

## **Debate and Discussion**

### ***Comment by the Managing Editor***

Rather pungent remarks are often exchanged among scholars. When such a diatribe occurs, it is the policy of the *Journal* to publish both the criticism and the retort, without anything more, leaving it to the reader to choose sides, if he or she wants. We must remain neutral. Let us, however, point out that the question of long distance migrations in prehistoric times is a disputed issue. Let us only say that, at an early stage, the Indo-Europeans were equipped to undertake long treks over hostile territories, and were able to impose their language upon substrate populations.

## **Modern Necromancy, or How to Make Mummies Speak**

Stefan Zimmer  
*University of Bonn*

This is a critical review, written upon request by the managing editor, of the series of articles published under the heading 'The Mummified Remains Found in the Tarim Basin' in *JIES* 23.3/4, fall 1995 (published spring 1996), 279-444. The sensation caused by the discovery of the mummies has led to a neglect of methodological principles by a number of authors. The necessity of strictly adhering to the most cautious scrutiny, especially in such complex problems as raised by the mummies, is underlined. The possible contribution of Tocharian linguistics and philology is sketched with additions to the relevant bibliography.

I. The following is a reaction to the publication of 'The Mummified Remains Found in the Tarim Basin', a collection of papers guest-edited by Victor H. Mair in *JIES* 23.3/4, fall 1995 (published spring 1996), 279-444. This section of the journal is introduced with the anonymous statement (p. 279): 'Numerous mummified corpses have been uncovered recently in the Tarim

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Basin of Xinjiang. These have been dated from the second and first millennia BC and have attracted much attention because of their physical resemblance to North Europeans. It has been suggested that they may be ancestral to the Tokharian speakers later located in that area, whose language is known from historical records.'

II. The extremely dry continental climate of the Tarim Basin in the heart of Central Asia is responsible for the preservation, in mummified state, of dead bodies when disposed of in such a way that neither animals of prey nor elementary forces could destroy them. No wonder then, that mummies have been found there, earlier sporadically only by the first explorers a hundred years ago, now in considerable numbers ('more than 100' is all we are told) during systematic explorations. What astonishes the 20th century public is an obvious physical resemblance of these mummies to the European physical type (called also 'Caucasoid' and 'Caucasian' by various authors), not to the North-Asian type ('Mongoloid') of the modern surrounding populations. This resemblance seems to be regarded as a great sensation by Mair, an American scholar with special interest in Sino-Indian and Sino-Iranian cultural contacts according to his own words. In consequence to his personal excitement, he tried to arouse the public by a number of articles in popular and learned journals, obviously in order to raise the financial support needed to realized the ambitious research programme he has developed.

III. I should like to thank the editor of this journal for his kind invitation to comment on the collection which nearly amounts to a special issue of *JIES*. Purely biological aspects (genom analysis, cranial statistics, and the like), by their very nature, fall out of comment by an Indo-Europeanist with special interest in Tocharian as well as in Indo-European Cultural Studies. The articles seem to be partly a pre-publication of papers to be read at an International Conference on the subject, organized by Mair at Philadelphia (April 19-21, 1996),<sup>1</sup> and partly a justification for his endeavour. Such a collection is by necessity quite heterogenous, even unbalanced. Three linguists (Adams, Pulleyblank, Ringe) and two archaeologists (Mallory, Xu), but

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<sup>1</sup>I thank Dr. Mair for his kind invitation to this conference. Unfortunately, I was unable to participate.

also technical specialists (E. Barber, P. Barber, Good, Francalacci, Opie[?]) have contributed overviews and methodological reflections, and a few studies of scientific details. The lack of more historical background information, and of a comprehensive contribution from the side of Iranian Studies is painfully felt. A good map with names in the various languages would have been of help for the reader. As constant border-crossing from one field of scholarship to the next is likely and sometimes even compulsory in treating such complex questions, the grave risk of one scholar taking for granted his neighbours' more or less qualified hypotheses is hanging over the whole discussion. The guest editor has given no reason for his choice of collaborators nor for the unsystematic arrangement of the respective articles, so that a certain momentum of accident is clearly felt.

IV. It should be clearly stated at once that those mummies do in fact present the scientific community with a number of big questions and most intriguing problems. There can be no doubt about the seriousness of Dr. Mair's scholarly aims, but what pleads for extreme carefulness is his obvious emotional excitement which seems to have led him to a number of all too rash assumptions. The mixture of physical anthropology with human history has been discredited to the bottom by political misuse, not only but most spectacular in the recent past. Unwise intermingling of natural and cultural factors is still a major factor in nourishing racism all over the world in our own days. Historical linguistics, in spite of constant warnings by (unfortunately, all too few) serious scholars<sup>2</sup> has equally been misused, and is still often grossly misunderstood by many contemporary writers as a means of direct access to the past. Even today, bold assumptions, ranging from daring hypotheses to sheer nonsense, are constantly published in popular (more or less) scientific literature. Authors are most often insufficiently informed journalists, but also scholars who—

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<sup>2</sup>Cf. e.g. Max Müller (1890: 223) "There are Aryan and Semitic languages, but it is against all rules of logic to speak, without an expressed or implied qualification, of an Aryan race, of Aryan blood, of Aryan skulls, and to attempt ethnological classification on purely linguistic grounds. These two sciences, The science of Language and the Science of Man, cannot, at least for the present, be kept too much asunder ... it would be as wrong to speak of Aryan blood as of dolichocephalic grammar."

being specialists in their own field—accept uncritically hypotheses of their fellows from other disciplines. One of the biggest problems is, of course, the different terminology in different branches of research which, nearly inevitably, must lead to constant misunderstandings (e.g. ‘culture’, ‘people’). Language is still all too often identified with ethnicity, and literature with history.

V. In his opening paper, ‘Prehistoric Caucasoid Corpses of the Tarim Basin’ (281-307), Mair presents ten reasons why the ‘Tarim mummy problem’ is so important to be attacked now. Some are scientific, some personal, and some political. Nearly all historical problems involved need careful checking by specialists. Mair himself shows astonishing little of his own Chinese studies (cf. the articles by Xu and Pulleyblank), but is eager to include everything in his dossier which seems to suit his hypothesis. He states it frankly at the very beginning: ‘the Tarim Basin Caucasoid corpses are almost certainly the most easterly representatives of the Indo-European family’ (282). This seems to misunderstand the purely linguistic notion of language family, in our case Indo-European, with the biological fact of genetical family filiation. It is true that modern sciences present the archaeologist with an astonishing range of possibilities to gain knowledge (e.g. DNA-analysis), but it is equally true that many old questions have to be constantly re-asked and re-discussed because no definite solution has yet been—or even never may be—found (e.g. domestication of the horse; invention of cart, wagon, chariot; spread of Indo-European languages). It is extremely dangerous to invent ‘peoples’ simply on the basis of a few skulls or mummies (e.g. Mair’s Sampul people, 292), still more so to equate those imaginations with vaguely postulated ancestors of historically attested ethnic groups. It is irresponsible to cite, without any qualification, the highly problematical classification of ‘the Wusun people’ as ‘undoubtedly Indo-European’ (292) or to overgeneralize by suggestion as in ‘Central Asian Pamir (Saka)’ (ibid.). A more convincing (or simply more explicit) chain of argumentation for a possible link between the Tocharians and the mummies is given by Adams (403-4): some of the youngest mummies (c. 400 BC - 200 AD) are found in places near the archeologically attested Tocharian settlements. As the great majority of mummies from all chronological layers closely

resemble each other in physical type and clothing, they all may have belonged to one ethnic unity.

Personal feelings<sup>3</sup> must be kept under strict control by the scientific reasoning. The reader more than once feels embarrassed by Mair's unrestrained enthusiasm. To be fair, one may also be amused by the author's comic formulations, e.g. 'who are the corpses' (289) or 'the ... corpses ... were their (viz. the Tocharians, SZ) ancestors' (299). Later on, we even are to swallow that the mummies are the 'ancestors' of 'modern men and women' ... 'in the United States and Europe' (305).<sup>4</sup> It is true that the presence of the Tocharian language, an Indo-European language of allegedly Western type, in the heart of Central Asia poses many questions deserving research, and even that those mummies offer valuable hints for the further investigation of Central Asia's early history. It is quite natural to ask 'who were the Tocharians' (i.e. the people speaking the two languages), but it does not make much sense to state that their 'identity' is 'still an enigma' (297). Their identity can be found out from what they have left us: the archaeological finds, the remains of their literature, the (all too few and imprecise) records in Chinese historical literature (cf. Mayer 1995). For some basic information, see Liu (1969) and von Gabain (1973). The scholarly community will have no problems to acknowledge Dr. Mair's enthusiasm and zeal. But for every step in his—at present rather loose—chain of argumentation (or to be more precise, suggestion), a display of much more critical spirit is urgently needed if he wants to win acceptance for his bold hypothesis among his colleagues.

VI. There are two archeological chapters. Xu Wenkan's report on 'The Discovery of the Xinjiang Mummies and Studies of the Origin of the Tocharians' (357-369) contains, among many useful information, the bold affirmation that 'Most of the ancient residents in Xinjiang were white people, and their languages *must* (my italics) have belonged to the Indo-European language family' (360), thus excluding—without any

<sup>3</sup>Or even superstitions, cf. 'It may be more than sheer coincidence that my father was born in ... Pfaffenhofen not far from the spot where Ötzi the Iceman was found...' (286).

<sup>4</sup>Confronted with such utterances, it may not be pedantic to recall OED's definition (explicitly given as 'ordinary current sense') of *corpse*: 'dead body (of a person)'.

argument—even the possibility of alternatives. But on the whole, the author's views are very prudent, and he is completely right to warn against relating 'nationality, race, language and culture' 'to one another at will' (363). Renfrew's<sup>5</sup> and Anthony's theories are cited with due caution, and the necessity of further research is stressed. Most welcome are references to recent Chinese publications, but speculations about the Tocharians, allegedly consisting of two distinct ethnic layers (367) are far from being substantiated by facts. Sapir's suspicion, cited from a letter to Maenchen-Helfen 'that at least two distinct IE languages appeared in Chinese Turkestan and West China at a quite early date and that these two languages belonged to distinct branches of IE' (367-8) seems to be misunderstood. Sapir, there can be no doubt, thought of Iranian and Tocharian. As Xu wisely reminds (368, n. 33), R. N. Frye's proposal to bring speakers of an early form of Burushaski into the discussion, also deserves attention.

VII. J[ames] P. Mallory, the well-known author of what is currently the best archaeological book on the Indo-European question (1989), presents a sober methodological overview in his 'Speculations on the Xinjiang Mummies' (371-384). He sees three (archaeologically defined) time 'windows' for the genesis of the Tocharians: the Early Iron Age (coming up to Heine-Geldern's 'Pontische Wanderung', 9th-8th centuries BC, which is nowadays commonly regarded as unlikely, and is, after all, too late for the mummies), Andronovo migrations (2nd millennium, more likely to be linked to Indo-Iranian peoples; because Tocharian is not a member of the Indo-Iranian branch, it must have separated earlier); Afanasievo migrations (Early Bronze Age, c. 3500-2200, Mallory's most likely candidate). Surprisingly, Tocharian is refused the status of a 'peripheral language' because it shares so much (i.e. for Mallory, too much) PIE basic vocabulary: 'grain', 'plow', 'cattle', 'ox', 'horse', 'sheep', 'bull', 'milk'. Later terms, only partly shared with other branches, are 'donkey', 'gold', 'wheel', 'wagon'. Mallory's main source for these words is Pokorny's clearly dated IEW (1953)!<sup>6</sup> The author's statements are again very prudent

<sup>5</sup>The eminent Scottish archaeologist, Lord Renfrew, is referred to as 'an English scholar Colin Renfrew' (363).

<sup>6</sup>'Central' vs. 'peripheral' status is defined by phonological, morphological and syntactical criteria in Comparative Grammar, not by lexical items.

(‘there are no serious grounds for excluding the Afansievo culture as non-Indo-European’, 381) and make ample use of the potential mood (‘at least potentially’, ‘may have spoken’, ‘gambit play’ - *ibid.*). His last word, for the time being, is—not unexpectedly—that the question for ‘their (*viz.* the mummies’, SZ) ethnic identity must still remain open’ (382).

VIII. The linguistic contribution consist of three unequal parts Adams and Ringe are historical linguists working on Tocharian, but without published philological and historical experiences. Of quite different weight then is the article of Edwin G. Pulleyblank, one of the leading scholars in Older Chinese language, literature and history. Pulleyblank, asking the obvious question ‘Why Tocharians?’ (415-430) has based his first sketch of a possible scenario on earlier suggestions by Mair and publications of Mallory. Accordingly, the Afanasievo culture on the Upper Yenisei (3rd millenium BC) is proto-Tocharian; the Tocharians arrived from the north c. 2000 BC in the Tarim region (*cf.* the mummies). The reason of Pulleyblank’s dismissal of ‘the supposed migration of the Tocharians from Europe to Xinjiang through alien territory’ as ‘hard to motivate and strain(ing) credulity’ is difficult to see: Such movements are historically well attested, and not only of nomads (*cf.* the American settlers crossing the great plains, or the Boer treks to Transvaal); motives can be too subjective to be obvious to the historian, etc. But whatever opinion is preferred, migrations cannot be an argument in the discussion about the rise of the *centum/satəm* - isogloss this being a strictly linguistic question. By the way, Greek cannot be denied central status within IE, as Pulleyblank seems to imply.

Quite independently from this shaky starting point, Pulleyblank then goes on to give a survey of his personal views on a broad array of historical problems, all inextricably involved in the subject, e.g. the Yuezhi, the domestication of the horse,<sup>7</sup> and the technique of mounted archery.<sup>8</sup> From his rich experience of many decades of research in Chinese, the author ‘warn(s) against the easy assumption that one can extrapolate features of those later nomadic hordes back into prehistoric times’ (420). His consideration of the early Yuezhi history as recorded in Chinese sources leads him to the double

<sup>7</sup>Obviously, Hänsel-Zimmer 1994 had not yet been noticed by the author.

<sup>8</sup>Earlier Hittite and Assyrian cavalries are not mentioned.

assumption that they were 'ethnically and linguistically related to their close neighbors in Loulan' (422) and that 'the native language there was a form of Tocharian' (ibid.), the latter thesis obviously being based on Burrow's loan-word studies of 1935, though he gives no reference. It is important, however, to note that—as Pulleyblank clearly states—even if the local language of Shanshan (= Loulan = Krouraina) really was 'of Tocharian type', this 'does not ... prove anything about the language of the Yuezhi themselves' (423). He adds to the discussion another people called Xiao Yuan 'Little Yuan' placed by Chinese sources between Loulan and Kushan on the Southern Silk Road. The name stands, according to him, for Xiao [Da]yuan, with *Dayuan* the Modern Chinese reading for an Early Middle Chinese transcription of \**Taxwār* = *Tocharoi* (according to Henning); this finds support in Xuanzang's later record (7th c. AD) of the place name Old Duhuolo 'Old Tocharistan'<sup>9</sup> from the region attributed to the Xiao Yuan still later. This collocation of ethnic names is then equated by Pulleyblank with the Western tradition about the Asioi (Strabo)/Asiani (Pompeius Trogus), Sacaraucae or Sacaraulae, and Tocharoi who together conquered Bactria (c. 100 AD). The author refers to his former suggestion that *Yuezhi* reflects a Tocharian \**Ywati* (425) = Greek *Iátoi* (Ptolemy) = Latin *Asii*, but has to admit that many phonological problems remain. Being in no position at all to judge this eminent scholar's handling of Chinese literary sources and of Chinese historical linguistics,<sup>10</sup> I can only say that his theories seem quite often be built on unfounded hypotheses from adjacent fields. Bailey's suggestion e.g., that *Kangju*, a country in the region of Tashkend in the Early Han period 'can probably be equated with Tocharian A *kāñk-* 'stone' (426) is of doubtful value, the unidentified Tocharian word being attested only once (if at all) in a poorly understood metrical (?) Maitreyasamitināṭaka passage: 264 a2 /// *sānārikāñkañ waṣir ṣṣāñ* ∴. This makes the author's assumption that 'the Kangju people ... were Tocharian speakers' (247) hard to believe. In passing, 'Tocharian loanwords in Chinese' are cited: *mi* 'honey' < B *mit*, *shizi* 'lion' < B *ṣecake*, A *śiśāk*.

<sup>9</sup>This seems to be based on a doubtful reading, cf. Brough BSOAS 28. 1965. 593 with note 42.

<sup>10</sup>A word of comment on Baxter 1992 would have been most welcome.



Nobody will find problems with the author's final remarks that 'the Tocharian speaking inhabitants of the oases on the north side of the Tarim Basin were in place long before the time when the Tocharian manuscripts were produced', but much more of solid evidence would be needed to convince of his proposed wider borderlines of 'Tocharian speaking territory', which comes near to the whole Tarim Basin including Gansu beyond Dunhuang (428). This is no doubt an important paper, but barely of direct relevance to the mummies. With respect to the Indo-European problem, it's a pity that Pulleyblank seems not to know Wilhelm E. Mühlmann's painstaking publications on the emergence of nomadism and my proposal to use Mühlmann's model for the investigation of Proto-Indo-European (Zimmer 1990, with references).

IX. Douglas Q. Adams' 'Mummies' (399-413) sketches some<sup>11</sup> general information on the Tocharian languages, with appropriate remarks on the Iranian, Indian and Turkish languages also attested in the area, and discusses possible linguistic affiliations of ethnic units (sometimes tribal names only) attested in Chinese historical texts or otherwise: Xiongnu, Yuezhi, Wusun, etc. He is right to warn, with compelling arguments, against equating the original language of e.g. the Yuezhi with the language remains ascribed to them much later (in this case, at least 500 years later, viz. Bactrian, an Iranian language). Bailey's attempts of finding Iranian roots (in the technical sense of historical linguistics) for many Chinese words is dismissed as being fraught with far too many problems to be reliable.<sup>12</sup> Adams accepts the possibility that at least the younger mummies (found in the Tekes River valley and dated c. 400 BC - 200 AD) 'when alive' (403) spoke some early form of Tocharian. All the other finds are much earlier (c. 1000 BC, c. 1800 BC, c. 4000 BC), but as all those mummies resemble each other, he finds it likely to ascribe to the earlier populations one or more similar, but of course earlier, language(s). This is a

<sup>11</sup>Cf. my bibliographical suppletions to Adams' and Ringe's contributions in paragraph XI below.

<sup>12</sup>Adams is obviously not aware of Ts. Chang's current research (cf. last Chang 1991) which seems much more promising. Comments from sinologists on Chang's reconstructions of Old Chinese would be most welcome to Indo-Europeanists.

reasonable way of thinking though becoming less and less convincing in going backwards in time. It would have been useful at this point to discuss more closely Burrow's theory (1935) that the people or at least a considerable part of the population of Shan-Shan, a short-time kingdom at the Southern Silk Road with centres at Lou-lan, Endere and Niya, spoke a language 'remarkably like the two dialects of Tocharian' (1935, 667). It left us records (and one Buddhist text, the Dharmapada) written in Kroraina Prakrit (also called Gandhara- or Niya-Prakrit, 3rd century AD), which probably was not their everyday language. According to Burrow (who fails to provide a full list), these texts contain roughly 1000 proper names and c. 150 appellatives of obvious non-Indian origin—from a kind of early Tocharian? In fact, we know next to nothing of this postulated language; the few similarities presented by Burrow need to be carefully checked against our present knowledge of Tocharian, and the whole corpus of Gandhara-Prakrit must be taken into account.<sup>13</sup> The unknown substrate language there may have been of any other type (attested in the region or not—how many languages have died out<sup>14</sup> since then, nobody knows). The allegedly Tocharian looking words may be due to secondary contact or adstrat, or may be simply a mirage.

Adams then gives a list of isoglosses linking Tocharian with other IE languages. On the whole, this is common stock since long; a slight addition of disputable items (cf. his theory of *n*-stem adjectives reinterpreted as animate forms, 406) can do no harm. It is well-known since the beginning of Tocharian Studies that this branch is of more Western than Eastern IE type. Nothing more can be gained by the isogloss method. The much more interesting path of detecting morphological archaisms (cf. recent publications by K. T. Schmidt and others) is not pursued. Conclusions on the earlier relative position of Tocharian within the family can only be based on proof of nontrivial positive innovations, shared with specific other branches. Loanwords offer a possibility for detecting earlier contacts between separate languages. Both Tocharian languages, as most probably already their common ancestor

<sup>13</sup>Cf. Burrow 1937, Bailey 1943-46, and the references given by von Hinüber 1983, footnotes 1-2.

<sup>14</sup>Note that some languages found in the Turfan documents are still unidentified today (first informations to be found in Schwentner 1935, 4).

Proto-Tocharian, have had many contacts with spoken (and in later periods, also written) Iranian and Indo-Aryan languages.<sup>15</sup> The large variety of loan words may be arranged according to their respective sources and consequently, periods of borrowing. Adams<sup>16</sup> gives only a superficial classification (407), mainly based on geography, thus disregarding the time depth offered by the much older Iranian tradition: from Khotanese (also called Khotan Saka; Adams fails to mention the related Tumshuqese or Tumshuq Saka which probably is still more important since Tumshuq is close to Tocharian-speaking Maralbashi!), from Sogdian, 'Middle Pahlevi' (sic!, probably for Middle Parthian), or Middle Persian (unspecified further), from 'general eastern Iranian' (without explanation of the term, and unspecified), from an Eastern Iranian language similar to a proto-form of Ossetic (which he thus refuses to call by its commonly used name, Alanic). More attention could have been paid to the possible (and quite probable) existence of so-called 'Wanderwörter' (words of unknown origin which travel widely with the items they denote). Adams admits this category for 'iron'; 'peacock' surely is another example, this bird being indigenous far from Central Asia.<sup>17</sup> The Indian loan-words are all relatively recent and due to the Buddhist mission or missions (cf. the various Buddhist sects attested in Tocharian literature), but at least two layers have to be distinguished: the first are loans of Prakrit terms (probably from Gandhara-Prakrit, and thus c. 3rd c. AD or slightly earlier), the second from Buddhist Hybrid Sanskrit (the canonical language of Central Asian Buddhism). 'Donkey' is either inherited or a Wanderwort (see the details in Mayrhofer EWAia I 473). The author's conclusion that it 'is only a reasonable hypothesis, not a certainty' to assume that the *early* (my emphasis) mummies also 'may have spoken a variety of Tocharian' (410) seems debatable, but rather risky for the time being.

X. Donald Ringe's report 'Tocharians in Xinjiang: The Linguistic Evidence' (439-444) is strangely titled: if by 'Tocharians' the speakers of the two Tocharian languages are

<sup>15</sup>For the latter, Adams used the rather strange 'Indic'.

<sup>16</sup>Who fails to mention the standard work on Tocharian borrowings from Indian and especially Iranian, viz Isebaert 1980.

<sup>17</sup>Coming from the South Asian and Indonesian jungles. The word is thus rather not of Iranian origin as Adams suggests.

understood (what is usually done; the 'Tocharians' in Bactria are always qualified accordingly), they need no account of 'Linguistic Evidence': all we know about their language and literature is contained in Manuscripts found at various places on the Northern Silk Road in what is called nowadays Xinjiang in Chinese. On the whole, his account is reasonable; it adds nothing new to what Adams has said (the two contributions written independently from another). In distinction to Adams, Ringe carefully avoids any reference to the mummies and all the speculations around them—probably the wisest thing a Tocharianist can do at the moment.

The possible contribution of Tocharian historical linguistics to the elucidation of Central Asia's early history has not been sketched by the two authors. We should distinguish three avenues of research, one internal, and two external. Internal investigation consists in detecting traces of the pre-Buddhist culture and history in the Tocharian languages: this could be poetic formulas going back to Proto-IE Dichtersprache (recently, some have been detected by Pinault), or linguistic structures foreign to the IE tradition which can only be due to earlier language contacts, such as a special feminine form for the 1st person singular personal pronoun, the binoms (type *ak-mal* 'face'), Gruppenflexion, and the development of secondary case endings. There are many publications on such items, but no thoroughgoing study of the whole complex. The second and third way of proving interlinguistic contacts are the study of loan words, for practical reasons to be divided between foreign loans in Tocharian, and Tocharian loans in neighbouring languages respectively. All three linguistic papers discussed above have indicated a few points, but none has tried to give at least a possible scheme setting fully out the different chronological and geographical layers. It is clear that only prolonged collaboration among the various fields (Iranian, Tocharian, Turkish, Chinese, and perhaps some more) is able to come to reliable results.

XI. Both Adams' and Ringe's articles signal in quite obvious manner that trans-Atlantic cooperation needs to be strengthened in Tocharian Studies. Both their respective bibliographies show serious lacks. In such a narrow field as Tocharian, it is shocking to see that Pinault 1986, probably the most useful publication in Tocharian Studies of the last 35

years, seems to be unknown to both authors! Whoever cares for Iranian loan words in Tocharian must have come across Isebaert 1980 (being published outside the booktrade, the volume may be hard to find the USA—but inside a small community of scholars, this should be a tiny obstacle). In articles which, among other aims, also intend to provide fellow scholars from adjacent fields with sufficient references, I'd also expect to find mentioned the standard bibliographies (Schwentner 1959 and Zimmer 1976) and *Forschungsberichte* (Schwentner 1935, Thomas 1985, Schmidt 1994a and 1994b, with a few additions in Zimmer 1996). Adams, in mentioning the various Middle Iranian languages, should have referred to the latest international standard handbook (Schmitt 1989). Ringe's sometimes rather bizarre references could raise suspicions about his familiarity with Tocharian Studies: For the short denomination of East- and West-Tocharian as A and B, he refers to Winter and Stumpf! In fact, these were simply F. W. K. Muller's provisional labels in the very beginning of the decipherment (1907).<sup>18</sup>

XII. A few short observations on the technical contributions may be added:

In his study of the technical aspects of 'Mummification in the Tarim Basin' (309-318), Paul T. Barber finds 'stronger evidence for adaptation to local conditions than ... for the geographical origins of these people' (309). So, his only contribution to the discussion is the polite, but rather meagre negative result 'that there is nothing in the funerary rites of the Tarim Basin mummies that militates against a western origin' (318).

Irene Good's 'Notes on a Bronze Age Textile Fragment from Hami, Xinjiang with Comments on the Significance of Twill' (319-345) date it 'approximately 1200 BC', and say it to be 'virtually identical stylistically and technically to textile fragments found in later Hallstatt (C and D) period Europe' (331), 'probably several centuries earlier' and '*characteristically European*' (Good's emphasis, 332). It is supposed to be manufactured on a warp-weighted loom. The spread of this type of instrument is then suddenly linked with the Indo-

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<sup>18</sup>A misprint to be corrected page 441, line 18 read: Schmidt 1989 92-93.

Europeans<sup>19</sup> (with reference to a 40-year-old book by Gimbutas and an article by Mallory not listed<sup>20</sup> in the bibliography). This leads her to speculations about the various PIE homeland theories (only archaeologists are cited, no Indo-Europeanist) which she tries to connect with warp loom finds. All of this is unfounded: there is no PIE word for 'warp', 'warp-weight', or 'loom'. Technological innovations have nothing to do with language or ethnicity, so that the distribution of weaving utensils is scarcely relevant for any attempt to find out the 'ancestors of the Tocharians' (377). Fortunately enough, the author comes to the conclusion 'It is perhaps too early to be able to build a strong case for any one scenario' (338).

Very similar is the subject of 'A Weaver's-eye (sic!) View of the Second Millenium Tarim Basin Finds' by E[lizabeth] J. W. Barber (347-365). She at least refers to the most important fact of prehistoric trade on east-west routes. In spite of being generally well-informed, she cannot refrain (351, n.2) from linking the spread of certain types of spinning whorls with 'several phases of the expansion of the early Indo-Europeans—or Indo-Hittites!' Being a specialist for the history of textiles, she has obviously made quite reasonable use of the extant linguistic literature; but unfortunately, she lacks support from specialists who could have saved her from taking too serious rather weird ideas like e.g. 'Armenian ... represents an Indo-European group that may never have lived outside the Caucasus area' (353) or 'a major catastrophe ... that sent tribes running outward' (355). Her use of the 1st singular plural pronoun—'our reconstruction (sic!)' and 'we linguists' (both 355)—is, to say the least, surprising.

The results of Paolo Francalacci's 'DNA Analysis of Ancient Desiccated Corpses from Xinjian' (385-398) 'are in agreement with a possible European origin of the ancient Xinjiang corpses' (395). His reflexions on similarities (he says 'parallelism', with reference to Darwin, but this is far too strong) of biological and linguistic evolution is sound, but his very examples (Sardinians and Hungarians) should have prevented him from believing that 'population genetics can

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<sup>19</sup>It is strange to find just here so precise ideas on the internal structure of the IE language family p. 336, the proto-Greeks are mentioned as becoming 'differenciated from proto-Celts and proto-Indo-Iranians'. Unsurprisingly, no arguments are adduced for this statement.

<sup>20</sup>According to the page number indicated, 1987 is not a misprint for 1989.

help to find correlations among language families' (396): Sardinians, speaking an archaic Romance language, are genetically distinct from all surrounding populations; Hungarians, speaking the only non-IE language in Central Europe, are genetically indistinguishable from their neighbours. Fully unacceptable is the author's further affirmation that the 'Caucasian features suggest a relationship with the Tocharians' (ibid.). This forgets not only about the enormous time-span between the majority of the mummies under investigation and the date of the historically attested 'Tocharians', but also about possible alternatives offered by the complex (mostly unknown) prehistory of the Indo-European language family, and about the existence of various other European ethnic groups which may or may not have spoken IE.

The only strange part—perhaps meant as a satirical element?—in the whole collection is James Opie's 'Xinjiang Remains and "the Tocharian Problem"' (431-437). He attempts to combine two extreme opinions. One is W. B. Henning's equation of the tribal names *Guti* and *Tukri* attested in Babylonian sources from the end of the 3rd millennium BC with (Old Indian) *Kuca* and (Uigur) *twyri*, which Henning himself refrained to publish during his lifetime. The other is J. Haskin's (never published) identification of the Ta Yuezhi with the Massagetae (allegedly *Massa-Getai* 'greater Getai') who are—still according to Haskin—related to the Celts! Whereas the first may be tolerated as an educated guess of that eminent Iranian philologist, the second<sup>21</sup> is simply proof of his author's, an archaeologist's, complete ignorance in matters linguistic. The Getai were Thracians; the Massagetai are scarcely related to the Getai; the Thyssagetai or Thyrsagetai seem to have been Scythians, but are insufficiently documented. Opie, an art historian (?<sup>22</sup>), sees no problem of talking about 'evidence of Indo-Europeans ... in the form of their actual bodies' and of 'these Indo-European "mummies"', (435), thus taking already for granted what is just being investigated, and again mixing linguistics with biology. He seems to follow Voltaire's whitticism literally when he states *Getai* to be 'quite similar to *Cuti*' (434), by which the Tocharians are traced to the Celts: an easy

<sup>21</sup>Given without reference to similar ideas of Charpentier or to any other literature than Haskin.

<sup>22</sup>Cf. 'my own field: tracing woven Iranian folk motifs to their earliest sources' (436).

'explanation' of the astonishing *centum* character of Tocharian! He even ventures to hold it 'quite possible' that 'the Getae peoples of Thrace were related to Goths' because animal-style decoration have also been found in Viking objects from Gotland (436)! Here, every serious scholar's patience has an end. The inclusion of such nonsense in a learned journal is indeed a heavy blow for Mair's reputation.

XII. Progress in complex questions like that of the Central Asian mummies can only be achieved by international interdisciplinary scholarly discussion. Every participant is responsible for giving access to reliable basic information about his or her own field, to explain the specific methods used, and to make sufficiently clear the grade of probability of any proposed hypothesis. Indo-European Cultural Studies have to be kept under strict methodological control (cf. Zimmer 1990) in order to avoid gross misunderstandings. Raising sensations can only have short term effects. Long term patience alone, when puzzling with proven results from all the scholarly fields involved, may perhaps, one day, solve the riddle of the mummies. Hasty assertions can only discredit serious scholarship, suffering enough from financial restraint in a frenzy world economy chasing for short term profits. Of course, we all wait eagerly for the results of the Philadelphia meeting (referred to in III. above)!

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*The Journal of Indo-European Studies*



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