WHOSE PAST IS IT?
LINGUISTIC PRE- AND EARLY HISTORY
AND SELF-IDENTIFICATION IN MODERN SOUTH ASIA*

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The well-known misuse of linguistic prehistory and early history in 19th and early 20th century Europe, especially in support of the racist ‘Aryan’ ideology of the Nazis, requires linguists to consider the potential impact of their claims and to counter misrepresentations based on linguistic and textual evidence and its relation to archaeological findings. The present paper addresses this issue in reference to modern South Asian identity movements, with special focus on indigenist claims that identify the civilization of the earliest, Vedic Sanskrit texts as identical to that of the Indus Civilization and thus as the original, ‘authentic’ source of all South Asian civilization. I conclude that much of the evidence cited for and against these claims is too weak to be cogent. However, a comparison of the textual evidence of the Rig-Veda, the earliest Vedic text, with the archaeological remains of the Indus Valley Civilization shows that the two civilizations cannot be identified with each other. Moreover, the social and religious significance of early Indo-European horse culture in the Rig-Veda and the total absence of evidence for this culture in the rich iconography and artefacts of the Indus Civilization (except in its final phases and on the western periphery) provides conclusive support for the view that an outside, non-indigenous origin of the speakers of Sanskrit/Indo-Aryan is still the best hypothesis.

1. Introduction
The misuse of linguistic prehistory and early history in the Europe of the 19th and early 20th centuries is well known. A racial interpretation of the earliest stages of Vedic Sanskrit, projected back to Proto-Indo-European, formed fertile ground for the racist ‘Aryan’ ideology whose most terrible consequence consisted in the genocide of Jews, Gypsies or Roma, and other so-called ‘inferior’ races committed by the Nazis, in the name of Germany.1

It is also true that a somewhat milder racism characterizes a large part of all of the Indology of the 19th and early 20th centuries. This racism has led Indians of the most varied backgrounds to reject as racist all of western Indology and the theory of an Indo-Aryan invasion or immigration to South Asia, proposed by most western Indologists. Significantly, this rejection is not limited to partisans of Hindu, the exclusionary Hindu nationalist movement (such as Sethna 1992, Ta-
lageri 1993ab, Frawley 1994, Feuerstein, Kak & Frawley 1995, Rajaram 1995, Rajaram & Frawley 1997), but is found also with other national-minded Indians (such as the national-communist Singh 1995, the politically rather moderate archaeologist Chakrabarti 1997, and the linguist Misra 1992; see also Sharma 1995 and most of the contributions in Deo & Kamath 1993).

Developments of this type raise doubts about the comfortable assumption that linguistics and philology are ‘harmless’, in contrast to other sciences, and do not have the same potential for horrible consequences as, e.g., nuclear physics. Our statements on prehistoric and early historic issues have their consequences. The only thing is that they do not show up so much in linguistic controversies, but rather in public debates of questions such as ‘Whose past is it?’ To make certain that our statements are not misused, we must understand the nature of these debates more clearly, including the non-linguistic criteria that are introduced.

In this paper I try to live up to this task in the area of early history and prehistory and their role in the self-identification of Hindu and Dravidian nationalists, with focus on the former.

2. Early Indologist perspectives and the ‘Aryan Invasion Theory’

As is known, William Jones 1786 assumed that Sanskrit, Greek, and Latin derive from a language ‘which, perhaps, no longer exists’. Schlegel’s 1808 opposing opinion that Sanskrit is older and all the other languages derived from it led to the early tendency to consider India as the original home and the European members of the language family as victorious conquerors. As is known, in the German-speaking area this assumption played a role in creating a certain counterweight to the French and English self-derivation from Rome and Greece.

Early on, however, doubts arose about the assumption that Sanskrit was identical with the proto-language or at least most closely related to it. For instance, Pott 1833 proposed that the contrast dental: retroflex of Sanskrit was in part due to the influence of the autochthonous languages, and in 1836 he identified these as Dravidian. Especially the discovery of the so-called Law of Palatals toward the end of the 19th century (discussion in Collinge 1985) led to the fact that Sanskrit was considered a sister language and not the mother of the Indo-European languages. In this context the assumption became increasingly popular that the original home of the Indo-Europeans had to be sought outside of India and that the Indo-Aryans had conquered India and subjugated the indigenous Dravidians (and other populations). (See for instance the discussion in Childe 1926.)

The ‘Aryan Invasion Theory’ (AIT) quickly found proponents even outside linguistic and philological circles, no doubt aided by the fact that it was considered a parallel and prelude for the western, especially English, conquest of India. (See the extensive discussion and references in Chakrabarti 1997.) Also the subjugation of the non-white population of India by the British seemed to find a parallel in certain Rig-Vedic passages in which the enemies of the Vedic Āryas, often
designated Dāsas or Dasyus, are characterized as black or even as black-skinned. See, e.g., example (1).

(1) áryaṁ právad ... svāṁīheṣv ... l
... tvácaṁ kṛṣṇāṁ arandhayat (1.130.8) Geldner: ‘Indra half dem ... Arier ... in den Kämpfen um das Sonnenlicht ... machte er ... die schwarze Haut untertan [Indra helped the ... Ārya ... in the battles for the sunlight. ... he made ... the black skin subject (to Aryan control)]’ (With the added remark that the black skin refers to the black aborigines)

Linguistic support for the AIT was found above all in theories that proposed a Dravidian substratum explanation for features such as the contrast dental : retroflex (see (2a), the use of gerunds (or absolutives) instead of dependent clauses (2b), and the use of iti as ‘quotative’, i.e., as marker of cited discourse (2c). The Dravidian substratum theory found further support in the fact that an apparent Dravidian relic language, Brahui, is to be found in the northwest, in present-day Baluchistan; and it also seemed to be supported by various Dravidian loan words believed to be found in the Rig-Veda and later. (See Hock 1975, 1984, and 1996a with further references.)

(2) a. -vīt ‘finding’ : vīt ‘community, clan’

b. ādāya Śyenó abharat sómam (RV 4.26.7a)
‘Having taken [it] (= After he had taken it), the eagle brought the soma’

c. nākir vaktá ná dād iti (RV 8.33.15 )
‘Nobody will say, “He shall not give”’

The discovery of the Indus Civilization and of its demise at about the time of the assumed Aryan invasion seemed to further support the AIT, especially when Wheeler 1947 believed to have found evidence of murder and mayhem in the last phases of the civilization. In this context it was assumed that the purs, which in the Rig-Veda are frequently destroyed by the Aryas or their God Indra, refer to the cities or forts of the Indus Civilization.

Finally, the AIT also seemed to be supported by the fact that the most serious attempts at deciphering the Indus script started from the assumption that the language was Dravidian; see above all Parpola 1994.

As it appeared, then, the AIT was linguistically and archaeologically completely justified. Since in this context Dravidian gives the impression of a pre-Indo-Aryan presence in India, it is easy to understand that Dravida-nationalists consider the AIT as proof that the Indian past belongs to the Dravidians, and not to the Aryan invaders. (See, e.g., Arooran 1980:33-4, Venu 1987:10-11, and Pillai 1981:190,6,7)
3. Indologist arguments against the ‘Aryan Invasion Theory’

But this appearance did not remain unchallenged for long. Since the last century, doubts have been raised about the linguistic foundations for the assumption of a Dravidian substratum in the Rig-Veda. I myself have contributed to these doubts on several occasions (see above all Hock 1975, 1984, 1996a with references) and limit myself here to a short summary.

The assumption that Brahui constitutes a relic of an originally much more widely distributed northwestern Dravidian presence is made dubious by the fact that according to their own traditions, the most closely related languages, Kurux and Malto, have moved to their present areas in northern India from Karnataka, in the south, and the Brahuis, too, believe in an immigration from outside (except that they rather fancifully locate their origin in Aleppo, Syria. See also Elfenbein 1998.)

The alleged Dravidian loans in early Vedic are similarly questionable, since in every case a different explanation is possible. Kuiper 1991, to be sure, attempts to plead for large-scale non-Indo-European elements in the Rig-Vedic lexicon; but Oberlies 1994 and Das 1995 have raised important questions about his findings and his methodology. And what remains of the non-Indo-European elements is not necessarily Dravidian; see also Witzel 1999 with references.8

The structural evidence for Dravidian influence on Vedic is likewise open to question. The contrast dental : retroflex can be explained language-externally, or perhaps as a convergent innovation in Dravidian and Indo-Aryan (and partly also in East Iranian); see above all Hock 1996b. And as I show in Hock [Forthcoming a], the syntactic parallels of Dravidian and Vedic can be considered innovations which can be explained in terms of a syntactic typology that was similar even before contact.

Finally, attempts at deciphering the Indus script based on the assumption that the language was Dravidian are just about as unproven as attempts to interpret it as Indo-Aryan; see above all Possehl 1996.9

If these arguments are accepted (and scholars such as Emenau are not prepared to do so10), does this prove that the Indo-Aryans had no early contact with Dravidians or with other non-Indo-Aryan languages? Of course not. It only means that the evidence for early Dravidian/Indo-Aryan contact is not cogent. As the American linguist Paul Postal is said to have expressed it, ‘you can’t prove that the platypus doesn’t lay eggs by showing a picture of a platypus not laying eggs.’

The textual testimony for a ‘racial’ difference between Indo-Aryan immigrants or invaders and the autochthonous people is likewise questionable.11 This can be illustrated by our example (1). Significantly, the krṣṇā tvāc ‘black skin’ of the second line corresponds to svāt ‘sun’ in the first line. A more detailed investigation of all passages that offer enough context for interpretation shows that the black or dark color of the Dāsas/Dasyus is contrasted not with a light or white skin-color of the Āryas, but with their light, sunny WORLD. (Similar conclusions
are already found in Schetelich 1991.\textsuperscript{12} Even the expression \textit{tvāc} ‘skin’ need not be understood literally, but can refer to the surface of the earth. In fact, the assumption of a racial self- and other-identification, as well its alleged parallel in the English conquest of India, is extremely questionable for the time of the putative Indo-Aryan immigration. We only need to consider the multicultural, multiethnic, multilingual armies of the Huns and of their Roman opponents (where, e.g., Germanic people fought on both sides) to understand how little such concepts as ‘race’ and ‘national identity’ are applicable for earlier times. In South Asia, by the way, this non- or pre-nationalist tradition extends into the early modern period, as can be seen in the well-known fact that the Hindu-controlled Vijayanagara empire had Islamic auxiliaries and population (Gollings, Fritz, & Michell 1991:43, 96) and its Islamic opponents, the Bahmani sultans. Hindu auxiliaries.\textsuperscript{13,14}

Finally, since Wheeler’s time, archaeology, too, has offered fundamentally different interpretations of the demise of the Indus Civilization; see, e.g., Shaffer 1982, 1984, Shaffer & Lichtenstein 1995, 1999, Kennedy 1995. Thus, Wheeler’s murder and mayhem scene is chronologically and areaally much too limited to suggest wide-spread hostile destruction. Rather, the civilization seems to have perished on account of internal or environmental developments. In addition, the Vedic \textit{purs} do not refer to the cities and forts of the Indus Civilization, but more likely to small strongholds surrounded by earthen embankments; see above all Rau 1957, 1976.

Even more significant is the archaeological realization that the skeletal evidence does not offer any indications of immigration of a new population; see, e.g., Shaffer 1982, 1984, Shaffer & Lichtenstein 1995, 1999, Kennedy 1982, 1995.

4. The indigenist responses and the ‘Aryan Emigration Theory’

Under these circumstances it is understandable that nationally-minded Indian scholars and interested laypeople, especially partisans of Hindutva, consider the entire AIT as questionable and racist or hegemonic and, in order to explain the linguistic relationship of Sanskrit with the other Indo-European languages, propose a theory which can be called the Aryan Emigration Theory (AET). For adherents of this movement, therefore, there is no doubt that the Indian past belongs to the Āryas. (True, the Hindutva movement often assumes an inclusive position, according to which distinctions such as ‘Aryan’ : ‘Dravidian’ have been introduced by the British in the name of ‘divide and conquer’;\textsuperscript{15} but when the issue of the decipherment of the Indus script comes up, an Indo-Aryan interpretation is always preferred, and Dravidian interpretations are either rejected\textsuperscript{16} or passed over in silence.)

On the other hand, since as already indicated, many linguists still consider the linguistic evidence for early Dravidian substratum influence on Sanskrit to be valid, and since scholars such as Parpola 1994 advocate a Dravidian interpretation of the Indus script, partisans of the Dravida movement are able to hold on to the AIT and hence can consider themselves the original Indians.
5. A critique of the ‘Aryan Emigration Theory’

For reasons of space I will limit myself in the remainder of my presentation to a discussion of the AET, without detailed consideration of the possible consequences for the Dravida movement. (As already mentioned, the rejection of the linguistic arguments for Dravidian influence on Vedic does not exclude the assumption of an Aryan immigration and of early contact between Dravidian and Indo-Aryan.)

5.1 The skeletal archaeological evidence

First, it is important to note that archaeology not only does not offer skeletal evidence for the AIT; it also offers no evidence for the assumed AET. The latter fact is significant, since the AET would have to postulate repeated emigrations which could be expected to have left much more robust traces than an Aryan immigration.\(^{17}\)

To this must be added the fact that the historically attested incursions of Greeks, Sakas, Hunas, and various Islamic invaders likewise do not change the northwestern skeletal profile or even the cultural profile; see, e.g., Shaffer \& Lichtenstein 1999:256\(^{18}\) and Chakrabarti 1997:225; similarly also Dhavalikar 1997.

Chakrabarti (1997:225) tries to explain this situation as follows:

> Looked at from this point of view, the invasions, which are considered foreign invasions in the study of Indian history, all originated precisely in this interaction area [between the Oxus and the Indus]. Geopolitically, these invasions, inclusive of the Muslim invasions right up to the invasion of Nadir Shah ..., can hardly be called entirely alien in the subcontinental context.

Interestingly, however, Chakrabarti fails to draw the logical conclusion that an Aryan invasion would have had to be of the same nature.

The evidence of the archaeology of the subcontinent thus does not contribute anything to the debate about AIT and AET. Apparently, incursions of this type do not leave the kind of traces that traditional archaeology would expect — and this expectation is perhaps again a questionable inheritance from the 19th century. In this context I find Ratnagar’s discussion (1995:222), with reference to similar difficulties outside South Asia, to be especially à propos.

5.2 Textual evidence and the AIT vs. AET

Proponents of the AET, however, attempt to support their theory by means of a number of further arguments.

A common argument is that there is no textual evidence in the Sanskrit tradition for an in-migration to India, but that the Purāṇas offer support for an out-migration; and although the extant Purāṇas are quite late, it is suggested that they preserve Vedic traditions; see, e.g., Rajaram & Frawley 1997:233. Rajaram and Frawley further claim that the Zoroastrian tradition, in contrast, does recognize an
external origin, the *airiianām vaējah*. The latter suggestion goes back to Bhar- 
gava 1956 via Talageri (1993a:180-1, 1993b:140-1), and is also advocated by Elst 
(1999a:197-8).\(^19\) In Talageri’s interpretation of Bhargava, the *airiianām vaējah* ‘is 
obviously Kashmir’, and the next region inhabited by the Iranians is ‘The Hapta 
Hindu … obviously the Saptasindhu (the Punjab region).’ Put differently, under 
this interpretation the Iranian tradition offers clear evidence, not only for an out-
side origin, but for one inside India.

A closer examination of the Iranian tradition (the late Avestan Vidēvdād 
\(^{120}\) shows a rather different picture (for further details see Hock 2000). The 
sequential list of areas is as follows (with indications in the right column of the 
geographical identification where such an identification is possible):

(3) First, there is the *Airiianām Vaējah* of the Good Dāitiīā river

Second, there is the progression:

1. *Airiianām Vaējah*  
2. Gāva inhabited by the Sogdians  
3. Margiana  
4. Bakhtria/Balkh  
5. Nisāy between Margiana and Bakhtria  
6. Harōiūm  
7. Vaēkārāta  
8. Urva  
9. Xnanta, inhabited by Hyrkanians  
10. Haraxvātti = Arachosia  
11. Haētumānt  
12. Raga  
13. Caxra  
14. Varāna  
15. Hapta Hāndu  
16. Raḷhā  

(NE, north of the Oxus)  
S. of the Oxus  
(Modern Herat)  
The area around the modern Helmand  
Ved. Sapta Sindhavaḥ  
Ved. Rasā (uncertain location)

As can be readily seen, the first identifiable area after the *airiianām vaējah* is 
‘Gāva inhabited by the Sogdians’, which is located north of the Oxus and clearly 
outside India. The areas 3 - 11 are all located beyond the mountain ranges that 
separate the Indian subcontinent from Iranian territory. It is only toward the very 
end of the enumeration, when we get to Hapta Hāndu and the Raḷhā, that we 
arrive at the Vedic horizon and, in the case of Hapta Hāndu, in a clearly identifi-
able part of India.

Whatever the intended meaning of this list of areas may have been, then, the 
one least likely to be correct is the one advocated by Talageri, Rajaram & 
Frawley, and Elst. If anything, it might suggest an origin yet farther north of the 
Oxus than ‘Gāva inhabited by the Sogdians’ — unless Gnoi (1989:38-51) is 
right in assuming that the *airiianām vaējah* was a priestly invention ‘to place 
their Prophet at the centre of the world’.

In fact, no early Indo-European tradition offers references to an external 
origin, except for the Roman attempt at a dynastic self-derivation from Troy. The
assumption of immigration of, say, the Greeks, or of the Anatolians, rather goes back to the same linguists who have produced the AIT. Why then should we give credence to the ‘Greek or Anatolian Invasion Theory’, but not the AIT?

Just like the evidence of archaeology, the absence of credible references to outside origin in the various early Indo-European textual traditions contributes nothing that would help resolve the AIT vs. AET debate.

5.3 The river Sarasvati

A second argument for the AET is connected with the river Sarasvati. Rajaram (1995) claims that a catastrophic desiccation of the Sarasvati around 1900 BC caused the demise of the ‘Indus-Sarasvati’ Civilization. Since the Sarasvati is one of the most important Vedic rivers, and since one Rig-Vedic hymn (see 4) mentions its flowing to the sea, it is assumed that the Vedic tradition in South Asia goes back to a time before 1900 BC, that it thus is contemporary and identical with the Indus Civilization, and that therefore there can be no truth to an AIT after the collapse of this civilization.

(4) ēkācetat sārāsvati nādīnām śucir yāti giribhya ā samudrāt I rāyās cēntanti bhūvanasya bhūrer ghṛtāṁ pāyo duduhe nāhuṣāya II (RV 7:95:2)

‘Pure in her stream, from the mountain to the sea, filled with bounteous abundance for the worlds, nourishing with her flow the children of Nahuṣa.’ (Rajaram’s translation; emphasis supplied)

Now, it is indeed true that the Sarasvati is one of the most important Vedic rivers. It is also true that one hymn (see 4) mentions its flowing to a samudra. And it is further true that samudra in later Sanskrit means ‘sea, ocean’. Beyond these facts, however, there are numerous problems with Rajaram’s hypothesis; and some of these are fatal.

It is, first of all, doubtful whether the date of 1900 B.C. for the Sarasvati desiccation is correct. Rajaram (1995:xvi) simply claims that the ‘dates found in Indian publications’ are underestimates — without furnishing any evidence for this claim, except for the additional claim that astronomical evidence argues for much earlier dates. Mughal, whose excavations laid the foundation for the insight that a large number of Indus-Civilization settlements are situated in the present-day Hakra, a remnant of the former Sarasvati, finds evidence for a slow drying-up process which was completed only toward the end of the second or the beginning of the first millennium B.C., and believes that it is this final desiccation which ‘forced the people to abandon most of the Hakra flood plain’ (1993:94). Possehl’s recent extensive discussion (2000:462-84, with references to earlier and divergent views) recognizes multiple alignments and realignments of rivers in the area, which I believe makes it difficult to be certain as to what is the Sarasvati, at what time, and where. The area of heaviest settlement along the putative Sarasvati continues to be settled into the second millennium, or even to its end. In both Mughal’s and Possehl’s account, there is no evidence for a ‘catastrophic’
desiccation. To support his proposed date of 1900 B.C., Rajaram would have to provide explicit and convincing evidence that falsifies these observations.

Possehl (2000:372) further notes that ‘There is no direct, physical evidence to suggest that the Sarasvati ever flowed uninterruptedly to the Arabian Sea’. Rather, there is evidence for ‘an inland delta in the vicinity of Fort Derawar … that can be sustained on the scene by anyone with a knowledgeable eye. Another delta feature can be seen on landsat imagery.’ The area was ‘densely settled during Hakra Wares times as well as during the Mature Harappan’ (373). Possehl concludes that this inland delta ‘suggests that all, or most, of the Sarasvati’s water was “sopped up” in this area where it would have been used for intensive agriculture and pastoralism’, but that not enough water would have remained for a flow beyond the delta area and ‘through to the Eastern Nara’ (373). Under the circumstances, we must consider that the meaning ‘ocean, sea’ (in the modern sense) may be inappropriate for the samudra of example (4) above. Support for this view comes from the fact that the Pali outcome of the word, samudda, means not only ‘sea’ but also ‘large river’ (Hock 1999b). Similarly, there is Senanayake Samudra, an inland LAKE in Sri Lanka (Gal Oya National Park) and at least one other Sri Lankan Samudra lake (Pariskrama (?) Samudra near Polonnaruwa). Further, there are Jai Samand and Raj Samand, artificial lakes in Rajasthan, where Samand looks like a regional, simplified variant of samundr, samandr, the Hindi/Panjabi reflexes of Skt. samudra. These facts suggest that the samudra of (4) may have referred to an inland lake at the end of the Derawar delta. The passage in (4) thus cannot be considered to provide incontrovertible evidence that the Sarasvati flowed to the sea in Vedic times.

Further problems for Rajaram’s hypothesis arise from the availability of an alternative hypothesis according to which some of the Vedic references to the Sarasvati — including possibly the present one — may refer to East Iranian territory (Witzel 1995b:343). In that area,23 we find a river Haravaiti (Avestan) or Harauvati (Old Persian) — a perfect cognate of Skt. Sarasvati — now called Helmand, which flows into a large inland salt lake (Hāmūn-i Helmand or Daryāche-ye Sistān). If the ‘East Iranian hypothesis’ is correct,24 it may be this lake that corresponds to the samudra of our text (Hock 1999b). We thus cannot even be certain that the Sarasvati or the samudra of the passage in (4) are to be located in the Indian subcontinent.

The ‘East Iranian hypothesis’ of course implies a transfer of the river name Sarasvati from Eastern Iran to South Asia, i.e., from west to east. The transfer could have been motivated by the fact that, like the Indian Sarasvati, the Avestan Haravaiti is a holy river that flows into an inland lake. Moreover, the spread would be paralleled by that of several other river names with cognates both in Eastern Iran and South Asia. Consider the Gumal in Afghanistan (*gaumati), a western tributary of the Sindhu, and the Gom(a)ti of Uttar Pradesh, a tributary of the Ganga (beside a Gomti Creek near Dwarka and a Gomti in Bangladesh). Or consider the *Harayu contained in the Harōiium ‘Herat’ of example (3) and the modern river name Hari Rūd (< *Sarayu) and the Rig-Vedic Sarayu, or its modern counterpart Sarju, the name of rivers in Nepal/Uttar Pradesh (tributary of the
Ghaghra and in Pithoragarh district (tributary of the Ramganga, which in turn is part of the Ghaghara system, and also contained in Chhoti Sarju (Uttar Pradesh between Ghaghara and Gomti). (See also Witzel 1995a:105.)

There are many other examples within South Asia of the transfer of river names from west to east, such as the Ghaggar/Ghaghara of the Panjab, a relic of the old Indian Sarasvati, beside the Ghaghara/Ghâghra in Uttar Pradesh,25 or the Sindh(u), of the Panjab beside a Sind(h) which runs from the Vindhyas to the Yamuna,26 and the Yamunâ/Jamnâ of the Doab, which finds an eastern counterpart in the Jamunâ of modern Bangladesh.27

The eastern extension of river names also agrees with the eastern expansion of Indo-Aryan civilization, as it is attested by the Vedic texts. Even the late Śatapatha-Brâhmana (1:4:1:14-16) still mentions an eastern extension of the brahmins, beyond the Sadānirâ, which they did not use to cross before.28

Even more interesting in the present context, the name Sarasvati, too, has spread farther east, to West Bengal. But in addition, it is also found farther south and southeast than the Vedic Sarasvati: A Saurashtrian Saraswati flows into the Arabian Sea (after forming a triveni tirtha with the Hiran and Kapil); another Saraswati is found in Madhya Pradesh, near Mandla (forming a triveni tirtha with the Banjar and Narmada); a Saraswati Nadi, tributary of the Luni, flows near Pushkar (another tirtha); and there is also a Saraswati near Palampur, west of Sabarmati, which flows into the Little Rann. Perhaps significantly, most of these Sarasvatis are associated with tirthas and thus share the element of holiness with the Vedic Sarasvatî.

The existence of Sarasvatis not only to the east but also to the south and southeast of the Vedic Sarasvatî further provides support for the view that in South Asia the name did indeed fan out from the relatively northwestern Rig-Vedic area, in so far as the Vedic Sarasvati is the earliest and holiest of the rivers and thus is more likely to have been the source for naming the other rivers than the other way around.

Basically, then, the evidence of river names and their spread argues in favor of an expansion to the east and against the western expansion postulated by the AET. This is certainly true for within South Asia; but given the general west-to-east direction of the spread, it also is eminently compatible with the hypothesis that the Iranian-Indian river-name cognates result from a similar west-to-east spread.

5.4 The ‘horse culture complex’

AET proponents find support for their theory in the expansion of horse domestication, of the horse-drawn two-wheeled battle chariot, and of the religious significance of horse and battle chariot (see, above all, Anthony & Brown 1991),29 and above all in the fact that all three features play a prominent role in early Vedic, as well as in Avestan.30 Clear traces of horses, by contrast, are at best limited to the latest stages31 of the Indus civilization (Dhavalikar 1997, Chengappa 1998, as well as Ratnagar 1999) and on the periphery of the Indus Civilization, in Pirak,
near the Bolan Pass, and in Swat, near the Khyber Pass (Kenoyer 1995:226-7, Kennedy 1995:46) — i.e., in areas that would have been first affected by an immigration. And up to now, no certain indications of two-wheeled battle chariots and the religious significance of horses and battle chariots have been discovered in the Indus Civilization.

Adherents of the AET attempt to counter this argument with the claim that traces of horses can be found already during the Indus Civilization period and even earlier. Thus Misra (1992:58) states, ‘... the evidence of horse in the form of terracotta figures or equine bones have [sic] proved that there is a greater possibility for considering the Indus Valley civilization to be Aryan’.

Sethna 1992 and Singh 1995 make even stronger claims. Sethna points to signs of the Indus script, which in his view represent spoked wheels, and to an Indus symbol that he considers proof of the use of such wheels (1992:51, 173). and he gives a depiction of an alleged horse figure (419-20). Singh (1995:169) further adds ‘wheeled objects including toys’ as proof for the use of ‘carts and chariots with spoked wheels’.

But there are numerous problems with these claims. First, the only uncontroversial horse figures (Misra) that have been unearthed are the ones that come from the periphery of the latest Indus stages in Pirak. Sethna offers an apparently earlier image of what he claims is a horse figure, and Possehl (2000:189) reproduces several other examples of such claimed horse figures. Possehl is no doubt correct in admitting that they ‘seem to represent some form of equid’, but adding that ‘they are not sufficiently realistic in their rendering of an animal that one could distinguish Equus hemionus from E. caballus. Jha & Rajaram, (2000:177) to be sure, offer an ‘artist’s reproduction’ of a reconstituted ‘Horse Seal’; but comparison of the original seal with their reproduction/reconstitution has been met with extensive, and as far as I can see, justified criticism on the Indology List and elsewhere; see for instance Steve Farmer (7/24/00, 9:31, 9:37, 9:41 p.m.), Witzel (7/25/00, 1:40 a.m.), both on the Indology List.

As for the ‘equine bones’, Chengappa 1998 points out that it is impossible to be certain whether they come from hemiones (i.e., onagers) or from true horses. Possehl (2000:185-9) similarly argues that, given the fragmented nature of bone remains, it is difficult to determine whether fragments belong to the hemione/onager — which is indigenous to India — or to the true horse — which is not.

Sethna admits that the ‘spoked wheel’ symbol of the Indus script can be interpreted in a different way — one might for instance consider the ‘spoked wheel’ to be a symbol for the year and its six seasons. Moreover, the so-called spoked wheels are not always round but may be oval, a fact that diminishes their interpretability.

Sethna further admits (173) that the correctness of his rendition of the second symbol has been doubted (apparently one of the two ‘wheels’ in the drawing, which he has taken over from somebody else. is missing in the original). Much more important is the fact that where there is enough evidence for judg-
ment, the so-called ‘chariots’ of the Indus Civilization turn out to be ox-drawn carts and not battle chariots drawn by horses.

In fact, it is remarkable that the Indus Civilization does not offer clear traces of horses (except for skeletal evidence in the latest stages). For many other animals, including various kinds of bovine animals, buffaloes, lions, tigers, and even elephants, the Indus Civilization offers not only skeletal evidence but also many figurines and graphic depictions (see the survey in Possehl 2000:173-230). Moreover, even if incontrovertible evidence for horses should be found, what is conspicuously absent in the artifacts and iconography of the Indus Civilization is the cultural and religious ‘horse culture complex’ of the Vedas and of early Indo-European. Given the rich attestation for other animals and other cultural artifacts and iconography, this cannot simply be due to accident. And as long as these facts do not change, we must continue with the assumption that in this respect the Vedic and Indus civilizations differed so much that there can be no question of an identity between the two.

5.5 More differences between the Vedic and Indus civilizations

This assumption is supported by further facts. As is well known, Vedic culture also differed from the Indus Civilization by being oral, not written (Falk 1993). The widespread Hindutva or Hindu-nationalist view that the Indus script is to be interpreted as Indo-Aryan and that Vedic culture therefore was a written one is, at best, moderno-centric, in that it is considered inconceivable that such a developed linguistic culture as the Vedic one could have been able to function without writing. Moreover, under this view, the oral features of Vedic tradition, such as the multiple ‘backup’ versions of the texts (Sāṁhitā, Padapātha, and various krama-versions) and the complete lack of allusions to writing in the entire Vedic tradition must be implicitly considered a colossal scam by the brahmins who maintained the tradition. I would not be prepared to make such a judgment. (See also Hock 1999b.)

Further, Vedic culture, in contrast to the Indus Civilization, was not urbanized and perhaps not even completely sedentary. As Rau (1957, 1976) shows, based on Vedic testimony, even the word grāma, which later means ‘village’, seems to have meant only something like ‘clan, tribe’ in the Vedic tradition. Consider also the expression saṁgrāma ‘conflict, battle’, whose meaning is best derived from saṁ- ‘together, coming together’ and grāma ‘clan, tribe’, as the ‘coming together or clash of clans/tribes’.

Finally, the Vedas offer testimony for extensive hostile interaction of the Āryas with the Dāsas/Dasyus as well as amongst each other, and also for the frequent destruction of hostile pārśus. As mentioned earlier, evidence for murder and mayhem in the Indus Civilization is chronologically and areally extremely limited — which is why the earlier assumption of a destruction of the civilization by the Āryas is generally rejected by the scholarly community. The AET adherents have appropriated this finding and are trying to refute the AIT by means of it — without realizing that exactly the dearth of evidence for hostile destruction in the In-
Indus Civilization argues against identification of this civilization with the Vedic one.

Altogether, the differences between Vedic and Indus civilization favor the traditional AIT and not the nationalistically motivated counter-hypothesis of an AET.

In a recent electronic review, Koenraad Elst 1999b tries to avoid this difficulty with the assumption that the Vedic tradition was not contemporary with the Indus Civilization, but must be posited PRIOR to that civilization. This hypothesis would be able to explain the oral and non-urbanized nature of the Vedic tradition, but it would certainly fail because of the 'horse argument'. If Elst were correct, we would have to make the counterintuitive assumption (a petitio principii) that the pre-Indus Civilization knew horses, horse-drawn two-wheeled battle chariots, and the religious significance of horses, that for unknown reasons the entire horse culture complex later was lost in the Indus Civilization, and that only at a later stage (in the final stages of the Indus Civilization) were horses reintroduced.

In fact, the evidence of the 'horse culture complex' strongly favors the AIT. As Anthony & Brown 1991 show, the domestication of horses took place only at about the beginning of the 4th millennium BC, in the present-day Ukraine; and Anthony 1990 convincingly demonstrates that the words for the horse culture complex are semantically deeply moored in Indo-European. To this must be added that the first signs for horse culture in Mesopotamia apparently coincide with the first signs of Indo-European linguistic groups — about the beginning of the 2nd century BC. These facts suggest that horse culture in general was spread by speakers of Indo-European languages from the Ukrainian area of origination, and these languages also included Indo-Aryan or Indo-Iranian.

Such an expansion from the Ukraine would be easily compatible with the arguments in Hock 1999a, that the dialectal relationships of the Indo-European languages can be best explained in terms of origination somewhere in the large area between eastern Central Europe and the Urals. In a recent argument, which is not yet accessible to me, Koenraad Elst tries to pinpoint the Indo-European original homeland South Asia, by adapting a theory proposed by Gamkrelidze in Ivanov 1994 in the interest of their Caucasus homeland hypothesis, according to which the dialectal relations of the Indo-European languages are to be explained by means of migrations. At best, Elst in this way succeeds in proposing an alternative solution; but the question arises not only why this one should be preferred to that of Gamkrelidze and Ivanov, but also whether other, non-Caucasus, non-South Asia original homes could be proposed along the same lines of reasoning, and based on which criteria we should prefer one of these theories to the others. Moreover it seems to me that historical cases of similar migrations, such as that of the West Greeks in ancient Greece, did not produce the kind of dialectological layering that we find in early Indo-European. Finally, as far as I can see, Elst's counter-hypothesis cannot be reconciled with the evidence for an expansion of Indo-European horse culture from the Ukraine.
6. Conclusion

After all this, what remains in order to answer the question whether the Indian past belongs to the Hindu or Dravidian nationalists?

One great difficulty is presented by the fact that archaeology does not offer any clear evidence, either for the AIT or for the AET, and not even for the historically attested, multiple later immigrations or invasions into South Asia — at least when we limit ourselves to the evidence of skeletal types and general cultural tradition. As indicated earlier, incursions of this sort apparently do not leave the kind of traces that traditional archeology would expect.

The evidence so far advanced for Dravidian substratum influence on Vedic and for an assumption that the Dravidians were the original inhabitants is not probative. This does of not mean of course that there has been no early or prehistoric contact between the two linguistic groups. As in a court case based on circumstantial evidence, the non-existence of probative arguments means no more than an absence of probative arguments. New facts or theories could easily change the picture. For instance, I have proposed (Hock 1996b) that the development of the contrast dental: retroflex (± alveolar) represents a convergent innovation in Dravidian, Indo-Aryan, and partly also in East Iranian. If correct, this theory would suggest at least indirect contact between Dravidian and Indo-Aryan.

Even if this theory should not be accepted, this does not in any way affect the question whether the Dravidians represent the original inhabitants of South Asia, for even a priori it would be possible that the Dravidians immigrated from the outside. In this respect the various attempts to connect Dravidian with Elamite or Uralic are of special interest; see, e.g., MacAlpin 1974, 1981, and Tyler 1968.

The cultural differences between the Vedic and Indus civilizations and the evidence for expansion of the Indo-European and also Vedic horse culture complex from the Ukraine raise questions about the Hindutva thesis that all Hindus, whether Aryan or Dravidian, are original in India; and they also make dubious the thesis, maintained not only by Hindutva partisans, that the AIT is untenable and that Vedic culture is identical with that of the Indus Civilization. (The skepticism, however, of Hindu nationalists regarding the racial and racist interpretations of Vedic texts in the western Indology of the 19th and early 20th centuries is fully justified.)

We can thus conclude that the AIT is preferable to the AET. But this conclusion is justified only as long as there is no change in our knowledge of Indo-European civilization and expansion, or of the Indus Civilization. If, e.g., a decipherment of the Indus script should receive the same general acceptance as that of Linear B or Mayan writing, and if based on this decipherment the language of the Indus Civilization should turn out to be unambiguously Indo-Aryan, then our conclusions would of course have to be completely revised.
All serious interpretations of the early and prehistory of South Asia that have been proposed so far are, after all, at best scientific hypotheses that differ only in the degree of their probability. Faced with the often tense political situation in India as regards Hindutva and Dravida self-identification, it is, I believe, proper to remember the hypothetical nature of these hypotheses. It would be improper to impute to these hypotheses an unwarranted reality that justifies one or another group’s claim to being autochthonous. And it would be even less proper to derive from this claim an entitlement for one group to oppose or exclude other groups. If the now current hypothesis remains viable that all of humanity has its origin in Africa, then all Eurasians (and Americans) in the last analysis are immigrants and not autochthonous.

NOTES

* This is an expanded English version of Hock [Forthcoming b].
1 The blame for laying the foundation for Nazism or worse, giving direct support to it, does not rest solely with philologists, linguists, and anthropologists (as claimed by authors such as Riencourt 1986 or Poliakov 1971). Goodrick-Clarke 1985 and Lütt 1987 show that another important factor was a break-away, ‘Ariosophist’ branch of Theosophy which took as a starting point for its racist ideology the Theosophist theory of an Out-of-India migration of various ‘Arians’, among whom the Semites, especially the Jews, were considered the despised caste of Cauḍālas. (Interestingly, Thapar 1999a:20, with ref., speculates that the Theosophists’ views on Aryans as the indigenous race of northern India and their identification of ‘Aryan’ and ‘Hindu’ ‘could well have influenced Hindutva thinking.’)
2 Exceptions are the contributions by Mehendale and Mukherjee.
3 Interestingly, there are attempts in Pakistan to align the languages of the country with Dravidian; see above all Faridkoti 1992, and the less extreme view in Rahman n.d.
4 A further development is that of the Dalits, which will be briefly discussed further below.
5 As far as I know, this interpretation goes back to Zimmer 1879. While the view still lurks around in many linguistic and historical publications (see the references in Hock 1999b), it is necessary to note that Murray B. Emeneau, one of the main proponents of the thesis of a Dravidian substratum in Vedic, does not accept the racial interpretation (personal communication 1995).
6 Arooran and Pillay provide only vague references to scholars who advocate such an opinion. In general, it is remarkable that reports which I have seen about Dravida-nationalists such as Annadurai and Periyar have nothing to say about the early history in the north, but give the appearance that the contrast Aryan : Dravidian is limited to more recent history, and above all to South India. On the other hand I have found that the AIT and a belief in the Dravidians as the
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autochthonous people of India have adherents among Dravidian scholars. When in a talk at Pondicherry in 1987 I stated the opinion that Dravidian influence on Vedic cannot be considered established, a professor at the newly founded local Tamil university stormed out of the hall, shouting that Professor Emeneau has proved Dravidian influence and that to doubt this theory constitutes an insult to Dravidian identity.

7 Adherents of the Dalit movement also tend to accept the AIT and a racial difference between Aryans and autochthonous peoples, but often do not deal with the question of the indigenous language(s); see already Phule 1873:xxix-xxx (Preface) and see also, e.g., Carvalho 1975. Biswas 1995 likewise starts out from the AIT, but assumes that the Aryan invaders communicated only with ‘voice of sound-value’ (2-3), that their Sanskrit language was artificially developed from indigenous Prakrits (35), that indigenous Prakrit speakers in reaction to the Aryan invasion spread to Iran and there founded the Zoroastrian tradition [!] (135), and that the Dravidian languages rather belong to the south (202). Biswas’s assumption that the Indus script was retained among the [Munda] Santals at the Bihar-Bengal border (13-34), might indicate that for him the Indus Civilization also included Munda speakers; but he does not say anything further about this matter.

8 RV lángala, for instance, is more likely a Munda/Austric word. But note that its presence in the Rig-Veda does not, strictly speaking, constitute compelling evidence for direct contact with speakers of Munda languages, since words can be borrowed through intermediaries, as in the case of Engl. sugar and candy which, though ultimately derived from Skt. śarkara and khaṇḍa, came into English via Persian, Arabic, and Mediterranean mediation.

9 This holds true too for the recent claim by Jha & Rajaram 2000 to have successfully deciphered the script and to have identified the language as sutra-period Sanskrit. One of the fundamental problems of this proposal has been noted in a number of recent contributions to the Indology List, especially by Michael Witzel and Steve Farmer (see the Indology Discussion Archives at http://www.ucl.ac.uk/~ucgadkw/indology.html):

Even if Jha & Rajaram should be correct in their phonetic interpretation of the Indus signs, the decipherment leaves far too much latitude for interpretation in that it postulates a single sign for all vowel-initial akṣaras and assumes that vowels elsewhere are left unindicated. A sequence of V (= any vowel) + p (= p followed by any vowel) therefore could designate Skt. upa, apa, api, āpo, apo. Moreover, since Jha & Rajaram assume that inflectional endings often are not written out, the same sequence could additionally be read as āpiḥ and all other inflected forms of the word, all inflected forms of āp- ‘water’ — or even as Engl. up or ape. Especially disconcerting is the fact that many of the proposed Sanskrit interpretations are not well-formed Sanskrit. For instance, Jha & Rajaram offer a reading isādyatāḥ marah which they interpret to mean ‘Mara (forces of destruction) controlled by Ishvara’, referring ‘to the cosmic cycle of creation and destruction.’ By way of grammatical explanation they add that ‘Yattah is derived from the root ‘yam’, meaning to control …’ (2000:167-8). Setting aside faulty
transcriptions such as isāḥd for intended isāḥd, we can observe at least two violations of rather elementary facts about Sanskrit structure. One of these is the implicit assumption that the ablative case (isāḥd) can be used to designate the agent of a passive construction with the participle yata ‘controlled’ (the correct case is the instrumental). The other is the belief that consonant doubling applied freely in Vedic and thus could also apply to single consonants between vowels (hence yatta can be read as a variant of yata- ‘controlled’), whereas the Vedic prātisākhyas agree on permitting such doubling only in consonant groups (as in attra for atra ‘here’).

10 See also Southworth 1995.

11 The following discussion draws on Hock 1999b, which should be consulted for further details.

12 Hindutva adherents likewise tend toward this interpretation (see above all Rajaram & Frawley 1997:63-7). However, these authors do not support their view through careful philological (re-)examination of the relevant Rig-Vedic passages. Recently, Thapar (1999a:34-5) has come to a similar conclusion, based on the fact that Sāyāṇa’s commentary ‘explains the term ivacan kṛṣṇāṁ ... as a reference to the name of an asura and there is no mention of skin pigmentation’.

13 Similarly, Thapar 1999b presents detailed documentation that even after Mahmud of Ghazni’s destruction of the Somanatha temple in 1026, local relations between Hindus, Jains, and Muslims remained quite amicable, and a Muslim, Vo- 
hara Farid, on behalf of the local ruler, Brahmadeva, helped defend the town of Somanatha against an attack by the Turks.

14 This fact probably will run into opposition among the more radical elements of the Hindutva movement, since they assume an irreconcilable difference between Islam and Hinduism. Most of the Hindutva publications discussed here, to be sure, do not openly express this attitude. (Golwalkar at one point even acknowledges Shivaji’s Muslim army officer Ranadulla Khan (1996:131). But elsewhere (1996:125-6) he asks whether Muslims and Christians ‘feel that they are the children of this land and its tradition, and that to serve it is their great good fortune? Do they feel it a duty to serve her?’, and his answer is an emphatic ‘No! Together with the change in their faith, gone is the spirit of love and devotion to the nation.’)

A major exception to the general reticence on the issue of Hinduism and Islam (or Christianity) are the three chapters of Section I in Talageri 1993a, which contain an extensive diatribe against the foreign, colonialist invader religions of Islam and Christianity, which moreover are characterized as ‘Semitic’ (e.g., p. 42) — an unfortunate choice of terminology at best (which is incidentally not limited to Talageri). The possible objection that the Syrian church of Southern India is pre-colonial and was not introduced by invaders, is rejected as ‘the Christian carnival that it was not European invaders, but an ‘apostle’ of Jesus Christ, who first introduced Christianity into India.’ (p. 17) (While the claim that the disciple Thomas brought Christianity to Southern India is of course apocryphal, this does not
invalidated the fact that Syrian Christianity was introduced in the first millennium A.D., before Islam and long before western colonialist rule.)

In one of his early publications, Elst goes even as far as comparing Islam to Nazism (1991:224-6).

See also the indirect reference to the Hindu-Muslim divide in the citation from Jarrige 1994 by the national-communist Singh 1995:

Before the Muslim invasions and to be more precise the Mughal conquest of India, depictions of Indian kings or rulers trampling enemies, hunting wild enemies or diffusing their own glorified image all over their kingdom or any other symbol of their authority, are almost unknown.

It is of course true that the Islamic invasions and conquest, especially in the early stages, were accompanied by destruction, rape, and other horrors; but such behavior was not at all limited to Muslims. The complete destruction of Kalinga by Emperor Asoka is well known from his 13th Rock Edict: 150,000 were deported, 100,000 were slain, and even greater was the number of those who died; among the latter were also brahmans and monks. In similar manner Indra III destroyed the yard of the Kalapriya temple in the early 10th century and completely devastated the city of Mahodaya (see the quote in Willis 1993:59).

15 See, e.g., Golwalkar 1996:114-15; Jha & Rajaram 2000:9-10 w. note 5. But as pointed out by Trautmann 1999 with ref., the British orientalists were themselves originally divided on whether the Dravidian languages can be derived from Sanskrit (the Calcutta position) or whether they are a separate group, not related (or relatable) to Sanskrit (the Madras position). The linguistic evidence soon settled the issue in favor of the Madras position — pace (Jha and) Rajaram.


17 Further problems are pointed out in Hock 1999a.

18 'an identifiable cultural tradition has continued, an Indo-Gangetic Tradition ... linking diverse social entities which span a time period from the beginning of food production in the seventh millennium BC to the present.'

19 Without reference to either Talageri or Bhargava.

20 A shorter version is found in Yaṣṭ 10.

21 On the questionable nature of Rajaram's astronomical claim see Hock 1999b and MS (which adds to and supersedes Hock 1999b). Kak's proposed Rig-Vedic numerical code (1994) which supposedly provides further evidence for the astronomical dating of the Vedic texts must be met with great caution and skepticism in light of the recently published scientific critique of a similar numerical code proposed for the Jewish Bible (McKay et al. 1999).


23 See the areas 10 and 11 in (3) above.
24 The discussion in Macdonnell & Keith (1912, ssv. Sarasvati and Divodāsa) shows that the hypothesis has not been accepted by most early Indologists; but Witzel 1995b resurrects it with partly new arguments.

25 There is also a Ghaghar Nadi in Eastern UP (tributary of the Son) and a Ghagra in UP, tributary of the Sarda, which in turn is a tributary of the Ghaghara.

26 Note further the Kali Sindh of Madhya Pradesh/Rajasthan (tributary of the Chambal), the Sindhu Khola (Nepal; a tributary of the Indrawati which in turn flows to the Sun Kosi; Khola = River), and the Landay Sind (Afghanistan, tributary of the Darya-ye Konar, which flows into the Kabul).

27 Other Yamunas/Jamnas, etc., are found in Arunachal Pradesh, Bihar (near Bodh-gaya and near Dhanbad), Uttar Pradesh (near Jhansi), Nepal (near Bihar). Gangas are found throughout South Asia, including the Buri Ganga of Nepal (tributary of the Seti), Bangangas in Nepal and Rajasthan, the Wain-Gaṅga and Pen-Gaṅga in Madhya Pradesh und Maharashtra, the Panch Ganga and Dugh Ganga of Maharashtra, and the Manawali Gaṅga in Sri Lanka.

28 Talageri 1993, to be sure, assumes a reverse expansion to the west, based on the names of Vedic seers as they are given in the later tradition. But when the later tradition is in conflict with the early Vedic textual evidence, it is methodologically sounder to rely on the latter.

29 There is, to be sure, a certain controversy on the question whether these features belong to the time before or after the departure of the Anatolians; see the discussion and references in Hock & Joseph 1996:514-5. But this does not change the fact that the features are found in Indo-Iranian. (A renewed reading of Melchert 1985 convinces me that the linguistic evidence of the Anatolian languages does not create an obstacle to the assumption that the domestication of the horse took place before the departure of these languages.)

30 For Avestan, see, e.g., raθāezštā ‘standing on the chariot’ = ‘warrior (caste)’.

31 Dhavalikar’s conclusion that the horses and chariots of this later period are to be attributed to an Aryan ‘infiltration’ is however not a necessary one. The possibility of indirect commercial contact cannot be excluded. I find this especially likely in the case of the late horse figurines from Pirak which, to me, have a (late) Indus Civilization character and thus seem to represent an integration of a new element into the existing Indus tradition. The case is different for the horse burials in Swat, which seem to be culturally quite different from Indus practices and thus may well reflect the arrival of a new culture group.

32 Singh (1997:57-8) attempts to reduce the value of this fact by denying the importance of the horse in Vedic culture and claiming that aśva at first means ‘donkey’ and, in this meaning and form, has wide-spread parallels in other Indo-European languages, such as Old Engl. assadassen, Goth. asilus ... Lith. asilas, Gael. asal, Welsh asyn, Lat. asinus. words which he derives from aśva (fn. 4). In addition he claims that the words for horse, with few exceptions, do not concern its speed and then contradicts himself with the claim that ‘all the synonyms of
horse — vāji, haya, hari, paidva, sapti, arvā, maya, atya, vahni — denote either its carriage capability or superiority in speed in comparison to other animals.

Elsewhere (especially 62-3) he proposes that the horse is indeed imported from outside — by the Vedic Aryans, whom he identifies with the Harappans. Concerning the linguistic value of Singh’s suggestions, it is to be noted that the European words for ‘donkey’ all are direct or indirect loans from Lat. asinus, which in turn appears to be of ‘Mediterranean’ origin. Words that are genuinely related are of the type Lat. equus, Goth. aihwa- ‘horse’; the ś and the a-vowels of Skt. aśva are the result of specifically Indo-Iranian or Indo-Aryan innovations.

It is irrelevant for present purposes whether Jha & Rajaram’s reproduction/reconstitution was a ‘hoax’, as claimed by many of their detractors, or simply a case of being misled by an excessive zeal to prove the existence of horses in the mature Indus Civilization.

Elst (1999a:182) tries to account for this fact by assuming that there was a taboo on horses. But without supporting evidence, this account must be considered a petio principii.

Similar claims are found in most other recent Hindutva accounts.

Moreover, the chronology implicit in Elst’s hypothesis causes difficulties. The Vedas would have to be placed before the early 3rd millennium BC, the approximate beginning of the Indus Civilization. Since the Vedic tradition exhibits linguistic and other developments that must have taken several hundred years, the beginning of the tradition would have to be set to at least the middle of the 4th millennium. In order to explain the great linguistic differences between Vedic and the other, related languages, and the differences of all these languages from the Indo-European proto-language at least another millennium would be required. (The current view is rather that two millennia are necessary: from the early 4th millennium to the first attestations of the individual languages at the beginning of the 2nd millennium.) In this manner, then, we would get at least to the middle of the 5th millennium. This time, however, and the assumption of a South Asian original home are incompatible with the fact that according to archaeological evidence the domestication of the horse took place only in the early 4th millennium, and in the present-day Ukraine. To justify his hypothesis, Elst would have to be able to furnish clear and uncontrovertible evidence for an earlier domestication of the horse in South Asia.

Up to now, his claims have come to my attention only through the discussion by Edwin Bryant in the manuscript of his soon-to-be-published monography on the AIT.

For reasons of place I limit myself to the above remarks on the Indo-Europeanist issues regarding the AIT/AET and refer to Hock 1999a for further discussion.
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