

Indigenous Indoaryans and the Ṛgveda.

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I) Abstract. In this paper I argue that the IndoAryans (IA hereafter) are indigenous from at least 4500 (all dates are BCE except when otherwise stated) and possibly 7000. In this effort are utilized the latest archaeological finds and data from Archaeoastronomy, Anthropology and Palaeontology. I use in addition neglected cultural and linguistic evidence. I find no evidence at all for an invasion. The new term “migration” is a misnomer since a migration could not have produced the results found in that area. The *Ṛgveda* (=RV) is neither post-Harappan nor contemporaneous with the ISC but much earlier, ie from the 4th millennium (with minor insignificant exceptions) and perhaps before.

II) Introduction. 1. Two things must be said at the outset. First, an unambiguous decipherment of the Indus script would most probably settle the issue of the IndoAryan origin. All claims so far, whether by sanskritists (Rao et al) or by dravidianists (Parpola et al) are unconvincing as B. B. Lal’s survey shows succinctly (1997: 203-14). E. Bryant’s more recent survey (2001: 177-84) concurs with Lal: Bryant thinks S Kak’s (sanskritic) efforts promising (181-2) and mentions M Witzel’s thought that the Indus script may be Munda (184). But unless finds with longer legends or other writings are unearthed, the Indus script (like the Phaistos disc of Crete) will not be deciphered easily. Consequently I shall ignore this and make the best of the other available evidence. Second, at any time new finds may emerge (eg objects of iron from, say, 2800; large settlements in the Ganges area from 3000; tablets with recognisable Dravidian writing from 2500; or whatever else) that will change radically whatever picture we now have.

2. I am a sanskritist with a little Greek and less Latin. I am of Greek nationality (but a British subject) and although I have twice visited India for extended periods I know very little about the modern religion(s) there and practically nothing about its politics (except the unfortunate conflict over Kashmir). As an undergraduate and postgraduate at SOAS (London) in the 1960s I absorbed the mainstream “Aryan Invasion Theory” (AIT hereafter); for G Dales’ s work that knocked down Sir M Wheeler’ s extravagant view had not yet percolated down to us. In any case I was then, and continued to be for many years afterwards, interested in Sanskrit only, the dramatic works and the Upanishads and Vedānta; I had no interest at all in the early history of India. Subsequently I taught the AIT with the South Russian Steppe as the locus of dispersal of the IndoEuropeans (IE hereafter) for 18 years and I wrote a Course of Sanskrit (in Md Greek) in which I actually concocted fictitious passages about the Aryans invading with chariots and subduing the natives who thought it prudent to accept them and cooperate! In 1987, I began to wonder about the AIT. In the same year I went to India and collected much material which took a few years to sort out and digest, since I had little acquaintance with Indian archaeology and early history. What became abundantly clear in the early 1990s (and filled me with incredulity) was the fact that there was **no evidence whatever** for any invasion (which by that time was becoming “migration”). In 1996 I abandoned the AIT in all its forms and the mainstream chronology for Sanskrit literature. The experience was doubly painful: first it was not easy to give up an idea I had taken for granted for more than 20 years; second I could not understand why mainstream indologists adhered to the AIT so passionately, since it was supported only by linguistic data that could, in any event, be interpreted in different ways.¹

3. I shall not be dealing with the history of this issue (see Kazanas 1999 and 2000). J P Mallory (1973) and recently E Bryant (2001) have dealt with it very fully. I shall only mention the fact that M Witzel attacked several scholars who since the early 1990s manifested support for the Indian Indigenous Origin (IIO hereafter); amid various criticisms (especially 2000: 13) he used the term “revisionists” (also 2001: 22), ignoring obviously that in the early 19th century many European scholars took India, on the strength of Sanskrit, to be the original homeland (Mallory 1973:26-9).

1 I give this biographical note to forestall any suspicions that I am of Indian extraction or have any connections with India, though I do correspond with some Indian scholars.

Even after scholars revised their views and, rejecting India, adduced different locations from the Baltic to the Balkans and eventually rewrote Indian (pre-)history, there was a succession of Indian scholars, mainly, who maintained the IIO in one form or another (Rao 1880, Aurobindo 1914, Dhar 1930 et al, et al). The term “revisionist” is therefore inapplicable. Witzel ignores also that Copernicus, Kepler and Galileo were ‘revisionists’ in rejecting the geocentric system of Ptolemy (which held sway for some 1500 years) and, against an oppressive and repressive mainstream opinion (and officialdom), reinstated – with improvements – the heliocentric system of Aristarchos of Samos (3rd cent BCE).

4. E Bryant’s *The Quest for the Origins of Vedic Culture* (2001) is a long overdue study presenting the indigenist views, advocated mainly by Indians. Witzel describes it as “a balanced description and evaluation of the two century old debate” and Mallory as “not only an important work in the field of Indo-Aryan studies but a long overdue challenge for scholarly fair play” (both on the back jacket of the 2001 publication). Certainly, Bryant’s work is invaluable and balanced and fair in the usual academic way but like so many other studies it makes the issue seem far more complicated than (in my view) it is by giving undue weight to arguments that do not deserve it and by ascribing undue importance to the linguistic side.²

The IE and IA issues are, of course, primarily linguistic but only in that the terms IE or Proto-Indo-European (PIE hereafter) refer to languages, not races or nations. This does not mean at all that scattered linguistic data and conjectures, about which there are very grave disagreements and which give no sure dates, can be taken to be the decisive arbiter in this issue. (See IX 2, 3 & 4.) *The issue is no longer a linguistic one.* The nature of IA or Vedic and its relationship to other IE languages – which are purely linguistic matters – were decided long ago, quite properly, by the discipline of Philology. Other disciplines did not intervene at any time. But today the issue is quite different: we are now concerned with the question *when* and *how* Vedic appeared at its historical habitat. Since Vedic and other languages did not sound of themselves in a vacuum but were spoken by human beings, these questions involve movements of peoples and their material cultures. Philology has little competence in this field; even Historical Linguistics would be trespassing since it deals with changes within language(s) in relative not absolute chronology – actual dates being derived from other sources. This is the field of History and in prehistoric times of Archaeology and related disciplines. The difficulties besetting the larger IE issue are due, I think, to the fact that linguists continue to arrogate to themselves the competence of these other disciplines. Just as experts from other sciences do not intervene in Linguistics, so linguists should not tell historians, archaeologists et al what to do or how to do it just because the latter’s finds are at variance with linguistic wishes.

5. Mallory ended his overview of the “IE problem” (1973: 60) with this perceptive statement: “a solution to the problem will more than likely be as dependent on a re-examination of the methodology and terminology involved as much as on the actual data themselves”. The method of approach in the West has not changed since then. Methodology itself (and terminology, which may be regarded as one of its aspects) depends on the human element known as conditioning, or prejudice, which often reproduces mechanically inherited forms. And this, because scholars do not turn their attention inwards to examine the very instrument with which they do all their studying, thinking, comparing, etc. Attachment to any notion is not healthy: Plato asked of “academics” (=the students at his Academy) to let the mind (or ψυχή *psuchē* ‘soul’), free of all concerns, rest periodically on its own with true, immutable Being (*Phaidōn* 79C, *Phaidros* 247C-E). Max Müller

2 Two different examples should suffice. Bryant adopts too lightly H Hock’s view that “It seems simpler to posit one migration into India” rather than many out of it (p 146). This applies not only to India but to all other proposed urheimats and has no validity as an argument. (See below IX, 3) He also states that the Greeks and the Scandinavians “preserve no mention of their migrations into their historical territories” to counterbalance the non-mention of migration in the *RV*. Bryant’s statement is untrue. Both Greeks and Scandinavians do preserve some memories of migration, wrong though they seem to us (see III, below).

explained that his conjectural chronology of the *R̥gveda* (*RV* hereafter; the Vedic Hymns 1200-1000, Brāhmaṇas etc 1000-800, and so on) was only a *terminus ad quem* adding “we should not forget that this is a constructive date only and that such a date does not become positive by mere repetition” (1916: 34). Many scholars c 1900 CE tried to push the date of the *RV* to an earlier date, arguing that the 200-year periods are far too short for the development of these different types of literature and using such evidence as was then available, like astronomical data. Nevertheless the conjectural chronologies fell into mere repetition and became established dogma (Kazanas 2000: 81).

6. At that time (1875-1900CE) Philology had developed considerably whereas Archaeology was in its gestation period. It was unavoidable that linguistic considerations should play an important role. As it happened, such considerations played a decisive role and the eminent linguist M Emeneau wrote (1954: 282): “At some time in the second millennium BC a band or bands of speakers of an Indo-European language, later to be called Sanskrit, entered India over the north-west passes. This is our *linguistic doctrine* which has been held now for more than a century and a half. There seems to be no reason to distrust the *arguments* for it...” (my emphasis). The statement contains a factual error since it was less than a century that this doctrine had gained currency (see §3, above). But the important point is that Emeneau makes a judgment about a historical occurrence admitting it to be a received doctrine and using the word ‘arguments’ in its support, not, as one might expect in a case of historical events, terms like ‘evidence’, ‘data’, ‘records’ or ‘testimony’.

J. Shaffer, archaeologist/anthropologist, wrote succinctly about this situation: “The Indo-Āryan invasion(s) as an academic concept in 18th- and 19th-century Europe reflected the cultural milieu of that period. Linguistic data were used to validate the concept that in turn was used to interpret archaeological and anthropological data. What was theory became unquestioned fact that was used to interpret and organise subsequent data. It is time to end the linguistic tyranny...” (1984:88). Let me give an example or two of the received theory being used to interpret and organise other data. Accepting the *RV* date as c1200, Witzel examines the Near-East (NE hereafter) chariots (2000:6; 2001:47,59ff) then foists them onto the *RV* pointing out that such chariots (those in the *RV* also) are subsequent to 1700! He disregards the few details of *RV* chariots that make them quite different from the NE ones. Another example: an old text *Jyotiṣa Vedāṅga* dealing with Astronomy, has a certain astronomical reference which was thought by some to denote a date 1370-1150 (Witzel 2001:72-3) but Witzel writes that the style of this work places it during the last centuries *BC* and ignores completely that the chronological scheme itself is being questioned.³ The noted sanskritist Aklujkar (Professor at British Columbia, Canada) does not consider the mainstream chronology incontestable and writes “only *relative* chronology has been well argued for” (1996: 66).

One does not begin the study of the history of a people, even a people speaking a particular language (IE in this case), with highly ambiguous and controversial linguistic considerations. As was said, these are useful in establishing relations with other members of the language family but not in establishing dates. Linguistic phenomena would have been more significant only if there had been attested in the region at least one non-IE language of equal antiquity as Vedic (like Etruscan in Italy and Basque, still alive, and Iberian in Spain). But to start with the assumption that *Vedic was an intruder when no other language of equal age was attested* was wrong method or defective scholarship. Chronologies are usually established by written records; in the absence of such documents, we turn to archaeological finds and similar datable evidence. The unreliability of the linguistic data and theories based thereon can be amply demonstrated by the very case we are

3 This prejudicial approach goes far deeper and has wider implications. A Zide a priori ruled out any possibility of a sanskritic language being that of the Indus script (1979:257). G. L. Possehl too stated on the same subject that “Indo-Aryan is dismissed since the Fairservis position is that... [it]... arrived in the subcontinent at ca 1500 BC” (1996:153). A Hitze states “Theories according to which the Indo-Aryans would be autochthonous cannot be substantiated” (1998: 139 & n 2).

investigating. The same philological data with minor variations and differences in emphasis have been examined and interpreted differently by different scholars who reach thereby different conclusions. Thus T Burrow (1973:9ff) on purely philological considerations takes central Europe as the urheimat and the date of the dispersal from middle to late 3rd millennium. Gamkrelidze and Ivanov (1985,1990,1995) posit as the PIE urheimat the region south of Caucasus and the date of the dispersal or migrations in the early 3rd millennium. From the same data S.S. Misra derives dates ranging in the 5th and 6th millennium and prefers N-W India (1992). I. Diakonov favours the Balkans (1985) and G. Owens takes Minoan to be the first IE language, the Greeks indigenous and the Aegean the cradle of PIE culture (1999). Others again have other views. Mallory examines summarily some of these conflicting “estimates” and cries out “Will the ‘real’ linguist please stand up” (1997:98). No linguist is saved simply because he/she claims to have “correct” data and method. The “when” and “how” are concerns of experts other than linguists.

7. Apart from the linguistic issue, however, we have here another subtle aspect. Since there are all these different claims for the urheimat (and some more on additional criteria) from the Baltic region to the Balkans, the Pontic Steppe (Mallory et al), Anatolia (Renfrew et al), Central Asia (Sargent 1997) and N-W India-and-Pakistan, then any one location is controversial. Why mainstream scholarship should single out and ostracize only N-W-India-and-Pakistan is incomprehensible – particularly when Archaeology and Anthropology since the early 1980s stressed that there was no trace of mass invasion in this area (Jarrige & Meadow 1980; Lal & Gupta 1984: 343), unlike all other locations (see III, 3, below). Thus in a recent work (2001: 138-9) Mallory mentions Europe, the Pontic Steppe and Anatolia as possible urheimats but not Northwest India.

In an article of this size one clearly can’t deal with all authors and all details. I shall attempt to cover all important aspects but, as I said following Mallory, my methodology will be different. I feel certain that archaeologists will think I omit important aspects of their finds. Because of my method which assigns Philology to a subordinate position and must select from the vast field of Linguistics, philologists will feel even more offended. So be it. Taking it for granted that there is an IE family of languages and without speculating about the time depth and the form and extent of the PIE, I start with historical records. Since Witzel’s and Bryant’s studies are most recent, I shall refer to them frequently (and to Bryant with page number only). Naturally, I shall not reproduce all the data and arguments I have used in my published articles 1999, 2000, 2001c and 2001d.

III. Historical records

1. In the *RV* (and later Indic texts) there is no hint of the *Āryas* coming into the Saptasindhu⁴, the land of the seven rivers in North India and Pakistan (as known today). AB Keith wrote “It is certain... that the *R̥gveda* offers no assistance in determining the mode in which the Vedic *Āryans* entered India... the bulk at least [of the *RV*] seems to have been composed rather in the country round the Sarasvatī river” (1922:79). Witzel thought he found references to an immigration but admits them to be “indirect” and finally wrote that the *Brāhmaṇa* texts “manage to garble the evidence” (1995b: 321, 340). Several scholars indulge in semantic conjurings saying that various names in the *RV* refer to places and rivers in Afghanistan, Baluchistan, Iran etc, but this is not a very honest practice since by such interpreting (turning facts into metaphors and symbols, and vice versa)

4 Some may feel offended by my use of ‘Saptasindhu’. In the *RV* we find several references to the 7 rivers: I, 32, 12 and 35, 8 *saptá síndhūn* acc pl; I, 34, 8 *síndhubhiḥ* instr pl; etc, etc. Many others before me have used ‘saptasindhu’: it is short and most convenient (see Kazanas 2001b, §14).

one can prove anything.⁵ Even Witzel admits finally (2001 §3) that the *RV* was composed “round the Sarasvatī river”, as Keith put it.

Apart from its silence on a former homeland, or immigration, the *RV* contains positive indications about the Āryas’ very long presence in Saptasindhu. Hymn X, 75 gives a list of names of rivers not in the order west-to-east, as we would expect from invaders advancing in that direction, but from east-to-west, as of a people long settled and having the east as a starting point of reference. Then there are passages expressing the Aryans’ strong sense of being rooted in their lands when they recall their ancestors taking their place in the sacrifice “here”, like the Angiras family (IV, 1, 3) or the Vasiṣṭhas (VII, 76, 4), etc.

2. Furthermore, the Vedic texts refer to peoples being exiled or driven away from Saptasindhu. The early *Aitareya Br*⁶ (VII, 33, 6 or VII, 18) writes of sage Viśvāmitra exiling his 50 disobedient sons so that, in later periods, “most of the Dasyus are the descendants of Viśvāmitra, mostly in the east” (*VI*⁶, ‘andhra’). In the *RV* we have at least two similar and much more informative passages. *RV* VII, 18, 16 tells how Indra helped King Sudās defeat his numerically superior enemies (the 10 Kings), many of whom were of Aryan tribes, and “scattered them far over the earth” (*pārā sárdhantaṃ nunude abhí kṣám*). In VII, 6, 3 “Agni assailed repeatedly those Dasyus and from the east turned the unholy ones to the west (...*pūrvas-cakāra-áparāṃ*...)”. Now, to take the last reference, since the Dasyus are, according to the AIT, the native Dravidians or Mundas or whatever else, they should have been driven south; yet the text says unequivocally “west”. Consequently, the *RV* text not only negates any Aryan entry and displacement of ‘natives’ but states explicitly that both Aryans and Dasyus were driven westward and far over the earth. Even if we allow for some hyperbole here, it does not seem out of the bounds of possibility, surely, that people moved out in

5 R Kochhar, an Indian astrophysicist, surpasses the staunchest Western invasionist in having most of the action in the *RV* (and the *Rāmāyaṇa*!) set in Afghanistan (2000). Curiously, he makes little use of his own science, Astronomy. He admits that there are more than 150 references in the epic *MB*, and lists several (53-7, 112-3), but thinks it “not possible to single out those verses ... contemporaneous with the battle” (but see sect V, 3, below) and mentions only two solar eclipses Oct 955 and July 857, which BN Achar shows to be quite wrong and irrelevant (2001: 4-5). He cites Erdosy 1995 but not K Kennedy’s paper therein (see IV, 1 below) which denies any demographic disruptions during or after the Harappan decline (ie no immigrants c 1900-1500); nor does he cite Shaffer (also in Erdosy) who, again, denies any intrusion before 600. Witzel dismisses most of Kochhar’s conclusions (2001: 11, n 26) but indulges his own fancy of finding reminiscences of foreign places (ibid §9) and criticizes Hock who denies wholly the value of such a practice (n 41).

6 *Br* = Brāhmaṇa. *VI* = Vedic Index.

numbers to Iran and the NE⁷. Some suggest that IAs moved out also to spread the Aryan culture peaceably (Frawley 2001: 6, 124, 168, 231, 290) – as in X 65, 11 ‘you spread the Aryan laws over the earth’. When we look at the historical records of other IE peoples we find that, unlike the IAs, many of them have preserved memories of migrations.

3. The Jews record clearly the early migration of Abraham from Ur (*Genesis* 11) – to mention also a non-IE people. The Iranian *Avesta* (Fargard I) provides strong recollections: it names 16 locations, from which the Iranians passed before settling down, not in strict geographical order, and among them is *haptahendu* which sounds like the *saptasindhavaḥ*, the 7 Rivers of *RV*; no doubt, there are many regions on Earth with seven rivers, but only in NW India at that time do we find an actual reference naming a land of 7 rivers. The Irish Celts record 5-6 waves of immigrations but no place of origin (MacCanna, 1983:54-63). The Scandinavians recall coming from the south-east and Sturluson gives Troy as the place of origin (*Edda* 1-5, 57-8). The Anglo-Saxons composed poetry showing clearly that "the consciousness of their origin from and the strong links with the North West Europeans continued long in the new land [ie England]" (Branston, 1993:22). The Greeks recall mainly, though not exclusively, through Herodotos (I 57-8; II 51; V 44; etc) that the Pelasgians were the autochthonous people on mainland Greece who stayed put in Attica and elsewhere, while the invading Greek-speakers moved all over and southward (for other details, Eleutheriadou, 1997:120-

7 Witzel wrongly states that "nowhere in the Vedas do we hear of a *westward* movement" (2001: 15, n42 & text). Considering that the *RV* was composed and preserved not as a text for modern scholars to play with, not as an encyclopaedia of Mythology nor a text for early Indoaryan History, the two references that I give (there may be more) should be considered adequate. He also ignores that those who left would not be there to write about it.

S Talageri traces meticulously in the *RV* a westward movement (2000, ch 4). I am not fully convinced yet by this, but it deserves consideration. Witzel launched a critique (2001b): the derisive title is indicative of his attitude: ‘Westward ho! The incredible wander-lust of the Ṛgvedic tribes exposed by S Talageri...’ But Talageri (2001) met most points in a counter-attack admitting some errors and showing that Witzel had made both errors and misrepresentations. As the conflict is extensive and the issues many, I shall take 3 points only. **a)** W accuses T of ignorance of linguistics and zoology over T’s taking *Jahnāvi* to be the Ganges (post-Vedic *Jāhnavī*) and *Śiṃśumāra* the Gangetic dolphin (2000: 110-1) saying *Jahnāvi* is the wife of *Jahnu* while the dolphin is of the river Indus. T points out that a woman *Jahnāvi* is totally unknown in the whole Indic literature but is twice mentioned in the *RV* while *Jahnu* is not mentioned once; then, that W himself identifies *śiṃśumāra* with the Gangetic dolphin in two other papers of his (1987; 1999 *EJVS* 5-19, p 30: *non vidi*)! See also T 2000: 465, where W’s identification is spelled out in full. This means of course that, apart from anything else, W did not read T’s book in full. **b)** W stresses (§ 7) that *RV* VI 45 is late: “Applying the principles pioneered by Oldenberg, *RV* 6.45 can be shown to be a composite hymn built of *ṛcas* at an uncertain period... Such late additions must not be used as an argument for the age of the bulk of book 6”. T points out that E V Arnold, who criticizes Oldenberg on some points, places this hymn among the oldest and that Witzel himself in his 1995 paper (p 317) states that this very hymn VI 45 is “an unsuspecting hymn” that is “not suspected as an addition”. **c)** W attacks (§ 9): “Iranian has none of the local Panjab and UP loanwords that are found in Vedic, which means that the Old Iranian languages cannot come from Panjab”. T replies: “W claims not only to identify ‘non-Indoaryan’ loanwords in Vedic, he can also identify the exact regions from which these ‘loanwords’ were borrowed: ... Punjab... UP... Bactria... loanwords! W knows, with scientific exactitude that loanwords, from *imaginary* ‘substrate languages’, which are found in both Vedic and Iranian are definitely from Central Asia and not from Punjab or UP and, equally, that loanwords found only in Vedic are from Punjab or UP not because his theory suggests these locations, but because he has found actual inscriptions from *pre-RV* eras in ... non-Indo-Iranian languages where these words are actually recorded!”

Enough said. W replied to T with one page and a half of vague generalities and the accusation that T used unreliable texts. He does not explain how he knows what the local lexicon was in ancient Panjab or UP!

59). Archaeologists are uncertain about the ‘coming of the Greeks’ and the date but for the proto-Greeks give c 1900 or 2200 or c 3200 (Taylour 1990: 14-5; Coleman 2000; Mallory 2001: 140-1); a second wave of invasions at just after 1200 is much more certain. The Romans too thought they came from Troy (in Vergil’s *Aeneid*). Though early (c1620), the Hittite writings record no immigration, but they are hardly distinguishable from those of other NE cultures; nonetheless, historians OR Guerny (1990: 13) and WE Dunstan (1998: 161) and J Puhvel, the eminent hittitologist and comparativist (1994), regard them as intrusive in the area.

Thus as far as historical records go (and despite the fact that most of these may be more or less mythological), in comparison with their relatives, the Vedic people have a better claim for being indigenous. We know from other sources definitely that the Balts, Celts, Romans and Slavs migrated.⁸ (See Elst, 1999:194-7; for details Mallory 1989, ch 3.) The same holds for the Germanic peoples (we have already mentioned the Scandinavians), though there is some uncertainty about their early movements: as their literature (except for Wulfila’s biblical translation into Gothic, 4th cent CE) is very late, ie 8th century CE (Baldi, 1983:124ff), any memory of a migration could well have faded away.

4. In summing up his extended survey of linguistic evidence Witzel tells us that the mode of the IA entry is "archaeologically **still little** traced"; it is, he states, securely traced in the texts (horses, chariots, religion etc) and from linguistics and possibly from future studies of the male Y chromosome (2001: 55-6). Here we have an attempt at falsification ("little" when in fact it is *none*) and wishful thinking. Neither horses and chariots nor linguistic phenomena, such as Witzel provides, prove any entry. They are *interpretations* of facts by a mind already coloured by the AIT. I shall deal with horses, chariots and linguistics in later sections. I do not use the epic and puranic genealogies and vast spans of time because I consider them unnecessary. At various places, but mainly §19 (p 57), Witzel dismisses as "*worthless*" all dates based on the native tradition that has the onset of the Kaliyuga in 3102. True, the tradition, which is found in Epics, Purāṇas and astronomers, which knows nothing of an invasion but, on the contrary, has the IAs spreading out with their culture in all directions, and which places the *RV* (its arrangement) just before 3102, appears to be late, within, say, 1st-6th centuries CE. But we must not ignore the weighty evidence of the classical sources (the Megasthenes report c312-280 BC) giving related chronologies. Arrian (*Indika* 1, 9), Pliny (VI, 21, 4) and Solinus (52, 5) – all give dates of 6000+ for Indian royal genealogies: so this aspect of the tradition is at the *very latest of the 4th century BC*. I am not claiming here that the tradition is necessarily correct in all (though it could be in most of) its aspects but only that it is not as late as it seems at first sight. The evidence provided now by Archaeoastronomy will have something to say on this matter (see sect V, below).

One final point on historical records. Grammarians (Yāska, Pāṇini, Patañjali) distinguish between sacred *mantras* and correct worldly usage and vulgar, ungrammatical speech, but not of loans from non-Sanskritic indigenous languages. In their attempt to keep Sanskrit pure they would have been doubly careful about such loans. True, loans in Vedic might well have been forgotten, but, equally true, we don’t know this for a fact and the dates for the grammarians are given in the light of the AIT and should be now *sub judice*. Similarly, the Greek and the Chinese visitors’ records of India mention no subject-people speaking a non-Sanskritic language. (In contrast, Herodotos states explicitly (I 58) that the Pelasgians in Attica learnt the Greek tongue and thereafter (II 51) began to consider themselves Greek. But they retained the memory of their former condition; Homer calls Zeus of Dodone ‘Pelasgian’ (*Iliad* 16, 233) while Aeschylus (*Suppliants* 250-9) talks of the *παλαιχθων palaichthōn* "old autochthon" Pelasgian.)

The IAs had developed unrivalled mnemonic disciplines for preserving their sacred texts and

8 Here I ignore the Baltic people’s ‘tradition’ of descent from India since it is very late and, perhaps, based on ideas current in the early 1800s, despite arguments to the contrary (Chatterji, 1968; also Singh 1995: 64-5).

culture through an oral tradition, though writing on highly perishable material cannot be ruled out altogether. I see no reason at all why they would not have preserved (as the Jews did) tales of their long trek to Saptasindhu particularly if, as the AIT has had it, the *RV* hymns were composed soon after the arrival c 1500. So many other people are not ashamed to admit they are immigrants. Why should they – especially when they admit internecine conflicts and other unrighteous acts?... Yet they have left no statement of immigration the way other cultures have. Of course the *RV* is not a record of historical events: we call it “mythology” but it was faithfully and scrupulously preserved by the IAs because for them it was sacred Revelation. This is analogous to the Hebrew writings of the Old Testament, which was also sacred Revelation not only for the Jews but also for billions of Christians. Just as the OT contains historical events like migrations, battles, successions of kings, etc, so does the *RV* – except that it mentions no immigration.

There is, then, nothing in the “written” records that supports the AIT.

IV) Archaeology, Palaeontology, Anthropology.

1. Writes J.M. Kenoyer, specialist in the archaeology of the Indus Valley: “[T]here is no archaeological or biological evidence for invasions or mass migrations into the Indus Valley between the end of the Harappan phase, about 1900 BC and the beginning of the Early Historic Period around 600 BC” (1998:174). Shaffer and Lichtenstein confirm this emphasizing the continuity of the indigenous culture (1999).

The absence of archaeological evidence began to be noted in the late 1960s. Jarrige and Meadow established (1980) the indigenous Mehrgarh culture with cereal cultivation c6500 on the Bolan, north-west of Mohenjadaro and its gradual spread south-east to the Indus where it developed into the Harappan or ISC(=Indus-Sarasvati Culture) c3000. Subsequent studies confirmed this: “The shift by Harappan groups and, perhaps, other Indus Valley cultural mosaic groups, is the only archaeologically documented west-to-east movement of human populations in South Asia before the first half of the first millennium BC” (Shaffer & Lichtenstein 1995:135).

Other investigators provide additional evidence from their own branches of research. No flow of genetic traits occurred from Bactria into Saptasindhu c1800: “Parpola’s suggestion of movement of Proto-ṚgVedic Aryan speakers into the Indus Valley by 1800 is not supported by our data. Gene flow from Bactria occurs much later and does not impact Indus Valley gene pools until the dawn of the Christian Era” (Hemphill & Christensen 1994). K Elst, who quotes this passage, explains that the later flow is apparently that of the Shaka and Kushana invasions (1999:232; also Bryant, 231). K Kennedy (in Erdosy, 1995) concurs with this view: “There is no evidence of demographic disruptions in the northwestern sector of the [Indian] subcontinent during and immediately after the decline of the Harappan culture” (again in Elst, 233; also Bryant, 231). However, Kennedy’s investigation of the skeletal record does show a break at c 4500.⁹ This may or may not have wider implications (see n 12).

More recent genetic studies show that this break is not indicative of an influx of new people, the IAs, and even suggest that European peoples descended from the inhabitants of SE Asia. Cavalli-Sforza and his team state that “Indian tribal and caste populations derive largely from the same genetic heritage of Pleistocene [=10000 to 3 mya] southern and western Asians and *have received limited gene flow from external regions since the Holocene* [=c 10000 to present]. The

9 Personally, I distrust such finds but only because, where they indicate a flow of genes, we do not know if it was a migration, or visiting merchants, or slaves, or whatever (Kazanas 1999:18-9). Witzel mentions the “male Y chromosome” that has not been studied yet adequately and quotes Cavalli-Sforza (2001: §7 & n23), but the Cavalli-Sforza work has difficulties and contradictions (1996:5, 29, 32, 210, 299, etc) and the whole discussion is full of uncertainty. Lord Renfrew pointed out that in this area there are “difficulties of methodology not yet resolved” and gave examples (1997:89).

phylogeography [=neighbouring branches] of the primal mtDNA and Y-chromosome founders suggest that these southern Asian Pleistocene coastal settlers from Africa would have provided the inocula for the subsequent differentiation of the distinctive eastern and western Eurasian gene pools". (Emphasis and square brackets added; Cavalli-Sforza 2003 These data have been put in after 2003 with some revisions for this publication.)

Another geneticist, S. Oppenheimer, offers independent confirmation (2003) that there was no Aryan entry, either male or female; he focuses on the M17, or so-called "Caucasoid" (=Aryan!), genetic marker: "South Asia is logically the ultimate origin of M17 and his ancestors; and sure enough we find highest rates and greatest diversity of the M17 line in Pakistan, India and eastern Iran, and low rates in the Caucasus. M17 is not only more diverse in South Asia than in Central Asia but diversity *characterizes* its presence in isolated tribal groups in the south, thus undermining any theory of M17 as a marker of a 'male Aryan invasion' of India" (2003: 152). He adds that this M17 marker travelled from India or Pakistan (= our Saptasindhu?) through Kashmir, Central Asia, Russia and then Europe after 50000 BP. (These data have been put in after 2003 with some revisions for this publication.) Thus migration is from east westward.

2. Witzel admits the absence of archaeological and palaeontological evidence for an immigration: "*So far* archaeology and palaeontology, 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" (2001: 9: my emphasis). In view of some linguists' disparaging remarks about Archaeology (see n 10 b and n 21) please note the change from "invasion" to "(im-)migration": Archaeology of course, and not Linguistics, showed beyond doubt that the Harappan cities were simply abandoned, not destroyed by enemy-attackers. Note also the qualifier "so far". It is customary with invasionists not to be content with stating the fact of absence of evidence but to add that it is only until now implying thereby that the evidence *will* turn up. Ten years after Dales' first publications on the true causes of the collapse of the Harappans and the work of other archaeologists (eg Rao 1973), Burrow wrote: "The Aryan invasion of India is recorded in no written document, and it cannot *yet* be traced archaeologically, but it is nevertheless firmly established as a historical fact on the basis of comparative philology" (1975:21: my emphasis). The terms "invasion" and "historical fact" betray a very deep prejudice no less than the little word "yet". This is a sample of the linguistic tyranny that Shaffer mentioned (sect II, above). Today, more than 25 years later, the evidence is still not forthcoming. Edmund Leach too dismissed linguistics and the AIT: "Because of their commitment to a unilateral segmentary history of language development that needed to be mapped onto the ground, the philologists took it for granted that proto-Indo-Iranian was a language that had originated outside India or Iran... From this we derived the myth of the Aryan invasions." Leach went further: "Indo-European scholars should have scrapped all their historical reconstructions and started again from scratch. But this is not what happened. Vested interests and academic posts were involved" (Leach 1990; Kazanas 2001a: §19).

R & B Allchin, British archaeologists (Cambridge), begin their 1982 study of the ISC by stressing its continuity but in ch 11 they usher in the Aryans; they also note the presence of several cultural elements in the ISC and point out their similarity with elements in "later Vedic Literature" (1982:203). Here one should note that it is **the later Vedic Literature not the RV itself**. In their 1997 study they again stress the continuity (p191), then, following A Parpola, they bring in the Aryans: "Their presence *should* therefore be in evidence archaeologically... But *as yet* it is **scarcely** attested in the archaeological record (...*presumably because*) their material culture and lifestyle were already indistinguishable from those of the existing population" (pp 221-2: my emphasis). Here we note the linguistic device subtly suggesting the existence of evidence (...*scarcely* attested) when there is none. Then there is the subtle contradiction in the pleonastic use of "as yet": for if the material culture and lifestyle were indistinguishable then obviously there will *never* appear any

distinguishing evidence. Note also the mingling of a hypothetical explanation (...presumably because) with actual facts. The Allchins are saying that the Aryans lived in close proximity to the ISC natives, acquired their "material culture and lifestyle" (ie clothing, tools, weapons, means of transport, building methods and the like) but not their language, *nor writing* (?!), then gradually infiltrated the ISC society or entered in two waves (so Parpola), took over and imposed their own language and religion expunging the native ones from the area. I might add here Witzel's "successive waves of migrations" (1995a:337)¹⁰ and trickling or "transhumance movements" (2001: §11).

There are enormous gaps in the logic of the alleged IA migration. The Harappans were literate and civilized, maintaining their 1000 year old culture through peaceful means, trade and perhaps religion – rather than expansion through war and conquest. Their area is given as 1.5 million sq km (Rao 1991:1), but I suspect it was much bigger: recent archaeological finds show that Harappan wheat was cultivated in the region of Bihar (ie far to the east of Saptasindhu) at 2300 (Frawley 2001: 70-71). Then c2000-1800, because of ecological and environmental changes including the alteration of river routes and the desiccation of the old Sarasvatī, they, or many of them, began to move eastwards to the Gangetic basin. At about this time enter our illiterate Aryans, according to the AIT.

3. The end result of all this is the aryanization of North India (Witzel 1995:106). But apart from non-testable vague conjectures, no invasionist gives a reasonable explanation of how this enormous event materialized in so vast an area. Renfrew used the model of *elite dominance* not precluding conquest or some form of violence. Witzel seemed to use a similar confused model in his 1995 study

10 a) In this study Witzel talks of a peaceful immigration but also uses the terms "battles" and "campaigns" (324), "initial conquest" (326) and "frequent warfare" (339) thus indicating that beneath the lipservice to "migration" (which became fashionable) lurks the notion of invasion. The impression is supported by his use as an (elite) "dominance model" of the "Norman French introduced by a few knights and their followers in Anglo-Saxon England" (2001: 30, n 85). He obviously does not know that the "few knights", (most of them literate, unlike the IAs) had in fact 12000 soldiers: led by William the Conqueror (no peaceful immigration with such a title), they hewed down King Harold and his loyal thanes then proceeded to destroy villages in Southern England until London accepted William as their lawful king (Trevelyan 1972: 106). The example was used also in Witzel's 1997 work (p xxii).

b) I can see why Witzel mentions C Ehret (2001:12, 22) but Ehret's views, formed mostly from well documented cases in Africa are not relevant to prehistoric Saptasindhu and, in any case, I would like to see some real paradigms especially where Ehret says that the "linkage of pottery and ethnicity breaks down in class societies" (1988:572). The reference to Ehret is thus neither illuminating nor relevant. The quote from Anthony 1995 (also vague and irrelevant) cannot be traced in Witzel's Bibliography.

Witzel's n177 (§20 on Archaeology) says that recently some archaeologists "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 few years they dug them up "and found the same material culture! So much for the often used and alleged overlap of language and culture". In other words, archaeologists are useless and should leave all such investigations to linguists. But note that this esteemed Professor does not bother to specify who were the archaeologists, what was the area, who were the communities, what were these "material remains" and so on. Instead, on this vaguest of generalities he condemns the entire science of Archaeology.(For similar views, see also n 21, below.)

That this superior attitude towards other scholars and scientists is customary is shown by a brief exchange between Prof Witzel and Dr Wujastyk (*Indology* website, On Line 8/3/2000). Wujastyk writes: "I am afraid that I neither trust nor believe anything said by atomic scientists about humanistic subjects (or most other subjects). Let's try to keep this list scholarly, shall we!" And Witzel agrees(!). Thus in this steam-roller statement scientists are considered incompetent or irrelevant for "most other subjects". To be scholarly is equated with indeterminate "humanistic subjects" – and a supercilious attitude(?). It is all rather extraordinary.

but abandoned this and, like the Allchins, adopted a more sophisticated process of acculturation and infiltration (2001: §10-11)^{10b}. But all the parallels given are not true, only approximate. I can't find a true parallel situation. The Kassites and Mitanni who came from the East and ruled Mesopotamia (Dunstan 1998) are thought to be "elite dominance" groups but, in fact, they lost their IE culture (preserved in some few linguistic elements, proper names and gods) and got totally engulfed by the highly developed, literate local culture(s). The Anglo-Saxons in Britain managed to drive the Celts westward and confined the Picts to the north, but they succeeded because they invaded in large numbers. When the Slavs (illiterate barbarians) made their waves of incursions into Byzantine Greece in the 8th century CE as far south as central Peloponnese, they were soon afterwards absorbed by the natives. The Vikings founded a state at Kiev in the Slav heartland and conquered Normandy but despite "elite dominance" they got absorbed into the Slavic and Latin cultures respectively. The Normans invaded England and Norman French certainly supplanted O English as the official language but they were numerous, literate and conquerors (n 10a above). We cannot find anywhere an example where an illiterate people immigrate peacefully in small waves then impose their culture on the literate civilized natives.

If there was acculturation and the Aryans had acquired the "material culture and lifestyle" of the Harappans *before* they entered into Saptasindhu, as the Allchins suggest, then the *RV* hymns ought to reflect some Harappan elements (urbanization or ruins, fixed fire-hearths, bricks, cotton, silver, etc); but it is the later Vedic texts, mainly the Brāhmaṇas and not the *RV* that do so (see §2, above, Allchins' statement). It is impossible that while texts many centuries later than the *RV* should contain ISC aspects, the *RV* itself, although being closer to the ISC, contains no traces of these aspects. But we must also ask why the Harappans, who were moving eastward then, should abandon their language and religion and adopt those of the Aryans. It is all very well to erect theories and models and write glibly about "elite dominance" and the like, but all this does not even so much as come near the question. A second question is why would they allow and accept the renaming of their mountains, plains and rivers. This too is not in the least explained satisfactorily. When immigrants arrive in a new populous country, they may retain their own culture, their language and religion, but, especially if they come "trickling" peaceably in small numbers, they learn the local language and customs and certainly do not rename places, rivers and lakes. Such at any rate has been the behaviour of the Jews and Gypsies in all the centuries of their wandering. Where renaming has taken place, it is invariably after invasion and conquest (though even conquest does not always have this result, for Greeks retained their religion and language and place names despite 400 years of Ottoman rule).¹¹

In this region of the Seven Rivers, then, we have an archaeologically well attested culture that seems to have no literature at all (other than the briefest inscriptions: no code of laws, no religious hymns or secular songs, no fables and tales), and then, in immediate succession, *an illiterate people, not archaeologically attested*, produces all the kinds of literature that the previous culture lacked. It is a most amazing paradox (first noted by D Frawley), an astonishing coincidence of space, time and people. The case of the Mycenaean and Minoan comes to mind. But the Mycenaean are well attested archaeologically; they produced no literature that we know of, only inventories (Ventris and Chadwick 1973), and were invaders and conquerors: so the parallel is not true.

One could of course suppose a model of cultural penetration combining technology with ideology (and perhaps religion), as English spread in the modern world. How feasible would such a model be? It would not be very accurate since (a) English was taken to North America, South Africa, Australia and New Zealand by different kinds of immigrants whereas in other countries it is

11 Many Greeks became Muslims, of course, either through coercion or choice. But while many others learnt Turkish and served as advisors to the Sultan, officials and even governors in some districts (Serbia, Rumania), they abandoned neither their Greek language nor their Orthodox faith. The Jews in diaspora have in some cases behaved similarly.

only the second language, as French and German were before the 1940's; (b) today we have very fast transport and satellite communications.

The AIT is, of course, possible – just as it is possible to be struck by lightning in one's bed, or to fall from the 10th floor on the lawn below and live with only a few bruises. Many wondrous things are possible in life, but the question is – do they really happen?... And because it seems so unrealistic I refuse to accept the notion of any kind of (im)migration. If there was an Aryan entry c 1500 and “aryanization”, it could only have been an invasion.

4. The supreme and overruling fact is that, despite decades of fervent search, there is not a shard of archaeological evidence (as Witzel admits, 2001: n7 and text) of an invasion. Many have done much earth-digging and book-research but have found nothing (eg Mallory 1989:46ff, 227-9; 1997: 113). It would be too tedious to refer to all this literature. Suffice it to quote Elst who has surveyed all the literature and says of B Sargent's 1997 *Genèse de l' Inde*, "the Indo-Aryan invasion doesn't get farther than Pirak in Baluchistan" (1999:320). V Sarianidi too cannot get any closer with his NE Aryan invaders(1999).

A final note on Archaeology. Much has been written about *pur* “fort,town (?)” and Witzel suggests this may refer to the Indus cities (2001: 63). There is only one insuperable difficulty applying to all scholiasts he cites: there is not one mention in *RV* of *iṣṭakā* ‘brick’, the material of the Indus cities, which, however, appears in post-rigvedic texts (see §2, above). The *RV* has purs of metal (!eg IV 27,1 *āyasī*) and of stone (eg IV 30, 20 *aśmanmāyin*). The stone *pur* is understandable in our terms. But what is the metal *pur*?... Furthermore, in VIII 1, 8 we find an extraordinary *pur* – *cariṣṇu* ‘mobile’ – which belongs to Śuṣṇa, generally thought to be the drought-demon. Now, how mobile and metallic purs can be Indus cities, or other immobile stone-structures found in adjacent areas, is not explained by all these experts. (No, I don't know what these purs are. But now see the paper on ‘Rigvedic *pur*’. I don't claim to understand much in the *RV*.)

Personally I would be content to rest here. The linguistic complications, which are not complications, can be easily accommodated and interpreted in this scheme of things. Nonetheless, there is more evidence of very firm nature, so we may as well look at it. I shall deal with the aspects of horses, chariots and the Sarasvatī further down.

V) Archaeoastronomy

BN Narahari Achar (of Memhis, USA) has used a combination of computer programmes to generate on the screen of a computer monitor the sky formation above any given location at any given time as far back as 8000 BCE. With his “Planetarium software”, as he calls his apparatus (2001:2), he has reconstructed a simulation of the ancient star positions in the sky above North India and was able to correlate various astronomical references in the ancient Indic texts with the formations (and dates) displayed in his monitor. Astronomical data have been under investigation since the early 1800s and many studies of varied results and quality were produced in the 20th century (Bryant 2001:35, 251-61; Witzel 2001: §29-30). I shall not be examining any of these because, (a) they are too numerous; (b) they have been examined for the most part by Bryant and Witzel; (c) they are all superceded now by Achar's Archaeoastronomy.

1. The first paper (1999, published in EJVS 5-2) dealt with a reference in *Śatapatha Br* II, 1, 2, 2-3 to the effect that the Kṛttikās/Pleiades are fixed in and do not swerve from the east. This reference has been examined, analysed, interpreted and discussed ad nauseam yielding all kinds of results according to the scholar's desires. S Kak arrived at a date 2950 (1994:35). This comes very close to what Achar finds, namely that such astronomical events could have been observed only c3000. Achar mentions that SB Dikshit had propounded the very same idea about 100 years earlier but later Western scholars rejected it by claiming that the *ŚB* phrase "never swerve from the east" means something else. Witzel writes an introduction to Achar's article suggesting we should not

really believe what the *ŚB* says and adds a long piece of his own on the Pleiades again "correcting" any "wrong" impressions we may have had from Achar's paper. All this is compressed in his 2001 study, §29; he adds the objection, however, that as "iron" is mentioned in the *ŚB* and the iron age does not begin in India before, say, 1200, the text cannot originate at c2950. He reiterated this objection in relation to another astronomical reference (2001: n 222). All this is hardly serious. If the Brāhmaṇa wanted to say something else, it would have done so. As for the use of "iron" – this objection will not stand. The Vedic texts (other than the *RV*) do mention a "swarthy metal" *śyāma* or *kārṣṇā-/krṣṇā-/yasa* but there are no references to smelting or other processing of this metal, only to the metal in contrast to other metals (*Vedic Index*). Since iron objects were found from 2600 in Harappan sites, Afghanistan and Baluchistan (Possehl & Gullapalli 1999:159-61), it is not remarkable that these texts should mention this metal. All this is well known to Witzel. Then, in Egypt, meteorite iron is known from 3000+. But there is another consideration probably unknown to most indologists. This "swarthy metal" could be blackened copper: to harden copper, the metal is heated up (but below melting point), then left to cool down without use of water and this *blackens* the copper, not with soot that can be wiped off; a similar effect is produced by oxidation with various sulfides (Hughes & Row, 1982: 92, 187). Thus the verse *AV XI 3, 7* speaks of flesh *māṃsa* being *śyāma* 'swarthy metal' and blood *lohita* being *lohita* 'red copper'; since the ṛṣis probably knew that flesh is produced from and maintained by blood, the correspondence is quite apt – reddish copper for blood and (processed) black copper for flesh. Some have claimed that all this is speculation on my part and ignore the decades of speculation that *śyāma-* or *krṣṇa- āyas* is "black iron"! There is no problem. Consequently I see no reason to doubt Achar's results.

2. I see no reason to doubt Achar's second paper (2000) regarding the date of *Joytiṣa Vedāṅga*. Here evidence is provided that an astronomical reference in this text is not of a date c400 as some western scholars think, nor of c1300 as others claim (identifying the star Dhaniṣṭha with, β Delphini) but c1800, when Dhaniṣṭha, now identified with δ Capricorn, receives sun and moon together for the winter solstice (Achar 2000:177). Witzel does not know of Achar's paper but quotes others with dates 1370 and 1150. His objection here is that the style of the *joytiṣa* is that of late epic and therefore the text cannot be earlier than the last centuries BC. This again is hardly serious. Are we to suppose then that a sober scientist is publishing fabricated evidence or that the writer of the text cast back somehow and concocted a misleading astronomical situation? And this because of the vague and variable criterion of "style"? But style and dating therefrom depend on chronologies derived from the AIT which has the IAs arriving at Saptasindhu c1500. This whole concept is now *sub judice*. Besides, the astronomical observations could have been recorded c1800 and the text retouched later. However, as the Indians preserved in ancient times so much of their lore through oral tradition, they could have preserved the late-epic style also for many centuries.

3. We now come to the epic *Mahābhārata* and Achar's third paper (2001). In this, Achar examined some astronomical references in Bks III, V and XIII of the *MB*. His sky map showed that of all calculations by Westerners and Indians only that of KS Raghavan (1969) was correct: the exact year for the great war of the Bharatas on the basis of all these data was 3067. In Bk V, to take some examples, Kṛṣṇa leaves for Hastināpura on the day of the *Revatī nakṣatra* in the month *Kaumuda* (=Kārtika, ie Oct-Nov) and arrives there on the day of *Bharaṇī* (81, 6ff); on the day of *Puṣya* Duryodhana rejects all offers of peace; Kṛṣṇa departs on the day of *uttara phālguni* and says to Karṇa that the *amāvāsyā* (day of the New Moon) will come after 7 days, then Karṇa describes the positions of some planets at that time (141, 7-10). All these data converge in agreement with Achar's sky formation only in the year 3067. Whatever other data are contained in the *MB* and whatever other dates are suggested thereby, the passages with the astronomical facts for the year 3067 remain unaffected. The ancient Indian tradition of the Purāṇas and astronomers was fairly correct in placing the onset of the Kali Yuga at 3102 and the Bharata war 35 years earlier: the disparity is only 70 years (Kazanas 2002). The medieval Kashmiri historian Kalhana (and his tradition), of course,

seems to have been quite right in setting the previous cycle at 3067 (Elst 1999:104).

Here however we must take into account that people begin to create tales and poetic cycles in fixed forms 2 or 3 generations after the event they celebrate when the actors have departed from the stage. So 3067 is a good date for the origin of the core of the *MBh*. It is the date when the sons and grandsons of the warriors began to recite/sing in established forms the deeds of their ancestors.

Neither Bryant nor Witzel mention this paper, so we don't know what sort of objections will be raised.¹² But, if these finds are correct (and I see no reason at all to doubt them), then, obviously, there is much work ahead for the younger indologists. As Leach said, the old chronologies and matters of style and so on will have to be scrapped and a new framework established. On the basis of the astronomical data the initial core of the *MB* belongs to the early 3rd millennium; the epic developed and grew in length in the subsequent 1000 years, when, c1800, perhaps, a new change of style, language and rearrangement of contents took place leading to the final form in the last centuries BCE. This is conjectural, of course.¹³

When, in 1996, I decided to abandon the mainstream view of the AIT and its chronology, I knew very little about astronomical data beyond the works of Jacobi and Tilak (both in the 1890s). Naturally it is good to have confirmation from Archaeoastronomy since heaven does not lie. But I had then done some comparative work on mythology and language (initially between Sanskrit and Greek) which also supported what the historical and archaeological records declared.

VI) The Preservation Principle (PP, for short)

In a 2001 paper I presented the Table of Deities that follows; it was built after extensive study. The IE branches examined were Vedic, Avestan, Hittite, Greek, Roman, Slavonic, Baltic, Germanic and Celtic; also some additional evidence from the Mitanni and the Kassites in the Near East. The Germanic branch comprises some early Germanic material (reported by Roman authors), some Anglo-Saxon and the later, richer Scandinavian lore. The Celtic branch consists of early Gallic (again reported mainly by Romans), Britannic, Welsh and Irish. (Other IE branches like Armenian, Tocharian, etc, provide negligible relevant material.)

I examined the various deities starting with the Vedic ones then moving westward. If I were to start with any other branch, I would soon need to shift to a different one and then another, because very few names of non-Vedic gods have correspondences in the other branches.

12 Achar's work gives a concrete terminus. The IAs are definitely in Saptasindhu by 3000. Since the *RV* antedates the ISC we must postulate an earlier date. As the only other indication is the break in the skeletal record c 4500 (IV, 1, end, above), we could posit a putative entry at this date. If archaeologists agree with this, then the IAs are immigrants at c 4500 but autochthons surely by 1500. My main concern since 1996 has been to show that the IA-speakers were long-settled inhabitants (*palaichthon*) in Saptasindhu and that the *RV* is pre-Harappan. I do allow the possibility that IA had come at a much earlier date.

13 Witzel derides (2001: n170) the idea of "kings" in the Gangetic plains of the 7th millennium "when this area was populated by a few hunter and gatherer tribes". Here again preconception projects backwards models of a later age and gives later significance to rigvedic *rājan* 'king', *sabhā* 'assembly', etc. There is nothing remarkable about a tribe of gatherers and hunters having a "monarch" in 6000+ or 60000+. If there is a group of people, someone of necessity will be "first among equals" and if his leadership proves good he is bound to pass into history/ legend.

Civilization (or culture) surely is not just technology with large buildings, fast machines and weaponry, but, more importantly, an inner state whereby people know and give to all beings that which is due. (See eg the discussion in Plato's *Republic* 369Bff, especially 372A-B.) R Rudgley cites Prof Yasuda Yoshinory who found "a marvellous principle" for civilization, that is "a respect for and co-existence with nature" and wrote further: "Civilization begins to appear when a workable system for living, that is a proper relationship between man and nature, is established in accord with the features of a given region". Yoshinory wrote this in respect of the Jomon Culture in Japan, 10700 to 400 (Rudgley 1998: 31-3).

Abbreviations for languages used are: Av=Avestan; E=English; Gk=Greek; Gmc=Germanic; Gth=Gothic; Ht=Hittite; Ir=Irish; L=Latin; Lth=Lithuanian; Ltt=Lettish (or Latvian); Mcn=Mycenaean; OE=Old English; OHG=Old High German; ON=Old Norse; Rs=Russian; S=Sanskrit; Sc=Scandinavian; Sl=Slavonic; V=Vedic; W=Welsh.

<i>Vedic</i>	<i>Other IE branches</i>
Agni :	Ht <i>Agnis</i> , Rs <i>Ogon</i> , L <i>ignis</i> , Lth <i>ugnis</i> , Ltt <i>uguns</i> . (Note: even the Iranians who had Fire-worship did not preserve this name, not even as a demon like Indra, Sauru etc, though the stem appears in the name <i>dāstāgni</i> .)
Aryaman :	Mcn <i>Are-mene</i> and Gk Ἄρης <i>Ares</i> ; Celtic <i>Ariomanus</i> (Gaul)/ <i>Eremon</i> (Ireland); Sc <i>Irmin</i> . The ar-stem in most IE languages.
Aśvin-	Celtic <i>Epona</i> (Gaul); Mcn <i>Iqeja</i> (horse-deity). Gk ἵππος, <i>hippos</i> (Mcn <i>iqo</i> , dialect <i>ikkos</i>) L <i>equus</i> , OE and Ir <i>eoh</i> , Baltic <i>ešva</i> .
Bhaga :	Kassite <i>Bugas</i> ; Sl <i>Bogu</i> ; Phrygian <i>Bagaios</i> (Ζεύς, <i>Zeus</i> Gk); Gk Φοῖβος <i>Phoibos</i> .
Dyaus :	Ht <i>DSiu-s</i> ; Gk Ζεύς/Δία- <i>Zeus/Dia</i> ; Roman <i>Ju[s]piter</i> ; Gmc <i>Tîwaz</i> . Lth <i>dievas</i> (usually ‘god’ cognate with S <i>deva</i> , √ <i>dñ</i>).
Indra :	Ht <i>Inar(a)</i> ; Mitanni <i>Indara</i> ; Kassite <i>Indaš</i> ; Celtic <i>Andrasta/Andarta</i> . Gk ἀνῆρ/ἀνδρ- <i>anēr/andr-</i> , Av <i>indra</i> (a demon).
Marut-as	Kassite <i>Maruttaš</i> ; Roman <i>Mars</i> ; Irish <i>Morrighan</i> . The stem <i>mar/mor/mer-</i> etc is common in all IE branches.
Apāṃ Napāt :	Roman <i>Neptunus</i> ; Celtic <i>Nech-tan</i> (Irish). Gk ἄνεψ(=πσ)-ιός <i>anēpsios</i> ; L <i>nep-</i> ; OHG <i>nevo</i> , OE <i>nefa</i> , OLth <i>nep-</i> , etc.
Parjanya :	Slavic <i>Perunŭ</i> ; Baltic <i>Perkunas</i> (and variants); Sc <i>Fjörgyn</i> (-n, Thor’s mother).
Ṛbhu	Gk Ὀρφεύς <i>Orpheus</i> ; Gmc <i>Elf</i> (and variants). Gth <i>arb-aips</i> (?); OSL <i>rabu</i> , R <i>rabota</i> ; L <i>orbu</i> (S <i>arbha</i> , Gk ὀρφανός <i>orphanos</i>); etc.
Sūrya :	Kassite <i>Šuriaš</i> ; Gk Ἥλιος <i>Hēlios</i> ; Roman <i>Sōl</i> . Gth <i>savil</i> , ON <i>sol</i> , W <i>haul</i> , OSl <i>slunice</i> , Rs <i>solnce</i> , Baltic <i>Saule</i> .
Uṣas :	Gk Ἥως <i>Ēōs</i> ; Roman <i>Aurora</i> ; Gmc <i>Ēostre</i> . Lth <i>ausra</i> , Ltt <i>ausma</i> , W <i>gwawr</i> , etc.
Varuṇa :	Mitanni <i>Uruwna</i> ; Gk Οὐρανός <i>Ouranos</i> ; Baltic <i>Vēlinas</i> (– and cf <i>jur-</i> = sea). L <i>ūrīna</i> , O N <i>ver</i> (=sea).
Vāstoṣ-pati :	Gk Ἑστία <i>Hestia</i> ; Roman <i>Vesta</i> . Gth <i>wisan</i> ‘to stay’; OHG <i>wist</i> ‘inhabiting’; Toch A/B <i>wašt/ost</i> ‘house’.
Yama :	Sc <i>Ymir</i> ; Av <i>Yima</i> .. L <i>gemi-nus</i> (=twin); Gk ζῆμια <i>zēmia</i> (=damage), Av <i>yam</i> ,

Attn: to this table should be added S *Mitra* and Av *Miθra*; also S *Śrī* and L *Ceres* (Gk *Kēr*?); inadvertently Ht *Agnis* was omitted from the original Table (while Ht *Wurun* (?) was wrongly included with *Varuṇa*). For a more up-to-date Table and discussion see paper ‘The Diffusion of

Indo-European Theonyms', (Kazanas 2006),

Here the upper line shows the incidence of the deities and the lower shows the cognate stems that occur in languages where the deities are not preserved. Thus it might be argued that the Rs 'Ogon' is a direct borrowing of the Vedic Agni, who is an innovation, and, less plausibly, the same for Roman *Neptunus*, Celtic *Nech-tan*, but the presence of the cognate stems in Latin and Baltic for *agn-* and in Greek and Germanic for *nep-/nev-* nullify such arguments. Some deities, again, may be disputed (eg Gk Οὐρανός *Ouranos* or Sl *Bogu*) and be removed, but the pattern would not change appreciably. There are many more motifs that have been preserved in Vedic as against other individual branches. In fact there is hardly a major motif common in two or more of the other branches that is not found in the *RV* (full discussion, Kazanas 2001c, 2001d).

The all-inclusiveness of Vedic is all too apparent and quite remarkable. Greek and Germanic managed to preserve only half as many deities as Vedic. Yet, to take some examples, Gmc preserves the stem *nep-/nev-* and *savill/ sol* but not the corresponding deities which are preserved in other branches. Greek too preserves *nep-*, *andr-*, and *zēm-* but, again, not the deities. Just as surprising are the very meagre retentions in Baltic, Slavic and, even more, in Anatolian. This situation can arise only from loss of memory of the significance of the mythological-religious figure over a long period of time because of lengthy geographical movement and/ or absorption of new elements (sometimes forcibly, perhaps, through subjection) from other culture(s). It is a well known fact of history that people on the move for a long period tend to lose elements of their culture while their language suffers changes, as they meet with other cultures and/ or have little leisure to pass their lore to the new generations – much more than a people remaining sedentary (Lockwood 1969:43; Burrow 1973: 10; Hock 1991:467-9).¹⁴

This then I call the Preservation Principle or PP hereafter: the people or culture that has preserved most *ceteris paribus* has moved least. I examine and re-affirm this with some 500 lexical items in my paper 'Coherence & Preservation in Sanskrit' 2005, Vishvesvarananda Vedic Research Institute *Research Bulletin* (Hoshiarpur).

2. The all-inclusiveness of the *RV* in the realm of mythology is also observable in the sphere of poetics. There is hardly a major poetic device in the various IE branches that is not present in the *RV*. A significant aspect, for example, is that in early Greek poetry (epics of Homer and Hesiod, and some epigraphic material) the fairly strict syllabic metre (the hexameter with its dactylic, iambic and other variants) is preponderant with only traces of alliteration; in Germanic poetry alliteration prevails while the syllabic metre is very loose: both are present in the *RV* (Kazanas 2001d). The situation becomes very clear in the detailed examination of the large range of material in Watkins 2001. Early Irish poetry (6th century CE) has both metre and alliteration (and rhyme) but this hardly counts since the Irish poets knew these poetic devices "from Vergil and Ovid" (Watkins 2001:121) and, of course, the Romans developed them from the Greek culture. Of the Vedic poetic art Watkins writes: "The language of India from its earliest documentation in the Rigveda has raised the art of the phonetic figure to what many would consider its highest form" (Watkins 2001:109).

14 Elst thinks otherwise (1999:121) and cites the example of Old Icelandic as being Proto-North-Germanic although Iceland was not the urheimat. The example is unfortunate. Old Icelandic moved from Scandinavia in the 9th century CE. From that area, in the 1st century CE (or thereabouts) the Goths moved south-east ending in (what is now) Ukraine c300 CE. So it would seem that the Old-Norse location was a sort of urheimat and a conservative one until about 700 CE when it began to undergo rapid change as the Vikings began to move in many directions (Lockwood 1972:96-7 & 121-2). This is not quite parallel to the movements of other IE and Gmc (sub-) branches: first, the Norsemen moved to Iceland within a short period in the 9th cent (during the oppressive reign of Harold Haarfagr) and, unlike all other branches, in ships; second, travel in ships is different from that on land with carts and animals; third, the ships met no other cultures on the seas, which subsequently protected Iceland from other influences. In these respects Old Icelandic is quite unique and therefore unsuitable for comparisons.

3. "The language of India from its earliest documentation in the Rigveda": in this sphere too the PP operates most profoundly. Burrow, whose *The Sanskrit Language* (1973) is still the authority in this field, says: "Vedic is a language which *in most respects is more archaic and less altered* from original Indo-European *than any other member* of the family" (34: emphasis added); he also states that root nouns, "very much in decline in the earliest recorded Indo-European languages", are preserved better in Sanskrit, and later adds, "Chiefly owing to its antiquity the Sanskrit language is more readily analysable, and its roots more easily separable from accretionary elements than... any other IE language" (123, 289). Nobody, as far as I know, has even attempted to dispute this and the presence of dialectal variants and innovations or erosions and losses in Vedic (and Sanskrit) does not invalidate Burrow's judgement. Vedic is superior also in respect of its inner organic cohesion: from roots *dhātu* by simple and fairly regular processes are generated primary (*kr̥t-*) and secondary (*taddhita-*) derivatives in nominal and verbal forms.¹⁵ This organic cohesion of Sanskrit is another example of the PP, confirming that the IAs moved very little or not at all.

4. Another aspect of the organic cohesion and the PP deserves a look. Consider the word 'son' and its cognates in other IE languages. Apart from S *sūnu*, it appears in Gmc branches with the stem *sun-* (also O Norse *sonr*), Gk υἱός- *huios* (and dialectal variants), Av *hunu*, Slavic *syn-* (and variants), Toch A *se* and Toch B *soy*, etc. It does not appear in Latin, in the Celtic branches or in Hittite. The stems for 'sow', which are generally accepted on philological grounds as cognates of the forms of 'son', also have widespread incidence; Gmc *sū* (*gu*), Gk σῦς, ὄς *sūs, hūs*, L *sūs*, Av *hū-* etc. Curiously, in no language do we find other cognates, nouns or verbs, nor an explanation of the relation between 'son' and 'sow': apart from late developments the two words hang isolated. Sanskrit provides both a plausible explanation and several cognates. Skt *sūnu* 'son' is a derivative from root $\sqrt{sū} > sūte$ 'beget': this is quite a regular formation, as with $\sqrt{gr̥dh} > gr̥dhnu$ 'eager, greedy' or $\sqrt{bhā} > bhānu$ 'shinning-one, sun', etc. The root *sū* gives in S not only a full declension for the verb 'beget' (*sūte*, *sauti*, etc., pr; *asāvīt* etc., aor; *suṣāva* etc., prf; *pra-sū* etc.) but also a host of nominal forms: *sū*, *sūti* (fem 'birth, production'), *sūtu* (fem 'pregnancy'), *sava* (m 'instigation'), *savitṛ* (m 'impeller'), *savitṛī* (fem 'mother'), *sāvaka* 'generative', etc. In S 'hog' or 'swine' is *sū-kara*, which some interpret as 'making the sound *sū*' while others connect it with $\sqrt{sū}$ 'begetting' – and the latter sounds the more probable since hogs/swine make grunts not hisses. Thus, when the nominal *sū* 'begetting, begetter' is taken also into account, Sanskrit furnishes a plausible link between 'son' and 'sow'. The word 'daughter' also is orphaned in English, Gmc *tochter*, Lth *duktē*, Gk θυγάτηρ *thugatēr*, etc; again S *duhitṛ* alone has a root *duh* 'milk' (cf \sqrt{jan} 'generate' > *janitr̥*, \sqrt{van} 'win' > *vanitr̥*) and other cognates. Another interesting example is 'mouse'. This too remains isolated in English, Gmc *mūs*, L *mūs*, Gk μῦς *mūs*, etc and only S *mūṣ-aka* has a root *muṣ* 'steal' (cf \sqrt{car} 'move' > *caraka*, $\sqrt{yāc}$ 'ask' > *yācaka*) and many cognates which denote theft, plundering and the like (and in Md Gk a thief is still known as a "mouse"). A final example is the 'sun'. In almost

15 Greek, which is supposed to be very close to Sanskrit, lags far behind. No common root emerges from φαρέτρα *pharetra* 'quiver', φέρω *pherō* 'bear', φόρος *phoros* 'tribute'; or φθείρω *ptheirō* 'waste', φθάρ-μα *phtharma* 'corruption', φθερ-, φθορά *phther-*, *phthora* 'ruin'; or πλέω/πλείω/πλώω *pleō/pleiō/plōō* all 'sail', πλευσ- *pleus-* both verbal and nominal, πλόος/πλοῦς *ploos/plous* 'sailing', πλοῖον *ploion* 'ship', πλωτήρ *plōtēr* 'sailor'; πέτο-μαι *petomai* 'fly', ποτάο-/ποτέο-, *potao-/poteo-*, contracted verbal/nominal stems πτα-/πτε-/πτη-/πτο- *pta-/pte-/ptē-/pto-*, and reduplicated πίπτω *piptō* 'fall', πτω- *ptō-*; δίδωμι *didōmi* 'give', εδο- *edo-*, εδω- *edō*, δέδο- *dedo-* verbal forms, δόσις *dosis* 'giving', δωτήρ *dōtēr* 'giver'. Even where scholars now give hypothetical roots (eg \sqrt{do} - \sqrt{do} - for δίδωμι *didōmi*) the formation of verbal and nominal forms is mostly haphazard, as the examples I give testify (and there are many more). The same applies to all other branches – and worse.

all IE languages the solar globe is masculine: Gk *ἥλιος hēlios*, L *sol*, S *svar/ sūryā*, etc, but in German it is feminine. Again Vedic alone has also the fem *sūryā* ‘sun-maiden’, thus covering both genders.

Perhaps philologists better qualified and equipped than myself will turn their attention to this aspect of language as much as to the isoglosses, uncertain loanwords and conjectural PIE asterisk forms. I am well aware that there are cognations in IE branches exclusive of Sanskrit, particularly the common words in the languages of North-West Europe. But (a) many words listed by eg Meillet (1908: ch 1) can be shown to be connected with S stems: Meillet himself mentions some, then rejects them, while with L *vas (vadis)* ‘pledge’ etc may be cognate S *√vad-* ‘speak’ and with L *homo* ‘man’ etc S *ksā/jmā* ‘earth’. (b) The examples I give for Sanskrit explain lacunae in other branches and are derivatives of roots having other verbal/nominal cognates within Sanskrit itself.

5. No other IE branch preserves so much of the PIE inheritance as the Vedic tradition; and a cursory glance in Baldi 1983 or any other similar study will demonstrate that the Vedic language itself suffered far fewer losses (Kazanas 2000:87-90). Retaining the dual to this day, conservative Lithuanian has, even so, lost the neuter gender and the ablative case, reshaped its verbal system and regularized the comparison forms of adjectives. As for Slavonic, which for many centuries occupies one of the most favoured urheimat (the Pontic Steppe or the Urals), though it came here long after the early dispersal, it did not retain the common IE stems for ‘horse, dog, town, copper’, having instead *kunji* (and variants), *pis-*, *grad-*, *med-* respectively. The difference in the actual preservations in Vedic is far too great to be assigned to chance. No people who had been on the move for many decades, if not centuries, could have preserved so much.

On the basis of the foregone discussion, I think it is as certain as can be in present circumstances that the IAs are indigenous and that the bulk of RV was composed in the 4th millennium, before the rise of the ISC, while some of it may be far older. However, there are several other secondary aspects that seem to present problems. These are mainly archaeological and linguistic and I shall now attempt to offer solutions. Be it noted that these aspects are thought to be problematic only because of the heavy assumptions upon which depends the AIT.

VII) Horse and chariot

1. How do we know what kind of animal denoted the rigvedic word *aśva* (also *atya*, *vājin*, *haya* etc)?... Certainly the Greek, Latin etc cognate words denoted a particular equid, but how can we be sure that the rigvedic animal is the same?... After all, the only description we have is of an equid with 34 ribs in RV I, 62, 18 (a late hymn). This may contain a mythological element as Witzel says but since, on the other and real hand, there is a 34-rib horse (Witzel 2001: §21, and n 184), why assume that the Rgvedic *aśva* is of necessity the 36-rib horse found in other IE regions. Thus when R S Sharma describes domesticated horse remains at Mehagara dated at 5000 as "an isolated species of a horse distinct from the one inhabiting areas in the USSR, Iran, Afghanistan etc. and associated with the Aryans" (1995: 17), he has very obviously prejudged the case. Since the rigvedic horse has 34 ribs (I 62, 18), it has not come from distant lands where the horse has 36 ribs: like the IAs the rigvedic horse is indigenous.

Horses were used in racing (*āji*) and (more often mares) for pulling cars. One hymn undoubtedly refers to horses (*atya*, *aśva*) in races (*āji*). Another refers to war. Some hymns refer to horses running but not necessarily in a race (IX, 36,1; 74,8). Others refer to races (IV, 35 and VIII 80) but not necessarily horses. Hymn X, 102 is the only one that describes at length a chariot race and here the animals are oxen: this is the Mudgala-Mudgalānī race. Hymn III 53, 17-8 also has oxen, while stanza 5 has *vājin rāsabha* either ‘horse’ and ‘ass’ or ‘fast ass’ for Indra(!). The ass is not unusual since I, 34, 9 also has the yoking of *vājin* (‘horse’ or ‘fast’) (and) *rāsabha* (‘ass’), here for the Aśvins; also I 116, 2 and 162, 21! (Could one speculate further that *vājin* and perhaps *aśva* might at times denote ‘ass, onager, hemione’?). In post-rigvedic texts we find more races, often with

horses. In the early *Aitareya Br* IV, 7-9, however, in a race among gods, Agni's car is drawn by mules, Usas's by cows, Indra's by horses and the Asvins' by asses. It is a most curious situation that a post-Ṛgvedic text should have all these animals yoked to race chariots while that of the horse-deities themselves (Aśvins) is drawn by asses. But then in the *RV* the Asvins' chariot is often said to be drawn by birds (eagles in I, 118,4; swans in IV, 45, 4; birds unspecified in VI, 36, 6; etc). Pūṣan's car, again, is pulled by goats (VI, 55). Dawn's car is drawn by oxen (I 92; V 80) as often as by steeds (III 61; VII 75).

All this suggests to me that, contrary to widespread belief, horses may not have been plentiful at all periods and in all places. Certain hymns mention, of course, large numbers of horses: VI 63, 10 has 100s and 1000s; VIII 46, 22 has 60000! In VIII 55-3, 400 mares are mentioned in a *dānastuti* "praise of gift". What would anyone want with 400 or even 100 horses let alone thousands, unless they had a large force of cavalry? Or they drank the mares' milk and ate horse meat. Or have we here hyperboles?.. Other hymns speak of very few horses: IV 32, 17; VI 45, 12; etc. Now, if there were plenty of horses why should a sage like Vāmadeva (IV, 32) be praying to Indra for horses (for his whole clan, the Gotamas)?... Perhaps, and I repeat perhaps, the horse was not so common in Saptasindhu as is usually thought. Elst (1999:181) and R Thapar (1996:21) suggest that the horse was "symbolic of nobility" thus giving social status.

Now, Witzel cites R. Meadow and A.K. Patel to the effect that no **clear** examples of horse-bones have been found in the area before 1700 (2001: 59). What we are not told is that this paper by Meadow and Patel (i) seeks to refute S. Bakonyi, who actually does accept finds of horse remains at Surkotada, and (ii) was completed in 1994 (publ. in 1997) and therefore does not cover data presented in late 1994 and after. Be that as it may, B B Lal (1997: 285-6) presents sufficient evidence for horse in the ISC. He dismisses as suspect the evidence at Rana Ghundai (p 162) but finds evidence at Lothal, Surkotada and Kalibangan though he states "one would like to have more and more examples" (p 286). Kochhar also, who advocates the AIT, mentions horse remains at different sites of the ISC found in well-established strata before the alleged IA entry (c.1700 – 1500) from 1800 to 2155 (2000: 186, 192). GR Sharma who favours the AIT found ample evidence for wild horse c 18000 and domesticated horse 6570 to 4530 at the Bolan and Son Valleys (1980: 110 ff.; also Kazanas 1999: 33-4);¹⁶ this is in the Ganges basin well to the east of Saptasindhu. These bones were reexamined by another non-indigenist Indian, RS Sharma, who confirmed the early date for domesticated horse at 5000 and some c1000 (1996:17). How many horse-remains would satisfy invasionists?... Of the many millions of dead humans in the ISC (who were cared for often through burial) only a few hundred skeletons have been unearthed, so we should not have excessive demands for horses.

There are now several reports for horse remains from mature ISC.

(i) Allchin and Joshi found "lumbar vertebrae of horse" at Malvan, a Harappan site at Shaurashtra (1995: 95).

(ii) Dhavalikar (1995: 116-117) reports horse bones unearthed at Kuntasi, periods I and II (=2300-2000).

(iii) Thomas *et al* found 9 bones of true horse (0.13% of the total faunal remains) and 9 bones of the onager at Shikarpur from mature Harappan levels, ie c 2300 (1995).

Finally, there are the terracotta horse figurines. Their presence in ISC sites was acknowledged by Thapar and Mughal (1994: 254). Then Lal states again that the horse was present in the ISC and presents in addition the photograph of a horse figurine from mature Harappan levels in Rakhigarhi (2002: 73ff). Thus I take it that there is now sufficient attestation of the horse in the ISC.

2. As for the chariot, the basic assumption that the rigvedic *ratha* was like the chariots of the NE or Europe in the 2nd millenium may be justifiable under the preconceptions of the AIT but it is not

16 The Report was published by Allahabad University. Sharma had as co-workers Dr. M. Williams and K.Royce, members of a team under Prof. J. Desmond Clark.

warranted by the testimony of the *Ṛgveda*. Although many references to *ratha* and its aspects in the *RV* are mythological and we cannot be certain that they apply to human physical realities, there are enough others to enable us to form a good picture. The many more realistic details in the later Vedic texts are too far removed in time to be of indubitable relevance. Many interpretations of rigvedic issues suffer from precisely this drawback: because of insufficient information in the *RV* scholars seek help from later texts and even from non-Indic material, always under the spell of the AIT. Such procedures have generated assumptions that are untrue and arguments that are circular (as those noted by Bryant, pp 117, 144, etc). Here I shall use only rigvedic evidence and such references from later texts as do not affect it; I shall ignore historical semantics since most such material comes from IE branches of late attestation.

Witzel refers at length to an Egyptian chariot of the 15th century (now in Florence) with parts of it made of elm, ash, oak and birch, all imported from places like south Russia, and weighing 30 kg (2000:6). He does not say here that this is like the rigvedic chariot but as he states elsewhere that the latter also weighs c 30 kg (2201: n 192), this is what he intends. This may be legitimate but utterly irrelevant and misleading since the rigvedic vehicle is made of *śalmali* (X 85,20; also *kiṃsuka* ?) or *khadira* and *siṃśapā* (III 53,19) and its axle of *aratu* (VIII 46,27) - all these woods being native to India. We have no information at all about its weight.

Most of the evidence is collected in the *Vedic Index* under *Anas* and *Ratha* and all other erudite studies add nothing - except confusion imported from other texts and/or non-Indic material. Under *Anas* it is said that the cart is "sometimes expressly contrasted with the chariot (*ratha*) for war or sport": the reference III 33,9 is given (but note that the phrase "for war or sport" is not of rigvedic origin but an imported notion that beclouds the matter). This hymn doesn't present any express contrast: it says simply (in stanzas 9 and 10) that Visvāmitra "has come from afar *ānasā rāthena*, ie "by means of *anas/ratha*" which may mean "by cart [and] chariot" or "by cart [which is] chariot" (or vice versa). One must wonder here why a priest of high order, a renowned ṛṣi who displays magical powers in stopping the onrush of the river-waters, would need a chariot "for sport or war". The *VI* corrects its first statement saying (now under *Ratha*) that "this distinction [between *anas* and *ratha*] is not absolute". Indeed, Uṣas has *ratha* in (late) I 48,10 and (early) III 61,2 but *anas* in (early) IV 30,11 and (late) X 73,7. Indra, the mighty warrior who is called arranger *ājikṛt* and lord *ājipati* of the race (or battle: VII 53, 6-14), is said to be *anar-viś* (in late I 121, 7) "seated on a cart" not chariot. The references are by no means exhausted but enough has been said to show that, in fact, there is little if any distinction in *anas/ratha* : "of differences in the structure of the two we have no information" (*VI, Ratha*).

3. Measurements and dimensions of the chariot are given in the much later Śulba Sūtras, so I shall ignore them. But there is one passage in the *RV* that is helpful (perhaps more). In VI 61,13 the river Sarasvatī is likened to a chariot: *rātha iva brhātī* : "like a chariot tall/big/stately/bright". So if a large river is compared to a chariot for size (*brhat-*), the chariot cannot be a small and narrow contraption of 30 kg. (In III 33,2 a river is again compared or related to a chariot *rathyā+iva* but the size is not explicit here). This hint of large size is reinforced by the references that follow.

These vehicles, *anas* or *ratha*, were drawn by 1,2,3 or 4 animals. "Horses were normally used for chariots but the ass (*gardabha*) or mule (*aśvatari*) are also mentioned" (*VI, Ratha*) as indeed we saw above. What is surprising is that while in the Upanishads the cars are said to have two wheels, in the *RV* they have one wheel (I 53,9 & 164, 2; VI 54,3; VIII 63,2 where the sun is obviously meant), sometimes 3 wheels (e.g. that of the Ṛbhus in IV 36,1), sometimes 7 (II 40,3) - all obviously mythological. Once the car has 2 wheels and, all-golden, is that of the Aśvins (VIII 5,29, again mythological) but in 6 other instances this car is said to be 3-wheeled *tricakra*. In X 85,14 the car is again *tricakra* but in the next stanza *one of the wheels is missing* ! It is not clear to me whether the Aśvins had a 4-wheeled car, now left with three, or a 3-wheeled car now left with 2 wheels. These

cars have another curious aspect in the *RV*: the *ratha* has normally "seats" or space for two, the driver and another, but often it has space for three. I am not referring only to the *Aśvins'* car which carries the Sun-maiden too, but also to III 6,9 and VI 47,9 where the *ratha* carries three and more on its *vāriṣṭhe ... vandhūre* : "widest seat/box". Then in (late) X 53, 7 we find a chariot *ratha* that has seating for 8 *aṣṭāvandhura*.

All these details (plus the fact that, as we saw in §1, above, the chariot is drawn by an ass or ass and horse) constitute the picture of a vehicle that is not at all like the (war) chariots appearing in the 2nd millenium in the NE. P Raulwing's admirably erudite study on the IE chariots and horses sheds not one ray of light on the rigvedic vehicles. The evidence for the development in the NE of the first light chariots for war (Littauer & Cronwel 1996) as against the Pontic Steppe (Anthony & Vinogradov 1995) seems fairly convincing. But neither the former not the latter tell us anything useful about the *RV*.

Witzel took umbrage (2003) to this view accusing me of using only mythological evidence (which is untrue: see VII,1 re Mudgala race) but himself used the most obviously mythological references (*Aśvins'* golden wheels, *RV* 8.5-59; etc and *RV* 10.85.11 which speaks of *Suryā's* bridal car which has *the sky as its covering* (st 10) and is in fact *manas* 'the mind', but, because he probably used Geldner's German translation, he thought this was *ratha* whereas it is *anas* (st 10, 12!) etc).

4. Further confusion comes from projecting non-Indic material onto the minor aspect of the *rathavāhana*, in VI, 75,8. Mentioned by Witzel (2001: n192) and usually translated as a platform or large vehicle for transporting the supposedly fragile, light (30 kg) chariot, the word occurs also in *AtharvaVeda*, III 17,3 in a list - "cow, sheep, *prasthāvad rathavāhamam* and a lusty fat girl": all these a plough should dig up (*lāṅgalam ... udvapatu*). This passage with slight variations is found also in some *Brāhmaṇas*. If we put aside any notions from NE (hypothetical) parallels and modern racing-cars (Witzel *ibid*), it is very difficult to see how this "chariot-transport that-has-a-support or -a-platform" (surely pleonastic ?) fits with the cow, sheep and girl. To my mind the whole phrase seems to be a metaphor for a horse ("chariot-puller that-has-stability") but I wouldn't bet on it and WD Whitney translates "on-going chariot-frame" (note: *not* 'platform'). The word occurs also in the prose texts and there it may have the meaning "platform, conveyor", though in a text like *Baudhāyana Śrauta Śrūta* XI, 6, 72,8 it is probably used metaphorically: *athaihi yajamāneti: ratha eṣa dakṣiṇe śronyante rathavāhana āhito bhavati*: "Come, O sacrificer! he says; this chariot is placed on the platform [which is] the southern hip (śroni-) [of the altar]". All such references are much too late. (See Sparreboom 1985 *passim* for the mainstream view).

The use of the word in *RV* VI 75,8 can be taken differently. First of all, if the *ratha* was dismantled and placed on the platform, as is generally thought (Sparreboom, p. 30), this hymn would have been an ideal place to mention such a fact. But the hymn does not say that wheels and box are separate; in fact the chariot *ratha* is not lauded *per se* as other things are. Here the *rathavāhana* itself is equated with the oblation *havis*, just as in other passages the hymn or thought offered is given in the figure of a chariot (eg V 29, 15). Then since the chariot itself is not lauded (as the warrior is in st 1, the cow in 2, the good charioteer in 6, horses in 7, the whip in 13, the mail or armour 18-9), it seems reasonable that *rathavāhana* is the chariot itself, lauded *per se* in st 8; to use Whitney's translation, it is the "(on-going) chariot-frame". There is no need to assume a chariot-carrying platform.¹⁷ Pāda 8c *tātrā rātham ūpa śagmām sadéma* is translated by W. O'Flaherty

17 *rathavāhana* could mean "the conveying by chariot" or "the chariot's [function of] conveying" or even "the conveyor that is a chariot". Some might refer to grammar rules and accent and insist that here is an Acc *tatpuruṣa* "chariot-conveying". I would simply remind that the accent is not invariable (e.g. *áśvam-iṣṭi* "horse-seeking" but *viśvam-invá* "all moving", etc). Then we find *aritra-páraṇa* "going-across with oars" and not "going-across oars" (Macdonell 1916: 454-7).

(1981:237) as "on it [i.e.the platform] let us place the working chariot". This rendering can hardly be correct since the verb

upa-sad- means "sit on/by, approach respectfully" and the like (the causative *upasādaya-* alone means "place on"): so the phrase should give "let us revere/honour the efficient chariot." Furthermore, this very stanza says that upon this vehicle are already laid *nihita* weaponry *āyudha* and mail *varma*. So O'Flaherty and others say that on this "platform" weapons and mail are first laid and then the chariot itself. Is it likely that practical men would load the chariot (whole or dismantled) afterwards and thus possibly damage the weaponry? Of course not.

It may be thought that my interpretation of the *rathavāhana* is far-fetched and ignores the traditional meaning given by scholars. This may be so, but the more I read the hymns the more I wonder whether the traditional, generally accepted meanings that I learnt formerly are true. Consider one well-known passage. Every sanskritist knows the *Nāsadiya Sūkta* X 129: pādas c and d read *kīm āvarīvaḥ kīha kāsya sārman āmbhaḥ kīm āsid gāhanam gambhīram*: roughly 'What covered (or was-there-covering)? Where? Whose protection? Was it *ambhaḥ* profound unfathomable?' All translations that I know give 'water' for *ambhas* (Geldner: 'Wasser'); even Jeanine Miller, who always approached the hymns with great sensitivity and strove to bring out some spiritual significance, here translated 'water' and connected this with other mythologies (1972: 68). As most mythologies/religions speak of "water" at a very early stage of creation (Judaic, Egyptian, Mesopotamian, etc) it is assumed that *ambhas* (connected with *abhra*, *ambara*, *ambu*, all denoting 'cloud, sky, rain, water') also does the same. But this cannot be justified: it is negated by the first hemistich which states unequivocally "Then there was no existence nor non-existence, nor space (*rajas*) nor the upper heaven". How, when there was no existence of any kind (known to us), could there be "water"? There couldn't. Here the word *ambhas* means 'potency' (Mayrhofer: *Gewalt*). Behind the traditional (Western) thinking is the notion "Why shouldn't the rigvedic mythology be like the other *primitive* mythologies?" And so this preconception will continue to produce translations of "water", ignoring what the text says and what plain logic enjoins. The hymn here is significantly different from other mythologies in advocating a Primal Unity as the First Principle of all creation. Similar preconceptions (and prejudices?) operate and, I think, misinterpret or ignore the data about chariots and many other rigvedic matters.

Final point. Even if some rigvedic vehicles resembled the early one-man, two-wheeled chariot of the NE, this does not mean that the Vedic *ratha* came from there and, still less, from the Urals.¹⁸ The Harappans already had the technology for its construction. For other types of vehicles, of course, there is ample evidence: "a more sophisticated type of vehicle with one or two pairs of wheels ... is known from the Rhine to the Indus by around 3000 BC" (Piggott 1992: 18; for more details Kazanas 1999: 33). The absence of actual remains does pose a difficulty but is not in itself evidence of absence.

VIII) The river Sarasvatī

1. Many hymns in nine of the ten books of the RV (but not the 4th) extol or mention a divine and very large river named Sarasvatī. This flows mightily "from the mountains to the [Indian] Ocean" *girībhya ā samudrāt* VII, 95,2 and gives sustenance to many kings and the five Aryan tribes that were settled along its banks (VI 61: etc) and along the Indus, *Dr̥ṣadvatī* and the other rivers. In historic times the river appeared to be a minor stream *Sarsuti* (<*Sarasvatī*) or Ghaggar or Hakra,

18 In yet another misrepresentation Witzel writes: "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". (2001: n 194). However DK Sethna makes it quite explicit (1992: 50-1) that these identifications were first made by Parpola and other Finnish scholars; he merely followed! Parpola is an invasionist and co-editor of Witzel's EJVS!

which ended in the desert at Bhatnair, far from the ocean so that modern scholars (Roth, Griffiths et al, VI vol 2, 434) thought that the poets referred, in fact, to the Indus which alone still flows to the ocean and justifies such references.

Witzel does not think that Sarasvatī tells us much about the date of the *RV* and the IA origins; in fact he doubts it is the Indian river at all, writing: "Whether the immigrant Vasistha [i.e. the poet of VII 95 hymn] was from the Iranian area of *Haraxvaiti* (= Sarasvatī Arachosia) or not, he may have echoed the praise of the *ancient* Sarasvatī, that is the local S Avestan Haraxvaiti ... or he may just have spoken in the hyperbolic style of the *RV*" (2001: 66).¹⁹ It would be too time-consuming and tedious to describe all the various notions Witzel has resorted to in the last few years both in publications and on the Internet to explain why the Sarasvatī is not really the Sarasvatī but some other river "having pools" or being in Afghanistan or flowing in the sky as the Milky Way, and so on. I shall deal only with Witzel's point about the river Beas capturing the river Sutlej from Sarasvatī and thus diminishing it: in the end Sarasvatī flows to *samudra* which is not the ocean but only terminal lake(s) (2001: 65). This is thought important by Bryant also (2001:168). But before proceeding I should agree that Sarasvatī is also a goddess and had a celestial aspect for the IAs (eg *RV* V 43, 11; VI 61, 11; X 17, 7-9) as the river Nile was also the Milky Way in heaven for the Egyptians (Shafer 1991: 108).

2. Sarasvatī is praised, like no other river, in many hymns and to confine the discussion, as Witzel does, to two hymns III, 33 and VII, 95 ignoring the information contained in others, is not good scholarship. Hymn III 33 indeed says that ṛṣi Viśvāmitra, domestic priest to King Sudās, went to the confluence of the two rivers Beas and Sutlej and there asked the rivers to hold their current so that he (and the accompanying Bharatas) could cross. People, of course, do not usually go to a confluence, where the current is most turgid and vehement to cross over: they either go higher up and cross two rivers or lower down where the flow is smoother. However, since Viśvāmitra was a mighty sage, he could perform miracles, so the rivers complied with his request and stopped flowing. The word *samāraṇa* also suggests a "confluence". But to me it is not as clear as I would like. Then, in the late X 75 the river Beas *Vipās* is not mentioned, only Sutlej *Sutudrī* (st 5): this suggests that the latter absorbed the former rather than the other way round! Adding to the confusion, III 53,9 says that Viśvāmitra induced only one river to hold back (*astabhñāt*) its flow: here it may be a different river, Sindhu itself (*sindhu* = "river in general" and "Indus"), in which case Viśvāmitra made a habit of stopping rivers! The *VI* thinks III 53, 9 to be a (later) reference to the same feat (in III 33): indeed, it is a brief reference in one half-stanza (a & b) and it includes King Sudās (c-d) while the three following stanzas pray that Sudās defeats his foes and wins wealth and the Bharata people be protected. (For what it is worth, Indra too stops with his *māyā* the overflowing waters of river Vibālī in *RV* IV 30, 12. In V 31, 8 also he stops the rushing waters for Yadu and Turvaśa. Then in VII 18, 5 Indra made "the spreading tumultuous waters shallow and easy for Sudās to cross": this hymn in praise of Indra is by Vasiṣṭha but seems to refer to the same incident as III, 33, which is a hymn in praise of Indra by Viśvāmitra.) The situation as a whole is not at all clear. (*Nirukta* II 23-5 & *Brhaddevatā* II 135-7 add nothing to our understanding.)

Be that as it may, when we turn to the information in other hymns we see that Sarasvatī is a very great river in the later hymns as well, except *perhaps* in X 75. In II 41,16, Gṛtsamada calls Sarasvatī, *nadītamā, ambitamā and devitamā* "best river, best mother, best goddess". Following Oldenberg,

19 It is worth noting that Haraxvaiti is one of the 16 places the Iranians had passed through before settling down. See section III, 3 above. In Indo-Iranian linguistics the sound shift is from *s* to *h* (*Vsu, somal Av hñ, haoma* etc); so *Sarasvati/Haraxvaiti* would indicate a movement out of Saptasindhu (as also *Haptahendū*) rather than the reverse. This suggests to me a movement of IAs north-westward and eventually into Iran. It could have been a large contingent, and the areas sparsely (or not) populated so that the immigrants could give new names reminiscent of their homeland.

Witzel considers Bks II-VII to be the oldest ones but also gives several conditions which nearly nullify this (eg “the ‘younger form’ of a hymn does not always signify its ‘younger age’,” 1995: 309-11). Thus he considers II, 41 and III 33 to be older and therefore describing the river as it was initially and then losing one of its tributaries and so shrinking. But in addition to the later VII 95 (see above), VI 52,6 also praises Sarasvatī, which is (still) **swollen *pinvamānā* by many rivers *sindhuhīḥ***: so ṛṣi Bharadvāja tells us that this river is still fed by many others! Thus even if Sutlej had been weaned away, there were still enough rivers pouring into Sarasvatī and keeping it swollen and large. Then hymn VI 61, 8-13 lauds the river as endless, swift-moving, roaring, most dear among the sister-currents and, together with her divine aspect, nourishing the 5 tribes. Surely it can’t be that the ṛṣi of Book VI also like “immigrant Vasiṣṭha”, twice lauds an ancient or non-Indic river. In the definitely later VIII 21, 17-8, the ṛṣi Kāṇva says that king Citra bestows wealth only like Indra or Sarasvatī and that Citra and many lesser kings dwell along Sarasvatī: this does not sound like a diminished river. Then, VII 96,2 (as well as 95) prays to the rivergoddess for sustenance and good fortune and so does again X 177 while X 64, 9 calls upon Sarasvatī, Sarayu and Indus as “great” and “nourishing”. All these poets can’t be referring to an ancient river or a shrivelled one (and these 3 hymns are not singled out in Witzel’s 1995 paper as early).

3. Witzel writes: "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 the water to the Sutlej (and even earlier, much of it also to the Yamuna). It was no longer the large river it *might* have been before the Ṛgvedic period [i.e. before 1200]" (2001: 66: emphasis in the original; square brackets mine). This statement is of course a gross misrepresentation of the situation since it ignores the facts. These middle and late hymns (VII 95 and 96, VIII 21, X 64 and 177) describe *in the present* the river’s greatness, say that many kings live along its banks and pray for sustenance and good fortune *without any hint anywhere* (in these or other hymns) that the river has shrunk (as indeed we find in later texts like eg *Manusmṛti* II 21 and scholiasts thereon). If Sarasvatī had shrunk, surely these hymns would be lamenting such a misfortune and praying for the reappearance of ample water – as Vasiṣṭha confesses former sins to Varuṇa and begs for forgiveness in VII 86 and 89 or as the gambler bewails his predicament and begs for release in X 34.

Consider 3 more points on this. (a) The terminal “lake(s)” is etymologising on Witzel’s part since *Sarasvatī* is usually rendered ‘she who has pools’. The word *saras* certainly came to mean ‘pool’ and this may refer to the sources of the river being lakes which formed as the ice was melting: considering the name was given when the river was at its grandest (from the mountains to the ocean), it would refer to lakes at the origin and not the terminal point. But there is another aspect here. The root \sqrt{sr} and all its derivatives imply ‘motion, extension, running-on’; *saras* too originally meant most probably (not ‘pool’ but) ‘whirlpool’ or ‘eddy’ in a river’s current. Sarasvatī was the river with the mighty current and strong swirls. (b) Hymn X, 75 does not give prominence to Sarasvatī but praises *sindhu* which may be the river Indus or *the Spirit of Rivers deified*; in any case, it is a list of the rivers from east to west and Sarasvatī is in the correct place, after Ganges and Yamuna. (c) More important, hymn III 33 speaks of the two rivers Vipāś and Śutudrī (st 1-2) as rising from the mountains (*parvata*) and flowing down to the ocean *samudra*. Or should we here also take *samudra* to be “a terminal lake”, as Witzel would have it (2001: 76) for Sarasvatī in VII 95,2? In both we have the rise of the rivers from mountains and their flow to *samudra*. But I 71,7 also says *samudrām ná sravātaḥ saptá yahvīḥ* ‘[sacrificial offerings turn to Agni] like the 7 mighty rivers flowing to the *samudra*’: is this *samudra* too Sarasvatī’s “terminal lake” into which turn/flow all 7 rivers?... I find it more reasonable to take *samudra* as the ‘ocean’ and that Sarasvatī also flows there.

To sum up, Sarasvatī in all of the *RV* is a mighty river that nourishes the 5 Aryan peoples (and many kings) along its banks as is shown by VI 52, VI 61, VII 95 and 96, VIII 21 and X 64 and 177.

All these were composed before 1900, when by most accounts the river lost its tributaries and shrank (Rao 1991: 77-9; Allchins 1997: 117). Consequently the (bulk of the) *RV* must be dated prior to 1900 and since it knows nothing of the ISC²⁰, as Witzel too admits, it must be even older, i.e. before 3000.

IX) Some linguistic considerations

I have no desire, and it would take too long, to follow Witzel along all his tortuous twists and turns in the conjectural philology of proto-languages. I shall deal only with four aspects three of which are considered important by Bryant.

1. Witzel admits (2001 :§18) that the earlier Kassites who conquered Mesopotamia had IA elements but attempts to show that the somewhat later Mitanni language has no such elements and that any similarities derive from a Pre-Vedic Old Indo-Aryan. Fact is that once you start postulating Pre-This-language and Proto-that-language, you can prove almost anything you like in this field. Other philologists are fairly explicit about this matter. Burrow accepts the IA presence in Mitanni and that the elements "are to be connected specifically with the Indo-Aryans ... partly on *linguistic grounds* [my emphasis here] and partly on ... the Aryan gods mentioned ... [who] are specifically Vedic... [and] the word *eika* - which corresponds to Sanskrit *eka* whereas Iranian has *aiva*-" (1975 : 28-30). Bryant endorses Burrow's view finding "Indo-Aryan prominence in this field" (p 136). S S Misra, Professor of Linguistics at Benares, argues along similar lines (1992:13-4) but he is an indigenist and Witzel feels he can deride him describing his work as "nothing more than a cottage industry exploitation of a new popular trend"(2001: §12,1 and § 12,4 - 12,8).

Why Witzel, Professor at Harvard, thinks it fit to insult his colleagues is not very clear.²¹ This mode of attack reaches a climax in his article in *Frontline*, a marxist journal, in which he assails some publication which allegedly used fraudulent material (*non vidi*) but goes beyond this to insult all indigenists, Indian and Western alike, non-academics and academics, including Misra (2000: 14). Why he should have resorted to a marxist magazine to publish such a critique when India has so many academic journals is not very clear either.

2. More important is the matter of **loanwords**. A number of words in Vedic, not having correspondences in other IE branches and not being capable of resolution into some (hypothetical) PIE form are thought to be borrowings from Dravidian or Munda or "some unknown northeastern language" (Hock 1996: 113 citing B Tikkanen). Several scholars advocate this borrowing (Burrow 1973, Mayrhofer 1956-, Kuiper 1991&1995, Witzel 2001). This is said to be very important, if not

20 a) Another revealing point is Witzel's misrepresentation of ruins "*armaka, vailaṣṭhāna*, cf Falk 1981) in the *RV*" (2001: §22). The implication is that there are references in the *RV* to ruins, particularly I, 133(*armaka, vailasthana*) and that H. Falk (1981) presents all this evidence. This is very far from the truth. Falk deals with *kapāla* "potsherds" in *the later texts* (1981: 167 ff) as one would expect. *RV* I, 133 does not refer to any ruined cities as Burrow tried to show (1963: 159-68): there is no mention at all in this hymn of ruined houses or walls, no stones or bricks (building material of the ISC) - only ghosts and friends (see Kazanas 2000: 95 and n 14; also Sethna 1992: 130-4).).

b) He misrepresents Misra again, writing (2001: 34) "Misra's main thesis, emigration from India, has already been refuted on linguistic grounds by Hock (1999)." Hock, on the contrary, admits the possibility of emigration *on linguistic grounds* and rejects it on quasi-archaeological considerations about horse and war-chariot (1999: 13). See § IX 3, below.

21 G. Erdosy, another invasionist, refers to S. Talageri, whose publications (1993, 2000) may contain errors and faults but are fairly honest attempts to present the indigenist case (see Bryant 2001 : 65-6, etc), as a "lunatic" (Elst 1999:55). The same scholar makes curious remarks about archaeologists who, he thinks, do not understand "gradual and complex phenomena" consequently "denying the validity of any migrational model" (1995: xiii, xv): here we have an implicit charge that archaeologists are obtuse whereas invasionists are perspicacious and imaginative - which of course is derogatory sophistry (see also n 10c, above).

decisive evidence and Bryan states “any discussion of Indo-Aryan origins that neglects the substratum data simply cannot be taken seriously”(107). Excellent! But first let us consider some pertinent points.

To begin with, the disagreement among linguists is startling. A long line of non-indigenist mostly Western scholars, including Emenau (1980), expressed grave doubts about the nature and the extent of this borrowing: P Thieme doubts Burrow’s lists, Hock those of Mayrhofer and R Das the list of some 380 words of F Kuiper (Bryant, 84-90); Misra also rejects Kuiper’s list (1999: 17-8). In the 1980’s Emenau rejected the lot, even the unknown tongue “totally lost to the record”. More recently Witzel rejected Bryant’s suggestions (2001: nn 64, 65, 66). It is worth quoting Das, who is a non-indigenist but on different grounds: “*not a single case* [exists] in which a *communis opinio* has been found confirming the foreign origin of a Ṛgvedic (and probably Vedic in general) word(...) many of the arguments for (or against) such foreign origin are often ... statements of faith” (1995:228, emphasis original). Let one example suffice: Burrow gives *bala* ‘strength’ as of Dravidian origin (1973: 384) but Mayrhofer (under *balam*) connects it with Gk βελ-τίων, L *de-bilis* etc. Such disagreements may not impress philologists themselves since they are habituated to disputes and controversies but to others they indicate that something important is amiss. Just as remarkable is the fact that some scholars reject Dravidian and Munda and postulate an unknown language - as if every word in Vedic must have a correspondance in some other IE branch or the (hypothetical) reconstructed PIE. What is strange here is that scholars discuss this in seriousness not realizing that they are attempting to explain in this complicated science-fiction fashion what needs no explanation, as many of them hint anyway! If, as I maintain (sect VI, above) Vedic *is* the oldest IE branch preserving many more linguistic and cultural elements than the others, but not without erosions too, then it is just as likely that the other branches lost any trace of these alleged loans. (But see two paragraphs from end of this §2.) Also, if we postulate one non-IE language, why not have two or more?... Where would this lead us then?... What is the benefit? We are dealing with totally unknown and unhelpful entities. All we can say with realistic certitude is that *there may have been loans from X language(s)*.

To what extend can we trust the reconstruction of proto-languages?... True, many fine results have been obtained by the detection (or application) of philological laws operating in large areas of a language or of a family of languages, like IE. However, the old but regular correspondance of S *bh* and Gk *ph* as also in the perfect stem *lebhe* and εἴληφα *eilēpha* ‘have received’ would lead us to expect a Gk present stem *λαφ-/λαμφ- **laph-/lamph-*, but in actuality this is λαβ-/λαμβ- *lab-/lamb-*. Hock (1991) gives many more examples with laryngeals (582), Armenian *erk-* (583), Baltic, Slavic and Germanic (585) and so on (up to p 591). Thus it is equally true that there are areas where the general rules do not apply and we would reach wrong conclusions – but for the documentation. No law, moreover, can account for the variations in the following S and Gk correspondences: *tiṣṭhāmi*/ἵστημι *histēmi* ‘stand’, *dadāmi*/δίδωμι *didōmi* ‘give’, *dadhāmi*/τίθημι *tithēmi* ‘put’ *piparmi*/πίμπλημι *pimplēmi* ‘fill’ (the Gk nasal, an echo of *pr̥ṇāmi* ?) and *juhomi*/χέω *cheō* ‘pour’ (cf aor pass ἐχούθη, *echuthēn* χύτρα *chutra* ‘pot’ etc which suggest a root-stem *χv- chu-* similar to S *√hu*). Can we be 100% sure then that the forms we ‘reconstruct’ for protolanguages, (and especially Munda), are actual? I don’t think so; nor would any judge and jury in an impartial court of law. Writing on Kurylowitz’s ‘laws’ of analogy, Hock, one of the most eminent scholars in Comparative Philology, states: “a prediction of when change will or must occur is impossible” (1991: 211). And of course, without the necessary analogical information (ie documentation of some kind) we cannot predict *what* the change will be.

Let me give some examples. In Greek the guttural plosive γ (=g) is now pronounced as a fricative, sometimes like *y* in ‘you’ and sometimes like *w* in ‘wolf’ – and this in both initial and medial positions. It is a general law without any exceptions. However there are many words in Md Gk that did not originally have an initial γ but developed it from different vowels and in some cases

through loss of vowels: eg γεράκι < ἱέραξ *geraki* < *hierax* ‘hawk’, γερός < ὑγιηρός *geros* < *hugiēros* ‘healthy’ (but cf γέρω/ων/ον- *ger/-ōn/-on* ‘old’), γιάλ-/γυαλ- < ὑάλ- *gial-/gual-/* < *hual-* ‘bright’ (Md Gk ‘glass’), γιαλός < αἰγιαλός *gialos* < *aigialos* ‘shore’, etc. Without the documentation we would be ‘reconstructing’ some very absurd unreal forms. Without documentation no law could possibly lead back to the originals (second in each of the pairs): δένω/δέω *denō/deō* (and δίδημι *didēmi*) ‘bind’ (cf μένω/μένω *menō/menō* ‘abide’: no change), λύνω/λύω *lunō/luō* ‘loosen’, χύνω/χέω *chunō/cheō* ‘pour’; γλείφω/λείχω *gleiphō/leichō* ‘lick’, (cf ἀλείφω/ἀλείφω *aleiphō/aleiphō* ‘besmear’: no change). Then take the group – αδράχνω *adrachnō* (fut -δράξ- *drax-*) ‘grasp’, δείχνω *deichnō* (fut δείξ- *deix-*) ‘show’ and σπρώχνω *sprōchnō* (fut σπρωξ- *sprōx-*) ‘push’. With the help of S *dis-āmi* (fut *deks-*) and L (*in-*)*dic-are* both ‘show’, an original **dek-/dik-* may be postulated; encouraged we may now take S *sprś-* (fut *spraks-*) ‘touch’ or, perhaps, *sprh* ‘be eager’ (and L *sparg-ere* ‘strew, hurl’) as cognate with σπρώχνω *sprōchnō*, and S *drś-* (*darś-* fut *draks-*) ‘see’ with -δράχ-νω *-drach-nō*; then we postulate a law that original *κ>χ* *k>ch* with an intrusive *-v-* *-n-* or an original *-κν-* *-kn-* with loss of *-v-* *-n-* in Sanskrit and Latin. This ‘law’ is then applied to other similar verbs and we have original **ρίκνω* **rhiknō* for ρίχνω *richnō* ‘throw’, **διώκνω* **diōknō* for διώχνω *diōchnō* ‘drive away’. Unfortunately, all this apparently ‘scientific’ approach is sheer fantasy and only διώκ-ω *diōk-ō* (without the *-v-* *-n-*) would be correct: for the rest the originals are δείκνυμι *deiknumi*, (α-)δράσσω (*a-*)*drassō* (or δράπτω *drattō*), for ρίχνω *richnō* ῥίπτω *rhiptō* and for σπρώχνω *sprōchnō*, after several incredible changes, (εἰς-?)*προσθεῶ* (*eis-?*)*proōtheō*! Without documentation analogous fantasies will, of course, be generated by the following: θλίβω *thlibō* ‘press/pinch’, νίβω *nibō* ‘wash’, σκύβω *skubō* (pronounced *skivo*) ‘bend’, στρίβω *stribō* ‘turn/twist’, τρίβω *tribō* ‘rub’, etc which come from θλίβω *thlivō* (no change), νίπτω *niptō*, κύπτω *kuptō*, στρέφω *strephō* and τρίβω *tribō* (no change). I trust I am forgiven for not having any confidence at all in comparisons and reconstructions of this type (without documentation).

E. Pulgram discussed caustically the same dangers with regard to Latin and the Romance languages (1958: 147). He also dismissed ‘linguistic palaeontology’ with very good reasons – as many others have done before and after him (see also Bryant, ch 6). As S Zimmer put it (1990): “The long dispute about the reliability of this linguistic palaeontology’ is not yet finished, but approaching its inevitable end – with a negative result, of course”. Indeed, some few scholars still resort to it judiciously (Elst 1999: 130-3; Witzel 2001: 42-6) to prove their (opposite) view.

Now, if so many important changes in a language occur, despite literacy, outside the influence of any recognizable regular law within only a few centuries, then the changes in a language without literacy (unless it has a strong hieratic oral tradition like that of the Indoaryans) in a period of over a millennium can hardly be calculable. “The hypothesis of an unknown substratum ... is methodologically dubious (...) The enormous difference between the Indo-Aryan [*alleged!*] arrival ... and the first attestation of the northwestern languages places formidable obstacles” (Hock 1996: 79-80, my square brackets). Surely then, when reconstructions of protolanguages, and consequently phenomena arising from them, cannot really be trusted, and when eminent linguists cannot agree among themselves about the nature and extent of the subject under discussion (in this case loanwords), how could we and why should we take this sort of ‘evidence’ seriously?...

However, I do accept the possibility of the presence of other non-Sanskritic languages in the larger area and of loanwords in Vedic. After all some denote important items, like *khala* ‘threshing-floor’, *bila* ‘hole’ and *lāṅgala* ‘plough’ and occur frequently. Nonetheless, even if all these loanwords and other similar linguistic elements are taken to be valid, this does not indicate an IndoAryan intrusion, even if such elements are present only in Vedic and in no other IE language. As I pointed out on the Internet (2001a: n8) we can envisage such non-IE influences coming from south and east of Saptasindhu and affecting all adjacent dialects but not those further north-west

(which in fact emigrated – before, during or after the arrival of the foreign elements).²²

Not surprisingly, Bryant himself arrives at a similar conclusion: “As far as I can determine, there is very little that is decisive that can be brought forward to deny the possibility that Dravidian or Munda speakers intruded upon an Indo-Aryan speaking area and not vice versa” (105); here again there is, in fact, *nothing* decisive beyond the AIT itself. In support of this Bryant cites Witzel who provided data that “Dravidian influence is not visible in the earliest layers of the R̥g but only in subsequent layers” (102). And he concludes: “In short, while certainly suggestive, it is difficult to see how the ‘evidence’ of a linguistic substratum in Indo-Aryan, in and of itself, can be used as a final arbitrator in the debate over Indo-Aryan origins” (107). Bryant should have added that there is nothing else.

3. *The ‘Out of India’ theory (=OIT)*. Even more important is Hock’s article which discusses the possibility of Old IndoAryan being the PIE language and the possibility of IEs emigrating out of India (1999).

Hock rightly rejects the notion that PIE was Vedic, but he is wrong in ascribing this view to Misra, who makes no such claim as far as I know. Misra exhibits some sentiment, perhaps because of patriotism and religious feelings, but he is careful throughout his study (1992) to keep Sanskrit quite distinct from PIE: “the Sanskrit language has also suffered linguistic changes and the original Indo-European proto-speech was not the same as Sanskrit” (p2); also, Sanskrit “is the oldest Indo-European” and the “nearest language to original Indo-European” (p84).

Then Hock, unaware of J Nichols’s evidence which requires a locus of dispersal at Bactria - Sogdiana (unlike his own vague “vast area from East central Europe to Eastern Russia”, p17), nonetheless indicates that there are no substantial linguistic arguments against the proposition that IE branches moved out of India. He states that apart from the gypsy emigration, there are “three more IA languages moving out of India: Gandhari Prakrit (in medieval Khotam and farther east), and Parya (in modern Uzbekistan)... and Dunaki (close to present-day Shina)... to the outer northwestern edge of south Asia” (also in Hock 1996: 82). He states also that the PIE could “a priori” have been “originally spoken in India” (p11) and rejects the idea *not on linguistic* but archaeological (!) grounds (p13) of the kind usually employed by invasionists (horse and chariot). This, as we saw (sect VII) is no real difficulty. On p14 we find Fig1, the genetic table of the IE branches which is generally accepted. On p15 is Fig2, the isoglosses which, despite uncertainties about archaisms and innovations, again present no difficulties (see next § 4). On p16 we read that if the model in Fig1 is accepted, then the OIT would be “relatively easy to maintain”. He then invokes the “principle of simplicity” as an additional difficulty (p16): one migration into India as against many out of it. But he ignores the fundamental fact that there is plenty of evidence of IE branches invading the areas they occupy but there is none for India: this makes considerable difference, surely. What is more, this “simplicity” applies equally to all proposed homelands. Now, if the difficulties are insuperable or grave, then a view is *not* “relatively easy to maintain”; if it is so, then there are no serious difficulties. (And “relatively” to what?... There is only the (opposite) view, the AIT.)

Just as important is Johanna Nichols’s view (1997, 1998) stating (in her Introduction) that “the locus accounting for the distribution of loanwords, internal innovation and genetic diversity within IE could only have lain well to the east of the Caspian Sea” (1997:123). As my article has gone well out of the limits originally envisaged, I won’t reproduce her detailed analysis of the evidence (see Bryant for good summaries and implications - pp 124-6, 151-4). All the different aspects of the data point to a location “far out in the eastern hinterlands... a locus in western Central Asia”, which, in her ‘further implications’ she specifies with precision: “the locus of the IE spread was ... somewhere

22 Elst suggested (1993), when D McAlpin’s Elamo-Dravidian affinities and Dravidian migrations were fashionable (see also Mallory 1989: 55-6 and Renfrew 1991: 13-4), that the Elamo-Dravidians moved along the Gujarat coast and down into south India without intruding into the ISC (127-9). Without new firm evidence all such proposals are conjectural and of little value.

in the vicinity of ancient Bactria-Sogdiana” (ib 137). Examining archaeological data too, B Sargent also finds that Bactria is the urheimat and outlines a movement from there to the Russian Steppes which generated the Kurgan culture (1997)²³. Sargent sends the Indo-Iranians also from there to their historical habitats. However, Nichols’s evidence shows “westward trajectories of language” (p135) but there is no mention of any south or southeastern trajectory to account for Indo-Aryan. Bactria is not far from Saptasindhu and could be a first concentration point for out-of-India travellers and subsequent dispersals.

4. Isoglosses and palatalization. I am not sure that I agree with everything that experts say about the isoglosses, and particularly what archaisms are and what innovations. I note that Hock in his 1999 paper mentions no obstacle or difficulty presented by the isoglosses with regard to the out-of-India scenario. Indeed, to the best of my knowledge no invasionist has used this as an argument against the OIT, except indirectly, with attempts to prove the Russian Steppe as the original homeland. There is moreover general agreement, whatever other disputes may exist, that according to the isoglosses the IE branches form two distinct groups: (A) Hittite, Tocharian, Italic, Celtic, Germanic, Baltic and Slavonic; (B) Indoaryan, Iranian, Thracian-Phrygian (Armenian) and Greek (or Hellenic). This group B has certain isoglosses in common: the augment in past tenses; the formation of the perfect; the prohibitive *mā* (I-Ir)/ *μη mē* (Gk)/ *mi* (Armenian); etc. No major isogloss cuts across the line between the two groups except palatalization which is now generally thought to be a “late phenomenon” occurring in “a post PIE era” when the original unity had broken down after “most of the dialect groups had dispersed” (Winn 1995:324). (Incidentally, the r-isogloss whereby the passive of verbs is formed and is shared by group A (Hittite etc) is sometimes ascribed to Phrygian, but this is rather speculative.)

S.M.M. Winn uses palatalization to establish the urheimat. It is a late phenomenon, as was said, so he writes: “Looking at the geographical distribution of this isogloss, we may note its absence from the peripheral languages: Germanic (at the northwest limit...); Celtic (western limit); Italic, Greek and Hittite (southern limit); and Tocharian (eastern limit). It is the languages at the center that have changed...[Palatalization] never reached far enough to have any effect on the outlying languages” (1995:326). Winn accepts the Pontic Steppe as the urheimat taking as central languages naturally Baltic, Slavonic, Iranian and Indoaryan. We observe also that Iranian and Indoaryan are not placed in their historical habitats, unlike all other branches including Greek.

Here we have two difficulties. One, Slavonic is a comparatively recent (c 450 CE) intruder into the area of the Pontic steppes (though this situation could have been true at a very early period, the Balto-Slavs moving northwest and the Slavs returning much later). Two, the IA branch is not put at its historical habitat as it would then have formed the southeast limit and this would have spoiled the balanced picture of the developing centre and the outlying periphery where palatalization could not reach (as with Greek in the South and Tocharian in the East): so IndoIranian is made to stay in the central region. But here is the second difficulty. Greek belongs to group B (with Phrygian, Iranian and Indoaryan). If it had left before palatalization, then it is difficult to envisage the situation whereby it developed the isoglosses it has in common with IndoIranian to the exclusion of BaltoSlavonic (which, despite palatalization, belong to group A). The whole concept is self-contradictory.

Let us now transpose all Winn’s conditions to Saptasindhu. Here again palatalization began after the various branches moved to their historical habitats (allow for some variation in the order): Hittite leaving first westward, then Tocharian to the east, then Germanic and Balto-Slavonic to the west and north-west, Celtic and Italic to the west and south-west, and finally Thracian-Phrygian and

23 “Archaeologists have not in fact succeeded in locating the Indo-Europeans... [but] are generally agreed that the so-called Kurgan peoples... spoke an Indo-European language... [and] some time around the middle of the fifth millennium... expanded from the steppe zone north of the Black sea... into the Balkans and adjacent areas.” (Watkins 2000: xxxiv)

Greek (with the distinctive isoglosses they shared with the Indo-Iranians). Here too we have the difficulty of the position of the Balto-Slavs, but, as was said, perhaps in the early period (in the 6th millennium?) they were close enough to the IndoIranians. Thus palatalization spread from the Indo-Iranian “core regions” to the adjacent Slavo-Balts but not to the extreme limits of the “periphery” (particularly as many non-IE cultures intervened between the Indo-Iranians and the Hittites, Phrygians and Greeks).²⁴ Bryant discusses other possible OIT scenarios (146-9).

This scenario with Saptasindhu as the urheimat seems much more reasonable. Or so it would be, if I accepted the theories about protolanguages and isoglosses. I don't - and this has little to do with C Melchert's arguing (1987) that Anatolian Luvian is neither *centum* nor *satem* (where palatals have become affricates *z* and sibilants *s*; see also Hock 1991: 13-5), or with the Himalayan proto-Bangani (perhaps) being *centum* (Elst 1999:122-3; Bryant,142). We know, or rather surmise, that there was a PIE language but *we don't know what it was*, nor when, where and how it changed: we merely conjecture and theorize about these phenomena - and writing pseudoscientific texts just perpetuates the illusory knowledge, which becomes deception. Thus, again, without documentation we would not know that English 'help-helped' or 'starve-starved' were strong verbs in OE 'helpan -healp - geholpen' and 'steorfan - stearf -gestorfen'. Nor would we know that in Celtic alone of all branches the original IE *p* was dropped but retained in altered forms (eg *-pt = -cht*) and that many words with *p* are loans from other languages (eg Welsh *pysq* 'fish' from L *piscis*). Nor, again, that S *g* would correspond in Gk sometimes to **b** (*gauḥ/bous* 'ox'), sometimes **g** (*gir/gērus* 'voice') and sometimes **k** (*grha/korthis* 'house/heap') and in Latin, similarly, sometimes **b** (*gauḥ/bos*) and sometimes **g** (*guru/gravis* 'heavy').

When Bryant states “ Archaeologists, whether western or Indian, cannot simply dismiss or ignore linguistic reality”, he is making a sweeping and confused statement. Even more than linguists' interpretations of facts, linguistic conjectures, which posit hypothetical, non-existent factors *x* and *y* and then take them as *facts*, **are not realities**. Consequently I have irremovable doubts about any linguistic phenomenon one step away from facts like correspondences that are generally accepted because of phonological and semantic correlations. Nobody should dismiss or ignore linguistic realities but everybody should avoid fantasies. For a comparative study of Sanskrit and PIE, see the paper 'Sanskrit and Proto-Indo-European'.

X) Conclusion

The Aryan Invasion Theory, despite its 150-year-long life, has no real support anywhere except continued prejudice. It has now been substituted, in a similar shameless frame of mind, by “migration” of an alleged complex and to the archaeologist or anthropologist incomprehensible nature; this is a deception, since the aryanisation of North India on so enormous a scale could not possibly have been effected without conquest and coercion – for which there is no testimony of any sort. Why this preposterous proposition should have acquired the status of historical fact among serious indologists is for me a mystery. There may have been racist prejudice as many writers aver (Shaffer 1984; Leach 1990; Frawley 1991, 1994; Feuerstein 1995; Trautmann 1997; Bryant chs1-2, 13; many Indian writers like Talageri 2000 and Indian-American Kak 2000); this was perpetuated by mechanical repetition rather than logical consideration. Renfrew too was right perhaps in seeing nothing in the *RV* demonstrating that the Indoaryans “were intrusive to the area: this comes rather from a historical assumption about the ‘coming’ of the Indoeuropeans” (1989: 182): this also is a kind of prejudice- illustrated by Hitze's “*Like nearly all the peoples of Indo-European origin, both the Iranians and Indians first came as immigrants into the lands which they later inhabited*”(1998: 139, my emphasis: and note that most IE peoples appeared not as “immigrants” but invaders).

In sharp contrast, all the primary materials of a historian agree in showing no evidence at all for

24 Here I accept for argument's sake that the group-B isoglosses are innovations. But in the IIO scenario these could just as easily be archaisms lost by group-A branches.

any entry. On the contrary, such testimony as has been preserved, early historical documentation and later traditions testify that Indoaryans are indigenous to Saptasindhu. These traditions (corroborated by foreign writers of the 4th cent BC) affirm that the IAs have been in Saptasindhu since at least the 4th millenium; this is now fully supported by Archaeoastronomy which places the great Bhārata war at 3067, a Brāhmaṇa text c 3000 - 2900 and the Vedāṅga *Jyotiṣa* c1800. Given that archaeologists, anthropologists et al, specialising in the prehistory of that area, affirm unequivocally since 1980 that the local culture has an uninterrupted continuity since c7000 (except for the break in the skeletal record c4500), we can say that the IAs have been in North India since that time. There is also the fact that the *RV* knows nothing of elements in the ISC whereas the later texts have these elements; moreover even in very late hymns the Sarasvatī is a large river supporting the Aryans on its banks: therefore the *RV* must belong to a period before 3000.

On the reasoning so far, the PIE language or forms of it were spoken in that area and therefrom arose Vedic with its own dialects which later developed into Sanskrit. People may have emigrated in northwestern directions at different times early in the 4th millenium and before; or there was a common language and culture continuum from North India to the Caspian Sea which gradually lost its unity so that different cultures developed at different locations. Evidence from the *RV* suggests the former. Linguistic evidence suggests that there was no unitary PIE language, that we can reach. "In fact, detailed comparison makes it clear", states Burrow, "that the IndoEuropean that we can reach by this means [of comparison] was already deeply split into...varying dialects" (1973: 11, my brackets). Most probably there were also spoken non-IE languages in the region. Unless fresh evidence comes into the light of day, I doubt whether we can get any closer – remaining within the rational bounds of realism.

Quite rightly some scholars expect that one should demonstrate how various linguistic developments and archaeological data harmonize in the movement(s) away from the urheimat (Mallory 1997). With the available evidence this expectation will face grave difficulties. As was pointed out above, the PIE was as full of dialectical variants as Old English, Italic, Germanic or Greek: no protolanguage can be reconstructed in reality. In fact, it is just as likely that different dialects or branches of Greek (or Germanic or whatever) developed independently of one another from separate but close dialects or branches of PIE with the influence of substrata: we simply don't know.²⁵ Then again, with the available archaeological evidence the difficulties are probably insuperable since tools, weapons and other finds do not provide clear patterns of diffusion and certainly cannot furnish linguistic facts. Mallory is sufficient realist to acknowledge all these difficulties (1997: 117). On this, I Pejros too mentions various difficulties (1997: 156-7).

We are left with some clear realities and possibilities. All IE branches other than the Indoaryans have demonstrably moved to their historical habitats from some homeland: there is evidence of this of one kind or another. There is no evidence of any kind that the Indoaryans moved into their historical habitat at c 1500 as the AIT maintains. New evidence may emerge showing beyond doubt that the Indoaryans were intruders, but this seems at present extremely unlikely. The linguistic exodus from Saptasindhu is, to use Hock's words, "relatively easy to maintain". How exactly the various branches moved westward to the extremities of Europe, I do not know. I could offer several variations of elite dominance, trading and artisan movements or even cultural missions, but they

25 See V Pisani's view (1971: XXXII): le varie lingue ie. non sono la continuazione di un tipo linguistico completamente uguale per tutte: bensì esse rappresentano lo sviluppo di antichi dialetti o gruppi di dialetti i quali tutti erano tenuti assieme da un certo numero d' isoglosse: 'The various IE languages are not the continuation of one linguistic type, entirely uniform for all; but they represent the development of ancient dialects or groups of dialects which were all held together by a certain number of isoglosses' and so on. I consider this just as possible as any monogenetic evolution. We simply don't have all the facts.

would all be conjectural. Personally I prefer to leave it to others to create their own scenarios²⁶ while I remain with “not-knowing”; after all, there are so many other important things in life I don’t know – the beginning of language and of man, birth and death. It may happen that competent archaeologist/indologists will re-examine the accumulated evidence without the prejudices of the past and discover, perhaps, clear patterns of diffusion from Saptasindhu – as was very nearly done by B Sargent (above, IX, 3).

That most mainstream philologists will react unfavourably to this thesis I take for granted. I know well in myself the force of habit and of attachment to deep-rooted notions that reacts more through emotional outbursts than cool rationality. I repeat that the issue of origins, of *when* and *how*, is one not for philologists but for archaeologists and experts in related fields. We owe it, other than to the peoples of India who, I think, have long been wronged (by their own faults no less than foreign influences), to truth itself, which is the primary concern of all of us, to consider this thesis without prejudice.

26 Kak suggests several modes of dispersal (1996). So does Talageri who, further, identifies some peoples in the West with Vedic tribes and clans: Phrygians/Bhrgus, Hellenes/Alinas and so on (2000: 204-5; 258-66); but some of the cognations here are implausible.