Indo-Aryan indigenism and the Aryan Invasion Theory arguments (refuted).

N Kazanas, Omilos Meleton, Athens, January 2006.

1. Abstract. This paper examines the general IE[1] 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 PIE1 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 and confidence in this reconstruction are misplaced. Indeed, all reconstructions of Proto-languages seem futile and, since they are in no way verifiable, should not be used as evidence for historical events. The AIT claims that the IAs [1] entered Saptasindhu (=N-W India and Pakistan) c 1700-1500[2]. Actual archaeological evidence and the IA documents are totally against such an entry. The linguistic data used as evidence for the AIT can furnish no evidence at all either for the date of this entry or for the entry itself: in fact, they can be, and have been, interpreted quite differently. It is generally 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; but even so, if these people were the IAs, they must, surely, be regarded as indigenous by 1700 or 2000! In anycase, recent genetic studies do not suggest any entry of IAs in such numbers as would accomplish the full Aryanization of Saptasindhu and the farther North India in the Holocene, i.e. within the last 10 000 years; on the contrary they state that the European peoples came out of South Asia (and India) after 50 000 BP. Apart from recent genetic studies, other kinds of evidence and arguments will be used in full to demonstrate indigenism: the evidence consists in hard facts and data, in contrast to the AIT arguments. Indeed all the data used as evidence by the AIT are wholly conjectural and arbitrary and often consist of misrepresentations and distortions, as will be clearly demonstrated in detail. All the arguments used for the AIT have been analytically presented by E. Bryant (2001) and summed up in his concluding chapter. These will be examined one by one and shown to be fallacious. We shall also refer to some material not in Bryant – e.g. genetic studies after 2001CE and mythological motifs never examined in this connection.

Indoeuropean and ProtoIndoEuropean

2. 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 Primeminister 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.

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[1] IE = IndoEuropean; PIE = ProtoIndoEuropean; IA = Indoaryan(s).

[2] All dates are BCE except where stated otherwise. Dates in brackets, e.g. (2001) denote a publication by a modern author and will be found in the Bibliography.

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 extravagant 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 1957 , 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.

3. No 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. We seem just as anxious to fill the gap of our ignorance in respect of much more important matters like our origin before conception and birth, our purpose in the world (if there is one) and our destination beyond death. 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 (§7, 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 §4, 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).

4. Let us look in detail at some of the difficulties involved in diffusion-waves.

(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 2005 b). 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 *bratrǔ*; 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/lufaþ*; Sl *ljubiti* : 'loves'..

6 eyebrow : S *bhrū* ; Av *brvat-*; Gk *o-phrus* ; C *brūad*; Gmc *brūn* ; B *bruvis* ; Sl *brǔvǐ* ; Toch A/B *pärwān(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 *nosĭ*.

12 palm (of hand) : S *pṛtha*; Ht *paltana*; Gk *palamē*; L *palma*.

13. reward : S *mīḍhạ* ; Av *mīžda*; Gk *misthos;* Gmc *mizdō;* Sl *mĭzda.*

14. shoulder : S *aṃsa*; Gk *ōmōs*; L *ume-rus*; Gṃc *ams*; Arm *us.*

15. tooth, molar : S *jambha* ; Gk *gomphos* ; (B *žaṁbas* 'sharp edge';) Sl *zŏbŭ* ; 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 questionmark after questionmark, but at some point we must stop this easy way of escaping and get down to facts and sound reasoning. The answer is – there is no ostensible reason whatever.

(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
veda	*vaēδa*	–	*Foida/e*	*vīdi(t)*	*wait*	*vaīdi-*	*vĕdŭ*

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 S *vidām as-/kṛ-/bhū-* 'to have known'.

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

S	Ht	Gk	L	OE
*ad-**ant**-*	*ada-**anz**-a*	*ed-**ont**-*	ed-**ent**-	*et-**end**-e*

iii) Reduplication of stem in the present active 'to stand', 3rd sing

S	Ht	Gk	L
*ti̱**s**tha**ti***	*tiya(-az-)**zi***	***hi**stē**si***	***si**stit*

The Latin *sistere* is of course a causative 'make stand'. Reduplication is found also in the formation of the Perfect for many verbs as in S *ba-bhū-* , Gk *pe-phu-* 'become/grow'; L *cu-curri* 'run'; Gth *staistaut* 'struck'; S *dadhā-*, Gk *tethē-*, Gmc *deda-/teta-* 'have put'; etc.

iv) Consider the endings for, say, 1st and 2nd Indic Pres of 'carry'.

S	Gk	L	Gth	Sl
*bharā**mas***	*phero**men***	*feri**mus***	*baira**m***	*bere**mŭ***
*bhara**ta***	*phere**te***	*fer**tis***	*bairi**þ***	*bere**te***

v) Declension of some nouns (fem and masc) in -is in nom and acc sing and loc pl.

	S	Ht	Gk	L	Sl
	thought	army	seer	sharp (fem)	guest
Sing Nom	*mat-is*	*tuuzz-is*	*mant-is*	*ācris*	*gostī*
Acc	*mati**m***	*tuuzzi**n***	*manti**n***	*ācre**m***	»
Pl Loc	*mati**şu***	*tuuzzi-aš* (Dat)	*mante**si*** (Dat)	*ācri-bus* (Dat)	*gosti-**xū***

vi) Consider finally the word for 'name' in sing NVA and Loc and Pl NVA.

	S	L	Gth	Sl
Sing NVA	*nāma*	*nōmen*	*namō*	*imę*
Loc	*nām(a)**ni***	*nōmi**ne***	*nam**in***	*ime**ne***
Pl NVA	*nāmā(**ni**)*	*nōmi**na***	*nam**na***	*ime**na***

Some of the above examples are taken from Misra (1968), Held (1907) and Szemerényi (1996). Here, all these similarities (and there are many more, as in Marcantonio's Introduction) 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 languages do not have these elements.

   (e) Some of the features in (b), (c) and (d) 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" (Beveridge 1968; for fuller discussion, see §12 below). 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 §§7-8.

   Then, statistics is a purely quantitative factor that has no bearing in quality or significance–matters which require different criteria[3]. Statistics and numbers here must give way to more important considerations like the improbability of so many

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[3]   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. Or take intelligence. Very few people are highly intelligent and wise. A grade lower there are many more intelligent people and even more of average intelligence, while at a yet lower grade, those below-average are far more numerous. The highly intelligent ones are statistically insignificant but it is from them that we would choose to send as representatives of our human race to another inhabited solar system. In fact, reasonably used, statistics can lie as easily as any vagabond.

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.

5. 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-aɣni-*. 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 *ᴰSius*, Gk *Zeus/Dia-*, L *Ju[s]-piter*, Gmc *Tīwaz* and South Russian *Divŭ*. 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* (Kazanas 2006b). 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 (Walkins 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 2001b.)

(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*, Acheans 'of fine greaves' *euknemis*; V *somapā* 'somadrinker' could be used of any god but is applied only to Indra (2.12.13; etc), *jalāṣa* '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 *gṛhapati* 'lord of house' for Agni (1.45.1), *vajrin* 'he of the bolt' for Indra (7.49.1), *urugāya* '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 both strict metre like Anuṣṭubh (4 lines of 8 syllables) or *Gāyatrī* (3 lines of 8 syllables) and rich alliteration. In Greek and Latin poetry we find different metrical units (iambic hexameter, anapaest etc) while in Germanic we find alliterative lines with loose metre.

All these features, except for some recurring epithets, are not found in 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 Saraṇyū 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 *Saraṇyū*?) 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 (see Kazanas 2005a: §4).

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 2004: 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 Bellerofon 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, 2001b, 2004b and 2005a, but the four examples which appear with divergences in the east and in the west, but not in intermediate Near East, suggest a common origin.

6. The aspects examined in §§ 3-5 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 principal" 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.

7. 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 is all wishful thinking. I have argued elsewhere (Kazanas 2002, 2003, 2004a) 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). 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.[34]

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 may be 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 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. Angela Marcantonio (2005) criticizes many aspects of these so-called "laws"[45.] The changes in Vedic, Greek, Germanic and other languages differ enormously among themselves: eg the various forms of 'be/become' as in S √*bhū* (>*bhava*-), Gk *phuomai*, L *fui*, C *buith*, Gmc *be*- etc. How or why did the original initial consonant – whatever it was – change into these sounds? Linguists don't know. The changes occurred in particular, if not precisely determined places, periods and 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 to enter the realm of the absurd.

Consider a different example. In Greece, in the late 1970's an Act of Parliament imposed reforms in the spelling, accent and accidence 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,

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[4]   This may sound insulting but I know of no shorter or better expression that describes such a deep and wide inconsistency between thought and action.

[5] ..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" 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.

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. Reconstructions are futile, wasteful exercises.

Another obvious absurdity 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(and here absurdity reaches its peak), the labio-velar family, is unpronounceable! How on earth can anyone pronounce as one unitary sound *k*^w or *g*^w*h* which contain both a guttural and a labial element? One simply can't, however neat it looks on paper. I challenge anyone of these bright comparativists to give a full description of the pronunciation of these sounds. Also to explain how to pronounce the monstrosity **dhg*^w*hec* 'to perish'! We find thousands of pages of discussion about these sounds and their supposed existence in imaginary proto-languages but not one paragraph on their actual pronunciation!

8. A most interesting aspect of Indoeuropean Linguistics is the root and the vowel gradation or *ablaut* system. 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.

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 ĭ always changes to *e* and *ai*, never to *a* or *u/o*. Similarly radical *u* → *o* → *au* and *ṛ* → *ār* → *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, ḷ* (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.* 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).

However, comparitivists have 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 the sheer audacity of this presentation, which merely increases the disorder. Here we have more bizarre phenomena. le*ip-* 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 dipthong *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[6].

But what principles govern these so-called vowel-grades in Greek? No principle at all is given by anybody! 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.

9. 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 §10g, 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. §5). 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 (Kazanas 2002, 2003, 2004, 2005b, 2006a) 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[7].

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[6] Here too we have examples showing apparently great confusion as noted in §7.

[7] In a private communication, Nov 2004, Mallory mentioned the Jews who wandered

10. 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; Frawley 1994 and with Rajaram 1997; Feuerstein et al 1995; and others[8]. In fact more and more scholars in the West have re-examined the issue and rejected the mainstream view advocating a movement Out of India into Europe: Schildman 1994, 1998; Friedrich 2003, 2004. 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 spiteful slogans or unthinking 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. Roughly the main evidence and arguments are as follows:

a) 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; Possehl 2002; McIntosh 2002).

The early Vedic texts mention no migration at all (see n7 and n9). Both these facts are quite different with other branches. There is ample evidence of entry in Greece, Italy and the rest of Europe and Anatolia. Moreover, most of these branches have texts that mention a migration – Greek, Roman, Irish, Scandinavian and Iranian (Kazanas 2002, 2003). In the Vedic texts we read also that the IAs take it for granted that they have been "here" for a long time without any hint that they have recently arrived "here" from elsewhere (e.g. the Aṅgiras family in *RV* 4.1.3 or the Vasiṣṭhas in 7.76.4). Then the river Sarasvati is said to be the best river, to be swollen by other rivers and to "flow pure from the mountains to the ocean", a fact which could be objective truth only before 3200 BC (Kazanas 2003, 2004b).

b) Frawley first pointed out, some ten years ago, that here in Saptasindhu in the ISC, we have an extraordinary paradox. On the one hand there is in the third millennium a technologically advanced civilization with large buildings, arts and crafts, trade that reached Mesopotamia and other distant regions, and literacy; yet, unlike Egypt and Mesopotamia, it has no literature at all! Then, even as this culture begins to collapse and the inhabitants move eastward, enter as per the AIT the IAs who

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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.

[8]   S. Talageri should perhaps be included but despite having some very good ideas, this author knows no Sanskrit and has no training in Archaeology or other related disciplines and so goes astray constantly.

are non-literate barbarians with highly developed poetics (Watkins 2001), and very swiftly produce the hymns of the *RV* and *AV*. It is "an astonishing coincidence of space, time and people" (Kazanas 2002:292). But we have more paradoxes.

c) The *RV* hymns, which by general agreement among AIT adherents are the earliest productions of the IAs, do not reflect any of the ISC features. If, as the Allchins (1997:221ff) and others suggest, the IAs acquired the "material culture and lifestyle" of the Harappans before their entry into Saptasindhu, then the *RV* ought to reflect some of the more pronounced Harappan elements (urbanization or ruins, bricks, fixed fire-hearths/altars, cotton, silver, etc). But the *RV* knows nothing of such things. Paradoxically, again for the AIT mentation, it is the later Vedic texts, mainly the Brāhmaṇas (not the *RV*), that, as the Allchins state in another study (1982:203), reflect all these features. It follows that the *RV* is pre-Harappan and later literature is contemporaneous with the mature ISC or later. (For a full discussion of this paradox – Kazanas 2006b.)

d) Now, most vedicists think that the Rigvedic word *pur* means 'citadel, fort' and designates the ISC towns! There are two insuperable difficulties with this shallow interpretation. First, the Harappans used bricks for their buildings and town-walls. In Vedic 'brick' is denoted by *iṣṭakā*, abundantly present in the later Vedic texts; but *iṣṭakā* is not present in the *RV*. In the hymns we frequently find *purs* made of metal (e.g. 4.27.1 *āyasi*) and, less frequently, made of stone (e.g. 4 30 20 *aśman máyin*) but nowhere *iṣṭakāmáyin*. Then we learn of an extraordinary *cariṣṇu* 'mobile, wandering' pur belonging to a demon of drought! And this brings us to the second barrier. Not one of these purs is constructed or destroyed by humans. Purs are, in fact, occult, supernatural defences. (For details see Kazanas 2002b; 2003:224 and 2005c.)

e) Yet another paradox is the Aryanization of Saptasindhu. In the early 1990's the AIT adherents stopped writing about "invasion" and introduced the term "immigration", and in peaceful wave(s), at that. But how could 2 or 10 peaceful waves of illiterate nomads (albeit with great poetic talents that had not yet flourished[9]) accomplish the "almost complete Indo-Aryanization in northern India"? The quotation is from M. Witzel (1995:107). In the same passage Witzel, this most fervent and vociferous proponent of the AIT, expresses his bafflement at this: "The Indo-Aryan influence... was powerful enough from early on to replace local names in spite of the well-known conservatism of river-names. This is especially surprising in the area once occupied by the Indus Civilization, where one would have expected the survival of earlier names". Such a change does not come about through peaceful means: only conquest can impose it and often not at all thoroughly. Consequently the claim for "immigration" is just as unrealistic and inapt as "invasion": both are contradicted by reason and the facts.

f) B. N. Narahari Achar published three papers on Archaeoastronomy showing that various astronomical references in Indic texts, *Śatapaṭha Brāhmaṇa* 2.1.2 2-3, *Jyotiṣa Vedāṅga* and the epic *Mahābhārata*, are much earlier than mainstream indologists thought them. By generating on the screen of a computer monitor the sky formation above any given location (North India in our case) as far back as 8000 BC, he was able to pinpoint the star-references in the texts. The *Brāhmaṇa* text was thus dated c3000, a date close to S. Kak's calculation of 2950 (1994: 35). The *Jyotiṣa Vedāṅga* was dated c1800. Finally, the references in the epic (Books 3, 5 and 13) all converge exactly in the year

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[9] Now this was so, as per M. Witzel (2003). If it was not so and the incoming IAs had already composed hymns and whatever else, then, surely, some passages would reflect their recent journey(s), the (mis-)adventures they went through and the various people(s) they met on the way. But there is not a hint of such things in the *RV*! (See n6.)

3067, a date calculated also by S. Raghavan (1969): this suggests that the core of the epic began at about that time, three generations after the civil war among the Bhāratas which the native tradition placed at 3137 BCE. (For full discussion with references see Kazanas 2002, §V, 1-3.)

g) Recent genetic studies using more secure methods agree now that there was no Aryan immigration/invasion into India. K. Elst (1999) cited two geneticists (Hamphill and Christensen 1994) who had refuted the view that the IAs moved from Bactria into NW India: "[A.] Parpola's suggestion of movement of Proto-R̥gVedic Aryan speakers into the Indus Valley by 1800 is not supported by our data. Gene flow from Bactria occurs much later and does not impact Indus Valley gene pools until the dawn of the Christian Era" (Elst 1999: 232; also Bryant p 231). K. Kennedy (1995) confirmed this view: "There is no evidence of demographic disruption in the northwestern sector of [India]... during and immediately after the decline of the Harappan culture" (again in Elst, p233, and Bryant p231). The Harappan decline is placed c1900-1600. However, Kennedy found a break in the continuity of the skeletal record in the region c6000-4500 – which could indicate some unspecified entry.

More recent genetic studies show that this break is not indicative of an influx of new people, the IAs, and even suggest that, on the contrary, the European peoples descended from the inhabitants of SE Asia. Cavalli-Sforza and his team state that "Indian tribal and caste populations derive largely from the same genetic heritage of Pleistocene [=10000 to 3 mya] southern and western Asians and *have received limited gene flow from external regions since the Holocene* [=c 10000 to present]. The 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). He adds that this M17 marker travelled from India or Pakistan (= our Saptasindhu?) through Kashmir, Central Asia, Russia and then Europe after 50000 BP. Thus migration is from east westward.[10]

Enough has been said on this. Here I have outlined the main arguments in the form of paradoxes based on facts and reason; but, of course there are many more arguments and evidences. Before we turn to the AIT arguments themselves there is one more point to examine.

11. In Indology and IE studies the mainstream orthodoxy, despite the preceding evidence, will not allow the case for Indoaryan Indigenism and a *RV* composed in the 4th millennium to appear in any form in the media it commands – i.e. just about all Western Journals (and some University Journals in India). E. Bryant brought out his non-committal book *The Quest for the Origins of Vedic Culture* in 2001 and J. Mallory,

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[10]   In 2002 when the *JIES* debate took place, I was right to doubt the insecure finds of other researchers which were methodologically wrong. See Kazanas 2002, §IV, 1 and n 9, p. 287-8 and 2003, §14, p. 200). I am indebted here to Dr N. Rajaram of Bangalor (India) and Prof. S. Kak of Louisiana who sent me the information about these two 2003 publications.

editor of *The Journal of Indo-European Studies*, agreed in 2002 to a "debate" where I was given both the first and last word (much to his credit). I had the year before sent to him my 'Indo-European deities and the Ṛgveda', which had been rejected by two journals because it had two pages of arguments for Indigenism and a *RV* composition of 3000+ BC: these had to be removed if the paper was to be published. Since Mallory commented that this issue required a full paper which he would be prepared to consider as a basis for debate, I complied and the castrated paper was published (*JIES* 29:257-293). At about the same time two more papers dealing with parallels between Archaic Greece and the Veda but containing clear statements about Indigenism and a *RV* of 3000+ were rejected for the same reasons by several Journals including two classicist ones.

The theory informing this mentality is nakedly stated by Witzel: "It is certain that Kazanas, now that he is published in *JIES*, will be quoted endlessly by Indian fundamentalists and nationalists as 'a respected scholar published in major peer-reviewed journals like *JIES*' – no matter how absurd his claims are known to be by specialist readers of those Journals" (2003, p23, §5 end). And now, the more practical application of the theory.

After the debate I sent three new papers to four Journals – one American and three European. The papers were rejected. One Journal has not replied to this day! Two replied but gave no reasons for the rejection and one replied and gave reasons. It is worth giving details. The last one was the *JIES* and the paper was 'Rigvedic *Pur*' (now published in India). I made all corrections according to the referee's indications and sent it back for re-consideration because I thought the evidence I had gathered (§10, d)would interest not only vedicists but also indoeuropeanists. The paper was rejected again: the referee found new faults! The funny thing was that I had left – unwittingly – an error in the text but the referee missed it both times. However, he/she took exception to my sharp criticism of W. O'Flaherty who displayed considerable confusion following Geldner's translation of *RV* 2.35.6 and his introduction of "unbaked bricks" which occurs nowhere in the text! (However note that, another referee from the same Journal was delighted that I had accused O'Flaherty of confusion in my 2001 paper 'IE deities and the *RV*': see Kazanas 2001: 283, n 14.) Furthermore, these scholars, editor and referee (and others on the Board), did not mind at all Witzel's scurrilous paper in *JIES* (2003) which reeked with invective and vituperation. My own paper 'Indigenous Indo-Aryans and the Ṛgveda' had been subjected to thorough scrutiny and I had to make several corrections. What is of great interest, however, is a remark by one of the referees. He/She wrote: "Maybe the age of the first arrival of Indo-Europeans (or of the beginning of a cultural influence of Indo-European) in India should be pushed further back: thus important archaeoastronomical dates mentioned in the article may be relevant for the definition of this time" (in Kazanas 2003: §6 end). What is important is the referee's viewpoint and his/her interpretation of the facts: by all means, push the date further back by 1500 years so long as we keep the IE entry into India! But how my learned sir/madam do you know for certain that the IE influence was west-eastward? Why don't you keep a window open in case the reverse is true?[11]

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[11] The same referees thought that my dismissal of the "laryngeal theory" was superficial since Kyrilowitz (and others) had "established" it. There are, of course, many linguists since the days of Kyrilowitz, devoted comparativists, who don't care at all for the "laryngeal theory" and consider it very troublesome (Szemerényi 1996: 138; Sihler 1995: 111; Fox 1995: 162, 181). For the "fraud" involved in reconstructions and the facilitation of matching variants with laryngeals see Brady and Marcantonio 2003. But as I wanted to have the paper published, I thought my retreat on this issue would be a very small price to pay. Nonetheless, one gets a good, clear picture of the nature of these "peers" in this particular realm and cannot but recall

Peer-reviews in such controversial cases would be truly valuable only if the "peers" condescended to put their names so that they could be checked and critiqued if they made errors or missed errors. Otherwise the system is a device for perpetuating mainstream theory and superciliousness.

12. Many of the scholars in our field write with aplomb of "science" and the "scientific" method (e.g. Huld, Mallory 2002; Witzel 2003; Marcantonio 2002, 2005) thinking perhaps that there is something magical, absolute or inerrant about this, and that they themselves operate as infallible "scientists". It is quite a widespread tendency even among ordinary folk. Obviously, such people don't realize that there is nothing extraordinary about the "scientific" method and that scientists, despite highly sophisticated and expensive instruments in many cases, are just as liable to error and paranoic notions as anyone else (Cohen 2001, Pert 1997, Kuhn 1970, Beveridge 1968).[12] The much used and abused term "scientific" ought to refer to a method that entails passionate, sustained interest in its object, sharp, unwavering observation of the data in natural or man-made conditions and clear, impartial reasoning to arrive at correct conclusion(s) (Beveridge 1968). When the aura with which goggle-eyed laymen have invested "science" is removed, the "scientific" method is seen to be no more and no other than the method of sound reason used in every human endeavour that hopes for success – and this includes research in the Humanities as well. The experts in the Sciences no less than the Humanities are often aided in their investigations and discoveries by inspiration, luck and similar factors, that are outside one's control (Beveridge 1968:27). The historian and philosopher of science Th. Kuhn angered many when in the early 1960s he pointed out with piles of evidence that even blue-blooded scientists in the mainstream ignore or resist "anomalies" and act so as to preserve the model/paradigm/theory within which they operate (1970; also Cohen 2001). Unless this is recognized, research will not really advance.

In Indology and IE studies we have the same situation. One can't help wondering what all these mainstreamers will do when they finally realize that the new, secure genetic studies not only confirm the finds of ISC archaeologists in the last quarter of the 20th century but, moreover, show that the exodus westward to Europe came from India!

13. Since the mid-1990's there arose in the USA a loose movement called Intelligent Design composed of academics (mainly) most of whom specialize in the biological sciences – but with some mathematicians, lawyers, philosophers, physicists and theologians . Being declared Christians (of all denominations), they set out to refute mainstream orthodoxy consisting in (Neo-)Darwinian evolution and materialism and to offer an alternative scientific paradigm, i.e. Intelligent Design, which does not require or include reference to God[13]. Many publications have appeared, starting oddly enough with lawyer Ph. Johnson's *Darwin on Trial* (1991, several reprints). There were counter-attacks from the mainstream camp who mistakenly branded these scholars

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Kuhn's words about mainstreamers wanting to protect and perpetuate the model within which they themselves operate (1970).

Thereafter I decided I would not send any articles to any publications in the West (particularly so-called 'peer-reviewed' Journals) except I am asked specifically: there are plenty of prestigious Journals in India only too happy to publish my papers.

[12] The subject was mentioned earlier in §4, d.

[13] To my mind, this is not exactly a 'paradigm' but rather a basis for a new scientific viewpoint and approach.

'creationists', and the tug-of-war continues vigorously. Here again, as in Indology and IE studies, there is an increasing number of rebels ("revisionists" Witzel would call them, or perhaps "fundamentalists") who challenge and reject vociferously one of the holiest cows of the sciences – evolution! But here there is a marked difference. For some years now rebels and mainstreamers mingle together in conferences exchanging views, and produce publications under joint editorship with equal papers from all sides (there are others rebel groups beside Intelligent Design). See for instance W. Dembski and M. Ruse (eds) *Debating Design* (2004)[14].

Perhaps a similar development will take place in our own field. But there are difficulties. In the biological sciences investigators deal primarily with facts – entities and processes observable in living organisms and also structures frozen in fossils. There one can't claim that our cells have no DNA or that a shark is an insect. In our field, there is a prodigious prejudice despite invocations of reason and Occam's razor! Then, the mainstreamers (including some archaeologists like M. Gimbutas, C. Renfrew and Mallory) ignore what archaeologist and anthropologist experts in the ISC area have been saying since the early 1980's,that there is unbroken continuity in the culture of Mehrgarh and the ISC down to 600 (Shaffer & Lichtenstein 1999, preeminently) and even to several aspects of modern Hindu culture (Lal 2002). They pefer to deal with k^wek^wlo, *$gheu$, *$zdema$,*$nizda$, *$sizda$, *sel (all from Huld 2002, Witzel 2003) and similar conjectures that exist in no known language but only in the imagination of professional indoeuropeanists. Why people dealing with such utterly imaginary entities demand to be regarded as "scientists" and accuse others (who don't accept that such conjectures are worth discussing) of being "unscientific" is something of a mystery that can be explained only as a natural feature of the modern Tower of Babel [15](Kazanas 2004, §35).

Let us now turn to the conventional arguments employed by the AIT proponents over the years and usefully summarized by Bryant. I have given the evidences for indigenism in §10a-g. As far as I know and despite claims by mainstreamers that they have disposed of them, not one of them has been met except by beating about the bush and prevarication. But, as Darwin wrote in the *Origin of Species*, "A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of each question" (1859:2). So let us now see the other side.

Let us be clear that although I cite Bryant, the AIT arguments are not his own: Bryant merely quotes them and sums them up in his final chapter.

14. The Mitanni evidence. Bryant writes: " The date of the Mitanni kingdom helps secure the date of the composition of the Ṛgveda in India to around the middle of the second millennium BCE" (p 299). I repeat that this is not what Bryant himself believes but only what the AIT adherents say. But in what way does the Mitanni evidence help secure, as the AIT holds, the date of the composition of the *RV* to c1500 or 1200? In no way whatever.

The Mitannis established a kingdom in Syria (north Mesopotamia) late in the 16th century reaching its peak of power in the 15th century[16] (Dunstan 1998: 155; Roux 1992: 255). From the Mitannis has survived a treaty mentioning gods *Indara*, *Mitrasil*,

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[14]   Indeed, see this volume (and others like it) and compare it with the so-called debate in the JIES 2002-3 where N. Kazanas alone had to joust with nine opponents! Initially the agreement was that there would be five and five (i.e. five from India) but the Editors did not keep it. When I complained, they replied that I myself could get the information from Indian scholars! (Yes, I have all the documents for anyone interested!)

[15] Yet another example of the prevalent confusion.

[16] Through oversight no doubt Bryant gives the 14th century (p 135).

Nasatyanna and *Uruvanassil* (= Vedic Indra, Mitra, Nāsyatyas/ Aśvins and Varuṇa); also a guide on horse training and upkeep wherein some words and technical terms are recognizably of IA provenance, not Iranian nor the conjectual Indo-Iranian. Bryant does not mention here the names of Mitanni kings *Sutarna, Paršasatar, Artadāma* etc which are also of IA descent ' Sutaraṇa, Praśāstar, Ṛtadhāman' (Burrow 1973: 27). So the Mitannis were of IA origin.

The argument here is that "these Aryans were a segment of the Indo-Aryans (after the split with the Iranians) somewhere in north Iran or central Asia.… who were migrating east towards India [and] leaving the larger body, they sought their fortunes in the Near East, where, although successful, they eventually became subsumed by the local population" (Bryant, 136).

This of course is no evidence for the composition-date of the *RV* and is no argument at all. It is based on three assumptions: **a)** there was a migrating group moving towards India from the North-East (Urals or wherever); **b)** there was an Indo-Iranian body that split, the Iranian staying in Persia and the Indoaryans moving to Saptasindhu; **c)** the *RV* was composed at about this period, c1500, or afterwards. None of these assumptions can be demonstrated but rest on other assumptions and conjectures. The chief is that a large body of Aryans moved south and eastwards to become the Iranians and Indoaryans. That Middle Indoaryan, i.e.the various Prākrita dialects, with which the Mitanni linguistic elements have close affinities, was contemporaneous with the *RV*, proves absolutely nothing, since all could go back to the 4th millennium.

It is more reasonable to assume that the Mitannis left Saptasindhu at a date after (the composition of much or some of) the *RV* and after much wandering, during which they lost most of their Indoaryan culture, eventually settled in the Near East – as the Kassites had done before them. After all, there is no trace of Iranian in their linguistic elements, and archaeologists and historians of that region assure us that there is no trace of any cultural elements in the Mitanni area that comes from eastern Europe, the Caucasus or Central Asia (Bryant, 137, following Brentjes 1981). In relation to the last point, it must be noted that repeatedly in this debate the archaeological evidence is disregarded in favour of linguistic conjectures. Fortunately, in this instance linguistic and archaeological evidences are not at odds. Then, we have the Kassites.

15. The Kassites established themselves in Babylon, middle and north Mesopotamia, in the last half of the 18th century, i.e. about 200 years before the Mitannis. Bryant does not mention them but they are mentioned by other indoeuropeanist linguists (e.g. Burrow 1973: 28ff). They are thought to have come from the Zagros foothills in Persia (Dunstan, p 239) but certain linguistic elements they left indicate, as with the Mitannis, an Indo-Aryan origin, not Iranian. These elements are names of gods: *Šuryaš* rendered as *Šamaš* in Babylonian (= the name of the sungod) which is the Vedic *sūrya* 'sungod', and *Maruttaš*, the wargod (rendered in Babylonian as *Enurta*, known also as *Ninurta*), the name being cognate with Vedic *Marut-as* 'storm-gods'. A king's name *Abirattaš* is obviously Indoaryan *abhi-rathas* 'facing chariots (in battle)', while the name *karaindaš* contains *-inda* which is the Vedic *Indra*, and *Nazī-Bugaš* where the second element is Vedic *Bhaga* (cf also names *Nazi-Maruttaš* and *Sagarakti-Šuriaš*, all in Oppenheim 1977: 338). Then there is the number *aika* 'one' corresponding to Vedic *eka*[17].

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[17] Iranian origins are excluded because the Avesta has no gods Maruts and Sūrya; it has only the stem *hvar* (=V *svar-*). Indra in the Avesta is a demon and it is not likely that a king would take on a demon's name. In Avestan the number 'one' is *aiva*, not *eka* (>*aika*).

The Kassite evidence is not as rich as the Mitanni, but it is sufficient to indicate, if we follow the whole AIT argument about the Mitannis, that the *RV* composition-date is c 1750-1700. But here again it is much simpler to say the the Kassites left Saptasindhu after the *RV* was composed and appeared in Mesopotamia c 1750-1700. They too wandered considerably and lost much of their inherited culture before settling in their historical seat.

16. Iron. In presenting the AIT argument on the composition-date of the *RV* based on the Mitanni evidence, Bryant adds that this "is further solidified by the references to iron in slightly later Vedic texts" (p 299). This is just as airy-fairy as the Mitanni evidence.

The "references to iron in slightly later Vedic texts" are found, in fact, in *AV* IX, 5, 4 and XI, 3, 7, then the *Taittirīya* and other Saṃhitās and the *Chāndogya Upaniṣad*. In the later texts we find *kārṣaṇāyas* and *kṛṣṇāyas* both 'black metal'. The earlier *AV* references have the term *śyāma* 'swarthy' (+*ayas* 'metal').

Now, iron metallurgy in India surfaces in the archaeological record in the 13th century. Therefore, the AIT argument runs, these texts cannot be older than 1300. True. But here the AIT again uses several assumptions and the argument finally is circuitous. The general AIT, going back to the mid-nineteenth century, holds that the IAs entered Saptasindhu c 1500 and the *RV* was composed c 1200-1100.The *AV* and other texts mention a *śyāma ayas*, which is rendered as 'iron' (=black, swarthy metal). Since iron-smelting is not archaeologically attested before 1300 (or at most 1500: Bryant, 247) these texts cannot possibly be earlier. Q.E.D.

The main loophole here (and there is another one) is the interpretation of *śyāma* or *kṛṣṇa-/kārṣa* as 'iron'. Why take this "black metal" to be iron?

The *RV* was translated into modern European languages and interpreted under the light of the AIT. Subsequently, these interpretations were used to prove the AIT! One such interpretation is that *pur* means 'fort,town' and that the invading Aryans destroyed the forts/towns of the natives of the ISC; but of course a close philological study of the word *pur* reveals that this means a magical or super-natural defence, not a material citadel (see §10, d)! Then we read constantly about cattle-rustling in the *RV* and that the Indoaryans were "nomad pastoralists". But in a philological study of the word *gaviṣṭi* S. A. Dange demonstrated a long time ago that this word **does not** indicate any cattle-rustling (1967); then, a careful reading of the *RV* hymns reveals that the rigvedic people were on the whole settled agriculturalists (Kazanas 2003a: 197, §11). And so on.

17. The Vedic words *śyāma* and *kṛṣṇa-/kārṣṇa* need not at all refer to iron. A fact unknown probably to most indologists and archaeologists is that there can be **black copper**! Copper (=*ayas*, by general agreement) and its metallurgy are well attested in the *RV* (IV,2,17; V,9,5; IX,112,2; etc). **Iron is not mentioned in the *RV* since there is no other word for metal;** some have taken *asi* to mean 'iron sword' on the basis of Latin *ensis* (see Bryant, 338, n 5 for ch 12), but V *asi* means 'knife' and no more. Others take *ayas* as 'iron' (Misra, 1992 66-8, whose arguments here are totally unrealistic and unconvincing) but in this case the *RV* would be left without a word for copper!

To harden copper, the metal is heated up (but *below melting point*), then left to cool down without use of water: this turns the copper **black** - not with soot that can be wiped off but substantially so; this effect can be produced also by oxidation with various sulfides (Hughes & Row 1982: 92, 187). And I am not referring to an alloy like bronze. Thus AV XI,3,7 speaks of flesh *māṃsa* being *śyāma* and blood *lohita* being red-metal *lohita*. This compressed analogy is quite apt since flesh is produced from and maintained by blood and the 'red metal' (=copper) becomes 'black' (i.e. black hardened

copper [18]).

However, there is another side to this issue which shows how defective and shaky is this AIT argument about iron. Let us assume that the post-rigvedic *śyāma and kṛṣṇa-/ kārṣṇa* refer to iron. Bear in mind that the references are not to smelting or other processing of iron; so there is no question about iron technology which is not attested, as was said, before at best 1500. But iron objects were found at Harappan sites in Afghanistan and Baluchistan from 2600 (Possehl & Gullapalli 1999: 159-161). In Egypt, we might add, meteorite iron is known from before 3000. So there is nothing remarkable for post-rigvedic texts to mention iron and such references do not require the iron metallurgy of the 13th century. This aspect is acknowledged by Bryant himself (p 247). Yet it is mostly ignored, even by Bryant (2002: see n4), and scholars go on writing about 'black iron' as an element for decisive dating.

Thus, whichever way we take this "black metal", it does not prove that the texts mentioning it must necessarily be assigned to the 15th century. They could well be and, as other evidence indicates, they probably are from c2800-2200.

18. Finno-Ugric evidence. "From wherever might have been the original matrix, the Indo-Aryans spent a period to the north of the Caspian Sea, adjacent to the Finno-Ugric speakers (either as a distinct group or while still part of the undivided Indo-Iranians)" so Bryant sums up this particular argument for the AIT. The data upon which it is based are the loan words from Vedic in the Finno-Ugric languages or dialects.

Indeed, there are many worlds borrowed by the FU family. First is given the word in S(anskrit) then the word(s) in the Finno-Ugric. Thus: S *śata*, Fin(nish) *sata*, Lapp *cuotte*, Mord(win) *'śado* – all '100'; S *asura*, Mord *azor-o*, Vot(yak) *uzïr* – all 'lord'; S *ūdhar*, Fin *utar*, Mord *odar*, Çer(emis) *vodar* – all 'udder'; S *aṣṭrā* , Hung(arian) *ostor*, Čer *woštyr* – all 'whip'; S *svasar*, Fin *sisar*, Mord *sazor*, Čer *šužar* – all 'sister'; S *surā*, Hung *sör*, Vot *sur* – all 'strong drink, beer'; S *hīraṇya*, Hung *arany*, Mord *sirńe*, Vot *zarni* – all gold; S *vajra* (=Indra's weapon), Fin *vasara* 'hammer' and so Lapp *voečer*, Mord *vizir*; S *chāga*, Mo rd *śeja* – both 'goat'. More words could be added, but enough have been cited to show the extent of borrowing by the Finno-Ugric family.

As often, there is much controversy whether the originals are IA, Iranian or the conjectural Indo-Iranian (Bryant, 126-8; also Burrow 1973: 24-5). Some words like S *ūdhar* and *chāga* are not found in Avestan, so these cannot be said to come from attested Iranian. The Indo-Iranian is wholly unattested, a concoction of modern linguists, so it can be ignored. But, in any case our concern is that the IAs passed, according to the AIT, from an area close to the FU-speakers on their way to India. This is what matters. So the AIT gives again a splendid circular argument: – The IAs passed from the specific location on their way to India, hence the FU have loans; there are FU loans from the IAs, therefore the IAs passed from the specific location on their way to India. QED.

However, it is fairly usual that where there is giving there is also taking but, as Misra, R.P. Das and others cogently pointed out, there do not seem to be any certain loan-words in Sanskrit from FU; so the IAs did not stop at that location on their way to

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[18] I presented this interpretation in an earlier paper (Kazanas 2002b: 295). M. Witzel commented (2003) on this referring to bronze: this was irrelevant, since I did not at all refer to bronze. Bryant too commented (2002) calling it "speculation". But, of course my "speculation" is far more probable than the speculative interpretations of "black iron" made under the AIT; mine is not at all speculation in that it accommodates all facts except the AIT, which is not a fact.

India; consequently the movement of IAs was the reverse, i.e. from east to west (1992: ch2)[19]. Similarly Joanna Nichols argues for an east-west movement to explain loans both in FU and in the southern Caucasian languages (1997-8). And Bryant concludes: "Ultimately, there is little in the history of loanwords that can eliminate a variety of historical possibilities" (p 129).

So we can put aside the Finno-Ugric loans and state that they do not constitute any evidence for an IA west-to-east movement. This conclusion is supported by additional data.

19. Mythological borrowings, not in Bryant. IA mythological elements are evident also in the Finnish epic *Kalevala*, first compiled (1849) by E. Lönurot from runes and songs found even as far as Archangel and Olonetz, now in north Russia. It is understandable that linguists do not deal with this type of evidence since it does not fall within their own circumscribed field, but they should not ignore it altogether as if it does not exist. Correspondences of mythological themes, motifs or images are not all that different from lexical correspondences: as the same words common in two different languages (e.g. S *śata*, Lapp *cuotte* '100') are almost never identical but have linguistic affinities, so mythological motifs are never exactly identical but have common features. Thus while much of the *Kalevala* mythology is non-IE and some themes are similar to and may derive from Norse myths, there are some motifs and images that can be connected only with the early Vedic lore.

20. R. B. Onians noted many years ago the motif of the ***Cosmic Egg*** appearing in the *Kalevala*, in Greek Orphism, in Egyptian texts and in the Veda (1989/1951: 177). In the Egyptian texts, the sungod Re, or air-and-light-god Shu, breaks out of the cosmic Egg and manifest creation begins (Kazanas forthcoming: §114 with full references). In the Orphic tradition, the Egg breaks open and out of it emerges a light-deity, Phanes, with whom begins the visible creation. The Egyptian Egg first appears in the *Pyramid Texts*, dated c 2450. The Greek Egg is much younger, of the fifth century, so this may be a borrowing. But since the name *Orpheus* is undoubtedly cognate with Vedic *r̥bhu* and Germanic *Elf* (Mayrhofer 1956-; Kazanas 2001) and the Greek-Vedic mythologems have other affinities (Estell 1999) it seems reasonable to take it all as PIE (=Proto-Indo-European), though the Cosmic Egg does not appear in other IE branches.

In *Kalevala* the virgin daughter of the Air, Ilmatar, floats on the waters and there Ukko, a kind of Finnish Zeus, sends his bird, which lays upon Ilmatar's lap one egg in one version and seven in another, out of which come different aspects of creation. This has affinities with the Greek myth of Leda (wife of king Tyndareus of Sparta) who bore to Zeus (in the form of a swan) two eggs.[20]

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[19] Bryant refers to two scholars who think there may have been some few Sanskrit borrowings from Finno-Ugric (p 127) but these are very uncertain. Burrow too (1973: 26) finds it possible that some few words like S *chāga* may be loans from Finno-Ugric, since they have no cognates in other IE languages. But this is no sure criterion and even Burrow mentions only a "possibility". Certainly, if the IAs had stayed for long near the Finno-Ugrians, one would expect nearly as many loans from them and not just the 5 or 6 dubious loan-words mentioned by Burrow and others, which may have only fortuitous similarity, or could be FU loans from Indoaryan.

[20] In the Greek (late) mythologem Zeus comes to queen Leda as a swan and impregnates her. She gives birth to two eggs out of which issue two pairs of twins: Castor and Pollux (= Vedic 2 *Aśvins*) and Helen (= Vedic *Saraṇyū*) and Clytaimneastra. Although Egypt as a source cannot be wholly eliminated, the strands are palpably IE. The Finnish myth may be a fusion of Vedic and Greek elements.

However, the Veda has even closer affinities. In *RV* X, 72, Aditi, who may represent here the primeval waters, brings out of herself the eight Ādityas (= a group of gods). She takes with her seven and casts away the eighth: this one is named *mārtāṇḍta* 'dead egg' and is the sungod, brought here to regulate life and death. The hymn does not say whether the other seven issued out of eggs too. Hymn X, 121 says that in the beginning [of manifest creation] rose Hiraṇyagarbha (= golden embryo), the one Lord of all that comes to be. A later text, *Śatapatha Brāhmaṇa* XI, 1, 6, 1-2, says how from the primeval waters a golden egg was produced and from this came Prajāpati, the Lord of creatures, of wisdom and creativity. The Greek-Finnish exclusive affinity is Zeus/swan and Ukko/bird, while the Vedic-Finnish affinities are the numbers 7-8, the waters and the cosmogonic aspect; the egg is in all three and in the Egyptian texts.[21] So without excluding a Greek influence it is quite likely that the Finns borrowed from the Indoaryans – a probability supported not only by their lexical loans but also by two other mythological motifs.

21. Another striking motif in the *Kalevala* is the creation of "the many coloured dome of **Sampo", the world-mill**, which, having roots in the sky, waters and earth, is also equivalent to the common theme of the world-tree or *axis-mundi* : this Sampo was created by the superhuman smith Ilmarinen. This Finnish myth may be indebted to two Norse themes, that of the world-tree, Yggdrasil, and that of the flood where Bergelmir and his wife escape from the inundation in their *lur* 'box-mill' (Stone 1997: 44, n5). The discussion of this topic would end here, but some philologists claimed that *Sampo* comes ultimately from IA *Skambha* 'support': the two world-supports, Finnish Sampo and IA Skambha (*AV* X, 7) are constituted of sky and earth and other universal elements (Santillana & Dechend 1977: 26, 111, 233). On this I have doubts and have not found a convincing statement either way. There is an additional Vedic-Finnish affinity. Ilmarinen, "the great primeval smith", erects a smithy complete with bellows, out of which he eventually creates the Sampo. The Vedic Bṛhaspati, lord of prayer and holy-power, in *RV* X, 72, 1-2, in the figure of a smith fashions the generations of gods with blasting and smelting. (The Greeks also had their divine smith, but the work of Hephaistos is in no way cosmo- or theo-gonic.)

22. A third mythological motif-image is that of **the (cosmic) cow.** In the *Kalevala* she rose upward out of Ilmarinen's smithy "With her horns all golden-shining / With the Bear-stars on her forehead / … on the ground her milk she wasted" (Santillana & Dechend, p 101). The image of the sky-cow shedding her milk down to earth is far too common in the *RV* (e.g. I.164.27-8; 3.55.1; 6.66.1; etc) to need further comment. This also is PIE since we find in the Norse *Edda* the cosmic cow Audhumla; this cow's milk sustains Ymir, the first creature, out of whose dismemberment arose various parts of the world.

Of some importance are "the Bear-stars" on the Finnish cow's forehead. The rigvedic cow has no stars on her forehead. But in the Veda, the best horse for the *aśvamedha* sacrifice is said to be one that has spots like the *Kṛttikās,* the Pleiades, on his forefront (*Śatapatha Brāhmaṇa* XIII, 4, 2, 1-4). The presence of stars on the forehead of a domestic animal, horse or cow, is a rare image. However, a similar image is found in the ritual of an animal-sacrifice (ass, bull, horse) in Mesopotamia: there too, the sacrifice animal (bull or horse) should have the Pleiades on his forefront (Albright & Dumont 1934: 119-123). Another telling detail is that in the Mesopotamian ritual the priest

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[21] In Greece the tale of Achilles and his parents, king Peleus and sea-goddess Thētis, involves the number 7 (i.e. their total children). This has close parallels with the tale of goddess Gaṅgā's transformation into a woman and giving birth to the Vasus in the Mahābhārata (see discussion in Kazanas 2004b).

whispers to the left ear of the animal while in the Vedic one the priest whispers to the right ear.

It is not very likely that the image of the stars reached Finland from Mesopotamia leaving no trace in intermediate regions. Nor is it likely that the Indoaryans borrowed this ritual from the Babylonians. "We may legitimately look upon the Aśvamedha as the principal Indo-European kingship ritual" (Watkins 2001: 265). Moreover some IE peoples, especially the Indoaryans, had a rich horse-Mythology (§5,d, earlier). Mesopotamia, on the other hand, had no horse-myths of any kind and the horse-sacrifice (as distinct from the bull-sacrifice which is old) was instituted not much before 1600, since only at that date the horse began to come into use. So if there was borrowing (and the detail of the whispering to the animal's ear as well as the stars on its forefront suggest there was), then the influence went from India to Mesopotamia – perhaps c 1700 when the Kassites established themselves in Babylon. (For a full discussion and the wider implications on IA chronologies, see Kazanas 2005.)

Since the detail of the stars *is* Indoaryan and, as part of the horse-sacrifice, may be PIE, it is most likely that the FUs borrowed this along with the other mythologems and lexical items. In no way do these mythological affinities indicate an Indoaryan movement from west to east and south. Rather the contrary is true since the Iranians do not seem to have any of these elements.

23. Other linguistic traces. According to another AIT contention, the IAs left "a trail of linguistic evidence... particularly in the form of hydronyms accross central Asia" (Bryant, **208**). It is claimed that various names of peoples and rivers and other lexical items have been left by the IAs north of the Black Sea and "around the Caspian Sea" and in Iran and Afghanistan (ibid 130-3). It would be too tedious to examine all this evidence. Bryant discusses it all and gives all the necessary references. His conclusion is – "The Avesta, then, simply deepens the mystery of Indo-Aryan origins" (p 133). But this is wrong. On the contrary, the Avestan evidence sheds ample light because it is unambiguous.

To begin with, let us accept that the IAs forgot their trekking over 100s of miles down to their final destination in Saptasindhu and consequently there are no references in the *RV* to previous areas and adventures. Let us ignore here and now G. Gnoli's (1980) carefully collected evidence of geographical references in the *Avesta* delimiting its area to south-eastern Iran (not north), Afghanistan and only as far as the Indus river. Let us ignore also the *airiianəm vaējo*, one of the locations the *Avesta* mentions as a former habitat (indeed, the original homeland), before the Iranians settled in Iran. We are now left with 15 regions named in the *Avesta* west and north of the Indus. But one of them is *Hapta Həndu* 'seven rivers' which happens to be a name frequently used in the *RV* to denote the area of the rigvedic people themselves (*RV* I.32.12; 34.8.35.8; etc, etc). No doubt there are many places on earth with seven rivers, but at that time the only evidence we have is that of the *RV*. Surely even the staunchest non-indigenist cannot get round the fact that Avestan *Hapta Həndu* corresponds to the rigvedic *sapta-sindhavaḥ* (plural = 7 rivers). Consequently, it cannot be claimed that the IAs travelled first beyond the Indus into Saptasindhu and then the Iranians followed but stopped before crossing over into the same area yet somehow acquired (how?) a memory of a region called "Sever-rivers" – for which there is no evidence in that larger area other than Punjab. No! Surely reason dictates that the Iranians had at one time lived in a region with 7 rivers, then left but retained a dim memory of the place in the name *Hapta-həndu*[22]. This is supported by the preceding discussion and the name of

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[22] Hopkins noted this Vedic and Iranian connexion but, like all invasionists, he too saw this as indicative of the Indoaryan south-east movement from Iran to today's Punjab (1898).

the river Sarasvatī (§25).

24. The common Indo-Iranian period is closely related to this and similar issues. The AIT has as a major constituent this entirely hypothetical notion – that the IAs and the Iranians came down south-eastward from the Urals and the F-Us as one united people with a single culture, or, at worst, as two closely related peoples with practically the same language and culture. This may well have been true but not within the AIT framework. The two were indeed one people but only in Saptasindhu: it is from there that the Iranians (originally Indoaryans themselves) moved away northwestward, roamed around for a time and eventually settled in their historical seat in Iran. G. Gnoli, who is by no means an indigenist, shows very clearly that the early portions of the *Avesta* hardly know northern and western Iran and he analyses migrations there from south to north and east to west but never north-west down to south-seast (1980).

Let us now examine some lexical items which, according to AIT scholars, the IAs borrowed from the F-Us: this will demonstrate how unreasonable are these notions and the entire AIT framework. One word is *kūpa* 'hole, well' (Burrow 1970: 27). But note that this word, according to M. Mayrhofer is IE and has cognates in Gk *kúpē* 'hole', Gmc *hūf-r* 'ship's hull', etc (1956, vol 1, 253: note this date against Burrow 1973!). However, let us accept T. Burrow's proposal that here we have a genuine loan in IA from F-U. Then we observe that Avestan does not have this word: for 'well' Avestan has *xan-* and *śāt-*. The first is obviously cognate with S √*khan* 'dig' (>*khāta* ppp 'dug, buried' but also 'ditch, well'); the second, *šat-* stands isolated. How is this divergence to be explained? Are we to assume that the IAs left the common habitat in Iran taking with them the words *kupa* and *khan-* as they separated themselves from the Iranians while the Iranians somehow lost *kūpa*, retained *khan-* in the form of *xan-* ('well' now) and acquired (where from?) the word *šāt-*? Is not this thinking problematic, not to say absurd? Surely, it is much simpler and utterly reasonable to say that IEs left Saptasindhu (or thereabouts) and in passing from the Urals gave the word *kūpa* (or whatever the original was) to the F-Us and then carried it further west where the languages diverged into Gk, Gmc, etc; and also IAs settled in Iran where *kupa* was forgotten, and was substituted by *xan-* (=S *khan-*) and an adstratal loan *šat*. The stem from which *khan-/xan-* derive was lost in the other branches (except perhaps Gk *chain-ō, chaos, chasma* 'opening, gap'.)

Another interesting case is the alleged IA loan *chāga* 'goat'. This too is supposed to come from the F-Us during the common IndoIranian period. But this stem is not in Avestan which has only *būza-*. If the IndoIranians borrowed from the F-Us *chāga* why is it only in Vedic, which moved even further away? Now, some claim that Av *būza-* is IE, cognate with Gmc *buc-/boc-* but this is rejected by others. L, Gk and S, despite their vast and early literature, have no trace of this. But even if we suppose it to be IE, why is it not in S? And why is *chāga* not in Avestan? Absent from Avestan is also the IE cognate stem for Lth *ozys*, Gk *aix* Arm *aic*, S *aja*, *eḍa* (seen only in Av *iz-aēna* 'feathery') – all 'goat'. Surely, here again, the problems disappear when we follow a movement away from Sapasindhu (or thereabouts) in a north-western direction. This proposition explains satisfactorily also the similarity of Iranian *Harahvaiti* and Vedic *Sarasvatī*, both river-names.

25 The rivername *Sarasvatī* in the *RV* is, it is claimed by the AIT, a memory of the Iranian river *Harahvaiti*: having met this river in Iran the IAs came to Saptasindhu and gave the name to the local river. Then the Iranians followed staying in Iran and retaining the name *Harax^vaiti*. But how is this possible?... Unless some IAs stayed behind in that area and told the Iranians that that particular river had already been named *Sarasvatī*, how would the latter know and so give the very same name (in their

own language) to that river?

On this issue, linguistics, which is constantly invoked to "prove" the IA trek down to Saptasindhu, is completely ignored by non-indigenists. The name *saras-vatī* means 'she who has *saras* (=lake-s)'. This, according to Witzel (2001), means that the river Sarasvatī did not flow down to the ocean (=Indian/Arabian sea) like the Indus but into terminal lakes far north of the ocean. This is entirely whimsical and must be discarded. The well-known and much discussed verse *RV* VII 95, 2 says that the river flows pure in her course from the mountains *ā samudrāt* 'to the ocean'. If the poet wanted to indicate that the river ended in pools or lakes, he would have said *ā sarobhyaḥ* or *ā hradebhyaḥ* 'to the lakes' – without change in the cadence – ‿ – – . Then, the name *Sarasvatī* itself could well indicate that the river had lakes indeed, but these were the lakes on the mountains *where it originated*: this of course is quite possible. But *saras* may originally have meant not 'pool, lake' but 'eddy, whirlpool, swirl, current'. The root $\sqrt{sr}$ from which the noun derives means 'moving, flowing' and its derivatives *sara*, *sarit* etc, contain the idea of motion and flow. Thus *saras* originally may have denoted motions within the river-flow rather than the static waters of a pool/lake; so *saras-vatī* 'the river having swirls/rapids'. Other references in the *RV* (2.41.16; 6.61.8-13; etc) leave no doubt that Sarasvatī was a mighty river.

Now in Avestan 'lake/pool' is *vairi-*. In fact Avestan has no verb or nouns cognate to the Sanskrit ones $\sqrt{sr}$ >*sar-*. The only cognation is *harah-* in the name of the river *harah^vaiti* – nothing else. But S $\sqrt{sr}$ >*sar-* is a perfectly PIE morpheme appearing in Tocharian B *sal-ate*, Gk *hiallō/hallomai* and Latin *salire* all implying 'moving, jumping'. Avestan somehow lost this root and its derivatives. So, how did the Iranians manage to concoct this name *Harah^vaiti*, that sounds so exactly like a transliteration of the Vedic *Sarasvatī* , when they had no words from $\sqrt{sr}$ > *sar-*? Adherents of the AIT offer no rational answers. (For full discussion see Kazanas 2004b, Prabhakar 1994/1995.)

There is only one possible explanation. The Iranians, having lived in Saptasindhu moved to Iran (retaining the memory of the place as *Haptahəndu*); on meeting an amenable river there they gave it the name of the river they had formerly known – Sarasvati > Harahvaiti. The AIT can in no way, except by violating rationality, explain the two Avestan names *Haptahəndu* and *Harahvaiti*. This, if nothing else, should have alerted the AIT adherents to the possibility that there is something very seriously wrong in their migrationist scenario. Moreover, it is part of the general linguistic theory that the Avestan **h** derives from PIE **s**: so, it is again extremely difficult to see how the IAs who moved further southeast, retained the original s (in *saras* and *saptasindhu*) while the Iranians changed it to *h*.

26. Bryant (following Mallory) mentions also several **Baltic river-names** having forms like 'Indus, Indura, Indra' etc (p 133). The similarity with Vedic *indu* 'drop' and the name of god *Indra* is all too obvious. Are we here to assume that the IAs set off from the Baltic region and carried the memory of these names all the way to Saptasindhu in the words *indu* and *Indra*?... Perhaps so. But the only other possible cognates are Gk *oid-éō* 'I swell', Celtic war-goddess *Andar-ta* and, perhaps, Hittite goddess *Inar-a* (both probable cognates with *Indra*)[23]. It is very difficult to see how river-names, 'drop' and 'swelling' on one hand connect with war-deities on the other. In any case, here as elsewhere, there is no proof of an IA eastward migration. The Balts themselves in their songs record a descent from India (Chatterji 1968; Singh 1995). Some of these songs (and there are 28.000 *dainas-* with variants – as they are called) may come from an older period when the knowledge of India and Sanskrit had not yet been widespread in

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[23] For the cognation see detailed discussion in Kazanas 2001a. Also Das 2002.

Europe. [24] It may well be that the Balts, having arrived at their historical habitat from the east, gave these names to the rivers in honour of the god (=Indra) who had released the waters, then they lost and forgot the god due to substratal or cultural influences.

27. There are **other river-names in Europe**: Don, in Scotland (from Celtic and Old English), Danube (from Latin *Dānuvius*), ultimately from Celtic, the Russian Don, Dnepr (from Scythian **dānu apara* 'farther river'[25]) etc. These are not connected with any IA river-names but they seem to be connected with Gk *danaoi* 'dried-up, dead', the Danaids, who were the daughters of Argive king Danaos and had no husband but had leaky pitchers that could not hold water, and also were believed to be well-nymphs and rain-cloud-spirits (Onians, p 273, n2) and *Danaē*, who received Zeus as a golden rain-shower; also with the Irish (<Celtic) *tuatha de Danaan* 'children of Danaan (=Danu)' which is how some of the Celts called themselves – and, as said, the large river Danube derives its name from Celtic. It is possible that the Celts considered the river *Danu* a father-figure but there is no evidence for this. In Greece, Danaos was indeed an ancestral figure and in Iliad (I, 42, etc) Greeks generally are called *Danaoi* 'descendants of Danaos'.

Not surprisingly, Sanskrit has *dānu* 'stream, dew-drop' and *Dānu*, the name of Vṛtra's mother; the epithet *dānava* is applied to Vṛtra and other demons (Macdonell 1898: 158) while *sudānu* means 'pouring out, bountiful' and is applied to various gods in the *RV*. But Dānu, Vṛtra's mother, may well indicate 'river' or 'water' also, since *RV* I 32, 8, says that Vṛtra is engulfed under torrential waters, that he lies under his mother Dānu (st 9) and then, again (st 10), that the currents cover him and carry him (but not his mother!) away: this suggests an identification of Dānu with the river.

28. All the evidences in the preceding sections, §§11-14, do not remotely show a movement from Europe (or the Russian Steppe) to India; on the contrary, they should more reasonably be interpreted (certainly in §18) as a west- and north-ward IA movement. This is supported by yet another piece of evidence. In the *Edda* we find the mythologem wherein the primeval giant-being **Ymir (=Vedic *Yama*)** is dismembered and from his parts (as from Vedic Puruṣa's parts in *RV* X, 90) are made various sections of the world. In *Germania* II, the Roman historian Tacitus reports an early anthropogonic version whereby god *Twisto* engendered *Mannus* and he in turn begot three (or more) sons from whose names are known the three (or more) tribes of the Germanic people. The two latinized Germanic names are met nowhere else in the IE culture(s), except the Veda. In the *RV* Manu is born to sungod Vivasvat from Saraṇyū's double (*savarṇā*) and Saraṇyū is the daughter of creator-god Tvaṣṭṛ who "begets mankind in varied manner" (*RV* 3.55.9 and 4.9). Manu himself is "father Manu" (*RV* 2.33.13; etc) and in later texts is said to be the survivor of the flood and progenitor of a new human generation[26]. Thus in the Veda we have Tvaṣṭṛ, Manu and mankind. But closely connected with them is *Yama* (=Norse *Ymir*), another son of Saraṇyu. Thus the Vedic mythologem holds unified the two separate strands of the two Germanic ones.

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[24] When in 2002b (285, n8) I said I ignored the Baltic tradition and accepted that it was due to early nineteenth-century fashion, I really had not as many data as I have now.

[25] For the Russian names I follow the etymologies in Watkins 2000 (under *dānu*). Curiously, Walkins gives a conjectural Iranian **dānu* as the source for all these but does not mention the Vedic *dānu* 'stream, water'! *aparo-* is a genuine Avestan word (=S *apara*) but **dānu* is a conjectural reconstruction in Avestan while it is so in Vedic! Let us keep the IAs out of this!

[26] For a fuller discussion including Yama and Yamī and the Aśvins, all offspring of Saraṇyū, see Kazanas 2001: 278-281.

Surely, it is much more reasonable to suppose that the myth travelled westwards in fragments: Yama and Yamī are *Yima* and *Yimeh* in Persia; Saraṇyu is *erinus* and *helenē* in Greece (§8 and n6, above); Yama, Tvaṣṭṛ and Manu appear as *Ymir*, *Twisto* and *Mannus* in Germania. It would be rather unreasonable to say that the fragmented, unconnected elements in the other IE mythologies coalesced into a single mythologem in the Veda when the IAs arrived in Saptasindhu!!

29. Substratal influences. To return to linguistics, the *RV*, "compiled some time after the arrival of the Indo-Aryans", according to the AIT, shows "clear linguistic traces of non-Indo-Aryan language speakers that preceded the Indo-Aryan on the subcontinent" (Bryant, 299). Elsewhere Bryant opines that indigenists "must accept that any discussion of Indo-Aryan origins that neglects the [linguistic] substratum data simply cannot be taken seriously" (p 107). So, examine it I shall, even though it seems to be a waste of time, since after 32 pages of discussion Bryant himself concludes, "it is difficult to see how the "evidence" of a linguistic substratum in Indo-Aryan [=Sanskrit], in and of itself, can be used as a final arbitrator in the debate over Indo-Aryan origins" (p 107).

Indeed, Burrow finds in the *RV* some words that he thinks are borrowed from a substratum, Dravidian or Munda (=Austro-Asiatic): *ulūkhala* 'mortar', *kaṭu-ka* 'sharp, pungent, fierce', *kuṇḍa* 'hole, pit', *khala* 'threshing floor', *bala* 'strength', *bila* 'hole, cave', *mayūra* 'peacock', *lāṅgala* 'plough'. There are many more such words in post-rigvedic texts.

All these words have no apparent cognates in other IE stocks and cannot, apparently, be analysed back to some acceptably IA root. Moreover, such words show affinity with lexical items in those other languages, Dravidian and Austro-Asiatic. This thinking seems quite reasonable, but it has certain pitfalls. In fact, Burrow unwittingly fell into one regarding *bala*. For, although Dravidian branches have the stem *bal-/val-* 'strength, strong', other linguists have connected the Sanskrit stem *bal-* with other IE stocks, e.g. Gk **bel**-*tiōn* 'better' and Latin *de-***bil**-*is* 'strengthless' (Mayrhofer 1956 under *bala*). Another blunder is *kupa* also thought to be substratal (Burrow 1937: 27); it is not substratal since we find cognates in Gk & Gmc (see §18, above). So a first lesson is that one must not be hasty. And since the *Dhātupaṭha* gives a root √*bal-a prāṇane dhānyāvarodhe ca* 'breathing and conserving wealth', even if it is found as a verb rarely and not in early texts, we need to learn a second lesson: the fact that a root does not appear as a verb in the texts does not necessarily mean that it is not genuine Sanskrit. Sanskrit too in its long history has suffered many losses as is obvious from the many nominal and verbal forms that did not make it into the classical period: e.g. *át(t)ra* 'food', *īḍ* 'praise', *kā́rṣman* 'goal (furrow-line)' *tánas* 'offspring' etc.

30. Before proceeding let us be clear about the situation.

The *RV* is assigned even by the AIT to c 1200. Apart from the undeciphered Harappan seals, no other language is attested at that period. The first Dravidian attestations come at least 1000 years later and Munda even later. (This does not mean that the Dravidians or the Munda-speakers did not have an oral tradition going as far back as the *RV*, but, in any event, the fact is we do not know what the language would have been like.)

The absence of a rigvedic word in the other IE branches means little more than that. To take as an example our mythologem of *Tvaṣṭṛ, Saraṇyū, Manu, Yama* and *Yamī* (see §28), let us now suppose that the strands found in the Avesta, in Greece and in the *Edda* had not survived except for the Norse motif of Ymir's dismemberment. What would the IE comparativists have said about this? Probably that Ymir was not connected with Yama and that the Vedic mythologem may be connected with and,

perhaps, is derived from the Egyptian myth of Osiris and Isis since both Yama and Osiris become kings of the dead in the Afterlife. Fortunately, we have all those strands scattered in other IE branches and so know that the Vedic myth is genuinely PIE. To take a second example, if the name of Vedic firegod *Agni* was not preserved in Hittite also as *Agnis*, although the Hittite word for fire is *pāḫḫur*, and in Slavic as *Ogon* and since the other IE branches have no firegod Agni but some have a corresponding stem for fire (Latin *ignis* and Baltic *ugnis*), comparativists would probably have said that the fire-cult in the Veda was a specific IA innovation. The fact is, however that in both cases (and in many others: Kazanas 2001) the Vedic tradition has preserved genuine PIE mythological/religious material, lost in the other branches. Thus, the same may well have happened in language: the other IE stocks lost lexical items that are preserved only in Sanskrit.

The absence of certain lexical items in other IE branches and their presence in Sanskrit, therefore, does not mean that these are not PIE. The presence of similar stems in non-IE languages on the subcontinent may be fortuitous or may indicate borrowings from Sanskrit on the part of the latter. Even Sanskrit words with cerebral phonemes (which are supposed to be non-IE and their stems exist in non-IE languages) need not be borrowed by Sanskrit; the cerebrals (as I argue elsewhere, 2004a) could well be PIE and therefore IA.

The hunt for foreign, non-IE words in Sanskrit meets other difficulties too. Just because a word or dhātu does not appear in the *RV* or the *AV*, it does not mean that the word was not in the early language: it can hardly be claimed that the two Saṃhitās contain the entire vocabulary of the early language. Only if a word that has no IE cognates at all first appears in late, classical texts (i.e. after the sūtras and the epics), then there is certitude that it is a loan. But then, at say 200 BC, such words are of no relevance to the debate on the AIT.

31. At different times throughout our 20th century, different scholars have made different claims for loans in Sanskrit, sometimes as many as over 1000 lexical items and sometimes from an unknown language X, now wholly lost to the record. However all or most items on such loan-lists have been disputed at one time or another. For instance, Bryant cites F. Kuiper's (1991) claim to 380 foreign words, or "4 percent of the Ṛgvedic vocabulary" (p 87). But R. P. Das refutes not only Kuiper but all other lists of foreign loans, as P. Thieme, M. Emenau and others had done before him and we should note that Das and the others are not indigenists. For Das there is "not a single bit of *uncontroversial* evidence on the actual spread of Dravidian and Austro-Asiatic speakers in per-historic times, so that any statement on Dravidian and Austro-Asiatic in Ṛgvedic times is nothing but speculation" (1995: 218). Later he adds: "*not a single case* [exists] in which a *communis opinio* has been found confirming the foreign origin of a Ṛgvedic (and probably Vedic in general) word...[M]any of the arguments for (or against) such foreign origin are often ... statements of faith" (id, 228: emphasis original).

Let us look at one more and recent example. Bryant cites F. Southworth's work (*non vidi*) who finds that from a total of 121 terms for plants only just over a third have IE etymologies (p 93). Many of these plants came from abroad and, as with practically all modern languages, these naturally have foreign names. Other plants may be indigenous and have both Sanskrit and Dravidian names and obviously the situation is very unclear. But, Byant comments, "Southworth's lists show no instance of plants native to the Northwest that have non-Indo-Aryan etymologies" (p 94).

32. Bryant gives many more examples of possible loans but always supplies counter arguments or evidence (pp 95-105) thus confirming the words of Das that "not a single bit of *uncontroversial* evidence" exists in this matter. We can therefore conclude this

topic with Bryant's citation of M.Witzel's discovery "that there was no Dravidian influence in the early Ṛgveda" (p 101). Witzel postulates then a Munda substratum for the oldest portion of the *RV* ; Munda, or Para-Munda or a western form of Austro-Asiatic (itself being an overlay over an earlier unknown, now lost language), was the language of the Harappans. All this is sheer speculation on Witzel's part and utterly valueless. As Bryant points out, "If Dravidian has influenced Indo-Aryan through adstratum...and *not* through a substratum relationship, then why could Munda (or other languages) not likewise have done so?" (p 102). But Witzel's conclusions receive heavy criticism from a Dravidian expert, B. Krishnamurti, who shows that Witzel's analyses are wrong and that the Munda prefixes are, in fact Dravidic (2003: 37-8). Witzel will probably say that Krishnamurti and the scholars he follows are wrong. Confusion always results from comparisons with Proto-languages at those time-depths; scholars can prove anything they like with "reconstructions".

The long and the short of all this discussion of loans is that it does not indicate, let alone prove, that the IAs came into Saptasindhu c 1500. Even if it could be demonstrated on non-speculative grounds that Dravidian, Austro-Asiatic and any other language(s) were present in the vicinity – east and south – of Saptasindhu and that the *RV* has indubitable loans from these languages, this would not prove that the IAs arrived at c 1500. Borrowing could have occurred much much earlier.

33. One final point: horses and chariot. For many decades it was believed that there were no horse-remains in the ISC. This is no longer so since in the 1990's especially horse-remains in several sites have been unearthed: see Kazanas 2003, 196-7 §10 with full references. The presence of the true horse (*equus cabalus, ferus*) in the ISC is now amply and firmly established. The two-wheeled chariot is not. But then the *RV* has no such vehicle as many scholars (especially Witzel) claim drawing their model from the Near-Eastern fighting chariot. It was shown long ago (see *Vedic Index* under *anas* and *ratha*) that the distinction between *anas* 'cart' and *ratha* 'chariot' (as is the usual explanation), "is not absolute". Uṣas, the Dawn-goddess, has *ratha* in (early hymn) 3.61.2 and *anas* in (late) 1.48.10 but *anas* in (early) 4.30.11 and *ratha* in (late) 10.73.7. Again Indra, the mighty Wargod is said to be in (late) 1.121.7 *anarviś* 'cart-seated'! The *Vedic Index* concludes "of differences in the structure of the two we have no information". All this, and more, I presented in the *JIES* 2002 (pp 305-7). Witzel launched an attack, accused me of not using realistic data and provided his own (2003). Unfortunately for him he relied on secondary sources and on the German translation of the *RV* and it turned out he was using wholly mythological references and wrote of the 'chariot' thinking the original (10.85.10-12) was *ratha* whereas it is in fact *anas*! (For the amusing disclosure see Kazanas 2003, p 226 §40.)

Then there is *ara* 'spoke'. Indeed the *RV* has many references to spokes and spoke-wheels. It is, as usual, assumed by every body that *ara* designates the 'spoke' as we understand it. So strong is this notion, that several Indian writers say that the hymns containing *ara* (eg 5.13.6) are late or interpolations and so manage, as they think, to preserve the antiquity of the rest of the *RV*. All this is possible, of course, but there are other sides. It is by no means certain that *ara* means 'spoke' as we know it. The wheel could be solid, made up of *aras*, i.e. 2, 3 or 4 pieces joined together and joining at the hub. Spokes as we know them are designated by different words in the branches (Gk *aktis*, L *radius* etc); this indicates that the spoked-wheel was most probably adopted after the dispersal. But another possibility is that there were spoked wheels even as early as the 4th millennium but have not been found yet. After all, in the ISC the remains from carts are very very meagre. Such wheels are clearly present in the ISC as Lal shows with photographs of them (2002). Thus neither the horse, nor the chariot and its spoked wheel present any serious difficulties.

Why the AIT?

34. This is a good question because when thoroughly scrutinized the AIT is found to consist of nothing other than tiers of assumptions, the basis of which hangs in the air of speculation and does not touch any ground of real evidence. [27] The most important tiers are linguistic data which, as has been shown, are either ambivalent or positively indicative of an IA movement out of Saptasindhu. In a different sphere we have the statements of numerous (Western) archaeologists specializing in the ISC that the local culture displays an undisturbed continuity from the early period of Mergarh (c7000) down to the Persian incursions after 600 B.C. Moreover, the *RV* knows nothing of urban structures or ruins thereof and nothing of fixed fire-hearths/-altars, of bricks, of cotton, silver and rice – all constituents of the ISC and found in abundance in post-rigvedic texts (Kazanas 2001b 2005b). We should also note N. Achar's archaeo-astronomical studies of some IA texts showing, on the basis of their astronomical references, that these texts were composed in Saptasindhu long before 1500. Finally, in a paper studying the correlation of Vedic and Mesopotamian religious ideas, Dr S. H. Levitt finds that "the Ṛgveda would date back to the beginning of the 3rd millennium B.C. with some of the earliest hymns perhaps even dating to the end of the 4th millennium B.C." (2003: 356). In view of all these considerations one must wonder on what the AIT is based and why its adherents persist in their attachment to it.

Bryant professes 'agnosticism'. But I find even this inexplicable. For the view of archaeologists is wholly uncontroversial. Back in 1984 D.L. Heskel wrote: "It is also evident that previous theories of wholesale population migration and invasions… are not acceptable in the light of archaeological evidence" (p 343). Fifteen years later J. Shaffer and D. Lichtenstein wrote devastatingly: "the academic investment in this hypothesis [=AIT] is so great that the distinguished scholar Colin Renfrew (1987) opts to distort the archaeological record rather than to challenge it… The South Asian archaeological record… does not support Renfrew's position or any version of the migration/invasion hypothesis" (1999: 258). Since *all* the linguistic data are either ambivalent or against the AIT, surely scholars should incline towards the archaeologists' view which is unanimous.

35. Prejudice is obviously involved. And not only of the kind often mentioned even by Western scholars – Eurocentric, racist, religious and the like (Shaffer 1984; Leach 1990; Feuerstein *et al* 1995; Trautmann 1997; Bryant 2001). In a curious way, Indian protohistory became the concern of vedicists, linguists and comparativists, who often ignored Archaeology, (Archaeo-)Astronomy and the other disciplines. Even in Europe Archaeology had not been established as a discipline in the 1850s, whereas comparative philology had made big strides. In the latter half of the 19th century philologists decided that Sanskrit should not be the basic source for IE linguistic studies and laid more emphasis on Greek and the other known branches (especially Brugmann 1897–). So a new PIE language was "reconstructed" and this would receive a further boost by the discovery of Hittite. Some philologists went as far as writing the Christian 'Lord's Prayer' (i.e. "Our father which art in heaven" etc) and other texts in this totally conjectural language. (This has been revised and the indoeuropeanists think they have now "got it"!) At the same time, archaeological finds, linguistic

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[27] The absence of securely established horse-remains (*equus cabalus*) in the ISC is still being used by non-indigenists as "archaeological proof" for the AIT. However, I have pointed out, as others have done before me, that the horse, despite the frequent mention and praise it has in the *RV*, was not very common in the Aryan society (Kazanas 2002: §VII, 1). I have also shown (§33) that since 1995 the presence of the true horse is very thoroughly established in the archaeological record of the ISC (Kazanas 2003: §10). Doubters need only look up the references.

investigations and historical studies, between them, disclosed the fact that the early European peoples, Greek-speakers, German-speakers et al, came to their historical seats after migrations and invasions. So a theory was erected that the matrix of these languages and peoples was located somewhere in east Europe or the Pontic Steppes of southern Russia (there were variants of this urheimat from the Balkans to the Baltic sea). And, of course, the IAs also went from this matrix to India.

Archaeology in India made a giant stride forward with the excavation of the ISC which began under Sir John Marshall in 1924. Later detailed studies of the ISC (e.g. Lal 2002) show beyond any shadow of doubt the continuity of cultural elements from that ancient era to modern times. But in the 1920s and 1930s (and until early 1980s) the notion of IE migration and invasions was far too well-established. Besides, here in Mohenjodaro was further proof of the doctrine: here was a ruined town (and others in other areas) and skeletons bearing what at first sight seemed battle-wounds. It was easy for Sir M. Wheeler to accuse Indra and the invading Aryans for the ruination of the ISC and the "massacres" of the natives. In the late 1960s G. Dales demonstrated that there had been no massacre and no battle (1966). As I wrote above (§§10a, 34), in the early 1980's archaeologists stressed the unbroken continuity of the culture in that area and rejected any notions of mass invasion/migration. Instead of paying heed to the unanimous voice of the archaeologists, mainstream indologists and IE comparativists continued with the myth of the IA arrival c1500 and the *RV* composition c1200 and after, accompanied by their theories about IE dispersals and the "reconstructed" PIE language. The only change is that the "invasion" became "immigration" and this latter proceeded in small waves. But, of course, small peaceful waves of non-literate immigrants could not so totally aryanize the area of Saptasindhu and culturally subjugate the literate native population (Kazanas 2003; Witzel 1995: 106-7).

36. In view of all this any rational being must wonder at the AIT. Bryant himself supplies, as we saw, counter-evidences and counter-arguments against almost all points of the AIT. On the other hand, there are three aspects of the indigenist view that cannot be circumvented. First, the archaeoastronomical evidence (Kazanas 2002, with references). Even if one has doubts about the accuracy of this, one must face two more aspects. One is the solid view of all archaeologists, experts on the ISC, who insist on the unbroken continuity of the Mergarh-Harappan culture and reject any mass entries: here, one should note that the aryanization of the Saptasindhu (place-names, river-names etc) could not come about through small, peaceful waves of IA entrants; it could only come about through conquest by a fairly large force. The third aspect is the pre-Harappan character of the *RV* which has no knowledge of urban structures and ruins, of bricks, cotton and other elements of the ISC, which should be present in it if this text was post-Harappan— as they are present in post-rigvedic texts (Kazanas 2005b).

Why do mainstream academics persist with the AIT? I think simply because its rejection would upset the various superstructures upon which they thrive for many decades. As another scholar of repute, the late E. Leach, Master of King's College, pointed out (1990), academic posts and reputations are involved. Scholars are people and people tend to be attached to and identified with their ideas, posts and reputations (and incomes). Consequently they persist with their models and paradigms irrespective of other factors. Such considerations, however, have never proved durable obstacles to the progress of true knowledge.

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