

Nine: **Indo-European Linguistics and Indo-Aryan Indigenism.**

**0. Abstract.** This paper examines the general IE<sup>1</sup> issue and argues in favour of Indoaryan indigenism against the AIT (Aryan Invasion/Immigration Theory) which has been mainstream doctrine for more than a century. The extreme positions that there was no PIE<sup>1</sup> language or that this language is as currently reconstructed are refuted: the evidence suggests there was a PIE language but this cannot be reconstructed and all efforts in this reconstruction are misplaced. Since they are in no way verifiable, they should not be used as evidence for historical events. It is admitted even by rabid Indian nationalists that humans came to India from Africa sometime in the Pleistocene, and now there is evidence of change in the skeletal record of the region indicating that a new people may have entered c 6000-4500<sup>2</sup>; even so, if these people were the IAs, they must, surely, be regarded as indigenous by 1700. Recent genetic studies do not suggest any entry of IAs within the last 10 000 years but state that the European peoples came out of South Asia after 50 000 B(efore)P(resent). Apart from such studies, other kinds of evidence and arguments will be used in full to demonstrate indigenism.

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<sup>1</sup> IE = IndoEuropean; PIE = ProtoIndoEuropean; IA = Indoaryan(s). A large section of this paper is a revised version of a talk given at a Conference at the Center of Indic Studies, University of Massachusetts, in June 2006.

<sup>2</sup> All dates are BCE except where stated otherwise. Dates in brackets, e.g. (2001) denote a publication by a modern author which will be found in the Bibliography.

I use the name Saptasindhu simply as a *bahuvrīhi* adjective. The 7 rivers are frequently mentioned in the RV – 1.32.12; 34.8; 35.8; etc, etc. In the *Avesta* it is said that the Old Iranians had, before settling in Iran, been to various places and one of them was *Haptaḥəndu*. I take it that *Haptaḥəndu* indicates Saptasindhu, a name which has been frequently used by others before me.

## Indoeuropean and ProtoIndoEuropean



Map of IE languages

The map shows the location of languages broadly, not peoples or races; it is a modification of the map used by J. V. Day, 2001, p xiii.

### 1. Let us first see the ensuing discussion in its natural historical context.

American archaeologist J. Shaffer had the courage to call the AIT of India "a myth" (1984). The development of this "myth" which had obtained mainstream status in academia is well traced by E. Bryant (2001), J. Day (1994) and J. Mallory (1973). Here suffice it to say that having started as a linguistic theory, it soon acquired biological undertones involving more or less obvious ethnic/racial prejudices (Bryant 2001; Trautmann 1997; Shaffer 1984). Before the Nazi "aryanism" of the 1930's, the AIT was used by colonial politics as is obvious in British Prime Minister Baldwin's speech in Parliament in 1929: " Now after ages ... the two branches of the Aryan ancestry have again been brought together by Providence ... By establishing British rule in India, God said to the British, 'I have brought you and the Indians together after a long separation ... it is your duty to raise them to your level as quickly as possible ... brothers that you are'"! God's ways were no longer so mysterious.

The linguistic theory which c 1800 CE sought to account for the similarities between Sanskrit, Greek, Latin, Germanic etc, contained the notion of an IE family of languages and a PIE mother-tongue. The literature on the subject is increasing enormously year by year. Hereafter I shall refute first the rejection by some scholars of a PIE tongue and even a family of IE languages and then the claims of others about the nature of PIE and its alleged "reconstruction". There is sufficient evidence to show that there had been a unitary PIE language and civilization but not enough to enable us to go very far with reconstructions. In any case, in the absence of people speaking PIE and of recognizably original texts, the reconstruction of the language – even if it were possible, which it is not – would be pointless and wasteful of other people's money. E. Leach wrote that after the discovery of the ISC (=Indus-Sarasvati Civilization) "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" (1990).

'Vested' interests and academic posts are still involved and will continue to be involved because the human ego is not educated to let go of claims that are shown to be untrue, despite much trumpeting to the contrary. At the same time, although this phenomenon may cause and has caused considerable delay and darkness on the path of progress, it cannot ultimately

prevent the establishment of true knowledge. A good case in point is the so-called “Copernican revolution” which, in general terms, ousted the Ptolemaic geocentric system of the heavens that had beclouded knowledge for some 18 centuries since Aristotle, and brought the heliocentric system which had been known by Greek naturalists (especially Aristarchos of Samos) even before Aristotle (Kuhn 1970, Koestler 1954). For many decades after the publication of Copernicus’s work *De Revolutionibus ...* in 1543 the mainstream savants of the day concocted more and more epicycles of the planets to account for the mathematics involved and the new observations. Eventually, after about a century the old model collapsed: more and more naturalists abandoned it in favour of the new heliocentric system. Similar concoctions, like the astronomical epicycles, are being produced in the field of Indology but with the new genetic studies (§19) the issue of indigenism can no longer be easily refuted.

**2. The IndoEuropean family.** That the IE family of languages is not a “family” is by no means a recent view, though it is not as old as the initial hypothesis that there is a “family” and a unitary PIE civilization and language. We love to speculate and set up theories and models – the more elegant the better. The human mind does not ordinarily seem to like gaps and emptiness. So it hastens to fill a gap in knowledge with some hypothesis. Then others raise objections and set up a different or even an opposite theory and so on. Thus, we speculate about everything raising one theory after another to keep mind and pen occupied. Here is an example out of myriads. A. Speirs writes (1978:26) “The principle of economy of hypothesis requires us to assume or at least to test the proposition that the labio-velars were PIE phonemes which in the PIE period shifted to labials and dentals in some circumstances, lost the labial element in others...” etc. We have the theory of a PIE, then the theory of the existence of labio-velar phonemes (§6, end), then speculation about what happened to these imaginary entities. Of course theories are useful because they become a tool for further research so long as one does not cling to the theory and argue that this is the one and only truth. Thus from the first half of the 20th c. several scholars began to doubt the existence of PIE. I give a selection of them: Bonfante 1931; Walder 1936; Trubezkoy 1939; Pisani 1949/71; Morgan Keeley 1992; Marcantonio 2002/05. There are many more, all doubting that what are described as IE branches (=Sanskrit, Greek, Germanic, etc) are offshoots of a unitary PIE. One of the explanations is that the similarities in these so-called IE branches are due to chance and have no further significance. Now, some fortuitous similarities in lexical items are not merely possible but quite probable – and this could apply to languages outside the IE family. But we are hardly justified in evoking “chance” or “accident” when we meet 25 undoubtedly cognate stems present in three or more IE branches accross Eurasia and designating the very same parts of the human body: e.g. eye, nose, tooth, jaw, breast, knee, foot, flesh, heart, blood etc. We find also at least 15 stems of equally certain cognation in three or more branches denoting relations and functions like brother, chief, carpenter/fashioner, daughter, father, husband/master, man, mother etc (see §3, b). Then could it possibly be accidental similarity that we find accross Eurasia but not in the N-East S *lobha* ‘longing’, L *lu/li-bido* ‘desire’, Gmc *lufu/liubi* and Sl *ljubi* ‘love’ or S *mās Av māh-*, Toch A *mañ*, Gmc *mona*, B *menuo* Sl *meis-ic* all ‘moon’ and Gk *meis/mēn*, L *mens-is* and C *mī* ‘month’?

Perhaps, yes, it may be, as others have suggested, that borrowing or waves of diffusion, sporadic or whatever, or convergence through long/repeated contact, are responsible for the similarities and cognations (Trubezkoy 1939). But how probable is this? ... It may be probable that 10, 20, 50 perhaps lexical items of everyday common use travelled accross Eurasia at different times and in different directions. But this involves grave difficulties since the similarities and cognations in three or more IE branches run into many hundreds. Beyond this, there are close similarities in inflexion and conjugation and also in many social and cultural aspects (always exclusive of other non-IE peoples). Moreover, these similarities are not evident at all in Near Eastern languages.

Another objection raised is that common grammatical features appear only in S, Av, Ht, Gk and Italic. These branches, it is said, formed a closely related group that developed these common features. This is fallacious because, to take just one isogloss, while S and Av are *sātem*, Ht, Gk and Italic are *kentum*. Moreover, as we shall see in §3 both lexical items and grammatical features (nominal and verbal endings, reduplication, etc) are spread right accross the IE spectrum and are not confined to the group of S, Ht, Gk and Italic.

3. Let us look in detail at some of the difficulties involved in diffusion-waves and the close group.

(a) All Near-Eastern languages are – but for negligible exceptions which are easily accountable as borrowings – unaffected by these hypothetical waves or long/repeated contacts. Yet, roughly speaking, they are located in between Tocharian, Sanskrit and Iranian in the east and Greek, Italic, Germanic, Baltic and Celtic in the west. Some studies that claim elements “shared by Indo-European and Semitic” (eg Levin 1991) and Greek and Semitic (Burkert 1992) show very clearly that the “shared” material is utterly insignificant in comparison with the correspondences in IE branches.

(b) The obviously cognate words (=similar in sound and sense) in three or more IE branches are far too many for random or deliberate borrowing. Taking these very criteria, ie presence in three branches and designation of more or less the same (relatively) invariable entity, quality or activity, I easily collected about 500 stems (Kazanas 2007). Some are common to all or most branches and many to more than three. E.g.:-

1. arm: S *bāhu*; Av *bāzu*; Gk *pēchus*; Gmc *buog*; Toch A *poke*.
2. belly: S *udara*-; Av *udara*-; Gk *hoderos* (=gaster); L *uterus*' (venter?); B *vēderas*.
3. brother: S *bhrātṛ*, Av *brātār*-; Gk *phratēr* (= member of brotherhood); L *frāter*; C *brathir*; Gmc *bruodar*; B *broter*-; Sl *bratrŭ*; Toch A *pracar* .
4. clan, tribe: S *jana/jāti*; Av *-zana/-zantiš* ; Gk *genos* (*phulē*); L *gens* (*tribus*); Gmc *cynn/kyn*.
5. to desire/love: S *lubhyati*; L *lub-/lib-et*; Gmc *liufs/lufap*; Sl *ljubiti* : 'loves'..
6. eyebrow : S *bhrū* ; Av *brvat*-; Gk *o-phrus* ; C *brūad*; Gmc *brūn* ; B *bruvis* ; Sl *brŭvŭ* ; Toch A/B *pārwan(e)* .
7. flesh: S *māṃsa*; Gmc *mimz*; B *meisa*; O Prus *mensā*; Sl *meso*; Alb *mish*; Arm *mis*; Toch B *misa*.
8. heel : S *parṣṇi*; Av *pāsna*-; Ht *parsna*-; Gk *pternē*; L *perna*; Gmc *fiersn*.
9. jaw : S *hanu*; Av *zānu*-; Gk *.genus*; L *genu-* (back-tooth); C *gen*; Gmc *cin/kin*; B *žan-das(?)*; Toch A *śanw-em* (fem dual).
10. knee : S *jānu*; Av *žnu*-; Ht *genu*; Gk *gonu*; L *genu*; Gmc *kniu*; Arm *cun-r*; Toch B *keni*-.
11. nose : S *nas*-; Av *nāh*-; L *nārīs*; Gmc *nasa*; B *nosis*; Sl *nošŭ*.
12. palm (of hand) : S *pṛtha*; Ht *paltana*; Gk *palamē*; L *palma*.
13. reward : S *mīḍha* ; Av *mīḍa*; Gk *misthos*; Gmc *mizdō*; Sl *mīḍa*.
14. shoulder : S *aṃsa*; Gk *ōmōs*; L *ume-rus*; Gmc *ams*; Arm *us*.
15. tooth, molar : S *jambha* ; Gk *gomphos* ; (B<sup>~</sup>*žam̃bas* 'sharp edge'); Sl *zōbŭ* ; Toch A/B *kam/keme* .
16. woman : S *gnā/jānī* ; Av *jani*-; Gk *gunē* ; Gmc *cwene/qino* ; Sl *žena* ; Arm *kin* .

Note that there are some 40 stems designating parts of the human body which are the most invariable of all words since people have their bodies in all places and conditions and can hardly confuse eyes with ears or breasts with cheeks. One might dispute 10 or 15 of them but 25 at least are certain cognations.

The question now arises: Why should, say, the Baltic-speakers give up their native non-IE word for 'belly' (assuming it was different) and adopt *vēderas*, changed from the IE stem used by the Greeks (*hoderos*) or the Romans (*uterus*)? Or, why would the Germanic-speakers give up their own non-IE word for 'jaw' and adopt *cin/kin*, changed from the IE stem used by, say, the Celts (*gen*) or the Romans (*genū*)? ... Well, yes we can hypothesize successively at question mark after question mark, but at some point we must stop this easy way of escaping and get down to facts and sound reasoning. Yes, indeed, there may be some few freaky examples of inexplicable borrowing. But in the end the answer is– there is no ostensible reason whatever. Such changes could come about on a large scale only through contact with a superior (and literate) culture or through conquest and coercion. Otherwise, there must be a genetic relation.

(c) The similarities extend to genders, verbs and terminations. I take some random examples. Consider:

i) The forms and endings of the 1st and/or 3rd sing of the non-reduplicating Perfect of the verb 'to know':

S	AV	Ht	Gk	L	Gth	B(OPr)	Sl
<i>veda</i>	<i>vaēḍa</i>	-	<i>Foida/e</i>	<i>vīdi(t)</i>	<i>wait</i>	<i>vaīdi-</i>	<i>vēdū</i>

Ht has only a Periphrastic Perfect which is formed with the nom/acc of the neuter participle plus the auxiliary *har-ak-* 'to have' as in *markan har-ak-* 'to have cut'. Cf similar periphrasis in S *vidām as-/kṛ-/bhū-* 'to have known'. This example of Perfect as given in the forms above implies a whole conjugation in existence in all these branches and this precludes borrowing or a limited group (no Ht here!).

ii) The same should be said for many more like the following verb 'one is' (3rd pers sing):

S	Av	Ht	Gk	L	Gth	B(Lith)	Sl
<i>asti</i>	-	<i>ēšzi</i>	<i>esti</i>	<i>est</i>	<i>ist</i>	<i>ēsti</i>	<i>jestū</i>

iii) Consider the endings for, say, 1st and 2nd plural Pres Active of 'carry' (no Ht again).

S	Gk	L	Gth	Sl
<i>bharāmas</i>	<i>pheromen</i>	<i>ferimus</i>	<i>bairam</i>	<i>beremū</i>
<i>bharata</i>	<i>pherete</i>	<i>fertis</i>	<i>bairiḥ</i>	<i>berete</i>

iv) The present participle active of the verb 'to eat'

S	Ht	Gk	L	OE
<i>ad-ant-</i>	<i>ada-anz-a</i>	<i>ed-ont-</i>	<i>ed-ent-</i>	<i>et-end-e</i>

This too precludes borrowing or the limited group.

Here all the examples above show a wider spread including Gmc, B and Sl. Thus the notions of borrowing and of the limited group are nullified. The same is demonstrated in the noun example that follow. More on this at the end of this section in paragraph (d).

v) Take the sing Gen for 'father' (using 'mother' for Sl); note that in some branches like S this is also Abl.

S	Gk	L	Gth	Sl
<i>pitus/r/h</i>	<i>patros</i>	<i>patris</i>	<i>fadars</i>	<i>(matere)</i>

vi) And the sing Gen for 'dog'

S	Ht	Gk	Lth	OIr
<i>śunas</i>	<i>kūnar</i>	<i>kunos</i>	<i>śums</i>	<i>coin</i>

vii) Also sing Gen of 'cloud'

S	Av	Ht	Gk	Sl
<i>nabhasas</i>	<i>nabaḥhas</i>	<i>nepisas</i>	<i>nepheos</i> <i>(nephous)</i>	<i>nebhesa</i>

Note that these examples also exclude chance similarity and the narrow group. Now let us examine oblique cases other than the Gen.

viii) Consider the word for ‘name’ in sing NVA and Loc and Pl NVA.

	S	L	Gth	Sl
Sing NVA	<i>nāma</i>	<i>nōmen</i>	<i>namō</i>	<i>imę</i>
Loc	<i>nām(a)ni</i>	<i>nōmine</i>	<i>namin</i>	<i>imene</i>
Pl NVA	<i>nāmā(ni)</i>	<i>nōmina</i>	<i>namna</i>	<i>imena</i>

Note that Gk has *onoma* for NVA, *onoma-t-i* for Dative (=Loc) and Pl NVA *onoma-t-a*. So here too, no narrow group!

Some of the above examples are taken from Misra (1968), Held (1997), Szemerényi (1996) and Clackson (2007). Here, all these similarities can be due neither to accident nor contact and diffusion.

It may be argued that other languages (usually reconstructed Proto-this and Proto-that) show certain affinities (nominal or verbal terminations) but these are very few and very tenuous and do not prove borrowing or convergence due to long/repeated contact. They may be due to accident that is independent native change, or to inherited memory from a distant common past. The similarities in the IE branches have sufficient divergence to show that they are not direct borrowings and sufficient similarity to show that they are devolutions from common original forms. Moreover, intermediate Near Eastern (non-IE) languages do not have these elements.

(d) Some of the features in (b) and (c) may not occur in statistically significant numbers, as Angela Marcantonio has argued (2002, 2005), but this is not a significant objection. It is generally assumed that statistics is a vital constituent of the “scientific” method. It is not realized that the scientific method is not applicable to many areas of human life and that, in any event, it is as much dependent on inspiration and luck on the one hand, and as much liable to blunder on the other, as any other “method” (Pert 1997; Cohen 1985; Beveridge 1968). In any event, in the area under discussion, the proper “scientific” approach would be to ignore rigidity, regularity and uniformity since the linguistic and cultural changes occurred in diverse ways and certainly under no observable law as we shall see in §§6-7.

Then, statistics is a purely quantitative factor that has no bearing on quality or significance – matters which require different criteria<sup>3</sup>. Statistics and numbers here must give way to more important considerations like the improbability of so many similarities resulting from sheer accident or from various diffusion waves. Apart from subjection or migration and the like, it is most improbable that, in normal circumstances, any ancient people would adopt the foreign linguistic features examined in (b) and (c), which in some cases would make speech extremely difficult. Consider the complexities alone in the conjugation of the reduplicating verbs in Greek (in *-mi*) and in Vedic. In historical times the tendency for languages is to become simpler, more streamlined by analogy and more synthetic with prepositions and auxiliary verbs. Consider also the difficulty mentioned in (a) that all such similarities are absent in Near-Eastern languages.

It may be and has been claimed that some languages or groups of them have innovated: e.g. Sanskrit, Greek and Latin (and less Hittite) spent time together and thus developed their highly complex verbal system to the exclusion of Germanic, Baltic etc. Such generalizations are entirely conjectural, unproductive and, since they don’t embrace all relevant facts, totally unconvincing. First, no IE language in the historical period is known to develop from a simple to a complex inflective state, as has just been said; the contrary is observed in the cases of (various Afro-Asian languages, too) Vedic, Greek, Latin and, even, Old English which devolved into a simpler non-inflected language increasingly using prepositions and auxiliary verbs. Second, there are lexical items and isoglosses that are shared by groups other than Sanskrit, Greek and Latin; thus palatalization separates Sanskrit from Hittite, Greek and Latin and joins it to Slavic

<sup>3</sup> Take as example the hospital. Here the patients are far more numerous than the doctors. Numerically they are more significant but it is the doctors who do the truly significant work, restoring health.

and Baltic. Third, certain grammatical features join together Sanskrit Greek and Baltic – like the sigmatic Future; moreover, the reduplicated Perfect is found in Germanic as well as Sanskrit, Greek and Latin but not Baltic. To these should be added nominal inflexional features like neuters of the *nāma/nomen* type (in §4, c, viii) which are shared by Sanskrit, Latin, Gothic and Slavic (but Gk has stem *onoma-t-*). Thus the notion of a group consisting of Sanskrit, Greek and Latin (and Hittite) must be abandoned. Otherwise, we must also wonder whether Slavic, Baltic and Germanic also started innovating just like Sanskrit in the same areas (by sheer coincidence!) and then suddenly stopped. Such claims, therefore, are valueless.

4. Another important type of evidence for a unitary PIE civilization is the presence of identical or very similar **cultural features** in the different branches.

(a) The spread of theonyms is our first consideration. We find a Firegod with the same or cognate name in three branches and nowhere in non-IE languages: thus V *Agni*, Ht *Agnis*, Sl *Ogon* (and variants) – while the stem for ‘fire’ (but not the theonym) appears as L *ignis*, Lth *ugnis* and Iranian *dašt-ayni-*. The name of another god appears as V *Aryaman*, Mycenaean *Areimene* (and probably later Gk *Arēs*), C *Ariomanus* (in Gaul)/ *Eremon* (in Ireland) and Gmc *Irmin*. The Skygod is V *Dyaus*, Ht <sup>o</sup>*Sius*, Gk *Zeus/Dia-*, L *Ju[s]-piter*, Gmc *Tīwaz* and South Russian *Divū*. One Rain- or Thunder-god is V *Parjanya*, Sl *Perenŭ*, B *Perkunas* and Gmc *Fjorgyn*. Then the Sungod is V *Sūrya*, Gk *Helios*, L *Sol* and B *Saule* – while the stem for ‘sun’ appears as Gmc *savil/sol*, Welsh *haul* and Sl *solnce* (and variants). The Dawngoddess is V *Uṣas*, Gk *Ēōs*, L *Au[s]rora*, Gmc *Eos-tre* (=Spring) – while the stem for ‘dawn’ appears as B *aušra* and probably C *gwawz* (paper Three). Surely all these agreements are not accidental.

(b) Many religious, ritualistic practices are shared by several IE branches. A well-known one is the horse-sacrifice, found in one form or another in India, Greece, Rome, Germania and Ireland (for details see Andersen 1999). But they share also several customs and legal practices (eg prohibition of incest, division into five tribes or demes/regions, etc), apotropaic and purificatory rituals and, of course, divination (Dumézil 1952, 1954; Puhvel 1970; Kazanas 2001 for Greek and Vedic).

(c) Poetics too is a feature common in IE branches. Here we must exclude Hittite poetry which falls wholly within Near-Eastern traditions and has very little or no relation to IE forms and themes. Moreover, since Latin poetry imitates largely Greek prototypes (epic, lyric, drama) and Celtic, Germanic etc are again largely imitative of Latin and Greek forms (Watkins 2001), comparisons here must be made between Greek and Indic materials. In Vedic and Greek poetry we find three types of stock epithet: the Greek ones are all culled from *Iliad* 1 and the Vedic ones from the *Ṛgveda*. (Much of this comes from Kazanas 2001a.)

(i) Vague adjectives like Gk *dios* ‘bright, divine’, *diogenēs* ‘nobly born’, *megathumos* ‘big-hearted’; etc. Corresponding Vedic ones are *daivya* ‘bright, divine’ (*RV* 1.35.5; 2.33.7), *ugra* ‘fierce, mighty’ (2.33.9; 10.34.8), *ṛtavan-* ‘holy, observing order’ (2.35.6; 7.61.2); etc.

(ii) Epithets denoting a specific feature that could be used of anyone but are applied only to a hero or divinity: e.g. Gk *Hera* ‘of white hands’ *leukolenos*, *Athena* ‘of blue/grey eyes’ *glaukopis*, Achaeans ‘of fine greaves’ *euknēmīs*; V *somapā* ‘somadrinker’ could be used of any god but is applied only to *Indra* (2.12.13; etc), *jalāśa* ‘cooling’ used of *Rudra* (2.33.7, 7.35.6), *jātavedas* ‘all-knowing’ of *Agni* (1.44.1; 4.3.8), etc.

(iii) Epithets used of one figure (hero or deity) and denoting a specific feature thereof: Gk *asteropētēs* ‘who throws the bolt’ for *Zeus*, *hekēbolos* ‘aim-attainer, farshooting’ for *Apollo*, *polumēchanos* ‘of many devices’ for *Odusseus*; V *gr̥hapati* ‘lord of house’ for *Agni* (1.45.1), *vajrin* ‘he of the bolt’ for *Indra* (7.49.1), *uruḡāya* ‘far-going’ for *Viṣṇu* (1.154.1) etc.

(iv) Then there are items of a common lexical stock (again in the *Iliad* and the *RV*): V *śravas/śruta* = Gk *kleos/kluto* ‘fame(d)”; V *uru* = Gk *euru* ‘wide’; V *āśu* = Gk *ōku* ‘swift’; V *patnī* = Gk *potnia* ‘reverend lady’; etc, etc.

(v) In Vedic poetry we meet strict metre like *Anuṣṭubh* (4 lines of 8 syllables) with an iambic cadence in the second and fourth unit, or *Gāyatrī* (3 lines of 8 syllables) but also within these strict metrical lines rich alliteration. In Greek and Latin poetry we find different metrical units (iambus, anapaest, hexameter etc) while in Germanic we find alliterative lines with loose metre.

All these features, except for rare recurring epithets, are not found in ancient Near-Eastern

literature.

**d)** Common incidents, motifs, themes in religion (=mythology). A most interesting motif is that of **the goddess who becomes a mare** and a pursuing god turns into a stallion and mounts her: from this union comes a twin-pair or some other marvellous creature. In the Vedic tradition we have Sungod Vivasvat who becomes a stallion to mount his run-away bride Saranyū who had turned into a mare and later gave birth to the twin Horsegods Aśvins (*RV* 10.17.1-2; *Bṛhaddevatā* 6.162ff). In Greece (Pausanias VIII.25.5), Poseidon chases Dēmētra Erinus (= V Saranyū?) then both take on equine form and later Dēmētra gives birth to the beautiful horse Areion and a daughter Despoina. Finally in Norse legends (*Edda*, 35-6), Loki, the god of tricks and transformations, becomes a mare to attract from work the giant-mason's stallion, Svadilfari, then gives birth to wondrous Sleipnir, the eightlegged swiftest racer in the world, given as gift to Odin.

Another motif is that of the heroes or **divine youths who rescue** the Sunmaiden from a dangerous situation or become her companions. In India it is the Aśvins (saviours of men from tempests and other calamities) who accompany Sūryā the Sun-maiden, even to her wedding. In Greece it is the *Dioskouroi* 'Zeus's lads', Castor and Pollux (also saviours and expert horsemen) who rescue their abducted sister Helenē. Among the Lithuanians it is the *Dievo Sūnelai* 'the [sky-] god's lads' who rescue and escort the Sunmaiden *saules dukterys*.

A third example is the unusual motif of **the thigh-born child**. In Greece we meet the mythologem of Dionusos being born from the thigh of Zeus. In one version Semelē asked Zeus to appear in all his brilliant glory while she was six-months pregnant with his child; he did so, blazing away with his lightning and Semelē was burnt up. But then Hermes rushed and saved the embryo and stitched it within Zeus's thigh. The earliest attestation is in Euripides's *Bachai* 88-100 (*kata mērōi de kalupsas* 'having covered [the embryo] within the thigh' 97) which means c420. In the Indian epic *Mahābhārata* Bk I, 169-71 there is the story of a radiant brahmin lady who fled from some cruel warriors carrying her child in her thigh; they found her but then the child issued out of the thigh blazing like the sun and blinded them. Here it may be claimed that the Indian tale is late and perhaps borrowed from Greece. But in the much earlier *Jaiminīya Brāhmaṇa* (III, 199), which would be 5th c. at the latest, the child Kutsa is born from Indra's thigh: no details are given here, but Indra was, like Zeus, the storm-and-lightning god (Kazanas 2004c: 46-49).

Let us take a fourth and final motif – **the severed head**. In Norse legendry, Odin preserves wise god Mimir's head (cut off by the Vanir) to consult it in times of danger and doubt (Davidson 1981:146). In the Welsh tradition the family of Lyr preserve Bendigeidfran's head (MacCana 1983: 78). In Greek myths Bellerophon holds Medusa's head which still has the power to turn the onlooker into stone; then, Orpheus's head, after the Maenads tore him to pieces, floated down the river Hebros still lamenting and at Lesbos was installed as a shrine of prophecy. In India, in the *RV*, the Aśvins substituted sage Dadhyañc's head with that of a horse to obtain secret knowledge and when Indra cut off the horse-head they reinstated the original. (See Macdonell 1898:141-2.)

Many more such motifs and parallels will be found in Kazanas 2001a, 2004c, all of which appear, as expected, with divergences in the east (=India) and in the west (Europe), but do not appear at all in intermediate Near East; these also suggest a common origin.

**5.** The aspects examined in §§ 3-4 indicate clearly that there is sufficient evidence to justify the claim for a unified PIE civilization which had a definite location somewhere in Eurasia before 6000 BC. It is generally assumed that 'civilization' means tools, large buildings, statues and paintings, ornaments, vehicles, weapons of war and other material artefacts. But civilization can flourish without advanced technology and artefacts, such as we find in Ancient Mesopotamia and Egypt: it can develop and thrive on elementary technology and very simple agriculture for a very long period of centuries and millennia. The difficulty will be that without the material evidence we don't know that there had been an advanced civilization. R. Rudgley cites a modern scholar, Prof Yoshinory Yasuda who found "a marvellous principle" for civilization which is "respect for and co-existence with nature" and added: "Civilization begins to appear when a workable system for living, that is a proper relationship between men and

nature, is established in accord with the features of a given region". Yasuda wrote this in regard to the Jōmon culture in Japan beginning c10 000 BC (Rudgley 1998: 31-33). No doubt there are many more definitions. One definition that is interesting because very unusual in directing our attention inwards is by A. West who wrote about Ancient Egypt: "In a civilization, men are concerned with the quality of the *inner* life rather than with the *conditions* of day to day existence... a concern... to master greed, ambition, envy" (1993: 6-7). Then, Plato's first ideal society is a community with simple agriculture, animal husbandry, essential crafts and trade (exchange), feeding on barley-bread and bulbs, drinking wine in moderation and singing hymns to the gods (*Republic* 370 C ff). The PIE civilization may have been of this nature with a "proper relationship between men and nature" and perhaps "a concern to master greed, ambition, envy" as their highest aspiration.

The evidence we have examined indicates clearly a unitary PIE civilization. The same evidence has sufficient diversity to show just as clearly that this civilization, wherever it was located (and this could have been an extensive area), lost its unity as, at a first stage perhaps, some sections of its population in different regions began to lose their pristine character deviating from the original norm and developing distinct linguistic and cultural peculiarities. At a second stage, again, different sections, large or small, successively moved away to new areas and eventually, settled to their respective historical habitats – Celts, Germans, Greeks etc. In this they influenced and were influenced by other cultural groups they encountered on the way or at the locus of settlement. Some lost many aspects of their original PIE culture, others lost most (at different indeterminate periods) and still others retained only meagre linguistic elements. Hence the considerable diverse cultural groups which, mainly through linguistic studies, have come to be known as the IE branches.

**6a. Proto-Indoeuropean ?** From the linguistic fossils in the IE branches modern scholars have reconstructed what they claim to be the forms of words in the PIE language. They even call it a "science"; but it all seems wishful thinking. I have argued elsewhere (especially paper One) that reconstructions of Protolanguages are unreliable because they are conjectural and unverifiable: even if they happen to be right, we shall never be certain of this. O. Szemerényi admits that reconstructions are used to facilitate comparisons, using one word instead of many IE variants, and cites Hermann's statement that "complete forms (e.g. \*deiwos [=S *deva*-s]) cannot be reconstructed at all, only single sounds, and even these are meant as approximation only" (Szemerényi 1996: 33; my square brackets). Twenty years earlier Burrow had said much the same: "in the case of Indo-European it is certain that there was no such unitary language which can be reached by means of comparison... the Indo-European that we can reach by this means was already deeply split up into a series of varying dialects" (1973:11). More recently, exhibiting scepticism like mine, X. Tremblay writes (of various IE branches but mainly Iranian): "la grammaire comparée est en réalité *radicalement* incapable de discriminer entre parenté divergente ... et parenté convergente" (2005: 63). This does not stop comparativists from reconstructing PIE forms and evince considerable faith in their 'scientific' reconstructions. And here we meet a serious instance of a conflict/divergence between thinking and acting that indicates great confusion.<sup>4</sup>

Now, let me take a simple example as starting point. Although I often use Rix's *Lexikon der indogermanische verben...* (e.g. §17, below), I cannot but agree with A. Marcantonio's critique of his methodology in taking often only 2 similar stems which, indeed, may be loans or may be devolutes of one stem in two closely related branches, e.g. Baltic and Slavic or Germanic. The examples I gave earlier (§3,b) all have three or more cognations and encompass eastern and western branches. So, to start with, many stems may not be Proto-Indo-European and all such reconstructions are obviously pointless and utterly misleading.

Linguistic changes (vocabulary, accidence, spelling etc) are not subject to universal laws. The way English has changed from 1100 CE is quite different from the way French or Greek changed, even though some aspects are general and common (loss of inflexion, increasing use of auxiliaries etc). Now, while certain general phenomena have some regularity and invariability and thus may be said to approximate the nature of "law" within documented and defined

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<sup>4</sup> A deep and wide inconsistency between thought and action must indicate something is amiss.

periods of changes in any one language or in relationships within a group of languages (like the IE), nonetheless all such phenomena have exceptions and, in any case, are specific to the particular period of the language or group of languages under examination. Marcantonio (2005) rightly criticizes many aspects of these so-called “laws”<sup>5</sup>. The changes in Vedic, Greek, Germanic and other languages differ enormously among themselves: eg the various forms of ‘be/become’ as in S  $\sqrt{bhū}$  (>*bhava-*), Gk *phuomai*, L *fui*, C *buith*, Gmc *be-* etc. Then again, Gk plosive *ph* became a fricative *f* as is the Italic *f*. How or why did the original initial consonant – whatever it was – change into these sounds? Linguists don’t know. One comes across various hypotheses but there is no sure knowledge – because there is no documentation. The changes occurred in particular, if not precisely determined places, periods and peoples and stopped there. If linguists don’t know how or why these changes occurred, then they most assuredly cannot know from existing material what the original forms were. It may be legitimate to make guesses and postulate certain forms (for the sake of convenience). But to proceed then to regard these entirely hypothetical forms as factual and use them in further comparisons with other hypothetical forms or build upon them structures and employ them as arguments in discussions about historical events seems to me wholly unacceptable.

**6b.** Consider a different example. In Greece, in the late 1970’s an Act of Parliament imposed reforms in the spelling, accent and accident of Modern Greek. Nobody could have predicted these changes 100 years earlier just as nobody could have predicted at c900 CE the changes in English that would come with the Norman Conquest or later, after Chaucer. All such changes have no regularity, constancy or universality and are of the nature of accident – not law – due to social factors. If we can have no reliable predictions about future developments we can have no reliable reconstructions of forms in past periods before documentation: so also most emphatically Hock 1991. Reconstructions are futile, wasteful exercises.

Another obvious inconsistency is the reconstruction of three dorsals which are thought by many to be unnecessary (Szemerényi 1996; Sihler 1995; Lehman 1993; Speirs 1978) and one of which, the labio-velar family, is unpronounceable! How on earth can anyone pronounce as one unitary sound  $k^w$  or  $g^wh$  which contain both a guttural/velar and a labial element? One simply can’t, however neat it looks on paper. To state that labio-velars exist in Latin (and other languages) is an assertion of no value. Certainly, we have L *qu-a-*(*lis/* etc) and in English *qua-*(*lity*) and Italian *quando* etc. But in every case we have three separate, successive sounds: velar *q* (pronounced **k**), a glide vowel **-u-** and a vowel; in no case do we have a single, unitary consonant. In fact, the phonemes **qu-** do not differ from L **cu-**(*lpo*). If it is claimed that L **qu** is pronounced **q-v-** then, again we have two distinct sounds – the velar **q/k** and the labial semivowel **-v-**. Sanskrit also has *kva*, *dva*, *śva* etc: these can be pronounced only as separate successive sounds not as a single one. Attempts at describing how these hypothetical phonemes are pronounced (e.g. Clackson 2007: 49-51) are theoretical and of no practical value. Please, try yourselves! Also to pronounce the monstrosity *\*dhg<sup>w</sup>hec* ‘to perish’! (For other unpronounceable examples see, for instance, Watkins 2000, 2001; Clackson 2007.)

<sup>5</sup> She points out that, e.g., there are 36 sound-rules governing consonantal differences between Latin and Germanic but only 34 attested parallels to support them. Marcantonio is, however, much too rigid in laying down her four criteria for scientifically determining genuine correspondences. Her 2nd criterion – that “a given sound in language A can correspond with only one sound in language B, or perhaps two (similar) alternatives in language B” contains an assumption which is untrue, the assumption that languages change uniformly. Changes in different languages are neither uniform nor parallel, as she herself points out (Marcantonio & Brady 2005), and therefore one can’t rely on such strict correspondences. Ultimately one must rely on “naked eye” as she puts it, or “impressionistic” perceptions. If some regular sound-correspondences can be established, all well and good; but they should not be taken as absolute and exclusive of other alternatives. Thus Gk *theos* ‘god’ could well = L *deus*, S *deva*, and S *dvār* ‘door’ could well = Gk *thura*, Gmc *dawr/tor* etc. Marcantonio cites also S *napāt* ‘offspring’, Gk *nepodes* ‘children, offspring’, L *nepōs* ‘nephew’ and Gmc *nefo* ‘nephew, relative’ and takes exception to the sound *d* in Gk *nepodes* (pl with unattested sing) observing that it should – according to the specific “law” – be **t**. Well, as I said, the changes were not uniform and one must rely on one’s discrimination, not “laws” nor statistics, though both are often useful.

A final point about laryngeals, which are wholly unattested except in Hittite (a language whose IE character is much decayed, being smothered by other neighbours rich in laryngeals). Let us consider the example of H<sub>2</sub> (or h<sub>2</sub> or ə<sub>2</sub>). This appears in PIE \**dhugH<sub>2</sub>ter* ‘daughter’ (Fortson 2004: 204): appears as *a* in Gk *thugatēr*, but as *i* in S *duhitār*. However, Av *duYdār* (Hale 2004: 748) or *duxtar* (Fortson, 204) has neither *a* nor *i*. So what was the form in Proto-Indo-Iranian?... Not known. Old Avestan has *pta* for ‘father’ but later *patar* and *pitar* (Mayerhofer KEWA, vol 2, 277); this is S *pitṛ*, Gk *patēr* and L *pater* (Fortson, 23, 276) all allegedly from PIE \**pH<sub>2</sub>ter*. But, again, what was the form in Proto-Indo-Iranian?...Unknown. First of all consider that unlike S (which has many cognates from √*duh*), Av *duYdār* and Gk *thugater* stand isolated without related stems in their languages. Then, as M. Hale observed, the *i* was not an invariable feature of Proto-Indo-Iranian (2004: 748). The cognates for ‘father’ expose yet another inconsistency. L has also *Ju[s]pitar* with *i* as well as *pater* in the selfsame phonetic environment and B. Fortson offers no explanation at all (2004: 23, 33, 253, 261; also *Marspitār* 276, 406). A further difficulty sprouts out from S *pitṛ*. We mentioned already that Av has *pta*, *pitar* and *patar*, despite the selfsame phonetic environment. But according to IEnists S should have \**phitṛ*! Because according to the IE “reconstruction”-system, the laryngeal H<sub>2</sub> becomes *i* in S but also aspirates the previous consonants. Thus PIE \**stH<sub>2</sub>to* > S *sthita* ‘one who has stood’ and PIE \**pletH<sub>2</sub>-* > S *prathiman* ‘width’. However, *pitṛ* has no aspirate *ph*! Those disparate phenomena show most flagrantly that these IE “reconstructions” and “phonetic laws” are anything but satisfactory.

**7a.** A most interesting aspect of Indoeuropean Linguistics is the root and the vowel gradation or *ablaut* system (or apophonie). Indoeuropeanists adopted an hypothetical five-grade *ablaut* from Greek. Now, the only language that has roots is Sanskrit. To begin with, the word ‘root’ does not strictly translate the S *dhātu* ‘layer, element, constituent, seed-form’; nor can a “root” exist as an independent and generative element of a plant as a seed can. But putting this aside, only Sanskrit has roots and a proper vowel gradation. **All other IE branches have stems, not roots as such.** Like every other modern IE branch English has no actual working concept of root. (Of course ‘act’ in English can function as a root and generate ‘act-ed, act-ing, act-ion, act-ivate, in-act-ively’ etc, but ‘act’ itself comes ultimately from Latin.) Greek had verbs, and scholars say that nouns derive from the verb-stem: e.g. *che-ō* > *che-u-ma* ‘a flow/stream’; *cho-ē* ‘pouring, libation’, *cho-a-nē* ‘melting pot’; *chu-ma* ‘the fluid’, *chu-s-is* ‘shedding’, *chu-tra* ‘earthen pot’; etc. Even if we took *che-* as the root, it is difficult to see how this develops into *cheu-*, *cho-* and then *chu-*! One realizes how inconsistent Greek is when one considers two similar verbs: *deō* ‘bind’ > *de-ma* ‘band, rope’, *de-s-is* ‘the binding together’, *de-s-mos* ‘bond’, (*dia-*)*dē-ma* ‘ribbon round hair’ – but no *deu-*, *do-* and *du-*; *pne-ō* ‘blow, breathe’ > *pne-u-ma* ‘blast of air’ (later ‘spirit’), *pne-u-s-/pno-ē/pnoi-a* ‘blast, breeze, breath’ – showing unexpected *pnoi-* but not *pnu-*! If one examined other similar verbs (*bdeō*, *zeō*, *keō*, *xeō*, *neō* etc) one would find even more bizarre changes in the stem. There is no regularity; moreover the vowels change from palatal *e* to labial *u/o* etc without rhyme or reason.

Sanskrit has three gradations in the development of the root-stem: e.g. √*cit* ‘being conscious of’ > *cet-as* ‘mind, intelligence’ or *cet-a-ti* ‘he/she realizes’, *a-cait* ‘realized’ (aor), *cait-anya* ‘consciousness’ etc. *ī* always changes to *e* and *ai*, never to *a* or *u/o*. Similarly radical *u* → *o* → *au* and *ṛ* → *ar* → *ār*. Now, *ṛ* sometimes will give *ra/ri/ru* but will never become *i/e* or *u/o*. Thus there is the basic grade of the simple vowels *a*, *i*, *u*, *ṛ*, *!* (though some roots have a ‘developed’ vowel), the strong (*guṇa*) grade *a* (same), *e*, *o*, *ar*, *al* and the fully developed one (*vṛddhi*) *ā*, *ai*, *au*, *ār*, *āl*. This triple gradation has its equivalent in Vedic cosmology and philosophy where we find three main world-levels – heaven, midair and earth (*svar*, *antarikṣa* and *pṛthivī*) – and causal or natural, subtle or mental and gross or material. As nouns and verbs are generated from the root, the radical vowel changes according to constant regulations (except, as was said, in the case of *ṛ* which is somewhat unstable). This process is absent from other IE branches. (And, as we see in Greek, it is utterly confused. Modern studies since, say J. Kurylowicz(1956), O. Szemerényi (1972) *et al*, rationalize but also tacitly acknowledge this fact.)

After Brugmann had turned against Sanskrit and favoured Greek (1912-16), comparativists adopted a five-grade *ablaut* from Greek *patēr* ‘father’ and *eupatōr* ‘good father’ in incredible disorder (Szemerényi, p84):

- i) *pa-ter-a* (acc sing) where *-ter-* shows **e** as the basic grade (to be distinguished from S *e* which is long and second grade).
- ii) *eu-pa-tor-a* (acc sing) where *-tor-* shows the **o**- grade (again to be distinguished from S *o* which is long). But note that, unlike the Sanskrit series, here we have a different noun declension and sound-family: *e* is palatal and *o* labial!
- iii) *pa-tr-os* (gen sing) where *-tr-* shows the zero or nil grade! But here we have in fact syncopation or loss of vowel (*lopa* in S).
- iv) *pa-tēr* (nom sing) where *-tēr* shows the long-vowel grade (**ē**).
- v) *eu-pa-tōr* (nom sing) where *-tōr* shows the long **ō** grade (**ō**-mega).

Obviously there is neither rhyme nor reason in all this. More importantly, the ablaut occurs in the affix *-t-r-* not the root stem. This series, if it can be considered such, might have some validity if it occurred in one noun or one verb and if it followed some principle(s) that governed the same changes in many other nouns or verbs. Szemerényi gives instead the following examples stating that “Very often only full grade [i.e. the vowel *e*], o-grade and zero grade are attested” (p84):

- a) *leip-ō* (pres) *le-loip-a* (perf), *e-lip-on* (aor) ‘leave’
- b) *derk-omai* (pres), *de-dork-a* (perf), *e-dr-akon* (aor) ‘perceive’.
- c) *penthos* (neut nom sing), *pe-ponth-a* (perf), *e-path-on* (aor) ‘grieve’.

Here one cannot but wonder at this astonishing presentation, which merely increases the disorder. Here we have more bizarre phenomena. First, the vowel-change occurs in the stem, not in affixes or terminations, as above. Then *leip-* and *derk-* are verbs but *penthos* in (c) is a noun! Then the zero grade is not syncopation or disappearance of the vowel as in (iii) above, but a different vowel **-i** in (a) and **a** in (c). Another difficulty is the diphthong *ei* in (a); for we find also *eu-pa-teir-a* (nom sing fem ‘she of a noble father’) and we now do not know whether this is full grade as with *leip-ō*, or a sixth grade according to the five-grade presentation above<sup>6</sup>.

**But what principles govern these so-called vowel-grades in Greek? In fact there are no principles such as we have in Sanskrit!** Clackson writes at length about the significance of the root but does not give a single example in PIE or any branch of a root generating primary and secondary derivatives as in Sanskrit (2007:65-9, 187-191). This issue and others like the obvious problems of reconstructed sounds, especially consonants, I have discussed at great length elsewhere (Kazanas 2004b) and we need not therefore spend more time with them. A quotation from Burrow should suffice here: “Chiefly owing to its antiquity the Sanskrit language is more readily analysable and its roots more easily separable from other accretionary elements than any other IE language (Burrow 1973: 123, 289, my emphasis).

**7b.** By way of conclusion, let me repeat that PIE cannot be reconstructed at all from the available data in the known IE branches. The comparative efforts fail as a science miserably, because they employ far too many “laws” which at the same time have far too many exceptions and discrepancies. One of the worst aspects is the reconstruction of words that, as we saw, look very nice on paper but cannot be pronounced – like *\*nek<sup>w</sup>ts* ‘night’ or *\*dhǵhmma* ‘on the ground’ etc. The positive sciences have as their final testing ground Maths and the reality of the material world. What corresponds to this in the linguistic/phonetic field is sound, *pronounceable sound!* If the lexemes we produce are unpronounceable, then our system is wrong.

A linguist not involved in IE comparativism, R. Nixon (Professor of Linguistics at Australian National University) points out sharply (1997: 46) that R. Hall reconstructed the Original for the Romance languages (Italian, French, Spanish etc) but this was only a partial approximation to Latin, though some elements were correct. F. Southworth made a similar study of the various Indoaryan languages and his result also was only a partial approximation to Sanskrit. Nixon points out that each scholar knew the respective original language. Nixon’s implication is that not much of the PIE reconstruction could be correct when the IEnists do not and cannot know the PIE since there are no documents in the PIE languages. Enough said on this subject.

<sup>6</sup> Here too we have examples showing apparently great confusion as noted in §7.

## Indo-Aryan

8. Having held and taught for more than 18 years, but without investigating, the received doctrine that the IE branches dispersed from the South Russian or Pontic Steppe (as per Mallory 1997, 1989; Gimbutas 1985, 1970; and others), and that the IAs had entered Saptasindhu c1500, I began to examine these mainstream notions thoroughly and c1997 abandoned them. I decided that no evidence of any kind supported them; on the contrary, the evidence showed that by 1500 the IAs were wholly indigenous and that the elusive IE homeland was very probably Saptasindhu and the adjacent area – the Land of Seven Rivers in what is today N-W India and Pakistan; this area could well have extended as far northwest as the Steppe.

Apart from the (recent) genetic studies, which at that time were not so well-known nor so secure (see §19, below) the decisive evidence for me now is the antiquity of Sanskrit indicated by its inner coherence and its preservation of apparently original PIE linguistic features (like the dhātu, five families of phonemes, etc) and cultural elements (e.g. §4). The Vedic language as seen in the RV alone, despite much obvious attrition and several innovations, has preserved many more features from the putative PIE language and wider culture. This was due to its well attested and incomparable system of oral tradition (papers One, Three and Kazanas 2004b) which preserved the ancient texts fairly intact (RV, AV, etc) and continued even into the 20th c. An oral tradition of this kind cannot be maintained by a people on the move for decades if not centuries over many thousands of miles, as the AIT proposes. Such a tradition could be preserved only by a sedentary people where the older generation would have the necessary leisure to pass the communal lore to the younger one<sup>7</sup>.

9. I was not of course the first to come up with such a view. On the strength of Sanskrit many European scholars in the early 19th c. thought India was the original homeland (Mallory 1973:26-9). Even after scholars rejected India in the later 19th c and began to adduce different urheimats from the Baltic to the Balkans rewriting Indian (proto-)history, there was a succession of Indian scholars, mainly, who maintained the indigenist view in one or other form: Rao 1880; Shri Aurobindo 1914; Dhar 1930; A. Das 1971 et al. Indeed, in recent years also many publications advocated indigenism: Sethna 1992; Elst 1993, 1999; Talageri 1993, 2000; Frawley 1994 and with Rajaram 1997; Feuerstein et al 1995; and others. In fact, more and more scholars in the West have re-examined the issue and rejected the mainstream view advocating instead a movement Out of India into Europe: Schildman 1994, 1998; Elst 1999; Friedrich 2003, 2004; Danino 2007. Here we must add the fact that since 1984 there is full consensus among archaeologist specialists in the area that the ISC has unbroken continuity (Lal & Gupta 1984; Allchin 1997; Kenoyer 1999; Shaffer & Lichtenstein 1999; McIntosh 2002; Possehl 2003). Thus the mainstream strident cries of warning about “fundamentalists, nationalists, revisionists” in India and their colleagues in the West (Witzel 2003; Huld 2002; Kuzmina 2002) are seen to be either hollow slogans or echoes of those slogans; for neither the indigenist scholars mentioned above nor the archaeologists specializing in the ISC and insisting on its unbroken continuity down to the 6th century belong to this (non-existent) band of bogies. I benefited from all of them.

Nonetheless, the AIT persists. Several linguistic arguments have been adduced to prop up this fanciful theory one of them being the (loan-)words in the RV language which were allegedly borrowed from other languages met by the IAs on their way to and at Saptasindhu. A closely

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<sup>7</sup> In a private communication, Nov 2004, Mallory mentioned the Jews who wandered much and long and preserved their traditions of the *Old Testament*. If we take the Hebrew traditional orthodox view (which is by no means accepted generally), the Judaic people had literacy certainly since the time of Moses (c1300-1100?) and most probably since their stay in Ur of the Chaldees c1700-1600 (*Genesis* 11), since the Mesopotamians had literacy for more than a millennium earlier. So we can't say that the Jews maintained an oral tradition during their long travels. Then, the Jews mention constantly the (mis-)adventures they had and the different people they met on their travels. If we assume that the IAs had their oral tradition even as (according to the AIT) they travelled from the Steppe to Saptasindhu, what was it they were transmitting? The Vedic texts were composed (it is universally agreed) in India. So what were they transmitting before?... Why is there not one mention in the Vedic texts of dangers, mishaps and alien people met on the way – as we find in the Judaic texts? There is no such mention for the simple reason that, unlike the Jews, the IAs did not migrate. See discussion in paper Three.

related argument is the alleged common Indo-Iranian period during this journey to Saptasindhu<sup>8</sup>. Good examples of a combination of these two arguments are found in M. Witzel 2001, 2005 and F. Kuiper 1991. R.S. Sharma too makes the exaggerated claim that Vedic is “a mixture of Munda and Dravidian words with the Indo-Aryan language” (2004: 142) which is linguistically an absurd statement. But that 4% of the Rigvedic vocabulary might consist of loanwords (as these controversial studies claim), or even 6% or more, surely should not be surprising since the IAs did not live in a vacuum and there must have been peoples with different languages around them. However, this pet linguistic subject of Witzel’s has been attacked by R.P. Das (1995 general critique), by Krishnamurti (2003: 37-8 mainly against Dravidian cognations) and paper One<sup>9</sup>. Recently even (non-indigenist) H.H. Hock questioned all supposed borrowings finding them unconvincing (2005: 286) – and he happens to be a most eminent comparativist. In any case, no amount of arguing along these lines gives of itself any actual dates even approximately and the direction of movement can more reasonably be the reverse. There is B. Sergent who, although not at all an indigenist, argued in favour of a movement from Central Asia north-westward (1997). Also, Johanna Nichols, an eminent Indo-European linguist, found on purely linguistic grounds that “the locus of the IE spread was ... somewhere in the vicinity of ancient Bactria” (1997: 113) – which is only a stone’s throw away from Saptasindhu. Linguists will continue to disagree violently among themselves about the original locus (from the Baltic to the Balkans and to Bactria), about the direction of the movement and about the date: some give a late period somewhere c 3000, others an early one, in the 7th millennium (Misra 1992) – and all these disagreements on exactly the same linguistic data! I ignore all such linguistic arguments and conclusions since they are so contradictory. I ignore also the results from the reappearance in Gray & Atkinson (2003) of the discredited method of glottochronology. The situation is in no way improved by claims that “I am right” and allegations that “the others are wrong”. We must bear in mind also that all such arguments involve “reconstructed” proto-languages which are sheer conjectural affairs and therefore inadmissible in any impartial court of law.

**10. Horses and chariots** One of the best known comparative linguists of our times, Hans Hock, pointed out repeatedly (1999, 1996) that several dialects spoken in N-W India travelled north-westward at different times in the Common Era, the most notable one being that of the Gypsies in the early centuries CE (see also Fraser 1995). Hock stated that Indoaryan indigenism and even the Out of India Theory could theoretically be possible, provided the distribution of the IE isoglosses was accepted and the Vedic language was seen to be a devolved form of Proto-Indo-European – facts which I certainly accept fully. But he opted for the mainstream doctrine (=the AIT out of the Pontic Steppe) because of the archaeological evidence of horses and chariots arriving in Saptasidhu in bulk after 1700 BCE (1999:13). Thus he supports the spread of the Kurgan culture (as do also Allchin B. and R., Gimbutas, Kuzmina, Mallory, Parpola, Witzel et al), even though the cranio-skeletal evidence for this diffusion is very limited according to J. Day (2001). Hock also mentions that it is simpler to have one movement into India rather than many out of it to account for the diversity and chronologies of the spread of the various IE branches. But this is rather self-contradictory since, as he wrote (1996, 1999), the Indoaryan language left India north-westwards three times in the Common Era and generated new dialects and the Gypsies did reach the north-west extremity of Europe into England; so Indoaryan could have done the same in the misty past BCE. Besides, the ancient IAs could, in the Out of India scenario, have left in a sizeable mass, stayed in a location in Baluchistan/Afghanistan (or even the Steppe) for some time, then, successively, in groups, moved away in diverse directions. We need not make a problem out of this rather simple affair. (But I have argued in other papers that we must not preclude the possibility of a broad IE continuum from the Kurgan steppe down to Saptasindhu at a very early period from which, within which and into which occurred various undetermined movements.)

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<sup>8</sup> A common Indo-Iranian period there definitely was but this, according to a different view, is to be found in Saptasindhu whence later the Iranians left.

<sup>9</sup> Witzel’s earlier views on these loans and Bryant’s emphasis on linguistic argument (2001) have been contested by me in various articles; article 2002b is paper One herein.

On the matter of horses and chariots, Hock was misinformed, I like to think. The evidence of horses and chariots proves the very opposite if examined with a modicum of attention. As one authority put it, “the horse was widely domesticated and used in India during the third millennium BCE over most of the areas covered by the Indus-Sarasvati [or Harappan] Civilization” (Pande 1999: 344; the square brackets original). I myself collected much evidence for horse-finds in different ISC (=Indus-Sarasvati Civilization/Culture) sites in a papers of 2003, §10 (pp 196-7): thus one may further consult Kochhar (2000 pp 186, 192), Chakrabarti (1999), Lal (1997), Sharma (1996 p 17), Thomas et al (1995), Dhavalikar (1995: 116-7), Allchin & Joshi (1995: 95), Rao (1991) and, necessarily, Sharma (1980: 110ff). M. Danino’s paper ‘The horse and the Aryan Debate’ is a more recent addition to this literature (2006c). These publications show abundantly that the true horse, *Equus ferus* and *Equus caballus*, was present in its wild state in India from c 17000 and there is much evidence now for the domesticated horse in the Mature Harappan period. Moreover there are several terracotta figurines of this animal from different ISC sites as reported by Lal (2002) and Tharpar & Mughal (1994).

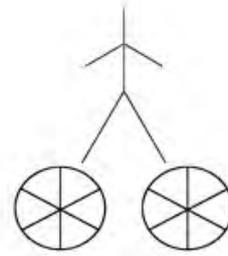


Lothal: Terracotta horse. Mature Harappan

As for the chariot, S. Piggott established the presence of a sophisticated type of vehicle with “one or two pairs of wheels with their axles... from the Rhine to the Indus by around 3000” (1992: 18).

Archaeological evidence does not consist only of the actual remains of buildings, weapons, tools, chariots etc. Pictures, reliefs, toys and figurines of these things are also evidence. Many years ago H. K. Sankalia had pointed out that the six-spoked wheel appears on seals and signs of the alphabet (1974: 363). S. R. Rao found at Lothal “terracotta wheels ... with diagonal lines suggesting spokes” (1973: 124). This representational practice seems to have been widespread, for S. Piggott mentions similarly marked wheels found in the Karpathian Basin from the Earlier Bronze Age (1983: 91-92). In his recent study, Lal presents four terracotta wheels (from Mature Harappan sites Banawali, Kalibangan, Rakhigarhi) with spokes painted on (2002:74, Figs 3.28ff). The Harappans had the technology for making spoke-wheels (Kazanas 1999:33; Basham 1954:21).

Finally, it was A. Parpola, an inveterate adherent of the AIT, who identified the figure on Harappan seal No 3357 as representing simplistically a man with outstretched arms standing on two six-spoked wheels (of a chariot) realizing that this was “a later invention of the Aryans’ (1969: 24). The later notion that it is a potter on two wheels is obviously far-fetched and, in any case, the wheel is spoked (Sethna 1992)! L. S. Rao has recently presented many more finds of models of terracotta toy wheels with spokes from Harappan locations in *Purātattva*, vol 36 (pp 59-66), 2006.



Spoked Wheel



Rakhigarhi: Terracotta wheel with painted spokes



Kalibangan: Terracotta wheel with painted spokes

11. The question of course is whether there were spoked wheels before 3000 since the *RV* uses the word *ara* which is usually translated as 'spoke' (1.32.15; 5.58.5; etc). We don't know. Some Indian scholars approach the issue strategically and say that Rigvedic hymns with *ara* are later intrusions from the second millennium when the spoked wheel was quite common. This is possible of course, since we know that some hymns are later interpolations. However, there could have been spoked wheels as we know them even at c3200 and before. But then again, *ara* need not mean 'spoke'. It could mean a section of the (solid) wheel. After all, different IE branches have a different stem for spoke (Gk *aktis*, *knēmē*; L *radius*; Gmc *speca/speihha*, etc) which suggests that spokes were developed after the dispersal (in the 6th or 8th millennium, or whichever). So *ara* could well have meant something other than spoke and only later acquired the meaning of 'spoke', as we know it now. The spoked wheel poses, in fact, no problem for dating the *RV*. There are other more clearcut types of evidence which are examined below.

The usual objection is that there are no chariot remains from that period. Here, there is the double assumption that *RV* chariots are necessarily like near-eastern ones of, say, 1500, or the Androvo c1900, and that there should be remains. But why assume that the *RV* chariots are like the near-eastern ones? The only real-life vehicle in a race that we know is in 10.102 and this is pulled by oxen not horses while 1.123.1 alludes to a 'broad chariot' *pṛthú rathaḥ* ! Elsewhere in the *RV* the chariot *ratha* is described as being *bṛhat* 'tall, big' (6.61.13), also *variṣṭha...vandhura* 'widest... seated' (6.47.9), *trivandhura* 'three seated' (1.41.2; 118.2; 7.71.4; 8.22.5; etc) and once even *aṣṭavandhura* 'eight-seated' (10.53.7)! These are all **rathas** and hardly like the near-eastern battle-chariots. Then again, there are no remains of chariots in India from 1500 or 1000 or 700. So, on the strict AIT thinking, we should say no chariots were brought in by the alleged IE entrants either.

12. **Harappan features absent in the *RV*.** The Harappan or Indus-Sarasvati Culture has certain characteristics which help to define its uniqueness. A number of these features are absent from the *RV* and this absence indicates that the *RV* is pre-Harappan. Arguments *e silentio* are not decisive since absence of evidence is not always evidence of real absence . But in this case the

features are far too many. (Some of these were noted by Sethna, 1992.)

**a)** *iṣṭakā* 'brick'. The *RV* mentions as building materials metal, stone, mud and wood but not 'brick', which was the basic material in Harappan constructions. This is found in post-Rigvedic texts: the word *iṣṭakā* is not in the *RV*. Archaeologists write of the early Harappan or Ravi phase (ie 3300-2800): "These early settlers built huts made of wood with wattle-and-daub" (K. Kenoyer and R. H. Meadow 2007:125). This is the common habitation in the whole of the *RV*. *Brick-walls came later, as Kenoyer pointed out much earlier: these appear after this early phase, ie after 2800 (Kenoyer 1997/2000:56). The dates 3300-2800 BCE are different from those given by S. P. Gupta who places this early phase c 3700 and before and calls it Hakra-Ravi (2007:223).*

**b)** Urbanization is wholly absent in the *RV*. There certainly was "nomad pastoralism" as mainstreamers emphasize repeatedly but there were also agricultural settlements (a fact which mainstreamers underplay or do not mention). The hymn to Kṣetrapati 'Lord of the Field/Soil' (4.57) alone should suffice but also the girl Apālā refers to her father's *urvarā* 'fertile field' (8.91.5); then there are many cultivation implements *khanitra* 'shovel', *lāṅgala/sīra* 'plough', *ṣṛṇī* 'sickle', etc. Moreover, there is weaving with loom, shuttle, warp and woof (*RV* 1.134.4; 1.3.6; etc, etc) and metallurgy with smithies of sorts (4.2.17; 5.9.5; etc). Such activities imply settlement.

Some writers think the Rigvedic and Harappan cultures converge (Gupta 2005, Bisht 1999, Lal 1998, Singh 1995). As evidence is cited the word *pur-* which denotes 'city, citadel, fort, town' as its Greek and Baltic cognates '*polis*' and '*pil(i)s*' do. This is a very general misconception. In the *RV* *pur* never means anything other than an occult, magical, esoteric defence or stronghold which is not created nor ever destroyed by humans as we saw earlier as in 'R̥gvedic Pur', paper Four. The ISC cities had regular blocks, large buildings, also domestic and urban water-supply (McIntosh 2001: 100-101; Gupta: 2007 235): the *RV* knows nothing of all these. There are references to *oka*, *gr̥ha*, *dama*, *dhāma* etc, all of which can denote any type of 'home/house' (made of wood and mud), or the thousand-pillared mansion of kings Mitra and Varuṇa in the sky (2.41.5; 5.62.6: probably suggested by sunrays streaming down through clouds; for not even ISC cities had such mansions!). These most certainly do not indicate any urbanization: neither brick- nor stone-walls are mentioned nor other features found in the ISC towns.

Such rigvedic settlements are pre-urban as known in the mature Harappan. "These are found spreading from Taxila in the northwest through the eastern foothills of Baluchistan through Rajasthan and Haryana via Punjab, Sindh and Kachch. All this presupposes the existence of a very strong internal network which was fully operational by 3000 BC" (Gupta 2007: 214).

The words for 'council' *sabhā* and *samiti* are also cited by some scholars but, surely, any community can have a council of elders without urbanization. Allusions in the *RV* to chiefs/kings *rājā* and overlords/emperors *samrāt* also do not show urbanization since such offices can just as easily exist in rural communities. (The Red Indians in North America, nomadic and rural tribes, had local chiefs and overlords.) Pathways and/or roads (*path-*) also have been mentioned as crossing or branching out, but these too can be just as easily seen in a rural context. (For all these claims see Singh 1995; Bisht 1999 and Lal 2002, 2005.)

**c)** Fixed altars or hearths are unknown in the *RV* but common in the ISC cities. The Rigvedic altar is a shallow bed dug in the ground and covered with grass (e.g. *RV* 5.11.2, 7.43.2-3; Parpola 1988: 225). Fixed brick-altars are very common in post-Rigvedic texts.

**d)** Many cities were abandoned and fell to ruination after 1900 BC when the Harappans began to move eastward because of the drying up of the Sarasvatī and of the more general desiccation due to tectonic disturbances and climatic changes. The *RV* knows nothing of such ruins even though, according to the AIT, the IAs moved through these regions c 1700-1500 (in small waves, settled there, in the midst of deserts, and wrote the hymns which praise the mighty Sarasvatī!). Some attempts have been made to read hymn 1.133 with its *arma-ka* (=of unknown meaning, really) as a description of a ruined city (e.g. Burrow 1963, Rao 1991:32) but, in fact, the hymn mentions no ruined buildings, no fallen walls and no materials such as wood, stone or bricks! It is a ghostly scene of frightful desolation, peopled only with unfriendly she-fiends and demons (*yātumati*, *piśāci* and *rakṣas*). In sharp contrast the Old English poem *The Ruin* contains such persuasive details of the ancient remains (from Roman times?) that some scholars

think it refers to the town of Bath (Mitchell & Robinson 1996:252-5).

**e)** No cotton *Karpāsa* appears in the *RV* although this plant was extensively cultivated in the ISC and the fabric was exported as far as Egypt in the middle of the 3rd millennium while the Mesopotamians adopted the name as *kapazum* (? from *prākṛta kapāsa*). The *RV* has ‘skin’ *eta* (1.166.10; *ajina* in *AV* 5.21.7 etc), ‘wool’ *avi* (*RV* 9.78.1) and *sāmūlya* (10.85.29) and numerous terms for clothing and weaving but no mention of cotton. Be it noted that *karpāsa* is the only word for cotton in Sanskrit. It is found first in the *sūtra* texts, in Gautama’s (1.18) and in Baudhāyana’s (16.13.10) *Dharmasūtra*. Now, although cotton seeds were found at Mehrgarh period II, c5000, none were found in subsequent periods. Cotton cultivation appears only in the Mature SSC, after 2500. Thus the *sūtra* texts can be placed at this period at the earliest, i.e. c 2600.

**f)** Silver *rajata* also makes no appearance in the *RV* though gold and copper are well attested and silver is plentiful in the ISC. The word *rajata* occurs in *RV* 8.25.22 and it denotes a steed or a chariot ‘shining white’. Only in later texts is it used singly (*AV* 5.28.1) or with *hiraṇya* to denote ‘silver’ or ‘white gold’ = ‘silver’ (see *Vedic Index* 2: 196-7 and Lubotsky 2: 1169).

**g)** Rice *vr̥hi* too is absent from the *RV* although it appears in various sites of the ISC from at least 2300 (and in the Ganges Valley from the 6th millennium)<sup>10</sup>. The *RV* knows only *yava* ‘barley’. Rice becomes important in post-Rigvedic ritual and the more general diet. Some writers argue that the *RV* has food-preparations of rice like *apūpa*, *puroḷāś* and *odana* (Talageri 2000: 126-7). This is possible, of course. All three are in post-rigvedic tradition said to be rice-preparations (though *apūpa* is given as flour-cake in most texts and ‘wheat’ in Lexica). But *odana* is primarily a water or fluid preparation (*ud-*) and *odatī* ‘refreshing, dewy’ is an epithet of *Uṣas*, the Dawn goddess. Since *vr̥hi* ‘rice’ does not appear in *RV* (but does appear in *AV*) and rigvedic *yava* is from the earliest tradition accepted as ‘barley’, I take it that the rigvedic people had barley and not rice.

**h) Literacy is not known in the RV.** Some scholars think it was known and cite *RV* 10.71.4 or *AV* 19.72 (e.g. Kak: 2003, Frawley 1991).

*RV* 71.4a says: *utá tvaḥ páśyan ná dadarśa vācam, utá tvaḥ sr̥ṇvān ná śr̥ṇoti-enām* ‘seeing indeed (utá) one sees not *Vāk* [and] hearing indeed one hears her not’. *Vāk* is the mighty goddess of Speech (or Language) through which all things excellent and pure were manifested (*nīhitam-āviḥ*, stanza 1). What st 4a-b says is that some people see and hear but, in fact they don’t know *Vāk* (a) and the goddess reveals herself to those [she chooses] as a well-dressed, loving wife to her husband (b). There is not a single hint in all eleven stanzas that there is writing. In fact st 11 says that some sing (*gā-*) the psalms and prosper and some brahmins utter (*vad-*) knowledge and thus deal out (*vi-mā-*) the measures/regulations of sacrifice; there is no suggestion of writing: it is all oral. *vāc* means ‘speech, utterance’, never writing.

D. Frawley thought that a passage in *AV* 19.72 may refer to writing: ‘From whichever receptacle *kośāt* we have taken the Veda, into that we put it down’. Books in ancient India consisted in collections of palm-leaves or strips of birch-bark and were kept in boxes (1991: 249). Whether this is enough to establish knowledge of writing is doubtful. The word *veda* is ‘knowledge’ generally and not necessarily one of the three Vedas which in any case were transmitted orally. The word *kośa* could refer to some (metaphorical) non-material storing-place, e.g. memory, lower mind (*manas*) higher mind (*cetas*, *bodha*, both in *AV*) etc. In any case, there is no other passage even remotely hinting at writing.

Words like *likh-*, *lekha(-na)* and mainly *lipi* (Pāṇini 3.2.21) denoting ‘writing’ are not in use before the *sūtra* texts. The vast corpus of *Brāhmaṇa*, *Aranyaka* and *Upaniṣad* texts have not a single hint about writing and so create an enormous gap between the *AV* and the *sūtra* period. Someone would have made a reference to writing somewhere in all these texts! So, writing was known in the ISC and the *sūtras* but not in the *RV*.

<sup>10</sup> Private communication from S.P. Gupta, Chairman of Indian Archaeological Society (June 2006). See also Sharma 1980 for rice in the Ganges basin in the 5th millennium and R. Tewari et al in *Purātattva* 2006 (vol 36: 68-75) for rice, again in the Ganga basin, north-east, in the district Sant Kabir Nagar (UP) in 7th millennium. These locations are too far from Vedic Saptasindhu but it seems likely that by 3000 the composers of the *AV* had become acquainted with rice *vr̥hi* and later this grain was cultivated in the ISC too.

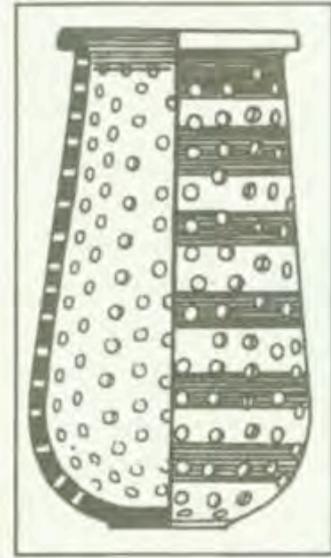
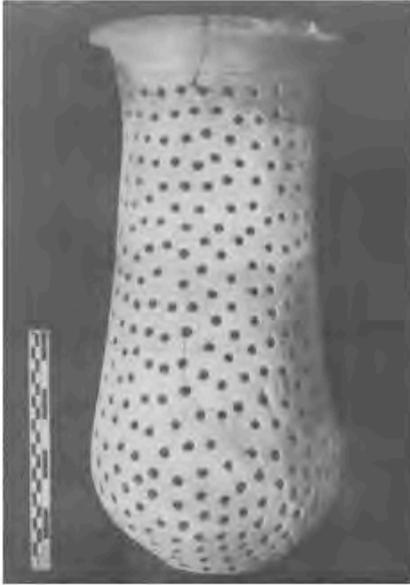
True, *Aitareya Āraṇyaka* 5.3.3 has both *ul-likhya* and *ava-likhya* and at first sight one might think these are references to writing particularly as they occur in a context about study (Deshpande 1966). But this very context, in fact, disallows the meaning “writing”. For the text says “the student should not study/learn (*adhīyāta*) ... after “writing” (*ul-likhya*); if the student does not learn in conjunction with writing, then when would he do so? Moreover, the wider context stresses memorizing from the guru’s utterances; again, if there was writing, it would have been mentioned and memorization would not have been stressed. (See Falk 1992 with full references). So, in fact, there are no allusions (*likh-*, *lip-*, ) with the meaning ‘writing’ before the Sūtra-texts.

i) The *RV* has no allusions to artistic iconography – paintings, relief representations, statue(-tte)s or seals, all so common in the ISC. (The *RV* 4.24.10 asks “Who will buy this my Indra” and this is thought by some to refer to a statuette, but this could be a transfer of favour and it is the only reference in the whole *RV* without the use of any word for statue or icon.)

**13.** Now all the fore-mentioned features are found in post Rigvedic texts – the *Samhitās*, the *Brāhmaṇas* and fully in the Sūtra literature. For instance, brick altars are mentioned in *Śatapatha Brāhmaṇa* 7.1.1.37, or 10.2.3.1 etc. Rice *vrihi* is found in *AV* 6.140.2; 7.1.20; etc. Cotton *karpāsa* appears first in Gautama’s (1.18) and in Baudhāyana’s (14.13.10) *Dharmasūtra*. The fact of the convergence of the post-Rigvedic texts and the Harappan culture was noted long ago by archaeologists. B. and R. Allchin stated unequivocally that these features are of the kind “described in detail in the later Vedic literature” (1982: 203).

If we had a case of 2 or 3 items we could bypass them saying indeed that absence of evidence is no evidence of absence; but the items are many and the absence of the building material *iṣṭakā* in the *RV* (against the presence of wood, stone, mud and metal) seems to me quite decisive.

We must also take into account that many iconographic motifs, Harappan artefacts, decorations or seals, show affinity with elements found in post-Rigvedic texts. Thus PK Agrawala (2005) draws attention to round-bottomed perforated pots from Harappan sites and a vessel (*kumbha*) with nine holes (*navavitṛṇṇa*) or 100 holes (*śatavitṛṇṇa*) mentioned in *Śatapatha Br* 5.5.4.27 and *śatavitrṇṇā kumbhī* ‘a pitcher with 100 perforations’ in 12.7.2.13. These and other similar descriptions echo the White Yajurveda ( *Vājasaneyī Samhitā* )verse 19: 87 ‘a pitcher with 100 streams’. Such vessels were used for ritual sprinkling. A second parallel is furnished by the two-horned bovine-like animal, duplicated and facing itself, on a Mohenjodaro seal with long necks and the pipal tree growing out of their juncture. This corresponds (writes Agrawala) to the two-headed cattle *dvāyā* in *AV* 5.19.7. Agrawala mentions other parallels of a two-headed tiger and a two-headed bird (2005: 10 - 13). Thus it is indeed the later Vedic texts that have parallels with the Harappan arts and crafts, not the *RV*.



Pot with 100 perforations: Śatapatha Br 12.7.2.13.



dvāyā in Atharvaveda 5.19.7

N Rajaram follows a similar line of investigation (2005) and adduces iconographic motifs from Harappan seals but does not distinguish between Rigvedic (or early Vedic) and post-Rigvedic (or late Vedic) periods and cultures. Thus he cites the *aśvattha*-tree foliage and the horse and links them to the *RV* disregarding the fact that these are not exclusively Rigvedic and therefore could be simply from a post-Rigvedic period. Moreover he cites the *svastika* and the sign of sound *OM*, both of which are, as he states, from post-Rigvedic texts. It is also doubtful

that the seal-sign which is horizontal, depicts the symbol for OM, which should be upright, anyway. (But, of course, the seal could be seen as upright since we don't know what the symbols indicate.)

It is an astonishing phenomenon but some scholars make the extraordinary claim that because these items are not in the *RV*, this text must be post-Harappan (Witzel 2001; R.S. Sharma 2004)! Thus, blithely and illogically they put the proverbial cart before the horse and – and push!

On this score, then, the bulk of the *RV*, is pre-Harappan, or 3000+. This, of course, could well be the very early proto-ISC (=Harappan culture) starting at c3800 (Gupta 2007: 214; see §12b).

**14. Some Brāhmaṇas comment on** or give explanations of incidents in the Rigvedic hymns. For example, the *Aitareya Br* 7.13.33 narrates extensively the story of Śunaḥśepa, alluded to briefly in *RV* 1.24.12-13 and 5.2.7, while the *Śatapatha Br* 11.5.1 comments on the the Pururavas and Urvaśī love story given elliptically in dialogue form in *RV* 10.95. Another point to note. Some legends in the *RV* remain unexplained. For instance, who was Bhujyu whom the Aśvins saved from a tempest (1.116.3-4; etc) and how did he find himself in that predicament? The later texts say nothing more. Or, take the birth of Indra; was he an unwelcome child to Aditi and did he commit parricide (4.18.1ff; etc)? Again, the Brāhmaṇas tell us nothing. Obviously such exegetical texts would not have been composed until the understanding regarding the older texts had lapsed: this implies many centuries.

What is the date of the Brāhmaṇas? Well, S. Kak ascribes the *Śatapatha* to the early third millennium (1997, 1994) saying that the Pleiades/Kṛttikas not swerving from the east, as is stated in this *Brāhmaṇa* (2.1.2.3), could only occur c2950 BCE. Narahari Achar (Prof. Astrophysics, Memphis, USA) using computer and planetarium apparatus arrived at a date c3000 pointing out that S. B. Dikshit had arrived at similar conclusions 100 years earlier but was ignored by Western scholars (1999): these scholars really had no knowledge of astronomy. B. B. Lal too thought that “astronomical calculations (re *Aitareya Br*) would place the *RV* in the fourth millennium” but did not then adopt such a date only because he could not judge the evidence (1997: 286).

So we could again place the *RV* easily before 3000.

**15. The *Bṛhadāraṇyaka Upaniṣat*** has three *vaṃśas*, i.e. list of teachers, each comprising 65-70 names. The first 4 or 5 are names of gods in the normal Indic way which ascribes the beginning of every human activity to some deity. Let us take the mainstream date of early sixth century for this text (say 550) according to the AIT premises and let us take 60 teachers giving to each one an average of 15 years, though 20 and 30 would be more realistic. This exercise has now been tilted very heavily in favour of the AIT and mainstream views because, in truth, in this text we read “I ask about that upanishadic Person (*aupaniṣadam puruṣam pṛcchāmi* 3.9.26) which indicates that there was a traditional upanishadic teaching about Puruṣa as a spiritual being (=Self) and this aspect we meet in the *Atharvaveda*. However, let us bypass this point. These calculations (60×15=900 plus 550) give us a date c1450 for the inception of the doctrines in this *Upaniṣat*. The chief doctrine is that the self of man (*ātman*) is the same in all beings and the same as the Self of the universe (*brahman* ‘Mystic Spirit’ or ‘Absolute’). We should also bear in mind that the teachers’ names are quite different from those of the seers of the *RV* hymns as given by the native tradition. Following others, M. Witzel thinks these lists “rest on typically weak foundations” (2001, §19) but this is a typically weak subterfuge because the results of calculating the number of years prove how unrealistic the AIT chronologies are! Let us see, then.

The Upanishadic doctrine of the identity of the individual self and the universal self, in the formulations *ayam ātmā brahma* and *ahaṃ brahmāsmi* ‘the Self is the Absolute’ and ‘I am the Absolute’, should be known, then, c1500 or 1300 or 1200, when, according to the AIT, the *RV* was composed. Yet, quite surprisingly, this doctrine is totally unknown in the *RV* in these terms (although enunciated differently) and begins hesitantly to appear in the *AV* (eg 10.2.32-3; 11.4.23; etc). Consequently, the *AV* should be placed at least c 1600 and the *RV* c 1800, always following the AIT assumptions. But the *RV* is composed, even according to Witzel (2005: 90), in Saptasindhu, yet the IAs do not appear in this area before, at the very earliest, 1700, and the *RV*

gets composed after several centuries!

This is one of the comical paradoxes that the mainstream chronology refuses obdurately to resolve. Yet, on the grounds given in §§12-13 above, we saw that the *RV* should be assigned to the 4th millennium, say around 3600. Now subtracting two hundred years for the *AV* hymns and the start of the Upanishadic doctrine and a further 900 or 1000 years (for the teachers) we should place the early Upaniṣads at the start of the period of the Mature Harappan, i.e. 2500 (with the *ātma-brahman* doctrine having come down orally) and the Sūtra texts immediately after. These dates satisfy yet another requirement. The word for cotton *karpāsa* is first used in the Sūtra texts as we saw in §12,e above and the cultivation of the plant (although seeds of it were found in much earlier periods) gets well established c 2500. All these dates are, of course, approximate.

**16.** Astrophysicist Achar pursued his **palaeoastronomical research** into the *Mahābhārata* epic also, examining astronomical references in Books 3, 5 and 18. His sky map showed that all these converge in the year 3067. (Achar 2003; see also paper One). Achar acknowledged that, in 1969, S. Raghavan had arrived at the same date. Now, it is obvious that the *Mbh* had acquired many accretions over many centuries and that it was streamlined stylistically perhaps first in the 2<sup>nd</sup> millennium and finally at about the start of the Common Era. It is obvious that it contains much late material like 2.28.48-9, which mentions Rome and Antiochia (*romā* and *antakhī*): this could not be earlier than 300 BCE since Antioch was founded in 301. On the other hand, the frequent use of the bow and, moreso, the use by Bhīma of a (tree trunk as a) club show much more primitive conditions.

Thus the war took place in 3067 and the core of the *MBh* in poems and songs was laid down in that year. This and the native traditional view that the Kali Yuga came at 3102 are both correct, according to Achar. He pointed out that the Kali Yuga had no full force until the death of Kṛṣṇa which occurred 35 years after 3067, at 3032 (private communication June 2006); but immorality had set in already, as is shown by the unjust behaviour of the Kauravas and some reprehensible acts during the war itself. Surely no bards (compilers or redactors) in the 3rd cent CE or the 3rd cent BCE could possibly know the star and planet positions relative to the *nakṣatras* or the zodiac signs of the year 3067. The astronomical references examined by Achar (and Raghavan) are so numerous that chance coincidence has to be discarded (Achar 2003). However, that the war took place in 3137 and bards began to sing of these events two generations later should not be precluded. Personally, I still tend towards the traditional view of the War taking place in 3137. The Megasthenes report (from c300 BCE) of the ancient kings from 6000+, surviving in Arrian and other classical writers, supports these long periods of the past (paper One). Then, deterioration in behaviour would have started in the *sandhyā* transitional period before the onset of the Kali Yuga – when also the Kṣatriyas passed away. Here a question remains: how did the astronomers (and Āryabhaṭa especially) determine the date for Kali Yuga as 3102? Is there an answer?

**17. The Sarasvatī** river furnishes useful literary and archaeological evidence for dating the *RV*. It is a mighty river extolled in all Books of the *RV* except the fourth. It is *nadītamā*, *ambitamā*, *devitamā* ‘best river, best mother, best goddess’ (2.41.16); it is swollen and fed by three or more rivers *pinvamānā sindhubhiḥ* (6.52.6); it is endless, swift moving, roaring, most dear among her sister rivers; together with her divine aspect, it nourishes the Indoaryan tribes (6.61.8-13). In 7.95.2 the river is said to flow pure in her course “from the mountains to the ocean” *giribhyaḥ ā samudrāt*. Then 7.96.2 and 10.177 pray to the rivergoddess for sustenance and good fortune while 10.64.9 calls upon her (and Sarayu and Indus) as “great” and “nourishing”. Clearly then, we have here, even in the late Bk 10, a great river flowing from the Himalayas to the ocean in the south, fed and swollen by other rivers and sustaining the tribes of the IAs on its banks – not a river known in the past or in some other region, or a river now considerably shrunk (Witzel

2001)<sup>11</sup>.

Some scholars claim that here *samudra* does not mean 'ocean' but confluence and especially the place where a tributary flowed into the Indus (e.g. Klaus 1989 and Witzel 2001). The last point can be discarded since there is not the slightest hint elsewhere that the Sarasvati flowed into the Indus – in which case the Indus and not Sarasvati would have been lauded as the best river (2.41.16) We can also aver with full certitude (as the *Vedic Index* does under *Samudra*) that the rig-vedic people knew the ocean (see paper Five, above; also Prabhakar 1994). The meaning 'terminal lake(s)' adopted by Witzel is entirely fanciful. In his Dictionary M. Mayrhofer gives for *samudra* only 'confluence' and 'ocean/sea' (1996 *EWA*). And the Vedic poet would certainly have used (not *ā samudrāt* but) *ā sarobhyaḥ* 'to the terminal lakes' maintaining his – ॐ – cadence. This phrase would then have indicated clearly the alleged fanciful etymological connection of the name *Saras-vatī* 'she who has (terminal) lakes'. The name means rather 'she who has swirls and currents', since the primary sense of  $\sqrt{sr}$  (>*saras*) is 'movement' (*gatau*) or 'flowing, leaping, rushing'.

Please, consider also that the Vedic **-s-** is inherited from PIE, according to all IEnists, whereas Avestan **-h-** is a devolved, not PIE, sound. Vedic  $\sqrt{sr}$  has many primary and secondary cognates like *sara*, *sarit* etc. Now Avestan has no cognates for  $\sqrt{sr}$  and its products, and the Avestan noun for lake is *vairi-*, while *vār-* is 'rain(-water)' = S *vāri*(?). The stem *hara-* (cognate with Vedic *saras*) occurs only in the river name *Hara<sup>v</sup>vaiti*. Consequently, it is the Iranians that moved away from the Indoaryans as, indeed, is shown by their memory of having lived in a location they called *Hapta<sup>h</sup>handu* = Saptasindhu. The root *sr* has cognates in other IE branches, Gk *hial-*, Latin *sal-*, Toch *sal-* etc as is shown by Rix 1998. Now, it would be ludicrous to claim that the IAs left the common Indo-Iranian habitat, as per the AIT, moved into Saptasindhu and turning the *Harax<sup>v</sup>vaiti* name into *Sarasvatī* gave it to a river there to remember their past while they proceeded to generate the root *sr* and its derivatives to accord with other IE languages. Occam's razor, which here is conveniently ignored by AIT adherents, commands the opposite: that the Iranians moved away, lost the root *sr* and the name *Sarasvatī* in its devolved form *Hara<sup>v</sup>vaiti* was given to a river in their new habitat. This, together with the fact that, like Greek, Avestan has no obvious system of roots and derivatives (as Sanskrit has), should be enough to question if not refute various IEnists' claims that Avestan retains older forms of nouns and verbs and that therefore the Indoaryans were with the Iranians in Iran in the common Indo-Iranian period – before moving to Saptasindhu. One should also note that these linguists rely entirely on linguistic facts amenable to a reverse interpretation and ignore other aspects – literary, mythological, archaeological and genetic (for which see §19).

The river *Sarasvatī* in Saptasindhu is thought to have dried up almost completely by 1900 (Allchins 1997: 117; Rao 1991: 77-79). In previous years it had lost tributaries to the Indus in the West and the Ganges in the East. Is there any evidence that it flowed down to the Indian (or Arabian) ocean at any earlier period?

G. Possehl examined (1998) all the palaeoenvironmental and geological data relevant to the *Sarasvatī* river and concluded that the river could have flowed down to the ocean only before 3200 at the very latest and, more probably, before 3800! He re-stated his finds in his study of 2002 (pp 8-9). P-H. Francfort has been just as certain of a date 3600-3800 in his survey of 1992.

All this helps us place the passages ascribing the grandeur of river *Sarasvatī* at a date before 3200 at least. (For more recent scientific investigations through satellite showing the course of the old *Sarasvatī* reaching the ocean see Sharma J.R. et al 2006.)

**18.** In a recent publication **Dr S. Levitt** ( of New York), who is by no means an indigenist, examined the development of the "early Indic tradition" and the development of religion in ancient Mesopotamia.

<sup>11</sup> The mainstream view (Witzel's really) that the Vedic river is merely a memory of the Iranian *Harax<sup>v</sup>vaiti* which belongs to the common Indo-Iranian period, when the Iranoaryans lived together in Iran before the IAs moved further southeast (according to the AIT), is no more than modern myth-making. Mainstreamers often invoke Occam's razor (i.e. that the simpler solution is more probable) but here they forget it and prefer their own complex scenario.

After comparing several elements in the Vedic and Mesopotamian religions, Levitt concluded: “We can date the early Indic tradition on the basis of comparable points in ancient Mesopotamia. By this, the *R̥gveda* would date back to the beginning of the third millennium BC, with some of the earliest hymns perhaps even dating to the end of the fourth millennium BC” (2003: 356).

However, unaware of Levitt’s paper, I myself made at about that time a very detailed comparative study of Vedic and Mesopotamian religious (mythological) motifs, published in *Migration and Diffusion* vol 24, 2005<sup>12</sup>. In this I showed that since more than 20 motifs in the Vedic texts had close parallels in other IE branches (e.g. the horse mythology, the skyboat of the Sungod, the Flood, the elixir from heaven, the creation of cosmic parts from the dismemberment of a divine being, etc) and were therefore of Proto-Indo-European provenance, they could not have been borrowed by the Vedics from the Mesopotamians as is usually alleged (McEvilley 2002; Dalley 1998) but must be inherited and therefore older than the Mesopotamian (Sumerian, Babylonian etc) parallels. Since the Mesopotamian culture (starting with old Sumerian) surfaces c 3000, the Vedic motifs must be earlier. Most of these have no parallels in Ugarite, Hebrew and other intermediate Near-Eastern cultures.

Thus again we arrive at a date before 3000 for the bulk of the *RV*.

**19.** Since, according to the preceding discussion we must now assign the (bulk of the) *RV* to c 3200 at the latest and since the *RV* by general consent was composed around the Sarasvatī, then it follows that the IAs were ensconced in Saptasindhu by 3200 and that the ISC was a material manifestation of the early oral Vedic tradition expressed in the *RV*. This issue was treated by me extensively in preceding papers and no more need be said now. I should only add that, in fact, more and more scholars in the West have re-examined the issue and rejected wholly or in part the mainstream view advocating instead a movement Out of India into Europe: Schildmann 1998; Klostermaier 1998, 2000; Friedrich 2004; Hasenpflug 2006. (See also §9, above.)

To all this I should add the increasing evidence from Genetics which declares that no substantial flow of genes occurred from Europe or the northwestern adjacent areas into India before 600 BCE. On the contrary, recent genetic studies show an outflow from India into countries west and north and Europe (Sahoo et al 2006; Oppenheimer 2003). M. Danino has made a useful overview of the evidence produced by studies over the last 10 years in his paper ‘Genetics and the Aryan Debate’, published in *Purātattva* (2006b). Thus the Indoaryans are now shown to be indigenous to Saptasindhu and there is nothing to undermine my contention that the *RV* in its bulk is pre-Harappan. What I am arguing for primarily and most emphatically is the proposition that, since the only biological change is in the skeletal record of the area and occurs c 6000-4500 (paper One with references), if the Indoaryans made an entry it would have happened at that period; but by 1700 BCE they certainly were indigenous. Genetic evidence, however, suggests strongly now an Out-of-India scenario.

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 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).

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: my emphasis). He adds that this M17 marker travelled from India or Pakistan

<sup>12</sup> This was badly printed and the Sanskrit transliterations are unreadable! A revised version was published in *Adyar Library Bulletin* 2007. This is now paper Seven in this volume.

(= our Saptasindhu) through Kashmir, Central Asia, Russia and then Europe after 40000 BP (p 154). Thus migration is from east westward.

A more recent study (Sahoo ... Endicott, Kivisild... Kashyap 2006) concludes: "The Y-chromosomal data consistently suggest a largely South Asian origin of Indian caste communities and therefore argue against any major influx, from regions north and west of India" (p 843); then again: "It is not necessary, based on the current evidence, to look beyond South Asia for the origins of the paternal heritage of the majority of Indians at the time of the onset of settled agriculture. ... our findings do support a local origin of haplogroups F\* and H" (p 847).

However, all these finds are by no means final and we need to be cautious; but the indications now are that there will be no reversal. If there was a migration, this was most probably out of Saptasindhu and the adjacent region.

Why mainstreamers insist on the AIT is a mystery. Lord C. Renfrew wrote of the AIT (1989:182): "this comes rather from a *historical assumption* about the 'coming' of the Indo-Europeans" (my emphasis). Then Edmund Leach wrote (1990): "Because of their commitment to a unilateral segmented 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 has originated outside India or Iran... From this we derived the myth of the "Aryan invasion". These are the two legs of clay upon which stands the AIT and its variants.

Leach went further saying that after the discovery of the Indus-Sarasvati Civilization "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" (1990). This is still true. But the new genetic evidence will soon perhaps force linguists to reconsider their theories. And we must not forget that there may well have been an IE continuum from the Steppe to Saptasindhu and the IAs did not move from their location. It is worth noting that S. Zimmer admitted (2002) that (although himself a mainstream non-indigenist) he could not be certain of the exact location of the PIE homeland since the facts are so obscure in those far-off times. More recently, H-P Francfort, the eminent excavator of Shortughai, expert on Central Asia Oxus area (or BMAC) and N-W India, critiqued V. Sarianidi, E. Kuzmina and J. Mallory and their theories about [proto-]Indo-Iranian movements through the Oxus region (2005: 262-8); further on (p 283 ff) he pointed out that the pantheon in the Oxus iconography has a dominant goddess and so does not tally with Iranian and Indoaryan religions: on the whole he is most reluctant to accept Indo-Iranians (or Aryans) passing through that area c 1800-1400 BCE. So even some mainstreamers have now serious doubts about the alleged Aryan immigration/invasion.

**20.** Does it matter whether the RV was composed in the 4th millennium or c 1000? whether the Indoaryans are indigenous or came to India c 1500? After all life goes on irrespective of such ancient events. So from one point of view, no, it doesn't matter. But from another, it does matter. First, it is the function of scholarship to establish and promote true knowledge so that our life be regulated by this – not prejudices, partisan views (even patriotic but false) or pet theories. Second, Indian (proto-)history must be restored and revalued in a correct time-frame. It is sad, indeed, as Frawley wrote (2002 *The Hindu* June 18th), "to note how intellectuals in India are quick to denigrate the extent and antiquity of their history". Third, the RV contains ideas that are of great value to mankind and reveal, as other recent discoveries in prehistoric cultures (Rudgley 1998 *passim*), that so-called "primitive" people had much important knowledge that held them at a fairly high level of civilization enabling them to live in harmony with the natural processes (*ṛta*) of their environment. The RV, e.g., preserves for us, from that so ancient period, the idea of a Primal Unity that is the First Cause of the universe and all its phenomena: not only in the *Nāsadiya sūkta* and the *Puruṣa sūkta* (10.129 and 90) but also in 1.164,6; 3.55 refrain; 6.75.19; 8.58; 10.114.5 and less obviously in others. This concept is absent from all other IE traditions (Hittite, Greek, Roman etc) and may well have been an essential constituent of PIE culture lost in the other branches. Thus the Rigvedic IA culture (and perhaps PIE) consisted not so much of material artefacts but of inner knowledge and spiritual strength – *bráhma várma mamāntaram* 'the holy-power is my inner defence' RV 6.75.19.

Let us hope that the noxious AIT and all notions rooted in it will sooner rather than later end up in the only place they should be – the dustbin of History. Let us also hope that the *RV* will be re-examined not only for its historical information but its cultural ideas that will illuminate many other studies and disciplines in Ancient History, Anthropology, Civilization, Linguistics, Religion, Sociology, etc.

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