previous evidence he marshals, none of which is, even collectively, decisive. But there seems to me a much more fundamental question to be asked: why must a Buddhist *nikāya* be affiliated with a particular language? We can assume that individual schools assembled their canons of scriptures -- and here we should reiterate Salomon's observation that "canon" may refer to a far more circumscribed body of literature than we have come to assume -- in one or another of the Middle Indo-Aryan languages. As these schools moved, presumably their canons moved with them. But we also know that these texts underwent change -- be it translation or transposition -- as they moved into new territories. It is highly unlikely that Gāndhārī was the original language of any one school, even the Dharmaguptakas, but was a product, as Salomon rightly contends, of transplantation into the northwest region:

> It should not always be assumed -- though it often is -- that different versions (in terms of language, contents, and arrangement) of a given text necessarily correspond to sectarian divisions or that, conversely, a particular sect will necessarily have a single and distinct version of a given text.... Therefore, simplistic identifications of particular recensions with particular schools may produce misleading results (175).[25]

There seems to be no good reason to suppose that other schools in Gandhāra did not also use Gāndhārī or that the Dharmaguptakas elsewhere in India would not have used other MIA languages or even Sanskrit.

One might even wonder whether the enormous energy expended on establishing "sectarian" affiliations for the *āgama* corpora has been well spent. Even if there were not significant problems and uncertainties with such identifications, the preoccupation with "sectarianism" -- and this is almost certainly the wrong word for the relationship between vinaya lineages (*nikāya*) and their sūtra texts -- exposes a number of scholarly assumptions. First, what would such a match between text and school, even if we could be certain of it, tell us about the text in question? Could we assume, for example, that monks of a Dharmaguptaka monastery in Haḍḍa held the same views, engaged in the same

[25]Cf. J. Brough, *Gāndhārī Dharmapada*, 42-43: "A given language need not have been the exclusive property of a single religious sect. It is perhaps hardly necessary to enunciate a proposition so evidently true. But frequent mentions of '*the* Sanskrit canon' and '*a* Northwest Prakrit canon' may give the impression that there was only one in each language, even although individual writers using these expressions may have intended no such implication. In Sanskrit, canonical works are known of at least

practices, and were preoccupied with the same concerns as Dharmaguptaka monks in Mathurā or eastern India? Much of what is exciting about a find like the British Library manuscripts is that it gives us a rare glimpse, however hazy, of the literature of a particular monastery, at a particular place (albeit here uncertain), at a particular time (convincingly deduced as the first century C.E.). In other words, it's the local character of these documents that captures our attention and justifies the enormous output of energy for their study, not their capacity to be fit into a generic class of Dharmaguptaka texts whose readership is unknown in real time and space.

Secondly, and this point Salomon himself raises, the modus operandi here is that there must be a close connection between *nikāya* and sūtra recension, that monks who recited the same *prātimokṣa* biweekly also read the same texts. There is in the Indian context no way to test this hypothesis. The anthropology of Buddhism in Śrī Laṅka, Southeast Asia, and Tibet shows monasteries to be complex places, inhabited by monks (and frequently by others) of very different persuasions and inclinations. In the absence of an ecclesiastical authority that could have imposed sectarian conformity, it is difficult to believe that premodern monasteries would have been any less complex or regionally distinct.

My remarks above that question the utility of the "sectarianism" dominating *āgama* studies should not be interpreted as a criticism of Salomon, who throughout this volume anticipates just such problems. Quite to the contrary, Professor Salomon has brilliantly illuminated the path to a more historically nuanced approach to the study of Buddhist manuscripts. This monograph, provisional though it may be, will amply repay a reading by anyone interested in Indian Buddhism, early Chinese Buddhism, the transmission of Buddhism through the Tarim Basin, Indian paleography, or Buddhist textual history. Moreover, the publisher is to be congratulated for producing a volume of such high quality at an affordable price. The numerous color plates, maps, and charts are a joy to behold. While there are a few misprints in the volume,[26] they are remarkably few, given the complexity of the task. Let us hope subsequent volumes will meet these same high standards.

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three, and probably four sects; and there is no reason to think that Gāndhārī, if used at all for scripture, would have been more restricted."

[26] For example, p. 42, Senart 1998 should be 1898; p. 111, on the chart of the *kharoṣṭhī* script, the labels for unvoiced aspirated consonants and voiced unaspirated consonants have been reversed; p. 256, the title to Fussman 1980 should read "Nouvelles inscriptions Śaka: ère d'Eucratide, ère d'Azès, ère Vikrama, ère de Kaniṣka."

Who would have thought that a small collection of obscure *kharoṣṭhī* manuscripts could create such an avalanche of interest among diverse scholars and laymen alike? This volume will serve as a benchmark of clarity, readability, and scholarly precision for anyone attempting to work in similar materials in the future. Having so whetted our appetites, we can only hope that Richard Salomon and his students will bring forth the subsequent, detailed studies of the individual texts in the British Library collection as soon as possible.

The following 8 reviews are by the editor:

JI Xianlin, transliterated, translated, and annotated, in collaboration with Werner Winter and Georges-Jean Pinault. *Fragments of the Tocharian A* Maitreyasamiti-Nāṭaka *of the Xinjiang Museum, China.* Trends in Linguistics, Studies and Monographs, 113. Berlin and New York: Mouton de Gruyter.

The publication of this book is a major event in Tocharian Studies. Essentially, with this work, our access to and evidence for Tocharian A have increased substantially. The manuscript presented here consists of 44 leaves (i.e., 88 pages) of the *Maitreyasamiti-Nāṭaka [Dance-Drama of the Encounter with Maitreya]* in Tocharian A. The fragments were accidentally discovered by forestry workers near the Temple of a Thousand Buddhas in Qarashähär District of the Xinjiang Uyghur Autonomous Region of the People's Republic of China. Since the Chinese call this place Yanqi, the fragments bear the designation YQ for short.

The YQ manuscript is the longest version of the *Maitreyasamiti-Nāṭaka* discovered so far. It is beautifully written in Central Asian slanted Brāhmī script on yellowish paper about 32 cm in length and 18.5 cm in width. Most of the pages contain eight lines per page, but on some pages only six lines survive. The manuscript was heavily damaged by fire, especially on the left side; the left margin and corners of each page are entirely missing. There is not a single complete page or even a single complete line. But this is often the case with Tocharian manuscripts, so specialists must make the best of what they have. In the case of the manuscript remains under discussion, the YQ *Maitreyasamiti-Nāṭaka*, we could not have hoped for a better treatment than that provided in the volume prepared by Ji Xianlin, Werner Winter, and Georges-Jean Pinault.

Ji is China's greatest Indologist and was trained in Tocharian Studies under Emil Sieg (who, along with Wilhelm Siegling, was one of the two original decipherers of Tocharian during the first quarter of the last century) at the University of Göttingen. His collaborators are Winter, the outstanding German Tocharianist who also happens to be the editor of the distinguished series in which this volume appears, and Pinault, the greatest Tocharian specialist in France. Since all three of these scholars have linguistically sharp minds, their approach is lean and spare. In other words, they provide everything that is necessary to make this text available to other specialists in the clearest, most straightforward possible manner, but no more and no less.

They began with a description of previously published fragments of the *Maitreyasamiti-Nāṭaka* in Tocharian A and in Uyghur. The latter, incidentally, has been of tremendous value in understanding the Tocharian manuscripts of the text. Next comes a list of parallel versions in other languages, including Chinese, Tibetan, Khotanese Saka, Sogdian, Pāli, and Sanskrit. The bibliographical references are precise and detailed. I should note that, except for the Uyghur manuscripts, the other versions are only helpful insofar as they provide the general outlines of the story. The Tocharian and Uyghur manuscripts present a very elaborate rendition of the narrative with important implications for the history of drama and picture storytelling in Asia. For these matters, one may consult Victor H. Mair, *Painting and Performance: Chinese Picture Recitation and Its Indian Genesis* (Honolulu: University of Hawaii Press, 1988), esp. pp. 40-42 and Dolkun Kamberi, "The Study of Medieval Uyghur Drama and Related Cultural Phenomena: From *Maitrisimit* to *Qutadghu Bilik* ca. 767-1069 A.D." (Columbia University Ph.D. dissertation, 1995).

Next comes a brief outline of the contents of the first, second, third, and fifth acts of the *Maitreyasamiti-Nāṭaka*. This includes a translation of the Chinese version of the story that is found in the *Sūtra of the Wise and the Foolish (Xian yu jing)*. The brief outline is followed by technical remarks concerning the principles of the section division of the text and previous publications of YQ fragments by Ji and Pinault. After that are abbreviations of reference works and a lengthy table of abbreviations, mostly of a grammatical nature, plus six special signs used in the transliterations of the text.

The edition of the fragments, which is, of course, the *raison d'être* for this book, runs from pp. 21 to 211. It consists of very careful transliterations on left-facing pages and literal translations on right-facing pages. We can be grateful to the editors for this extremely convenient arrangement. At the end of each section of the text are to be found the explanatory, textual, historical, and Buddhological notes that pertain to it. The placement of the notes right after each section is also very handy, since one can consult them readily without having to turn to the back of the book.

The third major part of the book, after the introduction and the edition of the fragments, are the indices. These are: an index verborum of all forms which occur in the

YQ *Maitreyasamiti-Nāṭaka* fragments, even when only a single letter remains; an index of all verbal forms found in the text according to their "roots" (which are accompanied by English translations); and a glossary of non-verbal forms.

The book concludes (pp. 304-391) with photographic plates of the manuscript, one page of the original on each plate. The plates are printed on thick, glossy stock and are exceptionally clear, revealing all necessary details of the aksaras.

With such an ideal apparatus and superlative scholarship, I have only praise for this wonderful book. Considering the vital importance of Tocharian for Indo-European linguistics and the history of Central Asia, indeed the whole of Eurasia, one can only hope that the Chinese authorities will make available to the international community of scholars all of the manuscripts in their possession in a timely fashion. I hold the same hope for inscriptions in cave-temples which are in danger of further deterioration and destruction. Every single word of Tocharian that is added to our meager inventory is a priceless gift, and we can only be infinitely grateful to Ji Xianlin, Werner Winter, and Georges-Jean Pinault for the rich lode they have presented to us in this superb volume.

Gang Yue. *The Mouth That Begs: Hunger, Cannibalism, and the Politics of Eating in Modern China.* Durham and London: Duke University Press, 1999.

The title of this book is an embarrassment. One does not know whether to laugh or cry upon reading it. I feel so sorry for the author that all of his learned friends and teachers (not to mention the reviewers who approved the manuscript for publication) failed to correct the horrendous error that is not only emblazoned across the cover and title page but is outrageously presented at the beginning of the description of the book on the back cover and everywhere that Duke University Press has publicized the book: "The Chinese ideogram *chi* is far richer in connotation than the equivalent English verb 'to eat.' *Chi* can also be read as 'the mouth that begs for food and words.'" What?!?! Such a preposterous, presumptuous proclamation causes me to cringe to the very core of my being. It is almost unbelievable that the University of Oregon would have awarded a Ph.D. to a dissertation (the basis for the book) predicated upon such shallow scholarship and that such a prestigious press as that of Duke University would accept for publication a work that is full of such facile blather. It is no excuse that the author may have picked up his fatally flawed formulation from one of the most famous contemporary Chinese writers, Mo Yan.

Part of the series entitled "Post-Contemporary Interventions" edited by Stanley Fish and Fredric Jameson which, I suppose, is intended to advertise the aspiration to go one step beyond Postmodernism and to be more politically active than a purely scholarly approach might allow. Already before he left Duke University for the University of Illinois at Chicago Circle, Fish had begun to see the folly of academics engaging in politics and has lately been denouncing this sort of liberal posturing in rather vitriolic terms. Not having heard much of Jameson in recent years, I have no reason to believe that he has abandoned his adherence to Post-Capitalism and Marxism. Regardless of the original intentions of the Series Editors, it seems to me that the theoretical model which serves as the foundation of *The Mouth That Begs*, a typical volume of "Post-Contemporary Interventions," trivializes the real social and human issues (viz., cannibalism and hunger) that are being addressed:

> Thus, the interaction between the mouth and the world is always mediated by specific semiotic systems and historically situated modes of cultural embodiment. Because "eating" is inscribed in various economic, political, social, and cultural codes, its semantic and symbolic field cuts across the disciplinary and discursive boundaries. The principle that governs my reading is a dialectic between the physical body and the body politic, with "eating" posing as the central locus on which the natural body and the social body join and shape each other in their dynamic interplay. This conceptual model is formulated in figure 1, where "eating" is foregrounded as an epistemological concept much as a trope that organizes various textual practices.

The reader must bear in mind that these reflections follow directly upon the author's infamous assertion that the Chinese term for "eating" "encompasses a far broader semantic and discursive field and possesses more generative and transformative capacities than its English counterpart" which has, as we have already seen, been taken up by the marketing department of Duke University Press in its opening salvo to promote the book to the world. Let us just consider the following colorful expressions in English: eat crow, eat dirt, eat the air, eat humble pie, eat stick, eat high off the hog, eat (one's) heart out, eat (one's) head off, eat (one's) words, eat (one's) terms, eat out of (someone's) hand, eat (someone) alive, eat (someone's) salt, eat (someone) out of house and home, eat in / out, eat up / down, eat away / into, eat off, eat me, eat the wind out of, and so forth. Obviously, the list could be extended, but there is no sense in getting into a contest to determine whether English "eat" or Mandarin *chi* possesses a richer assemblage of idioms and connotations.[1] First of all, who would be a fair judge in such a contest and what standards would he / she employ? Secondly, it does not take much effort to realize that verbs for "eating" in all languages of the world are highly multivalent because they have to do with one of the most basic bodily functions and are inescapably intertwined with questions of survival, deprivation, and excess. Comparative assertions of the kind that lie at the heart of *The Mouth That Begs* are nonsensical and futile.

Still worse, however, are the author's allegations about the etymology of the Modern Standard Mandarin word *chi* ("eat"), namely, "the mouth that begs for food and words." Now, I must say that in my long life as a Sinologist, I have encountered many ridiculous explanations of the root meanings of Sinitic words, but this one vies with the most fatuous and farcical. Before dissecting the author's bizarre etymology of *chi*, however, it is necessary to observe that the term "ideograph" which he uses to designate Chinese characters (called by the Chinese themselves *hanzi* ["sinographs"] or *fangkuaizi* ["tetragraphs"]) is even more grossly inappropriate than the dreaded "pictograph." The author -- and all of those who signed off on *The Mouth That Begs* -- should be required to read two books by John DeFrancis (*The Chinese Language: Fact and Fantasy* and *Visible Speech: The Diverse Oneness of Writing Systems*, both from the University of Hawaii Press [respectively 1984 and 1989) to gain an understanding of how characters are constructed and function as well as the best way to refer to them. One of the most elementary facts about the nature of Chinese characters is that approximately 85% of them (including both variants of *chi*) consist of a component that conveys weak semantic significance (often called a "radical") and a component that conveys a rough approximation of the sound of the syllable that the character stands for (the "phonophore"). And this leads to the second major misconception under which the author is laboring, namely, that the shapes and structures of Chinese characters are equivalent to Sinitic words. His misunderstanding regarding this tiny word *chi* is so monumental and so many people have acquiesced in it or are being deceived by it that I must devote a rather large amount of space and detail to demolishing (not "deconstructing") the ludicrous exposition which informs the entire book.

Without explaining clearly what he means by *jianti* and *fanti*, which will surely be confusing to those who do not know Chinese, the author asserts that 吃 is the *jianti* of *chi* and that 喫 is its *fanti*. Even if we knew that *jianti* means "simplified form" and *fanti* means "complex form," his statement would not ring true. 吃, which I henceforth will refer to as *chi*I is actually older than 喫, which I henceforth will refer to as *chi*II, so *chi*I couldn't really be a simplification of *chi*II. *Chi*I is securely attested in the *Shuo wen jie zi [Explanation of Simple and Compound Graphs]* which was completed in the year 100 of our era. In contrast, *chi*II is first securely attested in the *Yu pian [Jade Leaves]*, compiled in 543, and was only "newly appended" (*xin fu*) to the *Shuo wen jie zi* in the tenth century.

The earliest meaning of *chi*I was "stammer";[2] before the time of *Hong lou meng [Dream of Red Towers]*, published in 1791, the sole occurrence of *chi*I in the sense of "eat" that I know of (in the *Xin shu [New Writings]*, traditionally attributed to Jia Yi [200-168 BCE] but of dubious authenticity), is textually suspect. Be that as it may, it is wrong to claim that *chi*I could possibly be intrepreted as signifying "a mouth that begs [for food and words]." The small box-like component on the left side of the graph is indeed a mouth, but it is here being used as a radical with the implication "having to do with the mouth"; there are separate radicals for "words / speech" and "food". Furthermore, there are at least two good reasons why the component on the right has nothing whatsoever to do with "begging." First is the simple fact that it is a phonophore and is being used to convey, by a rough approximation, the sound of the word that the graph stands for. Second, the earliest form of *chi*I does not have as its phonophore the graphic component which, when standing alone, means "beg"; instead, it is written with the homophonous and near-homographic (but still quite distinct) component which, in isolation, means "breath, air, vapor" (the characters for "beg" and "breath, air, vapor" are both pronounced *qi* in Modern Standard Mandarin [MSM]). Therefore, if we are discussing the deep cultural symbolism of eating in China, it has nothing inherently, linguistically, or graphically to do with begging. The whole premise of Gang Yue's book utterly evaporates.

Let us turn our attention to *chi*II for awhile. Like *chi*I, *chi*II is composed of a mouth radical and a phonophore. In this case, the phonophore is homophonous with the characters for "beg" and "breath, air, vapor" in MSM (though not in medieval and earlier stages of Sinitic) and means "notch, contract, agreement" when it stands alone. As we have seen, *chi*II seems to have appeared in the Sinitic lexicon around the middle of the sixth century. It is reasonable to ask how and why the word for such a fundamental action as eating could have arisen so late in Sinitic. The fact that the most common word for "eat" in Literary Sinitic (i.e., Classical Chinese) is pronounced *shi* in MSM and is written with a graph that depicts a vessel filled with food might prompt one to speculate that *chi* was borrowed from some non-Sinitic language. However, when we examine possible candidates among neighboring languages that were in contact with Sinitic at this time

(Iranian, Turkic, Indic, Tocharian, etc.), there are none that have words for "eat" which would be obvious candidates to match with *chi*. Furthermore, it is unlikely that the word for such a basic notion as eating would have to be borrowed from some other language.

Another possibility is that *chi* evolved out of some other Sinitic word meaning "eat." Aside from *shi*, the only other fairly common word for "eat" in Old Sinitic is *can*, but the phonological configuration of *can* is so vastly different from that of *chi* that the latter could scarcely have evolved out of the former. Even in MSM, *shi* and *chi* resemble each other much more closely than do *can* and *chi*. Indeed, I believe that *shi* and *chi* ("eat") may well be phonologically and etymologically related. Of course, they both mean exactly the same thing. As for their pronunciations, both begin with fricatives and the quality of their vowels is nearly identical. Still more intriguing is the fairly neat geographical split between *chi* in the north and *shi* in the south, with the southern topolects retaining the more archaic word. If we carefully examine the breakdown of the words for "eat" in the *Hanyu fangyan cihui [Vocabulary of Sinitic Topolects]*, 2nd. ed. (Beijing: Yuwen chubanshe, 1995), p. 335a, it is remarkable how close the pronunciations of the words for "eat" in languages ranging from Beijing to Canton are, despite the fact that those in the north are all written with the graph that is pronounced *chi* in MSM while most (but not all) of those in the south are written with the graph that is pronounced *shi* in MSM:

| | | |
|---|---|---|
| Beijing | 吃 | tʂʻʅ˥ |
| Jinan | " | " ˧ |
| Xi'an | " | " ˨˩ |
| Taiyuan | " | tsʻəʔ˧ |
| Wuhan | " | tɕʻi˨˩ |
| Chengdu | " | tsʻɿ˨˩ |
| Hefei | " | tɕʻiəʔ˦ |
| Yangzhou | " | " |
| Suzhou | " | tɕʻiɪʔ˦ |
| Wenzhou | " | tsʻɿ˧ |
| Changsha | " | tɕʻia˦ |
| Shuangfeng | " | tɕʻio˨˩ |
| Nanchang | 喫 | tɕʻiak˥ |
| Meixian | 食 | sət˥ |
| Guangzhou | " | ʃɪk˨ |
| Yangjiang | 吃 | hɒt˦ |

| | | |
|---|---|---|
| Xiamen | 食 | tsiaʔ ˥ |
| Chaozhou | 〃 | 〃 ˥ |
| Fuzhou | 〃 | sieʔ ˧ |
| Jian'ou | 〃 | iɛ ˨˦ |

Particularly arresting is the pronunciation of *shi* in Amoy (Xiamen) and Swatow Chaozhou) which is very close to that of *chi* in Hefei and Yangzhou. How can this peculiar situation be explained?

I submit that *shi* and *chi* may actually derive from the same Sinitic root but that they took two different roots of phonological evolution, just as English "eat," Latin *ēsse*, Slavic *jasti*, Sanskrit *ad-*, etc. all derive from the Indo-European root **ed-* but end up looking rather different.[3] Strong support for this hypothesis is to be found in the lexicons of Tibeto-Burman languages where there are numerous cognate words meaning "eat" whose pronounciations resemble the various topolectal pronunciations of *chi* and *shi* in Sinitic.[4] The ambivalent position of Sinitic in relation to Tibeto-Burman is compatible with current discussions in historical linguistics.[5]

It is interesting that a similar process of phonological divergence occurred with another Sinitic alimentary word that is celebrated worldwide. Several times in the *Shi jing [Poetry Classic]* there is mentioned a plant whose name in MSM is pronounced *tu* 荼. The commentators aver that it is a type of bitter plant, a characteristic that may be gleaned from some of the occurrences of the word in the *Shi jing* itself (e.g., 35.2, where we read *tu ku* ("the *tu* plant is bitter") and 257.11, where we find the line *ning wei tu du* ("why are [the bad people] bitter poison?" -- a metaphorical usage).[6] The phonological reconstruction of *tu* in Old Sinitic is very complicated, particularly since the graph not only appears to have been used for at least two separate plants, but also because it seems to have been borrowed for several different words, among them *shu* ("a baton-like jade implement; name of a god; relaxed"), *ye* (a surname), and -- *mirabile dictur*! -- *cha* ("tea")! Historical phonologists disagree so wildly on the Old Sinitic reconstruction of *cha* ("tea") that, *faute de mieux*, I will follow Bernhard Karlgren's **d'ă* (*Grammata Serica Recensa* 82x) for the nonce.

Now, it is most curious that the graph that is currently used to write the word *cha* ("tea") did not even exist until the mid-Tang period (around the eighth century). More curious still is the peculiar difference of only a single, tiny stroke between the graph used to write *cha* ("tea") and the graph used to write *tu* ("bitter weed"). To be sure, the same graph that is used to write *tu* ("bitter weed") is also held by authorities on the script to be the correct form of the graph for *cha* ("tea") and the graph that is nowadays universally used to wrote *cha* ("tea") is declared to be a vulgar form.

What is going on here? While this is not the place to embark upon a complete history of tea-drinking in China, we may state briefly that its historical roots in north China are very shallow and that, as late as the sixth and seventh centuries of our era, tea was looked down upon as belonging to the *nan Man* ("southern barbarians"). It was only in the eighth century that tea became an acceptable beverage in the north, and this was due largely to the efforts of Lu Yu (733-804), the author of the *Cha jing [Tea Classic]* and founder of the tea cult in China. Botanically, the origins of tea are in the Yunnan-Burma-Assam triangle, and it moved from there to Sichuan, where we find the first tentative evidence for tea-drinking in China, then down the Yangtze Valley, and from there to the rest of China and to the world.

It is significant that the earliest presumed reference to tea in China, in the "Tong yue (Contract for a Youth)" by Wang Bao (fl. 61-54 BCE), is both textually and semantically ambiguous. While the setting is in Sichuan, textual variants have both *cha* and *tu*, so nobody is certain whether Wang Bao intended for the slave to boil tea or some other sort of bitter concoction. Equally revealing is the fact that, as late as the twelfth century, the graph for *tu* is still occasionally being used in a manner which indicates that it is almost certainly meant to refer to tea.[7]

Once again, if we look at the various topolectal pronunciations of *cha* today, we will see that they reveal what appears to be a split between those in the north that resemble *cha* and those in the south that resemble *tu*:

| | | | |
|---|---|---|---|
| Beijing | 茶 | tʂʻa | ˧˥ |
| Jinan | " | " | ˦˨ |
| Xi'an | " | tsʻa | ˨˦ |
| Taiyuan | " | " | ˩ |
| Wuhan | " | " | ˨˩˧ |
| Chengdu | " | " | ˨˩ |
| Hefei | " | " | ˥ |
| Yangzhou | " | " | ˧˥ |
| Suzhou | " | zo | ˨˦ |
| Wenzhou | " | dzo | ˧˩ |
| Changsha | " | tsa | ˩˧ |

| | | |
|---|---|---|
| Shuangfeng | 茶 | dzo ˩˧ |
| Nanchang | " | tsʻa ˨˦ |
| Meixian | " | " ˩ |
| Guangzhou | " | tʃʻa ˨˩ |
| Yangjiang | " | " ˦˧ |
| Xiamen | " | te ˨˦ |
| Chaozhou | " | " ˥ |
| Fuzhou | " | ta ˥˨ |
| Jian'ou | " | " ˧ |

Languages such as French and English borrowed their word for tea (probably through Malay and Dutch) from a language like that of Amoy, whereas Russian and Japanese must have received their word for tea from one of the northern topolects.

In this case, when we examine the words for tea in Tibeto-Burman languages, many of which are located in the ancestral homeland of the tea plant, we find that the majority of them clearly resemble the pronunciation *cha* rather than *tu*. This is difficult to understand, because we would normally expect them to possess a more ancient pronunciation within Sino-Tibeto-Burman than northern Sinitic topolects which, in general, are more highly evolved (in the phonological sense) than southern topolects. There are only two possibilities I can think of to explain this odd situation. First, the northern Sinitic pronunciation of *cha* may have spread toward the Tibeto-Burman region and been adopted there. This seems unlikely, however, because of the great distance involved, the necessity for *cha* to have been borrowed repeatedly into dozens of Tibeto-Burman languages in a bewildering variety of forms, and the fact that the botanical homeland of tea must have had a word for the plant long before the appearance of the northern Sinitic word for tea which, as we have seen, is quite late. The other possibility, which seems much more likely, is that the original Tibeto-Burman word for the tea plant had a pronunciation that was intermediate between *cha* and *tu*, but somewhat closer to the former. When it spread toward China, the Tibeto-Burman word evolved along two paths: as it moved into north China, the word for tea evolved into *cha*, whereas it was transformed in to *tu*, *te*, and so forth as it travelled east (down the Yangtze) and then south (to Fukien and beyond). Before acquiring a penchant for drinking it as a beverage, the Chinese would have initially written the word for this plant with the graph that is now pronounced *tu* but which earlier had a pronunciation that was intermediate between *cha* and *tu*. As the habit of drinking tea spread northward and the sound of the graph in that part of China diverged more toward *cha*, speakers of northern Chinese topolects would eventually have created a new character to distinguish *cha* from *tu* by simply removing a single, small stroke from the original form of the graph, the latter thenceforth being reserved for the *tu* of the time of the *Poetry Classic*, long before the plant became the favorite beverage of the Chinese people.

Thus we have observed a parallel development in the creation of a new graph to account for the phonological shift toward *cha* similar to that which occurred when *chi* diverged from *shi* and an entirely new character was created to write it sometime around the 6th century. By the 18th century, some writers must have felt that the 12 strokes of the character that had been coined for *chi* were an annoyance, so they decided to use the nearly homophonous graph meaning "stammer," which had only half as many strokes, to replace it.

I cannot resist noting that, when we speak or write the colloquial expression *chi cha* ("eat [i.e., drink] tea"), we are employing two words that are of relatively late vintage in the Sinitic lexicon and two graphs of comparatively recent coinage in the script.

This has been a somewhat lengthy excursion into the realm of linguistics, but it was necessary to counteract the massive confusion engendered by the blithe assertion that the Chinese concept of eating has something to do with "begging for food and words." I regret that I have had to be so direct in my criticism, but the potential damage from such spurious methods of interpretation is so great that it is the duty of the conscientious scholar to expose them for what they are.

Despite having to point out that the author's assertions about a deep connection in the Chinese psyche between begging and eating are completely without foundation, this does not mean that I consider his book to be totally devoid of any redeeming features. The book actually contains mainy valuable insights about nutritional discourse in China and demonstrates that the author has read widely and thought deeply about the significance of food and eating in Chinese culture throughout history. The first chapter presents an admirable survey of the semiotics of eating in ancient China. Subsequent chapters focus on Lu Xun, Shen Congwen, Zhang Xianliang, Ah Cheng, Liu Zhenyun, Mo Yan, Xiao Hong, and Wang Anyi. The author even astutely examines images of Chinese food in America as refracted through the lenses of Maxine Hong Kingston, Jade Snow Wong, and Amy Tan. Included here is a deeply perceptive exposé concerning the role of the fortune cookie in American life. The book ends with a sensitive discussion of Wang Meng's monumentally controversial 1989 short story entitled "Jianying de xizhou (Crusted Congee)."

Especially thought-provoking is the author's account of cannibalism in China.[8] This is obviously an extremely sensitive topic, but the author tackles it without flinching. Perhaps the most moving aspect of *The Mouth That Begs* are the glimpses of the author's own soul that it bares. It is clear that he came to America in search of therapeutic answers to the anomie that he had experienced in China. Instead, he found here moral ambiguity and methodological uncertainty, leaving him still adrift after decades of searching for psychological, emotional, and intellectual healing.

It is enough of a tragedy that too many young American scholars have their heads filled with fatuous literary theory. It is even more of a calamity when students from other countries come to our universities seeking refinement of their analytical and interpretive skills, only to be indoctrinated with one fashionable set of dogmas after another. Too many of them end up being able to cite chapter and verse of the latest Western critical icons, but fail to acquire even a basic understanding of the application of rigorous philology to their native language and literature. When students work for years to produce books like *The Mouth That Begs* and no one has the common sense to point out the folly of their basic premises but, instead, encourage others to write in the same vein, our entire scholarly enterprise becomes infected by a dangerous disease -- that of muddled thinking and pretentious pontificating. Those who come to us for guidance deserve better.

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1. The *Oxford English Dictionary* gives scores of different senses, subsenses, and subsubsenses for "eat" whereas the *Hanyu Da Cidian* offers about a score of different senses for *chi*; most of the extended, figurative, and slang usages of *chi* ("eat") are very recent -- within the last century or two.

2. In the *Han shu [Han History]*, it is said that the famous rhapsodist, Sima Xiangru (179-117 BCE) "stammered (*chi*I) but was an excellent writer."

3. It would appear that Sinitic *can* ("eat"), discussed above, is linked to another Tibetan word for "eat" but not to Tibeto-Burman as a whole. See Weldon South Coblin, *A Sinologist's Handlist of Sino-Tibetan Lexical Comparisons*, Monumenta Serica Monograph Series, XVIII (Nettetal: Steyler, 1986), p. 69, where we find MSM *can* < Old Sinitic **tshan* < Sino-Tibetan ***tshal* (cf. Tibetan *'tshal-ba* ("to eat") and *'tshal-ma / tshal-ma* ("breakfast").

4. See *Zang-Mianyu yuyin he cihui* bianxiezu, comp., *Zang-Mianyu yuyin he cihui [Tibeto-Burman Phonology and Vocabulary]* (n.p.: Zhongguo Shehui Kexue Chubanshe, 1991], p. 904, no. 533; Huang Bufan, ed., *Zang-Mian yuzu yuyan cihui (A Tibeto-Burman Lexicon)* (Beijing: Zhongyang Minzu Xueyuan Chubanshe, 1992), p. 400, no. 1198; W. W. Hunter, *A Comparative Dictionary of the Languages of India and High Asia* (New Delhi: Cosmo, 1978; originally published 1868), p. 198.

5. George van Driem, "Sino-Bodic," *Bulletin of the School of Oriental and African Studies*, 60.3 (1997), 455-488; van Driem, "A New Theory on the Origin of Chinese," *Indo-Pacific Prehistory: The Melaka Papers*, vol. 2, Proceedings of the 16th Copngress of the Indo-Pacific Prehistory Association, Melaka, Malaysia (July 1-7, 1998), *Bulletin of the Indo-Pacific Prehistory Association*, 18 (Canberra: Indo-Pacific Prehistory Association, Australian National University, 1999), pp. 43-58.

6. The same graph may also refer to another plant in the *Shi jing* since in 93.2 we have the line *you nü ru tu* ("there are girls [thronged] like reeds").

7. See, for example, the preface to *Yi'an shi gao [Poetry Drafts from the Hut of Nourishment]* by Yang Wanli (1127-1206) where he describes how drinking *tu* first gives a sensation of bitterness but then leaves a sweet taste in the mouth, which is exactly what tea does. Thus it is not surprising that, when the Chinese first encountered tea, they would have described it as "a bitter plant," which is precisely what *tu* signifies.

Wang Pao's "Contract for a Youth" is translated on pp. 510-513 of Victor H. Mair, ed., *The Columbia Anthology of Traditional Chinese Literature* (New York: Columbia University Press, 1994) and Lu Yü's "Autobiography of Instructor Lu" is to be found on pp. 699-702.

8. The author devotes a great deal of attention to the sensational revelations of Zheng Yi, the PRC master of reportage, on this gruesome subject. In the bibliography, he lists the Chinese edition of Zheng's *Hongse jinianbei (Red Monument)* (Taipei: Huashi wenhua gongsi, 1993) which is nearly 700 pages in length. To this may now be added an abbreviated English version that has been edited and translated by a group of writers using the joint pseudonym of T. P. Sym, *Scarlet Memorial: Tales of Cannibalism in Modern China* (Boulder, Colorado: Westview, 1996).

CHEN Gang, SONG Xiaocai, and ZHANG Xiuzhen, comp. *Xiandai Beijing kouyu cidian [A Dictionary of Modern Spoken Pekingese]*. Peking: Yuwen Chubanshe, 1997. 16 + 475 pages.

This book is a travesty and a sham. Were Chen Gang, the author of the work upon which it is based, alive today, he would surely protest vehemently at the grotesque changes Song Xiaocai and Zhang Xiuzhen have worked upon his brilliant dictionary of Pekingese colloquialisms. I am a proud owner of a copy of the original dictionary that was signed by the author himself. It is the first and best dictionary of special Pekingese terms in my collection of more than two dozen such reference tools. So impressed was I by Chen Gang's great work that I wrote a highly laudatory assessment of it in the first review issue of this journal (*Sino-Platonic Papers*, 8 [February, 1988], 26-27). Just for the record, the original dictionary was entitled *Beijing fangyan cidian [A Dictionary of Peking Colloquialisms]* (Peking: Shangwu Yinshuguan, 1985).

It is enough to make a grown man cry to see what has become of Chen's magnificent dictionary. Chen cared passionately about the subtle nuances of Pekingese and, furthermore, he was a good linguist. Neither of these things can be said about Song and Zhang. Where Chen's dictionary was arranged according to a single sort alphabetical order, the Song-Zhang makeover groups entries under head characters. There are many advantages to the former arrangement and many disadvantages to the latter arrangement. With a single sort alphabetical order, one can rapidly and easily look up terms that one hears in speech, whereas trying to find a given entry under various possible head characters is both time-consuming and clumsy. This is especially important for colloquial language where the choice of sinographs to write words is often totally arbitrary (a fact which I have proved repeatedly in my many reviews for *SPP* and other journals, especially those dealing with Pekingese).

This leads to the second great defect of the Song-Zhang revision, namely, where Chen listed all of the variant sinographic representations of a given expression at the end of its romanized entry (often as many as half a dozen or more different writings), the traducers of his dictionary always reduce them to a single form. This gives the false impression that the chosen sinographic form is the etymologically correct one, when it is usually entirely arbitrary.

Another example of the cavalier attitude with which Song and Zhang have treated Chen's lexicogarphical legacy is the replacement of all schwas by the letter "e". Since Cheng employed both the schwa and "e" in recording Pekingese speech, he surely intended to make a distinction between the two phonemes, but Song and Zhang could care less.

The list of the transgressions committed by Song and Zhang against the cherished memory of Chen is astonishingly long. To save space, I will not give examples of each, but will only observe that the ravishers of Chen's dictionary have removed all of the following categories of information so usefully provided by the original compiler: parts of speech and usage notes (whether pejorative or commendatory, nicknames, if used in arguments, if rare or out-of-date, restricted to children's speech, satirical or critical, stilted or aristocratic, emotional, palace language, the professions and crafts, terms of address, used primarily by Muslims, euphemisms and taboos, quacks and mountebanks, suburbs, borrowings, older language, gangsters, curses and vulgarity, superstition and religion, making slight of, deriding, Manchus, humble or modest, legends, parts of animals that are eaten, roundabout or circumlocutory, neologisms, children's prophecies, extended usage, metaphorical, transferred usage, respectful). Lost! Gone! Destroyed! All of this precious information so painfully and lovingly gathered, distinguished and magnanimously provided by Chen has been totally annihilated by Song and Zhang. The enormity of their violations against Chen's marvelous work of scholarship and preservation of culture will go down as one of the greatest abominations ever to have occurred in the history of linguistics. And they dare to put Chen Gang's name next to theirs as an author of this piece of unmitigated trash!

The new title Song and Zhang have used for their book is fallacious. This is not a dictionary of **modern** Pekingese, since it includes many old and outmoded expressions no longer in general use.

*Xiandai Beijing kouyu cidian* is equipped with two indices: a list (pp. 465-475) of the head characters (all simplified forms where relevant) arranged by total stroke count -- this is especially hard to use for characters having between 8 and 12 strokes, where there may be upwards of 200 or more items to sort through; a chart (pp. 8-16 [first pagination]) of the syllables arranged by pinyin romanization -- this is a waste of space because it replicates the main order of the dictionary.

The only conceivable improvement one could point to in the Song-Zhang revision is that it has expanded the total number of entries from more than 6,000 to over 11,000. But the new entries were mostly taken from note cards that Chen himself had prepared, so we can scarcely give Song and Zhang the credit for that. Furthermore, if you can't readily find an item you're looking for and if the new material is presented in an unscholarly, incomplete fashion, the added entries are of diminished value.

I do not know who Song and Zhang are, but it is sad that Yuwen chubanshe, which used to publish some important books for the study of Sinitic languages, has agreed to put this inferior work before the public. If the dictionary under review had appeared in America, I would bring suit against it in a court of law as a defamation of the good name and character of an honest topolectologist. Unfortunately, the law is no defense against such crimes in China.

ZHOU Yimin. *Beijing kouyu yufa: cifa juan [A Grammar of Spoken Pekingese: Morphology]*. Peking: Yuwen Chubanshe, 1998.

This is a relatively straightforward, systematic, and rigorous introduction to the morphology of Modern Spoken Pekingese. The author makes an effort to distinguish the spoken language from various types of book language, elements of which previous studies of Pekingese grammar all too often allow to slip into their analyses. Following the lead of the eminent linguists ZHU Dexi and HU Mingyang in work that they did during the 80s, the author wisely stresses the need to differentiate the grammar of the Peking dialect from that of Putonghua (Modern Standard Mandarin).

The niceties of Pekingese grammar can be quite complex, as is evidenced by the author's list of possible verb tenses: present continuative, past, future, past continuative, past perfective, past future, future continuative, future perfective, future future. The author provides each tense with a brief discussion of its structure and function, plus two or more example sentences.

Zhou Yimin treats Pekingese as a "normal" language susceptible to "universal" grammatical categories and does not see the need to establish a "unique" system of grammar for this colorful, lively language. He does not hesitate to point out features of Pekingese that can only be described by such terms as "marvelous." For example, he sets up a category of *shenqi dongci* ("miraculous / magical [!] verbs"). Among these are *lai* which overtly signifies "come" but in spoken Pekingese can mean the following: buy, order, go, arrive at, play the role of, compete, play [a game], fight, argue, draw, write, paint. Other verbs in this category are *nong* ("toy with"), *da* ("hit"), *wan$_r$* ("play"), and *gan* ("attend to"). These are multiple-purpose (one is tempted to say "all-purpose") verbs which, depending upon the context, can mean almost anything.

Another fascinating category of Pekingese words treated by the author are onomatopoeic expressions. While many other northern Mandarin topolects also possess abundant resources of this sort, in Pekingese they are particularly colorful and numerous. Fortunately, Zhou Yimin is a decent enough linguist to admit that it is impossible to write many of these vivid and characteristic expressions in sinographs, so he sensibly gives them in romanization. Some of these onomatopoeic expressions occur in a dizzying variety of alternative forms, e.g. *jingdinggangdang / qingtingkangtang / jingdingguangdang / qitingkuangtang / jingdingguangdeng / qingtingkuangteng* ("sound of objects striking together; sound of a train passing"). So far as I can tell, only first tones are used in this type of expression, except where there are regular substitutions of the neutral tone, as in the second syllable of the series that I have just listed. Does this mean that the first tone is the basic tone, the fall-back tone? What does this tell us about tonogenesis in Sinitic languages? I believe that research and analysis on this phenomenon are called for. Here are a couple of my favorite onomatopoeic expressions in Pekingese: *wala* ("the sound of foreigners talking" -- but I'm sure that I have also heard people use this to describe the sound of a baby crying) and *xiliu* ("the sound of sucking in one's snot").

These are but a few of the topics covered by Zhou Yimin in this valuable account of one of China's countless living languages. It is well worth reading, if only to remind oneself how very different "sayable" Sinitic languages are from written Sinitic, whether of the classical or the vernacular varieties.

Lewis, Mark Edward. *Writing and Authority in Early China.* Albany: State University of New York Press, 1999. 544 pages.

It is uncanny that the last chapter of this book has a title that closely resembles that of a book I reviewed for this journal a year ago, namely "The Empire of Writing." But Mark Edward Lewis's book could hardly be different from Christopher Leigh Connery's *The Empire of the Text* (see *SPP*, 90 [January, 1999], 3-4) in approach, style, method, and intent. Connery is interested in ideas and in trying to understand how writing, as a technology, was a fundamental feature of the ancient Chinese intellectual landscape and how it helped to shape and define the course of history. Lewis is devoted to amassing a tremendous amount of textual material and making minimalist comments upon it. Connery is not afraid to mention modern theories and is willing to analyze and interpret historical data in stimulating ways. Lewis's approach is traditional, careful, and cautious. Even though Lewis's book is more than twice as large as Connery's, I can honestly say that I learned next to nothing from it, because it consists almost entirely of references and passages from classical Chinese texts that I had already encountered at one time or another in my three decades as a Sinologist. Reading Connery's lithe volume took me much less time to read than Lewis's hefty tome did, but it has kept me thinking ever since.

Lewis seems to have sensed that a book as big as his ought to make one think, so he pays lip service to ideas in a curious manner. First of all, he imparts a pseudo-postmodernist flavor to the work by unimaginatively but trendily entitling each of his first four chapters in the same way: "Writing the State," "Writing the Masters," "Writing the Past," and "Writing the Self." But this is a purely sanctimonious nod to current intellectual fashion (actually beginning to get somewhat out of date by the time the book appeared in print), since nothing could be further from postmodernism than the wrting of Mark Edward Lewis (except for half of his chapter titles!).

It is also rather strange that Lewis begins and ends his book with an exceedingly brief mention of spoken language. As someone who has devoted his entire professional life to the study of vernacular and colloquial languages in China, I am mystified by why Lewis felt compelled to sandwich hundreds of pages of dense discussions of Literary Sinitic with a perfunctory acknowledgement of spoken language that is not integrated into his overall presentation in the slightest. Does this mean that he recognizes the vernacular to be an important topic but that he doen't know what to do with it? Or that encasing his stolid scholasticism with a veneer of populism would insulate it from certain types of criticism?

Potential criticism from another direction is warded off with the same sort of sandwich effect. Within the first three pages of his book, Lewis ostentatiously rattles off Plato, Xenophon, and Aristophanes ("Other examples are Buddha, Confucius, and Christ" [!!]), French, Latin, and Sanskrit, plus an obligatory pietism to Lévi-Strauss's *Tristes tropiques*, then plunges headfirst into the Warring States, never coming up for air -- not even a single breath -- until the last three pages of his text when he invokes a contemporary icon in such an incongruous manner that I actually burst into loud laughter when I finally stumbled upon this too-obvious signpost of relevance and rectitude. The opening sentence of Lewis's "Conclusion" must be quoted: "When discussing the relation of writing to reality, one always comes to Borges." The inevitability of this pronouncement is stunning! After struggling dutifully and numbly through hundreds of citations to late Warring States and Han works, to be hit with Borges -- like a lifeless, wet fish on one's cheek -- is a truly sobering experience.

If the opening sentence of the first paragraph of Lewis's "Conclusion" is jolting, the opening sentence of the final paragraph of his "Conclusion" leaves one as limp as the piscine corpse at one's feet: "Having begun with Borges, we finish with Genet." (I did not make that up.) Nothing could be more inevitable and obligatory than to chant the holy names of Borges and Genet before one signs off on a book that nowhere else outside of the first three pages makes the slightest attempt to communicate to the non-Sinologist.

And what does Lewis have to say to his fellow Sinologist? Perhaps the most adventuresome notion put forward within the envelope of our discipline is that of proto-Taoism. While this idea is not unique to Lewis, it is nonetheless newish and controversial, almost exciting. The trouble is that proto-Taoism may be a chimera because neither has it been adequately demonstrated how it relates to religious Taoism (which came centuries later) nor how it fits in with the equally elusive "Tao-school / lineage" (Tao-chia) of pre-Qin times. In any event, Lewis does not spell out adequately what he means by proto-Taoism, so its precise nature and role in his overall scheme are difficult to assess.

Another minor risk taken by Lewis is his hesitant movement toward joining with those who wish to abandon "classic" for "canon" as the standard Sinological translation of the key term *jing*. This has now become quite the rage among some of our most venturous colleagues, although Lewis still hedges his bets by occasionally speaking of *jing* as "canons / classics" or in similarly wavering formulations. I am opposed to this innovation for many reasons, among them the following: 1. "canon" is usually thought of as a collective term when applied to books; 2. if we start to call *jing* "canons," what then will we style the *Buddhist Canon (Fozang)* and the *Taoist Canon (Daozang)*, both of which are made up of hundreds of *jing* (usually rendered respectively as "sutra" and "scripture" in these particular cases); 3. if we identify *jing* as "canon," what shall we call *dian*, which is normally translated as "canon" in the sense of "a body of rules or texts" and how would we deal with a title like that of Lu Deming's (566-630) *Jingdian shiwen*, where *jing* and *dian* occur right next to each other; 4. semantically speaking, "canon" (law or code of laws; standard; criterion; an authoritative list of works) is by no means clearly superior to "classic" (a work considered to be of the highest rank or excellence; something considered to be typical, traditional, or authoritative) as a rendering of *jing* and is arguably inferior.

This leads us to the question of what precisely the meaning of *jing* is, quite apart from the problem of how best to translate it into English. Using traditional methods of character exegesis, the author (pp. 297-99) does about as good a job as could be expected. Although the index of the book identifies what Lewis provides here as the "etymology" of *jing*, I would not accept that as an accurate characterization because he nowhere discusses the historical phonology of the word (essential for any genuine etymology), relying entirely on such devices as paronomastic explanations and analyses of the shapes of the graph used to write *jing* ("classic") and other graphs that include the same phonophore. Admittedly, the phonophore in the graph for *jing* ("classic") originally had a root signification which seems to have entered into the meanings of a whole group or family of cognate derivatives. The difficulty with Lewis's type of traditional Sinological exegesis of ancient Sinitic words is that it gives precedence to the graphs employed to write them and virtually ignores the evolution of sounds, which is the key to an accurate understanding of the early stages of etymological derivation. (The is merely one reason why it is so perilous to give short shrift to spoken language.)

The meaning of the "root" element (the phonophore in this case) of *jing* ("classic") is hotly contested. It has so far not been identified on the Oracle Shell and Bone Inscriptions (the earliest stage of Chinese writing) and, by the time it appears in the Bronze Inscriptions, it is impossible to tell for sure what this component is meant to depict. The famous *Shuowen jiezi (Explanation of Simple and Compound Graphs)* (100 CE) glosses it as a "subterranean channel of water" but few nowadays would accept that as very convincing. Lewis miscites Bernhard Karlgren (*Grammata Serica Recensa*, p. 219) as maintaining that this component depicts a device used in weaving, with threads coiling around a staff. Actually, Karlgren just suggests that the graphs may depict some sort of loom, which is probably not too far off.

Nearly all the characters containing the element in question indicate something passing straight through a body or object and holding it tightly together like a thread or tendon. In my notes to the *Tao Te Ching (The Classic of the Way and Integrity)*, I made a radical proposal that the basic meaning of the *jing* element was "file," the root meaning of which is precisely "thread, tendon". About half a dozen years ago, probably about the time Lewis was doing his research for this book, he and I had some correspondence on the matter of the etymology of *jing* ("classic") and I was quite surprised when he told me that he did not find my proposal objectionable. Of course, he does not mention it in the present work and seems to have forgotten it altogether -- at least insofar as public consumption is concerned.

Xu Shen, author of the *Shuowen jiezi*, glosses the graph used to write *jing* ("classic") as "The vertical line [i.e., warp] of weaving. It is derived from [the radical / signific] *si* [silk] and is pronounced *jing* [as indicated by the phonophore]." We know from various contexts in which it appears that the basic meaning of *jing* ("classic") in Warring States times was indeed "warp," so Xu Shen could scarcely go wrong in identifying it as such. However, he was mistaken in attributing the meaning of "warp" solely to the silk signific, leaving the phonophore purely for the purpose of indicating the sound. The character actually gets both its basic meaning and sound from the component on the right side; the silk radical on the left simply reinforces in a secondary manner that the word has something to do with thread(s).

The fact that the graph used to write *jing* ("classic") often occurred in combination with or close proximity to that for *wei* ("woof, weft") and that the latter graph also had the extended connotation of "apocrypha" leaves little doubt that *jing* ("classic") is an extended connotation of a word meaning "warp." Yet the true etymology of *jing* ("classic") and related words in Old Sinitic remains to be determined. Further advances in archeology and phonology will almost certainly play a major role in any breakthroughs along this line.

I shall close this review with the observation that *Writing and Authority in Early China* is an informative work by a competent author, but it does not sing. There is no exhilaration in reading this volume. When all is said and done, what we have before us is a reliable reference work that covers a large number of topics relating to writing in ancient China. It is well organized, extensively documented, and exceedingly dry.

Keith Quincy. *Hmong: History of a People.* Cheney: Eastern Washington University Press, 1988; 2nd ed. 1995. xii + 244 pages.

The Hmong, more than 10,000,000 strong worldwide, are concentrated in Laos, northern Vietnam, Thailand, Burma, and mostly in the southern Chinese provinces (in numerically descending order) of Guizhou, Hunan, Yunnan, Sichuan, Guangxi, Hubei, and Hainan). There are also sizable refugee communities in many American cities such as Philadelphia and Seattle. The Hmong were one of the original groups of people occupying what is now south China, but were pushed out of the valleys into the mountains and beyond by ethnic Hans moving into the region from north China.

In Modern Standard Mandarin, the Hmong are referred to as Miao, which is simply a poor transcription of their own name. Because the Chinese chose to write the syllable *miao* with a graph meaning "sprouts," this has led to all sorts of condescending expressions applied by ethnic Hans toward the Hmong. The Hmong / Miao are actually a very ancient people and are mentioned in some of the earliest Chinese texts, usually as the San Miao (the three Miao) or You Miao (the Freehold of Miao). At this stage, they were located in the Yellow River Valley, from which they were expelled southward.

This thoughtful study includes the revelatory information that, when foreign missionaries first began to make contact with the Hmong / Miao in the seventeenth century, they were suprised to find a people who used spoons instead of chopsticks and many of whom had Europoid physical characteristics, including red or blond hair, light skin color, absence of epicanthic fold, narrow faces, aquiline noses, and even occasionally blue eyes, despite the fact that they lived in some of the most remote and inaccessible areas of south China. According to the author, "Recent studies of Hmong in Laos and Thailand have led some anthropologists to classify them as the most Caucasian population of Southeast Asia." (p. 18) Quincy's book includes pictures of such individuals who somehow have managed to survive till today amidst a sea of Mongoloid peoples (not to mention determined Chinese attempts to eradicate them on a racial basis). How do we account for these extraordinary features?

The first outstanding scholar to study the Hmong, Father F. M. Savina, mastered their difficult language and declared that it belonged to Ural-Altaic. While few linguists would agree with him, it is difficult to classify Hmong as belonging to any of the major language groups of Southeast Asia. (Yao and Hmong / Miao are often grouped together but the evidence for such a linkage is not very persuasive and, in any event, does not lead to any connections with larger groupings in Southeast Asia.)

Savina also studied the myths and legends of the Hmong and discovered that they had stories about creation, the original sin, a great flood, incestuous children, a tower of Babel, and a great migration from a fertile homeland that became overpopulated to an icy, northern plateau where people wore furs and the days and nights lasted six months, and from thence to north China. While much of this sounds rather fantastic and later researchers have not been able to verify all of the elements of Hmong mythology identified by Savina, it is partially corroborated by recent archeological discoveries. We now know that Europoid peoples really did extend eastward across Eurasia to northern China in Neolithic times and that, already during the first millennium, they reached all the way down into Yunnan (e.g., the ruling segments of the Dian Culture). It may well be that the fair-skinned, light-haired Hmong are the descendants of the members of these prehistoric archeological cultures. To find out their actual affiliations, however, will require much more work in genetics, linguistics, archeology, physical anthropology, and other fields.

This thought-provoking work is devoted mostly to the known history of the Hmong and to a description of their society (including the important role of shamanism). Because of their marginal position vis-à-vis the Chinese, the Vietnamese, and other major peoples, as well as their cultivation and manipulation by the French and the Americans, much of Quincy's account has a rather clandestine air to it. Be that as it may, if his book serves to promote deeper research on the Hmong, it holds the potential to help in the unravelling of some of the knottiest problems concerning the development of civilization in East Asia.

YE, Yang, tr., annot., and intro. *Vignettes from the Late Ming: A* Hsiao-p'in *Anthology*. Seattle and London: University of Washington Press, 1999. xxviii + 152 pages.

Whenever I come upon an English title which includes an untranslated term that might give trouble to a non-specialist, it intrigues me. First of all, it makes me wonder why the author declined to render the term into English? Is he / she implying that the term is untranslatable? Does he / she wish to establish the untranslated word or words as a technical usage? In the present instance, Yang Ye appears to have declined to provide an English translation of *hsiao-p'in* (Wade-Giles romanization; in Pinyin this would be *xiaopin*) for both of these reasons. The closest the author comes to providing a translation of the term *hsiao-p'in* is the definition he gives on p. xiv: "a short belles-lettres prose piece or vignette, usually informal in structure and mostly casual and spontaneous in mood and tone." Embedded within this definition is the French word "vignette," which has been borrowed into English. Since "vignette" also occurs in the main title of Ye's book in parallel with *hsiao-p'in*, and since it may also be found in the index as the cross-referenced main entry for *hsiao-p'in* and *hsiao-p'in-wen*, it would seem that this is the author's preferred translation. However, throughout his introductory discussion, he never explains why he thinks "vignette" is a good rendering for *hsiao-p'in*. In the second footnote to the introduction, Ye states that "For the convenience of the Western reader, I have used as an approximate to the Chinese term *hsiao-p'in* the word 'vignette,' of Old French origin, which has been defined as 'a short sketch chiefly descriptive and characterized usually by delicacy, wit, and subtlety.'" Fair enough, but Ye doesn't really use the term "vignette" -- whether for the convenience of the Western reader or not -- since he doggedly sticks to *hsiao-p'in* throughout the book.

This lack of commitment to his own rendering of *hsiao-p'in* made me wonder all the more exactly what a *hsiao-p'in* is. All of the popular Chinese-English dictionaries do offer concise definitions of *hsiao-p'in(-wen)*. *Lin Yutang's* gives "belles-lettres, essays, sketches"; *Han-Ying cidian* (Foreign Language Teaching and Research Press) has "a short, simple literary or artistic creation; essay; sketch"; *Far East* and *ABC* have just "essay"; and *Mathews'* has "short essay(s); trifles of literature." There is thus broad agreement that *hsiao-p'in* is a type of essay. However, since there are other Chinese terms that may also be translated as "essay" (e.g., *wenzhang /wen-chang*), we are still somewhat at sea.

There are several ways we can attack this problem. One is to examine the term itself in an attempt to extract meaning from its constituent elements. Another is to study the origins and evolution of the term. Still another is to examine the texts that are customarily referred to as *hsiao-p'in* and extract from them common denominators that may serve as parameters of the genre. We shall briefly try all three of these methods.

The first syllable, *hsiao*, does not present much difficulty: it means "small," "minor," and so forth. The second syllable, however, offers many more alternatives: "personality," "article," "chapter," "rank," "grade," and the like are all within the realm of possibility. Relying on the first method alone will not give us a clear indication of the meaning of *hsiao-p'in*, so we must move on to the second.

The earliest occurrence of the term *hsiao-p'in* is in the title of the abbreviated translation of the *Prajñāpāramitā-sūtra (Xiaopin boruo boluomi jing)* where it stands in contrast to the full translation of the same text, the *Dapin boruo boluomi jing*. Here it is very clear that *pin / p'in* signifies "chapter," a standard Buddhist usage. Liu Yiqing (403-444) makes a reference to the *Xiaopin boruo boluomi jing* in the 30th, 43rd, and 45th sections of the chapter entitled "Wen xue (Letters and Scholarship)" of his celebrated *Shishuo xinyu (A New Account of Tales of the World).* It is noteworthy that most of the anecdotes recounted by Liu Yiqing in this chapter are Buddhistic in nature and that the modern Sinitic word for "literature" (*wenxue*) was calqued during the 19th century upon the title of Liu Yiqing's chapter (its antecedents lay in the *wen xue* ["civil learning"] of the Confucians which appears already in the *Analects*), perhaps via Japan.[1] Hence, not only does *hsiao-p'in* possess deep Buddhist resonances, even the nascent concept of literature was nurtured in a 4th-century atmosphere that was redolent with Buddhist discourse. Yet the *hsiao-p'in* of the late Ming presented by Ye in the book under review certainly cannot be comprehended by applying merely the signification of the term in early Buddhist usage. Thus the second method for understanding the meaning of *hsiao-p'in* as a literary genre also fails us.

Basically a late Ming phenomenon which tailed off into the early Qing period, in other words, belonging to the 16th and 17th centuries, *hsiao-p'in* is not easy to characterize in formal terms. Observation and analysis of the 70 pieces collected and translated by Ye does not yield a consistent set of guidelines describing exactly what a *hsiao-p'in* is. In length, they range from a couple of sentences to three or more pages. They are usually nonfictional, but some of them are highly imaginative. They are usually exclusively prose, but it is not rare for verse to creep in. And so on. What is common to all of the *hsiao-p'in* is their style of informality. Therefore, William H. Nienhauser, Jr. is justified in calling them "informal essays" (*Indiana Companion of Traditional Chinese Literature*, passim), where he probably interprets *hsiao* in the sense of "minor".

In the end, I would suggest that the *hsiao-p'in* be referred to as "brief essays" because in spirit they are comparable to the familiar essay which began with Montaigne, with the distinction that they are usually much shorter than their Western counterparts. The *hsiao* of *hsiao-p'in* really does mean "small" and *p'in* indicates a text (< "chapter") (viewed in this light, the *wen* of *hsiao-p'in-wen*) is redundant. This understanding of *hsiao* as referring to brevity is reinforced by the fact that the term *hsiao-p'in* also came to be applied to **short** dramatic performances (e.g., excerpts). Furthermore, the word "essay" by itself conveys a sense of tentativeness and informality. Finally, I prefer "brief essay" or "short essay" over "vignette" because the latter usually refers to portraits, but even in a broader sense it captures neither the literal nor the functional meaning of *hsiao-p'in* as well as "brief essay."

The experience of reading *hsiao-p'in* is often like that of reading the *Tsurezure-gusa (Essays in Idleness)* (c. 1330) of the Buddhist monk Yoshida Kenko (c. 1283-c. 1350/52), although not quite so mind-altering. Incidentally, *Tsurezure-gusa* became especially popular in Japan after the 17th century.

*Vignettes* consists of a twenty-page introduction (on periodization, forerunners, rise, subgenres [travel notes, prefaces and colophons, biographical sketches, personal letters, chief practitioners, and previous studies), editorial notes, a map of China, essays by Gui Youguang, Lu Shusheng, Xu Wei, Li Zhi, Tu Long, Chen Jiru, Yuan Zongdao, Yuan Hongdao, Yuan Zhongdao, Zhong Xing, Li Liufang, Wang Siren, Tan Yuanchun, and Zhang Dai, the last and greatest of them all (each essayist is represented by an average of six pieces), a table of Chinese dynasties, a list of late Ming and early Qing reign periods, notes, bibliography, and an index.

This book is well-produced and well-designed. It is also itself rather like a *hsiao-p'in* -- short and elegant.

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1. See Federico Masini, *The Formation of Modern Chinese Lexicon and Its Evolution Toward a National Language: The Period from 1840 to 1898*, Journal of Chinese Linguistics Monograph Series, 6 (Berkeley: Project on Linguistic Analysis, 1993), pp. 25, 30, 86, 115 and Victor H. Mair, "Two Papers on Sinolinguistics / 2. East Asian Round-Trip Words," *Sino-Platonic Papers*, 34 (October, 1992), p. 5.

M. Holt Ruffin and Daniel C. Waugh, eds. *Civil Society in Central Asia*. Center for Civil Society International (Seattle), The Central Asia-Caucasus Institute (Nitze School of Advanced International Studies, John Hopkins University). Seattle and London: University of Washington Press, 1999. 342 pages.

The first and more substantial part of this book consists of analytical papers that were presented during a two-day conference held in 1998 under the auspices of the center and the institute named in the bibliographical information given above. Many of these papers deal with the subject of NGOs (nongovernmental organizations), which was a very hot topic during the latter half of the 90s (it is often used synonymously and interchangeably with another term that was fashionable at the same time, the "civil society" of the title of the book under review). The countries considered are Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, and Uzbekistan -- all former republics of the Soviet

Union. The Xinjiang Uyghur Autonomous Region of the People's Republic of China is conspicuous by its absence. Although certainly a part of Central Asia and linguistically, ethnically, and culturally closely linked to Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, and Uzbekistan, the political and military situation in Xinjiang preclude the discussion (and arguably the existence) of NGOs and a civil society within it.

The partially academic nature of the volume under review is evident both from the list of organizations which supported it and the authors who wrote the various chapters in it. The former include the Carnegie Corporation of New York, the Earhart Foundation of Ann Arbor, and the Central Asia Institute of Bozeman, Montana, as well as a few presumably wealthy and / or influential individuals from Seattle and Tacoma. As for the authors, there is a marked preponderance of activists over academics. The scholars include Reuel Hanks (Assistant Professor of geography at Oklahoma State University), Aziz Niyazi (Institute of Oriental Studies of the Russian Academy of Sciences), S. Frederick Starr (former President of Oberlin College who was Chairman of the Central Asia--Caucasus Institute at the time that the conference leading to this book was held), and Daniel C. Waugh (Associate Professor of history and international studies at the University of Washington). The activists include Jay Cooper (Director of Counterpart International in Kyrgyzstan and Training Coordinator for the "NGO Support Initiative in Central Asia"), Scott Horton (a partner in an international law firm with offices in New York and Moscow and affiliated offices in St. Petersburg, Nizhny Novgorod, Kyiv, Tbilisi, Erevan, Baku, Tashkent, Bishkek, and Almaty who founded his firm's practice in the Commonwealth of Independent States [CIS -- the association of former Soviet republics] and who formerly served as the counsel to Andrei Sakharov, Elena Boner, Sergei Kovalev, and other leaders of the Russian human rights and democracy movements), Ula Ikramova (Program Officer and Competition Coordinator for the Eurasia Foundation Central Asia Regional Office in Tashkent), Oleg Katsiev (Managing Director of Internews Almaty in Kazakhstan), Erkinbek Kasybekov (Consultant to the Kyrgyz Ministry of Labor and Social Protection under the Social Sector Adjustment Credit program funded by Japan through the World Bank), Alla Kazakina (a Russian attorney who practices as a foreign legal consultant in New York City), Kathryn A. McConnell (Creative Services Manager, Document Sales Division, U.S. Government Printing Office), Abdummanob Polat (Chairman of the Human Rights Society of Uzbekistan and Director of the Union of Councils' Central Asian Human Rights Information Network), M. Holt Ruffin (Executive Director of the Center for Civil Society International), Kate Watters (Director of Programs at the Initiative for Social Action and Renewal in Eurasia), and Evgeny Alexandrovich Zhovtis (founder and Executive Director of the Kazakhstan International Bureau for Human Rights). Several of the authors already named may straddle both sides of the academic-activist fence, but they tend to fall more heavily into one or other other of these two camps. The orientations of a few of the authors, however, are more difficult to identify. Among these is Olivier Roy (Senior Researcher at the Centre Nationale de Recherches Scientifiques [CNRS] in Paris and consultant for the French Ministry of Foreign Affairs).

The specific topics addressed include the legal regulation of NGOs in Central Asia, freedom of association, the relationship between governments and NGOs, the environment and ecology, collective farms, media, democracy, cultural and ethnic identity, women's rights and roles, and Islam. Ruffin, who wrote the Introduction to the book, frankly admits that the reason Central Asia has elicited so much attention in the West since the breakup of the Soviet Union in 1991 is "largely for economic and geopolitical reasons." With the collapse of the Soviet Union, there has been a vacuum of power, authority, organization, and even values in these republics. Foremost in the minds of virtually all of the authors is precisely what will be the nature of the social, political, and cultural factors that will inevitably fill up that vacuum. The overall message of the book is that the five countries of Western Central Asia that it focuses upon were not ready for independence when the Soviet Union disappeared. The purpose of the book is both to assess the current status of the Central Asian republics and to provide some signposts for desirable directions in which to point them.

Although the book understandably devotes most of its attention to current and very recent affairs, here and there we find scattered about useful historical and cultural (especially religious) information that helps to put contemporary affairs in context. One of the big dilemmas facing Western-oriented, liberal planners is the place of Islam in all of these nations. While, on the one hand, there is much concern about the fundamentalist aspects of which could lead to a Huntingtonian "clash of civilizations," on the other hand the prospect of dictatorial, single-party states seems equally unappealing. Consequently, one of the themes of the book is the reiteration of the hope that Islam is not incompatible with a civil society.

The second part of *Civil Society in Central Asia* (beginning from p. 235) is a list of relevant organizations in the five countries treated in the volume. Entries include telephone numbers (with country codes and city codes), postal and e-mail addresses, fax numbers, names of contact persons, and brief notes about the history and purpose of the various organizations listed. One thing that I find particularly striking about the list is that more of the organizations are headed by women than by men. One wonders how to explain this in light of the male-dominated societies of all of the countries in question. Perhaps this is a way for the women to rebel and to pursue more democractic rights.

Furthermore, it is ironic, but also sharply indicative of the deep and lasting impact of Czarist and Soviet Russia upon these countries, that the names of most of the organizations are given in English and in Russian, but not in any of the indigenous Turkic languages. The list is quite lengthy and impressive; much of the credit for compiling it must go to Bryan Bushley, a graduate student at the Univesity of Washington.

The second main part of the book is followed by a directory of online resources for keeping abreast of developments in Central Asia. Since this is the only part of the book where Xinjiang is conspicuously (and somewhat ironically) present, we may predict that there will be a continuing incentive for the formation of NGOs and a civil society in Eastern Central Asia, despite the best efforts of the Communist authorities in Beijing to suppress them.

Two maps showing national boundaries and giving the locations of cities named in the volume may be found just after the Preface. The last section of the book is a glossary of about 50 foreign words and special terms. There is no index.

Mette Halskov Hansen. *Lessons in Being Chinese: Minority Education and Ethnic Identity in Southwest China.* Studies on Ethnic Groups in China. Seattle and London: University of Washington Press, 1999. 205 pp + xxi. 12 photographs, bibliography, index.

Reviewed by Sara Davis
University of Pennsylvania

In *Lessons in Being Chinese*, Mette Hansen sets out to examine the Naxi of China's Yunnan province and the Tai Lüe of southern Yunnan, in order to discover what minorities learn about themselves through the state education system. The aim of this state system is the production of Chinese national subjects and, in the long term, the cultural assimilation of ethnic minorities. Yet recent uprisings and minority religious movements in Xinjiang, Tibet, and Yunnan suggest that these goals are not being achieved, and the percentage of Chinese citizens identifying themselves as minorities increases yearly. Why, Hansen asks, does an ostensibly standardized national system appear to be increasing ethnic difference, instead of eliminating it? Why does it succeed in some regions and not in others?

The resulting work is a significant contribution to the study of ethnic minorities in China. It will also be of interest to scholars of education, nationalism, and sociolinguistics. Hansen concludes that the state educational system "preaches the constitutional equality of *minzu* while impressing on minority students immense feelings of cultural inferiority" (Hansen, 1999: 4). At every turn, it seems, minority students are told that their native cultures are primitive, their languages backwards, and their institutions feudal.

Hansen has served her fair share of time in Yunnan's dusty schoolrooms and on its bumpy back roads. Her fieldwork includes 173 formal interviews over a period of three years, as well as participant observation conducted in schoolrooms and village homes.

In Lijiang (northern Yunnan), she finds that textbooks describe the Naxi as "culture-lovers" who are also "willing to learn from more advanced cultures" such as the Han. She argues that the relatively high rate of Naxi assimilation to the Chinese state conversely makes it possible for Naxi leaders to create alternative minority institutions -- societies and institutes -- that are not perceived as threatening to the state.

In Sipsongbanna (southern Yunnan) the situation is different. Tais in Sipsongbanna could also be fairly described as "lovers of culture", since for centuries they have preserved a written tradition and educational system in Buddhist monasteries -- but they are not. Rather, Han teachers and Party officials see Buddhist "superstition" and the persistence of the Tai script as an obstacle to a proper Chinese education. Nonetheless, Buddhism, dealt a serious blow during the Cultural Revolution, has been rapidly reviving in Sipsongbanna. Hansen notes that in 1994, there were 509 monks and 5,336 novices in Sipsongbanna (Hansen, 1999: 111); by 1998, I found a total of 7,500 monks and novices. Dissatisfied with the government's (mis)representation of their culture, Tai parents are increasingly voting with their feet by sending sons to the village temple instead. This is a move that appears to resist the state.

Hansen also shows a commendable (and all too rare) awareness of Tai studies done in Thailand. She avoids the pinyin system of romanization for Tai names, which, because it is based on Chinese characters, usually mauls Tai names into unrecognizability (e.g., turning the Tai "Sipsongbanna" into pinyin "Xishuangbanna"). Instead she uses a Thai-based system of romanization ("Sipsong Panna"). Strictly speaking, this is ideal, as the Tai are a linguistic minority in Thailand, too. However, Thai romanization is much closer to Tai than is pinyin, and we would do well to follow her lead.

As Harrell, the series editor, argues elsewhere, the Confucian "civilizing project" approach to border cultures is comparable to the contemporary Chinese state discourse of Han social evolution and minority "backwardness". In the depth and sensitivity with which it examines these unequal power relationships, Hansen's work sets a new standard.

Harold D. Roth. *Original Tao: Inward Training and the Foundations of Taoist Mysticism.* Translations from the Asian Classics. New York: Columbia University Press, 1999. xv + 275 pages. ISBN 0-231-11564-4.

"Inward Training" is the author's translation of "Nei-yeh" 内業, the title of chapter 48 of the *Kuan-tzu* 管子. As Roth argues, the "Nei-yeh" has been buried for centuries in this compendium, and has consequently not received the attention it deserves, but recent advances in the study of ancient China reveal its foundational position in the history of Taoism. Though published in the series "Translations from the Asian Classics," *Original Tao* is more than just a translation; it includes an incisive discussion of the rhetoric of the "Nei-yeh," its concepts of inner self-cultivation, and its relation both to other early Taoist lineages and to other mystical traditions across the globe. It is an estimable achievement by one of the foremost scholars of early Taoism in North America.

An ancillary accomplishment of *Original Tao* is to reclaim "Taoism" as a workable category for early Chinese intellectual history. As is well known, the term "Taoism" has come under considerable scholarly attack when applied to any tradition or philosophy other than the religion founded by Chang Tao-ling 張道陵 around A.D. 142.[1] The argument has been, essentially, that before the founding of the Taoist religion, there was no identifiable group of believers or practitioners in China who identified themselves as "Taoist," and that the *Lao-tzu* 老子, though accepted by Chang Tao-ling and his followers as the preeminent Taoist revelation, contains a philosophical outlook that is in many respects antithetical to the precepts of the Taoist religion. Many scholars, therefore, recommend avoiding the term altogether when dealing with the *Lao-tzu*, *Chuang-tzu* 莊子, and related texts that have been conventionally classified as "Taoist."

In *Original Tao*, Roth argues, on the contrary, that "a distinct group of people existed who can justifiably be labeled Taoists because they followed and recommended to others an apophatic practice of breathing meditation aimed at the mystical realization of the Way and its integration into their daily lives" (p. 173). Roth suggests, furthermore (pp. 181-85), that

[1] Cf. e.g. Michel Strickmann, "On the Alchemy of T'ao Hung-ching," in *Facets of Taoism: Essays in Chinese Religion*, ed. Holmes Welch and Anna Seidel (New Haven and London: Yale University Press, 1979), 164ff.; and Nathan Sivin, "On the Word 'Taoist' as a Source of Perplexity," *History of Religions* 17.3-4 (1978), 303-30.

although none of these practitioners may have called themselves "Taoists" (or adherents to the *tao-chia* 道家, the category invented by Ssu-ma T'an 司馬談 [d. ca. 110 B.C.] in his famous outline of classical Chinese thought),[2] there was indeed a term that they regularly used to identify their unique practices: *tao-shu* 道術, "the techniques of the Way."[3] Roth concedes that other thinkers also adopted this term and applied it in different ways, "but the evidence is strong of a consistent and a predominant use of the 'techniques of the Way' in Taoist sources from the *Chuang Tzu* to the *Huai-nan Tzu*, where it refers to the techniques of inner cultivation" (p. 184).[4]

In Roth's view, then, the essence of Taoism is what he calls "inner cultivation," or meditative practices which "aim to generate and retain vital essence through developing an inner tranquility and an inner power associated with attaining the numinous 'mind within the mind,' the nondual awareness of the Way" (p. 109). Since the "Nei-yeh," as he cogently argues, is the oldest surviving text describing these practices (see esp. pp. 185-90), it "must be regarded as one of the foundations of Taoism" (p. 185). Most specialists would have been aware of the

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[2] Preserved in "T'ai-shih kung tzu-hsü" 太史公自序, *Shih-chi* 史記 (Peking: Chung-hua, 1959), 130.2288-93. There is some disagreement as to whether Ssu-ma T'an, and not his son Ssu-ma Ch'ien 司馬遷 (145?-86? B.C.), actually wrote the text as it has survived. See e.g. Wang Ch'ü-ch'ang 王蘧常, *Chu-tzu hsüeh-p'ai yao-ch'üan* 諸子學派要詮, Chung-hua wen-shih ching-k'an (Peking: Chung-hua, 1936; rpt., Shanghai: Chung-hua shu-chü and Shanghai shu-tien, 1987), 159n.1.

[3] Incidentally, this contention also appears in Roth's "The *Laozi* in the Context of Early Daoist Mystical Praxis," in *Religious and Philosophical Aspects of the* Laozi, ed. Mark Csikszentmihalyi and Philip J. Ivanhoe, SUNY Series in Chinese Philosophy (Albany, 1999), 61.

[4] Roth acknowledges "two uses of this phrase in Confucian sources," but there are many more appearances of the term *tao-shu* in non-Taoist material. For example, it is found three times in the *Mo-tzu* 墨子, in the "Shang-hsien shang" 尚賢上, "Fei-ming hsia" 非命下, and "Fei-Ju hsia" 非儒下 chapters; see Wu Yü-chiang 吳毓江, *Mo-tzu chiao-chu* 校注, ed. Sun Ch'i-chih 孫啓治, Hsin-pien Chu-tzu chi-ch'eng (Peking: Chung-hua, 1993), 2.8.66, 9.37.424, and 9.39.438, respectively. The term also appears twice in other chapters of the *Kuan-tzu* that focus on statecraft: "Chih-fen" 制分 and "Chün-ch'en hsia" 君臣下; text in Tai Wang 戴望 (1783-1863), *Kuan-tzu chiao-cheng* 校正, Chung-kuo ssu-hsiang ming-chu (Taipei: Shih-chieh, 1990), 10.29.161 and 11.31.174, respectively. In the above passages, *tao-shu* seems to have an effective meaning of moral or statesmanlike excellence. Similarly, the phrase *yu tao-shu chih shih* 有道術之士 appears in various contexts where the referents can hardly be practitioners of meditation. See e.g. the "Nan-yen" 難言 chapter of the *Han Fei-tzu* 韓非子; text in Ch'en Ch'i-yu 陳啓猷, *Han Fei-tzu chi-shih* 集釋, Chung-kuo ssu-hsiang ming-chu (Peking: Chung-hua, 1958; rpt., Taipei: Shih-chieh, 1991), 1.3.49f. The figure of Yen-tzu 晏子 also refers to *yu tao-shu chih shih* in the item entitled "Ching-kung wen kuo ho huan" 景公問國何患 in the *Yen-tzu ch'un-ch'iu* 晏子春秋; text in Wu Tse-yü 吳則虞, *Yen-tzu ch'un-ch'iu chi-shih* (Peking: Chung-hua, 1962), 3.196; and see the parallel in "Cheng-li" 政理, *Shuo-yüan* 說苑, in *Han-Wei ts'ung-shu* 漢魏叢書 (1592; rpt., Ch'ang-ch'un: Chi-lin Ta-hsüeh, 1992), 7.413c (where the lecture is attributed to Kuan Chung 管仲 rather than to Yen-tzu). (There are many other versions of the last story in classical texts, but as far as I know, only the two listed above contain the term *tao-shu*.)

Roth also does not cite an important recent article on *tao-shu*: Mark Csikszentmihalyi, "Chia I's 'Techniques of the Tao' and the Han Confucian Appropriation of Technical Discourse," *Asia Major* (third series) 10.1-2 (1997), 49-67.

importance of the "Nei-yeh" even before reading this book, but Roth's claims are astonishing and unprecedented: the "Nei-yeh" is not simply one of many early Chinese texts; it is our earliest source for Chinese mysticism and represents the absolute beginning of Taoism. Hence the title *Original Tao*.

There are occasions where Roth's emphasis on meditation leads him to positions with which not all readers may agree. The translation "Inward Training," for example, is not as natural as it sounds. Roth lists the meanings "work, deed, achievement" for *yeh* (p. 11)—but there is a substantial difference between "achievement" and "training." The latter suggests a constant regimen of practice, and this nuance is not normally present in the term *yeh*. While the "Nei-yeh" may well derive from a group of practitioners who attempted to describe their meditative routine, that particular purpose is not as apparent from the title "Nei-yeh" as Roth makes it seem.

Similarly, Roth regularly translates the term *cheng* 正 as "align" (thus *cheng-hsing* 正形: "align the body," p. 56), by which he intends a precise technical meaning: "To align the body means to sit squarely and firmly in one place and harmonize the flow of the vital energy within the body's five systems" (p. 221, n. 48). All this from the humble little term *cheng*? Can a more general "rectification" of the body not be intended here? Elsewhere, Roth elaborates:

> "Aligning the body" and "aligning the four limbs" are closely related. From their basic meaning, they seem to refer to sitting in a stable posture in which the limbs are aligned or squared up with one another. Sitting in a stable position with the spine erect is a posture described in the macrobiotic hygiene texts of Ma-wang-tui and Chang-chia-shan. Therein, in such a posture, one practices a form of circulation of the vital energy. This is also the basic posture in which Buddhist meditation was practiced in India and China. Chuang Tzu also makes reference to such a posture in his famous passage on "sitting and forgetting." For these reasons and those provided by the larger context of *Inward Training*, "aligning the body" and "aligning the four limbs" appear to refer to a specific posture within which breath meditation was practiced. (p. 110)

Roth is correct that "sitting in a stable position with the spine erect is a posture described in the macrobiotic hygiene texts of Ma-wang-tui and Chang-chia-shan," but it is significant that those works do not typically refer to this practice as *cheng*. They use such phrases as "sitting up" 起坐 or "straightening the spine" 直脊,[5] but not "aligning the body." In the absence of any

[5] See "Shih-wen" 十問, *Ma-wang-tui Han-mu po-shu* 馬王堆漢墓帛書 (Peking: Wen-wu, 1985), IV, 149; and "T'ien-hsia chih tao t'an" 天下至道談, *Ma-wang-tui Han-mu po-shu*, IV, 164. The text from Chang-chia-shan to

other attestation of *cheng* with such a peculiar meaning, it is tendentious to insist that it must refer to a specific meditative posture. Again, while the "Nei-yeh" surely contains unmistakable references to meditation, and while the authors may have been thinking of such postures as "sitting up and straightening the spine," these conclusions are not self-evident from the unadorned phrase *cheng-hsing*.

Consider also the section that Roth calls "verse XVII":[6]

> 凡道
> 必周必密
> 必寬必舒
> 必堅必固

This list of attributes evidently refers to the Way itself:

> Whatever is the Way
> Must be universal, must be mysterious,
> Must be broad, must be easy,
> Must be firm, must be solid.

But Roth takes each of these terms as the technical name for a meditative position:

> For all [to practice] this Way:
> You must coil, you must contract,
> you must uncoil, you must expand,
> You must be firm, you must be regular [in this practice]. (p. 78)

In a footnote, he explains:

> I take coiling/contracting and uncoiling/expanding to refer to breathing and the entire passage to refer to a practice of meditation in which one pays careful attention to breathing. Although veiled in technical language, this is one of the earliest references to regularized breathing meditation in the extant literature. I surmise that the text is not more specific because it was originally intended as an

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which Roth alludes is the so-called *Yin-shu* 引書 (*Pulling Book*); see Chang-chia-shan Han-chien cheng-li tsu 張家山漢簡整理組, "Chang-chia-shan Han-chien *Yin-shu* shih-wen" 張家山漢簡引書釋文," *Wen-wu* 文物 1990.10, 82-86. For an authoritative discussion of these texts, see Donald Harper, *Early Chinese Medical Literature: The Mawangdui Medical Manuscripts*, The Sir Henry Wellcome Asian Series (London and New York: Kegan Paul International, 1998), 110-47; and *idem*, "The Bellows Analogy in *Laozi* V and Warring States Macrobiotic Hygiene," *Early China* 20 (1995), 381-91.

[6] Roth divides the "Nei-yeh" into sections of various length, which he calls "verses." While this usage of the term "verse" is accepted by *OED* (definition 5: "A small number of metrical lines so connected by form or meaning as to constitute either a whole in themselves or a unit in a longer composition; a stanza"), it is somewhat unusual, and most readers would expect a word like "stanza" or "strophe." *The Random House College Dictionary* (revised edition, 1979), for example, writes that this sense is "not in technical use," adding: "VERSE is often mistakenly used for STANZA, but is properly only a single metrical line."

> oral instruction given by masters to disciples. (p. 224, n. 80)

Maybe this is "one of the earliest references to regularized breathing meditation in the extant literature," but Roth offers no reason why one should not read this passage as a straightforward description of the Way in its magnificent non-duality (universal yet mysterious, broad and easy yet firm and solid). The problem with interpreting such ordinary passages as "veiled technical language" is that it becomes unclear when to stop. How do we decide whether any reference at all is what it seems—or really a "veiled" technical term for some arcane practice?[7]

One final general comment before proceeding to specific issues in the translation. Roth adopts a great number of emendations to the text. There are, to be sure, cases where it is obvious that the text must be emended somehow, and the frequency with which homophones and near homophones are interchanged lends considerable support to Roth's thesis that the "Nei-yeh" is a written collation of teachings that were originally transmitted orally. But sometimes Roth accepts commentarial emendations that do not materially improve the original. Section XXIII is a case in point. In the received text, the opening lines read:

> 凡食之道
> 大充傷
> 而形不臧

This yields a satisfactory sense as it stands; I would translate the lines as follows:

> As a general rule, in the Way of eating,
> If you stuff yourself greatly, it causes injury,
> And the body will not store [the nourishment].[8]

But Roth emends the text to read:

> 凡食之道
> 大充氣傷
> 而形戕

which he translates as—

> For all the Way of eating is that:
> Overfilling yourself with food will impair your vital energy
> And cause your body to deteriorate. (p. 90)

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[7] Some of Roth's renderings are also forced here; *ku* 固 does not normally mean "regular," for example.

[8] Or possibly "And it is unsuitable for the body"—*pu-tsang* 不臧 can mean "inappropriate, incorrect," as in *Shih-ching* 詩經, Mao 54 ("Tsai ch'ih" 載馳): "I regard you as incorrect" 視爾不臧.

Are those changes really necessary?

And Roth virtually rewrites the end of this same section. Here is the original text:

飽則疾動
飢則廣思
老則長慮
飽不疾動
氣不通於四末
飢不廣思
飽而不廢
老不長慮
困乃速竭

These lines are not easy to construe, but they could be translated as follows:

If you are full, then hasten your movements.
If you are hungry, then broaden your thoughts.
If you are old, then plan far ahead.
If you do not hasten your movements when you are full,
Your *ch'i* will not pass freely to your four extremities.
If you do not broaden your thoughts when you are hungry,
You will not stop [eating] when you are full.
If you do not plan far ahead when you are old,
You will be quickly exhausted when you encounter hardship.

This is how the text appears after Roth's emendations:

飽則疾動
飢則曠思
老則忘慮
飽不疾動
氣不通末
飢不曠思
食而不止
老不忘慮
淵乃速竭

which he translates as—

When full, move quickly;
When hungry, neglect your thoughts;
When old, forget worry.
If when full you don't move quickly,
Vital energy will not circulate to your limbs.
If when hungry you don't neglect thoughts of food,
When you finally eat you will not stop.
If when old you don't forget your worries,
The fount of your vital energy will rapidly drain out. (p. 90)

In addition to the fact that they are tenuous on text-critical grounds, Roth's multiple emendations here fail to improve the sense of the text. If anything, they diminish it.

Most of these emendations have been suggested by previous scholars, but there are a few cases where Roth proposes his own. The end of section XVII, for example, according to the original text, reads:

既知其極
反於道德

Once you know the extremes,
You will return to *tao* and *te*.

Roth changes *chih* 知 to *chih* 致, and translates the clause as "And when you reach its ultimate limit..." (p. 78). However, *chih* does not exactly mean "to reach"; it means "to bring about," or, at best, "to cause to reach" (in other words, it is generally the causative of *chih* 至). *Chi chih ch'i chi* 既至其極 would be close to what Roth has in mind here—but what was wrong with the text as it stood?[9]

Some minor comments on the translations.

Section I. "When flowing amid the heavens and the earth/We call it ghostly and numinous./When stored within the chests of human beings,/We call them sages" (p. 46). There are two anacolutha in this translation: strictly speaking, "flowing" is a misplaced modifier and "stored" is a dangling modifier. ("Them" also has no grammatical referent.)

Section V. The text reads 彼道之情，惡音與聲？修心靜音，道乃可得. Roth emends both occurrences of *yin* 音 to *yi* 意 (p. 55). But *yin* ("tone") appears correct here: "The state of the Way—how could there be tones or sounds? In cultivating your mind, quiet your sounds; then the Way can be attained." The point seems to be that the Way is fundamentally silent, and that we should imitate this state in ourselves.[10]

Section VI. The text reads 人之所失以死，所得以生也, which Roth translates as: "When

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[9] Incidentally, I think "the extremes" 其極 refer to the excesses of *yin-po* 淫薄, or "overflowing and exiguousness," which we are told to "chase away" 逐 in the previous line—and not, as Roth's translation implies, to the limit of the Way. Roth emends *chu yin-po* 逐淫薄 to *chu tse shih po* 逐澤釋薄, which is also wholly unnecessary.

[10] Cf. e.g. Paul Rakita Goldin, *Rituals of the Way: The Philosophy of Xunzi* (Chicago and La Salle, Ill.: Open Court, 1999), 30f., where the concept of "tranquility" 靜 in the "Nei-yeh" is contrasted with Hsün-tzu's 荀子 use of the same term.

people lose it they die;/When people gain it they flourish" (p. 56). *Sheng* 生 probably has its normal sense of "to live" here, especially since it is being contrasted with *ssu* 死, "to die."

Section XI. The text reads 淫然而自至; Roth emends the *chih* to *lai* 來, and translates: "Then it will gradually come on its own" (p. 66). The basic meaning of *yin* 淫 is "a heavy downpour" (hence the prepotent derived meaning of "promiscuity" or "licentiousness"), so I would say, "Like a torrent, it will arrive on its own." *Yin* appears repeatedly in the "Nei-yeh," and Roth treats it differently each time: in section IV, he has "surging forth" (p. 52); here he has "gradually"; in section XII, he has "overflow" (p. 70); in section XIX, he has "overstimulated" (p. 82).

Section XIII. The text reads 嚴容畏敬, which Roth translates as "Be reverent and diligent" (p. 70). "Reverent" does not capture *yen-jung* 嚴容 very well, and "diligent" completely misses the element of fear in *wei* 畏. I would say: "Be of stern countenance, and reverent as though in dread."

Section XV. The text reads 乃能窮天地, which Roth translates as "You can then exhaust the heavens and the earth" (p. 74). "Exhaust" is a kind of stock-translation for *ch'iung* 窮 (see also section XVI, where Roth has "exhaust everything within the Four Directions" for 窮於四極, p. 76), but this is not very meaningful in ordinary English. I would say "go to the limit of Heaven and Earth"; the intended image is probably one of shamanistic spirit-flight (especially since the next line speaks of "covering the Four Seas" 被四海).

Section XVIII. The text reads 善氣迎人，親於兄弟。惡氣迎人，害於戎兵. Roth translates this as follows: "If with this good flow of vital energy you encounter others,/They will be kinder to you than your own brethren./But if with a bad flow of vital energy you encounter others,/They will harm you with their weapons" (p. 80). The first half of the translation is fine, but *hai yü jung-ping* 害於戎兵, preserving the parallelism, must mean: "They will be fiercer than weapons."

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In spite of these criticisms, there can be no doubt that *Original Tao* is a powerful and original book. The translation is a major accomplishment in itself, lucid and coherent. But Roth's discussion of the significance and import of the "Nei-yeh" is even more impressive because his conclusions are completely without precedent. In this vein, Roth's willingness to read even unexceptional passages in the text as references to specific meditative practices is understandable. He is convinced that the varieties of mystical experience are universal, and that the "Nei-yeh" describes states of consciousness essentially similar to those described in other traditions of meditation (such as Zen, etc.) whose particular practices are known to us. Even those readers who are not always convinced by his interpretations must acknowledge that they are original, trenchantly argued, and intellectually challenging.

PAUL RAKITA GOLDIN

UNIVERSITY OF PENNSYLVANIA