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Autochthonous Aryans?
The Evidence from Old Indian and Iranian Texts.

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The[*] ''Aryan question'' is concerned with the immigration of a population speaking an archaic Indo-European language, Vedic Sanskrit, who celebrate their gods and chieftains in the poems of the oldest Indian literature, the Ṛgveda, and who subsequently spread their language, religion, ritual and social organization throughout the subcontinent. Who were the 'Aryans'? What was their spiritual and material culture and their outlook on life? Did they ever enter the Indian subcontinent from the outside? Or did this people develop indigenously in the Greater Panjab? This, the 'Aryan' question, has kept minds -- and politicians -- busy for the past 200 years; it has been used and misused in many ways. And, its discussion has become a cottage industry in India during recent years. In this paper, it will be attempted to present the *pro*s and *contra*s for the (non-)occurrence of a movement of an 'Aryan' population and its consequences. First, a summary of the traditional 'western' theory, then the recent Indian counter-theories; this is followed by an evaluation of its merits; the paper concludes with some deliberations on the special kind of 'discourse' that informs and drives the present autochthonous trend.

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[*] A first, shorter version of this paper was written in 1997 and was to be published that year in a special issue of a science journal in India; this has mysteriously not materialized and was in fact abandoned in 1999; this paper has been constantly updated in light of recent indigenist discussions; it has been revised now (Dec. 2000), especially in the linguistic section, as H. Hock's discussion (1999) of "Out of India" scenarios has relieved me of a detailed treatment of several such theories (Misra 1992).

§1. Terminology

At the outset, it has to be underlined that the term *Ārya* (whence, Aryan) is the *self-designation* of the ancient Iranians and of those Indian groups speaking Vedic Sanskrit and other Old Indo-Aryan (OIA) languages and dialects. Both peoples called themselves and their language *ārya* or *arya*: The Persian King Darius (519 BCE) was the first who wrote in *ariya* and a Late Vedic text, Kauṣītaki Āraṇyaka 8.9, defines the Vedic area as that where *āryā vāc* "Ārya speech" (i.e. Vedic Sanskrit) is heard. The ancient Eastern Iranians, too, called themselves *a^iriia*: their *assumed* mythical 'homeland',[1] *a^iriianąm vaējah,* is described in the Avesta (Vīdēvdād 1); and the name of the country, *Irān*, is derived from this word as well. Speakers of Aryan (i.e. of the IIr. languages) occupied, e.g. in the first millennium BCE, the vast area between Rumania and Mongolia, between the Urals and the Vindhya, and between N. Iraq/Syria and the Eastern fringes of N. India. They comprised the following, culturally quite diverse groups.

(a) North Iranians: Scythians in the vast steppes of the Ukraine and eastwards of it (surviving as the modern Ossete in the Caucasus), the Saka of Xinjiang (Khotanese and Tumshuq, mod. Sariqoli) and western Central Asia, the Saka *tigraxauda* (the "pointed cap" Saka) and the Saka *haumavarga* ("the Soma pressing Saka");

(b) West Iranians: the ancient Medes (*Māda* of Rai and Azerbaijan), the mod. Kurds, Baluchis, and Persians (ancient *Pārsa* of Fārs) as well as the Tajik;

(c) E. Iranians in Afghanistan, Baluchistan, Tajikistan, Uzbekistan: speakers of Avestan, Bactrian, mod. Pashto, the mod. Pamir languages, Sogdian (mod. Yaghnobi), and Choresmian;

(d) The recently islamized Kafiri/Nuristani group in N.E. Afghanistan with the still non-Islamic Kalash in the Chitral valley of Pakistan; to this day they have preserved many old traits, such as the c. 2000 BCE pronunciation of '10' (*duc*) and the old IIr. deity *Yama Rājā* (*Imrå*);

(e) The speakers of Indo-Aryan: from Afghanistan eastwards into the Panjab, and then into the north Indian plains. By the time of the Buddha, the IA languages had spread all over the northern half of the subcontinent and had displaced almost completely the previously spoken languages of the area.

Linguists have used the term *Ārya* from early on in the 19th cent. to designate the speakers of most Northern Indian as well as of all Iranian languages and to indicate the reconstructed language underlying both Old Iranian and Vedic Sanskrit. Nowadays this well-reconstructed language is usually called Indo-Iranian (IIr.), while its Indic branch is called (Old) Indo-Aryan (IA). An independent third branch is represented by the Kafiri or Nuristani of N.E. Afghanistan. All these languages belong to the IIr. branch of the Eastern (or Satem) group of the Indo-Euroepan (IE) languages which differs from the phonetically more conservative western IE by a number of *innovations*. The IE languages (which, confusingly, sometimes were also called "Aryan") included, in ancient times, the vast group of tongues from Old Icelandic to Tocharian (in Xinjiang, China), from Old Prussian (Baltic) to Old Greek and Hittite, and from Old Irish and Latin to Vedic Sanskrit.

However, the use of the word Ārya or Aryan to designate the speakers of all Indo-European (IE) languages or as the designation of a particular "race" is an aberration of many writers of the late 19th and early 20th centuries and should be avoided. At least from Neolithic

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[1] On this question see now Witzel 2000; see below § 9, end.

times onwards, language had little to do with "race"; language *also* cuts across ethnic groups and cultures,[2] and had little to do with ancient states or with nationhood, as the use of Aramaic in the Persian empire, Latin in Medieval Europe and Persian in much of the Near East and in medieval India may indicate.

It is clear that in the India of the oldest Vedic text, the Ṛgveda (RV), *ārya* was a cultural term (Kuiper 1955, 1991, R. Thapar 1968, Southworth 1979, 1995) indicating the speakers of Vedic Sanskrit and the bearers of Vedic culture and Vedic ritual; it simply meant 'noble' by the time of the Buddha and of the early Sanskrit drama. It is also clear that the poets (*ṛṣi, brahmán, vipra, kavi*) of the Ṛgveda and their aristocratic patrons regarded themselves and their followers as *arya/ārya*. (Thieme 1938).

In the sequel, I will carefully distinguish between the following usages: first, the *ārya/ariya/a^iriia* languages, which I will call by their technical name, *Indo-Iranian* (IIr).[3] When referring to their Indian sub-branch, I will use *Indo-Aryan* (IA, or Old IA). However, the tribes speaking Vedic Sanskrit and adhering to Vedic culture, I will call *Indo-Aryan* or *Ārya*. (In common parlance in India, however, *Aryan* is used both to refer to IA language as well as to the *people* speaking it and belonging to the sphere of Vedic culture, or even to an Aryan '"race'").

§ 2. Texts

Since most of our evidence on the ancient 'Aryans' comes from the texts and from the linguistic and cultural data contained in them, it is necessary to give an outline what kind of texts we have for the early period.

For India, we have the Vedas, a large collection of texts, orally composed and orally transmitted well into this millennium. Tradition has taken care to ensure, with various techniques, that the wording and even tone accents, long lost from popular speech, have been preserved perfectly, almost like a tape recording. This includes several special ways of recitation, the Padapāṭha (word-for-word recitation) and several complicated extensions and modifications (*vikṛti*).[4]

They contain mainly religious texts: hymns addressed to the gods (RV), other mantras in verse or prose (YV, SV, AV Saṃhitās) which are used in the solemn Vedic (*śrauta*) ritual and the "theological" explanations (Brāhmaṇas and Kṛṣṇa YV Saṃhitās), composed in the expository prose of the ritual, and the Mantras used therein. The Upaniṣads contain (along with some late RV and AV hymns) early speculation and philosophy, and the ritual is summed up in systematic form in the Sūtras dealing with the solemn ritual (Śrauta-S.), the domestic ritual (Gṛhya-S.) and proper *Ārya* behavior (Dharma-Sūtras). The traditional division of the Four Vedas into four Śruti levels of Saṃhitā, Brāhmaṇa, Āraṇyaka and Upaniṣad and the ensuing Smṛti level (with the Sūtras), is somewhat misleading as far as the development of the texts are concerned. For, the Vedic texts show a clear linguistic development, just as any other living language; we can distinguish at least five clearly separate levels of Vedic (Witzel 1989):

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[2] See, however, such early and clear statements against an "Aryan race" as those by M. Müller 1888, H. Hirt 1907: 6-7, Franz Boas 1910 [1966].

[3] Confusingly, linguists sometimes use "Aryan" as a shortcut designation of IIr. because both Iranians and Indo-Aryans call themselves and their language *arya/ārya* (see below).

[4] Staal 1983: I 683-6, with special reference to techniques of memorization; Staal 1986, 1989.

1. Ṛgvedic (with many hymns of RV 10 as a late addition);
2. 'Mantra language' (AV, SV as far as differing from RV, YV Mantras, RV Khila);
3. Prose of the Kṛṣṇa Yajurveda Saṃhitās (MS, KS/KpS, TS);
4. Brāhmaṇa language, where the late (and mainly S.-E.) level includes the Āraṇyakas and the early Upaniṣads but also the early Sūtras such as BŚS;
5. Sūtra language which gradually gives way to Epic/Classical Sanskrit.

This distinction is important as it represents, apart from a relative chronology based on quotations, the only inner-textual way to establish a dating of these texts.

The Iranians have a set-up of texts quite similar to that of the Vedas (though this is little observed). However, only about a quarter of the original Avesta has been preserved after Iran became an Islamic country in the 7th c. CE. The 5 long Gāϑā (with 17 individual Gāϑās = Yasna 28-53) are the RV-like poems of Zaraϑuštra himself; the contemporaneous ritual text embedded among the Gāϑās, the Yasna Haptaŋhāⁱti, is a YV-like collection of Mantras used for fire worship.

The rest of the Avestan texts is post-Zoroastrian: some sections of Y 19.9-14, Y 20-21 are like a Brāhmaṇa passage; the Yašt pick up themes of RV style praise of certain gods (Miϑra, Vāiiu, etc.), while the Nirangistān is of Śrautasūtra style, the late Vīdēvdād reads like a Gṛhya/Dharmasūtra, and the Nighaṇṭu list of the Nirukta has its echo in the Farhang-ī-ōim. Importantly, the whole Avesta has come down to us (just like the one surviving version of the RV) in Padapāṭha fashion, with most of the *sandhi*s dissolved. The list of genres and of the ordering of texts indicates how close both traditions really are, even after the reforms of Zaraϑuštra.

However, in spite of being geographically closer to the Mesopotamian cultures with datable historical information, the Avestan texts are as elusive to absolute dating as the Vedic ones. Mesopotamia (or early China) simply do not figure in these texts.

§ 3. Dates

An approximation to an absolute dating of Vedic texts, however, can be reached by the following considerations:[5]

(1.) The Ṛgveda whose geographical horizon is limited to the Panjab and its surroundings does not yet know of iron but only of the hard metal copper/bronze (W. Rau 1974, 1983; *ayas* = Avest. *aiiah* 'copper/bronze'). Since iron is only found later on in Vedic texts (it is called, just as in Drav. **cir-umpu*), the "black metal" (*śyāma, kṛṣṇa ayas*) and as makes its appearance in S. Asia only by c. 1200 or 1000 BCE,[6] the RV must be earlier than that.[7] The RV also does not know of large cities such as that of the Indus civilization but only

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[5] Max Müller had come to a similar chronology, but --long before the prehistory and archaeological past of S.Asia was known at all-- one based on internal evidence and some speculation, a fact he often underlined even late in his career. This is nowadays misrepresented by the autochthonists, especially Rajaram (1995), who accuses Müller to have invented this chronology to fit in with Bishop Usher's biblical calculations!

[6] This date obviously depends on Archaeology. While dates for iron had been creeping up over the last few decades, there is a recent re-evaluation of the Iron Age, see Possehl 1999b, and Agrawal & Kharakwal (in press). Apparently, the introduction of iron in India differs as per region but is close to 1000 BCE. Occasional finds of meteoric iron and its use of course predate that of regularly produced, smelted iron.

[7] For indigenous dates which place the RV thousands of years earlier, see below §11 sqq. Similarly, Talageri (2000, cf. below n. 84, 87, 140, 173, 175, 216) who purports to have based his historical analysis of the RV only on the text itself, betrays a Purāṇic mentality and inadvertently introduces such traditional data (see below, and

of ruins (*armaka*, Falk 1981) and of small forts (*pur*, Rau 1976). Therefore, it must be *later* than the disintegration of the Indus cities in the Panjab, at c. 1900 BCE A good, possible date *ad quem* would be that of the Mitanni documents of N. Iraq/Syria of c. 1400 BCE that mention the Ṛgvedic gods and some other Old IA words (however, in a form slightly preceding that of the RV).[8]

(2.) The Mantra language texts (AV etc.) whose geographical horizon stretches from Bactria (*Balhika*) to Aṅga (NW Bengal) mention iron for the first time and therefore should be contemporaneous or slightly rather later than 1200/1000 BCE.

(3.) The YV Saṃhitā prose texts have a narrow horizon focusing on Haryana, U.P. and the Chambal area; they and (4a.) the early Br. texts seem to overlap in geographical spread and cultural inventory with the archaeologically attested Painted Gray Ware culture, an elite pottery ware of the nobility, and may therefore be dated after c. 1200 BCE (until c. 800 BCE).

(4b.) The end of the Vedic period is marked by the spread of the Vedic culture of the confederate Kuru-Pañcāla state of Haryana/U.P. (but generally, *not* of its people) eastwards into Bihar (ŚB, late AB, etc.) and by a sudden widening of the geographical horizon to an area from Gandhāra to Andhra (Witzel 1989). This is, again, matched by the sudden emergence of the NBP luxury ware (700-300 BCE, Kennedy 1995: 229) and the emergence of the first eastern kingdoms such as Kosala (but not yet of Magadha, that still is off limits to Brahmins). The early Upaniṣads precede the date of the Buddha, now considered to be around 400 BCE (Bechert 1982, 1991 sqq.), of Mahāvīra, and of the re-emergence of cities around 450 BCE (Erdosy 1988). In short, the period of the four Vedas seems to fall roughly between c. 1500 BCE[9] and c. 500 BCE. (For other and quite divergent dates and considerations, see below § 11 sqq).

Old Iranian texts

Dating the Avestan texts is equally difficult. Internal evidence (Skjaervø 1995) of the older Avestan texts (Gāϑās/Yasna Haptaŋhāiti) points to a copper/bronze (*aiiah*) culture quite similar to that of the RV. The younger texts might to some extent overlap with the expansion eastwards of the Median realm (c. 700-550 BCE), while parts of the Vīdēvdād were probably composed only in the post-Alexandrian, Arsacide kingdom. An indication of the date of younger Avestan dialects is the name of Bactria, is Y.Av. Bāxδī, which corresponds to AV *balhika*; this would indicate a Y.Av. dialect at the time of the AV, c. 1200/1000 BCE (Witzel 1980). Zaraϑuštra who spoke Old Avestan should be dated well before this time. Current estimates range from the 14th to the 7th c. BCE. An early date is confirmed by linguistic

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Witzel 2001). His analysis is based on an inappropriate RV text, the *late* version compiled and redacted by Śākalya in the later Brāhmaṇa period. This includes various additions and changes made by centuries of orthoepic diaskeuasis. Such a procedure must lead to wrong results, according to the old computer adage: *garbage in, garbage out*. In order to reach an understanding of the actual Ṛgvedic period, one has to take as one's basis a secure text without additions, as established by Oldenberg already in 1888. Talageri's 500 pp. book is dealt with in detail elsewhere (Witzel 2001); it suffices to point out this *basic* flaw here. (Interestingly, he quotes and approves, five years later, my 1995 approach but proceeds to turn it on its head, using the dubious methods detailed above, and below n. 40 etc.)

[8] See below §18, on *vašana [važana], -az- > e*. The reasons for the older forms in Mitanni IA seems to be that the Mitanni, who had been in contact with speakers of pre-OIA before the RV, have preserved these archaic forms.

[9] Maximally, but unlikely, 1900 BCE, the time of the disintegration of the Indus civilization. The exact date of IA influx and incursion is still unsettled but must be pre-iron age (1200, or even 1000/900 BCE, see Possehl and Gullapalli 1999).

arguments: The name of Ahuramazdā appears, in O.Av. as *mazdā ahura (or ahura mazdā)*, but in Y.Av. as *ahura mazdā*, and in Old Persian (519 BCE) already as one word, *A[h]uramazdā,* with a new grammatical inflexion. The long history of the word points to an early date of Zaraϑuštra and his Gāϑās.[10]

§4. Indo-Aryans in the RV

A short characterization of the early Indo-Aryans based on the text of the RV can be attempted as follows. The Indo-Aryans (*ārya*) spoke a variety IIr., Vedic Sanskrit, and produced a large volume of orally composed and orally transmitted literature.

They form a patri-linear society with an incipient class (*varṇa*) structure (nobles, priest/poets, the 'people'), organized in exogamic clans (*gotra*), tribes and occasional tribal unions (Anu-Druhyu, Yadu-Turvaśa, Pūru-Bharata, the Ten Kings' coalition of RV 7.18, the Bharata-Sṛñjaya, etc.) The tribes are lead by chieftains (*rājan*), and occasional Great Chieftains, elected from the high nobility, and often from the same family. The tribes constantly fight with each other and with the with the non-IA *dasyu,* mostly about ''free space'' (*loka*, grazing land), cattle, and water rights: the Ārya are primarily half-nomadic cattle-herders (horses, cows, sheep, goats), with a little agriculture on the side (of barley, *yava*). In sport and in warfare they use horse-drawn chariots (*ratha*) on even ground and the *vipatha* (AV+) for rough off-track travel.

Their religion has a complicated pantheon: some gods of nature (the wind god *Vāyu*, the male fire deity *Agni*, and the female deities of water *Āpaḥ*, father heaven/mother earth *Dyauḥ Pitā/Pṛthivī [Mātā]*, the goddess of dawn, *Uṣas* etc.). These deities, however, are not simple forces of nature but have a complex character and their own mythology. They are part of a larger system which includes the moral gods of 'law and order': the *Āditya* such as *Varuṇa, Mitra, Aryaman, Bhaga*, and sometimes even *Indra,* the prototypical IA warrior; they keep the cosmic and human realms functioning and in order. All deities, however, are subservient to the abstract, but *active* positive 'force of truth' (*Ṛta,* similar to though not identical with the later Hindu concept of *Dharma*), which pervades the universe and all actions of the gods and humans. The gods are depicted as engaging in constant and yearly contest with their --originally also divine-- adversaries, the *Asura,* a contest which the gods always win, until next time.[11] Zaraϑuštra used this particular old IIr. concept to establish his dualistic religion of a fight between the forces of good and evil.

All gods, in the Veda especially Indra and Agni, are worshipped in elaborate rituals (e.g. the complicated New Year *Soma* sacrifice). The rituals follow the course of the year and are celebrated with the help of many priests; they are of a more public nature than the simple domestic (*gṛhya*) rituals or rites of passage. In these rituals, the gods are invited, in *pūjā*-like fashion, to the offering ground, are seated on grass next to the sacred fires, fed with meat or grain cakes and with the sacred drink of *Soma* (and also, the alcoholic *Surā*), are entertained

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[10] For details, and for the transfer of Zoroastrianism into the Persis, see K. Hoffmann 1992.

[11] Elst 1999: 207, along with many other Indian writers, curiously takes the Asuras as *real life* enemies of the Vedic Aryans; he then turns this conflict into one between the Iranian and Vedic peoples, with their different kinds of worship, and makes the "Kashmir-based Ānava (= Iranian) people fight "against the Paurava/Vedic heartland in Sapta Saindhavah"; consequently, he claims, the Iranians also changed the meaning of *deva* 'god' to *daeuua* 'demon'... (All these are outdated views that were prominent around the turn of the 19th/20th century).

by well-trained, bard-like poets (*brahmán, ṛṣi, vipra*). These compose hymns (*sūkta*), after long concentration (*dhī*) but often also on the spot, meant to invite the gods and to praise the nobility (*dānastuti*), that is the patrons of the ritual. In the few philosophical hymns of the RV the poets speculate about the origin of the universe, the gods, and the humans, the forces that keep the world moving (*ṛta, yajña, śraddhā*, or poetic speech, *vāc*).

The rites of passage are less visible in the RV (except for marriage and death); it is clear, however, that a period of training in traditional knowledge (*veda* 'knowledge'), interspersed with periods of roaming the countryside in search of a start capital of cattle (*gaviṣṭi*) as *vrāta/vrātya* (Falk 1986), is followed by the full admission to adult society and marriage. However, there is no *varṇāśrama* system yet.

§5. Irano-Aryans in the Avesta

Like the Ṛgvedic society, with its three Ārya classes (RV 10.90), the Avestan texts, especially the later Y.Av., know of three classes, the priests, noblemen, and the "farmers", for by then agriculture has become more important. However, just like the RV, the Y.Av. also knows of an artisan class (corresponding to the Ṛgvedic *Śūdra*). The O.Av. texts, however, still indicate a half-nomadic cattle-based tribal culture with small tribal units (*airiiaman*) occupying a larger territory (*dax́iiu*). The younger texts, have a clear view of all of Eastern Iran: Choresmia, Sogdia, Bactria, Margiana, Arachosia, the Helmand valley, Xnənta (Gorgān), Raɣa (Rai), Varna (Bannu, NWFP), "The Seven Rivers" (Greater Panjab, see Witzel 2000). Even in the fairly late list of V. 1, the west (Persis and maybe even Media) are conspicuously absent. Many of these tribal areas/incipient states reappear as Persian provinces (*dah^{a}yu*), but *Pārsa* is not called so as it not a "foreign (*dasyu*) territory".

Some definite historical information exists about the W. Iranians (Persians, Medes) as they were close neighbors of the Mesopotamian civilizations. They are first mentioned in Assyrian inscriptions at 835 BCE as the 27 *Paršuwaš* tribes and the Medes (c. 744/727 BCE). Thus, the W. Iranian appear early in the first millennium, while the E. Iranians can be dated only with reference to the Veda and to the early Iranian empires.

The Zoroastrian reform of the Old IIr. religion had erroneously been regarded, around the turn of the 19th/20th c., as caused by a split between the two peoples. This is still echoed nowadays in some writings but the situation is much more complex. Early IIr. religion focused on the contrast between the *deva* and the *asura*: IIr *daiua*, Av. *daēuua*, OP. *daiva* :: IIr. *asura*, Av. *ahura*, OP. *a$^{[h]}$ura-(*mazda*). In the RV both groups are regarded as are 'gods' --probably due to their equal status in the New Year contests -- and only in the post-Ṛgvedic texts, the Asura have definitely become demon-like. Of the major Asura (or, Āditya) Varuṇa, sometimes called *Asura* and *medhira/medhā* in the RV[12] appears in the Avesta as *Ahura mazdā* (cf. Ahura and Miϑra, Y. 17.10), Mitra as Miϑra, Aryaman as A^{i}riiaman, Bhaga as Baɣa, Vivasvant (Mārtāṇḍa) as Vīvaŋhuuant, and Mārtāṇḍa's brother Indra as the demon Indara.

While Zaraϑuštra kept Ahura Mazdā as (sole and supreme) deity, *the* Ahura, all other IIr. *deva* (Av. *daēuua*) are relegated to the ranks of demons, e.g. Indara, Gandarəβa (Gandharva), Nåŋhaiϑiia (Nāsatya = Aśvin). A few *deva*s and *asura*s were retained, apparently after Zaraϑuštra, as divine helpers of the Lord: Miϑra, A^{i}riiaman, Ātar (standing

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[12] RV 1.25.20; cf. also RV 7.87.4, 7.66.8 .

in for Agni), Haoma (Soma) etc. The old state of contest between the *deva* and *asura* was amalgamated with the another old opposition, that of between Ṛta (Av. *Aṣa*) and *Druh* (Av. *Druj*), Active Truth and Deceit. The Ahura(s) are the champions of Truth, the Daēuuas those of Deceit. The righteous must choose between Aṣa and Druj, between Ahuramazdā and the Daēuuas, and will be rewarded in Ahura Mazdā's heaven. -- Many of the old IIr. rituals are, however, continued in Zoroastrianism as well: there is a daily fire ritual (text in Yasna Haptaŋhāiti), a Soma (*haoma*) ritual, even animal sacrifice.

§6. The Indo-Iranians

The preceding sketch indicates the very close relationship between the two peoples calling themselves Arya. Not only are their languages so closely related that their oldest attested forms might often be taken as dialects of the same language, but their society, their rituals, their religion and their traditional poetry resemble each other so closely that it has always been regarded as certain that the Vedic Indo-Aryans, the Iranians and the Kafiri (Nuristani) are but offshoots of one group speaking IIr., a few hundred years before the RV and the Old Avestan texts.

The IIr. language, as a branch of Eastern IE, shares many peculiarities with other E. IE. languages such as Balto-Slavic: in sounds ($*k' > š/ś$: Latin *equus* 'horse', O.Irish *ech*, Toch. *yuk, yakwe* :: Lithuanian *ašvà* (fem.), IIr *$*ac'ua$* > E.Ir. *aspa*, Vedic *aśva*), but also in vocabulary (Sanskrit *dina* 'day', O. Slav. *din^i* :: Lat. *dies,* cf. Schrader 1890: 312), and perhaps even in mythology: Ved. Bhaga ''God 'Share' '', Iran. (Med.) *baga* 'god', Sogd. *baɣa* 'Lord, Sir', O. Slav. *bog^u* 'god' (though probably from N. Iranian *$*baga)$*, Skt. *Parjanya*, Lith. *Perkúnas*, O. Slav. *Perun^u* (Schrader 1890: 414). Iranian and Vedic are so close that frequently whole sentences can be reconstructed: IIr. *$*tam$ $*mitram$ $*yaj'āmadhai$* > Ved. *tam mitraṃ yajāmahe,* Avest. *təm miϑrəm yazama^{i}de.* (For more on Central and North Asian connections, see below § 12.1, 12.2., 12.6).

An IIr. parent language and large parts of the IIr. spiritual and material culture can be reconstructed by *carefully* using the method of linguistic palaeontology.[13] A very brief summary of IIr. would then include: These tribes spoke the IIr. language, had a common archaic poetry (e.g. *triṣṭubh*-like poems), with many common expressions such as 'nondecaying fame'. They had the same type of priests and rituals (Ved. *hotṛ* : Avest. *zaotar, soma : haoma*), the same set of gods and a similar mythology: Yama (Yima) and Manu descend from Vivasvant (Vīvaŋhuuant). Some of these deities are IIr. innovations (the Asura / Āditya), others go back to IE times (*agni,* Latin *ignis*; *hutam,* Greek *khutón* 'sacrificial libation' :: Engl. *god*).

IIr. society had a patriarchal, exogamic system of three classes, with tribal chieftains, and a priest/poet class. They were semi-nomadic cattle (*paśu : fšu*) herders, constantly in search for water and open pastures (*uru gavyūti : vo^{u}ru.gaoiiaoiti*), and with just a little agriculture (*yava : yauuan*). At the New Year rituals they engaged in chariot races (*ratha/raϑa* 'chariot', *ratheṣṭha : raϑaeštā-* 'charioteer'), and other sports (*muṣṭihan*), and speech contests (Kuiper 1960).

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[13] Generally, against its use, Zimmer (1990) and cf. Cowgill (1986: 66-68); but note its usefulness (§12.6), in the discussion of plants and animals.

Their society was governed by set of strict moral principles, including adherence to truth (s*atya : haiϑiia*), oaths (touching or drinking water, *kośam pā*) and other oral agreements between individuals (*arya-man : aⁱriia-man,* especially for marriage and guest friendship) and between tribes (*mitra : miϑra*) which regulated water rights and pasture.

In sum, all the linguistic and textual data mentioned so far link the Indo-Aryans of the Ṛgvedic Panjab with languages spoken in areas to the northwest of the Indian subcontinent, even if local South Asian elements already figure prominently in the RV.

§7. An "Aryan" Race?

This close resemblance in language, customs and beliefs does not, of course, imply or involve, nor does it solve the question of who exactly the people(s) were that called themselves Arya/Ārya, whom they included, or even how they looked. The question of physical appearance or 'race'[14] is of the *least* importance in describing the early Ārya, but since race has always been injected into the discussion,[15] a few words are in order.

The combination of a specific language with any 'racial' type is not maintained by linguists. At this late, post-Meso-/Neolithic stage in human development, language no longer has any *very close* relation to 'race'. Even the early Indo-Europeans were a quite mixed lot, as has been stressed for decades.[16] Recently developed methods of genetic testing (mtDNA, non-recombinant Y chromosome) have and will shed further light on this (Cavalli-Sforza 1994, 1995, Kivisild 1999, Semino 2000, Underhill 2000, Bamshad 2001, etc.). It must be pointed out that genetic evidence, though still in its infancy, is often superior to (even multi-variate) palaeontological evidence as it more specific than distinguishing types reflected in osteology, based on the simple phenotype adaptation to living conditions. Genetic evidence frequently allows to pinpoint (sub-)branches in the cladistic tree at a particular point in time and space.

In the present context, however, it is *not* important to find out what the outward appearance (''*race*'') of those speaking Indo-Aryan languages was, but how they lived, worshipped, thought, and especially what kind of poetical texts they composed. The rest is

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[14] For many decades now, a discredited term which is too vague to describe the great degree of variation among humans and not a valid indicator of anthropological and genetic distinctions between various human populations; see Cavalli-Sforza 1995.

[15] Some writers are still confused by the racist terminology of the 'blond, blue-eyed Aryan'. As Cavalli-Sforza (1994) has shown, such physical characteristics are local adaptations to a northern climate (e.g., prominent in the non-IE speaking Finns). Elst (1999: 230) strangely concludes from such data that the home of IE "lay further to the southeast," [in N. India] and that the Panjab "was already an area of first colonialization, bringing people of a new and whiter physical type [= Panjabis] into the expanding Aryan [= IE!] speech community which was originally darker". Patañjali, Mahābhāṣya [2.2.6: 411:16 sqq.] with a reference to *piṅgala-* and *kapila-keśa* 'golden/tawny haired' Brahmins is discussed as well. -- For those who still stress outward appearance ('race') it may be instructive to look at the photos of a well known actor (turned from 'white' > 'black') or a female of mixed "African-American and Native American" ancestry, who after a little make up, convincingly appears as 'Caucasian', Black, East Asian, etc. (Stringer and McKie 1996: 172-3).

[16] Curiously, Elst 1999: 174 sq., elaborates on this well known fact by stressing that the European Pre-Kurgan population has come from the East, and considers it "one of the reasonable hypotheses" that they came from India. Reasonable? India has always functioned --apart from being a stepping stone the very early migration of Homo Sapiens from Africa to (S.)E. Asia and Australia in c. 50,000-40,000 BCE-- as a *cul de sac.*

interpretation, but it is already the interpretation of the Ṛgvedic Puruṣa hymn (RV 10.90) with its four classes, *varṇa* ("colors"), which seem to be related to the traditional colors of the three IE classes, white-red-blue/green. (Puhvel 1987, cf. now also Hock 1999: 155). The term is attested since RV 2.12.4, etc. The RV often makes a distinction between light : darkness, good : evil, between Ārya : Dasyu. In many cases this is just a cultural distinction, defining the boundaries between 'Us' and the 'Others' (Witzel 1995).[17] However, many scholars of the past two centuries automatically assumed that the immigrating Indo-Aryans (coming from somewhere to the North of India/Iran) were light-skinned people. All such terms are relative, yet, the Kashmirian author Kṣemendra (11th c.) speaks of a Bengali student in Kashmir as a 'black skeleton, monkeying about' and the cult of lighter skin still is undeniable, as a look at Indian marriage advertisements will indicate.

Such 'racial' characterizations tell us little about the look of contemporary people, and as indicated above, this is not important for our investigations.[18] The speakers of (pre-)Old Indo-Aryan (pre-Vedic) *might* have been quite a diverse group from the very beginning, and even *if* many of the original immigrant bands *might* rather have looked more like Kashmiris or Afghanis and not at all like their various European linguistic relatives or the 'typical' North Indian[19] of today. Again, outward appearance, whatever it might have been, is of *no consequence* for our studies.

So far archaeology and palaeontlogy, based on multi-variate analysis of skeletal features, have not found a new wave of immigration into the subcontinent after 4500 BCE (a separation between the Neolithic and Chalcolithic populations of Mehrgarh), and up to 800 BCE: "Aryan bones" have not been discovered (Kennedy 1995: 49-54, 2000), not even of the Gandhāra Grave culture which is usually believed to have been IA.[20] There are of course minor differences between the various areas of the northwestern subcontinent (such as Sarai

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[17] Elst 1999: 209 discusses the designation of the 'Others' in the RV as 'black' by simply pointing to the richness of metaphors in Sanskrit. See rather Witzel 1995 and Hock 1999; Elst's discussion of *varṇa* (1999: 210) lacks the old IE aspect of attributing color to the three classes (Puhvel 1987); he rather combines them with the much later Indian concept of the colors of *sattva, rajas,* and *tamas*!

[18] The point is merely mentioned here in passing as some writers still use such characterizations frequently and as they attach importance to such sentences as the preceding one from Kashmir which simply express regional racism. Others, usually 'autochthonously' minded writers have frequently attacked, preferably on the internet, my earlier statements (1995) which were made precisely in the same spirit as the ones here. At any rate, what kind of outward appearance would one expect from northwestern immigrants? That of Bengalis or Tamils, or rather that of Afghanis?

[19] The term *a-nās*, which occurs just once in the Ṛgveda, was originally translated as 'mouthless' by Grassmann etc. (see below, n. 230), but has later on been understood by MacDonell-Keith etc. as 'noseless, snub-nosed'; see now Hock (1999) and cf. the speculations and elaborations of Elst (1999: 208).

[20] He summarizes the results presented by Hemphill, Lukacs and Kennedy, Biological adaptations and affinities of the Bronze Age Harappans, in: Harappa Excavations 1986-1990, edited by R. Meadow; see now Kennedy 2000. -- Apparently, the distinction is between early 2nd millennium skeleta and samples from populations dated to after 800 BCE (late Bronze age and early Iron age of Sarai Khola). Given the difference in time, this may not mean much. Note also that the calibration of radiocarbon dates in the Eighties was inconsistent, and that around 800 BCE the amount of C_{14} in the atmosphere started dropping. Ordinary radiocarbon dates for the period 800 - 400 BC, have highly unpredictable uncalibrated values. A new investigation is in order. -- Similarly about the continuity of Indian populations, Kenoyer (as quoted by Elst 1999: 236; -- Elst, however, then lapses into an altogether inappropriate political discussion of what Kenoyer might have thought, or not, about present Indian politics and the BJP! It is a mystery why such political items constantly get introduced into discussions of archaeological and literary facts).

Khola : Harappa, or even Harappa: Mohenjo Daro). Anyhow, the genetic and therefore, skeletal contribution of the various IA bands and tribes *may* have been relatively negligible (cf. n. 21,23). However, a single excavation can change the picture. Even the large *invading* force of the Huns was not attested in European archaeology until some graves were found in Hungary some two decades ago.[21] The cemeteries (if any at all in Ṛgvedic times) of the small, semi-sedentary pastoral IA groups were composed, according to the texts, of 3-6 yard high grave mounds; they are not likely to be found easily in the alluvium of the constantly shifting rivers of the Panjab.[22]

Once genetic testing will have provided us with more samples of the (few not cremated) skeletal remains from contemporary burials and of modern populations we may be in a better position to judge the phsyical character of previous and modern populations. This will become apparent even more, once not just mtDNA (inherited by females) but also the male Y chromosome (some of it likely that of immigrating tribesmen) will have been studied.[23] Only then we will be able to tell which particular strains, corresponding to which neighboring areas,[24] were present in the Northwest of the subcontinent at that time.[25]

In the end, to be absolutely clear, what *counts* is the Indo-Aryan culture, their social system, their texts, their rituals, and the frame of mind they brought into the subcontinent.

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[21] This point, already mentioned in Witzel 1995, is deliberately(?) misunderstood by indigenists and Out of India proponents (usually, on the internet). It does not matter that the Huns' intrusion was an actual *invasion* (and not a trickling in) by a group of horse riding nomads: they left as little genetic imprint in the European subcontinent as the immigrating IA bands and tribes did in the Indian subcontinent. In so far, both types of incursions can be compared well, in spite of the loud protests of the autochthonists who like to brand such statements as 'invasionist'; however, see below n. 23.

[22] RV 10.16.14, etc. speaks of burial, cremation, exposing bodies on trees and of 'throwing' dead bodies away.

[23] While preliminary mtDNA data taken from *present day* populations do not show much variation -- mtDNA is restricted to the (frequently more sedentary) female lineage only -- there are indications already that the study of the male-only Y chromosome will revolutionize our thinking. In any immigration scenario, the Y chromosome obviously is of more interest. The matter has been discussed at length at the *Third Round Table on Ethnogenesis of South and Central Asia* at Harvard University, May 12-14, 2000, see: http://www.fas.harvard.edu/~sanskrit/RoundTableSchedule.html. Just as in Bamshad et al. (2001), there are clear indications of *several* incursions, after c. 50,000 BCE, of bearers of different types of Y chromosome polymorphisms from Western Asia, terminating in South Asia or proceeding further eastwards. Several of them do not correspond to, and go beyond, the seven Principal Components of Cavalli-Sforza et al. (1994: 135-8). The impact of immigrants, however, can have been relatively minimal. See for example Cavalli-Sforza about the immigrant Magyars (Hungarians). They now *look* just like their neighbors, as these late, 9th cent. CE, horse riding invaders left only a minimal trace in the larger Danubian gene pool (quoted by Elst 1999: 224, from an interview of Cavalli-Sforza in *Le Nouvel Observateur* of 1/23/1992); see now Semino 2000: 1158 for lack of "Uralic genes" in Hungary. Nevertheless, the Magyars, just like Indo-Aryan speakers, imposed or transmitted, under certain social conditions, their language to the local population, and the Magyars also retained their own religion until they turned to the local religion, Christianity, around 1000 CE.

[24] It is a fallacy to compare various Brahmin groups of India in order to establish a common older type. Brahmins, just like other groups, have intermarried with local people, otherwise how would some Newar Brahmins have 'Mongoloid' characteristics, or how would Brahmins of various parts of India have more in common with local populations than with their 'brethren', e.g. in the northwest? Studies based on just one area and a few markers only, such as E. Andhra (Bamshad 2001) do not help much (cf. also Elst 1999: 214, 217). Early acculturation processes (especially when following the model of Ehret, 1988) may have resulted in the inclusion of many local elements into the Brāhmaṇa class, cf. Kuiper 1955, 1991, 2000, Witzel 1989, 1995, 1997, 1999a,b.

[25] Note the difficulty of obtaining *contemporary* DNA materials due to the (telling!) transition to cremation in the early post-Indus period (Cemetery H at Harappa and in Cholistan).

These items are treated at some length below; in addition, we have to take into account the facts from archaeology, human palaeontology, genetics, history of technology, and incidental features from astronomy to zoology.[26]

§8. Immigration

Immigration, however, has often been denied in India especially during the past two decades, and more recently also by some western archaeologists. How likely is an immigration scenario on the basis of comparable cases from Indian and non-Indian history? Leaving aside the prehistoric migrations starting with the move of Homo Sapiens 'Out of Africa' some 50,000 years ago, we actually *do know* that one group after the other has entered the Indian subcontinent, as immigrants or as invaders, in historical times. They include tribal groups such as the Saka, the Yue Ji (Tukhara), Kushana, Abhīra, Gurjara as well as large armies, such as those of Darius' Persians, of Alexander's and the Bactrian Greeks in the first mill. BCE, of both the Chinese via Tibet, Ladakh and Nepal, and the Arabs into Sindh in the 7-8th c. CE; further the Ahom Tai in Assam, and the Huns, Turks, Moghuls, Iranians, and Afghans via the northwestern passes in the first and second mill. CE. In addition, small-scale semi-annual transhumance movements between the Indus plains and the Afghan and Baluchi highlands continue to this day (Witzel 1995: 322, 2000). Why, then, should all immigration, or even mere transhumance trickling in, be excluded in the *single* case of the Indo-Aryans, especially when the linguistic evidence, below §10 sqq., so clearly speaks for it? Just one "Afghan" Indo-Aryan tribe that did not return to the highlands but stayed in their Panjab winter quarters in spring was needed to set off a wave of acculturation in the plains, by transmitting its 'status kit' (Ehret) to its neighbors.[27] The vehement denial of any such possibility (see below §11 sqq) is simply unreasonable, given the frequency of movements, large and small, into South Asia via the northwestern corridors.

The important, clinching factor (§ 10) to decide the question is the following: the Indo-Aryans, as described in the RV, represent something definitely *new* in the subcontinent. Both their spiritual and much of their material culture are new; these *and* their language link them to the areas west and northwest of the subcontinent, and to some extent beyond, to the

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[26] Cf. Witzel 1995. Many of such data have been summed up and cogently discussed by Kochhar 1999; however, not all of his results (e.g. the restriction of the RV habitat to S. Afghanistan) can be sustained.

[27] Actually, even this is, strictly speaking, not necessary. The constant interaction of "Afghan" highlanders and Indus plain agriculturists could have set off the process. A further opening was created when, after the collapse of the Indus Civilization, many of its people moved eastwards, thus leaving much of the Indus plains free for IA style cattle breeding. A few agricultural communities (especially along the rivers) nevertheless continued, something that the substrate agricultural vocabulary of the RV clearly indicates (Kuiper 1991, Witzel 1999a,b). In an acculturation scenario the actual (small) *number* of people (often used a 'clinching' argument by autochthonists) that set off the wave of adaptations does not matter: it is enough that the 'status kit' (Ehret) of the innovative group (the pastoralist Indo-Aryans) was copied by some neighboring populations, and then spread further. -- Hock (forthc.) seems to have misunderstood me (1995: 322) when I mention transhumance movements. He thinks that this weakens my case. On the contrary, such constant, repetitive movements *strengthen* the case for close contact with the plains and eventual acculturation, a fact well known from nomad studies elsewhere. (Note also the take-over model: nomads, such as Arabs, Turks that were in close contact with sedentary populations and who eventually usurped power in their host societies).

Ural area and to S. Russia/Ukraine. The obvious conclusion should be that these new elements *somehow* came from the outside.

It is indeed historically attested that the Paršumaš (Persians) moved from northwestern to southwestern Iran, but this is limited to a relatively small area only. More important are the 'Mitanni' Indo-Aryans in N. Iraq and Syria (c. 1460-1330 BCE), who clearly show IA, *not* Iranian influences (*aika* 'one' instead of Iranian *aiva*), and the Kassites who, as a first wave, preceded them in Mesopotamia. They dislodged the local Akkadian kings for several centuries, c. 1677-1152 BCE, and they have preserved names such as *Šuriiaš* (Ved. *Sūrya*) or *Abirat(t)aš* (*Abhiratha*).[28] All these groups that are in various ways culturally related to the IIr.s are *intrusive* in their respective areas of settlement. The same may be assumed as far as the Greater Panjab is concerned.

For, the massive cultural changes in the subcontinent could not have *spontaneously* developed locally in the Panjab, even assuming an amalgamation (why, by whom, how?) of various components that had been there *before*. Instead, it is easier to assume that a new element actually brought in *new* items such as the domesticated horse and the horse-drawn chariot (§21), and IE/IA style poetry, religion and ritual. Also, it is not very likely and, indeed, not visible that leaders of the Indus civilization or rather their 'Panjabi' village level successors *planned* and executed such a universal shift of the cultural paradigm themselves. A massive, if gradual introduction of (some, if not all) IA traits seems the only viable conclusion (see below, on Ehret's model).

The denial of immigration into the area of an already existing culture has recently been proposed by some archaeologists as well; they posit a purely local, indigenous development of cultures, e.g. by the British archaeologist Lord Renfrew (1987)[29] and by some Americans such as Shaffer (1984, 1999) who think that new languages were introduced by way of trade and by taking over of new models of society.

If there was immigration, who then were the indigenous inhabitants of the subcontinent? They can in fact *still* be traced in the substrates of the RV and of modern languages: an unknown Indo-Gangetic language has supplied the c. 40% of the agricultural terminology in Hindi (typical already for the RV, Kuiper 1955, 1991). A clear hint is provided by Nahalī, a small IA language spoken on the Tapti River, NW of Ellichpur in Madhya Pradesh. At successively "lower" levels of Nahali vocabulary, 36% are of Kurku (Munda) and 9% of Dravidian origin, while the oldest level, some 24%, do not have any cognates (Kuiper 1962: 50, 1966: 96-192, but see now *Mother Tongue II-III,* 1996-7) and belong to the oldest language traceable in India (Witzel 1999a,b). Clearly, Munda, Dravidian and IA are consecutive(?) overlays on pre-existing languages. Again, such a scenario is met with in many other areas of the world.

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[28] Others are more problematic. Elst (1999: 183) has the IA gods Inda-Bugash, but this collocation is not listed in Balkan (1954). We find the Maruts, perhaps Bhaga (as *bugaš*!).-- *Himalaya* (Rajaram & Frawley 1997: 123) is a phantom, as it refers to the Kassite female deity *Šumaliya*, see Balkan (1954). Incidentally, note that [Kikkuli's] manual on horse training in not at all "written in virtually pure Sanskrit" (Rajaram and Frawley 1997: 123). From what tertiary sources did they derive these innovative insights? -- Curiously, Elst (1999: 184) lets the Kassites immigrate, without any evidence (but probably following Rajaram & Frawley 1997: 124), "from Sindh to S. Mesopotamia" as a "conquering aristocracy" in a "planned invasion," after the "desiccation of the Sarasvati area in 2000 BCE." Actually, the Kassite language is neither Indo-Aryan, nor Sumerian, Elamite, Akkadian or Hurrite. It is belongs to altogether unknown language group; for details see Balkan 1954.

[29] For other areas of Eurasia; -- in the case of South Asia, however, he thinks of elite dominance achieved through Indo-Aryan immigration.

§9. Remembrance of immigration

It has frequently been denied[30] that the RV contains any memory or information about the former homeland(s) of the Indo-Aryans. It is, indeed, typical for immigrant peoples to forget about their original homeland after a number of generations (e.g., the European Gypsies claim to have come, not from India, but from Egypt and Biblical Ur in S. Iraq), and to retain only the vaguest notion about a foreign origin. Or, they construct prestigious lines of descent (Virgil in his Aeneid makes the Romans descendants of the heroes of Troy).[31] However, in the RV there are quite a few vague reminiscences of former habitats, that is, of the Bactria-Margiana area, situated to the north of Iran and Afghanistan, and even from further afield.

Such a connection can be detected in the retention by the Iranians of IIr./IA river names (Witzel 1987, 1999, Hintze 1998) and in the many references in the RV to mountains and mountain passes.[32] The mythical IIr. river *Rasā corresponds in name to the Vedic Rasā (RV, JB), the E.Ir. (Avest.) Raŋhā, and the N.Ir. *Rahā that is preserved in Greek as Rhā and designates the R. Volga.[33] Further, there are the (Grk.) Sindoi people on the R. Kuban, north of the Caucasus, and there is the (Grk.) Sindēs, the R. Murghab/Tedzhen on the borders of

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[30] See Hock, forthc. (lecture at the July 2000 meeting of the World Association of Vedic Studies at Hoboken, NJ, kindly made available to me by the author).

[31] The Gypsies claim to be from Egypt or from Ur, that is biblical S. Iraq, the Afghanis from Palestine (see below).

[32] *Necessarily*, in the (north-)west. Who, in all seriousness, would claim IA immigration via the difficult western Himalayan/Pamir trails or, worse, from South of the Vindhyas? (The Vindhyas, incidentally, are not even mentioned in Vedic literature). Immigration or large scale movement by armies via the often difficult high passes of the Himalayas has been extremely rare, and is attested apparently only in the case of some Saka at the beginning of our era, of the Turkish adventurer Haidar into Kashmir in the early 15th cent., of a Chinese army into U.P. in the early 7th c. CE, called in to help Harsha's successor, see n. 37. -- Individual Vedic passages, including those used in my 1995 paper -- in general, this is merely a first brief outline of method and a first summary of a longer study to follow,-- certainly can be discussed or challenged, which is always welcome. For one such case, see below n. 46. -- Hock (forthc.) has now challenged my interpretation (actually merely *an aside, in parentheses*, Witzel 1995: 324) of another passage, RV 2.11.18, where I took savyataḥ "on the left" as meaning 'north'. This statement was based on a previous detailed study of the designations for the directions of the sky (Witzel 1972) that was ignored by Hock (who, ironically, then proceeds to tell readers virtually the same IE facts as given in more detail in Witzel 1972). In that early paper, I pointed out cases where 'right' = south, and where 'left' (savya, even uttara!) mean 'north' in IE languages. In that sense, my apparently enigmatic statement: "Vedic poets faced the east - their presumed goal -- in contemplating the world." Hock seems to have misunderstood the passage: the "presumed goal" of course refers to the immigration theory, "contemplation" to the Vedic (and IE) world view. -- While this passage *by no means* is a proof for an eastward immigration of the Indo-Aryans and certainly was not presented as one, it fits in the general scheme of movement, for which I presented an initial account and cumulative evidence in my 1995 paper. And that is why it was quoted. In short, a lot ado about nothing. Of course, this singular sentence (as discussed by Hock in his forthc. paper, at a conference) has again be used to advantage by some fervent adversaries of the immigration theory, as always on the internet, to "prove" that the immigration (their "invasion") *theory as such* is wrong.

[33] We *cannot* rely at all on a connection between rip- and the Rhipaen (Ural) mountains, as mentioned by Bongard-Levin (quoted in Witzel 1995). Since my casual reference to his paper has been repeatedly discussed (and misinterpreted) on the internet (and by Talageri 2000: 96, 467, in 'psychological' fashion!), I underline, *again*, that the similarity between Greek Rhip- and Ved. rip- is accidental, and that RV rip- 'deceit' has nothing to do with the Ural Mountains.

Iran, Afghanistan and Turkmenistan (Tacitus, *Annales* X.10). It divides the (Lat.) *Dahae* (Ved. *Dasa/Dāsa*) from the (Lat.) *Arii* (Humbach 1991), -- a statement that almost looks as if it was taken from the RV. Both *Sindoi* and *Sindēs* preserve, with their *s-*, a pre-Iranian form of the name (details in Witzel 1999)[34] that reminds of Vedic *Sindhu* and Iran. *Hindu,* the border river of Iran and India and of the habitable world in general (Witzel 1984).

Another N. Iranian tribe, the (Lat.) *Dahae,* (Grk.) *Daai,* occurs in Vedic as *Dāsa or Dasa*. Related forms are Skt. *dāsa* "slave", the Avest. tribe of the *Dåŋha* (next to the *Airiia*), (N.)Iran. (a demon, *Aži*) *Dāha-ka,* cf. Ved. *dāsa Ahīśu* (Witzel 1995, Hock 1999), and the Uralic loan word (Vogul. Mansi) *tas* 'stranger', as well as IE > PGrk. **doselo-* > Mycenean Grk. *doero,* Grk. *doulos* "slave"; note further: Ved. *das-yu* 'enemy, foreigner', OIr. **dah-yu*, O.P. *dah^{a}yu* 'province', Avest. *daiŋ'hu-* "foreign country, enemy".[35] Apparently, foreign or conquered territory was regarded as that of the enemy and caught enemies became slaves. Conversely, one of the many loan words from IA in Finno-Ugrian is the Finnish word for slaves, captured in raids into Southern territory, *Orja*, "Aryans",[36] confirming that the North Iranians, just like the Scythian *Alan* (the mod. Ossetes) called themselves 'Arya' as well.

Another N. Iranian tribe were the (Grk.) *Parnoi*, Ir. **Parna*. They have for long been connected with another traditional enemy of the Aryans, the *Paṇi* (RV+). Their *Vara*-like forts with their sturdy cow stables have been compared with the impressive forts of the Bactria-Margiana (BMAC) and the eastern Ural Sintashta cultures (Parpola 1988, Witzel 2000), while similar ones are still found today in the Hindukush. The RV regards the cattle-rich *Paṇi*, with their walled forts (*pur,* Rau 1976, Elizarenkova 1995), as the traditional, albeit intentionally semi-mythical enemies. A Ṛgvedic myth locates the primordial cows in a cave (*Vala*, cf. Avest. *Vara*) on an island (JB) in the *Rasā*, where they were guarded by the demoniac *Paṇi*s. Against the background sketched above, this myth looks like a semi-historical 'update' (but still, a myth) involving the great/mythical border river, past foes of the BMAC area, and contemporaneous, very real enemies of the Greater Panjab.

Further traces of an Iranian connection can be seen in the hydronomical evidence discussed above and in the many references in the RV to mountains and mountain passes.[37] Also, the retention and adaptation by the Iranians of earlier pre-Ṛgvedic river names points to an earlier IA settlement in Afghanistan (*Sarasvatī = Harax^{v}aitī / Arachosia, Sarayu = Harōiiu-*

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[34] The *Sindhu* = O.P. *Ha^{n}du*, Avest. *Həndu*, if with P. Thieme, from *sidh* 'to divide', does indeed divide not only the Vedic and Iranian territories, but it also is the boundary (cf. Avest. *zraiiah vourukaṣa*) between the settled world and the Beyond; however, in several Indian languages (incl. Burushaski *sinda,* Werchikwar dial. *sende* < Shina : *sin*?) it simply seems to indicate 'river', perhaps a secondary development. A. Hintze (1998) has shown early take-over of IA geographical terms into Iranian; note also that the mythical central mountain, *us.həṇdauua* 'emerging from the river/ocean [*Vourukaṣa*]' (see Witzel 2000, 1984) presupposes an IIr word **sindhu* 'boundary of the inhabited world, big stream, ocean'.

[35] Elst (1999: 206), neglecting or misrepresenting the linguistic arguments, takes the Dāsa/Daha as "the Vedic people's white-skinned Iranians cousins" (*sic!*) while most of the Dasyu, Dāsa of the RV clearly are Indian tribes of the Greater Panjab. Rather, he takes, against the Greek, Iranian and Indian evidence quoted above, the *specialized* North Iranian (Khotanese) meaning 'man' as the *original* meaning of the word.

[36] Parpola 1988; cf. also Harmatta, in Dani 1992: 357-378, Rédei 1986, 1988.

[37] The little used Himalayan route of immigration is to be excluded (only some Saka and medieval Turks are known to have used it). The RV does not contain strong reminiscences of Xinjiang or W. Tibet, with the only possible exclusion of the Rasā RV 10.75, cf. however Staal 1990 (and a forthc. paper). -- For the Afghani highland areas, see now Witzel 2000, with references to some non-IA reminiscences in Avestan texts.

/*Harē* = Herat R.*, Gomatī* = Gomal R., *Sindhu* = *Hindu/Həṇdu*, etc., Witzel 1999, cf. Hintze 1998). One of the semi-demonic enemies in the (Afghani) mountains is *Śambara*, son of *Kulitara*, with his many fortresses (*pur*, cf. above on Hindukush forts).

Such names (studied at least since Brunnhofer 1910, Hillebrandt 1913; now Parpola 1988, Witzel 1999) retain pre-Old Iranian forms and they clearly lead back into Central Asia and Greater Iran. They also retain some vague reminiscences of former enemies (*Parna, Dāsa, Śambara) and of place names (Rasā, Sindēs, Sarasvatī,[38] Sarayu, Gomatī, Sindhu), all aligned along the *expected* route of immigration *into* the subcontinent,[39] from the northern steppes (such as those of the Volga/Urals) via Margiana/Bactria to Herat/Arachosia and E. Afghanistan (Gomal R.)[40] Then, there are the many instances in the RV which speak about actual transhumance movement of tribes through mountain passes and into the land of the 'seven rivers' (Witzel 1995) that were more open to extensive pastoralism after the decline of the Indus civilization.[41] Individuals such as the great Ṛṣi Vasiṣṭha and his clan (RV 7.33.1-3), and whole tribes such as the Bharata and Ikṣvāku (JB 3.237-8 : Caland §204), are described as crossing the Sindhu. (Incidentally, nowhere in the Vedas do we hear of a *westward* movement, as some 'Out of India' proponents would have it nowadays).[42]

The early YV Saṃhitās (KS 26.2, MS 4.7.9), however, continue to report such movements *into* the subcontinent. They state that the Kurus move *eastwards* or *southwards* victoriously, and TB 1.8.4.1 adds information about raiding expeditions of the Kuru-Pañcālas *into the east* (no longer practiced by the time of ŚB 5.5.2.3-5). The YV Saṃhitās clearly belong

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[38] Elst (199: 167) brings up the indigenist contention of a 'sea-going' Sarasvatī -- for this see below §26 and n. 202, 206. Note, however: while the Iranian Haraxˇaitī does not flow to the 'sea' but into a lake or rather, series of lakes (the Hamun) -- Elst and others autochthonists generally neglect the meaning of the word *samudra* in the Veda (see Klaus 1986)-- both rivers end in inland desert deltas of terminal lakes (Hamun) viz. the Sarasvatī inland delta near Ft. Derawar; see §25.

[39] Elst (1999: 166) excoriates me for not supplying data of reminiscences that are in fact well known (Parpola 1988, etc.) and that are actually mentioned in Witzel 1995: 321, 103, 109 sq. -- In addition, he reverses such data to make them fit an unlikely emigration of the Indo-Europeans from India (see below). In the same context, Elst (1999: 168 sq.) misrepresents, in a discussion of Staal's theories of the directions in the Agnicayana, the meaning of Indo-Iranian directions of the sky. Avestan *paurva* (correctly, *paouruua*) does not mean 'south' (Elst) but 'east', see Witzel 1972.

[40] Elst (1999: 171) excoriates me for not noticing that Iranian connections in the RV are restricted to the 'late' 8th Maṇḍala and that are, in his view, *not* found in the oldest parts of the RV. This is a fallacy: see above on the rivers Rasā, Sarasvatī, Sarayu, Gomatī, Sindhu and persons such as the (half-mythical) mountain chieftain Śambara who are prominent in the *old* books 4-6. In this context, Elst brings up and relies on the conclusions of Talageri (2000) whose "survey of the relative chronology of all Rg-Vedic kings and poets has been based exclusively on the internal textual evidence, and yields a completely consistent chronology" and whose "main finding is that the geographical gradient of Vedic Aryan culture in its Rg-vedic stage is from east to west..." This view is based on a fallacy as well: Talageri, in spite of claiming to use only RV-internal evidence, uses the post-Ṛgvedic Anukramaṇīs as the basis of his theory and even surrepetitiously injects Purāṇic notions (see § 7, n.178, Witzel 2001).

[41] See Witzel 1995. Individual passages can and should certainly be discussed. However, Hock (forthc.) goes too far in denying *any* value to allusions and descriptions referring to immigration as found in the RV: against the background of strong linguistic and (so far, sporadic) archaeological evidence, they serve as *supporting* materials and additional evidence; cf. n. 26 sq., above.

[42] They rely on <u>one</u> mistranslated statement in the Purāṇas (see Witzel 2001, and below n. 86), composed and collected several thousand years after the fact. On the unreliability of the Purāṇic accounts see §19, and Söhnen 1986.

to the post-copper/bronze age period, as they know of the use of iron. In other words, we hear about eastward/southward raids and movements of Vedic tribes towards Bihar and the Vindhya at about/after c. 1000 BCE; the same middle Vedic texts actually speak of the necessity to constantly watch one's back (Rau 1957).

Finally, in the same vein, there also is a so far neglected passage from a late Vedic text in Brāhmaṇa style, BŚS 18.44: 397.9 *sqq.* It plays on the etymologies of *ay/i* 'to go' and *amā vas* 'to stay at home', and actually seems to speak, once we apply Brāhmaṇa style logic and (etymological) argumentation style,[43] of a migration from the Afghani borderland of Gandhāra and Parśu (mod. Pashto) to Haryana/Uttar Pradesh and Bihar: *prāṅ Āyuḥ pravavrāja. tasyaite Kuru-Pañcālāḥ Kāśi-Videhā ity. etad Āyavam. pratyaṅ Amāvasus. tasyaite Gāndhārayas* [+]*Parśavo*[44] *'rāṭṭā ity. etad Āmāvasyavam.* "Āyu went (*ay/i*) eastwards. His (people) are the (well-known) Kuru-Pañcāla and the Kāśi-Videha. That is the Āyava (group). Amāvasu (stayed at home,[45] *amā vas*) in the West. His (people) are the (well-known) Gāndhāri, Parśu and Arāṭṭa. That is the Āmāvasyava (group)."[46]

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[43] Witzel 1979, 1986, Wezler 1996.

[44] The Sandhi in *gandhārayasparśavo* is problematic. The MSS are corrupt and differ very much from each other. However, *Parśu* must be intended; it is attested since RV 8.6.46, a book that has western (Iranian) leanings (Witzel 1999), cf. OP *Pārsa* 'Persian' < **pārsva* < **pārc'ua*. The Aratta (with various spellings, *Āraṭṭa, Arāṭṭa),* are a western people as well, like the Gandhāra and other 'outsiders' (*Bāhīka,* ŚB 1.7.8.3, Mbh 8.2030). One may compare the old Mesopotamian name *Aratta*, indicating a distant eastern country from where Lapis Lazuli is brought (Witzel 1980); it seems to refer to Arachosia, which is just north of the Chagai Hills that produce Lapis (just as the more famous Badakhshan, north of the Hindukush); see now Possehl 1996b and P. Steinkeller 1998. -- Elst 1999: 184 wants to understand this ancient Sumerian term as a Prākṛt word, from *a-rāṣṭra*, again inventing an early Prākṛt before 2000 BCE, which simply is linguistically impossible (see n.167, on Mitanni *satta*) and which also does not fit the non-IIr. linguistic picture of 3rd millennium Greater Iran (see § 17).

[45] Alternatively, echoing the first sentence: "Amāvasu (went) westwards." See discussion in the next note.

[46] This passage, quoted in an earlier publications (1989, excerpted and --unfortunately-- simply computer-copied in 1995), was not correctly translated as printed in 1989/1995. It has elicited lively, if not emotive and abusive internet discussions, even alleging "fabrication of evidence" (see also Elst 1999: 164, who misattributes to me "the desire to counter the increasing skepticism regarding the Aryan invasion theory" as reason for writing my paper), -- all of this in spite of repeated on-line clarifications over the years and general apologies (Witzel 1997: 262 n.21). -- Retrospectively, I should have printed the full explanation in that footnote, but I was sure then that I could do so in the earlier version of this very paper, slated for print in 1997).
What had occurred was that I had unfortunately misplaced a parenthesis in the original publication of 1989 devoted not to the Aryan migration but to OIA dialects (and simply copied in my 1995 paper, a short summary of RV history), -- i.e. I printed: "(His other people) stayed at home in the West" instead of: (His other people stayed) at home in the West" or better "Amāvasu (stayed at home) in the West." In this way I had unfortunately intermingled translation and interpretation in these two summary style papers, without any further discussion, -- which set me up for such on-line criticisms as that of recent adversaries who deduce (e.g., amusingly, in the Indian right wing journal, *The Organiser*) that I do not even know the rudiments of Pāṇinean grammar. (Of course, I teach, in first year Sanskrit, the past tense of *amā + vas* as *amāvasan*, not *amāvasuḥ,* a 'mistake' some critics rhetorically accuse me of, in spite of hundreds of correct translations of such past tenses!) Or worse, they accuse me of "fabricating evidence" for the invasion theory.
However, the passage plays, in the usual Brāhmaṇa style, with these names and their Nirukta-like interpretations and etymologies. They are based (apart from *Āyu* : *āyuṣ* 'full life span'), on the names of the two sons of Purūravas, Amāvasyu : *amā vas* 'to dwell at home', as opposed to Āyu : *ay/i* 'to go', contrasting the 'stay home' peoples in the west (*Āmāvasyavaḥ: Gandhāra, Parśu, Arāṭṭa*) with those (*Āyavaḥ: Kuru-Pañcāla, Kāśi-Videha*) who went /went forth (*ay/i + pra vraj*) eastwards, as the text clearly says. --

The last account is quite different in tone and content from the well known tale of Videgha Māthava (ŚB 1.4.10-18), which is *not* a 'history of the settlement of Bihar' but a myth about the importation of *Kuru orthopraxy* and Brahmanism[47] into N. Bihar. (Witzel 1989, 1995, 1997). Such tales of authorization, empowerment and justification of rule, spiritual authority and social set-up (the Videgha or the Śunaḥśepa legends)[48] have to be carefully separated from the rather *unintentional* mentioning of little understood, dim memories of earlier homelands, notions which are fading already in the RV itself. However, these tales are perpetuated for several hundred years as far as movements further into the subcontinent are concerned.

All these data cannot be just accidental or due to the imagination of Ṛgvedic and Brāhmaṇa authors who looked for a prestigious origin of their lineage, tribe or culture: why should they look outwards to the 'barbaric' countries of Central Asia/Iran/Afghanistan?[49] The center of the world was, even according to the later parts of the RV (3.53), on the Sarasvatī in Haryana. This attitude continued to be the norm in the Brāhmaṇa period, and it is vaguely remembered in the Pāli canon; it clearly referred to even in the Manu-Smṛti (ch. 2). The northwest, denigrated by the AV (5.22, PS 12.1-2), and depicted in Nirukta 2.2, cf. 3.18 and in Patañjali's Mahābhāṣya (ed. Kielhorn, I p. 9) as occupied by Avestan speakers of the Kamboja land in S.E. Afghanistan (Witzel 1980: 92), is regarded as non-*ārya*.

Rather, the data mentioned above seem to reflect very dim memories of people and places much further west than the Panjab. Or, if one still wants to be even more cautious, one may say that the texts preserve some little or no longer understood words and phrases that

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A note of caution may be added: The missing verb in the collocation *pratyaṅ Amāvasus* allows, of course, suppletion of *pravavrāja*. If one follows that line of argument, one group (the Āyavaḥ) 'went east', the other one (the Āmāvasyavaḥ) 'went west', both from an unknown *central* area, to the west of the Kuru lands. The Kurukṣetra area is excluded as the Kurus went eastwards (i.e. toward it!), apparently from somewhere in the Panjab, (e.g., from the Paruṣṇī, the place of the Ten Kings' Battle, RV 7.18).

While the syntax may speak for the second possibility, the inherent etymological and stylistic possibilities render *both* interpretations given above somewhat ambiguous. -- Whatever interpretation one chooses, this evidence for movements inside the subcontinent (or from its northeastern borders, in Afghanistan) changes little about the bulk of evidence assembled from linguistics and from the RV itself that points to an outside origin of Vedic Sanskrit and its initial speakers. In other words, the weight given by some the internet to their point that a different interpretation of this passage would remove (all) evidence for an immigration/trickling in of speakers of Indo-Aryan is, at a minimum overblown, and in fact just a rhetorical ploy. This passage is of course just *one*, and a late one at that, speaking of tribal movements. Therefore, Elst's overblown summary (1999: 165) "The fact that a world-class specialist has to content himself with a late text... and that has to twist its meaning this much in order to get an invasionist story out of it..." is just rhetorics. The passage in question is just one point in the whole scheme of immigration and acculturation, a fact that Elst does not take into account here. ---

The Gandhāri clearly are located in E. Afghanistan/N. Pakistan, the Paršu in Afghanistan and the Arāṭṭa seem to represent the Arachosians (cf. Witzel 1980); note the Mesopot. Aratta, the land of Lapis Lazuli (cf. Possehl 1996b, Steinkeller 1998).

[47] The Paršu and Arāṭṭa are not known to be orthoprax, the Gandhāri may be so, if we apply Upaniṣadic notices, such as BĀU 3.3., cf. Witzel 1987.

[48] The adoption of the eastern tribes (Puṇḍra etc.) legend by Viśvāmitra in the Śunaḥśepa legend (AB 7.13 sqq.) clearly reflects this policy. The Ārāṭṭa (BŚS 18.13) appear next to other peoples outside the Kuru orthoprax orbit: *Gāndhāra, Sauvīra, Karaskara, Kaliṅga*; some of these and others in eastern and southern India are still regarded as 'outsiders' in late Vedic texts (AB 7.18); for earlier 'outsiders' such as the Balhika, Kāśi, Aṅga see AV 5.22, PS 12.1-2. and not the constant criticism of the "Panjabis", from the Brāhmaṇa texts onwards.

[49] An emigration westwards, as imagined by Out-of-India proponents, is excluded by a variety of arguments, discussed below, see §12.2 sqq.

point to Central Asia. In other words, there is no reason to dismiss this kind of evidence that involves a number of bands and tribes who spoke a language closely allied with Iranian, Slavic, etc., who followed customs, beliefs and rituals, and used a poetic tradition all of which go back to Indo-European sources. Just because a theory involving an *initial IA immigration*, or even a *gradual trickling in* of some bands and tribes is disliked now, regarded as historically tainted or as 'politically incorrect', this does not discredit the actual data.[50]

The Iranian textual materials on immigration are even more meager but they provide similar indirect reminiscences (*Rahā, dah^{a}yu/dai$ŋ$́hu, Hə$ṇ$du/H^{a}ndu, Parna, Daha,* etc.). These texts make, like the RV, a clear difference between the Arya and their enemies, e.g. *ana^{i}riiō da$ŋ$hāuuō* 'the non-Arya lands' (Yt 18.2 etc.) some of whose people, doubtless war captives, are described as concubines in the houses of the Mazdā worshippers (Geiger 1882: 176). The opposition between *A^{i}riia :: Tūra :: Sa^{i}rima :: Sāina :: Då$ŋ$ha*[51] (Yt. 13.143-5) is remarkable, though all these tribes are already described as having Zoroastrians among them.

A^{i}riianəm Vaējah, the first country in the list of Iranian countries (V.1) has usually been understood as the 'original' (northern, e.g. Choresmian) home of all *A^{i}riia* (a term indicating only the Eastern Iranians, Witzel 2000) However, this "best of all places and settlements" has ten winter months and only two cool summer months; such a description does not correspond to the hot summers of Choresmia etc., but refers to the climate of the mountain pastures with their numerous 'Aryan springs', that is central Afghanistan. This is an area right in the center of all the 'Iranian' lands of the Avesta, a region typical for transhumance pastoralism, which is nowadays inhabited, in part, by the Moghol descendants of the Mongol invasion of the 13th century. This so-called "homeland of the Aryans" thus occupies, for the Avesta, a central position: for the contemporary East Iranians it is the central *x^{v}anira$ϑ$a* region ('the one having particular pleasures of its own'), similar to that of *madhyadeśa*, "the Middle Country" of Manu. *A^{i}riianəm Vaējah* is certainly not located inside India (Misra 1992: 39, Elst 1999: 197 sq., Talageri 2000), nor does it have any bearing on the original home of all Iranians,[52] or even of the speakers of Indo-Iranian (Witzel 2000).

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[50] Curiously, Elst (1999: 172), after constantly propagating Out of India theories, makes a half-hearted turn: "perhaps such an invasion from a non-Indian homeland into India took place at a much earlier date, so that is was forgotten by the time of the composition of the Rg-Veda." When should Elst's hypothetical immigration have taken place, at the time of the African Exodus, 50,000 BCE? Or with the arrival of wheat in the last 10,000 years, from the Near East (Ved. *godhūma < gant-uma* < N. Eastern **xand ?

[51] In Vedic this would be: *Arya, Tura/Tūra, *Śarima, *Ś(y)ena, Dāsa.*

[52] Leaving aside various incorrect details (e.g., '*writing*' of the Gā$ϑ$ās by Zoroaster; A$ŋ$ra Mainiiu < A$ṅ$giras!), Elst's (and also Talageri's) identification of *A^{i}riianəm Vaē*jah as Kashmir is entirely gratuitous (Witzel 2000). -- Elst (1999: 196) even makes the Croats (Hrvat) descend from the Iranian Harax^{v}aitī (a feature now often repeated on the internet), while it is a well known fact of IE linguistics that Slavic retains IE *s* (but, Iran. *harah* < IIr *saras* < IE *seles). Of course, nothing is ever heard of a movement of the Arachosians towards Croatia... (and there are no connections with the Alans, who moved westwards from the steppes with the Vandals). -- Elst generally assumes, with Talageri, an emigration of the Iranians ("Ānava") from Kashmir into the Punjab and hence to Iran, just because the Vīdēvdād mentions the *Hapta Həṇdu* lands; he conveniently neglects that according to this text, the Panjab is one of the least desirable lands (15th out 16, being "too hot", see Witzel 2000). Hock (forthc.) discusses these assumptions of Elst and his predecessors (Talageri, Bhargava) in some detail, and states, correctly, that the Vīdevdād cannot be used to show an emigration Out of India (Elst's "obviously Kashmir"). However, Hock proceeds to use the text as a possible testimony for an immigration *into* India, including the old but wrong assertion that *A^{i}riianəm Vaējah* could be Choresmia. This entirely overlooks the ancient Indian and Iranian

§10. Acculturation: linguistic and cultural

While there are some such vague reminiscences of an immigration and of older homelands, it must be underlined that even the earliest RV hymns clearly reflect South Asian realities, in other words, they were already composed in the Greater Panjab. However, they also include many non-Sanskritic words and names. There are those of non-Aryan "foreigners" (*Kīkaṭa, Pramaganda*, etc.,) and demons (*Śambara, Cumuri,* etc.) but also those of noblemen and chiefs (*Balbūtha, Bṛbu*) and occasionally of poets (*Kavaṣa, Kaṇva, Agastya, Kaśyapa*). All these non-IA words do not have a Vedic or IE background (see below), something that can be determined by purely linguistic means; such words are neither *possible* in Vedic nor in IIr or Indo-European in general (Mayrhofer 1986:95, Szemerényi 1970 : 90sqq.); this is a point almost universally neglected by the advocates of the autochthonous theory (§ 11 sqq).

The appearance of such names among the groups belonging to the Indo-Aryans indicates, that *arya/ārya* does *not* mean a particular "people" or even a particular 'racial' group but all those who had joined the tribes speaking Vedic Sanskrit and adhering to their cultural norms (such as ritual, poetry, etc.) -- as has been underlined for decades (Kuiper 1955, 1991, Southworth 1979, 1995, Thapar 1968, Witzel 1995). The *Others* such as the *Kīkaṭa* (RV 3.53), who inhabit the greater Panjab *together* with the Ārya, are even declared "not to be fit to deal with cows." They form the amorphous group of the *Dasyu* "the foreigner, the enemy." While the *ārya* frequently fight among themselves, their main enemy are the *dasyu* who are portrayed in typical half-mythical fashion as "foreign devils" and demons.

In short, the Ṛgvedic evidence does not supports a clear-cut division between the various tribes/populations of those originally external, non-South Asian (i.e. Indo-Aryan) and of autochthonous nature, but it distinguishes between *ārya* and *dasyu;* it also does *not* allow for a happy co-existence (Kalyanaraman 1999) between speakers of Vedic IA (the 'cultural' *ārya*) and those who oppose them (*Kīkaṭa*, and the other *dasyu*). While it was a matter of (tribal) choice to which cultural group one belonged and which model of society and religion one followed, this choice had serious consequences for one's status and, ultimately, for the cultural survival of one's group.

This picture, clearly visible in the middle and later strata of the Ṛgveda (books 3, 7, 2, 8; 1, 10), is supported by the evidence from the older books (4-6). There must have been a long period of acculturation between the local population and the "original" immigrants speaking Indo-Aryan. Indeed, the bulk of the RV represents only some 5 generations of chieftains (and some 5 generations of poets, Witzel 1987, 1995; Talageri's claims (2000) of some two thousand years of RV composition are fantastic, see Witzel 2001). These sets of five generations are *rather late* within the framework of the RV; the famous chieftain of the Bharata, Sudās, is one of the latest mentioned. On the other hand, a number of tribal federations (Anu-Druhyu, Yadu-Turvaśa, etc.) preceded that of the Pūru and the Bharata who were dominant in the middle RV period (Witzel 1995, 1997). It is during the long period of *initial* acculturation that some of the linguistic (and cultural) features (Kuiper 1991, 1955)

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schemes of organization of territories (summed up in Witzel 2000). The text simply has an anti-clockwise description of the (east) Iranian (*Airiia*) lands.

of the early (pre-)Ṛgvedic period must have evolved. They include new grammatical formations such as the absolutives in *-tvā, tvī* (based on the archaic suffix *-tu*, as in *gatvā*)[53] and its correspondent form in *-ya* for verbs with preverbs (*saṃ-gamya*) (Tikkanen 1987). This split in absolutive formation corresponds, e.g., to Dravidian verbal structure, but absolutives are *not* found in Iranian. Significantly, Vasiṣṭha the self-proclaimed immigrant author of much of book 7, avoids them. The speakers of Indo-Aryan and the local population must therefore have interacted on a bilingual basis for a long period, *before* the composition of the present RV hymns with their highly hieratic, poetical speech (Kuiper 1991, and 2000).[54] An absolute date for this extended period can be inferred from the linguistic peculiarities of Mitanni-IA (c. 1400 BCE) that slightly predate those of the extant RV. Constant contact and bilingualism between speakers of OIA and of the local language(s) of the Greater Panjab produced such calques as the absolutives, or the use of *iti*, and perhaps even the rapid change to some Prakrit-like forms (*jyotiṣ, muhur*, etc., which have been disputed as such, see Kuiper 1991:2, 27 sqq., 79; 2000, aan de Wiel 2000).

Local influence is indeed what the non-IE part of RV vocabulary suggests, by Kuiper's count some 380 words or about 3.8% of the vocabulary of the RV (Kuiper 1991, 1995: 261). Such local substrate words can easily be identified because of their isolation within the IE-derived IA vocabulary, i.e. they always do not have Iranian, Slavic, etc. counterparts. Frequently, their sounds and syllable structure are non-IE as well. This is a point so far completely neglected or simply derided,[55] even when the evidence stares into their faces, by the advocates of the autochthonous theory (with the --only very partial-- exception of Elst 1999, Talageri 1993, 2000).[56]

Since the very concept of a substrate is often misunderstood (see the discussion by Bryant 1999), a brief characterization is in order (Witzel, forthc. b). Most words in early Vedic that do not conform to IE/IIr word structure (including sounds, root structure and word formation) and have no clear IE/IIr etymology must belong to a preceding language, a non-IA *substrate*; some of them, however, are loans from a neighboring non-IA language (*adstrate,* the favored position by those indigenists who recognize that they actually have a problem, see e.g. Lal 1997). It is, however, important to underline that it is the factor of phonetic and grammatical *structure* that does not fit -- in these cases the IE/IIr/IA one of Vedic Sanskrit. Not

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[53] This calque was formed on the basis of the old Indo-European stem *-tu* which then became fossile (*-tvī, tum, tave*, etc.), see Kuiper 1967.

[54] The RV is, by and large, a composition of poets of the Pūru and Bharata, and not of some earlier IA tribes already living in the Panjab (Witzel 1995). -- Such types of linguistic relationship are, of course, different from a *genetic* relationship that some adherents of the autochthonous theory suppose (see below). Cf. also Deshpande's essay on Sanskrit in his *Saṃskṛtasubodhinī.*

[55] Rajaram 1995: 219 "unproven conjectures", and similar statements. He regards comparative linguistics as 'unscientific', -- strange, for a science that can make *predictions*! Yet, Rajaram is a scientist, an engineer and mathematician by training.

[56] Surprisingly, Talageri (1993: 205) finds that "the overwhelming majority of Sanskrit names for Indian plants and animals are derived from Sanskrit and Indo-European (Bryant 1999: 74), even such structurally unfit words as *aṭavi, kapi, bīja* etc. (see discussion below). Even a brief look into KEWA, EWA (Mayrhofer's "unclear" etc.) would have convinced him of the opposite -- but he simply does not use such basic handbooks. In addition, he regards linguistic arguments as 'hairsplitting' (2000: 248, 299), or as 'a linguistic ploy'.

just etymology (which may remain unsolvable in many cases[57] and is, in others, not even necessary),[58] but *all* the structural features are of equal importance here. [59]

A word that superficially looks IE/IA, such as *Kosala,* is simply disqualified linguistically by its *-s-* (*pace* the out of hand dismissal by Talageri 2000: 248, 299); or, words such as *kīnāśa, kīkaṭa, pramaganda, balbūtha, bṛsaya* can by no means be explained in terms of IE: (1) there are no IE/IA roots such as *kīn, kīk,*[60] *mag, balb, bṛs* as only roots of the format {(s)(C) (R) e (R) (C/s)} are allowed[61] and (2) the sound *b* is very rare in IE; (3) suffixes such as *-ā-ś, -ṭ, -an-d/-a-nd-, -būth-/-bū-th-* are not found in IE/IA; (4) only *ṣ* (but not *s*) is allowed in Vedic after *i,u,r,k*. In addition, these words do not have any cogent IE/IA etymologies.[62]

The use of such formal, structural categories immediately allows to detect many words as being non-IE, and as originally non-IA. Just as for IE and IA, similar structural rules exist Drav. and for Munda. The basic Dravidian word structure (in the sequel ə = long or short vowel) is (C)ə(C), and suffixes have the structure: -C, -Cə, -CCə, -CCCə; after a root -C the vowels -a-, -i-, or -u are inserted, thus əC-a-C etc., CəC-a-C etc..; and with base final -C-u, CəC-a-C-u (Krishnamurti, forthc. 2001). While the present Munda word structure includes (Pinnow 1959: 449 sqq.) CəCə, CəəC, CəCə, əCCə, əVVəC, CəCCə, CəCCəC, the oldest word structure was: (C)ə(C), Cə-CəC, CəC-Cə'C, CəC-əC, CəC-Cə'C-əC. Clearly, both Drav. and Munda words are frequently enough quite different from IE ones with: (prefix) + (C)(R)e(R)(C) + (suffix + ending). While Drav. and Munda share CəC, CəCəC, Munda words can often be distinguished, as Cə- in Cə-Cəc is a prefix, something that does not exist in Drav.; and while CəCəc may exist in IE/IA (even with a prefix Cə-), normally, CəC- will be the IA root and -əC a suffix.

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[57] Especially when the underlying language is not one of the known ones -- IA, Proto-Drav., Proto-Munda, Proto-Burushaski, etc. but one of the unknown Gangetic languages (such as Masica's "Language X", see Masica 1979) or my own proposal for the Panjab-based prefixing Para-Munda language (Witzel 1999 a,b); cf. Bryant 1999: 73.

[58] In the heavily Anglicized *Massachusetts* area, for example, one does not need to know the local native American language to notice that place names such as *Massatoit, Massachusetts, Wachusetts, Montachusetts, Cohasset, Neponset, Mattapoisett, Mattapan, Mashpee, Chicopee, Nantucket, Pawtucket* are related and without English etymology.

[59] The problem is entirely misunderstood by those (quoted by Bryant 1999: 72) who merely delight in pointing out the differences in etymological proposals by IE, Drav., or Munda proponents. That does not discredit the linguistic (or even the etymological) method, as these branches of linguistics are not yet as developed as IE/IA. Even when the linguistic method will have been refined in the non-IA languages of S. Asia, there always will be some difference in opinion in *those* cases that actually *allow* multiple interpretation, that is after one has applied the structural rules of IA/IE, Drav., Munda, described below; for details see Witzel forthc. b).

[60] With the exception of the onomatopoetic *kik* in 'magpie', Skt. *kiki-* in *kikidīvi* (EWA I 349); *mag/meg* does not exist in IE.

[61] C = consonant, M = voiced/mediae, T = unvoiced/tenues, R = resonants = *y/w/r/l*; not allowed are the types RCe- or Rse- (Skt. *ṛka, *usa, etc.), and the types: *bed, *bhet, *tebh, *pep, *teurk/tekt (Skt. *bad, bhat, tabh, tork). See Mayrhofer 1986: 95, Szemerényi 1970: 90 sqq.

[62] In short: (S) (T) (R) e (R) (T/S) where T = all occlusives, R = resonant; forbidden are: M - M (*bed), M - T (*bhet), T - M (*tebh), same occl. in one root, such as: no *pep (exc. *ses), final 2 occl. or final 2 sonants, no: *tewrk, *tekt; - but *s-Teigh* etc. are allowed. -- In spite of these rules, it does not mean that IA etymologies have not been attempted, see KEWA, EWA, often working with supposed Prakritisms, as in the improbable case of *Maganda < mṛgāda* 'deer eater'.

A comparison of these data frequently allows to narrow down the origin of a word,[63] though this has not generally been done in practice (Witzel, forthc. b). IA etymologies are now discussed at a high level of sophistication, with a complete explanation of all of their constituent parts, of related roots and of suffixes employed. However, the Dravidian dictionaries *DED/DEDR* still consist only of lists of related words without further explanation; a Munda etymological dictionary still is only in the planning and collection stage, not to speak of Burushaski and other languages of the subcontinent.

Instead, etymological discussions deal, by and large, with vague similarities of ancient Vedic, old Dravidian and modern Munda words which, to quote (*pseudo-*)Voltaire: etymologies, "where consonants count little and vowels nothing." How complex it is to establish a proper etymology actually can be checked by taking a look at K. Hoffmann's and E. Tichy's 36 rules of procedure (Hoffmann 1992).

In sum, there are clear and decisive rules in place that allow to narrow down, and in many instances even to determine the origin of Vedic words. Throwing up one's hands in post-modern despair (Bryant 1999), and certainly, the haughty, non-technical dismissal (Talageri 2000) are misguided.

The range of the non-Indo-Aryan words of the RV is perhaps even more interesting than their number. They include names for local plants and animals,[64] and also a large number of terms for agriculture -- precisely those terms which are not expected in the vocabulary of the largely pastoralist Indo-Aryans who left the tedious job of the ploughman (*kināśa*) and farming in general (*tilvila, phala, pippala, khala, lāṅgala*, etc.) to the local people. Instead, they preserved only a few general IE terms, such as *yava* 'barley, grain', *kṛṣ* 'to scratch, plough', *sā* 'to sow', *sītā* 'furrow', *sīra* 'plough' (see however, EWA II 733 for the problematics of the root *sā*). Some local river names, always a very resistant part of the vocabulary, were preserved as well.[65]

In sum, an early wave of acculturation of the immigrant speakers of Old IA (Vedic) and the local population has seriously influenced even the IA poetic language and many other aspects of their traditional IIr. culture, religion and ritual. This "Indianization" of the Indo-

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[63] This should eliminate the doubt of those indigenists (cf. Bryant 1999: 80) who simply reject the notion of an unknown language or language family as source for the local loan words, language(s) that have subsequently been lost. After all, Sumerian, Elamite, Etruscan etc. belong to such isolated language families and these language(s) (families) have disappeared without descendants. Such deliberations, however, do not deter linguistic amateurs such as Talageri (1993: 200) who speaks of "a twilight zone of *purely hypothetical* non-existent languages." How many languages disappear in India per decade now? Including Nahali, fairly close to Talageri's home. They all will be pretty "hypothetical" in a decade or so unless they are recorded now (see *Mother Tongue II-III,* 1996-97): a useful, but largely neglected field of study by those who engage in endless AIT/OIT discussions, and could do useful work in the linguistic/cultural history of India instead. Especially, as 'tribals' have been and to some extent still are off limits for non-Indian researchers.

[64] Cf. the discussion by Bryant 1999: 75. It is precisely these local words that are of importance if the Indo-Aryans would have been autochthonous to the Greater Panjab. But, such plants and animal names are 'foreign', non-IE/IA (see Witzel 1999a,b). -- It is quite a different problem (Bryant 1999: 76) that many plant names in IE do not have a clear etymon. Bryant overlooks that they are IE, IA in *structure* and as such, inherited from PIE into IA. Worse, Talageri simply does not understand how a language develops over time, from pre-PIE to PIE to IIr, to IA (1993: 206) when he thinks that such words simply were colloquial or slang words. That, of course, fits nicely with his view that 'rare' words in Skt. may have a colloquial origins as well. *All remain within the fold!*

[65] Details in Witzel 1999a, cf. Bryant 1999: 78. Significantly, there is a cluster of non-IA names in eastern Panjab and Haryana (including the local name of the Sarasvatī, Vi<šam>bal/ž!), where the successor cultures of the Indus Civilization continued for a long time.

Aryans began even *before* our extant RV texts (Kuiper 1967, 1991). A certain amount of codification of this process can be detected with the formulation, in the Puruṣa hymn (RV 10.90), of the system of the *four* classes (*varṇa*) instead of the more common IE three, which system has been called, by P. Mus, "the first constitution of India".

On the Iranian side, however, one has observed, so far, very little of linguistic and other acculturation (Skjaervø 1995). It would indeed be surprising, how little O.Pers. and the other Iranian languages seem to have been affected by the preceding (substrate) languages of great cultures such as those of the BMAC area, Shahr-i Sokhta, Mundigak, Yahya Tepe and Elam, all of which amounts to nothing that would be comparable to the influx of Dravidian, Munda or other local words into Ṛgvedic Sanskrit. However, this is an erroneous impression, due to the surprising neglect by Iranists of etymological studies of Old Iranian (not to speak of Middle Iranian where we even do not have comprehensive dictionaries). There are, indeed, quite a number of words that are foreign even in Indo-Iranian (Witzel 1995, 1999 a,b, Lubotsky, forthc.)[66] and there is a host of unstudied Iranian words taken from the various local substrates (Witzel 1999 a,b, forthc. b).

While we can observe the changes common to all Iranian languages (*s > h, p, t, k +* consonant *> f, ϑ, x +* cons., etc.), even Y. Avestan often seems quite archaic, both in grammar and also in vocabulary, while Vedic seems to have progressed much more, towards Epic and Classical Sanskrit (loss of injunctive, moods of the perfect, aorist etc.). Iranian, for whatever reasons and in spite of the influx of local words, simply was less affected by the substrate than Vedic Sanskrit. This feature is of extreme importance in evaluating the linguistic materials that speak for the immigration of speakers of Old Indo-Aryan into the subcontinent.

While the intrusive traits of Indo-Aryan language, poetics, large parts of IA religion, ritual and some aspects of IA material culture are transparent, the obvious continuity of local cultures in South Asia, as seen in archaeology, is another matter. Yet, the question to be asked, is: how much of the culture of semi-sedentary tribes on the move (Scythians, Huns, Turks, Mongols) would indeed be visible in the archaeological record? The remnants of the Huns, for example, have been found only recently in some Hungarian graves; otherwise we would only know about them from the extensive literary and historical record. To put it facetiously, the Huns have been in Europe only for some 20 years.[67] Secondly, the constantly shifting river courses in the Panjab may have obscured many of the shallow remnants of the Indo-Aryan settlements: temporary, rather rickety resting places (*armaka*, Rau 1983), not big brick buildings.

Thirdly, the Indo-Aryans are known, from their own texts, to employ the services of the local populations for agriculture (RV, Kuiper 1955, 1991; for washing (Witzel 1986), and especially for pottery (Rau 1983): only sacred vessels are made by Brahmins in the most archaic fashion, without the use of a wheel (as is still done in the Hindukush!) Such Vedic

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[66] Bryant's proposal (1999: 77) that the non-IE loanwords in Iranian must come from the Proto-IIr that was spoken in Eastern Iran before the Iranians moved in cannot be substantiated. The individual P-Iran. and P-IA forms of such loans often differ from each other (Witzel 1999a, b, Lubotsky, forthc.) which is typical for repeated loans from a third source. However, he thinks that there are no local loan words in Iranian from the pre-IE languages; nevertheless see Witzel 1999a,b.

[67] Similarly, the Huns in India are only known from historical records and from the survival of their name as (*Hara-)Hūṇa* in the Mahābhārata or *Hūṇ* in some Rajasthani clans.

pottery, always executed in the same traditional manner, is therefore undatable simply by style, even if found. Everyday vessels, on the other hand, were made by low class (Śūdra) workmen (see below § 24). Continuity of local styles thus is to be expected *a priori.* However, when traditional style pottery with traditional paintings, such as in the early post-Indus Cemetery H culture, appears *together* with a new burial style, that is cremation or exposition and subsequent deposition of the bones in urns, *and* with a new motif painted on them, i.e. a small human, a 'soul', drawn inside a traditionally painted peacock, then all of this draws our attention. The bird-soul motif seems to reflect Vedic beliefs about the souls of the ancestors moving about in the form of birds (Vats 1940, Witzel 1984, Falk 1986). While this assemblage seems to indicate early acculturation, more data would be necessary in order to turn the still little known Cemetery H culture in Harappa and Cholistan into one that would definitely reflect Indo-Aryan presence.

Presence of Indo-Aryan speakers would rather be indicated by the introduction of their specialty, the horse drawn chariots with spoked wheels, horse furnishings, etc. When such items are found, there is a good chance that this represents Indo-Aryans, but alternative scenarios cannot be excluded: tribes that were influenced and/or pushed forward in front of them, such as the Mitanni and Kassites in Mesopotamia and the Hyksos in Egypt; or, simply, neighboring local tribes that early on adopted Indo-Aryan material culture.

Ideally, an "Aryan" archaeological site would include the remnants of horses and chariots, horse furnishings, a Vedic ritual site with three fire places nearby (preferably west of a river), a rather primitive settlement pattern with bamboo huts, implements made of stone and copper (bronze), some gold and silver ornaments, but with *local* pottery, evidence of food that includes barley, milk products, meat of cattle, sheep and goat, and of some wild animals. However, this particular archaeological set (or part of it) has not yet been discovered, unless we think of the Swat Valley finds, c. 1400 BCE. Swat is an area known in the RV 8.19.37 as Indo-Aryan territory, Suvāstu "good ground," however, with sponsors of sacrifice that bear strange names*: Vayiyu, Prayiyu.*[68]

In sum, we have to look out for a 'Leitfosssil', clear indicators of Indo-Aryan culture such as the chariot and Vedic ritual sites. The obvious continuity of pottery styles, taken alone, tells little. Some archaeologists such as Shaffer simply restrict themselves to report the findings of archaeology and *intentionally* neglect all the linguistic and spiritual data of the texts; in fact, some denounce them as 'linguistic tyranny' (Shaffer 1984). While this procedure may be perfectly in order for someone who simply wants to do archaeology, this approach is not sufficient to approach the early history of the subcontinent. *All aspects* of material and spiritual culture, of linguistics as well as genetics, have to be taken into account.

Advocates of the autochthonous theory, however, also maintain that there is not any evidence of *demographic* discontinuity in archaeological remains during the period from 4500 to 800 BCE,[69] and that an influx of foreign populations is not visible in the archaeological record. The remnants of the Harappans, the Harappan Cemetery H people etc., all are physically very close to each other, while the people of Mohenjo Daro stand somewhat apart. In other words: 'Aryan bones' have not been found. (Kennedy 1995, 2000, cf. Meadow 1991, 1997,1998).

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[68]One may also think of part of the assemblage of the Cemetery H culture of the Panjab (see above, n. 25).

[69] J. Lukacs asserts unequivocally that no significant population changes took place in the centuries prior to 800 BC; see now Kennedy 1995, 2000.

The revisionists and indigenists overlook, however, that such refutations of an immigration by 'racially' determined Indo-Aryans still depend on the old, 19th century idea of a massive *invasion* of outsiders who would have left a definite mark on the genetic set-up of the local Panjab population. In fact, we do not presently know how large this particular influx of linguistically attested outsiders was. It can have been relatively small, if we apply Ehret's model (1988, derived from Africa, cf. Diakonoff 1985) which stresses the *osmosis* (or a 'billiard ball', or Mallory's *Kulturkugel*) effect of cultural transmission.

Ehret (1988) underlines the relative ease with which ethnicity *and* language shift in small societies, due to the cultural/economic/military *choices* made by the local population in question. The intruding/influencing group bringing new traits may initially be small and the features it contributes can be fewer in number than those of the pre-existing local culture. The newly formed, combined ethnic group may then initiate a recurrent, *expansionist* process of ethnic and language shift. The material record of such shifts is visible only insofar as new prestige equipment or animals (the "status kit", with new, intrusive vocabulary!) are concerned. This is especially so if pottery -- normally culture-specific -- continues to be made by local specialists of a class-based society.

Similarly, Anthony (1995): "Language shift can be understood best as a social strategy through which individuals and groups compete for positions of prestige, power, and domestic security... What is important, then, is not just dominance, but vertical social mobility and a linkage between language and access to positions of prestige and power... A relatively small immigrant elite population can encourage widespread language shift among numerically dominant indigenes in a non-state or pre-state context if the elite employs a specific combination of encouragements and punishments. Ethnohistorical cases ... demonstrate that small elite groups have successfully imposed their languages in non-state situations."

Furthermore, even when direct evidence for immigration and concurrent language takeover is absent, the texts often allow such deductions, as has been well articulated by W. von Soden (1985: 12, my transl.) with regard to the much better known history of Mesopotamia: "The study of languages and the comparison of language provide better possibilities for conclusions with regard to migrations in prehistoric times. New languages never are successful without the immigration of another group of people [different from the local one]. Influences of [such] other languages can be determined in vocabulary and certain grammatical formations. The older languages of an area, even when they are no longer spoken, continue to influence the younger languages as substrates, not in the least in their sound system; new, dominant classes influence the language of the conquered as superstrates in many ways. In the early period, the influences of substrates and superstrates are always discernible only to a certain degree."

Similar things could be said about Ancient Greece, but that would lead too far here. As will be seen below, the three descriptions given just now fit the Indus/Vedic evidence perfectly.

THE AUTOCHTHONOUS ARYAN THEORY

§11. The "Aryan Invasion" and the "Out of India" theories

The preceding sketch presupposes that groups speaking Old IA (Vedic) were an intrusive element in the North-West of the subcontinent. Since language is of crucial importance for this argument, it needs to be addressed here in great detail. However, the

revisionists and autochthonists have almost completely overlooked this type of evidence, or they have outrightly denied it. Recently, some have begun to pay attention (see discussion by Bryant 1999, cf. also Elst 1999), however, still in an unprofessional manner (Talageri 1993, 2000).[70] Unfortunately, this was in large measure even true for the apparently lone Indo-European scholar in India, S.S. Misra[71] (1992).

Any immigration scenario is strenuously denied by two groups of Indian scholars: first, the revisionists, who genuinely try to reconsider the writing of ancient Indian history which they believe was very much the creation of 19th century British political ideology, and second, the autochthonists who try to show (or who simply believe in) an indigenous origin of the 'Aryans' in the subcontinent. Of course, one can find various combinations of these two strands in any person's writing (see Bryant 1999).[72]

The theories of advocates of an autochthonous origin of the Indo-Aryans (always called "Aryans") range from (1) a mild version, insisting on the origin of the Ṛgvedic Indo-Aryans in the Panjab, the "autochthonous" or indigenous school (Aurobindo, Waradpande 1993, S. Kak 1994a, etc., see Elst 1999: 119, Talageri 2000: 406 sqq, Lal 1997: 281 sqq.), (2) a more stringent but increasingly popular "Out of India" school (S.S. Misra, Talageri, Frawley, Elst, etc.) which views the Iranians and even all Indo-Europeans emigrating from the Panjab, to the (3) most intense version, which has all languages of the world derived from Sanskrit: the "Devabhāṣā school", which is mostly -but not solely- restricted to traditional Pandits.[73] (For summaries see Hock 1999, Talageri 2000.)

In these views,[74] though often for quite different reasons, any immigration or trickling in (nearly always called "invasion") of the (Indo-)Aryans into the subcontinent is suspect or

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[70] Talageri, though mentioning --unlike other OIT advocates-- the value of linguistics (2000: 415), merely lists some words and compares them as look-alikes, in Nirukta fashion. Data are listed and discussed without any apparent linguistic background and with lack of any critical, linguistic faculty. Elst is better prepared philologically and linguistically, yet still lacks linguistic sophistication; his linguistic evaluation (1999: 118 sqq, 137) is lacuneous and misses much of what is discussed in this paper; this lack is substituted for by a lot of gratuitous speculation of when and how the *hypothetical Indian* Indo-Europeans could have emigrated from India.

[71] No doubt due to his complete (self-imposed?) scholarly isolation at Benares. His (lone?) trip to an international meeting in Dushanbe, duly noted in his introduction his 1992 book, provided him with some contacts, -- unfortunately not the best ones, see his rather uncritical use of Harmatta's materials (below §12.2, n.97).

[72] Bryant (1999) reports that he found, already in 1994-5, that a majority of Indian scholars "had rejected the Aryan invasion/migration completely, or were open to reconsider it."

[73] For one such case see below, n. 235. -- The opposite is seen in deriving Skt. from Arabic in a book published in Pakistan: Mazhar 1982.

[74] The list of such internet and printed publications waxes greatly, by the month. There now exists a closely knit, self-adulatory group, members of which often write conjointly and/or copy from each other. Quite boringly, they also churn out long identical passages, in book after book, sometimes paragraph by paragraph, all copied in cottage industry fashion from earlier books and papers; the whole scene has become one virtually indistinguishable hotchpotch. A 'canonical' list would include, among others: Choudhury 1993, Elst 1999, Danino 1996, Feuerstein, Kak, and Frawley 1995, Frawley 1994, Kak 1994, Klostermaier (in Rajaram and Frawley 1997), Misra 1992, Rajaram 1993, 1995, Rajaram and Frawley 1995, 1997, Rajaram and Jha 2000, Sethna 1980, 1981, 1989, 1992, Talageri 1993, 2000. Among them, Choudhury stands somewhat apart by his *extreme* chauvinism. -- These and many others frequent the internet with letters and statements ranging from scholarly opinions and prepublications to inane accusations and blatant politics and hate speech; such ephemeral 'sources'

simply denied: The Ārya of the RV are supposed to be just another tribe or group of tribes that always have been resident in India, next to the Dravidians, Mundas, etc. The theory of an immigration of IA speaking Ārya ("Aryan invasion") is seen as a means of British policy to justify their own intrusion into India and their subsequent colonial rule: in both cases, a 'white race' was seen as subduing the local darker-colored population.

The irony of this line of reasoning is that the British themselves have been subject to numerous IE immigrations and invasions (Celts, Romans, Anglo-Saxons, Vikings, Danish, and Normans -- and now Caribbeans and South Asians). Even more ironically, there is a strong non-Indo-European substratum in English which has left such common words as *sheep.*[75] The "Proto-Anglo-Saxons", and in fact all of Europe, have been subject to the same kind of Indo-European "invasions". Europeans and Indians alike could thus complain, for example with M. Gimbutas (1991, 1994), about the domination of a "peaceful matriarchal agricultural community" by half-barbaric, patriarchal, semi-nomadic and warlike invaders. However, this is not an issue in Europe (e.g., my own, predominantly Basque genes do not protest loudly against having been subjected to an IE language and culture several millennia ago), while religious and nationalistic attitudes in India have made such "invasions" *the* issue in recent years. European Indologists, and American or Japanese even less, do not have an axe to grind, here and now. Even less so, after the recent genetic discoveries that link all present humans to a fairly recent origin and all non-Africans to an even more recent emigration by some 10,000 people Out of Africa, 50,000 years ago: the problem of an "Aryan invasion" into India is as relevant or irrelevant to Indologists as a Bantu "invasion" of central, east and southern Africa, or an Austronesian immigration into the Pacific or a Na-Dene one into North America.

§ 11.1. Procedure

Like all scientific theories, however, the theory of an immigration into South Asia by speakers of IA has to be constantly and thoroughly (re-)investigated, and it has to be established whether (all) aspects of it and/or the theory itself are correct or not. But this must be done on the basis of *hard facts*, not, due to a *dislike* of earlier historical writing, by a selective use of or by twisting of facts, or simply by sophistic argumentation (see below, on current use of long-refuted propositions). It also has to be done independently both from the present climate in India, and from the present western post-modern/deconstructionist fashion of seeing political motives behind *all* texts; both attitudes are not conducive in this kind of investigation.

Scholars of the 19th/20 cent. obviously did not have the present discussion in mind when they wrote. The best ones among them may have come to certain conclusions quite independently of their 'ideological' background. At any rate, the better scholars of the 19th century were not colonialists or racists. They all were, however, limited to some extent by the general *zeitgeist* of the period, but so are present day scholars. We, too, must constantly strive to overcome this bias (Witzel 1999d), and we also must not to follow one current trend or momentary fashion after another. We can only approach a solution by patiently investigating the pros and contras of the various points that have been made -- or still are to be made. Scholarship is an *ongoing* dialectical process.

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are not listed here; I have, however, been collecting them as they will form interesting source material for a study of the landscape of (expatriate) Indian mind of the late 20th and early 21st centuries.

[75] For place names see also Szemerényi (1970), and Vennemann 1994, and the new (IA) substrate theories in Lubotsky (forthc.).

One should avoid, therefore, to revert to long-refuted propositions. Natural scientists, other than historians, do not seriously discuss pre-Copernican or pre-Darwinian systems any longer. In the subsequent sections, all too frequently old and long given up positions are brought up and juxtaposed to recent ones in order to show 'contradictions' in what is called 'the western approach'. This is improper procedure. In the same way, one should also not confound the autochthonous theories of the past two centuries (Dayanand Sarasvati, etc.) with the present wave of indigenism, and one cannot, therefore, accuse the present autochthonous and 'Out of India' movement for contradictions with the older position of Tilak of an original Arctic home of the Aryans, (even though it has been repeated quite recently in Ganapati's SV-translation (1982) where the 'Aryans' are portrayed as having lived "on the Polar circle").

In the natural sciences and in scholarship at large, old conclusions are constantly reviewed on the basis of new evidence. But such new evidence has to fit in with the *general framework* established by the many, completely unrelated observations in the various branches of scholarship; otherwise a particular theory is revised or discarded. For example, when certain irregularities in the course of the planets were noticed, it did not mean that post-Renaissance astronomy was wrong but that this observation was due to the mass of another planet, Pluto, that was correctly predicted and, then, actually discovered in the early 20th century. But, the opposite procedure, deducing a "paradigm shift" based on isolated facts, is quite common in the contemporary effort to rewrite Indian (pre-)history.

Unfortunately, thus, the subsequent discussion is studded with examples that *explain away* older theories and even hard scientific facts with the help of new, auxiliary, *ad hoc assumptions.* All of which are then used to insist that we are due for a "paradigm shift". Consequently, it will unfortunately take much more space even to merely describe and then to evaluate the arguments of the autochthonous school(s) than to describe the older, general consensus. All too frequently, we have to reinvent the wheel, so to speak, and have to restate, and sometimes even to prove, well-known and well-tested principles and facts: this includes those of comparative linguistics (summaries by Hock 1986, Anttila 1989, Szemerényi 1970, 1996, Beekes 1995), comparative epic studies (Parry 1930, 1971, Lord 1991), of S. Asian archaeology (Allchin 1995, Kenoyer 1998, Possehl 1999), Indus epigraphy (Possehl 1996), of zoology and botany (Meadow 1997,1998), or the evidence contained in the texts, as established by philology over the past two centuries (Witzel 1997).

§ 11.2. Evidence

For the subsequent discussion, is also very important that each single item be scrutinized well before it is brought forward. At present, we can observe a cult of 'science' in India, --I have even seen 'scientific tax forms.' However, this is part of an *inclusivistic* belief system that *encapsulates,* in facile fashion, older mythical and religious ideas (Witzel 1986, 1992, 1998). Further, in spite of the stress on the 'hard sciences', all too frequently 'scientific facts' are quoted which, on closer observation, are not hard facts at all. For example, an unsuspecting reader may take for granted that "LANDSAT photos show the drying up of the Sarasvatī river in 1900 BCE" (Kak 1994, cf. S.P. Gupta 1995). But LANDSAT or aerial photos cannot by themselves indicate historical dates. (For an update, with much more cautious claims by scientists, see now Radhakrishnan and Merh 1999). Or, some selected linguistic data, such as a *supposed* (but demonstrably wrong!) change from an older *aśva-* 'horse' (as in Skt.) to Latin *equu-s* (S.S. Misra 1992), are used to indicate an Iranian and IE emigration from India. This does not only contradict standard (IE *and* non-IE) linguistic knowledge (see now

Hock 1999). It also neglects a whole range of further *contradictory* evidence, e.g. the host of local, non-IA loan words in Vedic Skt. that are missing in the supposedly 'emigrating' languages such as Iranian, Slavic, etc. (Witzel 1999 a,b; for details, below § 13 sqq.)

Other inconsistencies derive from the evidence of the texts. If the RV is to be located in the Panjab, and supposedly to be dated well before the *supposed 1900* BCE drying up of the Sarasvatī, at 4-5000 BCE (Kak 1994, Misra 1992), the text should *not* contain evidence of the domesticated horse (not found in the subcontinent before c. 1700 BCE, see Meadow 1997,1998, Anreiter 1998: 675 sqq.), of the horse drawn chariot (developed only about 2000 BCE in S. Russia, Anthony and Vinogradov 1995, or Mesopotamia), of well developed copper/bronze technology, etc. If the Brāhmaṇas are *supposedly* to be dated about 1900 BCE (Kak 1994), they should *not* contain evidence of the use of iron which makes it appearance in India only at the end of the millennium, about 1200 BCE at the earliest (Chakrabarti 1979, 1992, see now Possehl-Gullapalli 1999 for a much later date of c. 1000/900 BCE). The list could be prolonged, and some of these items will be discussed below (§ 11 sqq.)

§ 11.3. Proof

In short, the facts adduced from the various sciences that have been operating independently from each other and independently from the present 'Aryan' question -- in most cases actually without any knowledge of the Aryan discussion, -- must match, before a certain theory can be accepted. If the linguistic, textual, archaeological, anthropological, geological, etc. facts contradict each other, the theory is in serious difficulty. *All* exceptions have to be explained, and well within plausible range; if they cannot, the theory does not hold. It *never* is proper working procedure that such inconsistencies are explained away by *ad hoc* assumptions and new theories, in other words, by special pleading. Occam's razor applies. We can no longer maintain, for example, that the earth is flat and then explain away the evidence of aerial or space photos by *assuming,* e.g., some effect of light refraction in the upper strata of the atmosphere, or worse, by using one conspiracy theory or the other.

§ 11.4. The term "invasion"

To begin, in any discussion of the 'Aryan problem', one has to stress vehemently that the "invasion model" which was still prominent in the work of archaeologists such as Wheeler (1966: "Indra stands accused"), has been supplanted by much more sophisticated models[76] over the past few decades (see Kuiper 1955 sqq., Witzel 1995, Thapar 1968). It must also be underlined that this development has *not* occurred because Indologists were *reacting,* as is now frequently alleged, to current Indian criticism of the older theory.[77] Rather, philologists first,

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[76] The recent denigration of this shift by some OIT-ers such as Elst is entirely disingenuous; he insists on calling any migration or 'trickling in' an "invasion". However, immigration / trickling in and acculturation (which works both ways, from newcomer to indigenous, and from indigenous to newcomer!) is something entirely different from a (military) invasion, or from overpowering and/or from eradicating the local population. -- Incidentally, I have it on good oral authority that the idea of Indra destroying the 'fortification walls' of the Indus towns was created by V.S. Agrawal who served as cicerone in Wheeler's time and that Wheeler merely overheard him and simply picked up the idea.

[77] To mention a personal experience: when I related some of the materials that went into this paper to a well-known scholar of the older generation some three years ago (that is, someone who has considerably advanced our

and archaeologists somewhat later, noticed certain inconsistencies in the older theory and tried to find new explanations, thereby discovering new facts and proposing a new version of the immigration theories.

For some decades already, linguists and philologists such as Kuiper 1955, 1991, Emeneau 1956, Southworth 1979, archaeologists such as Allchin 1982, 1995, and historians such as R. Thapar 1968, have maintained that the Indo-Aryans and the older local inhabitants ('Dravidians', 'Mundas', etc.) have mutually interacted from early on, that many of them were in fact frequently bilingual, and that even the RV already bears witness to that. They also think, whether explicitly following Ehret's model (1988, cf. Diakonoff 1985) or not, of smaller infiltrating groups (Witzel 1989: 249, 1995, Allchin 1995), not of mass migrations or military invasions. However, linguists and philologists still maintain, and for good reasons, that *some* IA speaking groups *actually entered* from the outside, via some of the (north)western corridors of the subcontinent.

The autochthonous theory, however, maintains that there has not been *any* influx at all, of Indo-Aryans or of other people from outside, conveniently forgetting that most humans have emigrated out of Africa only 50,000 years ago. On the contrary, some of its adherents simply *reverse* the 'colonial' invasion theory, with post-colonial one-up-manship, as an *emigration* from India (the 'Out of India Theory, OIT). Its advocates like to utilize some of the arguments of current archaeology, for example those of J. Shaffer (1984, 1995, 1999). He stresses indigenous cultural continuity from c. 7000 BCE well into the semi-historic times of the first millennium, as is evident according to the *present* state of archaeology. Consequently, he protests the "linguistic tyranny" of earlier models. This is a much too narrow, purely archaeological view that neglects many other aspects, such as all of spiritual and some of material culture, but it is grist on the mills of the autochthonists.

To get, finally, to some concrete, be it necessarily often torturous, detail: opponents of the theory of an IA immigration or trickling in, whether revisionists, indigenists, or OIT adherents must especially explain the following linguistic, textual, archaeological, geographical, astronomical, and other scientific data (§ 12-31) to become credible.

§ 11.5. Linguistics

As has been mentioned above, linguistic data have generally been neglected by advocates of the autochthonous theory. The only exception so far is a thin book by the Indian linguist S.S. Misra (1992) which bristles with inaccuracies and mistakes (see below) and some, though incomplete discussion by Elst (1999).[78] Others such as Rajaram (1995: 144, 217) or Waradpande (1993), though completely lacking linguistic expertise, simply reject linguistics as "pseudo-science" with "none of the checks and balances of a real science". They simply overlook the fact that a good theory *predicts*, as has occurred in IE linguistics several times (i.e., in predicting pre-Greek $*k^w$ or the IE laryngeals, see below §12.1). On the other hand one may still consult, with profit, the solid discussion of early Sanskrit by Bh. Ghosh (1937).

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understanding of the Indo-Iranian and IA question) this scholar was simply unaware of the present discussion, and in fact, could not believe what he heard.

[78] Elst, though not without philological and linguistic training (Ph.D. Leuven, Belgium), is quite lacuneous in his interpretations and does not discuss the fine linguistic details, see below and n. 70. In his "Update" (Elst 1999), he delights in speculating about an Indian *Urheimat* of IE and a subsequent emigration, with 'Indian' invasions of Europe, all while neglecting that linguistic data speak against it, see Hock 1999 and §12.3 sqq.

The linguistic evidence, available since the earliest forms of Sanskrit (Ṛgvedic OIA), is crucial, as the materials transmitted by language obviously point to the culture of its speakers and also to their original and subsequent physical surroundings. Language has, just as history, its own 'archaeology'; the various subsequent historical 'layers' of a particular language can be uncovered when painstakingly using well-developed linguistic procedures.

Language study, however, is not something that can be carried out by amateurs, even though a 'everyone can do' attitude is widespread. This is especially pervasive when it comes to etymology and the (often assumed) origin and the (frequently lacking) history of individual words. Here, total amateurism is the rule. "Oakish" etymologies, such as *England* from *aṅguli* 'finger', or *abād* from *bath* (Guptə 1990) have a long tradition both in occidental as well as in Indian culture. Plato's Kratylos propounds the same kind of unscientific explanations as Yāska does in his Nirukta. This has been tradition ever since the Brāhmaṇa texts (*Rudra* from *rud* 'to cry', *putra* from the nonexistent word *⋆put* 'hell', *bhairava* from *bhī+rav+vam*, etc.) A look into any recent or contemporary book on Indian history or literature will bring to light many examples: Assyria from *asura*, Syria from *sura*, Phoenicians from *Paṇi*, Hittites (Khet) from *Kaṭha*, Mitanni from *Maitrāyaṇīya*, etc. (Bhagavad Datta repr. 1974, Surya Kanta 1943, Guptə 1990, etc.).

In the South Asian context, cross-family comparison (Dravidian and IA, IA and Arabic, etc.) is especially widespread and usually completely wrong, as such comparisons are simply based on overt similarities between words. In comparative linguistics, however, it is not similarity that counts but the *regularity* of (albeit outwardly, non-intuitive) sound correspondences, for example Vedic *śv* in *aśva* 'horse' : Avest. *-sp-* : O.Pers. *-s-* : Lith. *-sw-*, Latin *-qu-* [kw] : Gothic *-hᵛ-* OHG *-h-*, O.Irish *-ch-*, Gaul. *-p-*, Toch. *-k/kw-* < IE *⋆k'w,* an equation repeated in many other words; or, to quote one of the most hackneyed, non-intuitive examples: the correct equation, sound by sound, of Skt. *dvā(u),* Latin *duo* = Armenian *erku* < IE *⋆dwō(u).*

Since language and (the necessarily closely connected) spiritual culture are crucial for any theory of an influx of speakers of OIA into the subcontinent --whatever form this influx might have taken initially-- the linguistic evidence will be dealt with in detail in the following sections. Unfortunately, since the linguistic ideas and 'arguments' of the autochthonists are far off the accepted norms and procedures, a discussion of their proposals and beliefs does not only take up much space but *must* be convoluted and torturous; in addition, it must be, in its very nature, often very technical. (The non-linguistically inclined reader may therefore prefer to jump to the concluding sections of §18).

§12. Vedic, Iranian and Indo-European

It is undeniable and has indeed hardly been denied even by most stalwart advocates of the autochthonous theory, that Vedic Sanskrit is closely related to Old Iranian and the other IE languages.[79] However, this relationship is explained in a manner markedly differing from the standard IE theories, that is by an *emigration* westwards of the Iranians and the other Indo-Europeans *from* the Panjab (see below).

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[79] Though Talageri (2000) even refuses the link of Vedic with Iranian.

Vedic Sanskrit is indeed so closely related to Old Iranian that both often look more like two dialects than two separate languages (e.g. *tam mitram yajāmahe : təm miϑrəm yazamaide* 'we worship Mitra'). Any Avestan speaker staying for a few weeks in the Panjab would have been able to speak Vedic well and --with some more difficulty - vice versa. However, that does not necessitate at all that the Old Iranian dialects were introduced to into Iran from the east, from India, as the autochthonist would have it. As will be seen below (§ 12 sqq.), there are a number of features of Old Iranian (such as lack of typical South Asian substrate words, § 13 sqq.) which actually *exclude* an Indian origin. Such data have not been discussed yet by the autochthonists.

The comparison of the many common features found in Vedic Indo-Aryan and Old Iranian have led to the reconstruction of a common 'mother' tongue, Indo-Iranian, spoken (at least) around 2000 BCE, by a group of people that shared a common spiritual and material culture (see § 4-5). Beyond that, the comparison of Indo-Iranian and other IE languages has allowed similar reconstructions for all IE languages from Iceland and Ireland to Xinjiang (Tocharian) and from the Baltic Sea (Lithuanian etc.) to Turkey (Hittite) and the Panjab (Vedic IA). This theory was first developed in the early 19th century and has been tested extensively. If there were still need of proof, one may point to the many *predictions* the theory has made, especially after its more developed form had emerged, about 1870 CE, with the establishment of regular sound correspondences (*Lautgesetze*) by the Leipzig *Junggrammatiker* school. Such cases include the rather old prediction of early Greek/pre-Greek $^\star k^w$ which was discovered in writing when Mycenean Greek was deciphered in 1952, or the prediction by the young F. de Saussure more than a century ago (1879), of a set of unknown sounds. These were later called laryngeals (h_1, h_2, h_3). They have disappeared in all known IE languages but have affected their surroundings in typical, to a large extent even then predictable ways. When Hittite finally was read in 1916, h_2 was still found written (in words such as *peḥur* = Gk. *pūr* = Engl. *fire*).

Yet, some revisionists and indigenists even call into question the theories and well-tested methods of comparative linguistics. Some of them clearly do so because of a considerable lack of understanding of the principles at work (Waradpande 1989, Kak 1994a, Talageri 2000, etc.; discussion in Bryant 1999, cf. Elst 1999). In addition, they make use of the expected scholarly differences of opinion between linguists to show the whole "theory of (IE) linguistics" does not work or is an "unproved theory" (Rajaram 1995: 144, 217), thereby neglecting such well known facts as: (a) that any science progresses and that certain opinions of the 19th cent. cannot be juxtaposed to those of the 20th, and (b) that in any contemporary field of science[80] there is a certain range of generally agreed facts but also a certain range of difference of opinion, such as between traditionalists, radical skeptics,[81] and those proposing new solutions to old or recently noticed problems. In short, there always are conflicting

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[80] Note for example, in the present context, the discussion among scientists about the *various* palaeo-channels of the Sarasvatī (Sarsuti-Ghaggar-Hakra), in Radhakrishnan and Merh (1999), or the first appearance of the horse in South Asia (Meadow 1998).

[81] Such absolute skepticism, though, is always welcome as a hermeneutic tool; but, it has to be relativized: one may maintain that linguistic palaeontology does not work (S. Zimmer 1990), but how is it that IE words for plants and animals consistently point to a temperate climate and to a time frame before the use of iron, chariots, etc.? The few apparent inconsistencies can be explained (e.g., doubtful etymologies for the 'elephant', etc. see below n.127, 149).

interpretations of the materials at hand that are discussed in dialectical fashion. Some interpretations are merely possible, others probable, and still others have actually been proved and have subsequently been shown to be correct. In present day genetics, for example, some still hold that the recently developed theory of an origin of all humans from one or from a small group of African ancestors is not valid as it involves misinterpretation of statistical data and the wrong type of computer models. However, nobody has claimed that genetic investigation as such is invalid, as has been done with regard to comparative linguistics by autochthonists on and off, or who say that it remains an 'unproved theory at best'. Unfortunately for this view, historical linguistics, just like any good science, has made a number of *predictions* that later on, with the discovery of new materials, have been shown to be correct (see above).

§ 12.1. The Misra case

Worse, the recent book of an Indian linguist, S.S. Misra (1992), is even a step back beyond what is demonstrable and, strangely for a linguist, often beyond the *hard facts*, i.e. his denial of PIE laryngeals as precursors of the actually *written* Hittite laryngeal sounds (Misra 1974, 1992). He simply rewrites, on an *ad hoc* basis, much of IE (and general) linguistics. The discussion and explanation of his examples (e.g., his supposed IE $*ś > k'$, $*a > e, o, a$ etc.) would have to be quite technical and is not pursued here in detail. (It has now been discussed by Hock, 1999). It is however, obvious even to an uninitiated observer that forms such as Skt. *cakāra* (instead of $*kakāra$) must rely on the palatalizing effect of an *e*-like sound in *ca-*; cf. the Romance development from *c* [k] as seen in old loan-words, German *Kaiser*, Greek *kaisar* (whence Urdu *kaisar*), to Romance *c* [tš], as seen in Ital. *Cesare* or even to [s] as in Engl. *Cesar;* cf. also the separate development Vulgar Latin *caballus* 'horse' > *French cheval*, etc., again before *-e-*. These changes are a feature known from many languages. Why should it only have been different for pre-Ṛgvedic (and pre-Old Iranian, in other worlds, for Indo-Iranian) as Misra maintains? A case of special pleading.

The whole matter of Misra's IE reconstructions has been discussed adequately by H.H. Hock (1999) and there is no need to go into further details here. In sum, Misra's ad hoc rules do not make for a new system,[82] they are, in fact, a throwback, a regression to the early stages of IE comparative linguistics when strict rules of sound correspondences (*Lautgesetze*) had not yet been established by the Leipzig *Junggrammmnatiker* School of c. 1870.

His dating of the RV, based on this "new" reconstruction, simply rests on the similarity of *his* "early 19th cent." Proto-IE (looking altogether like Sanskrit) with reconstructed Proto-Finno-Ugric (Uralic) forms, for which he accepts the *guess* of Uralic linguists, a date of 5000

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[82] It might be summed up as follows. If his rules were correct, we would expect Skt. *aśva* 'horse' to correspond to Latin *equu-s*, but then how could *ka* 'who' to correspond to Latin *qui-s*? How could *ś* as well as *k* turn into Latin *qu* (and how does the *-u-* come about)? Skt. *k* usually corresponds to c [k] in Latin, as in *kalaśa*, Lat. *calix*; *kañcate*, L. *cingō*; *kṛṇatti*, cf. L. *cratis, crassus*; *kavi*, L. *caveō*; *kūpa*, L. *cūpa*; *kupyati*, L. *cupiō*; *kakṣā*, L. *coxa*; *kraviṣ*, L. *cruor*, etc. On the other hand, Skt. *ś* corresponds to a palatal *k'* which appears also as c *[k]* in Latin. How would the early Latin speakers have 'decided' which sound to 'choose'? --- Again, if IE $*a > e, o, a$, how could the early Latin speakers 'decide' to turn the initial *a-* and final *a* of *aśva* into *e-* and *-u* respectively? Worse, if Skt. *agni* 'fire' corresponds to Latin *igni-s*, why does *a* turn to *i*? Or how can Skt. *avṛtta-/ajñāta* correspond to Latin *invert-/ignotu-*? Misra has not explained such cases and has provided only some *ad hoc* rules to show the closeness of IE and Skt. -- However, all these developments have been explained by IE linguistics, for more than a century, in a coherent way (IE = Latin *e, o, a* > Skt. *a*; IE > vowel *n* > Skt. a, Lat. *in*, etc.).

BCE. That guess is not any better than the various guesses for PIE, at 3000 or 4500 BCE. Misra's whole system rests on guesswork and on demonstrably faulty reconstructions.

It simply is uncontested among linguists of any persuasion that the remarkable *grammatically regular* features of Proto-IE (underlying, e.g., the differences in the present tense formation of Sanskrit, German, French *asti, ist, est :: santi, sind, sont,* < IE *$^*h_1$és-ti :: $^*h_1$s-ónti*) are part and parcel of the parent language, the original PIE. This was at first confined to an unknown area in a temperate (not a tropical!) climate.[83] This scenario is in stark contrast to the certainty with which autochthonist place the homeland of IE inside South Asia or even inside certain parts of India (Misra 1992), even more precisely in the Gangetic basin (Talageri 1993, 2000), not exactly unexpectedly,[84] in their own home land, India. (For this familiar 'principle' used in deciding the *Urheimat*, see Witzel 2000, and below).

On the other hand, the autochthonous school maintains that the very assumptions at the basis of the genealogical, family tree model of the Indo-European language family, deride it (cf. Elst 1999: 119, see discussion by Bryant 1999), or contest it *just* for the Indian linguistic area (see below). This is quite old news: various models have been proposed and tested for the development from Proto-Indo-European to the individual languages: the "family tree" model (A. Schleicher's *Stammbaumtheorie*, 1861-2), a theory of dialectal waves of innovation emanating from a certain center (Joh. Schmidt's *Wellentheorie*, 1872). Further, socio-linguistic theories include the development of Proto-Indo-European as a sort of camp language (another *Urdu*, so to speak), a new Pidgin, based on diverse original languages that eventually spread beyond its own rather limited boundaries, for example with the introduction of horse-based pastoralism (Anthony and Vinogradov 1995, Kuz'mina 1994, etc.).

Some advocates of the autochthonous theory (Kak 1994, Talageri 1993, 2000, Elst 1999: 159) use rather simplistic linguistic models, such as the suggestion that population increase, trade, the emergence of agriculture,[85] and large-scale political integration led to the extinction of certain languages and to a transfer of other languages across ethnic groups. However, all such factors have been considered over the past two hundred years or so; none of them, in isolation, nor a combination of all of them, lead to the surprising spread of Indo-

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[83] Archaeologists have proposed as area of the domestication of the horse and the (later) development of the horse drawn chariot, in the Ukraine and the plains west and east of the Urals. From there, a trail of evidence leads to Pirak (c. 1700 BC), the Swat valley (c. 1400 BC), -- and, of course, to the RV (textual evidence, see §8).

[84] The unspoken "principle" of locating the (IE) homeland: "the homeland is *at,* or *close to* the homeland of the author of the book in question..." (Witzel, 2000). -- Talageri claims to have based his study of the RV only on RV materials, but introduces late Vedic and Purāṇic concepts (see below §12.2, Witzel 2001); not surprisingly, then, the outcome is a Gangetic homeland.

[85] Elst 1999: 159 sq. stresses, like many other indigenists, that "India was the best place on earth for food production" and that "a generous country like India must have had a large population," both unsubstantiated articles of faith. Note that the Indus Valley has only gradually been settled, from the Baluchi/Afghani hills, and that the Gangetic plain remained very sparsely settled for much longer. (Cf. also the negative description of the Panjab by E. Iranians, in Vīdēvdād, see n. 52). Elst's imaginative description is compounded by repeating the nationalistic view that "the ancient Hindus colonized the world". But India, by and large, always has been a *cul de sac*. Otherwise, autochthonists wonder why a 'large population' could take over IA language(s) brought in by a few tribes. A few comparisons across history would have provided many and diverse examples. For the dominance model: Norman French introduced by a few knights and their followers in Anglo-Saxon England, or for a trade language: Swahili, starting out from the coast and by now covering most of E. Africa and the eastern half of the Congo (incidentally, mostly spreading without Islamization).

European languages inside and outside the subcontinent. In fact, most of the factors just mentioned were *not* present during the early Vedic period which saw the introduction and spread of IA all over the Greater Panjab.

Autochthonists further neglect that language replacement, such as visible during the Vedic period, depends on a range of various socio-linguistic factors and not simply on the presence of nomads, increasing population density, etc. Rather, the situation differs from case to case, and the important factors for any particular replacement must be demonstrated. For example, Renfrew's (1987) model of a very gradual spread of IE from Anatolia, along with agriculture, has not generally been accepted. If this agriculturally induced spread had taken place, I would be writing this paper in a descendent language of the non-IE Hattic of Turkey, and not in IE English. In the case of early India, the change from the language(s) of the urbanized Indus civilization to that of the pastoralist Indo-Aryans must be explained. It certainly cannot be done (see below) by positioning the homeland of the 'non-tropical' IE language inside India (Talageri 1993, 2000, Elst 1999: 118 sqq.) and make its speakers emigrate, across the Indus area, towards Iran and Europe.

§12.2 Language and 'Out of India' theories

Theoretically, a scenario of IE emigration from the Panjab is of course possible, --- the direction of the spread of languages and linguistic innovations cannot *easily* be determined, unless we have written materials (preferably inscriptions). However, some linguistic observations such as the distribution of languages, dialect features, substrate languages, linguistic palaeontology, etc. allow to argue against the Out of India scenarios.

The Out of India theorists such as Elst (1999:122, 124 etc.), Talageri (1993, 2000) envision an IE homeland in South Asia, to be more precise, in the Gangetic basin. Talageri simply assumes, without any linguistic (or archaeological, palaeontological) sources and proof, that in "prehistoric times the distribution of the languages in India may have been roughly the same as it is today: viz. the Dravidian languages being spoken in the south, Austric in the east, the Andamanese languages in the Andaman Islands, the Burushaski language in N. Kashmir, Sino-Tibetan languages in the Himalayan and far eastern border areas, and the Indo-European languages certainly in more or less their present habitat in most of northern India" (1993: 407). The rest follows logically: ..."a major part of the Indo-Europeans of southeastern Uttar Pradesh migrated to the west and settled down in the northwestern areas --- Punjab, Kashmir and the further north-west, where they differentiated into three groups: the Pūrus (in the Punjab), the Anus (in Kashmir) and the Druhyus (in northwestern and Afghanistan)", (cf. Talageri 1993: 196, 212, 334, 344-5, 2000: 328, 263).[86] Of course, all of this is based on data about peoples "clearly mentioned and described in the Puranas." Needless

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[86] It must be pointed out that all of this is based on one misrepresented passage from several Purāṇas, given by Talageri 1993: 368 and 2000: 260 sq., typically, twice in *untranslated* form, which makes it easy to impute any meaning desired, in case a "first historical emigration ... of the Druhyu into the areas to the north of Afghanistan (ie. into Central Asia and beyond)." The passage is found with some variants, at Brahmāṇḍa 2.74.11, Brahma 13.152, Harivaṃśa 1841, Matsya 48.9, Vāyu 99.11, cf. also Viṣṇu 4.17.5, Bhāgavata 9.23.15, see Kirfel 1927: 522: *Pracetasaḥ putraśataṃ rājānaḥ sarva eva te // Mleccharāṣṭrādhipāḥ sarve udīcīm diśam āśritāḥ*, which means, of course, *not* that these '100' kings conquered the 'northern countries' way beyond the Hindukush or Himalayas, but that all these 100 kings, sons of Pracetās (a descendant of a 'Druhyu'), kings of Mleccha kingdoms, are 'adjacent' (*āśrita*) to the 'northern direction,' -- which since the Vedas and Pāṇini has signified Greater Gandhāra. -- Elst (1999: 122) even weaves in the disputed Bangani evidence (Witzel 1999 a,b) that point to a western (*centum*) IE remnant in the Himachal Pradesh Hills, like that of Tocharian in Xinjiang, W. China.

to say, this kind of writing prehistory smacks of early 19th cent. writing of early European and Near Eastern history according to the Bible and Herodotos, before the ancient Egyptian and Mesopotamian texts could be read. It is based on a *naive* reliance on texts that were composed millennia after the facts, and that are the product of a lively Bardic tradition (L. Rocher 1986, Brockington 1998, Parry 1971, Lord 1991), influenced by Brahmanical redactors (Söhnen 1986, Horsch 1966). In spite of what Pargiter (1913) and even Morton Smith (1973) have tried to establish --obviously, without taking the later investigations into account-- we cannot write the history of archaic and ancient India based on the legendary and late Epic and Purāṇic accounts of the middle ages (Witzel 1990, 1995, 2001).

Talageri (1993: 407) continues his Purāṇic tale as follows: "... major sections of Anus ... developed into the various Iranian cultures. The Druhyus spread out into Europe in two installments." He actually *knows*, somehow, which IE group moved first and which later, and by which route: "the speakers of the proto-Germanic dialect first migrated northwards and then westwards, and then later the speakers of the proto-Hellenic and proto-Italo-Celtic dialects moved into Europe by a different, more southern, route. It is possible that the speakers of proto-Baltic and proto-Slavonic (or proto-Balto-Slavonic) ... of proto-Illyrian and proto-Thraco-Phrygian ... were Anus and not Druhyus, the Anus and Druhyus thus being, respectively, the speakers of proto- Satem and proto-Kentum." (1993: 407-8).

Or slightly differently (2000: 263): "The two emigrations ... from an original homeland in India: ... The first series of migrations, of the Druhyus, took place.... with major sections of Druhyus migrating northwards from Afghanistan into Central Asia in different waves. From Central Asia many Druhyu tribes, in the course of time, migrated westwards, reaching as far as western Europe. These migrations must have included the ancestors of the following branches... a. Hittite. b. Tocharian. c. Italic. d. Celtic. e. Germanic. f. Baltic. g. Slavonic.

 .... The second series of migrations of Anus and Druhyus, took place much later, in the Early Period of the Rigveda, with various tribes migrating westwards from the Punjab into Afghanistan, many later on migrating further westwards as far as West Asia and southwestern Europe. These migrations must have included the ancestors of the following branches (which are mentioned in the Dāśarājña battle hymns): a. Iranian. b. Thraco-Phrygian (Armenian). c. Illyrian (Albanian). d. Hellenic."

The strange or outdated terminology (Slavonic, etc. -- his source may be Misra's diction, see below -- Italo-Celtic, Kentum) indicates the limited linguistic background of the author sufficiently enough. Nevertheless, we also can learn of the solution to the long-standing enigma of the Indus language (Parpola 1994, Witzel 1999 a,b): "The Indus Valley culture was a mixed culture of Pūrus and Anus" (1993: 408). Nothing less, perhaps, could be expected, as the book is self-described as: "This whole description is based on the most logical and in many respects the *only possible*, interpretation of the facts... Any further research, and any new material discovered on the subject, can only confirm this description... there is no possible way in which the location of the Original Homeland in the interior of northern India, so faithfully recorded in the Puranas and confirmed in the Rigveda, can ever be disproved" (1993: 408). Luckily for us, the author names his two main sources: the Purāṇas and the Ṛgveda. The reliability of Purāṇic and Epic sources is discussed below (§19, Witzel 1990, 1995), and the RV does not support his theory either (it simply does not know of, or refer to central and eastern Northern India).[87]

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[87] Talageri achieves such evidence by twisting the facts his way: see the discussion of Jahnāvī, n. 90, Witzel 2001.

§ 12.3. Emigration

In order to achieve his new U.P. homeland, Talageri has not only to *rely* on the Purāṇas, he also has to *read them into* his RV evidence, though *pretending* to use only the RV to interpret the RV (Talageri 2000) -- in fact one of the basic requirements of philology (Witzel 1995, 1997). *In casu,* the single two appearances of *Jahnāvī* in the RV at 1.116.19 and 3.56.6 are made out to refer to the Ganges. However, both passages clearly refer to a Jahnāvī which translators and commentators (including Sāyaṇa) have taken as a tribal designation (cf., indeed, such an 'ancestral goddess' next to Hotrā, Bhāratī, Iḍā and Sarasvatī at RV 2.1.11, etc.). It is, thus, by no means clear that Jahnāvī refers to a river, and certainly not to the Ganges in particular (Witzel 2001). That is an Epic/Purāṇic conceit. Instead, it can simply be derived from the Jahnu clan. Yet, it is in this way that Talageri tries to strengthen his case for a Gangetic homeland: the Ganges is otherwise only mentioned twice in the RV, once in a late hymn directly (10.75.5), and once by a derived word, *gāṅgya* (6.45.31, in a *tṛca* that could be an even later addition to this *additional* hymn, which is too long to fit the order of the arrangement of the RV, see Oldenberg 1888). However, nothing in the RV points to knowledge of the Gangetic basin, or even of the lower Doāb. The medieval and modern Doāb rivers Sarayu and Gomatī[88] have sometimes been mentioned but the context of these RV rivers is one of the western hills and mountains, in Afghanistan.[89] Talageri's identification of Jahnāvī with Gaṅgā is clearly based on post-Vedic identifications;[90] the RV passages only speak about an ancient clan (deity) which could have 'settled' anywhere.[91]

The evidence set forth by Talageri is not conclusive even for the tribes of the RV, -- in fact the location of the Yadu-Turvaśa, Anu-Druhyu and Pūru is not very clear for most of the Ṛgvedic period (Macdonell & Keith 1912).[92] One hardly does have to mention the features that would not agree with a 'tropical' PIE language in the Gangetic Basin (see § 12.6). As a curiosity, it might be added, however, that we certainly would expect tribal names such as Druhyu (or Anu) in Europe, -- just as the Gypsies have carried their tribal/caste name *Ḍomba* to Europe, where they still call themselves *Roma*. However, we do not find any IE tribe or people in Europe derived from Ved. *druh* / IE **dhreugh*: there are no tribes called, e.g., German *Trug, Be-trüger,* Engl. **Tray, Be-trayer* -- we only find spirits: 'ghost' and 'apparition' (Pokorny 1959: 276).

In passing, it should be mentioned that the Epic and Purāṇic accounts of the western neighbors of India are based on a view, already found in ŚB and BŚS 18.13: 357.6 sqq,

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[88] Sarayu, then was not yet the mod. Sarju in U.P.; Gomatī, that in PB 25.7.2 is already located in Vibhinduka land, i.e. is the modern Gumti in U.P., Witzel 1987:193.

[89] RV 5.53.9, the mythical river at the end of the world or high up in the Himalayas, the *Rasā* /Avest. *Raṇhā,* and the *Kubhā* (Kabul R.), *Krumu* (Kurram), *Sarayu* (Herat R.); and 10.64.9: *Sarasvatī* (=*Haraẋaitī,* Helmand), *Sarayu* (Herat R.), *Sindhu* (Indus); (see Witzel 1987, 1995, 1999; note that both lists are probably ordered anti-clockwise, Witzel, 2000).

[90] Note Mbh 1.3722 etc., son of Ajamīḍha, his daughter = Gaṅgā; -- Jāhnāvī Mbh 3.8211; Jāhnava PB 22.12; cf. Jahnu's descendants at AB 7.18, ĀśvŚS 12.14, = 'Gaṅgā' at BhGītā 10.31, Viṣṇu Pur. 398; cf. Macdonell-Keith, *Vedic Index.*

[91] Note that the center of settlement in RV 3 is the eastern Panjab and the Sarasvatī area of Haryana, see Witzel 1995: 320.

[92] For example, settlement in Kashmir by any Ṛgvedic tribe is very doubtful, see Witzel 1994; in the later Brāhmaṇa period, Uttara-Madra (however, not Uttara-Kuru) *may* refer to Kashmir .

18.44:397.8 sqq, that regards all tribes and peoples outside the Center, the Kuru(-Pañcāla) realm, as 'outsiders' (*bāhīka* ŚB 1.7.3.8, *udantya, mleccha, asurya*). They are characterized by their 'incorrect' speech and obnoxious behavior (ŚB 9.3.1.24, Panjabis) and lack of proper *śrauta* ritual (ŚB 13.5.4.19, Kāśi).

Consequently, both the Panjabis (*Bāhīka*) as well as the Benares (*Kāśi*) and S. Bihar (*Aṅga*) people are denigrated by middle Vedic texts.[93] This attitude mellowed somewhat with regard to eastern North India (AB 7.18 where the Andhra, Puṇḍra, Śabara, Pulinda, etc. are included as Viśvāmitra's sons, Witzel 1997) but it continued with respect to the west which was under constant and continuing threat of immigration, incursion and actual invasion from the Afghan highlands (cf. Rau 1957: 14). In fact, the Panjabis have been regarded as outsiders since the AV and ŚB and Patañjali's Mahābhāṣya has preserved the oldest "Sikh joke", *gaur bāhīkaḥ* 'the Panjabi is an ox'. There is nothing new under the Indian sun.

There is, on the other hand, nothing particularly Indian about this attitude, it is reflected not only in Manu's concept of *madhyadeśa* (> mod. Nepali *mades* 'lowlands'), but also in ancient and modern China (*chung kuo*, 'the middle land'), and elsewhere. Ritual, world wide, often regards one's own location as the center of the universe (or its navel/eye, *o mata o te henua*, in Polynesian).

The Epic and Purāṇic accounts simply build on such Vedic precedents: the Panjabis are regarded as 'fallen Ārya', or in the words of BŚS, the Gandhāri have emigrated [from the center].[94] This is "the view from the center", Kurukṣetra, a view that was not yet present in Ṛgvedic times.[95] All of this is, incidentally, another indication of the (post-Ṛg)Vedic attitude against 'outsiders', the Other. To regard the alleged, actually mistranslated Purāṇic story (contra Witzel 2001, cf. n. 42, 86) about an emigration from India as statement of *facts* is as far-fetched and mythological as the Roman insistence of their descent from the heroes of Troy (Virgil's Aeneid, see above §9), or as the many tales about the lost tribes of Israel (note that the Pashtos, in spite of the E. Iranian language and pre-Muslim IIr culture, claimed to be one of them). It is completely anachronistic, and in fact unscientific, to use such legends, concocted long after the fact, as indications of actual historical events. (The Gypsies, who actually have emigrated from India, rather claim origins in S. Iraq or Egypt).

§12.4. Linguistics and 'Emigration'.

In addition, Talageri's new book merely restates, with the addition of Epic-Purāṇic legends, what S.S. Misra had written before him in 1992, just as so much of present autochthonous writing is nothing more than a cottage industry exploitation of a now popular trend. Misra's small book[96] of 110 pages, however, is a curious collection of linguistic data spanning the Eurasian continent, from Tamil to Uralic (Finno-Ugric), and from IE, Vedic and Mitanni Indo-Aryan to European Gypsy (Romani). All of this with an equally curious

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[93] Witzel 1987,1989, 1997. However, the "north", Gandhāra and Uttara-Madra, (Uttara-Kuru?) are always excluded, see Witzel 1989: 101.

[94] See discussion in §9, Nirukta, Patañjali and the Kamboja language.

[95] But see above §9 on the Sarasvatī as political center in Sudās' time.

[96] The following account was written before I heard, at the beginning of Oct. 2000, of the author's demise. I am sorry that he can no longer reply to the following points. However, as his book has been quoted in virtually every publication propagating the autochthonous point of view, it is important to point out the facts which remain, even if *de mortuis nihil nisi bene.*

conclusion: "the original home of the Proto-Indo-European speech community... was searched in Pamir, Caspian Sea etc. in spite of the fact that the most original and orthodox Indo-European speech, Sanskrit, was spoken in India.... The following ground may be assumed for dropping India. This was a nice place to live. People would not like to go to places like Europe... On the other hand, there is definite evidence of spread of Aryans (or Indo-Europeans) in different parts of Europe... A brief sketch may be.... The Greeks were invaders and came to Greece from outside... there was a vast substratum of pre-Greek languages... the Celtic people came from outside to Europe... That the Italic peoples were invaders is well-known... before the Hittite invasion to the area [Turkey] it was peopled by another tribe called Hattic... the Hittite speakers might have gone there in very early days from an original home (which was perhaps India)... The Slavonic people ... were invaders... at the expense of Finno-Ugrian and Baltic languages... The Germanic speaking Indo-Europeans... coming from an outside world... the movement of Iranians from India to Iran... The Finno-Ugrian contact with Indo-Aryans speaks of the movement of Vedic Aryans from India to that area. Therefore it is likely that Pre-Vedic Aryans also might have gone out of India in several waves. The migrations from India to the outside world might have taken the following order: The Centum speakers... in several waves... Out of Satəm speakers, Armenian first, the Albanian, next Baltic followed by Slavonic. The Iranian people were the last to leave... based on the linguistic analysis or relative affinity with Sanskrit. Similarly out of the Centum groups Greek might have left India last of all." (Misra 1992: 100 sqq.) A lot of invasions into and all over Europe -- quite politically incorrect now, it might be added, -- but no "invasion", not even an 'immigration' or a meager "trickling" *into* India.

There is no need to belabor Misra's wording, such as 'orthodox' (which language is 'orthodox'?), strange from the pen of a linguist. However, Misra's main thesis, emigration *from* India, has already been refuted, on linguistic grounds, by Hock (1999, see below) and I can be relatively brief here; however, many ingredients and conclusions of Misra's book are faulty as well. Since he is now quoted by OIT advocates as *the* major linguistic authority who has provided proof for the OIT, these must be discussed and summed up.

§ 12.5 Finno-Ugric data

Misra maintains (1992: 94) "the borrowed elements in the Uralic languages show borrowed Ṛgvedic forms in 5000 BC." Unfortunately, his discussion is based on two wrong premises: Harmatta's list of IA/Iranian loans in Uralic[97] and Misra's own 'unorthodox' but faulty reinterpretation of IIr and IA data.

To begin with, the date given by Misra to the RV "must be beyond 5000 BC" (1992) is based on the *guess* of Finno-Ugric scholars for Proto-FU, a date just as good or bad as any given for PIE at 4500 or 3500 BCE. What is of greater importance here is the exact form of IIr. that the various loan words in PFU have preserved. In addition to Harmatta, some other scholars, not mentioned by Misra, have worked on this problem as well, most recently Joki 1973, Rédei 1986, Katz (Habilschrift 1985).

Unfortunately, Harmatta has chosen to divide his materials into eleven stages, ranging from 4500 - 1000 BCE, with an arbitrary length for each period of 300 years. Worse, some of them have been placed at various unlikely dates within that time frame, e.g., the development

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[97] Reprinted in Harmatta 1992: 360-367. Harmatta actually is an historian who, nevertheless, is called by Misra "one of the leading Indo-Europeanists." His paper has been used by many indigenists who cannot judge these linguistic materials.

is > iš, which is already E. IE (Slavic, IIr, etc.) has been placed at 2000 BCE (as *iś*!), that is 600 later than the related changes *rs > rš, ks > kš,* and the same development *appears again* as PIIr *iś > iš* at 1700 BCE. However, it is on this arrangement that Misra based his conclusions. Though he corrects some of Harmatta's mistakes (such as misclassifying IIr forms as PIran.), Misra makes things worse due to his clearly faulty, 19th cent. type reconstruction of IE (see Hock 1999): "most of the loan words ... are in fact to be traced to Indo-Aryan. Of special importance is the borrowing traced to the earliest period (5000 BCE), which is clearly Vedic Sanskrit" (1992: 24). This refers to words such as Harmatta's FU *aja* 'to drive, to hunt', *porc'as, porśas* 'piglet', *oc'tara* 'whip', *c'aka* 'goat', *erśe* 'male', *reśme* 'strap', *mekše* 'honey bee', *mete* 'honey' (from Harmatta's stages 1-7). *Most* of these are actually pre-IA as they retain *c'* > Ved. *ś*, or *š* instead of Ved. *ṣ*, or the IE vowels *e, o* instead of Common IIr and Ved. *a*.[98] His use of Harmatta's list and that quoted from Burrow (1973: 23-27) and Abaev (1992: 27-32) suffer from the same methodological fault: forms that easily can be derived from IIr, such as Mordw. *purtsos, purts* (reflecting IIr *parc'as* [part^{s}as]) are declared by Misra as having come from the much later OIA (Vedic), in spite of their retaining the old pronunciation *c'* [t^s]; this is, in fact, still found in Nuristani, e.g. *du.c.* [dut^s], < PIIr *dac'a* < PIE *dek'm,* but not in the *linguistically already younger*, but historically speaking c. 3000 years older forms Ved. *daśa*, OIran. *dasa!* In short, this kind of combination produces a great, but confused and confusing scenario.

Most of the acceptable evidence derived from Harmatta's data[99] fall right into the Proto-IIr period. The shibboleth is the development of PIE labiovelars to velars: *k^w, k^wh, g^w, g^wh > k, kh, g, gh*, something that is clearly seen in PFU *werkas* 'wolf' < PIIr *vṛka-s* < PIE *wḷk^{w}o-s* (Misra, of course, takes this word as RV Sanskrit!). About the same time, the PIE *k', k'h, g', g'h* developed to *c', c'h, j', j'h*. This development is clearly seen in the *majority* of the loans into PFU, as in for example in *porc'as* 'piglet', *c'aka* 'goat', *aja* 'to drive'. (Misra derives these sounds from Skt. *c, j*, see Hock 1999). However, the PIIr affricates are represented in PFU in two forms, either as expected by *c'*, or in the younger (=Vedic) form, by *ś*[100] (late PIIr, not yet OIran. *s*, and *ś* preserved in Vedic).

Some confusion is raised by the various representations of PIIr *a* by PFU *e, ä, o, a*. This could, again, point to the pre-PIIr period when the differences between *e, o, a* as inherited from PIE were still preserved. In fact, *-o-* in these loan-words seems to be limited to *initial* syllables, while other syllables have *-a-* or *-e-*. The problem will be treated at length elsewhere (Witzel, forthc. b)[101].

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[98] Misra, of course, denies the development IE *e, o, a* > IIr, Ved. *a*; this reversal to early 19th cent. linguistics is refuted by Hock 1999.

[99] Harmatta's list has no clear examples that date back to PIE. One may discuss PFU *mete* 'honey' < PIE *medhu*, but the quality of the PFU vowels preserved in these words is open to doubt (see below). Further, the retaining of *-w-* in PFU *arwa* 'present given to a guest' surprises as PIE *org^{w}ha-* should have lost its labiovelar quality already by the time the word turned into IIr *argha*. Note, however, that Mayrhofer, EWA 114, regards the PFU form as problematic (from *arγa?*, Finn. *arvo*); Katz's Habilschrift was not available to me.

[100] Parpola 1998, however, conflates the two stages and further conflates them with the representation of IE *e/o/a* by FU *e, o, a, ä* etc.

[101] These facts should be counterchecked by FU specialists who may be able to explain this phenomenon by vowel harmony or by the peculiarities of PFU stress.

The important result is, quite differently from that of Misra's Sanskrit-like loans into PFU, the following: it was at the stage of PIIr (perhaps even at that of late PIE) but certainly not that of Ṛgvedic Sanskrit, that PFU has taken over a substantial number of loan words ranging from plants and animals to customs, religion and the economy.[102]

§ 12.6. Dating of RV

The last section has, of course, serious consequences for Misra's new dating of the RV, at 5000 BCE, which is anyhow impossible due to internal contradictions (relating to the horse, chariot, etc., see below). As the PFU loan-words point to pre-Ṛgvedic, PIIr. and even some (pre-)PIIr. forms, the RV must be considerable *later* than the reconstructed PFU (at 5000 BCE). All of which fits in well with the 'traditional' date for this text, in the 2nd mill. BCE, roughly contemporary with Hittite, Mitanni IA, and early, Mycenean Greek texts inscribed on tablets.

§ 12.7. Mitanni data

Misra's use of the Mitanni Indo-Aryan materials is clearly faulty as well. They *seem* to fit in well (at dates around 1400 BCE) with his theory of an early RV at 5000 BCE because he regards some of the Mitanni words as representing post-Vedic, Middle Indo-Aryan developments. He assumes (repeated faithfully by Elst 1999:183) that there is MIA assimilation of clusters in Mit. *satta* < Ved. *sapta* 'seven' (see n. 148), or replacement of *v-* by *b-* as in *biriya-* < Ved. *vīrya* (rather, to be read as *priya-,* see EWA I 139). However, such forms are due to the exigencies of cuneiform writing and Hurrite pronunciation found in the Mitanni realm (for details, see below §18). In sum, Misra's data are based on his insufficient knowledge of near Eastern languages and their writing systems.

However, it can even be shown that Mitanni IA words belong to a *pre-Ṛgvedic* stage of IA as they have retained *-zdh-* > RV *edh* and *ai* > RV *e*, and even IIr. *j'h* > Ved. *h* (see below §15, 18). Thus, Misra's early "Middle Indo-Aryan" at 1400 BCE simply evaporates, along with his early RV at 5000 BCE.[103] We are back to the 'traditional' dates.

§ 12.8. Gypsy language

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[102] Conversely, there is apparently little FU in IE. Such one-sided relationships, however, are not uncommon as they follow the predominant cultural flow. The reason for the early occurrence of word for bee (**mekše*) and honey (IE **medhu*) may lie elsewhere, in the usefulness of bee's wax to produce *cire perdue* metal products, which seem to be earlier in the Taiga woodlands than in the steppes and even further south. In other words, we here have a reverse cultural flow, from the woodlands into the steppes. -- It must be pointed out that the few words in PFU that still retain the nom. sg. masc. -*s*, such as *tarwas, martas, taivas, porc'as, werkas* (and, including the case of *pakas* 'god', with a typical, much later, Iran. semantic development from IIr Bhaga-s, the (god) "Share", see below) do *not* point to an earlier take-over than that of other words without -*s*. For, there are words such as the presumably very early **arwa* 'present', **jewä*, or **mekše*, where this has *not* taken place. -- However, the typical Iran. change *s* > *h* is not yet seen in Harmatta's material, and it may indeed be fairly late (c. 1000 BCE, see A. Hintze 1998). In short, some of the late words in the list may be of North Iranian (Scythian/Saka) origin. -- For connections between IE and Altaic, see A. Róna-Tas 1988.

[103] This is not to say that even the RV has a few forms, such as the -disputed- *jyotiṣ* < *dyotiṣ* (aan de Wiel 2000); however Mitanni does not have any such developments, see below (§18).

Though a detailed study of data from the Gypsy (Romany) language seems to be beyond the scope of the present discussion, some words are necessary as Misra has used the example of Gypsy as support for his theory of sound changes that affected the hypothetical IE emigrants from India when they entered the Near East and Europe. No matter that the two movements, thousands of years apart, would refer to one of PIE and the other to an MIA or ealry NIA language, and no matter that Romany is not as well studied as PIE. While it is clear that "the Gypsy languages are of Indo-Aryan origin is no more controversial..." it is not correct to say that "the Gypsy dialects present sufficient evidence which shows that Indo-Aryan a changed into *a,e,o* in European Gypsy..." (Misra 1992)

First of all, the emigrant Gypsies, probably first attested as migrant musicians in records of the Sasanide kingdom of Iran (at 420 CE), have retained a fairly old form of IA which looks, often enough, like MIA, for example in the northwestern MIA retention of *Cr* (*bhrātā > phral* 'brother'), or the present tense of 'to do' (*karáv, karás, karál*, etc.) Misra hinted at the reason why certain cases of MIA *a* have changed into Eur. Romani *e,a,o* : their distribution seems to be based on occurrence of *-a-* in an originally open syllable (in MIA, OIA) whence > *e*, or in a non-open syllable whence > *a*. However, this change is by no means universal even in European Romani. Its archaic Balkan version (of Bulgaria, etc., which I know from personal experience) has *kar-, karáv* etc. 'to do' (from *karomi*, as quoted above). In short, Misra's data are again incomplete, faulty and misinterpreted.

Second, his contention that "Thus in a way the linguistic change in Gypsy, suggests a clear picture of an assumption for a similar change in Proto-Indo-European stage, of Indo-European *a* (as shown by Sanskrit and as reconstructed by Bopp, Sleicher [sic!] etc.) into dialectical *a, e, o* (as shown by Gk. etc.). Uptil now no evidence to the contrary is available that Proto-Indo-European *a, e, o* (as reconstructed by Brugmann etc.) have merged in India" (Misra 1992: 81) can easily be refuted by any Indo-Europeanist (Hock 1999). In Greek, for example, we do not have a 'dialectal' change, whatever that may mean, of Misra's IE $*a > e, a, o$ but a clearly regulated one, in the case of laryngeals 1-3 > *e, a, o* : IE $*h_1esti$ > Gr. *esti*, Lat. *est*, but Ved. *asti*; h_2ner- > Greek *anēr,* Ved. *nṛ-*, $*h_2enti$ > Gr. *anti*, Lat. *ante*, Hitt. *ḥanti* (with *written* laryngeal!) but Ved. *anti*, $*h_3onkos$ > Gr. *onkos*, Lat. *uncus*, but Ved. *aṅku-ṣa* (Rix 1976: 68 sqq.). Not to speak of the well-established correspondences of PIE $*e, o, a$ in the various IE languages, which Misra simply denies on insufficient grounds (for details, see Hock 1999).

In sum, Misra's contention that "Gypsy languages show a repetition of the linguistic change, which occurred in a remote history of Indo-European, when the original groups, speakers of various historical languages, left their original homeland (India) and travelled to Europe... (1992: 82), ... the borrowed elements in the Uralic languages show borrowed Ṛgvedic forms in 5000 BC... the date of RV must be beyond 5000 BC..." (1992: 94) is based on insufficient materials, faulty interpretations and idiosyncratic conclusions that are at odds with anyone else's in the field.[104]

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[104] Some other topics of this nature will be taken up below (§13 sqq.) The following passage, however, does not need any comment: "In ancient times in India such *ṛṣis* were very powerful. They were great teachers, researchers, philosophers and scientists. If Agastya had some power he might have helped in bringing down the abnormal height of the Vindhya mountains which created a lack of contact of North and South. Thus a least this is much likely that due to some factor the height of the Vindhya mountains became abnormally high, so that the path for contact of North and South was blocked and due to the growth of population the people in the North had to spread, naturally farther North. They used the routes like the Khyber pass and left it and lost all contact and were finally lost to their people ... as a result the Aryans had to go outside to North-West through the Himalayan

§12.9 Contra: IE dialect clusters

Returning to the question of an IE homeland inside India, we can easily observe where IE innovations seem to cluster, right from the time of the common PIE language. For example, the famous Satem innovations all are limited to the IE languages in the east of the IE settlement area, with the exception of the (western-type) Centum language Tocharian, which actually is the easternmost IE language, in China (Xinjiang; to which add the Bangani substrate). Clearly, the older Centum block has been split by the Satem innovations (not withstanding that the speakers of Tocharians might have moved further east after the split). Such clustering indicates that Indo-Iranian is a southeastern extension of eastern (Satem) IE and that Vedic is the easternmost one of these. For a recent summary, see H.H. Hock (1986: 452, 1999). From this, as well as from a number of earlier studies, it is obvious that the 'dialectal features' in the arrangements of (P)IE languages indicate a general expansion of IE westwards and eastwards from an unknown center, somewhere close to the geographical center of the pre-colonial expansion of IE languages over Siberia, the Americas, etc.

The actual spread of IE across Eurasia points in the same direction. It has been well observed in various parts of the world that a settlement close to each other of related languages indicates their original habitat while a (geographically) wide spread of one of a (sub)family points to recent expansion. One can observe this with Bantu which covers all of Central, East and South Africa while its parent group, Niger-Congo, has a very dense arrangement of diverse languages in West Africa.[105] Or, even more recently, the large array of English dialects in England, and the very few but large variants outside England (N. America, Australia, etc.) clearly point to England as the place of origin.

In the case of IE, the application of this principle would indicate an original settlement of the ancestor language somewhere in (S)E. Europe; it must not be overlooked, however, that many early IE languages have disappeared since (Thracian, Dacian in the Balkans, Hittite, Luwian, etc. in Anatolia, and probably some languages in S. Russia/Ukraine as well, areas that were subsequently settled by Scythians and other (Turkic) steppe peoples, and finally by Slavs. The center *may* therefore have been situated somewhere between Greek, Hittite, Armenian in the South and Slavic, Iranian (Scythian, Saka, etc.) in the north, in other words, in the Greater Ukraine. This area is also at the fault line between the western Centum and eastern Satem languages and of certain syntactic features of IE (Hock 1999: 15).

All such observations make an Indian homeland of PIE a priori unlikely. Hock (1999) has adduced further reasons why this cannot be the case: all dialectal differences in PIE would have been exported, at various periods, and would exactly have reconstituted themselves geographically, all over Europe and the Near East, in the *same* geographical relationship as

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passes and this consequently was responsible for the spread of Indo-European language family to the outside world." (Misra 1992: 70) Is this linguistics, prehistory, a 'scientific' Mahā-Bhārata? Or just a reverse version of O. Rosenberg's *Myth of the Twentieth Century*?

[105] The same applies to Austronesian, with a very close grouping in Taiwan (and then in S.E. Asia), but subsequently, with the wider spread of just one subfamily, Polynesian, all across the Pacific. -- The center of Slavic languages would be in or near the northern Carpathian mountains, indeed close to the actual homeland of the Slavic speaking tribes. That of all Romance languages would lead to central Italy, in other words, to Rome. -- Elst 1999: 126 sq. points, as 'proof' for his Indian *Urheimat* of IE, to some asymmetric expansions which are found as well, as in the (easily explainable) case of Australia, with Arnhemland as the center and with the rest of the continent as the area of a more recent expansion.

originally found in the hypothetical Indian homeland. This certainly needs very special pleading, and simply falls prey to Occam's razor.[106]

§ 12.10 Other 'Out of India' theories: *Sprachbund*

Another new and equally misleading linguistic scenario has recently been created by writers such as Aiyar (1975), Waradpande (1993) and scientists such as S. Kak (1994a), or always on the internet, S. Kalyanaraman (1999). They contend that two of the major language families of South Asia, Indo-Aryan (i.e. IE) and Dravidian are not (very) different from each other. Both would rather represent two forms of an old South Asian Proto-language, which they call, variously, a Prākṛt or just the Indian Bronze Age language.

Again, the idea is not exactly new. A fore-runner is, quite unexpectedly and already at the beginning of the past century, Aurobindo[107] (cf. Talageri 2000). With the then usual conflation of outward appearance or 'race', ethnicity, and language (note: Hirt 1907), he found that his native people, the Bengalis, and the inhabitants of his new home, Ponchicherry (where he went into exile, evading the British), were not so different after all.

More recently, some Indian scholars have expressed the (ultimately correct) feeling of an All-Indian *cultural* unity in terms of *language* as well (Aurobindo, etc., cf. Bryant 1999). Swaminatha Aiyar's analyses (1975, quoted, with approval by Misra 1992: 73-78, and adopted) of common features between Aryan and Dravidian are a case in point:

> "...from a linguistic point of view also, Dravidian is more comparable to Indo-Aryan than to any other language family in the world... But Dravidian may be the first to have been separated and went north. Next the centum people separated and left through the Himalayan passes to Caspian or Pamir and then to Europe etc. The satem speakers left after that, batch by batch. The last batch might have been the Iranians."

The first part of the quote confuses descent (genetic relationship) of languages with secondary mutual influences of neighboring languages (S. Asian linguistic region, *Sprachbund*).

The very idea of a "pan-Indian Prākṛt" is, of course, a *contradictio in se*. As any beginner in linguistics should know, *Prākṛt* always refers to an Indo-Aryan language, Middle Indo-Aryan to be precise. The designation 'common South Asian Proto-language' or, worse, "Prākṛt", when used for Archaic Tamil, is imaginary and confusing, just as a Dravidian Proto-Vedic, P-Hindi, or a Mundic P-Bengali would be.

The issue at hand is whether there ever was such a thing as a common S. Asian or Indian "Prakrit". Kalyanaraman, Kak (1994a), or Misra (1992) simply (or handily) confuse

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[106] Elst (1999) includes a long chapter on links of IE with other language families, with a curious mixture of correct and incorrect data; wrong are, e.g., p. 141: Ved. *paraśu* 'axe' is not the same as Mesop. *pilakku* 'spindle' (see EWA II: 87); on p. 145 there is the linguistically surprising statement that, because Drav. and Munda are *attested later* than Vedic, there is no reason to assume a borrowing from these languages into Vedic, -- as if they did not have Proto-forms. -- Elst pays special attention to links with Austronesian (p. 152 sqq.) as this would push the *Urheimat* of IE into S. Asia, or even into S. China and S.E. Asia; this is followed by a curious speculation of a Manu who would have led the Indo-Europeans upstream on the Ganges towards the Panjab, ending with (p. 157) "India as a major demographic growth centre from which IE spread to the north and west and Austronesian to the southeast as far as Polynesia". The only redeeming feature here is that he concludes (p. 158) "it is too early to say that linguistics has proven an Indian origin for the IE family."

[107] Aurobindo felt that not only the people but also the original connection between the Sanskrit and Tamil tongues to be far closer and more extensive than is usually supposed and that they may have been two divergent families derived from one "lost primitive tongue".

the relatively new concept of a South Asian linguistic area *(Sprachbund)* with the 'genetic' relationship of the languages involved.

This idea was developed early in the 20th century when linguists where surprised that several disparate languages in the Balkans shared so many features. These include Rumanian, Macedonian, Bulgarian, Serbian, Greek and Albanian. Now, these are all Indo-European languages and thus have the same starting point, though Bulgarian has an old Turkish (Bulgar, different from modern Turkish) and an IE Thracian substrate. But they come from four quite different sub-families: Rumanian from the Western IE Vulgar Latin, Bulgarian, Macedonian and Serbian from the Eastern IE Southern Slavic, Greek from the Western IE Old Greek, and finally Albanian from the vague Illyrian/Dalmatian (etc.) subfamily. As such, they are much more different from each other than even modern Iranian and Indo-Aryan.

However, they have stayed together for a long time, and have had intermingled settlements (Albanian near Athens, Rumanian-type Romance speech in Bulgaria, etc.) for 1500-2000 years. Consequently, bilingual speakers have influenced each other considerably, especially in syntax and by mutual loan words. Yet, there still is no "new Balkan language" or a "Balkan language family" in sight. The basic vocabulary of these 6 languages still is very different and most of their grammatical formantia as well.

The same applies to S. Asia, where the idea of a linguistic area was pioneered by Emeneau (1956), Kuiper (1967). But here, the starting point is *unlike* that of the Balkans: S. Asia has at least 3 different large language families:[108] IE, Drav., Munda, which have nothing in common, neither in basic vocabulary nor in word structure nor in grammatical formantia. The situation is not unlike that in modern Europe, with Uralic (Finnish, Estonian, Hungarian, etc.), Basque, Altaic (mod. Turkish), and the rest (= IE). For details on the South Asian *Sprachbund* or linguistic area or convergence area, it is useful to consult Hock (1986: 491-512) though it is largely devoted to syntax. It is clear that, over the past few millennia, the three language families of S. Asia have converged to a large degree, including phonetics (retroflexes, see §15), word formation (Munda changed from a monosyllabic language with prefixes into a polysyllabic one working with suffixes) and syntax (spread of absolutives, see Tikkanen 1987, or sentence structure preferring SOV arrangements, see Hock 1986).

The spread of such convergent items has been taken by some (Kak 1994) as a sign that the various S. Asian languages are underway to form a new language family. This is overstating the matter by not just a little margin. It has not happened in the Balkans. Or, English, with its large share of Romance (French) vocabulary and some grammatical features (calques such as *more beautiful : plus beaux*), has not joined the circle of Romance languages, nor have French and Anglo-Saxon, or the other converging (Western) European languages coalesced into a new "(Western) European" family.

As has been mentioned above, the proponents of a 'common' South Asian Proto-language / 'Prākṛt' and a "new S. Asian language family *in statu nascendi*" confuse the outcome of a long stay together and original "genetic descent". Tamil speakers do not use Hindi words in their *basic* vocabulary, nor do Bengali speakers use Santali words, nor Kashmiri speakers Burushaski words, nor Nepali speakers Tibetan words, and *vice versa*. And, the various grammars involved still are far apart from each other, in spite of all the converge features evoked above. To state things differently simply is bad linguistics and special pleading, as already seen several times in the case of the Out-of-India theorists.

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[108] Nostratic, or Greenberg's just off the press *Eur-Asiatic,* are another matter, but even these new theories still do not turn Drav. and IE into Meso-/Neolithic neighbors *inside* India.

§ 12.11. Emigration and linguistic features

In order to approach and evaluate place and time of the *hypothetical* (OIT) Indo-European home in South Asia (or that of the even less likely common S. Asian Proto-language) and of the *hypothetical* emigration of the Iranian and other IE speakers from India, one has to look for terms that are *old* in PIE. For example, PIE **gwou-* 'cow', **dyeu-* 'heaven', and their archaic acc. forms **gwōm, *dyēm*, with PIE dissimilation of -*w*-, should have existed already in a hypothetical IE Panjab. However, these PIE forms are reflected in the various old IE languages (with their subsequent individual phonetic innovations): Ved. *gām* 'cow', Hom. Grk. *boun/bōn*,Ved. *dyām* 'heaven', Grk. *zēn;* etc. (EWA I 479, 752). In any autochthonous theory, this archaic dissimilation would either be due to pre-split PIE dialects *inside* India (refuted by Hock 1999, above) or to a subsequent *individual* development of the *same* traits outside India, after the IE languages would have left the subcontinent. Such an a priori unlikely scenario, however, is rendered altogether impossible as the subsequent eastern (Satem) developments (g^w- > *g* in 'cow') are restricted to a dialect *continuum* of eastern IE (where a dissimilation **gōum > *gōm* was no longer possible). Other such unique Satem and IIr cases involve **kw > k, *k' > c'*, then, **ke > *cæ > ca;* the change **e > *æ* is early in IIr. as it is seen in the *cakāra, jagāma* type palatalization, as well as that of **o > ā* in *Brugmann* cases (cf. Hock 1999); finally IIr. **æ* > Ved./Avest. *a*.   Clearly, several long term developments are involved. Just like the supposed (OIT) individual innovations in *dyām* and *gām,* such eastern IE developments (Hock 1986: 451 sq.) would have to be *re-imports* from their focus in E. Europe/Central Asia into India, -- all convoluted cases of very special pleading.

The first traces of IE languages are attested with Hittite around 2000/1600 BCE in Anatolia, Mycenean Greek at c. 1400/1200 in Crete, Mitanni-IA. in N. Iraq at 1380 BCE. All PIE and IIr terms and forms must precede this date by a large margin as even archaic languages such as Vedic and Hittite are separated from each other by many innovative developments. The date of the dispersal of the earliest, W. IE languages (including Tocharian, eastwards) must be early in the 3rd mill. BCE or *still* earlier.

But, in the autochthonous scenario of an emigration out of India, the Centum languages (Celtic, Germanic, Latin, Greek, etc.), then the Satem languages (Slavic, etc.), would have followed each other by a time span of at least a few hundred years, and Iranian would have been the last to emigrate from India as it is closest to Vedic; it should have left well before c. 1000 BCE, when W. Iranian is first found on the eastern borders of Mesopotamia.[109]

These dates allow to set the claims of the autochthonous school (Talageri 1993, 2000) into a distinct relief, especially when such early dates as 5000 BCE (based on a loan word link with Finno-Ugrian) are claimed for the RV (S.S. Misra 1992). While this is impossible on text-internal, cultural grounds, their *hypothetical old* RV would have the comparatively *modern* form of Old Indo-Aryan that would, nevertheless, *precede* that of the very archaic Hittite by a margin of some 3000 years. We know, of course, that Vedic is not *earlier* than Hittite but clearly *later*, i.e. lower in the cladistic scheme that is popularly called the 'family tree': it is *later* than Eastern IE (Satem innovations, RUKI, cf. Hock 1986, 1999),   *later* than Proto-Indo-Iranian (*e, a > æ, k' > c', o > ā* in open syllables, with *o > a* in all other syllables), and even *later than* Pre-Vedic (*c' > ś,* or *zd(h)* and *j'* > Ved. *h,* which still preserved as š [ž]

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[109] However, Iranian has some *pre*-RV features, while it misses all Indian innovations, all of which makes a late emigration impossible, see §17.

< *j'h* in Mitanni IA at 1400 BCE, see below §18). In short, all of the above indicates that neither time nor space would agree with a OIT scenario.

Another major obstacle against the emigration theory is that even the closest relative of Vedic, the *hypothetical emigrant* Old Iranian language, misses all Indo-Aryan innovations (see below §13-17). Any argument militating against this must use the special pleading that all Vedic innovations happened only *after* the emigration of the Iranians out of India; this is, however, impossible in cases such as *rāṭ/rāj-, ṣoḍaśa, voḍhar-, sede* and others such as the absolutive.

In other words, Misra's scheme (and that of all others who assume such early dates for the RV and an IE emigration out of India, such as Talageri 1993, 2000, Elst 1999) are not only badly deliberated but are plainly impossible: PIE, while still in the Panjab, would *not* yet have developed all the traits found in non-OIA languages (Satem etc.), while their close neighbor, the 'old' RV, would *already* have gone through all Satem, IIr, Pre-Vedic and RV innovations 7000 years ago, -- an unlikely scenario, to say the least. And, as such,[110] Ṛgvedic OIA would have exercised early influences on the rather distant Uralic languages in S. Russia/Urals/W. Siberia, while the non-IA neighbors of Uralic (Iranian, Baltic, etc.) would *not*. All of this is obviously impossible on grounds of space and time. Misra *et al.* have not thought through their idiosyncratic and ad hoc scenarios.[111] To do so is not *our* job, but that of the proponent(s) of the new theory. They should have done their homework.

§12.12. Emigration and culture

The matter can still further be elucidated by observing some cultural features: according to the autochthonous theories the various IE peoples ("Anu, Druhyu" of Talageri 1993, 2000) and their languages hypothetically left India (around 5000/4000 BCE). If put to a test by archaeology and linguistics, these 'emigrations' would *rather* have to be set at the following latest possible dates.[112]

3000/2500	W.IE leave	while possessing:
		ayas 'copper/bronze' > Lat. *aes* 'copper, bronze', etc.; but:
		no chariot yet: Lat. *rota* 'wheel', Grk. *kuklo-* 'wheel', Toch. *kukäl, kokale* 'wagon', etc.; note Grk. *new* formation *hárma(t)-* 'chariot' (Pokorny 1959: 58);
		yet, all parts of the heavy, solid wheel wagon are IE: *akṣa, ara nābha* 'nave'; Germ. *Rad*/Lat. *rota,*

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[110] Which, *pace* Misra, point to loans made during the Indo-Iranian and Iranian periods, not in the Vedic period, see above.

[111] In fact, most of the autochthonists have not even started to learn the linguistic 'trade', and simply reject linguistics out of hand, as mentioned above.

[112] Note that the following list can be read both in the new, autochthonous/indigenous way, that is of leaving India, or in the 'traditional' IE way, of leaving a S.E. European/C. Asian homeland.

		drawn by oxen (*ukṣan*); -- domesticated horse *$h_1ek'wo$ > Lat. *equus,* O.Ir. *ech,* Toch. *yuk, yakwe,* used for riding
2500/2000	E. IE leave	have *satem* characteristics (*$h_1ek'wo$*, O.Lith. *ašvà*), but still no chariots: Lith. *ratas* 'wheel, circle'
by 2000	IIr. unity	new : *ratha* > 'chariot' from Volga/Ural/N.Caucasus area; and *cakra* 'wheel, chariot' -- but how and when did it (and the domesticated horse) enter India? Innovative *Āditya* gods with artificial formations (*Arya-man* = Avest. *Airiia-man*, etc.)
1500/1000	Iran. move	with chariot, Ādityas, but keep old grammar, ntr. pl. + sg. verb, etc.
c. 1000	W. Iranians	are attested on the eastern borders of Mesopotamia

According to this list, again, all Vedic linguistic innovations (with the RV set at 5000/4000 BCE), and some E. Indo-European ones such as the IIr. chariot, would have happened *before* the supposed emigration of the Iranians from India! This is archaeologically impossible, unless one uses the auxiliary, equally unlikely hypothesis that some IIr.s left India before 2000 BCE and *reimported* the chariot into India (Elst 1999). All such arguments need very special pleading. Occam's Razor applies.

§ 12.13. Emigration & nature

While, *theoretically again*, a scenario of IE emigration from the Panjab is possible, this claim, too, contradicts all we know about IE material culture (e.g., horse, wagon, and the late chariot) and climate-based vocabulary (willow, birch, fir, oak, snow, wolf, beaver, salmon, etc.), all of which traditionally have been used to indicate a temperate IE homeland with cold winters, somewhere in E. Europe-C. Asia, (Geiger 1871: 133 sqq., Schrader 1890: 271, Hirt 1907: 622, Friedrich 1970, Mallory 1989: 114 sqq.), -- that is, an area that included at least *some* (riverine?) tree cover.

Even if we take into account that the Panjab has cool winters with some frost and that the adjoining Afghani and Himalayan mountains have a long winter season, the IE evidence does not bear out a South Asian or Indian homeland. The only true IE tree found in S. Asia is

the birch (*bhūrja*),[113] and some argument can be made for the willow ("willow" > Ved. *vetasa* 'cane, reed', see n.146), maybe the fir (*pītu*),[114] and the aspen (*varaṇa?*).[115] But why are all the other IE trees those of a colder climate non-existent in Indian texts, even when even the neighboring Iranians have some of them, e.g. in the eastern Afghani mountains (fir,[116] oak,[117] willow,[118] poplar[119])?

Or rather, to follow the autochthonous line: how did the IE tree names belonging to a cooler climate ever get out of India where these trees do *not* exist? One would have to use the auxiliary assumption that such trees were only found in the colder climate of the Himalayas and Pamirs, thus were part of the local South Asian vocabulary, and that they would then have been taken along, in the westward movement of the emigrants.

But, even this special pleading does not work: some of these temperate IE trees are *not* found in the S. Asian mountains. But, they still have good Iranian and IE names, *all* with proper IE word formation (see above). Interestingly, these words have not always been formed from the same stem, which reflects normal (P)IE linguistic variation and is not due to *completely* new, individual, local formation in one or the other IE language. Rather, the PIE variations in the name of the beech,[120] fir (and resin), and oak (see above) use the same roots and several of the available PIE suffixes. In other words, these cool climate, temperate trees and their names are already PIE.

If the indigenous theory of an emigration out of India would apply, these tree names should have taken *one* or *two* typical "Indian" PIE (dialect) forms and spread westwards, such

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[113] Only the birch tree is found all the way from India to Europe: *bhūrja* 'betula utilis' (KS+); note that the Indian birch differs slightly from the European one. We have: Iran. Pamir dial. *furz*, Shugni *vāwzn* < **barznī*; Osset. *bœrs(œ)*; Lith. *béržas*, Serbo-Croat. *brèza*; German *Birke*, Engl. *birch*, etc.

[114] The fir tree is found as Grk. *pítus*, Lat. *pīnus* <**pītsn-*, Skt. *pítu-dāru* KS+ 'a fir, Pinus deodora' (*pūtúdru* AV, *pū́tudru* TS+, *pūtudāru* KauśS), Dardic **pītsa*? 'fir' CDIAL 8236, EWA II 137. Note also the word for 'resin' which is closely related to trees such as the fir: Lat. *bitūmen*, OHG *quit* 'glue', Ved. (Sūtras) *játu* 'lac, rubber', N.Pers., *žād 'rubber'*, Pashto *žāšwla* 'resin' < IE **gᵂetu,* EWA I 565.

[115] Breton. *gwern* 'alder', Alban. *verə 'Populus alba'*, Armen. *geran* 'plank, board', *varaṇa* 'Crataeva Roxburghii', "unclear" EWA II 513; -- note also Thieme (1954: 16) *sphya* 'belonging to the asp tree', but cf. Pokorny 1959: 55, EWA II 779.

[116] The Kashmir Valley now has: deodar (Cedrus deodara), pine (*yar*, Pinus excelsa and *chīl*, Pinus longifolia), fir, yew (Taxus baccata), elm, cypress, plane tree (Platanus orientalis), poplar, lime tree, wild chestnut, willow, maple, hawthorn, many fruit trees, and at high altitudes: birch, alder, juniper and rhododendron. -- Note that none of the local words for these plants, except for the birch, exists west of the subcontinent, or in autochthonous parlance, was 'exported' westwards.

[117] Skt. *Parjanya*, Lith. *Perkúnas*, O. Slav. *Perunᵘ,* etc.

[118] Avest. *vaēti*, OHG *wīda*, Grk. *itéa*, Lat. *vitex*, Lith. *žil-vitis*; cf. also: OHG *felawa* 'willow', Grk. *helíkē*, Ossetic *färw, farwe* 'alder'.

[119] See above for 'aspen'.

[120] As for the distribution of the word, see Bartholomae 1898, Henning 1963, Lane 1967, summary by Cowgill 1986: 86 sq. Note the famous Greek adaptation of the word used for the temperate climate tree, the 'beech' > the mediterranean Grk. *phēgós* 'oak'; while Lat. *fagus* 'beech', Germ. *Buche*, OHG *buohha* > Slav. *buky*, and the *Bukovina* region retain the older meaning; contrast Russ. *bozᵘ* 'elder tree', Alban. *bunge*, Gr. *phēgós* > 'oak', and note that Kurd. *būz* 'elm' < **wyg* 'elm' is not derived from the 'beech' word. The word for 'beech' is not found in S.Asia, though the tree itself was historically found much further east during the *Atlanticum* than Thieme thought (1954: 16), that is further east than the famous 'beech line' (running from Königsberg to Odessa). Elst (1999: 130), while not mentioning the climatic factors, disposes of the beech argument wholesale.

as is the case with the two loans from Chinese, *chai* or *tea*. The opposite is the case. The individual IE languages have the *same* PIE word, or they have slightly innovated within the usual PIE parameters of ablaut and suffixes.

In short, whatever way one turns the evidence, all of the above points to some original IE tree names of the temperate zone exported southwards. Some of them therefore exhibit a change in meaning; others are an application of an old, temperate zone name to newly encountered plants, such as 'willow' > 'reed, cane'. Again, this change in meaning indicates the path of the migration, from the temperate zone *into* India.

If we carry out the countercheck, and search for Indian plant names in the west, such as lotus, bamboo, Indian trees (*aśvattha, bilva, jambu*, etc.), we come up with nothing. Such names are not to be found, also not in a new meaning, such as in a hypothetical case: *'fig tree' > *'large tree with hanging twigs', *'willow'.[121] The lack is significant as the opposite case, import into S. Asia, is indeed found. Again, this points to an *introduction* of the IA language into India, not an export *'Out of India'*.

The same kind of scenario is found with the typical PIE animals that belong to a temperate climate. While some of them such as the wolf or bear occur in South Asia as well, albeit in slightly different species (such as the S. Asian black bear), others are found, just as some of the tree names, only in new, adapted meanings. For example, the beaver is not found inside S. Asia. It occurs, however, even now in Central Asia, its bones have been found in areas as far south as N. Syria and in mummified form in Egypt, and it is attested in the Avesta (*baßri* < *babhri* < IE *bhebhr-*) when speaking of the dress ('made up of 30 beaver skins') of the Iranian counterpart of the river Goddess Sarasvatī, Arəduuī Sūrā Anāhitā: Yt 5.129 "the female beaver is most beautiful, as it is most furry: the beaver is a water animal" (*yaṯ asti baßriš sraēšta yaϑa yaṯ asti gaonō.təma, baßriš bauuaiti upāpō*).[122] Avestan *baßri-* is related to the descriptive term, IE *bhebhru* "brown, beaver" which is widely attested: O.Engl. *bebr, beofor*, Lat. *fiber*, Lith. *bēbrus*, Russ. *bobr, bebr-* (Pokorny 1959: 136). The respective word in Vedic, *babhru(-ka)*, however, means 'brown, mongoose' (Nenninger 1993). While the mongoose is not a water animal, some Indian types of mongooses vaguely look like a beaver, and clearly, the IE/IIr term for 'beaver' has been used, inside South Asia, to designate the newly encountered animal, the mongoose. This occurs today in the subcontinent, but in Greater Iran only in its southeastern-most corner, in Baluchistan. Interestingly, N.Pers. *bebr* < Phl. *bawrak*, Avest. *baßri* 'beaver' is a cat-like, tail-less animal whose skins are used (Horn 1893: 42); the beaver, though previously attested as far south as Syria and Egypt, is no(t longer) found in Iran; note also N.Pers. *bibar* 'mouse'.

The opposite direction of the spread of the word, 'out of India', is not likely as it is not Ved. *babhru* (or Avest. *baßri*) that spread westwards (following S.S. Misra 1992) but their original (and traditional) IE source, *bhebhru*. Such a hypothetical export would again have to suppose subsequent individual sound changes that mysteriously result in the various attested IE forms that cannot occur if one starts from Ved. *babhru*. It is unlikely, thus, that the original word, *bhebhru* signified the mongoose.[123] Other S. Asian animal names are not

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[121] The only exception from this evidence are certain later cultural loans, plants such as 'cotton' or 'mustard.'

[122] Differently, Oettinger, Habilschrift 1983 (unpubl.).

[123] For Elst (1999: 130,132) this is not a problem as he lets the IE first settle in India and name the mongoose a 'brown one.' Then, when emigrating westward, *each* IE language would mysteriously have transferred this designation *individually* to the beaver, and always in the *later,* post-PIE form, as per individual subfamily or

'exported' either. Occam's razor applies: all things being equal, it is easier to assume import into S.Asia, along with the other animal names of the temperate zone.

The case of the salmon may be added and briefly discussed in this context. It has often been used to define the original homeland of the Indo-Europeans, into the Fifties of the 20th century, by taking the *present* distribution of the salmon for granted (rivers flowing into the Baltic, Polar Sea, Thieme 1951).[124] However, another type of salmon is also found in the rivers flowing into the Caspian Sea. The word in question is attested in Osset. *läsäg* 'salmon' (*Salmo trutta caspius*, perhaps a kind of trout), Russian *losos*[u], Lith. *lašiša, lãšis*, Germ. *Lachs,* Toch. B. *laks* 'fish', Iran. **raxša* 'dark colored' > N.Pers. *raxš* 'red-white', Ved. *lākṣā* 'lacquer, red resin'. Again, the direction from 'salmon' > 'fish', > 'red-colored/lacquer' is more likely than the opposite one, (especially when we also include Thieme's suggestion that Ved. *lakṣa* 'wager' (in the dicing game using 150 nuts) is derived from 'salmon swarm', note also Class. Skt. *lakṣa* '100,000', see (EWA II 472, 477, EWA III, 83, 96-97, Pokorny 1959: 653).

All such evidence is not favorable for an emigration scenario. Rather, Occam's razor applies, again: PIE has a number of temperate/cold climate plants and animals which never existed in South Asia but which can be reconstructed for all/most of PIE; their names follow IE rules of word formation (root structure, suffixes etc.) and exhibit the typical formational possibilities of IE (ablaut, exchange of various suffixes). A few of them that designate flora and fauna actually occurring inside S. Asia have been retained in Vedic (wolf, birch, etc.), others have gained new meanings suitable for the animals or plants of a tropical climate ('willow' > 'reed', 'beaver' > 'mongoose').

Interestingly, the autochthonous counter-argument[125] relating to *tropical* plants and animals does not work either. If we suppose a South Asian homeland of PIE, we should be able to indicate at least *a few* terms that have been exported (north)westwards. This is not the case. Designations for typical Indian plants and animals that should be found in Indo-European and especially in Iranian, do not even appear in Iran, not to speak of C. Asia or Europe. Words such as those for animals, plants, and trees just do not make it westwards.[126] Nor do we find retained names for newly encountered plants/animals, although at least some of them are actually still *found* in Iran: the lion (see Old Pers. sculptures at Behistun, Iran. *šer* (Horn 1893: 178); the tiger, Iran. *bebr* (Horn 1983: 42) that is still found in the Elburz and Kopeh Dagh and as late as the Seventies around the Aral Lake; the lotus (again seen on Behistun sculptures), etc. Other words that have occasionally been used for the autochthonous argument, such as *kapi* 'monkey', *siṃha* 'lion' or *ibha* 'elephant' are rather dubious cases.[127]

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language in question. Occam applies. Derivation of the 'beaver' words from Skt. *babhru* is of course linguistically impossible.

[124] Bartholomae, *Indogermanische Forschungen* 9, 1888, 272, Eilers & Mayrhofer 1962, Henning 1963, Lane 1967, see summary by Cowgill 1986: 68.

[125] Elst (1999: 129 sqq.), simply denies the possibility of IE linguistic palaeontology and quotes an outspoken, always skeptic S. Zimmer (1990) as his crown witness. It is precipitous to dismiss carefully applied linguistic paleontology completely (which according to Zimmer is "approaching its inevitable end -- with a negative result, of course"); cf. n. 81.

[126] Excluded are, of course, the real exports from India such as rice, cotton, beryl, etc., see Witzel 1999a,b.

[127] They have been employed, by Ivanov-Gramkrelidze (1984, I 443), with a completely different result, as proof that the IE homeland was in Anatolia/Armenia. However, the irregular correspondences seen in *kapi* : Engl. *ape*; *i-bha : ele-phant-;* or *līs : leon,* etc. are typical for loan words, not for original, inherited PIE vocabulary. Cf. Elst 1999: 131 sq., who even uses words such as *pṛdāku* 'panther' which clearly are loans (Witzel 1999 a,b). The attested

Ved. *ibha* (RV) does not even seem to indicate 'elephant' but 'household of a chief' (details in EWA I 194); *i-bha* 'elephant' is attested only in Epic/Class. Skt. (EWA III 28), and the combination with Grk. *elé-pha(nt-),* Lat. *ebur,* Gothic *ulbandus* 'camel' suffers from lack of proper sound correspondences. The word for monkey, Ved. *kapi,* is represented in Europe by another form which is not directly related by regular sound correspondences either: Grk. *kēbos, kēpos,* (cf. also Hebr. *qōf,* Akkad. *uqūpu, iqūpu, aqūpu,* Coptic *sapi,* O. Egypt. *gfj*) :: Germanic *apan-, aban* > Engl. *ape* with an unexplained loss of initial *k-*. The change in initial consonant is typical for transmissions of *loan words* from an unknown source, and cannot be used as proof of an original PIE word *kāp/kap*.[128] Similar relationships are seen in the word for 'apple': Celt. *abal-,* O.Ir. *ubul,* Crimean Gothic *apel,* OHG *apful,* O.Norse *apal-dr,* Lith. *óbuolas,* etc., O.Ch.Slav. *abl^{u}ko,* including Basque, Caucasus and Bur. relations (Berger 1959).

Finally, it must be considered that, generally, the IE plants and animals are those of the temperate climate and include the otter, beaver, wolf, bear, lynx, elk, red deer, hare, hedgehog, mouse; birch, willow, elm, ash, oak, (by and large, also the beech[129]); juniper, poplar, apple, maple, alder, hazel, nut, linden, hornbeam, and cherry (Mallory 1989: 114-116). Some of them are found in South Asia, and their designations have been used for the local form of the animal or plant (such bear *ṛkṣa,* wolf *vṛka,* otter *udra,* birch *bhūrja,* etc.) But most of them are *not* found in India and their designations have either been adapted (as is the case with the beaver > mongoose *babhru*), or they have simply *not* been used any longer.

According to the autochthonous theory, these non-Indian plant and animal names would have to be new words that were coined only when the various IE tribes had already emigrated out of India. However, all of them are proper IE names, with IE roots and suffixes and with proper IE word formation. It would require extra-ordinary special pleading to assume that they all were created independently by the emigrant IE tribes, at different times, on different paths, but always from the *same* IE roots and (often) with the *same* suffixes: how could these 'emigrants' know or remember exactly which roots/suffixes to choose on encountering a new plant or animal? Rather, as usual by now with all such arguments, Occam's razor applies, and the opposite assumption carries: IE words of the flora and fauna of the temperate zone were adapted to a tropical climate wherever possible. We see immigration into, instead of emigration 'out of India'.

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use of *pṛdāku* for 'panther' *and* 'snake' as indicating closeness to the original designation is not only linguistically impossible (loanwords!) but also cognitively light-weight: animals similar in appearance (spots!) are named by the same word. Classical Sanskrit is full of them. The argument that some animal names in Skt. still are etymologically transparent can also be made for those of the "Druhyu emigrants", the Engl. *bear,* Dutch *bruin,* etc. -- Even *matsya* 'fish' is derived by Elst from *mad* 'wet' (EWA II 298 "hardly likely"), in spite of Avestan *massia,* Pers. *māhī* < IIr *matya*; it belongs, according to Mayrhofer EWA II, 1986: 298, not to a word for 'wet', but to *mad(a)s* 'food'. All of this demonstrates Elst's lack of linguistic sophistication. Just as (other parts of) his books, even such seemingly straightforward sections have to be checked and re-checked.

[128] Elst (1999: 131), taking his cue from Gamkrelidze and Ivanov 1984 (= 1995), takes these shaky etymologies for granted and concludes that IE came from a tropical area. He adds (199: 131-2) a few very unlikely comparisons on his own: Latin *leo(n)* from Skt. *rav* 'ho howl', *mayūra* 'peacock' from *mā* to bleat, *gaja* 'elephant from *garj* 'to trumpet'; *pṛdāku* (cf. Witzel 1999) which designates both panther and panther snake (note, Lubotsky 1999, lecture at the 2nd Ved. workshop at Kyoto) as referring to *primordial* formations in IE -- as if animal designations were not easily transferred!

[129] See summary by Cowgill 1986:86 sq.

In the sequel, some of the individual linguistic proposals of the 'Out of India' theory, and the sometimes rather technical arguments that speak for and against it will be discussed.

§13. Absence of Indian influences in Indo-Iranian

When compared to Eastern IE or to the rest of IE, Avestan and Old Persian share many innovations with Vedic, which was the initial reason to set up this group of languages as a separate branch of IE, IIr. Just as in biology (taxonomy, the human pedigree, genetics, etc.) or in manuscript study (setting up of a stemma), the occurrence of common innovations *always* indicates that the innovative group has split off from the core group, and obviously is to be dated *later* than the core.

For example, Vedic *ah-am* "I"= Avestan *az-əm*, *az-ə̄m* O.Pers. *ad-am* have added the additional morpheme IIr. *-am* (as in *ay-am, iy-am*); it was transferred to the rest of the pronouns: *tvam, vayam, yūyam* as well. This feature is not found in other IE languages: Lat., Greek *egō*, Gothic *ik* (Engl. *I*), O.Slavic *az^u, jaz^u*; it clearly separates IIr. from the other E. and W. IE languages.

While Iranian, at first sight, seems to be more innovative than OIA in its phonology (*s > h, kh > x*; *p, t, k* + consonant *> f, ϑ, x* + cons., etc.), it frequently is also more archaic than Vedic. It lacks the many innovations that characterize Vedic, for example the absolutives in *-tvā, -ya*, ntr. pl. in *-āni*, the perf. *jaga-u*, or the normalization in *g-* of the present stems beginning in *j/g-*: IE $g^w m$-*sk'e-ti* > IIr. **ja-šca-ti* > Avest. *jasaⁱti* :: Vedic *gacchati*. (Note that *j* is retained only in traditional names such as *Jamad-agni* and in the perfect, *ja-gām-a*, etc.) Importantly, Iranian misses the generalization of the already Ṛgvedic *e*-perfects, derived from IIr. **sazdai* (Avest. *hazde*) > Vedic *sede* with many analogical formations such as *mene*. Since sound changes are not random and develop in linear fashion, these innovations must have occurred well after Vedic had separated from late IIr./pre-Iranian, thus : IE --> E. IE -> IIr --> Vedic, or Iranian.

The advocates of the autochthonous theory, however, would have the Vedic innovations occur in the Panjab only *after* the Iranian speakers had left the subcontinent, while retaining some very archaic features. (Talageri 2000, against all linguistic evidence, even denies close relationship of both groups). Some other innovations found both in India and Iran would have occurred earlier than that while both groups still lived in the Panjab; still others (found in E. IE, such as in Slavic) would have occurred at a still earlier, third level, again in the Panjab, while languages of the fourth level (including Greek, Latin, Germanic, etc.) would have left the subcontinent even before this.

While all of this is possible in a purely *theoretical* scenario, there are a number of arguments that render it impossible. Some of them have been listed by Hock (1999, see above). Others include such items as the temperate, non-tropical core vocabulary of IE, early IE loans from Semitic somewhere in the Near East (***wVjn-*, IE **woin-* 'wine', cf. J. Nichols 1997: 143), or on a more typological level, the intermediate position of IE between the Uralic and Kartvelian (W. Caucasian) language families (Nichols 1997, 1998). As far as the Satem language IIr is concerned, one can add the early close links of IIr (and, later, early Iranian) with Uralic in S. Russia and in the Ural and W. Siberian regions, and the new terminology coined for the horse-drawn chariot (*ratha/raϑa*), first introduced in the S. Russia/Ural area. This list, which could be extended, clearly points to the areas north of the Near East, and

strongly militates against the assumption of an Indian homeland of OIA, IIr, and, worse, of IE (see below).

How can the autochthonous theory then deal with archaisms found in Iranian that are *not* found in Vedic? Such archaisms ought to have been preserved in Vedic; they must have been forgotten (just like the tree names mentioned above) all over the subcontinent when the Iranians *supposedly* left it. Such collective amnesia, and in addition, one restricted just to certain *archaic* items does not make for a good case. It is, again, one of very special pleading.

It should also be mentioned in passing, that if the Iranians emigrated from India, why we do not find ''Indian bones'' of this massive emigration in Iran and beyond? Indian skeletons are, as Kennedy informs us (1995), remarkably different from Near Eastern ones.[130] Again, indigenists would have to argue that only that section of the Panjab population left westwards which had basically 'non-Indian' physical characteristics, very special pleading indeed. To adopt an OIT stance precisely mirroring the Indo-Aryan immigration theory based on 'trickling in' is not possible as this 'trickling out' would comprise *all subfamilies* of IE, from Tocharian to Celtic, and would constitute a much more massive emigration.

The IE theory can explain the materials found in the various languages much more satisfactorily: the Iranian languages simply miss the *Indianization* of IE, just as the very conservative Old Icelandic or Lithuanian escaped the 'Christianization' and 'Europeanization' for a long time.

§14. Date of Indo-Aryan innovations

As has been mentioned, the linguistic innovations of Vedic Sanskrit are supposed by autochthonists to have taken place only *after* the Iranians (and other Indo-Europeans) had left the subcontinent (Elst 1999: 122,124 sqq). It is difficult to argue against this kind of assumption on general linguistic grounds as language changes cannot easily be tied to certain areas, unless there is evidence from inscriptions and clearly localizable texts. However, the distribution of IE dialect features mentioned above (Hock 1999) makes IE innovations *after* an Iranian/IE exodus from India unlikely;[131] for, even though the old Satem innovations include Vedic, they exclude Latin, Greek, Tocharian, etc.

Further, a good indicator is found in IE plant and animal names ("willow", etc.) and especially in the word for the horse drawn chariot, Sanskrit *ratha*, O.Iran. *raϑa*. This word is attested in the oldest IIr texts, in the RV and in the Avesta, also with the secondary formation Ved. *rathin-*, O.Av. *raϑī* 'the one who has a chariot, charioteer'. Even more tellingly, it appears

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[130] Small, transient and migrating bands and groups such as the Indo-Aryans or even the larger ones such as the Huns are not easily traced; and, will we ever will find archaeological traces of the well attested emigration of a small group such as that of the Gypsies? -- Linguistics (see above, n.23) and genetics, however, clinch the case: the Bulgarian Gypsies, for example, have the typical Indian mtDNA (M type) and Y chromosomes but are only to some 30% Indian; for the rest they have acquired European genes. This is the exact reversal of the general Indian situation, with some 25% of W./C. Asian genes (§7). -- Autochthonists will have a hard time to explain how these Indian emigrants 'selected' their genes on emigration from India, and 'export' only the 30% proper Indian ones... In short, the same impossible scenario as in the *assumed* earlier 'export' of Indian linguistic features westwards by the IE = "Druhyu" emigrants (see above, §12.2).

[131] Elst had not seen this paper by the time he wrote his 1999 book; he supplies a lot of completely unsubstantiated speculation instead, of how the Indo-Europeans could have left the subcontinent to settle in Central Asia and Europe, (see 1999: 126 sq.).

in the inherited, archaic compound, with a locative case ending in its first member, RV *rathe-ṣṭha*, Avest. *raϑaē-šta-* 'charioteer' (cf. also *savyeṣṭha* 'warrior').

As the autochthonous theory would have the RV at c. 5000 or, according to some, before the start of the Indus civilization at 2600 BCE, the Iranians or other Indo-Europeans should have *exported* the chariot from S. Asia at that time. But the chariot is first found in a rather archaic form ('proto-chariot'), betraying its origin in a ox-drawn wagon (*anas, *weg'h-o- > wagon, veh-icle*), at c. 2000 BCE, in Russia and at Sintashta, W. and E. of the Urals. As its invention is comparatively late, the western IE languages retain, not surprisingly, the older meaning of the IE word, **roth₂o-*"wheel" (Lat. *rota*, Germ. *Rad* 'wheel'); they simply have moved away, before this development took place, from the original central IE region (such as the Ukraine) westwards into Europe.[132]

The indigenist counter-argument could maintain that the newly introduced chariot spread quickly from the Near East or Central Asia all over the Iranian and Indian world, with its IIr name, **ratha*. It would thus belong only to a secondary historical level (after that of the earlier "Panjab Indo-Europeans"). This argument, however, would run into a number of difficulties: for, strangely, the word in its new meaning of 'chariot' never reached the neighboring Proto-Slavic tribes, nor the other European 'emigrants' (Grk. has *hárma/harmatos*, Latin *currus, curriculum, rota*) on the western side of Eurasia while it is known to the close neighbors, the (Northern) Iranians. Worse, the word and the object are found already in the RV (*supposedly* a text of pre-Indus age, 2600 or c. 5000 BCE!), well *before* its invention.[133] In short, multiple insurmountable contradictions emerge.

The word *cakra* 'wheel' may be a much older adaptation from Sumerian *gil-gul* 'wheel' and GIŠ*gígir* 'wagon,' to IE **kʷe-kʷl-o-* > IIr. *cakra* (or, it is derived from a common origin, Littauer and Crouwel 1996). However, the newly specialized meaning *ratha* "chariot" is restricted to IIr.; its archaeological attestation puts PIIr, again, close to the Urals. -- On the other hand, there are common PIE words for the cart or four-wheeled wagon (*anas*) and its constituent parts, such as and *akṣa* 'axle', *ara* 'spoke, pin', *nabhya* 'nave', *yuga* 'yoke', *raśmi*, *raśanā* 'reins', etc.; for details see EWA, s.v. They are much older, PIE, as they refer to the more primitive technology of solid wheel wagons and carts that was developed in Mesopotamian in the late 4th millennium.

In sum, if according to the autochthonous theory, the Iranians had emigrated westwards well before the RV (2600/5000 BCE), how could both the Indians (in the Panjab) and Iranians (from the Ukraine to Xinjiang) have a common word for the horse drawn chariot as well as a rather ancient word for the charioteer? Both words must have been present at the time of the Indo-Iranian parent language. As the linguistic evidence shows, the technical innovation was already Indo-Iranian (note Proto-IIr. **th* that regularly developed to > Ir. *ϑ*, as in OIran. *raϑa*), and it must have happened at the place of its invention, in the plains near the IIr. River *Rasā* (Volga), certainly not in the Panjab.

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[132] Change of meaning "wheel(s)" > "chariot" (*pars pro toto*) is a common occurrence in linguistic experience.

[133] There have been efforts, always on the internet, to push back the dates of chariots and spoked wheels (also implied by Talageri's 2000 years of composition for the RV, see Witzel 2001), to dilute the difference between chariots and carts/ four-wheeled wagons, to find horses all over India well before the accepted date of c. 1700 BCE; there even has been the truly asinine proposition to change the meaning of Skt. *aśva* 'horse' (*Equus caballus*)and to include under this word the ass/donkey (*gardabha, rāsabha, khara*, etc., *Equus asinus*) and the half-ass (*Equus hemionus khur*). Here as elsewhere, it is useless to enter a discussion, as such views are based, all too often, on lack of expertise in the very subjects such sites proffer to discuss. On the internet, everyone is his/her own 'expert'.

Consequently, the occurrence of *ratha/raϑa* in IIr. at c. 2000 BCE shows that its *import* was carried out, along with many other IIr. items of culture and religion, from the S. Russian/Central Asian steppes *into* the subcontinent, and not vice versa. *This is one of the few clear cases where we can align linguistic innovation with innovation in material culture, poetics and myth, and even with archaeological and historical[134] attestation.* Therefore, we have to take it very seriously. Anyone of the various revisionist or autochthonous dating schemes that circumvent this innovation in technology and language dealing with the horse drawn, spoke-wheeled chariot at c. 2000 BCE is doomed to failure.

Other (theoretically) possible scenarios such as an import, along with that of the horse (see below), from some (N.) Iranians near the Urals into the area of the Indo-Aryans who had *supposedly* remained stationary in the Panjab, run counter to the archaic formation of the words concerned (*rathesṭha, savyesṭha*) and the clearly secondary, inherited form in Iranian (*raϑa-*), and would amount, again, to very special pleading.

Likewise, the many linguistic archaisms in Old Iranian cannot readily be explained by a supposed Iranian emigration from India. The Old Avestan of Zaraϑuštra frequently is more archaic than the RV and therefore too archaic to have moved out of India *after* the composition of the RV (*supposedly*, before 2600/5000 BCE). For example, the Avestan combination within a sentence of neuter plural nouns with the singular of the verb is hardly retained even in the other older IE languages. Conversely, something not found in Iranian, i.e. the Ṛgvedic perfect forms *jabhāra or mene,* are a local IA *innovation*. All of this points to separation of Proto-Iran. and Proto-OIA at some time *before* the RV. Also, it cannot have happened *inside* S. Asia as the Avesta lacks all those typically S. Asian words that are local loans into Vedic (§16; Witzel 1999a,b). Incidentally, the lack of S. Asian substrate words in Iranian (cf. Bryant 1999) also explains why the archaic Iranian traits cannot have been preserved in the Panjab, side by side with the RV, before the *supposed* Iranian move westwards.[135]

One can only conclude that Proto-Iranian (> Avestan, O.Persian) split off from IIr and thus, from pre-Old IA. (> Vedic, Mitanni IA, etc.) at an *early* date, and definitely so while spoken *outside* the Panjab. Because of the early split, Old Iranian preserved some archaic features, while also developing innovations on its own (Iran. *x* < IIr *kh, h* < *s,* etc.). In sum, Proto-Iranian never was spoken in the Panjab.

Or, to give another example, according to the autochthonous theory, Proto-Ir. would have to had to leave the Panjab before the Vedic dialects of the RV took over (or developed) the so-called retroflex (*mūrdhanya*) consonants.

§15. Absence of retroflexes in Iranian

While the feature of retroflexion (*ṭ, ṭh, ḍ, ḍh, ṣ, ṇ*) is sporadically found also in some other parts of the world (Hock 1986), such as in Scandinavia or Australia (innovative in both

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[134] See now however, M.A. Littauer and J.H. Crouwel 1996 for a Near Eastern origin.

[135] Any other scenario would amount to very special pleading, again: One can hardly maintain that the Vedic 'Panjabis' received these local loans only *after* the Iranians had left.

cases), it is typical for S. Asia when compared to its neighboring regions, that is Iran, West/Central Asia, the Himalayas, S.E. Asia.[136]

In the autochthonous scenarios discussed above, the hypothetical emigrants from India would have lost the S. Asian "bending back of their tongues" as soon as they crossed the Khyber or Bolan Passes: not even Old Iranian (East Iran. Avestan) has these sounds.[137] But, conversely, the Baluchi, who originally were a W. Iranian tribe, have acquired retroflexion -- just in *some* of their dialects -- only *after* their arrival on the borders on the subcontinent, early in the second millennium CE (Hoffmann 1941, cf. Hock 1996, Hamp 1996). The same happened to other late, incoming groups such as Parachi, Ormuri (from W. Iran) that are found in E. Afghanistan, and also to some local Iranian Pamir languages such as Wakhi. Clearly, retroflexion affects those *moving into* the E. Iranian borderland/Indus plain. Importantly, the most widespread appearance of retroflexes is among the cluster of Hindukush/Pamir languages, that is the languages surrounding these mountains in the east (Nuristani/Kafiri, Burushaski, Dardic and the rest of these northernmost IA languages) as well as in the north (some of the Iranian Pamir languages: Wakhi, Yigdha, Sanglechi, Ishkashmi, Khotanese Saka), as detailed by Tikkanen (in Parpola 1994: 166). Retroflexes *may* also have belonged to a *part* of the Central Asian/ Afghanistan substrate of the RV (Witzel 1999a,b). Retroflexion clearly is a northwestern regional feature that still is strongest and most varied in this area.

Had retroflexion indeed been present in the pre-Iranian or the Proto-Iranian coeval with the (Ṛg)Vedic period, its effects should be visible in Old Iranian, at least in Avestan[138] which was spoken in East Iran, that means in part on the territory of modern Pashto (which has retroflexes indeed).

Cases such as IIr *waj'h-tar* > Av. *vaštar,* but > Ved. *voḍhar-* are clear enough and present perhaps the best testimony for the several stages of conditioned reflexes in the development from IE to Vedic: a change from Ved. *voḍhar-* --> Avestan *vaštar-* is plainly impossible in any version of phonetics, as also *voḍhar-* --> IE *wek'h-tor-* (as in Latin *vec-tor*)*:* missing consonants as in *vo-ḍhar-* do not suddenly (re-)emerge out of the blue in other languages, and nota bene: *not* as a phonetically changed *-š-* in Iranian, as *-k-* in Latin, or as *-k-* in Gaulish *Vectur-ius,* or as *-g-* as in Engl. *wagon*; rather, with the IE theory, they all stem from < IE *weg'h-tor-* (neglected by Misra 1992).

The case of *voḍhar-* is pre-conditioned by the development of IE *k', g'* > IIr *c', j',* which changed to Proto-Iran. and Pre-Vedic *š, ž,* then to early Vedic retroflex *ṣ, .ž.,* which *only then* could influence the following consonant (of the *-tar* suffix), as to deliver the retroflex 'suffix' *-ḍhar-*. At this stage, the same retrograde Sandhi as seen in *budh+ta > buddha* took place (*.žh.-da > .ž.dha*), and *only then,* the voiced sibilant *.ž.* disappeared, normally (as in *lih*: *li.ž.ḍha > līḍha*) with compensatory lengthening of the preceding vowel; but, in the particular environment of *voḍhar* (*a.ž. > o*, just as *az > e*) represented by *o* + retroflex consonant (*-tar* suffix), in short:

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[136] The map in Parpola 1994 includes Tibetan, but this development is late, and typical for the Lhasa dialect. However, Khotanese Saka, just north of the Pamirs, has retroflexes.

[137] This has indeed happened to the Gypsies: in Turkey, N. Africa, Europe.

[138] Interestingly, the c. 1000 year old *Indian* Parsi pronunciation and recitation in Zoroastrian ritual(!) of Avestan, while clearly Indianizing, as in *xšaϑra > [kṣatra],* has not yet developed retroflexes.

IE *weg'^h + ter > IIr * vaj'^{h}tar-   > pre-Ved. *va.ž.ḍhar-[139] > Ved.   voḍhar-
                                               > pre-Iran. *vaštar-   > Avest. vaštar-

In sum, the well-known rules of IE sound changes explain the development from the root *vah*
(IE *$weg'h$) without problem, while an OIT theory would have great difficulty to get from
voḍhar- to any Avestan, Latin, English, etc., forms.

    Again, it is important to stress that retroflexes have not occurred in (Old) Iranian,
which has kept the older sound sequences. In addition, these changes allow a relative and even
an absolute dating: *aždh > oḍh* is parallel to *sazd- > sed*, i.e. both are post-Indo-Iranian and
even post-Mitanni; as pointed out above, Mitanni OIA keeps the sequence *azd*. In other
words, R̥gvedic is younger than the Mitanni words preserved at c. 1450-1350 BCE. At any
rate, RV *-ed-* is definitely younger than the Mitanni forms because the IIr form *sazdai > Ved.
sede (3 sg. perf., cf. Avestan *hazde*) 'he has sat' has already spawned a number of analogical
formations in the RV which are *not* conditioned by *-azd-*. These are found even in the older
sections of the RV: *yam > yem: yemuḥ* 4.2.14, *pac > pec: pece* 4.18.13 etc.[140]

    In all the cases detailed above, the retroflex is a late, i.e. a Vedic *innovation* that is not
shared by Iranian and the other IE languages. In short, the innovation is rather *low down* on
the 'family pedigree', in cladistics. Any biologist would classify a similar development in
biological materials as a clear indicator of a late development, as an *innovation*, -- in case, one
that separates IA from the rest of IIr and IE. In other words, Vedic Sanskrit does *not*
represent the oldest form of IE as autochthonists often claim.

    The adherents of the autochthonous theory would again have to take recourse to
special pleading, arguing that retroflexion occurred only *after* the Iranians had *supposedly* left
(i.e., well before the RV, at 4-5000 or 2600 BCE), or while they were living in some area of the
Panjab untouched by this phenomenon. This individual argument is, again, not *a priori*
impossible. But, it is not admissible on other grounds, such as the occurrence of local loan
words in Vedic. These have been taken from the Panjab substrate (Witzel 1999a,b) that has
unconditioned retroflexes (such as in *vāṇā, vīṇā,* etc.), and these substrate words are, again,
missing in Iranian.[141]

    Retroflexion in Vedic must have been a regional feature, acquired, just as it was by the
Pashtos and the more recently arrived the W. Iranian Baluchis, at the time of immigration.

    In sum, retroflexion affects all those moving into the E. Iranian borderland, the Indus
plain and the subcontinent. but this does not work vice versa: those who move out of India,
sooner or later, loose it. However, if this would be taken as proof of OIT, it does not work at
all: this particular development does *not* help to explain words such as Ved. *voḍhar-* which
cannot turn into Iran. *vaštar-*, Latin *vector,* etc.[142] The same conclusion can be reached when
studying local Panjab loanwords in the RV.

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[139] Note that this stage, minus the Indian retroflexion, is still preserved in Mitanni IA *vash-ana-* [vāžh-ana].

[140] Other examples for the *conditioned* OIA development of retroflexes examples include : *k' > c' > ś*, and *g' > j'
> j* as seen in: IE *wik'-s > IIr *wic'-š > Av. *vīš* / > Ved. *viṭ* 'people, settlement'; but > Latin *vīc-u-s,* Germanic
vik- (as in *Viking*), etc.; IE *reg'-s > IIr *rāj'š > rāṭ; > Lat. *rēx,* Celtic *-rix,* Germanic *-rik,* etc.; cf. also Avest. *xšuuaš*
: Ved. *ṣaṣ;* Lat. *sex,* Germanic *sehs,* Grk. *heks-* etc.

[141] To justify this, the autochthonous theory must further assume that the people of the substrate moved into
the IA /IE Panjab only *after* the Iranians and IE had left. A string of secondary assumptions. Occam's razor
applies.

[142] The Gypsies *eventually* lost the retroflexes (but when?).

§16. Absence of 'Indian' words in Iranian

As has been underlined several times, the *hypothetical* emigrants from the subcontinent would have taken with them a host of "Indian" words -- as the Gypsies (Roma, Sinti) indeed have done. But, we do not find any typical Old Indian words beyond S. Asia, neither in the closely related in Old Iranian, nor in E. or W. IE, except for the usual words of culture (*Wanderwörter*) such as some recent imports into English (*orange, tea/chai*, or *curry, punch, veranda, bungalow*), or the older ones of the type *rice, beryl, hemp,* etc.[143] One would expect 'emigrant' Indian words such as those for lion (*siṃha*), tiger (*vyāghra* AV+, *pṛdāku* AV+, *śārdūla* MS+, *puṇḍarīka* lex.),[144] elephant (*gaja* Manu+ *ibha* RV?, *kuñjara* Mbh.+), leopard (*dvīpin* AV+, Ep., *citra-ka,* etc. lex.), lotus (*padma, kamala, puṇḍarīka*), bamboo (*veṇu*), or some local Indian trees (*aśvattha, śamī, bilva, jambu*), even if some of them would have been preserved, not for the original item, but for a similar one (e.g. English [red] squirrel > N. American [gray] squirrel). Instead of Indian words we find, e.g., for *siṃha* 'lion' new formations : Iran. *šer*, Grk. *līs*, Lat. *leō(n)* (cf. Witzel 1999a,b), and similarly, Gr./Latin ones for 'tiger', 'lotus'. Many of them come from a Mediterranean/Near Eastern substrate, but not as expected in any OIT scenario, from the S. Asian one visible in Vedic.

In sum, no typical Indian designation for plants or animals made it beyond the Khyber/Bolan passes. The only clear exception would be the birch tree, whose IE name **bhṛg'ho-* is found all the way from India[145] to Europe: Ved. *bhūrja* KS+, Ir. Pamir dial. *furz,* Shugni *vāwzn < *barznī*; Osset. *bœrs(œ)*; Lith. *béržas*, Serbo-Croat. *brèza*; German *Birke*, Engl. *birch*, etc. (cf. §12.6, n.113). The other 'European' trees that are found in the northwest of the subcontinent, and beyond up to Russia/Urals, are *absent* from Sanskrit vocabulary.[146]

This situation has been well explained by the assumption of IE linguists that these European/Caucasus/Ural tree names were remembered (sometimes, in the Central Asian steppes and deserts, only in old sayings or in poetry?) down to the very doorsteps of South Asia in Afghanistan, or were applied to similar items, but were utterly forgotten in the tropical S. Asia as there were no similar trees to which these IE names could be applied. One apparent

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[143] See Witzel 1999a,b for details: *karpāsa* cotton, etc.

[144] Note that the tiger, N.Pers. *bebr,* is found in the N. Iranian mountains from the Elburz to the Kopeh Dagh even today, and the last specimen in the Aral Lake area is reported to have been shot in the Seventies.

[145] The reason for its survival in South Asia (Panjabi *bhoj*, etc.) may have been the economical and common ritual use of birch bark, e.g. for amulets.

[146] Perhaps with the exception of the willow (Lat. *vitex,* etc., see above, n.118) which it is found, along with the poplar, in the riverine forests all over the steppe (Schrader 1890: 440, 275). It is attested in E. Iran where it grows prominently: Avest. *vaēti,* Pashto *vala < *vaitiya*; but it is not found in Vedic/Skt., unless it is retained in *veta-sa* "reed, ratan, Calamus", with the expected change in meaning "willow > reed". The poplar and the beech (Lat. *fagus* etc.) are not attested in Skt.: both trees are not found in S. Asia during the pre-Indus period, even though the beech was then found much further east (N. Caucasus, etc.) than the famous "beech line" (Königsberg/Kaliningrad-Odessa). On the other hand, the oak, though found in various forms in Afghanistan, is *not* attested in Skt., perhaps with the exception of the inherited name of the weather god, *Parjanya*, who is often linked with the oak in various IE mythologies, see EWA s.v.; for example, Lithuanian *Perkŭnas*, O.Slavic *Perun*[u,] Lat. *querquus*, etc., see Pokorny, p. 822; for Class. Skt. *parkaṭī* 'ficus inferiora' see EWA II 192 ~ Ved. *plakṣa.*

exception, *vetasa*, can easily be explained by a transfer of meaning, from the very pliable (Afghan) 'willow' twigs to the equally pliable 'reed, cane' (see above).[147]

The autochthonous theory again must introduce the improbable *auxiliary* assumption that *all such words* have been forgotten *inside* the subcontinent after, or even as soon as, the Iranians (and other Indo-Europeans) *supposedly* had crossed the Suleiman Range and the Khyber/Bolan passes into Afghanistan and Iran.

However, many if not most S. Asian plant and animal names have clear, non-IE local origins; in other words, they are loan words from the local S. Asian languages[148] (e.g., RV *mayūra* 'peacock', *vrīhi* 'rice', etc.). Others are new formations, built on the basis of IE words, e.g. 'elephant': *hastin* (+ *mṛga*) RV 1.64.7, 4.16.13 etc., 'the (wild animal) with the hand, the elephant', used for words such as Late Ved. *gaja,* ŚB 14.4.1.24 *mataṅga,* Epic *nāga*, RV(?) *ibha*.[149] Or 'tiger', *vyāghra* < 'who tears apart?' (KEWA III 274), 'who smells scents by opening [his jaws]'(?) EWA II 593, for VS *śārdūla, puṇḍarīka* (lex.), (note also N.Pers. *bebr*). These new formations must have been introduced when the *immigrating* speakers of Indo-Aryan (again, *not* the Iranians!) were first faced with them in the Greater Panjab. Indigenists (Talageri 2000, Elst 1999, etc.) denounce such cases as just one more of the common substitutions based on poetic or descriptive formations, or as dialect designations which can happen at any stage in the history of a language (e.g. Vulgar Latin *caballus* > French *cheval*, etc. for older *equus*). However, such critics once again overlook the wider complex, the complete *absence* of *original* IE/IA words for *S. Asian* plants/animals built with clear IE *roots* and/or *word structure.* The absence of IE/IA words for local plants and animals clearly militates against any assumption that Pre-IA, Proto-IIr or PIE was the *local* language of the Panjab or of Uttar Pradesh during (pre-)Harappan times.

This also agrees with the fact that most of the S. Asian loan words in the Ṛgveda, excluding some Central Asian imports, are *not* found in Iran and beyond.[150] These words include Kuiper's (1991) c. 380 'foreign words' in the RV. Again, not all of them could have been lost as soon as the *hypothetical* IE or Iranian emigrants crossed over into Iran and

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[147] Friedrich (1970) has pointed out that most IE tree names are not explainable by IE etymologies (except for the birch tree < 'shining', cf. Bryant 1999). Following the autochthonous line, one could therefore assume that such (supposedly non-IE) names have been borrowed/spread from India. However, IE tree names such as 'beech, oak', etc. have true IE word structure: their roots follow the IE pattern (see above §10), and the suffixes employed are IE as well. In other words, these tree names *are* IE. That there are isolated roots of tree names is not strange. After all, many basic words, such as 'eye' and 'hand', (Pokorny 1959: 775, 447) are isolated in IE, i.e. these roots are not employed outside their narrow realm as (root) nouns other than in clearly derived, secondary ways. Most other basic IE words are related to verbs and therefore have a much wider application in word formation. Yet, no one has ever suggested that a words such as 'eye' is *not* IE. In addition, many tree names will go back to pre-IE times when their roots still might have had a clear onomastic meaning; these pre-IE words subsequently were automatically changed to fit the IE root structure.

[148] Indigenists decry the very *concept* of substrates, see Elst (1999), --much as they now begin to decry the various historical levels established in genetics, based on the analysis of the male only Y chromosome-- as this would necessarily indicate that Vedic had not been present in NW India since times immemorial.

[149] Ved. *ibha* is of dubious meaning and etymology (Oldenberg 1909-12). At least 2 of the 4 cases in the RV do not refer to 'elephant' but rather to the 'retinue train' or the 'court' of a chieftain. The meaning 'elephant' is attested only in Class. Skt. (Manu+), Pāli, see EWA I 194; cf. nevertheless O.Egypt. ',*abw,* EWA III 28. -- Gamkrelizde and Ivanov link *ibha* with Latin *ele-phant-*, etc. but this requires special, otherwise unattested phonetic correspondences such as *ele - :: i-,* etc.

[150] Some of them are of Central Asian origin, see Witzel 1999, Lubotsky forth.

beyond. One would at least expect a *few* of them in the 'emigrant' languages. Such Indian words should have survived in the west and could have acquired a new meaning, such as British Engl. *corn* 'wheat' > 'maize' in America. The Gypsies, after all, have kept a large IA vocabulary alive, over the past 1500 years or so, during their wanderings all over the Near East, North Africa and Europe (e.g., *phral* 'brother', *pani* 'water', *karál* 'he does').

No amount of special pleading will convince an independent (linguistic) observer of a scenario that relies on the total loss of all *typical* S. Asian words in Iranian and all the other 'emigrant' Indo-European languages. Again, Occam's razor requires to scrap the theory of an 'Aryan' or, worse, an Indo-European emigration from the Panjab to the West.

§17. IE words in Indo-Iranian; IE Archaisms *vs.* Indian innovations

Conversely, and not unexpectedly by now, typical IIr. words indicating a temperate climate, and with IE root and suffix structure, such as 'wolf' (*vṛka:* Avest. *vəhrka*; cf. Lith. *vilkas*, O.Slav. *vl'kᵘ*, Alban. *ulk*, Grk. *lúkos*, Lat. *lupus*, Gothic *wulfs < *wl̥kʷos*), 'snow/winter' (*hima:* Avest. *zim/ziiam,* Grk. *xiōn* 'snow', *-khimos,* Lat. *hiems,* Gaul. *Giamon-,* Armen. *jiun* 'snow', etc.), 'birch tree' (*bhūrja,* Pamir Dial. *furz,* Osset. *bœrs(œ),* etc. are found in E. Europe, Greater Iran and on the northwestern borders of the subcontinent (Kashmir). However, neither snow nor birch are typical for the Panjab or Indian plains. It is, again, *theoretically* possible that these words belonged to the supposed original IE/IA vocabulary of the northwestern Himalayas and therefore could have been transported westward by a *hypothetical* IE westward emigration. But, this scenario is contradicted by the evidence of the last section dealing with all the other IE 'cold climate' words that have not been preserved in India, not even in the Northwest or in the Himalayas. Therefore, words such as those for 'wolf' and 'snow' rather indicate linguistic memories of a colder climate than an export of words to Iran and Europe, such as that for the high altitude Kashmirian birch tree.

More importantly, typical Indian grammatical and lexical *innovations* are not found among the other Indo-European languages. While some, stemming from the IIr period, are met with in Old Iranian (pronoun *ah-am* 'I', Avest. *azəm;* Nom.Pl. *aśvāsa-as,* Avest. *aspåŋhō,* etc.), the *typical Indian* innovations found already in the RV (*jabhāra* for *jahāra, sede/mene*, absolutives, etc.) are not. The first type of innovation is attributed to the common source language, i.e. Indo-Iranian rather than OIA influencing the neighboring Old Iranian.[151] It would be against all rules of (IE and non-IE) comparative linguistics to assume that such late, (low-level, in term of family tree or cladistics) developments should *not* apply just in the *single* case of Indo-Aryan, and to assume, instead, *early* innovation *inside* India (*aśvās-as, ratha, babhru* 'mongoose', etc.) that would have *selectively* been exported to Iran (of course, minus all *typical Indian* RV innovations!), innovations that would *not* have been carried out in the rest of the Indo-European languages: just too many auxiliary assumptions!

The autochthonous theory would, again, have to assume that all such Indian innovations would have been carried out *after the speakers of Iranian (and/or all other Indo-European languages) had left the subcontinent,* which is contradicted by absence of typical Indian words in other Indo-European languages and in Iranian, and by the absence further west of Indo-Iranian innovations such as the chariot (**ratha*). Occam's razor applies again.

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[151] E.g., a comparison between the 1st pl. English (*we are*), German (*wir sind*), Dutch (*wij zijn*), shows that Engl. *are* must be a late internal innovation due to analogy with the 2nd plural form, and the equivalent of 3rd pl. *sind/zijn* is *also* substituted by *are;* while 1st pl. *sind/zijn* itself comes from the 3rd plural: *sie sind/zijn.*

To go into some further detail, the many archaisms in Old Iranian cannot readily be explained by an Iranian emigration from India: First of all when and where should this have happened? SW and Central Southern Iran was occupied by the Elamians, the western parts were settled by W. Iranians only after c. 1000 BCE (cf. Hintze 1998) and were settled by non-IE peoples before. About E. Iran/Afghanistan we have only stray Mesopotamian, copious archaeological and a few isolated Vedic sources. They point to non-IE settlements as well: in S. Iran, Elamian up to Bampur, Meluhhan east of it in Baluchistan/Sindh, and Arattan north of it in Sistan, while the northern fringe was occupied by the Bactria-Margiana substratum that is visible in Indo-Iranian (Witzel 1999a,b).

If the Iranians had moved out from the Panjab at an "early date", they would have missed, the *supposed* 'Panjab innovation' of the use of the (domesticated) horse (already Indo-European: Latin *equus,* etc.), and especially the later one of the horse-drawn chariot (IIr. *ratha*). If, on the other hand, they had moved out a little later, say, after the Mitanni Indo-Aryans, all of this would have come too late to account for the non-appearance of Iranian tribes in the RV which has only some (pre-)Iranian looking names (Witzel 1999), camels (RV 8) and some Afghani rivers (Gomatī in the Suleiman Range, Sarayu in Herat, Sarasvatī in Arachosia). We cannot make the Iranians move from India to Iran, say, at 5000 or 2600 BCE, then to introduce the innovation of horse pastoralism (not present in the subcontinent then!), and then let them take part, at c. 2000 BCE, in the innovation of the *already IIr* horse drawn chariot (*ratha, § 12.6, §21).

In addition, Old Iranian *in general* is too archaic to have moved out of India *after* the composition of the RV: while Old Avestan (of Zaraϑuštra) has, to be sure, many forms which correspond to Ṛgvedic ones, much of his language is even more archaic: as has been mentioned, the retention of the use of neuter plural with singular of the verb is something that has elsewhere been retained in Hittite; the old nom. pl. masc. in *-ās* = Avest. *-ās-, -ā-* is found in the RV next to the innovation *devās-as*; an archaism in the perfect stem which appears in the RV such as *babhr-* (Avest. *baβr-*) next to the new formation RV *jabhr-*; archaisms in names such as *Jamad-agni* (= Avest. *jimat̰*) next to the innovative RV *gamad,* etc.

All of this points to a time of separation of IA and Iranian *before* the RV and thus, not inside India. The hypothetical argument that these traits were preserved in the Panjab side by side with the RV does not hold, for Iranian does not show any typical Indian elements (see above).[152]

If the Iranians had indeed left the Panjab before the RV, serious chronological difficulties would arise, whether we were to accept the autochthonous theory of the RV well before the Indus civ. (2600/5000 BCE) or whether we accept the traditional Indologist's dating of the RV sometime in the 2nd mill. BCE. In all these cases, Iranian is far too archaic to have been a *close* neighbor, in the Panjab, of the Ṛgvedic dialects. Further, it lacks any indication of Indian influence on its grammar and vocabulary (see above).

One can only conclude that Old Iranian, including Avestan, split off from (Proto-)Old Indo-Aryan (Vedic, etc.) at an early date, preserved some archaic features while developing innovations on its own (*s > h*, *kh > x, j'n > sn,* etc.) and that it was never in *early*

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[152] An auxiliary theory, e.g. of a strong local (Dravidian, etc.) influence on the RV *only,* as opposed to Iranian -- while still in India-- is implausible; the same applies to Drav. influence *after* the Iranians *supposedly* left: all of this would require an altogether new theory, constructed out of the blue, of a push towards the northwest by Dravidian.

close contact with the Panjab and its substrate languages. Such close contact would also have effected the one typical phonetical development that the Iranians actually 'escaped' *before* the Vedic dialects of the RV adopted or developed it, the retroflex sounds (see above §15).

§18. Absence of Indian influence in Mitanni-Indo-Aryan

The same scenario as discussed so far is indicated by the IA loan words in the Hurrite language of the Mitanni realm in northern Iraq/Syria (c. 1460-1330 BCE). Again, if there was an (early) emigration out of India by (Vedic) Indo-Aryans it would be surprising that even the Mitanni documents do not show typical *South Asian* influence.[153]

Rather, is obvious that the remnants of early IA in Mitanni belong to a pre-Ṛgvedic stage of IA, as is seen in the preservation of IIr -zdh- > Ved. -edh-, in *Priyamazdha* (*Bi-ir-ia-ma-aš-da*[154]) : Ved. *priyamedha* : Avest. *-mazdā.* These texts also still have IIr *ai* > Ved. *e* (*aika* : *eka* in *aikavartana*). Another early item is the retention of IIr. *j'h* > Ved. *h* in *vašana(š)šaya* 'of the race track' = [*važhanasya*] cf. Ved. *vāhana-* (EWA II 536, Diakonoff 1971: 80, Hock 1999: 2); they also share the Ṛgvedic (and Avestan) preference for *r* (*pinkara* for *piṅgala*, *parita* for *palita*). Importantly, Mitanni-IA has no trace of retroflexion.

How could all of this be possible if one supposes an emigration from India, in some cases (Misra 1992) even *after* the supposed date of the RV (5000 BCE)? The RV is, after all, a text that already *has* all these features.

The Mitanni loan words (Mayrhofer 1979, EWA III 569 sqq.) from Pre-Vedic OIA share the typical IIr innovations, such as the new *Asura* gods Varuṇa (EWA II 515 *a-ru-na, ú-ru-wa-na*, not found in Iran) and Mitra (Avest. Miϑra, Mitanni *mi-it-ra*), and Indra (Mit. *in-da-ra/in-tar,* Avest. *Iṇdra*)[155] who is marginalized in Iran, and the Nāsatya (*na-ša-ti-ya-an-na* = Aśvin, Avest. *Nåŋhaiϑiia*).[156] These innovations also include the new the concept of *Ṛta (*Iran. *Arta,* in *very late* Avest. pronunciation = *aša*), contained in names such as Artasmara (*ar-ta-aš-šu-ma-ra*), Artadhāman (*ar-ta-ta-a-ma*),[157] and perhaps also the newly introduced ritual drink, *sauma,* IIr **sauma* (Ved. *soma,* Avest. *haoma,* EWA II 749). The Mitanni sources show extensive use of the domesticated horse (*ašuua,* cf. names for horse colors[158]), the chariot (*rattaš*) and chariot racing (*a-i-ka-, ti-e-ra-, pa-an-za-, ša-at-ta-, na-a-[w]a-wa-ar-ta-an-na= [aika-, tri-, panca-, satta-* (see n.160), *nava-vartana];* *tušratta/tuišeratta* = RV *tveṣaratha*).

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[153] Brentjes' pointing to the peacock motive in Mitanni times art is a very weak argument (for detailed criticism, see Schmidt 1980: 45 sq.) We know that even the Sumerians imported many items from India (Possehl 1996). Further, the peacock motif is attested in Mesopotamia well before the Mitannis. For a list of Mitanni-IA words, cf. now *EWA* III, Appendix.

[154] Mayrhofer 1979: 47; in Palestine, cf. *Priya-aśva: bi-ir-ia-aš-šu-va.*

[155] Mayrhofer 1979: 53: *in-tar-ú-da, en-dar-ú-ta* (Palestine, 15th cent. BC); cf. Cowgill 1986: 23.

[156] Via Mitanni, perhaps also Hitt. *Agni* (*Akniš,* cf. Avest. *dāštāγni*), Mayrhofer 1979: 36, 51: *a-ak-ni-iš.*

[157] The lineage includes *Bar-sa-ta-tar, Sauššattar (Sa-uš-ta-a-tar, sa-uš-sa-ta (at)-tar), Artad(h)āma (Ar-ta-ta-a-ma), Sattarna II, Artasmara (Ar-ta-aš-šu-ma-ra), Tušratta (Tu-uš-rat-ta, Tu-iš-e-rat-ta, Tu-uš-e-rat-ta: *Tvaiṣáratha), KUR-ti-ú-az-za,* Mayrhofer 1979: 54 sqq., cf. Cowgill 1986: 23.

[158] Kikkuli *bapru-nnu:* Ved. *babhru, binkara-nnu* : Ved. *piṅgala, baritta-nnu* : Ved. *palita,* with Ṛgvedic *-r-* instead of later *-l-,* Mayrhofer 1979: 32, 52-3, cf. Cowgill 1986: 23.

To see in these names a post-RV form of OIA, a Prakrit (Misra 1992, Elst 1999:183),[159] is therefore misguided and based on insufficient knowledge of near Eastern languages. Misra's 'Prākṛtic influences' in Mitanni IA are due to the peculiarities of the cuneiform writing system and to the Mitanni form of the Hurrite language. It has been asserted for long that *satta* in *satta-vartana* 'seven turns' has been influenced by Hurrite *šinti* 'seven' (J. Friedrich 1940, cf. Cowgill 1986: 23, Diakonoff 1971: 81; this is under discussion again,[160] but clearly a Hurrite development); however, the words starting with *b-* such as *bi-* did not receive their *b-* from a MIA pronunciation of *vi*,[161] as Misra maintains, but are due to the fact that Mitanni does not allow initial *v-* (Diakonoff 1971: 30, 45). In sum, the Mitanni IA words are not Prakritic but (pre-)Ṛgvedic.

On the other hand, the Mitanni texts clearly indicate typical OIA (Vedic) linguistic innovation: *aika-vartana* (*a-i-ka-ua-ar-ta-an-na*)[162] instead of Ir. *aiva-* or general IE **oino- > *aina-*), and yet, the vocabulary does not yet show signs of typical *South Asian* influence: for example, there is no retroflexation in *mani-nnu,* Avest. *maini*, Elam O.P. **bara-mani*, and Latin *monīle*. But retroflexation is precisely what is found once OIA enters South Asia: RV *maṇi* 'jewel'.[163] Finally Mitanni IA has no typical South Asian loan words such as *āṇi* 'lynch pin'.

In sum, Mitanni-IA is older than the RV, cannot have come from the Panjab but must have been spoken in the north-eastern border areas of Mesopotamia where it influenced the Hurrite language of the Mitanni that belongs, just like its later relative Urartu, to the Caucasus group of languages.

Indeed, some of the rather indirect IA influx into the Near East may have been earlier than the one visible in Mitanni. The Kassite conquerors of Mesopotamia (c. 1677-1152 BCE) have a sun god *Šuriiaš*,[164] perhaps also the *Marut* and maybe even *Bhaga* (*Bugaš*?), as well as the personal name *Abirat(t)aš* (*Abhiratha*); but otherwise, the vocabulary of their largely

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[159] Elst sees here, of course, a confirmation of his belief that the RV is of hoary pre-Indus vintage. Thus, he can expect post-Ṛgvedic Prākṛt forms in 1400 BCE. While some MIA forms may be sought in the RV, their status is constantly questioned and further reduced. The latest form that has come under attack is *jyotiṣ < *dyaut-is*, see C. aan de Wiel 2000.

[160] "E. Laroche, in his *Glossaire de la langue hourrite*, lists the word *šittanna* from the Kikkuli text and comments: "... "sept", d'après l'indo-arien *šatta-wartanna*. - Forme de *šinti/a*??" S.v. *šinti*[2] he says: "Mais *šinti* "sept" doit encore être séparé... de *šitta*." He also lists a word *šittaa* (long a) from two (Hittite?) Kizzuwadna texts." (pers. comm. by Bjarte Kaldhol, Nov. 5, 2000).

[161] Incidentally, it would be *eastern* MIA, such as Māgadhī (which, however, does not agree with the extreme Rhotacism of Mitanni-IA but has *l* everywhere!), as western North India has retained *v-* , see Masica 1991: 99 sq.

[162] Thus also Cowgill 1986: 23. Note that Ved. has *eva* 'only' < *aiva* = O.Iran. *aiva* 'one', and that only MIr. has *ēvak* 'one', but this is due to the commonplace MIr. suffix *-ka*; Next to the usual [*tri-, pañca-, *sapta-, nava-vartana*]; and racing terms such as : *ua-ažan-na* 'race track', also with genitive in: *-na-ši-ia*!, and perhaps [Lú]*a-aš-šu-uš-ša-n-ni*, 'horse trainer', Diakonoff 1971: 81, Mayrhofer 1979: 52;.

[163] Mayrhofer 1979: 53; cf. RV *maṇi*, Av. *ma*[i]*ni*, Elam. O.P. **bara-mani,* Latin *monīle*, etc.; cf. also Varuṇa as *Uruna*, and Ved. *sthūṇā*, Av. *stūnā/stunā*, O.P. *stūnā*, Saka *stunā*.

[164] Explained as sun god, "Šamaš", Mayrhofer 1979: 32; cf. also the war god *Maruttaš = Marut-,* and king *Abirattaš = Abhiratha;* for details see Balkan 1954: 8.

unknown language hardly shows any IA influence, not even in their many designations for the horse and horse names[165] (Balkan 1954).[166]

If one now thinks through the implications of the autochthonous theory again, the ancestors of the Mitanni Indo-Aryans would have left India very early indeed (well before their favorite date of the RV, 2600/5000 BCE, and well before 1900 BCE, the *supposed* date of the Brāhmaṇa texts, Kak 1994). They would have done so with the Ṛgvedic dialect features (*ai > e, zdh > edh*) *not yet* in place, and without any of the alleged MIA forms of Misra (*satta,* etc.), but *with* the typical OIA and IIr terms for horses and chariot racing (*before* their invention and introduction into South Asia)! They would have lingered somewhere in N.W. Iran to emerge around 1400 BCE as Hurrianized Mitanni-IA, with some remnant IA words and some terms of IA religion. But they would have done so *without* any of the local South Asian innovations[167] (no retroflex in *mani-*, no *-edh-, -h-*, etc.) that are already found in the RV, and also *without* any particularly Indian words (lion, tiger, peacock, lotus, lynch pin *āṇi*) all of which would have been 'selectively' forgotten while only *typical* IA and IE words were remembered. In short, a string of contradictions and improbabilities. Occam's razor applies again.

Similarly, the Parna (Gr. *Parnoi,* Ved. *Paṇi*) and Dasa/Dāsa ~ Avest. (*Aži*) *Dahāka*, ~Ved. *dāsa Ahīśu*, Lat. *Dahi*, Grk. *Daai*, Avest. *Dåŋha* (:: *Airiia*, cf. Dahae :: Arii), would have escaped their Panjab IA enemies (RV Dasa, Dasyu, Paṇi :: *ari, Arya, Ārya*) northwards in order to settle at the northern fringes of Iran well before the time of the RV, e.g., as the *Parna,* still *without* retroflexion and accompanying loss of *-r-*. Unfortunately for the autochthonous theory, these N. Ir. tribes occur already in the RV, significantly *not* as real life but as *mythical* enemies, and now *with* retroflexion. Significantly, all while the same authors who composed the RV hymns are supposed by the indigenist and revisionist writers not to remember *anything* beyond the Panjab. Again, multiple contradictions: Occam's razor applies.

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Summary : Linguistics

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[165] Note, however, *timiraš* = Ved. *timira-* 'dark', cf. Balkan 1954: 29, also 1954: 27 *laggatakkaš = lakta*?

[166] Some early IA immigrants that according to Harmatta (1992: 374) seem to be recorded in a tablet of the Dynasty of Agade, at the end of the third millennium BCE, c. 2300-2100 BCE: A-ri-si(<sa')-en = Arisaina and Sa-um-si(<sa')-en = Saumasena, are *wrong* interpretations of Hurrian words: "Hurrian names in -*šen* (not -*sen*) are common in earlier periods. *Arišen* means "The brother gave", *and Šaumšen* (probably pronounced *Tsaom-then*) is made from a root *sa-* plus the verbal suffix -*um/-om* plus -*šen*, an abbreviated form of *šenni*, "brother". These names from Samarra were published by Thureau-Dangin in RA IX 1-4. See Gelb et al., *Nuzi personal names*, Chicago 1963, p. 255" (personal comm. by Bjarte Kaldhol, Nov. 6, 2000). On š = [ϑ] see Diakonoff 1971: 46. -- Harmatta (1992: 374) wrongly took these names as a sign of an early Indo-Aryan spread towards Mesopotamia.

[167] Some of the so-called MIA features of Mitanni-Indo-Aryan are due to the writing system (*in-da-ra*, etc.); *satta* is questionable as well: *ša-at-ta* is influenced by the Mitanni term, as **ša-ap-ta* would be possible in this writing system; S.S. Misra, however, has found linguistic features common to MIA (Middle Indo-Aryan) and even NIA: assimilation (*sapta > satta*); anaptyxis (*Indra > Indara*); initial *v > b* (*virya > birya*), read, however, *Priya*! -- K. Norman erroneously pointed to *pt > tt* (see discussion of *satta),* labialization of *a > u* after *v* (**ašvasani > aššuššanni*), see however, Mayrhofer 1979: 52.

In sum, all of the linguistic data and the multitude of possible autochthonous scenarios based on them lead to the same kinds of *culs de sac* or *Holzwege*.

There is *no evidence* at all for the development of IE, IIr, and even of pre-OIA/Vedic *inside* the subcontinent. It is contradicted, among other items, by the Iranian and Mitanni evidence. An *emigration* of the Iranians and other Indo-Europeans[168] from the subcontinent, as supposed by adherents of the autochthonous theories, is *excluded* by the linguistic evidence at large.

To maintain an Indian homeland of IE, IIr, and Pre-OIA requires multiple special pleading of a sort and magnitude that no biologist, astronomer or physicist would tolerate. Simply put, *why should we allow special, linguistic pleading just in the case of India*? There is nothing in the development of human language in India that intrinsically differs from the rest of the world. Occam's razor applies.

So far, most of the linguistic evidence presented in the previous sections has been neglected by advocates of the autochthonous theory,[169] and if it has been marshaled at all, it has been done so *ad hoc*, even by the lone, autochthonously minded Indian historical linguist, S.S. Misra. His *rewriting* of IE linguistics remains incidental and idiosyncratic, and it results in multiple contradictions, just as the rest of the theory. The autochthonists must do a lot of homework and try to contradict the linguistic data discussed above (detailed in § 13-18) before they can hope to have any impact on linguistic discussions.

Conversely, the data derived from linguistic study are consistent throughout: they clearly indicate that an Eastern IE language, the Vedic branch of IIr, has been *Indianized* and has grammatically *innovated after* its arrival in the Panjab, while Iranian has escaped this influence as it did not enter the subcontinent then. Exactly how the IA language *and* the *IA spiritual and material culture* of the archaeologically still little traced Indo-Aryan speaking tribes was introduced, that is still an open and very much debated question. It can be traced securely, so far, only in the evidence coming from the texts (horses, chariots, religion, ritual, poetics, etc.) and from the features of the language itself that have been discussed here at length. Possibly, genetic evidence, especially that deriving from studies of the male Y chromosome, may add to the picture in the near future.

In the sequel, the evidence from texts, archaeology, and some natural ("hard") sciences will be adduced. This is perhaps the right place to point out that these fields of scholarship proceed in their own fashion and with various methodologies, and that the data obtained from all these fields have their own characteristics. It is not always the case, for example, that evidence from archaeology can flawlessly be matched with linguistic or genetic data. The nature of evidence in these fields often is too disparate. Some scholars (such as the archaeologist Shaffer) actually refuse to take into account anything that is outside archaeology, especially the "tyrannical" linguistics. This is of course not quite true, as palaeontology is tacitly accepted. Second, it must be pointed out that many of these fields, such as archaeology, provide "hard" evidence, but then *interpret* their data in various ways, just as it occurs in the other humanities. The same is true also, e.g. for studies of palaeo-climate. The distinction between the 'hard sciences' and the humanities is not as strict as is often made out.

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[168] The much later emigration of the Gypsies and some others into Central Asia are of course excluded here.

[169] With the (partial) exception of Elst (1999), and Talageri (2000) for which see above.

Nevertheless, we should keep looking for overlaps in evidence and draw our own, often preliminary conclusions, -- preliminary as several if not all of the fields involved are in constant development.

CHRONOLOGY

§19. Lack of agreement of the autochthonous theory with the historical evidence: dating of kings & teachers

Turning, presently, to the evidence preserved in the texts themselves and in history as well as archaeology, it might be useful to deal first with an item that has captured the imagination of scholars east and west for at least a century, that is, the various lists of early kings (and also of Vedic teachers).

Advocates of the autochthonous theory stress that the traditional lists of Indian kings (in the Mahābhārata, Rāmāyaṇa, Purāṇas) go back to the fourth millennium BCE and even earlier. However, even during the formative period of the great Epic at c. 300 BCE, Megasthenes, the Greek ambassador to the Maurya court at Patna, reported to have heard of a traditional list of 153 kings that covered 6042 years.[170] This would, of course, lead back well beyond the traditional beginning of the Kaliyuga at 3102 BCE (cf. Witzel 1990). The latter date, however, is due only to back-calculation, based on the alignment of all then known(!) five planets, that was carried out by Vārāhamihira in the 6th cent. CE (Kochhar 1999). In other words, all *dates* based on a beginning of the Kaliyuga in 3102 *are worthless.*

The royal lists rest, as almost everywhere in traditional cultures, on Bardic traditions.[171] In India, they derive from lists orally transmitted and constantly reshaped by the Sūta bards according to local conditions and personal preference (Parry and Lord, 1930 sqq.)[172] The eager efforts made by many Indian scholars of various backgrounds to rescue

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[170] Megasthenes, the Seleucid ambassador to Candragupta (Sandrokottos) Maurya's court, at c. 300 BCE (Arrianos, Indika 9.9). -- All of this is called "entirely plausible" by Elst (1999: 192); however, there even is 6776 BCE as another starting point, according to Pliny, *Naturalis Historia* 6.59 and Arrianos. Elst strangely comments "even for that early pre-Vedic period, there is no hint of immigration". In short, according to Elst (and Talageri 2000) we get Indian "kings" in the Gangetic plains of the 7th millennium BCE, when this area was populated by a few hunter and gatherer tribes! These 'monarchs' would indeed be the first kings on the planet (Witzel 2001). Elst is not aware of the common (Indian, etc.) tendency to put contemporary lineages one before another when setting up long range 'historical' records (Witzel 1990). See also next note.

[171] See the lists in the Torah, Homers' list of ships, Polynesian lists of chieftains, and so on. *Listenwissenschaft* is one of the oldest 'sciences' in the world, cf. the Babylonian evidence in Z.J. Smith (1982) and Assmann (1987).

[172] Where we can check such Bardic traditions with the help of historical records, e.g. in the Germanic epic, they tend to telescope, rework the historical data; for example, they confound Ermanric, the king of the Goths in the Ukraine at the time of the Huns' invasion, with his grandson Theoderic, king of the Goths in Italy.

these lists as representing actual historical facts[173] therefore are ultimately futile.[174] The only early Purāṇic kings we can substantiate are those listed in the Vedas as these texts, once composed, could no longer be changed.

The process is exceptionally clear in the development of the tale of the Great Battle (*dāśarājña*, RV 7.18, see Witzel 1995). In the RV this is fought between the Bharata chieftain Sudās on the one side, and the Pūru chief with his nine 'royal' allies on the other. It took place on the Paruṣṇī in central Panjab. The Mahābhārata battle, however, is fought between the Kaurava (of Bharata descent) and the Pāṇḍava, both of the new Kuru tribe, near the Sarasvatī in Kurukṣetra (modern Haryana).

Because of the extremely careful oral method of RV preservation we can take the RV report as a sort of tape recording of contemporary news, news that is of course biased by contemporary political considerations and the mentality of the victor. However, already the Middle Vedic texts indicate a gradual shift in the non-Ṛgvedic and non-specialized, more popular traditions: there is a general confusion of the characters and the location involved, leading to that of the well known Mahābhārata personages and localities (details in Witzel, 1995). All of this does not inspire a great deal of credibility in the "facts" reported by the Epic and Purāṇic texts (Pargiter 1913, Morton Smith 1973, Talageri 1993, 2000).[175] These texts have clearly lifted (parts of) lineages, fragment by fragment, from the Vedas and have supplied the rest (Söhnen 1986) --from hypothetical, otherwise unknown traditions-- or, as can be seen in the case of the Mahābhārata,[176] from poetical imagination.

Similarly, the idea that the Vedas contain reliable lists of teachers rests on typically weak foundations. First of all, the various of Vaṁśa lists at the end of ŚB 10, ŚB 14 = BĀU 2, BĀU 6, JUB 4, KA 15, cf. ChU 8.15, etc.) do not agree with each other. Second, they trace the line

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[173] The latest example is Talageri (1993, 2000) who builds a whole imaginative prehistory of S. Asia on such 'data': with an early emigration of the Druhyu branch of the Aryans to Iran and Europe in the 5th millennium BCE, including such fantastic etymologies and identifications as Bhalānas = Baloch (who appear on the scene only after 1000 CE!), Bhṛgu = Phrygians, Madra = Mede (Māda), Druhyu = Druids, Alīna = Hellenic people, Śimyu = Sirmios (Albanians), etc. -- These are Oakish cases where even Elst (1999: 192 sq.) does not always follow him.

[174] The arguments used to justify the historicity of the Purāṇas (Elst 1999) are easily dismissed. While we can expect names of a similar sort in the older lists --some of them are also found in the Vedas (after all, names within a family often begin or end in the same way),-- they cannot be used to substantiate the *actual* existence of *complete* Purāṇic lists during Vedic times. See §19. -- Elst's further argument that early Purāṇic dynasties are not those of the northwest but of Bihar, Utkala etc. equally does not hold. It is clear that the beginnings of the lists, even in the Mbh and Rām., were reformulated to fit local demands: a western (Bharata) one for the Mbh and an eastern (Ikṣvāku) one for the Kosala area. (Witzel, in prep.) Agreement between the Epics, Purāṇas, Buddhist and Jaina texts does not vouch for a 'hoary' age of such lists, just for a common perception at the time these texts were composed, i.e. after 500 BCE. Only the Vedas are older, and they contain just small fragmentary sections of the later (enlarged, altered) Purāṇic lists. The influence of politics of empire (Nanda, Maurya, Gupta) and of local politics (or the wish by local kings to forge such a link to a well established lineage) should not be underestimated.

[175] Talageri turns things around and finds justification of the Purāṇic data in the Vedas, and thus a spread of the Lunar dynasty from Kosala (Prayāga) westwards. Strangely enough, these Pūru dynasties later on again spread eastwards (as is clear from the Vedas anyhow!) -- All of this is faithfully repeated by Elst (1999: 191). If this is not a *post-factum* justification, a *retrofit* as indigenist like to call such constructions, of the originally despised Ikṣvāku lineage (JB 1.338 = Caland § 115, see a first try at amelioration in AB 7, Witzel 1989), -- then what? (Discussion already in Witzel 1995).

[176] Especially clear with the introduction of the 'non-Vedic' Pāṇḍavas (Witzel, in prep.).

of teachers back to the gods, to Prajāpati. Yet even if we neglect this small detail and take only the later parts of these lists at face value (Morton Smith 1966), we do not know *when* to place them in time, as the absolute dates of the teachers are totally unknown, except for some overlaps with chiefs and kings known from the Vedic texts, as tentatively worked out by Morton Smith (1973).

Any historical reconstruction based on such lists must then start with assumptions, and even the usual average number of 20 or 30 years attributed to a generation does not work for teacher/student relationships, e.g., Mahidāsa Aitareya supposedly lived for 116 years and can have had many generations of students, just like any modern academic teacher. In addition, the Vaṁśa lists mention that certain Veda students had several teachers. In fact, Yājñavalkya, whom the ŚB sometimes pictures as an old man, could have had students throughout his life, and of various ages. All of this makes the use of the Vaṁśa lists for *reliable* dating almost impossible.

Again, the general question, asked several times already, has to be put here as well: if the traditional Bardic data are unreliable in traditional societies everywhere around the world, why should the same kind of data, shaped and reshaped by the later Vedic texts, the Epics and the Purāṇas, be a full and true account of South Asian prehistory? As in the cases listed above (and further below), this amounts to very special pleading, in fact again to another unmotivated exemption of India from the generally accepted procedures of the sciences, and of scholarship in general.

The genealogical data also do not readily fit into the combined, *general* picture as provided by the texts and by other disciplines such as archaeology, to which we will turn now.

ARCHAEOLOGY

§ 20. Archaeology and texts

Archaeology strives to discover, but cannot establish all the major factors that make up a certain civilization, as this science is limited to physical remains, from buildings and art to pottery, plants and human bones. As long as archaeologists cannot find readable inscriptions and texts along with their findings, the interpretation of the *spiritual* background and much of the society of the culture in question remains tentative.[177] The Mayas, e.g., were regarded as exceptionally peaceful people until their texts could be read. We cannot yet read the Indus inscriptions, and we do not yet have access to the archaeological remains, if indeed preserved, of the Ṛgvedic period. Many of the archaeological interpretations thus remain tentative, and by their very nature, they tend to shift with each new major discovery.

In the sequel, some of the archaeological and textual data are compared with what the autochthonous theories make of such evidence. It must be pointed out that autochthonists frequently rely on the *dicta* of recent archaeologists who stress that there was no major cultural break in South Asia from 6500 BCE well into the prehistorical period. However, archaeological evidence -- extremely important as it is -- forms just *one* facet of several of a given culture, and in many respects only of its the most materialistic aspects. It must agree with what the

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[177] Recently, it has been tested in Papua-New Guinea what the material remains of some five different linguistic communities belonging to one particular area would look like. After a deterioration of a few years, the archaeologists dug them up, and found -- "the same (material) culture"! So much for the often used or alleged overlap of language and culture.

other sciences supply on information about the period in question. In other words, where is the archaeologist that can tell us what the famous Indus "Śiva" or "Paśupati" seal really signifies? We will return to this question below.

§21. RV and the Indus civilization: horses and chariots

The autochthonous theory asserts a rather early date for the RV (pre-Indus civilization, at 2600 or 5000 BCE). Indeed, the RV does not know of the Indus towns, of international commerce, of the Indus script, of the Indus staple food, wheat, nor of the late-Indus cereal, rice (see below §23). However, all of that is only evidence *ex silentio*, while the rich Ṛgvedic materials dealing with the domesticated horse, the horse-drawn chariot, or chariot races do not fit *at all* with such early dates for the RV[178] (see immediately below) and rather put it after c. 2000 BCE. The closely related older Avestan texts, too,[179] point to a pastoralist, copper/bronze culture with use of horse and chariot, quite similar to that of the RV.

Clearly, the use of the horse drawn chariot in sport and war during the RV period was mainly, but not exclusively, a noblemen's occupation. In the autochthonous theory, the "relative absence of horse bones" in the Indus civilization[180] is therefore explained away by the auxiliary assumption that the horse was only occasionally imported for the nobility, who nevertheless were regarded as very good horse trainers. This overlooks the fact that riding, too, is attested in the RV and that is clearly linked to groups socially situated below the nobility (Falk 1995). However, not *one* clear example of horse bones exists in the Indus excavations[181] and elsewhere in North India before c. 1700 BCE (Meadow 1997, 1998). Even Bökönyi (1997), who sought to identify some horse remains in the Indus civilization, states that "horses reached the Indian subcontinent in an already domesticated form coming from the Inner Asiatic horse domestication centers."

Indeed, well recorded and stratified finds of horse figures and later on, of horse bones first occur in the Kachi valley on the border of Sindh/E. Baluchistan (c. 1700 BCE), when the

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[178] Similarly early dates are inherent in Talageri (2000). When tabled, the various family books in his reconstruction turn out to be spread out over two thousand years, well before the invention of the horse-drawn chariot. In addition, the very starting point of his book, on which his 'new chronology' of the RV books rests, is clearly wrong: as has been pointed out (n. 7, 84, 87, 140, 173, 175, 216), his investigation is based on the present Śākala 'edition' and arrangement of the RV, not on the first collection ("Saṃhitā", of the Kuru period) as established by Oldenberg (1888). How can one come even close to the period of the RV authors if one accepts any hymn inserted during the long period of orthoepic diaskeuasis, with additional, immeasurable influence by unknown teachers that existed between the first collection and the redaction by the late Brāhmaṇa scholar Śākalya (BĀU 3). - Talageri's ecstatic summary (2000) therefore is self-defeating: "Any further research, and any new material discovered on the subject, can only confirm this description.... but there is no possible way in which the location of the Original [IE] Homeland in the interior of northern India, so faithfully recorded in the Puranas and confirmed in the Rigveda, can ever be disproved." Interestingly, he has taken his initial historiographical cues from Witzel 1995 (and even lauds the general approach) -- only to reverse himself completely as to include the usual indigenous ("Purāṇic") agenda with chariots before their invention, IE emigration from Uttar Pradesh, etc. (Witzel 2001).

[179] Summary by Skjaervø 1995:160, sq., 167 sq.

[180] Elst (1999: 180) makes a lot out of this argument *ex silentio* but concludes "it is not as strong an argument against "Vedic Harappa" as it once seemed to be"!

[181] See R. Meadow and A. Patel 1997.

Indus civilization already had disintegrated. Some *supposed* early finds of horses elsewhere are those of equid bones and teeth at Surkotada[182] (in Cutch, W. Gujarat) from the late Harappan period,[183] which belong to hemiones (*Equus hemionus khur*, the onager or half-ass), not to true horses (*Equus caballus*, see Meadow and Patel, 1997, Meadow 1998). Other claims, such as the invented one of an indigenous Ṛgvedic 17-ribbed Sivalensis horse,[184] are totally unsubstantiated, or they are from unclear stratigraphies and/or have not been documented well enough[185] as to allow a clear distinction between horse, hemione or donkey;

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[182] Bökönyi 1997 finds it in Surkotada IA-B-C, (acc. to Sharma 1990: 382, from the Harappan period: 2300-1700 BCE, Joshi 1990: 17, 59 sqq.)

[183] However, note that (according to Meadow/Patel 1997): "Surkotada has dates that go into the second millennium, and the date of the "Harappan" layers themselves is not at all that clear." Cf. Joshi 1990.

[184] The latest folly (again, one created on the internet, this time by the proponent of an Austric 'theory' of IA origins) is that of the long extinct early Indian horse, *Equus sivalensis.* This early horse in fact emerged c. 2.6 million years ago, overlapping, in the Siwalik Hills, for a short period with the older (three-toed pre-horse) Hipparion (MacFadden 1992:139) that died out soon afterwards. Many internet writers now connect the Sivalensis horse with the 17-ribbed Ṛgvedic horse and modern S.E. Asian horses, however, without any evidence cited from archaeology, palaeontology or genetics. Fact is that horses (*Equus caballus*) have 18 ribs on each side but this *can individually* vary with 17 on just one or on both sides. Such as is the case (only 5 instead of 6 lumbar vertebrae) with some early horse finds in Egypt, from the mid-1st millennium BCE, horses that all were imported from the Near East (and ultimately from the steppe zone). Clutton-Brock (1992: 83) writes: "It is generally claimed that the Arab and the Przewalski horse [of Central Asia!] had only five lumbar vertebrae while all other horse breeds have six. In fact the number is very variable but it is true that the Arab is more likely to have only five lumbar vertebrae than other breeds of domestic horse (Stecher 1962)." Which only underlines that a domesticated, 17-ribbed horse has been brought into the subcontinent from Central Asia (Bökönyi 1997) -- just the opposite of what internet 'specialists' (and by simple extension, that excellent source of scientific information, the New Delhi party journal, "*The Organiser*") now claim, -- always without a single scholarly source. It should also be noted that numeral symbolism may play a role in the RV passage (1.162.18) mentioning the 17-ribbed horse, which is part of an additional hymn of a late RV book. The number of gods is given in the RV as 33 or 33+1, which would correspond to the 34 ribs of the horse (later on identified with the universe in BĀU 1); note further that the horse is speculatively in brought into connection with all the gods, many of them mentioned by name (RV 1.162-3)..

[185] In the Indus Valley, the horse (*Equus caballus L.*) was first reported, of course without palaeontological checks, at Mohenjo-Daro by Sewell (1931). -- Other spurious accounts: Bh. Nath 1962, Sharma 1974, 1993; similarly alleged for late Mohenjo Daro and late Harappa, for Kalibangan and Rupar (Bhola Nath, see B.B. Lal 1997: 285); for Malvan, Gujarat (Sharma 1990: 382); for Mohenjo Daro and in small numbers in rather recent levels, for Harappa from the late phase (Bökönyi 1997). Such strong assertions of 'archaeological' nature had even convinced R. Thapar (Social Scientist, Jan.-March 1996, p. 21). -- Elst 1999: 180 sqq. simply relies on these 'archaeological' data (and other writings) without questioning them on the ground of palaeontology. He even adduces the cave paintings at Bhimbetka "perhaps 30,000 years old" (Klostermaier, 1989: 35) while such paintings are extremely difficult to date so far and cannot be relied on, at present, as a major piece of evidence. In the end, while acknowledging the "paucity" (correctly: non-occurrence) of horse depictions and remains in the Indus Civilization, Elst thinks that it is *an explainable* paucity... "so that everything remains possible."

still others are simply too late.[186] At any rate, depictions of horses are altogether absent during the Indus period.[187]

Some of the earliest uses of the domesticated horse had been reported from the Copper Age site of Dereivka on the Dnyepr River (for riding, c. 4200-3800 BCE, now withdrawn)[188] and similarly, from the Copper Age site of Botai in N. Kazakhstan (c. 3300-2900 BCE.)[189] Some of the first attested remnants of primitive spoke-wheeled chariots and horse burials occur at Sintashta on the Tobol-Ishim rivers, east of the Urals (2100-1800 BCE.)[190] From there, a clear trail (Hiebert 1995, 192 sqq.) leads towards the subcontinent: from a somewhat unclear picture in the BMAC (Parpola 1988: 285, 288) to Pirak (horse figurines, c. 1700 BCE (Jarrige 1979),[191] bones in Kachi from 1700 BCE, the Swat Valley at c. 1400 BCE (painted sherds, horse burials, Stacul 1987).

In the subcontinent, the horse (along with the camel) first appears in the RV in literary context, and in Kachi in archaeological context at c. 1700 BCE. It is important to note that horse riding is not completely unknown to the RV; it is mentioned of the "horsemen", the Aśvin (Coomaraswamy 1941). It seems to have been common among the lower classes both among gods (Aśvin, Marut) and humans (Falk 1995) and may have been used for herding purposes while the nobility preferred chariots for sport and war. Without a proper saddle and stirrups, invented much later, warfare from horseback was not yet practical. However, just as clearly attested in Near Eastern documents of the second millennium BCE, chariots were used in warfare on favorable terrain (but certainly not while crossing mountainous territory!);[192]

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[186] For consideration are mentioned: from the Neolithic-chalcolithic levels of Hallur (1600 BC), early Jorwe (1400-1000 BC) and Late Jorwe (1000-700 BC), from the sites of Inamgaon in Maharashtra (Thomas 1988: 878, 883, Meadow & Patel, 1997). By this time, the domesticated horse was no longer rare (Thomas 1988: 878).-- Note that Thomas' material does not have measurements of the bones.

[187] For a fraudulous concoction of the picture of a horse on an Indus seal, see Rajaram and Jha (2000), exposed by Witzel and Farmer (2000). Elst (1999), as usual, swallowed Rajaram's initial, bold assertion of Harappan horses, hook and sinker -- in this case even Rajaram's artist's depiction of the half-horse (that is a bull!), referring (Elst 1999: 182) to Rajaram's hardly available book *From Harappa to Ayodhya*, Hyderabad 1997, see Frontline Nov. 24, 2000: 128 n.1. -- Recently, the picture of an Indus hemione (with typical short, stiff mane) was put on the internet as that of a horse, along with two already debunked horses (Frontline Oct./Nov. 2000) of the new species, to be called after its discoverer, *Equus asinus (?) rajarami*!

[188] The skeleton has only an carbon reading of c. 3000 BC; it shows evidence of a hard bridle bit; but the horse is unlikely to have been used for draught at this early period and was probably used for riding. This date has recently been withdrawn by D. Anthony (Antiquity 2000: 75), but has been supplemented by other early evidence for riding at Botai. -- Note, for a later period, that riding is a lower class occupation even in the RV, while the nobility drives chariots, see Falk, 1995, Anthony and Brown 1991; Anthony 1991, Telegin 1995.

[189] Zaibert 1993.

[190]   Anthony and Vinogradov 1995, Parpola 1995, Kuzmina 1994.

[191] It is of course an open question whether the inhabitants of Pirak were IA or, e.g., Drav. speakers; see the discussion of 'horse' words in Witzel (1999a,b) as well as a discussion of the languages of Sindh and Baluchistan. -- The Drav. and Mundas have their own words for the horse, and we can even assume different routes of the introduction of the horse (e.g. via Tibet and the Himalayan belt).

[192] Standard fare with autochthonists/Out of India advocates on the internet who continue to allege that I make "the Aryans thunder down the Khyber pass on their chariots" or, worse, their "on their Aryan *panzers"* (sic!), while I have not printed any such a folly anywhere. My *crime* was to have mentioned 'tanks' in a footnote (1995: 114 n. 74: "the thundering chariot, the tank of the 2nd millennium B.C."). --- We know that the RV clearly refers to a *rathavāhana* that was used to transport the quick but fragile, lightweight (c. 30 kg) chariot over difficult terrain, just as we do with modern racing cars. Note also that the wheels of such chariots would deform if left

and, the texts frequently refer to their use in sport. Horse riding is not important in the RV, and it is, so far, not found at all in the Indus civilization. If the horse had been an important animal of the Indus elite, one would also expect it in art - just as in Pirak or Swat, e.g., on the Indus seals. It does not show.

The occasional occurrence of horse riding in the RV and still earlier in the Ukraine (Anthony 1991, 1997, Falk 1995) cannot, of course, prove a date of the RV at 4000 BCE as early practices easily appear in *later* texts (see also §28-30). The use of the horse-drawn chariot in the RV at that early time is archaeologically impossible: even the heavy, oxen- drawn wagon evolved only in the late 4th millennium (first attested in Mesopotamia), and the chariot itself was developed only around 2000 BCE in the Ukraine/Ural area (and/or in Mesopotamia, Littauer and Crouwel 1996). The sudden appearance in South Asia of the (domesticated) horse and of the chariot remain clear indicators either of IIr/IA presence, or of their cultural influence on unknown, neighboring pastoralists who first brought the horse into S. Asia, -- in that case similar to what happened at the same time in Mesopotamia in the case of the Kassites and, somewhat less probable, the Mitanni.[193]

Autochthonists such as Sethna (1980, 1981, 1992) or Rajaram (2000) want to find horses and chariots in Indus inscriptions. However, this relies on interpretation of unknown symbols[194] and, in the case of Rajaram, even on actual fraud (Witzel and Farmer 2000). The original argument used by Sethna (1981) to date the Vedas before the Indus civilization, in autochthonous circles usually referred to as 'seminal,' 'clinching', etc., is the absence of the Indus commodity, cotton, in Vedic texts down to the Sūtras where *kārpāsa* 'made of cotton' is first attested. He wonders how the Vedic Indians would not have used cotton in the hot Indian climate. However, the texts regularly refer to woolen and flaxen garments. Wool is of course used in the cold Panjab winter. Absence of a word, such as 'rice' (see §23), in *sacred* (hieratic) texts does not prove its non-occurrence. With the same justification he could maintain that Vedic Indians did not yet fart since the non-hieratic, vulgar *pardati* is attested only in post-Vedic texts. The Iranians, again, have maintained the ancient custom (Avestan *pard,* IE **pṛd*) -- or did they learn it only after they left India?

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standing in assembled fashion; the chariots were disassembled and put together when needed. All of this corresponds with what we know from accounts of the avoidance by or difficulty of the use of chariots on uneven terrain from records of the ancient Near East and of Classical Antiquity. Nevertheless, the Veda also knows of a *vipatha* '[chariot used for] pathless [land]', attested in AV. Apparently, the autochthonists have not considered at all the role of horse-drawn chariots in sport and warfare of the Ancient Near East. Even a trip to the movies might help!

[193] Elst 1999: 178 concludes his somewhat superficial discussion of the Indo-Europeans and the horse, surprisingly, with an Out of India scenario: the Aryan 'emigrants' to Central Asia would have learned of the horse (he does not discuss the chariot, a clear indicator of time and location at c. 2000 BCE). They would then have transmitted this knowledge, and the actual animals, back home to India (while the RV supposedly does not know of Central Asia at all!) Occam's Razor applies. -- Again, I do not maintain, as some allege, that the Indo-Europeans were the 'sole masters' of horse riding and chariot driving. They were one of the several peoples from the Ukraine to Mongolia that made use of the new technology. The exact source and spread of this phenomenon is still under investigation by archaeologists. New technologies usually are taken over by neighboring peoples within a short time span: note the case of the Lakota (Sioux) who took over --from the Spanish-- the use of the horse and the rifle, a few hundred years ago, but remained Sioux in language and religion. But, just like the late-comer in their new hunting culture, the bison (they had been agriculturalists before the Little Ice Age) the horse, too, made it into their mythology!

[194] The spoked chariot wheels that Sethna wants to find on the Indus seals turn out to be, in most cases, oblong -- resulting in singularly bad transport for Indus merchants!

§22. Absence of towns in the RV

    The absence of towns and the occurrence of ruins (*armaka, vailasthāna,* cf. Falk 1981) in the RV poses another problem for the autochthonous theory. The urban Indus civilization disintegrated around 1900 BCE and the population reverted to village level settlements while expanding eastwards into Haryana/W. Uttar Pradesh (even with some smaller towns, Shaffer 1999).

    A *later* Vedic text (PB 25.10) tells of these ruins especially those located in the Sarasvatī (= Ghaggar-Hakra) region (cf. Burrow 1963, Rau 1983, Falk 1981). TB 2.4.6.8 actually says that inhabitants (of which areas?) had moved on (Falk 1981), and AB 3.45, one of the oldest Brāhmaṇa texts, speaks of the long wildernesses (*dīrgha araṇya)* in the west as opposed to a more settled east (Witzel 1987). This reflects reality: there are only a few iron age (PGW) time settlements in the Sarasvatī/Hakra area (Mughal 1997). TB may reflect some memory of the post-Harappan period,[195] when a considerable segment of the Indus population shifted eastwards after the loss of waters of the Ghaggar-Hakra to the Yamuna and Beas (Shaffer and Lichtenstein, 1995:138, Mughal 1997, Shaffer 1999).

    Some advocates of the autochthonous theory (Bh. Singh 1995) want to find in the references of the RV, with its large 1000-pillared houses, 100/1000-doored houses, etc. a reference to the Indus cities. Apart from the fact that 100-pillared houses have not yet been found in the Harappan civilization, such Ṛgvedic expressions are part and parcel of the traditional poetical hyperbole, where '100' or '1000' just mean 'many', and, amusingly, such expressions occur only in mythological contexts (*sahasradvār* 7.88.5; *sahasrasthūna* 5.62.6 (made of copper/bronze and gold, 5.62.7), 2.41.5; *śatadura* 1.51.3, 10.99.3). Who would deny the gods houses that are 100-1000 times bigger and better than human ones? Or, Indra his 1000 testicles? (6.46.3, 8.19.32). Occasionally, we even meet with metal forts -- but again only in myth. The same applies to 'boats with a hundred oars', RV 1.116.5. 'Ocean going' ships refer to the ships that travel through the (night time) sky, such as that of Bhujyu (RV 1.112.6, 116.3-5, 117.14, 119.4, etc., cf. the Avestan Pāuruua at Y 5.61, Oettinger 1988). All such items occur in comparisons or in mythology. In sum, all of this 'evidence' for RV Indus cities and oceanic trade (Frawley, S. P. Gupta, Bh. Singh, etc.)[196] is made of so many 'cities of the Gandharva', *gandharvanagara,* or 'fata morganas'. It is based on imaginary and erroneous RV interpretation, -- in short, on bad Vedic philology.

    Further, if the RV is older than 2600 BCE or even of 5000 BCE, how does it only know of *pur,* simple mud wall and palisade forts (Rau 1976, 1983, 1997), and not of the large, brick-built *human* houses, villages and cities of the Indus civilization? Note also that even in later

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[195] The question of post-Indus settlements that exceed the size of mere villages in Bahawalpur and the Panjab (Shaffer 1999) is in need of further attention: why is the RV silent about them? If iron is a late as it is said now (Possehl 1999), is the RV, too, so late as *not* to know these settlers any more, except for vague references such as those to the non-pastoral Kīkaṭa (RV 3.53)? Similar questions have to be asked about the overlap between the iron age PGW and the early YV texts (Witzel 1989).

[196] Gupta never translates the RV passages he quotes so that we can read into them whatever we want: a RV fort (*pur*) can be a modern town or a village (*pur*), etc. Frawley translates, but in the manner criticized here (n. 38, 204). He *believes* that his RV translations prove international trans-oceanic trade, but he never investigates what *samudra* or nau actually mean in the Veda (for which see Klaus 1986, 1989, 1989a).

Vedic texts, *grāma* does not mean "village" but only "wagon train (on the move), temporary settlement" (Rau 1997).

In short, the Indus cities are never mentioned; we only find, sometimes even named, ruins[197] and their potsherds (*kapāla*). Since an early, pre-Indus date of the RV is to be excluded on other, internal grounds (horses, chariots), these ruins as well as those on the Sarasvatī (PB) may refer to those of the Indus civilization.

However, both the Veda and the Avesta know of bricks: Ved. *iṣṭakā* (VS/TS), Avest. *ištiia, -ištuua* (cf. Tochar. *iścem,* Burushaski *diṣ.c.ík*). The similarity (but not, identity!) in sound allows to establish an isolated common IIr. root **išt,* an early loan-word that is supported by the divergent forms of the Tocharian and Burushaski words. The source, (an) unknown Central Asian language(s), with ***išt/ištš,* will be that of the Bactro-Margiana Archaeological complex (see Witzel 1999a,b) with its brick buildings and town-like settlements (of 2100 BCE). An Indus origin is unlikely, as the widely spread, slightly divergent form of the word in O. Iranian, Tocharian and Burushaski points to Central Asia, not the Indus.

§23. Absence of wheat and rice in the RV

The RV also does not mention the staple of the Indus civilization, wheat, found in the area since the seventh millennium BCE. It appears only later on, in Middle Vedic texts (*godhūma,* MS 1.2.8+). The form of the word is of clear Near/Middle Eastern origin (Hittite *kant,* O.Egypt. *xnd,* Avestan *gantuma*), but it has been influenced by popular etymology (Skt. *go-dhūma* "cow smoke"). It echoes, in its initial syllable, the Dravidian word for 'wheat' (Kannada *gōdi,* Tamil *kōti*) and its Pamir/Near Eastern antecedents, such as Bur. *gur* 'barley', 'wheat, wheat colored'.[198]

Just as in the much later case of tea/*chai,* the path of its spread is clear: Near Eastern **kant* /Pre-Iran. **gantum* has entered via the northern Iranian trade route (Media-Turkmenistan-Margiana/Bactria-Aratta/Sistan) and has resulted in Avest. *gantuma* and the later Iranian forms: M.Pers. *gandum,* Pashto *ɣanəm* < **gandūma*?, Yigdha *gondum,* etc. (Berger 1959: 40 sq, EWA II 498). It has been crossed with the PKartv., PEC **Gōl'e,* Burushaski/ Drav. form beginning with *g(h)o-* (for details see Witzel 1999a,b).

Instead of wheat, the Ṛgvedic people --and their gods -- ate barley (*yava*), but not yet rice which had already made its appearance in this region during the late Indus civilization (Kenoyer 1998). However, as is well known, ritual always is more conservative real life behavior, and the RV reflects ritual and is exclusively ritual poetry. The word for "rice" is of local S. Asian origin (Witzel 1999a,b) and ultimately perhaps Austric (note Benedict's Austro-Tai **boR[a]ts*). Just like wheat, rice is not yet found in the Ṛgveda, no doubt because this is a hieratic text that lists only the traditional food (also of the gods), barley.

Talageri 2000: 124 sqq. has misunderstood my reference (Witzel 1987: 176) to the absence of tigers and of domesticated rice in the RV --mostly grown, apart from the

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[197] See Falk 1981 and place names such as PB 25.10.18 *Sthūlārmaka* 'the large ruin' in Kurukṣetra; however, *Hariyupīyā* is a river, not Harappa as has been maintained by some historians for decades (it would have become something like **Harovī, *Haroī* in modern Panjabi).

[198] For the ultimate origin of the word, note also Bur. pl. *guriṅ/gureṅ* < **ɣorum* (Berger 1959: 43), *gurgán* 'winter wheat', and the connection with Basque *gari* 'wheat' < Proto-East Caucasian **Gōl'e* 'wheat', etc., Witzel 1999b. Harmatta (EWA II 499) thinks of an Anatolian **ghond[ū],* but cf. Klimov's Caucasian (Proto-Kartvelian) **ghomu.*

Himalayan regions, well east of Delhi throughout history -- by misconstruing a relative clause. (The matter is clearly indicated, however, in Witzel 1995: 101-2). Amusingly, he has therefore excoriated me for saying that there were no tigers in the Panjab then. (The absence of the tiger in the RV is more complex than that of rice and is in need of special attention; it may be due to an early conflation of the IIr/IA words for 'tiger', 'lion' and, maybe even 'panther').

In post-Ṛgvedic times (AV, YV), however, *vrīhi* is already the favorite food and an offering to the gods, though the gods *themselves* are still said to grow barley on the Sarasvatī (AV 6.30.1). The evidence of the cereals and culinary habits thus exactly fits the pattern of immigration: The speakers of Indo-Aryan (just as the Indo-Europeans: *yewo 'the (food) grass')[199] knew only barley and very gradually took over wheat and rice *inside* S. Asia.

If the RV had been composed in the Panjab in (pre-)Indus times, it certainly would contain a few notices on the staple food of this area, wheat. It is not found.

§24 RV class society and the Indus civilization

The autochthonous theory maintains that the Ṛgvedic Indo-Aryans were living in complex society, with mention of cities and numerous professions.[200] This, again, is careless philology: The 'complex society' of the RV is none other than the (Dumézilian) three class society of the Indo-Iranians, consisting of nobility (*rājanya*, later: *kṣatriya*), poet/priests (*brahmán, ṛṣi, vipra, kavi; ṛtvij, hotṛ, purohita*, etc., later: *brāhmaṇa*), and "the people" (*viś*, later: *vaiśya*). Very few occupations are mentioned in the RV, which is typical for a society of self-sufficient pastoralists. There are a few artisans such as the carpenter (*takṣan*), smith (*dhmātṛ, karmāra*), chariot-builder (*rathakāra*, attested only AV+).

It is also clear that the Ṛgvedic Ārya employed some sections of the local populations, i.e. the lower class, called Śūdra since RV 10.90, for agriculture (ploughman *kīnāśa*, RV, see Kuiper 1991, Witzel 1999a,b), and probably for washing (AV+, Witzel 1986), and especially for pottery (*kulāla* MS+, cf. W. Rau 1983). Sacred vessels were made by Brahmins in the most archaic fashion, without the use of a potter's wheel (as is still done for everyday vessels in the Hindukush!) and without change in style; such pottery is therefore undatable by style (without thermo-luminescence methods), if ever found. Vedic everyday, household vessels were made in *local* style by Śūdra workmen. (Note, e.g., the continuation of Indus style motives in the Cemetery H culture -- but with new cultural traits, that is, cremation and urn burial along with urn paintings expressing the Vedic belief in a homunculus 'soul', sketched inside the peacock (Vats 1940, Schmidt 1980, Witzel 1984, Falk 1986). All these are occupations are such that no member of the three Ārya classes would voluntarily undertake, as proud pastoralists.

As has briefly been discussed above, I neglect here all further discussions of a 'complicated class system, castes, foreign trade, elaborate palaces', and the like, as they are all based on bad Ṛgvedic philology. Typically, such assertions are made, while quoting Sanskrit sources from the RV (Bhagavan Singh 1995, Frawley forthc., etc.), without translation or without philological discussion, so that everyone is free to understand what one likes to see in

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[199] Avest. *yauua*, N.Pers. *jav*, cf. Osset. *jew, yau* 'millet'; for their Indo-European predecessors, note Hom. Greek *zeá*, Lith. *jawai* 'grain'; the word clearly is derived from *yu 'to graze', see now EWA *s.v.*

[200] Bh. Singh 1995; especially 'detailed' in this respect, Malati Shendge 1977 (e.g., with the "Indus official" Rudra in charge of mountain troops and house numbers!).

these passages. A Ṛgvedic 'boat with 100 oars' is not a kind of Spanish galley but clearly belongs to the realm of the gods, to mythology, -- and to modern, autochthonous myth making.

§25. The Sarasvatī and dating of the RV and the Brāhmaṇas

The disappearance of the Sarasvatī,[201] the modern Sarsuti-Ghaggar-Hakra river and dry river bed in the desert on both sides of the present Indian/Pakistani border, is often used by autochthonists as a means of dating the RV. It is well known from Brāhmaṇa texts that the Sarasvatī then disappeared in the desert (PB 25.10, JB 2.297 : Caland § 156). Landsat pictures (Yash Pal 1984) are interpreted by some as showing the drying up of this ancient river at various dates in the third millennium; Kak insists on 1900 BCE, Kalyanaraman (1999: 2) on 1900-1500 BCE (in 1999) now: 1700/1300 BCE).[202] However, Landsat or aerial photos by themselves cannot determine the date of ancient river courses; local geological and archaeological investigations on the ground are necessary. They still have not yet been carried out sufficiently, though the Hakra area has been surveyed archaeologically on the Pakistani side by M.R. Mughal (1997), and geological data are now also available in some more detail for the Indian side (Radhakrishnan & Merh 1999, S.P. Gupta 1995). They establish *several* palaeo-channels for this river, that easily changed course, like all Panjab rivers flowing on these flat alluvial plains. Which one of these courses would fit the Indus period and which one the Ṛgvedic period *still* needs to be sorted out. Choosing an arbitrary date of 1900 or 1400 BCE is useless in order to fix the RV (well) before this date.

The upper course of the Ghaggar, however, is not dry even today, as some scholars state; it is still known as the small river *Sarsuti*. Also, it has been long known, and is easily visible on many maps, that the lower, dry bed of the Sarsuti (Ghaggar) continues well beyond the Pakistani border as Hakra (Wilhelmy 1969, Witzel 1984, 1987), and it *seems* to continue further south as the Nara channel in Sindh, finally emptying into the Rann of Cutch (Oldham 1886, Raverty 1892, Witzel 1994). However, there is a *playa* next to the long gap in the lower course of the Hakra river and the Indus, covered by sand dunes near Fort Derawar, east of Khanpur, Pakistan. If the Sarasvatī indeed ended there in an inland delta (Possehl 1997), the Nara channel would rather represent the lower course of the Sutlej (or be a branch of the Indus).

It must be underlined that a considerable segment of the Harappan population shifted eastwards from the Indus and the Ghaggar-Hakra the post-Harappan period and built new settlements[203] in the Eastern Panjab and Haryana/UP. Shaffer and Lichtenstein (1995:138)

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[201] Yash Pal 1984, now Radhakrishnan and Merh 1999.

[202] Elst (1999:137) makes this into "great catastrophe in about 2000 BC, when the Sarasvati river dried up and many of the Harappan cities were abandoned... " [While the correct date(s) of the drying up of much of the "Sarasvatī" has not yet been determined!] "This catastrophe triggered migrations in all directions, to the Malabar coast, to India's interior, and east, to West Asia by sea (the Kassite dynasty in Babylon in c. 1600 BCE venerated some of the Vedic gods), and to Central Asia". I wonder where the evidence for such (e)migrations is to be found. The only archaeologically attested one is the move, by the Indus people, eastwards into Haryana/Delhi area, by c. 1400 BCE, see Shaffer and Lichtenstein 1995, Shaffer 1999, see also §22.

[203] Allchin et al. 1995: 37, with a typical development at Bhagawanpura, Haryana, that might reflect Indus/IA/PGW type populations: many-roomed houses of brick of the post-urban period, then single-roomed circular huts of timber and thatch, then many-roomed brick/pressed earth houses; the last two stages with increasing PGW.

attribute this in part to the loss of waters of the Ghaggar-Hakra to the Yamuna and Beas (Mughal 1997).

The basic literary facts, however, are the following: the Sarasvatī is well known and highly praised in the RV as a great stream. Once it is called the only river flowing from the mountains to the *samudra* (RV 7.95.2). *Samudra* indicates a large body of water (Klaus 1986), either the terrestrial ocean, or a mythological ocean (at the end of the world or in the night sky, Witzel 1984, cf. RV 7.6.7!), or a terminal lake, or just a "confluence of rivers" (RV 6.72.3).[204] Given the semi-mythical nature of the Sarasvatī, as goddess and as mythical river in the sky or on earth, the RV passages are not always clear enough to decide which one is intended in each particular instance (Witzel 1984). However, the Brāhmaṇa texts (JB 2.297, PB 25.10) clearly state that the Sarasvatī disappears or "dives under" in the desert at a place called *vinaśana / upamajjana*. (Later texts such as the Purāṇas mythologize that it flows underground from there up to the confluence of the Yamunā and Gaṅgā at Prayāga/Allahabad, something that is based on an old, general Eurasian concept, see Witzel 1984).

The Sarasvatī region, the post-Ṛgvedic *Kurukṣetra*, comprises the land between the Sarasvatī (mod. Sarsuti, Ghaggar) and the Dṛṣadvatī (mod. Chautang) to its east. It does *not* include the lower Sarasvatī (mod. Hakra) which is occasionally referred to as *Parisaraka, Parisrāvatī* (VādhB 4.75), *Parīṇah* (PB 25.10@##) 'the area surrounded (by the Sarasvatī)' (Witzel 1984), a wording that *clearly* indicates delta-like configurations (*playa*), with terminal lake(s) (*samudra*).

In the dry bed of the Hakra many potsherds (*kapāla*) used in ritual could be found (PB 25.10); they belonged to the given up settlements (*arma, armaka,* Falk 1981) of the late Harappan and post-Harappan period (cf. above, TB 2.4.6.8). Indeed, the dry bed of the Ghaggar-Hakra still is lined with Harappan sites (and cluttered with millions of *kapāla* sherds, Mughal 1997). But many of these settlements are situated *on* the actual *flood plain* of the Ghaggar-Hakra, which speaks against an enormous river during the Harappan (or the *supposed* 'pre-Harappan Ṛgvedic') period. In fact, the estimates of archaeologists on the exact date of the drying up of much of the Sarasvatī differ considerably. Mughal proposes that the Hakra was a perennial river in the 4th and early 3rd millennium BCE and that it had dried up about the end of the second.[205] Other dates range from 2500-2200 BCE to 2200-1700 BCE, and Francfort (1985 sqq.) thinks of a much earlier period. It is now supposed that the Sarasvatī lost the mass of its water volume to the nearby Yamunā due to tectonic upheaval (Yash Pal 1984; Radhakrishnan and Mehr 1999). Even then, the *old* Sarasvatī-Sutlej can *never* have been larger than the Indus, the only other river that is highly praised in the RV. The question thus is, why the Sarasvatī actually *is* praised that much?

RV 7.95.2, a hymn of the middle Ṛgvedic period, indeed speaks of the Sarasvatī flowing to the *samudra.* However, this is not unambiguous, due to the various meanings of

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[204] The meaning of *samudra* must be established well; see, however, Klaus 1986. Note that RV 6.72.3 speaks even of the (three or more!) *samudra*s of the rivers, *samudrāṇi nadīnām*. Note also that the AV 11.5.6 has an *uttara* 'northern/upper' ocean (Witzel 1984). Finally, compare also Avest. Y. 65 where the Iranian counterpart of the Sarasvatī, *Arəduuī,* flows, somewhat similar to the Sarasvatī and the later Epic Gaṅgā, from a mountain, *Hukairiia,* to the "Lake" *Vourukaṣa,* which indicates the Milky Way (Witzel 1984), (and then further down to earth).

[205] Possehl 1993: 85-94.

the word. Even then, the Sarasvatī may never have been as mighty a contemporary river as the RV wants to make us believe, because, as is well known, RV style is generally quite hyperbolic. In book 7, the Ṛṣi Vasiṣṭha, an immigrant from west of the Indus, praises the local Sarasvatī area of his patron Sudās after the victory in the Ten Kings' Battle. Whether the immigrant Vasiṣṭha was from the Iranian area of *Haraxᵛaitī* (= Sarasvatī, Arachosia) or not, he may have echoed the praise of the *ancient* Sarasvatī, that is the local S. Avestan *Haraxᵛaitī* or the Milky Way (Witzel 1984), or he may just have spoken in the hyperbolic style of the RV.

These textual data do not inspire confidence in the categorically stated autochthonous theory that the RV *proves* a mighty Sarasvatī, flowing from the Himalayan mountains to the Indian *ocean*.

However, a neglected contemporary piece of evidence from the middle RV period, believed to have been composed by Viśvāmitra, the opponent of Vasiṣṭha, is found in RV 3.33. Based on internal RV evidence, this hymn describes a situation of *only a few moths or years before* RV 7.95.2 (with the Sarasvatī 'flowing from the mountains to the *samudra'*, whatever its meaning*!*). The RV books 3 (Viśvāmitra) and 7 (Vasiṣṭha) both represent a relatively late time frame among some five known generations of the Ṛgvedic chieftains of the Middle RV period, chiefs that belong to the noble Bharata and Pūru lineages. The autochthonous theory overlooks that RV 3.33[206] already speaks of a *necessarily smaller* Sarasvatī: the Sudās hymn 3.33 refers to the *confluence* of the Beas and Sutlej (*Vipāś, Śutudrī*).[207] This means that the Beas had already captured the Sutlej away from the Sarasvatī, dwarfing its water supply.[208] While the Sutlej is fed by Himalayan glaciers, the Sarsuti is but a small local river depending on rain water.

In sum, the middle and later RV (books 3, 7 and the late book, 10.75) already depict the *present day* situation, with the Sarasvatī having lost most of its water to the Sutlej (and even earlier, much of it also to the Yamunā). It was no longer the large river it *might* have been before the *early* Ṛgvedic period.

The Ṛgvedic evidence, supposing the Indologists' 'traditional' date of the text at c. 1500-1000 BCE, also agrees remarkably well with the new evidence from Bahawalpur/Cholistan (Mughal 1997) which indicates that the area along the lower Hakra (Sarasvatī) was abandoned by its people who moved eastwards after c. 1400 BCE. The area was not settled again until well into the iron age, with the introduction of the Painted Gray Ware culture (PGW) in the area at c. 800 BCE. At that time, we indeed hear of sparse settlements in the west (AB 3.45). This also agrees with the scenario developed earlier (Witzel 1995): an early immigration (c. 1700 BCE - 1450 BCE) of the Yadu-Turvaśa, Anu-Druhyu in to the Panjab, when there possibly still was a somewhat "larger Sarasvatī" (Mughal 1997, with details), followed by the immigration of the Bharata tribe (from across the Indus, JB 3.237-8 : Caland § 204) only after the major part of the Sarasvatī waters had been captured by the Beas (and, before, a large part of it by the Yamunā). This scenario, consistent with the geological, archaeological and textual evidence is in striking contrast to that of the autochthonous theory.

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[206] In the new autochthonous version of RV history (Talageri 2000) this is the oldest book of the RV, -- which would make the Sarasvatī, very much against the wishes of the indigenists, a *small* river in the *early* RV period! As usual, Occam's Razor applies.

[207] Differently from the map in Kenoyer (1995: 245) where the Sutlej, Sarasvatī and Ur-Jumna still form *one* river which indeed flows from the Himalayas to the ocean (called Nara in Sindh).

[208] While in the still later hymn, RV 10.75, the *Vipāś* (Beas) is altogether missing and might have been substituted by the *Śutudrī* (Satlej), i.e. the joint Vipāś-Śutudrī (unless the Beas, unlikely, is called *Marudvṛdhā* here).

The area around the Sarasvatī also was not, as (some of) the autochthonous theorists maintain, the *center* of Vedic culture or of the whole of the Indus civilization, at least not during the whole span of this civilization. As Possehl (1997) shows, the clusters of settlement gradually moved eastwards, from Baluchistan/Sindh to Haryana (Possehl 1997), and this movement continued (Lichtenstein and Shaffer, 1999) into Haryana/U.P. even after the end of the Indus civilization in c. 1900 BCE. (Even then, the Sarasvatī area is not specially favored). During the RV period, there was no clear political, cultural center, either; the diverse, 30-50 tribes and clans were spread out over all of the Panjab, and there was no central authority. The situation in the Indus period was equally diffuse, with at least five major cities: Harappa, Mohenjo-Daro, Ganweriwala, Rakhigarhi, Dholavira in Cutch.[209] Even during its heyday, thus, there were several concentrations but no *central area*. It cannot be assumed that because there are many (c. 400) Indus settlements in the Ghaggar-Hakra are, this indicates *the center* of the Indus civilization. Rather, this concentration is due to something very obvious -- though not mentioned by advocates of a renamed "Indus-Sarasvatī civilization"-- that is, to the fact that the lower Sarasvatī area is "fossil": it has not changed, since the Indus period, in geomorphology, it has hardly ever been settled since by more than a few people, and, most importantly, it has neither received new alluvium nor has it been subject to ploughing.

The area around the upper Sarasvatī, the later Kurukṣetra, instead of being of central importance all through the older RV, is singled out only in the *middle* and *later* parts of the RV, in books 3, 7 (and 1, 10 etc.) as the 'best place on earth' (RV 3.53.11, Witzel 1995), as this had become the territory of the victorious Bharata tribe under Sudās (and, it may be added, also one of the major settlement areas of the post-Indus culture).

According to the autochthonous theory, the Sarasvatī dried out by 1900/1500 BCE, and the Brāhmaṇas which mention its disappearance must therefore be dated about that time. All of this does not fit the internal evidence, is based on bad philology and shows, once again, the rather *ad hoc*, selective methods used by advocates of the autochthonous theory. For, the first appearance of iron, the 'black metal' (*kṛṣṇa/śyāma ayas*) in S. Asia, well known to the Brāhmaṇa style texts, is only at c. 1200 BCE (Chakrabarti 1979, 1992, Rau 1974, 1983, cf. now, however, Possehl-Gullapalli who point to 1000 BCE). But, iron is already found in texts much *earlier* than the Brāhmaṇas (i.e. AV, and in the YV Saṃhitās: MS, KS, TS; however, not yet in the RV). This fact is frequently misunderstood by historians and archaeologists who simply quote the older RV translations that render *ayas* by 'iron' while it means 'copper' or maybe, also 'bronze' (Rau 1974, 1983). It was only in the post-RV period that copper was called *loha* 'the red (metal)' (VS 18.13, TS 4.7.5.1, ŚB 2.6.4.5, 13.2.2.18, etc.), often in opposition to the 'black metal'. To date Brāhmaṇa texts at 1900 BCE (see below on astronomy, §28-30) is simply impossible.

At the bottom of the sudden popularity of the Sarasvatī is of course the nationalistic wish to have the "center of the Harappan Civilization" within the boundaries of India, along a "Vedic" river the Sarasvatī -- as if such recent boundaries played any role in 2600-1900 BCE! Unfortunately for such chauvinists, neither are the majority of the 'Sarasvatī' sites along the Ghaggar in India, but along the Hakra in Pakistan. Nor does the name 'Sarasvatī' apply for the period in question. The old designation of the Sarsuti-Ghaggar-Hakra, later renamed as the Vedic Sarasvatī, seems to have been the substrate name *Višampaḷ/ž or Vipaḷ/ž* (Witzel 1999).

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[209] For a full list of settlements see now Possehl (1999) and note the theory of a handful of separate Indus 'domains'.

§26. Harappan fire rituals?

B. B. Lal and others claim to have discovered fire altars in the early and later stages (at least 2200 BCE, B.K. Thapar 1975) of the Harappan site of Kalibangan (Lal 1984, 1997: 121-124), and similarly, at Lothal. Some of these fire places are in a domestic and some in a public context: the latter are aligned on a raised platform in a row of seven, facing East, and near a well and bath pavements suggesting ceremonial(?) bathing. Some archaeologists, even including some who accept a version of the immigration theory such as R. Allchin, regard them as similar to, or identical with, the seven *dhiṣṇya* hearths of the post-Ṛgvedic, 'classical' Śrauta ritual. However, it should have raised some suspicion that 'fire rituals' are now detected at every other copper/bronze or even Neolithic site in northern and western India.

The amusing denouement is evident in Lal 1997:121, (plate XXXA) itself: "within the altar stood a stele made of clay". This kind of "stele" is still found today in modern fire places of the area -- it serves as a prop for the cooking pot.

What is indeed visible at Kalibangan (photos in Allchin 1982, Lal 1997: plate XXXIIIA, cf. Banawali pl. XXXVIA)? There are seven(?) fire places, three(?) destroyed by later construction. They are closely aligned next to each other and face a brick wall. Nothing of this, including the nearby brick-built bathing places, fits any recorded Vedic ritual, neither that of the RV nor of the later (Śrauta) ritual. The RV knows only of 1-3 fires, and in Śrauta ritual we find the three fires arranged in a typical, somewhat irregular, triangular fashion. The seven *dhiṣṇya* fire altars of the complicated *post*-Ṛgvedic Soma ritual are *additional* fires, which are placed east of the three main fires on the trapezoid Mahāvedi platform (Staal 1983). This feature, however, is not met with at Kalibangan either. It also does not fit the Vedic evidence, but that of a regular kitchen, that animal bones are found in some of the supposed fire altars. Further, Vedic fire altars are not apsidal as the fire places at Kalibangan and Banawali. At best, these are independent and untypical *precursors,* in a *non-Vedic* context, that were adapted into the later Śrauta ritual as the Soma *dhiṣṇya*s. However, this is entirely impossible to prove. Such proof would have to come from a study of the (so far hypothetical) interrelations between certain features of the Indus religion and the Śrauta ritual. The matter underlines how careful archaeologists should be in drawing conclusions about religion and ritual when interpreting material remains.

In short, the Kalibangan hearths do *not* represent Vedic ritual as we know it from the large array of Vedic texts. They may be nothing more than a community kitchen.[210]

§27. Cultural continuity: pottery and the Indus script

Advocates of the autochthonous theory also underline that the lack of dramatic change in the material culture of northern South Asia indicates an unbroken tradition that can be traced back to c. 7000 BCE without any intrusive culture found during this period.[211]

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[210] Thus Jamison and Witzel, (written in 1992 but still in press; however, see soon: http://www.people.fas.harvard.edu/~witzel/vedichinduism.htm), and similarly now R. S. Sharma 1995.

[211] Shaffer and Kenoyer argue for a continual, 'organic' archaeological evolution reflecting indigenous cultural development from pre- to proto-historic periods without intrusions in the archaeological record from the northwest (or anywhere else). However, recent excavations seem to indicate, for example, a strong BMAC

Archaeologists such as J. Shaffer and M. Kenoyer stress this remarkable continuity as well. Shaffer (1995, 1999) summarizes: "The shift by Harappans [in the late/post-Indus period] is the only archaeologically documented west-to-east movement of human populations in South Asia before the first half of the first millennium BC."

The advocates of the autochthonous theory therefore conveniently conclude that there has been no "Aryan invasion." However, as has been discussed above (§8-10) the Vedic texts themselves speak of various types of transhumance and migration movements.

On the other hand, there is, indeed, some degree of continuity from the late Indus civilization, that was carried over into the early Gangetic tradition. One clear example is the continuity of weights (Kenoyer 1995: 224, 1998). Many other cultural traits (such as pottery) have been carried over in the same fashion.

This, of course, also tends to explain why the "Vedic" (or IA) tradition is so little visible in the *archaeological* record so far. We still are looking, in the Greater Panjab, for the "smoking gun" of the horse, horse furnishings, the spoke-wheeled chariot, Vedic ritual implements, etc. However, at least on the fringes of the subcontinent, in the Kachi Plain of E. Baluchistan/Sindh and in the Gandhara Grave Culture of Swat, we find some indications, by mid-second millennium BCE, in the first horses of South Asia, and horse sacrifice (Allchin 1995, Dani 1992).

However, if one would again try to think through the autochthonous theory that stresses the strong continuity in Indian cultural development from c. 7000 BCE onwards, and would suppose, with them, that the RV preceded the Indus Civilization, one is faced by a paradox: how is possible that Ṛgvedic features such as horse races, preponderance of cattle raising, non-use of wheat (and rice), lack of permanent settlements, complicated Soma rituals without temples, cremation burial, etc. all of which hypothetically *disappear* completely during the Harappan period and *re-emerge* in the post-Ṛgvedic YV Saṃhitā, Brāhmaṇa and Upaniṣadic periods of the Gangetic epoch? This is yet another strange *non sequitur* which does not fit in with established cultural and textual sequences. In sum, the assertion that the RV is *older* than the Indus civilization does not work: there were no horse-drawn chariots yet at the beginning of the Indus period (2600 BCE) in the Greater Panjab or anywhere else, but they emerge only around 2000 BCE in the Ural area and in Mesopotamia.

Continuity of the Indus script

The autochthonous theory maintains that the Brahmī script of Asoka (3rd c. BCE is derived from the Indus script (Rajaram and Jha 2000). However, this is a complex logographic script with at some 400 (Parpola 1994), or rather some 600 signs (Wells 1998), many of which are used only in certain sign combinations, typical for logographic scripts such as Chinese or Japanese. The very number of signs makes an interpretation as alphabetic or syllabary script impossible.[212] Some of them were probably used as rebus symbols, just as is the case with all early logographic scripts from Egypt to China: the sounds of one word were used to indicate another one with same or *similar* pronunciation but with a different meaning, such as *pair/pear//bear/to bear/bare, two/too//to/do, their/there/they're,* etc.

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influence in late-Harappan (including several statues such as the so-called Priest-King), before its decay at 1900 BCE.

[212] For a survey see Possehl 1996; for the discussion of a recent, particularly blunt and fraudulous attempt (Rajaram and Jha 2000) see Witzel and Farmer in the Indian news journal, *Frontline*, Oct. 13, 2000 and discussion in subsequent issues.

Unlike the Indus script with its logograms, the Brahmī script, on the other hand, is a real alphabetical script (on phonemic principles) with only one quasi-syllabary feature: as in Devanāgarī, short -*a* remains unexpressed. In the North-West of the subcontinent, Brahmī had a predecessor, the Kharoṣṭhī script. Both go back, directly or indirectly, to the Aramaic script (Falk 1993, Salomon 1995), which was widely used in the Persian empire, and even by Asoka, in Afghanistan. Kharoṣṭhī, and Brahmī even more so, have been adjusted extremely well to represent the Indian sound system, certainly under the influence of traditional Brahmin phonetic science.

If the autochthonous theory were right, the descent of Brahmī from the Indus script would resemble that of the early Semitic alphabets from Hieroglyphic Egyptian. However, in the case of Egyptian we know the pronunciation of the Hieroglyphic logographs, while no accepted decipherment has emerged in more than half a century of study of the Indus script (Parpola 1994, Possehl 1996).[213] Given the c. 600 signs of the Indus script, it is of course very easy to find similarities in the 50-odd, very regularly shaped, geometrical signs of the Brahmī script (*ka* is a simple +, *ṭha* is: o, etc.). Even if there indeed was an initial carry-over of remnants of the Indus script into the post-Indus period (Kenoyer 1995: 224) there is no sign of any *continuity* of the use of the script before the first inscriptions in Brahmī in the middle of the third c. BCE.[214] The script simply vanished, like the Maya script, when its practical use for administration and/or business disappeared (Allchin 1995, Possehl 1996). In addition, writing and script are not mentioned in the Vedic and early Buddhist texts (v. Hinüber 1989). Typically, Pāṇini, probably a subject of the Persians in Gandhāra, has two *foreign* names, the Persian name of 'script' *dipi* (Pers. *dipi* [δipi] < Elamite *tip/tup*) as well as its regular development in East Iranian (*lipi*), from which the Skt. and Pkt. terminology is derived.

In short, just as in many other areas of S. Asian culture, the disappearance of writing is witness to the large gap between the well-organized urban civilization of the Indus culture at c. 1900 BCE, its village-like local successor cultures in E. Panjab/Haryana etc., the subsequent superimposition/adaptation of pastoral Vedic culture, and finally, the newly emerging Gangetic urban culture of pre-Mauryan times in the 5th century BCE.

VEDIC TEXTS AND SCIENCE

§28. The "astronomical code of the RV"

One of the most arresting claims of the autochthonous theory is that of an astronomical code in the organization of hymns of the RV (Kak 1994), which he believes to establish a tradition of sophisticated observational astronomy going back to events of 3000 or

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[213] I leave aside the question of decipherment. There is a new attempt about once per month now, increasingly claiming that the texts are in early Sanskrit. Non-Sanskritic ones include, e.g., R. Mathivanan 1995, Arun Pathak and N.K. Verma 1993; both find continued use of the (unchanged!) Indus script, after a lapse of evidence spanning some 4000 years, but exemplified by photos, on the house walls of the Austro-Asiatic Santals in S. Bihar.

[214] Coningham 1995 maintains an early --improbable--date for Brahmi at c. 500 BCE for Sri Lanka. This single, early date probably is due to unclear stratigraphy; the singular find of inscribed materials is situated barely below a much later level.

4000 BCE[215], a few millennia after the Aryans' *hypothetical* arrival in the seventh millennium BCE (Kak 1994: 20-22); or more specifically, that certain combinations of numbers enumerating the syllables, verses and hymns in the Ṛgveda coincide with numbers indicating the periods of planetary motions.

However, to begin with, Kak's discovery is derived from the traditional ordering of the hymns and verses of the RV, a schematic one of the post-Ṛgvedic period most probably executed in the Kuru realm of the Eastern Panjab/Haryana at c. 1200/1000 BCE (Witzel 1997, 2001); it was canonized a few hundred years later by an Easterner, Śākalya, during the late Brāhmaṇa period (roughly, 700-500 BCE) -- and that is the version Kak uses! Other versions of the RV differ slightly; even a text contemporary with Śākalya, ŚB, says that the Purūravas hymn (RV 10.95) had 15 verses while our RV has 18. Which size and ordering of the text to follow, then?

The real question, of course, is: why should anybody order one's texts according to some astronomical patterns? Rather, what kind of method would present itself to a people with a strong, well-trained memory but without the use of script? One could think, for example, of a strictly metrical pattern (as is indeed used in the Soma hymns of RV 9 or the Avestan Gāϑās), or one according to the use of the hymns in ritual (as is used by the Yajurveda). None of the two is the one followed in the bulk of the RV. Instead, as has been well known for more than a hundred years (Oldenberg 1888), and indeed since Vedic times(!), the RV is organized in three levels: according to authors, i.e. poets' clans (the 'family books', RV 2-7, and 8), deities (hymns to Agni, Indra, then others), and according to meter (hymns with longer meters come first). The core 'books' of the RV (2-7) are arranged from short books to long ones, and, conversely, inside each book according to a descending order numbers of hymns per deity, and numbers of verses per hymn. All of this is not mentioned by Kak; for details on the exact scheme and the -- only apparent -- disturbances[216] in it, see Oldenberg (1888, Witzel 1997). In sum, if one knows -- just as modern practice still prescribes-- the author, the deity and the meter, one knows where a hymn is to be found inside the core section (RV 2-7) of the RV collection. This is a simple but very effective method in an oral tradition without script.

Interestingly, Kak joins this theory with observations about the piling up of bricks of the Agnicayana altars. It certainly cannot be doubted that the altar is identified, in the typical fashion of the post-Ṛgvedic Brāhmaṇa texts, with Prajāpati, the divine sponsor of the ritual and the year, and that some calculations are connected with that. However, there was no Agnicayana yet at the time of the RV. Even the Mantra collections used for this ritual are late and form a third layer in the collections of the post-Ṛgvedic Yajurveda Saṃhitā texts; the same it true for the discussion of the ritual in the Brāhmaṇa style texts. Any combination of the numbers of bricks in the Agnicayana with the order and number of hymns and Mantras of the RV therefore is not cogent, to begin with.

To find astronomical reasons behind this arrangement requires extra-ordinary ingenuity on the part of the *original*, contemporary composers and arrangers of the RV -- or the decipherer, S. Kak. That they should constitute an *original* Ṛgvedic "astronomical code", -- based on the post-Ṛgvedic(!) arrangement of the RV-Saṃhitā and the later, post-

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[215] Cf. also the discussion by Elst 1999: 96 sqq.

[216] Which greatly irks Talageri (2000) who simply relies on the superficial outward appearance of the *present* (Śākala) RV; he is simply ignorant of the history of Ṛgvedic philology of the past 150 years and relies just on Griffith's outdated and similar uncritical English translation of the late 19th century and on some Skt. word indexes of the RV (for details, Witzel 2001).

Ṛgvedic(!) construction of the Agnicayana fire-altars -- is simply impossible. It also does not help the scheme that the knowledge of this code is said to have disappeared very shortly after the composition of the texts.

Further, Kak's scheme suffers, even if one takes its rather involved numerical schemes for granted, from inconsistency, such as the arbitrary use of multiplication factors that deliver the desired results for the various courses of the planets (which are not even attested in Vedic texts, see M. Yano, forthc.). In fact, references to astronomical data in the RV are generally very vague, and limited, as in other ancient cultures, to a few facts of direct observation by the naked eye (Pingree 1973, 1981, Witzel 1972, 1984, 1986, Plofker 1996, Yano forthc.).

More details could be added. To mention just the most elaborate one, K. Plofker's (1996) discussion of Kak's attempt in the section "Probabilistic Validation" (1994: 106-107). This section intends to prove that the presence of planetary period numbers in the Ṛgvedic hymn number combinations (containing 461 distinct integers ranging from 43 to 1017), derived from all ten books of the RV, cannot be coincidental. As Plofker shows, "the set of values generated from sums of a given set of numbers is generally *not* uniformly distributed over the interval it spans; as a rule, there will be a few very small sums and a few very large ones, but most will cluster about the middle of the interval. In this example, out of the 461 hymn combination numbers, no fewer than 320 fall within the range 301--800 containing most of the planetary period constants. This, combined with the fact that Dr. Kak (by his own account; p. 105) permits errors of at least *pm 1* in his matching of numbers, means that the high proportion of matches has no statistical significance whatever."

This mathematical demonstration would not even have been necessary because of the derived, secondary nature of hymn numbers in Śākalya's redaction of the RV (see above). Or, in the same vein, when it is alleged by Kak that the combined number of hymns in the fourth, sixth, eighth, and ninth books of the RV was chosen to be 339 because that number is roughly equivalent to "the number of disks of the sun or the moon to measure the path across the sky... [or] sun-steps" (Kak 1994: 100, accepted by Elst 1999: 110), one must immediately note, not only that RV 9 is a late book (Oldenberg 1888, Proferes 1999), but that these books have the following *additional* hymns (Oldenberg 1888): 4.57-58; 6.74-75; 8.96-101, 9.112-113, not to mention quite a few additional hymns *inside* these very books. This simple observation renders Kak's whole scheme numerically impossible.

In short, the whole matter boils down to over-interpretation of some facts that are internally *inconsistent*.[217] *Non licet.*

§29. Astronomy: the equinoxes in ŚB

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[217] Note that similar claims have been made for the Bible and other ancient texts. As it has been said: select some significant numbers relating, e.g. to the (19th c.) Washington monument, add some astronomical facts and --lo, behold-- unforeseen relations of the monument with the earth, space and time emerge!

Vedic astronomy has been discussed[218] since Weber (1860), Thibaut (1885), Tilak (1893), Jacobi, Oldenberg and Whitney[219] -- all of them writing well before the discovery of the Indus civilization, at a time when nothing of Indian prehistory was known before the supposedly firm date of the Buddha.[220] Some passages in the ŚB have been under discussion since then that seem to refer to the equinoxes, and would indicate the date of observation of these celestial phenomena. ŚB 2.1.2.3 seems to say that the spring equinox is in the asterism Kṛttikā: *kṛttikāsv agnī ādadhīta ... etā ha vai prācyai diśo na cyavante | sarvāṇi ha vā anyāni nakṣatrāṇi prācyai diśaś cyavante. ... saptarṣīn u ha sma vai pura rkṣā ity ācakṣate.* "One should found one's fires under the (moon house of the) Kṛttikās... These, they do not deviate from the eastern direction. All other moon houses, they deviate from the eastern direction... Formerly, one called the Saptarṣis 'the Bears'." This statement, if taken for a literal description of the 'immobile' position of the Pleiades, is possible only for the third millennium, at c. 2300 BCE (Kak even has 2950 BCE, cf. Elst 1999: 96) . Then, the Pleiades were at the equinox point, some 60 degrees off today's position due to precession (for details see Achar, *EJVS* 5.2, 1999).

The basic question is, of course, whether such astronomical references in Vedic texts must be taken at face value, i.e. literally. The above passage is followed by a set of other ones which allow setting up the fires at *other* times, most of which are motivated and justified, like this one, by inherent Brāhmaṇa texts' concerns and logic. Further, astronomical observations in the Vedic texts are of a more general nature, and are clearly based on what is easily observable with the naked eye over the course of a few years (Pingree 1973, 1981 Plofker 1996, Yano forthc., Witzel 1972, 1984, 1999c). If one takes this conclusion as one's baseline, some statements in the Babylonian text MUL.APIN are of interest. The text is probably to be dated in the late second millennium (Pingree 1998), thus earlier than ŚB but much closer to it than the supposed date of the Kṛttikā observation in the third millennium. MUL.APIN says more or less what ŚB does in the section under discussion, namely that the Pleiades are in the east and that Ursa Maior is in the north. And that would be the end of the whole question.

However, even if one admits that the sentences quoted above refer to *contemporary* observation and have been transmitted as such over several millennia, a serious problem remains: the advocates of the autochthonous theory, unwittingly, commit the rather common but no less serious mistake of dating a text according to a *single early* fact mentioned in it. But, one cannot, and in fact nobody does date the RV, just because Indra occasionally still has a stone weapon, to the (late) stone age. Texts contain reminiscences and archaic words and concepts; we can only date them by their *latest*, not their earliest datable features. Or, to put it somewhat facetiously, if I write "looking at my digital clock I saw that the sun rose at 6:00 a.m.," then my sentence cannot be dated, because of the unconscious, but *unscientific* use of "to rise", to the period before the revolutionary book of Copernicus (1507 AD), but only to the present computer age.

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[218] See the long list of late 19th and early 20th cent. discussions in L. Renou, *Bibliographie védique,* Paris 1931, 158-163: Weber 1860, Thibaut, *IA* 1885, p. 85 sqq., Oldenberg, *ZDMG* 48, 629 sqq, Jacobi 1893, 1894, *ZDMG* 49, 218 sqq., Oldenberg, *ZDMG* 49, 470 sqq., Jacobi, *ZDMG* 50, 69-83, Tilak 1893, 1903, Whitney 1894, JAOS 6, 413 sqq.; JAOS 8, 85 sqq, etc. Cf. Elst 1999: 96 sqq.

[219] Pingree does not find basic astronomical skills among the early Indo-Aryan because the texts do not specifically outline such skills.

[220] Autochthonists now date the Buddha to 1700 BCE or even 3139/8 BCE, and Candragupta Maurya (of c. 300 BCE) is replaced by Candragupta, the Gupta king; these and similarly absurd dates are found in Elst 1999: 97.

If ŚB 2.1.2.3 (and also the neglected passage in BŚS 27.5)[221] indeed would indicate the spring equinox in Kṛttikā, then this may very well be a popular or learned remembrance of times long past, for the same passage of ŚB also remembers that the Great Wagon/Big Dipper (*ursa maior*) was "formerly" called "the bears". This is an old Indo-European expression (Greek, Latin, etc.). The name *Ṛkṣāḥ* indeed occurs once in the RV and this is copied in TĀ, ŚB (Witzel 1999c), before the asterism acquired its well-known name "the Seven Ṛṣi" (*sapta rṣayaḥ,* cf. Avest. *haptō* [i]*riṅga* = **sapta liṅgā(ni)*), cf. now Plofker, *EJVS* 6-2, 2000).

In addition, we simply cannot date the ŚB in the third millennium BCE, as it has strong evidence of iron which emerged in India only by 1200/1000 BCE, and as ŚB is very close in its cultural, economic, socio-political, and philosophical development to the time of the Buddha, who lived around the middle of the first mill. BCE.

As seen many times by now, the advocates of the autochthonous theories take one --in case, a rather dubious-- datum and use it to reinterpret Vedic linguistic, textual, ritual history while they neglect *all the other* contradictory data derived from comparative astronomy, archaeology, textual study, etc. This does not achieve a 'paradigm shift', not even special pleading, but simply is faulty reasoning.

§30: The Jyotiṣa Vedāṅga and the solstices

Another favorite item brought forward for an early date of the Vedic texts has been the date assigned to the Jyotiṣa of Lagadha, a Vedāṅga text attached to the Ṛgveda tradition (a later version exists in the Yajurveda tradition as well). Since this is an appendix to the Veda, virtually all other Vedic texts must predate it. Its date, however, hinges on that assigned to the solstice as described in this text. The basic question is the same as in the case of the Kṛttikā equinox: whether the description as given in the Jyotiṣa is also the date of the text in which it is transmitted. Again, this would mean to date the text according to its *earliest* item.

However, the astronomy involved here is not as straightforward as it usually is made out to be. T. K. S. Sastry (1985:13) and R. Kochhar (1999) think of an early date, between 1370 and 1150 BCE, as the winter solstice is described to be in Śraviṣṭhā/Dhaniṣṭhā *nakṣatra.* Pingree's (1973: 10) estimate is c. 1180 BCE.

While Sastry believes that the text preserves a tradition dating back to that period, Pingree (1973: 10) stresses that it is unknown where Lagadha would have exactly placed the boundaries of the *nakṣatra* Dhaniṣṭhā, and what was his exact determination of the longitude of the Sun. Any mistake in the exact position of the beginning of a *nakṣatra* as well as the rough Jyotiṣa intercalation-cycle based on the inexact length of the year as 365 days (instead of c.365 1/4) makes all such back-calculations prone to error by centuries.

Further, Lagadha puts the winter solstice on the new moon of Māgha at the heliacal rising of Dhaniṣṭhā, which post-dates the establishment of the calendrical scheme with *amānta* months. This is late Vedic, at best. In TS 7. 4. 8 and KB 4.4, the beginning of the year is on a full-moon night, and the months are *pūrṇimānta*. KB 19.2-3, however, already has *amānta* months, the year beginning sometimes preceded by an intercalary month (as in the Babylonian calendar of MUL.APIN). This is just one of the several reasons why Pingree (1973: 3, 1987, 1998) introduces Babylonian astronomy and thinks that the astronomy of the Ṛk recension of the Jyotiṣa "was formulated in the fifth or fourth century BC on the basis of

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[221] Note that ŚB has the alternative dates Rohiṇī, Mṛgaśīrṣa, Phalgunī, Hasta, Citrā, and BŚS also has "at the appearance" of Śravaṇa, Citrā/Svatī, all indicating various ritual concerns, see Witzel 1999c.

information about originally-Mesopotamian methods and parameters transmitted to India during the Achaemenid occupation of the Indus Valley between ca. 513 and 326 BC." This would produce a fairly low date *post quem* for the section of KB in question; however, the transfer of such ideas can also have followed other methods and routes.

Sastry (1985: 15) agrees as far as the date of the Jyotiṣa text itself is concerned and adds the observation that its astronomical system is the same as that taught in the Gargasaṃhitā, which Pingree (1987: 295) places in the 5th or 4th centuries BCE. However, one of its constituent parts, the Yuga Purāṇa, which mentions the post-Alexandrian Greeks, was dated by Mitchiner (1986: 82) only to the end of the last century BCE.

Further indication for a late date of the Jyotiṣa is that the language of the text is post-Vedic, which lets Sastry assume that it was redacted by someone belonging "the last centuries BC" (1985: 12). However, it must be added and stressed that the text is actually *composed* in *late* Epic language. It has not been noticed that it does not only have the typical long compounds, but also those with *tat-* as first part, and many metrical 'space fillers' such as *tu, caiva, tathā, tathaiva ca, eva ca, api ca,* which must necessarily be part of the very composition. The particle *vai* occurs once, however not, as usual in Vedic, in second position of a sentence or Pāda but *at the end* of a Pāda (along with *eva ca*!). This agrees with late Epic practice, as seen in Mbh. 12 and Rām. 1 and 7 (Witzel, in prep.).

In short, only if one is *convinced* that Lagadha intended the solstice to be exactly at *alpha Delphini* of Dhaniṣṭhā, one can date his *observations* back to the late second millennium. Since that cannot be shown beyond doubt, since the composition of the text is in Late Epic language, and since its contents have clear resemblances to Babylonian works, the text must belong to a late period, to the last centuries BCE.

In sum, if one were to take seriously the autochthonous dates of the Jyotiṣa at 1400 BCE, (and, accordingly that of the ŚB, or even that of the BŚS, at 2900 BCE)[222], and if one would re-arrange the dates of Vedic literature accordingly, one would have the further, considerable difficulty of explaining, e.g., the use of iron and chariots at 2900 BCE, or the date of the later parts of ŚB at c. 1500 BCE, while they fit in with the cultural and political climate just before the emergence of the Magadha realm and the Buddha around 500/400 BCE.

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[222] The same applies, *mutatis mutandis*, to the Vedic references of a Magha solstice, see Elst 1999: 100, which, in his view, would allow to place the [iron age] Brāhmaṇa and Sūtra literature at 2300 BCE [long before the introduction of iron]. Other alleged astronomical evidence such as the Svarbhānu myth in RV 5.40.5-9 (a late appendix to RV, see Oldenberg 1888!), has been discussed already in the 19th century. Such references are much too vague to be used for dating (nevertheless see Elst 1999: 107). The same applies to the appendix hymn RV 8.93 which Elst (1999: 111 sqq.) wants to turn into a reference to the heliacal rising of the sun in Vṛṣabha. The bull here is, as so often, just Indra. Further, RV 3.39.3 (Elst 1999: 113) refers to the Mārtāṇḍa/Vivasvant myth, not to astronomy; RV 5.83.3 is a poetical image comparing thunder to lion's roar, and not the Siṃha zodiacal sign. Apart from the fact that Elst has to demonstrate the use of the zodiac for the RV, this is poetry, not astronomy. "It could not be clearer" (as Elst says -- but about the zodiac!) Again, RV 6.49.7 describes young women who are 'bright' (*citra*) not the asterism Spica in Virgo (cf. now also Hock, forthc.) Just as in the Gītā, the one who looks for Krishna everywhere will find him, *in casu* early astronomy in the RV; the same applies to S. Kak (1994). Elst's bold summary (1999: 117) is based on such shaky data: "the Ṛg-Veda was composed in the 4th millennium as... the Brahmanas and Sutras are products of the High Harappan period towards the end of the 3rd millennium BC." That this "has been a growing challenge to the AIT defenders for two centuries" is easily lead *ad absurdum*. -- The same fundamental mistake is committed by Klostermaier (1998): "Texts like the Rigveda, the Shatapathabrahmana and others contain references to eclipses as well as to sidereal markers of the beginning of seasons, which allow us by backward calculation, to determine the time of their composition." For all such monolateral assertions, see discussion below, §32 .

§31. Geometry: Śulba Sūtras.

The case of the geometry of the late Vedic Śulba Sūtras is of a similar nature. The advocates of the autochthonous theory maintain, with A. Seidenberg (1962, 1978, 1983),[223] that the geometry of the fire altars in the Śatapatha Brāhmaṇa and some earlier (translated) texts such as Taittirīya Saṃhitā, precedes the early geometry of Greece and Mesopotamia, and that it can be dated prior to 1700 BCE (cf. Elst 1999: 99).

Seidenberg has reached this conclusion by a comparison of the geometry of the Pythagoreans with that of the Vedic texts and some Babylonian sources. The latter have the full system in place at that early date, but their prehistory is not visible in existent Mesopotamian sources. Due to some differences in the three systems (such as algebraic vs. geometric procedures), Seidenberg (1983: 121) excludes mutual borrowing. Rather, he assumes a common source of the three systems that is older than 1700 BCE, and then tries to find echoes of it in pre-Brāhmaṇa texts, even at RV 1.67.10, etc. (which is much too vague about the building of fire altars to allow proof), all without the use of bricks. Staal (1999) has recently expanded on this problem, using my discussion of the common, non-Indo-Iranian words for 'brick' in Avestan, Old Persian and Vedic (from *išt-) and has assumed that the common source may well have been in the BMAC area (see §22) .

Be that as it may, it is not *a priori* necessary that the similarities and identities in mathematical procedure must go back to one *common* source. To paraphrase A. Michaels (1978: 52 sqq., cf. 1983), who has carried out an in-depth study of the Śulba Sūtras and their geometry: Vedic sacred geometry is autochthonous, and analogies between various cultures are not enough to prove actual historical exchange between them. The burden of proof always is with the one who proposes such an exchange. (This has not been supplied, *pace* Elst 1999: 99 sq.). In addition, Michaels distinguishes between sacred geometry in general and its form transmitted in the Śulba Sūtras. This is not always distinguished well (also not by Seidenberg), especially when one simply identifies the *theoretical* knowledge of the Śulba Sūtras with the more *empirical* knowledge and practice of the Brāhmaṇas and Śrauta Sūtras. However, it is likely that the Śulba Sūtras as such originated at the same time as the elaborate description of the ritual and that these texts were all integral parts of the ritual Sūtras (Kalpasūtra).

Michaels goes on to show (1978: 139 sq.) that the magical ideas of Vedic ritual, together with certain practical (artisan's) faculties, lead to the specific form of Vedic sacred geometry, which is basically a logic-free, elementary geometry. However, its various pre-scientific practices, or schemes of action, were transformed into general and theoretical sentences. These could, in turn, always be checked for truth and could be proved by the various practical schemes of action that were used in Vedic ritual with its pre-scientific norms of identity. Michaels also stresses that the connection between magical ideas and artisan's practice was from the beginning only accessible to a small circle of specialists, the ones knowledgeable in "measuring art"; its influence therefore is only visible insofar as it leads to a specialization of a portion of the complete Vedic ritual, again reserved for specialists.

While it has been quite clear for more than a hundred years that these Sūtra texts contain the knowledge of basic geometry (Seidenberg 1983, Michaels 1978), including

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[223] Seidenberg insisted that the geometry of the Śulba Sūtras must have been the origin of the Babylonian system and, accordingly, he would date it no later than 1700 BCE. He neglects other possibilities such as a common origin or a common origin in another area (see Staal 1999).

Pythagoras' theorem, it is now claimed that altar constructions were used to represent *astronomical* knowledge (Kak 1994) in the RV. However, even the post-Ṛgvedic texts say only that the three ritual fires represent the earth, sun and moon, and that the offering priests walk about in space. The complicated post-Ṛgvedic brick pilings on the Mahāvedi represent a bird (*śyena*) that will take the sponsor of the ritual to heaven (e.g., the year as eagle ŚB 12.2.3.7). There is no indication of any typical Brāhmaṇa style speculation that goes beyond an identification of the sponsor of the ritual with the creator god Prajāpati and the year (with its 360(!) days, 10,800 *muhūrta*, at ŚB 12.3.2.5; Śāṅkhāyana Āraṇyaka 7.20, etc. (cf. §22, 26). Complicated astronomy is absent.

If there is any surprising factor here, it is the ability of the Vedic priests to work with such large numbers while they belonged to a civilization that did not use the script or written numbers (though the priests occasionally use twigs to represent very complicated schemes, such as the order of certain repetitions of Sāmans). However, the piling of fire altars made of thousand(s) of bricks belongs to the *post*-Ṛgvedic period (*pace* Seidenberg 1983: 123-4), and even then, occurs only in comparatively late YV material, as has been pointed out above: the Cayana is much later than the Soma and other rituals of the YV Saṃhitās; it can at best be dated to the beginning of the iron age (if we take Tura Kāvaṣeya as one of its originators, see Proferes 1999).

If there indeed is any older, local tradition is hidden behind all of this, it may go back local, to non-Vedic (Indus?) sources. But that remains, for the time being, pure speculation.

SUMMARY

§32. Summary: The autochthonous theory

The autochthonous theory, in its various forms, leaves us with multiple internal contradictions and open questions as far as time frame, cultural content, archaeological, zoological, astronomical, mathematical, linguistic and textual data are concerned. If such contradictions are noticed at all by the revisionist and indigenist writers they are explained away by new, auxiliary assumptions and theories, -- that is, by special pleading, and often by extra-ordinarily special pleading. In short, all things being equal, the new, disjointed theory falls prey to Occam's razor.[224]

If we would in fact assemble all of the autochthonous ''evidence'' (as has been attempted here in brief form) and think it through, torturous as it may prove to be, we would have to rewrite not only Indian history, but also many sections of archaeology, historical linguistics, Vedic literature, historical geography, zoology, botany, astronomy, etc. To apply

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[224] Incidentally, autochthonists always insist on the lack of archaeological, palaeontological etc. evidence or the IA "invasion" (or immigration/trickling in) theory. However, it may be pointed out that none of the Out of India theories are substantiated by archaeology etc. either. The matter has not been raised yet, but it must be pointed out that just as there is clear linguistic, textual and now genetic evidence but "no Aryan archaeology, no Aryan bones", there also is no archaeological proof, but only historical, clear linguistic and now also genetic evidence for the *one* clear emigration of an Indian population westwards in historical times -- that of the Gypsies (Roma, Sinti etc.; there are one or two similar cases, attested in later times, but on a much more limited scale, see Hock 1999).

the new "theory" consistently would amount to a "paradigm shift" in *all* these fields of study. But biologists, for example, would not be amused.

In other words, should there be special rules in all these sciences only as far as evidence from South Asia is concerned? Either science is universal, or we may begin to write new regional or national accounts, in fact new mythologies that include *some* observations of nature and the sciences. Are we ready for a "Mythos of the Twenty-First century," written by a Mr. JapāGiri or SevatīParvat?

Certainly, a revisiting of old theories should be carried out if the new evidence is strong and unambiguous. But the observations made by revisionists and indigenists do not add up to a complete, self-contained theory that is in agreement with the other, independently developed fields of knowledge. Instead, it is rigged with lacunae and internal contradictions and it frequently clashes with the established sciences. These features make the autochthonous theory particularly unfavorable as a replacement of earlier explanations.[225] A 'paradigm shift' can be maintained, as has been shown time and again in the preceding sections, only by using very special pleading. Occam's razor applies.

If the model of a transhumance type immigration or trickling in of speakers of Old IA and subsequent acculturation (one last time, not an "invasion"!) is to be replaced, then such a new model has not yet been found, and it has certainly not yet been shown to be probable by the revisionists and indigenists. The burden of proof squarely rests on the shoulders of the advocates of the new autochthonous theory.

To sum up: even when neglecting individual quirks,[226] the various autochthonous proposals simply do not present a cogent picture. They almost completely neglect the linguistic evidence, and they run into serious chronological and geographical difficulties: they have horse drawn chariots in S. Asia before their actual invention, horses in S. Asia before their introduction from Central Asia, use of iron tools at 1900 BCE before its first use at c. 1200/1000 BCE. They have the Ṛgvedic Sarasvatī flowing to the ocean while the RV indicates that it had already lost its main source of water supply and must have ended in a terminal lake (*samudra*).

They must also distort the textual evidence of the RV to make it fit supposed Harappan fire rituals, the use of the script, a developed town civilization and its stratified society of traders and artisans, and international maritime trade. And, they must rewrite the literary history of the Vedas to fit in improbable dates for the composition of most of its texts so that they agree with *supposed contemporary* astronomical observations -- when everything else in these texts points to much later dates.

Finally, they have the Old Indo-Aryan, or even the Indo-European Proto-language, developing in the Panjab or even further east in northern India while all non-IA[227] linguistic and historical evidence, including that of linguistic palaeontology, clearly points to areas further northwest and west. They maintain an Indian homeland for IE, while the expected early South Asian loan words are entirely missing in all non-IA IE languages, including even

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[225] Except, of course, if the aim is some 'superior', religious or political motive.

[226] Such as Kak's "astronomical code" that is based on a combination of Ṛgvedic brick pilings of the still *non-existent* Agnicayana and the structure of the still *non-existent* complete RV collection. Note, that it is not questioned but favored by Klostermaier (1998), Elst (1999) and other revisionists/indigenists.

[227] Even that of Mitanni-IA, see above; excluding, of course, that of the comparatively late IA emigrants, the Gypsies.

the neighboring Old Iranian, and while, conversely, such loans are already copious in Vedic and are traceable to S. Asian substrate sources.

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Curiously, even the alleged historical development of the Aryan "invasion theory" is not correct as usually stated.[228] It was *not* developed and formulated in the 19th century to show that the Vedas were composed before the 'Aryans' mixed with the indigenous 'races' and to underline that the British conquest was similar to the 'Aryan conquest'. In fact, the early period of IE linguistics did not have that concept at all; the home of the IE language was thought, in the typical Romantic fashion of the day, to be in India or in innermost Asia. The concept of the IE language family, though first formulated by two late 18th century British citizens (Lord Monboddo and William Jones, and in both cases not yet scientifically at all[229]), the IE and (Indo-)Aryan theory was not developed by British imperialists but by Danish and German scholars of the romanticism era, such as R. Rask and F. Bopp (1816); it was further developed in the later 19th c. by German linguists such as the Leipzig *Junggrammatiker* school whose members had no interest at all in British imperial designs (cf. Kennedy 1995, Trautmann 1999). The theory of an immigration into or invasion of S. Asia by speakers of IA, based on the familiar concept of the Hunnic and Germanic invasions of the Roman empire, and the idea of an IE 'race' emerged only later in the 19th century and they were not even generally accepted; for example the concept of an 'Aryan race' was *rejected* by the now-maligned Indologist Max Müller (1888) or, at length, by the Indo-Europeanist H. Hirt (1907).

In addition, already by the end of the 19th century there was a reaction against reading too much of IE linguistics and reconstructed IE culture into the RV: the Frenchman Bergaigne stressed the complicated nature of RV poetry and ritual, and the Germans Pischel and Geldner saw the RV as a sort of Kāvya rather than the simple nature poetry of semi-nomadic pastoral tribals, a view fashionable in the first part of the 19th century. Max Müller was actually called *mokṣamūla[ra]* in his time because of the help he provided to the cause of Indian independence, all while working at Oxford in the midst of imperialistic Britain (Müller 1883, 1970). He still saw the RV in the rather Romantic fashion of his youth, the first half of the 19th century, as 'primordial' poetry of nature, as some of our earliest texts; yet already for him, the Aryan concept had nothing to do with 'race' but all with language and its 'decay'.[230]

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[228] The most blatant rewriting of 19th century (European) intellectual history (and much else!) has been carried out by the mathematician (Ph.D. 1976) and electrical engineer (B.A. 1965) N.S. Rajaram (1993, 1995, etc.) who sees missionary and colonialist designs all over Indology. Unfortunately, he had to rely on English summaries (of summaries) of 19th cent. sources written in various European languages -- hardly a good starting point to write history. Even a cursory reading of his many, repetitive books will indicate just one thing: a lot of fantasy. These books are nothing but a *new mythology* of the 19th century, written for and now increasingly accepted by (expatriate) Indians of the 21st century to shore up their claims to a largely imagined, glorious but lost distant past.

[229] It is usually not mentioned that W. Jones' formulation does include not only the languages belonging to the IE family, such as Sanskrit, Greek, and Latin but also unrelated ones such Malay.

[230] For example, the first translation and dictionary (1873) of the RV by the well-known German mathematician Grassmann analyses *anās-*, (which occurs only *once* in the RV, at 5.29.10!), as 'ohne Mund, Antlitz' (without mouth, face, *an-ās*); however, the word was taken by later 19th century writers as an indication of a racial characteristic, 'noseless' (*a-nās*), while the passage in question clearly indicates the 'speechlessness' and unusual speech of the *dasyu*.

If some British scholars used the evidence then available to cement the position of their empire, it was natural for them in their own, Victorian time, just as the use of the *same* data by, e.g., the champions of the Dravidian *irredenta* (Trautmann 1999), by those who followed the then fashionable 'race science' of the Frenchman de Gobineau and the British writer Hamilton, or by Dalit reformers and by the leaders of the Indian independence movement. However, the facts themselves remain, until (some of them) are shown to be based on incorrect data or conclusions.

Present day non-Indian scholars, however, do no longer have any colonialist or 'Eurocentric' agendas and, anyhow, do not feel the need to defend 'traditional' western conclusions and theories of the 19th or 20th centuries.[231] Rather, if anything has been typical for the development of western thought during the past few centuries, it has been the constant change in intellectual approaches and fashions (see below) in methods and in conclusions; all were guided, of course, by the ongoing dialectical process. These many diverse concurrent developments are, as has been pointed out above, often neglected by revisionist and indigenist historians who frequently juxtapose, compare, or even equate the writings of the 19th with those of the 20th century. Present day "western scholarship," however, is very much aware of its own historical situation and theoretical position; yet, it is firmly rooted, (post-modernism by and large excluded) in the enlightenment tradition.

<div align="center">∗∗∗</div>

Notwithstanding the internal social and political reasons for the clash between recent Indian historiography (now often termed 'Marxist') and the new wave of revisionist and nationalistic writing that culminates in the "Out of India Theory", it is its very emergence and relative popularity, as late as two generations after Indian independence, that must surprise. The 'revisionist project' certainly is not guided by the principles of critical theory but takes, time and again, recourse to pre-enlightenment *beliefs* in the authority of traditional religious texts such as the Purāṇas. In the end, it belongs, as has been pointed out earlier,[232] to a different 'discourse' than that of historical and critical scholarship. In other words, it continues the writing of religious literature, under a contemporary, *outwardly* 'scientific' guise. Though the ones pursuing this project use dialectic methods quite effectively, they frequently also turn traditional Indian discussion methods and scholastic tricks to their advantage.[233]

The revisionist and autochthonous project, then, should not be regarded as *scholarly* in the usual post-enlightenment sense of the word, but as an apologetic, ultimately religious undertaking aiming at proving the 'truth' of traditional texts and beliefs. Worse, it is, in many cases, not even scholastic scholarship at all but a political undertaking aiming at 'rewriting' history out of national pride or for the purpose of 'nation building'.

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[231] I have pointed to this (1995), when I discussed the various forms of argumentation that have to be avoided in writing ancient Indian history; however, this point has largely been misunderstood or blatantly disregarded by adherents of autochthonous or Out of India theories: in many web sites (and in Talageri 2000), these writers excoriate me for my critique of present revisionist/autochthonous writings, but they do not even mention my criticism of past western or of certain present archaeological and historical writings (often produced by "westerners").

[232] Witzel 1995, 1999d.

[233] See Caraka 3.83, Nyāyasūtra 4.2.50, the method is used in Mahābhāṣya, and still earlier in some Brahmodyas (Witzel 1987a, and forthc.)

If such writings are presented under a superficial veneer of objective scholarship they must be exposed as such,[234] at least in the context of critical post-enlightenment scholarship. Alternatively, they could simply not be taken seriously as historiography and could be neglected (which seems to be the favorite attitude of most scholars in Indology/Indian Studies). In both cases, however, they must be clearly *understood* and described as traditional, (semi-)religious writings. Therefore they should be regarded and used, not as scholarly contributions, but as *objects for the study* of the traditional mind, -- uncomfortable as this might be for some of their proponents, many of whom combine, in facile fashion, an education in science with a traditional mindset.[235]

In view of this, it might not even seem necessary to 'decolonialize' the Indian mind (cf. Witzel 1999d). However, the dominance of English as the only true language of communication throughout the subcontinent, and the strong Euro-American influence (even in non-Whorfian models) that this automatically creates in the mindset of the English speaking elite, points in the other direction. This is reinforced by the persisting dominance of an antiquated British style curriculum. Some adjustments both to local South Asian conditions and, simultaneously, to the emerging global village certainly are in order. On the other hand, present autochthonously minded efforts are the wrong way to follow. Fifty years after Indian independence, it should not be regarded as a *scholarly,* but simply as a political undertaking to 'rewrite' history for the purpose of national pride or 'nation building'. We know to what such exercises have lead during the past century.

If the present wave of apologetic, revisionist, and nationalistic writing should continue unabated, and if it should remain largely unobserved, unstudied and unchecked by post-enlightenment scholarship, future historians will look back at these excesses of the end of the 20th century and the beginning 21st in the same way as some now like to do with regard to the 19th century. And they will criticize the present generation of scholars for having looked the other way -- for whatever reasons.

It remains for us to hope[236] that the recent spate of revisionist, autochthonous and chauvinistic writings will not lead to similar, real life consequences as those that we have witnessed during the 20th century.

ABBREVIATIONS

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[234] Such as N.S. Rajaram's (2000) case of fraud and fantasy in 'deciphering' the Indus seals, see Witzel & Farmer 2000.

[235] If this is not believed, after the evidence presented throughout this paper, I may add a very recent experience: a visit from a "type 3" (see above, n. 73) graduate in mechanical engineering who firmly held that the Vedas are 2 billion years old, are *Īśvara's* revelation, can only be understood after initiation (*upanayana*), are the sources all languages in the world and of all sciences, etc., -- all of this internalized and integrated, without any problem, with his studies in the hard sciences.

[236] A sign of hope is that recent interviews with Indian College students from all over the country seem to indicate that they have no interest at all in this kind of debate. They are much more practically minded. ("The New Republic", Times of India, Jan. 26., 2001)

The abbreviations for texts are the commonly used ones; other important ones include those listed below. Note: for ready reference, the five historical levels of Vedic are indicated by numbers (1-5), followed by their geographical location, W: western North India = Panjab, Haryana, C: central North India = Uttar Pradesh, E: eastern North India = N. Bihar; S: southern N. India = between the Jamna/Ganges and the Vindhya mountains).

AA   Austro-Asiatic
AB   Aitareya Brāhmaṇa (4, W & E)
Akkad.   Akkadian
Armen.   Armenian
AV   Atharvaveda Saṃhitā (2 C)
Av.   Avestan
Avest.   Avestan
AVP   Atharvaveda Saṃhitā, Paippalāda version (2 W)
Beng.   Bengali
Brah.   Brahui
BŚS   Baudhāyana Śrautasūtra (4-5 C)
Bur.   Burushaski
Drav.   Dravidian
ep.   Epic Sanskrit
EWA   Mayrhofer 1956-76
Gr.   Greek
Grk.   Greek
GS   Gṛhyasūtra(s) (5)
Hitt.   Hittite
IA   Indo-Aryan
IE   Indo-European
IIr   Indo-Iranian
Indo-Ar.   Indo-Aryan
Iran.   Iranian
JB   JaiminIya Brāhmaṇa (4 S)
Kan.   Kannada, Canarese
Kazm.   Kashmiri
KB   Kauṣītaki Brāhmaṇa (4 C)
KEWA   Mayrhofer 1986-96
Khot.   Khotanese Saka
KS   Kaṭha Saṃhitā
KŚS   Kātyāyana Śrautasūtra (5 E)
Lith.   Lithuanian
Mal.   Malayalam
Mar.   Marathi
Mbh.   Mahābhārata
MIA   Middle Indo-Aryan
MP.   Middle Persian
MS   Maitrāyaṇi Saṃhitā (2-3 W)
MT   Mother Tongue
NP.   New Persian
NIA   New Indo-Aryan

Nir.   Nirukta (5)
Nur.   Nuristani (Kafiri)
OP.   Old Persian
O.Pers.   Old Persian
Osset.   Ossetic
PIE   Proto-IE
Pkt.   Prakrit
PS   Paippalāda Saṃhitā (2 W)
Rām. Rāmāyaṇa
RV   Ṛgveda Saṃhitā (1, Greater Panjab)
RVKh   Ṛgveda Khila (2 W)
SaMh.   Saṃhitā(s)
ŚĀ   Śāṅkhāyana Āraṇyaka (4 C)
ŚB   Śatapatha Brāhmaṇa (4 E)
ŚS   Śrautasūtra (5)
Skt.   Sanskrit
Sum(er).   Sumerian
Sū.   Sūtra(s) (5)
SV   Samaveda Saṃhitā (2 W)
StII   Studien zur Indologie und Iranistik
TĀ   Taittirīya Āraṇyaka (4 C)
Tam.   Tamil
Tel.   Telugu
TB   Tibeto-Burmese
Tib.   Tibetan
Tib.-Burm. Tibeto-Burmese
Toch.   Tocharian
TS   Taittirīya Saṃhitā (2 C)
Up.   Upaniṣad(s) (4)
V.   Vīdēvdād (Vendidad)
VādhB   Vādhūla Brāhmaṇa (Anvākhyāna) (4 C)
Ved.   Vedic
Ved. Index   Macdonell - Keith 1912
VS   Vājasaneyi Saṃhitā (2 E)
Y   Yasna
YV   Yajurveda (-Saṃhitā) (2)
ZDMG   Zeitschrift der deutschen morgenländischen Gesellschaft

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COLOPHON

(EJVS 7-4)

ADDENDUM TO EJVS 7-3, notes 45-46

The BŚS passage discussed in EJVS 7-3, notes 45-46, has evoked protracted discussion. A few weeks ago I came across a recent discussion by Toshifumi Gotō, Sendai University, Japan, which follows.

Toshifumi Gotō (2000) has translated and commented on BŚS 18.44: 397.9 sqq., without mentioning my 1995 paper, while I, in turn, had overlooked H. Krick's translation in her large Agnyādheya study (1982). As will be seen, both agree almost verbatim with the various possibilities I have sketched in EJVS 7-3 (notes 45-46). The only difference is that Krick takes *Amāvasu* from *amā* + *vasu* 'goods, wealth'. But note her last sentence (n. 90).

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GOTO: p.101sqq.:

"Nach Osten wanderte Āyu [von dort] fort. Ihm geh   ören diese [Menschen, die genannt werden:] "Kurus und Pañcālas, Kāśis und Videhas."[87] Sie sind die von Āyu stammende

_____

[87] *iti* kann hier kaum die die Aufzählung abschliessende Partikel (Fälle bei OERTEL Synt. of cases, 1926, 11) sein. In den beiden Komposita könnte der Type *ajāváḥ*' [die Gattung von] Ziegen und Schafen' vorliegen: pluralisches Dvandva für die Klassifikation, vgl. GOTO Compositiones Indigermanicae, Gs. Schindler (1999) 134 n. 26.

Fortführung.[88] Nach Westen gewandt [wanderte] Amāvasu [fort]. Ihm gehören diese: "Gāndhāris, Pārśus,[89] Arāṭṭas". Sie sind die von Amāvasu stammende [Fortführung].[90]

i.e: (in my literal translation from German):

"Āyu wandered forth westwards [from there]. To him belong these [humans, who are called:]"Kurus and Pañcālas, Kāśis and Videhas."[N87] They are the leading-away[N88] stemming from Āyu. Turnings westwards Amāvasu [wandered forth]. To him belong these: "Gāndhāris, Pārśus,[N89] Arāṭṭas". They are the [leading-away] stemming from Amāvasu.[N90]

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[88] Gemeint ist hier wohl die Erbschaft seiner Kolonisation ("Fortwanderung"); mit bekannter Attraktion des Subj.-Pronomens in Genus und Numerus an das Pr ädikatsnomen.

[89] Mit WITZEL, Fs. Eggermont (1987) 202 n. 99, Persica 9 (1980) 120 n.126 als *gāndhārayas parśavo* statt -*ya sparśavo* aufgefasst, wofür dann allerdings im rezenten BaudhŚrSū die Schreibung *gāndhārayaḥ parśavo* zu erwarten w äre. Der Fehler weist also auf die Zeit hin, in der -*SSP*- noch als -*SP*- ausgesprochen wurde (wie z.B. in der MS, vgl. AiG I 342) und noch kein *ḥ* (für das erste *S*) eingeführt wurde. -*yaspa*- entging einer (interpretatorischen) Änderung zu - *yaḥ pa*- oder -*yaḥ spa*-.

[90] Dahinter steckt wohl die Vorstellung von *āyú* als normales Adjektiv 'lebendig, beweglich' und entsprechend, wie KRICK 214 interpretiert, von *Amāvasu*-: "nach Westen [zog] A. (bzw.: er blieb im Westen in der Heimat, wie sein Name 'einer, der Güter daheim hat' sagt.".

[N87] *iti* can hardly be the particle that concludes enumarations (cases in OERTEL, Syntax of cases, 1926,11). In both compounds the type *ajāváḥ* [the category of] goats and sheep' may be the one in question: a plural dvandva of classification, cf. GOTO Compositiones Indogermanicae, Commemoration vol. Schindler (1999) 134 n. 26.

[N88] Apparently the inheritance of his colonization ("leading-away") is intended; with well-known attraction of the pronoun of the subject, in gender and number, to the predicate noun ["verbal phrase"].

[N89] Understood, with WITZEL, Fs. Eggermont (1987) 202 n. 99, Persica 8 (1980) 120 n.126 as *gāndhārayas parśavo* instad of -*ya sparśavo*, for which, however, a spelling *gāndhārayaḥ parśavo* would be exspected in the late BaudhŚrSū. The mistake therefore points to a period in which -*SSP*- was still pronounced as -*SP*- (as e.g., in MS, cf. [Wackernagel's] Altindische Grammatik I 342), and when *ḥ* (for the first *S*) had not yet been introduced. -*yaspa*- escaped a (interpretative) change to - *yaḥ pa*- oder -*yaḥ spa*-.

[N90] Apparently the idea of *āyú*- as normal adjective 'alive, agile' underlies this, and correspondingly, as KRICK 214 interprets, of Amāvasu-: "Westwards [trekked] A. (or: he stayed home in the west, as his name says 'one who has goods/possessions at home')".