Final Reply

N Kazanas, Omilos Meleton, Athens: Jan-Feb 2003.

Introduction.

1. Two clarifications seem to be needed at the outset.

First, the thesis of my essay 'Indigenous Indoaryans...' was that the IAs were indigenous at c1500 BC, when, according to the proponents of the AIT (=Aryan Invasion/Immigration Theory) the Aryans entered into Saptasindhu just as the Harappans were moving to the east. This thesis may prove to be wrong but only if sure proof, i.e. unshakeable evidence for this entry, is provided – not conjenctures and hypotheses. There may have been an entry c4500 or before. That the area of N-W-India-and-Pakistan and the environs might be the IE urheimat is a secondary issue. I gave as an alternative the extended continuum from Saptasindhu to the Caspian Sea (and I would add the Pontic Steppe).

Second, I was at pains to disclaim any connexion with Indian nationalists/fundamentalists etc because it is the truth. Nonetheless, M. Witzel's Comment hurls out charges of this kind, verging on libel. I shall deal fully with these in my reply to him. Suffice it to say here that *Omilos Meleton* was founded in 1976 by Dimitris Peretzis (an architect) together with some Greeks from the fields of the arts, economics, law etc, and some non-Greek hellenists from the American and British Archaeological Schools in Athens; it is recognised by and receives grants from the government (though never enough, of course). When Kazanas introduced Sanskrit in the 1980s there were already established courses in Philosophy, Economics, Greek and Art. Witzel also calls me "a stranger" in the *RV*. But the patient reader will discover that I am not alone – except that I don't hide it.

In the next pages I shall refer to each of the critical Comments by the writer's name and to my own essay by the initial K and the number of section and sub-section (eg K V, 1). It will be readily understood that I can't possibly deal with every point raised in all nine Comments. I shall try to combine parallel points from the different papers but even so I shall need to be selective, otherwise I shall be writing a very long book. Witzel's Comment, for example, is 82 pages as against my essay which is only 53; by a rough analogy I shall need about 100 pages just for Witzel. Then, many points are unimportant. For example S. Zimmer (6.1, n17) mentions the Irish and Greeks who used boats to travel, but this is not parallel to the point I made about the Scandinavians (K VI, 1, n 14). Another example is Witzel's criticism of my use of the *Vedic Index* calling it antiquated; by the same token, he should stop citing Grassman who is even older: a particular work is to be judged by its data, not its age, and I would be grateful if Witzel could suggest a modern work comparable to the *Vedic Index*. It would be tiresome for the reader to go through all such non-arguments which do not advance the discussion. Finally, there are some fresh aspects that need to be examined.

2. Before anything else, I must make a correction in my own text. In K V, 3 and X, I wrote that in the light of the new astronomical data the Great Bharata War occurred at 3067. I was carried away. I see no reason to alter the date given by the native tradition 3137, that is 35 years before the death of Kṛṣṇa and the onset of the Kaliyuga at 3102. The *M(ahā)Bh(ārata)* data which Achar provided as converging in 3067 indicate not the date of the war itself but of **the start of the core of the *MBh***. The 70-years difference is only three generations and it seems natural that bards should start telling in narratives, poems and songs the heroic events at about 3067 using star-references of their own year. So this date 3067 marks not the date of the war but rather the beginning of the epic.

D. P. Agrawal, M. Huld, E. E Kuz'mina, R. T. Meadow, S. Zimmer.

3. I start with Agrawal's Comment, which contains many relevant issues. Since he is not a linguist he pushes aside my "convoluted arguments" on language and ascribes to me three contradictions. a) K "is not aware … that the Saptasindhu was not a desolate wilderness" and K perhaps means "that the IVC intruded into a Vedic settlement" instead of the Indo-Aryans being the intruders. b) K "admits that there is no mention of the ... IVC and its towns" in the *RV*, but "fortifications, standardization of bricks, metallurgy etc" had begun in the IVC at c4000. c) K includes the river Sarasvatī into Saptasindhu "which is the Indus river system". I find it difficult to connect these three points with my essay.

a) Agrawal starts with the assumption that the IAs are intruders in the normal AIT thinking. I neither state nor imply anywhere that Satptasindhu was a wilderness nor that the Harappans or the IAs intruded into each other. It is obvious that, following the archaeologists' view of unbroken continuity in the area, I regard Harappans IAs.

b) In K IV, 3, I do state that the *RV* does not reflect Harappan elements like "urbanization or ruins, fixed fire-hearths, bricks, cotton, silver, etc". There is no contradiction.

c) In K VIII, 2, I state that *RV* hymn VI, 61, 8-13 "lauds the river [Sarasvatī] as ... most dear among the sister-currents". I omitted to include the adjective *saptasvasā* 'with seven-sisters' (st 10) – but I would expect any archaeologist or proto-historian to know that Saptasindhu includes the Sarasvatī, as is evident in E. Bryant's, A. Parpola's and Witzel's Comments.

4. Language. There is nothing "convoluted" about my linguistic arguments and I don't understand what all the fuss is about, as eg in M. E. Huld.

I wrote that "many fine results have been obtained by the detection (or application) of philological laws operating in large areas of a language or of a family of languages, like IE" (K IX, 2). I say further that linguists have disclosed to us certain universal laws that apply to all languages at all times and reflect realities: eg singular, dual and plural numbers in nominal concepts, present, past and future in verbal expressions, and so on. Here I include the *kārakas* of Pāṇini (and his predecessors) which signify the six deep-structure factors related to every action denoted by the verb: i) the motionless from which all movement flows which is also the cause or motive; ii) the indirect object towards which the action moves; (iii) the spatial, temporal or substantial frame within which the action unfolds; (iv) the instrument by which the action is performed; (v) the direct object; (vi) the agent or doer (Pāṇini's *Aṣṭādhyāyī* I, 4, 23-55). These *kārakas* are independent of the nominal cases. Thus the agent may appear in the nominative as in '*The man* ate the apple' or in the instrumental as in 'The apple was-eaten *by-the-man*'.[1]

Then, I said that reconstructions of proto-languages are not reliable because they are conjectural. Linguistic changes (vocabulary, accidence, spelling etc) are not subject to universal laws. The way English has changed from say 1100 CE is quite different from the way French or Greek changed, even though some features are common (loss of inflexion, additional use of auxiliaries and prepositions, etc). Now, while we find certain general phenomena (approximating the nature of "law") within the documented periods of changes in any one language and certain general relations (again="law") between languages within the IE family, nonetheless there are also exceptions to these general phenomena and relations. I took various examples from Greek (and Md Greek), Latin, Sanskrit and English to illustrate such exceptions. If we find so many bizarre phenomena that are outside the general 'laws', it seems to me very difficult to go back to undocumented periods and

[1] The following sentence has all the *kārakas*: 'In the palace, in the evening(iii), out-of-his-knowledge(i), the doctor(vi) restored(=verb) by-means-of-pills(iv) good health(v) to the sick king(ii)'. All complete active sentences have at least two *kārakas* (v and vi) explicitly stated and the others implicit in the context. There are intransitive verbs like 'walk, sleep' etc. These can be transformed into transitive and so give us a direct object: walk = move myself (or my body) by-means-of-the legs; sleep = put myself (or my mind) to sleep. Even the verb 'to be' can be transformed into 'make/turn oneself into...'.

claim certainty and reliability for reconstructions that can in no way be verified. Linguists should be proud of their discoveries, of course, but I think they should also be circumspect and curb excessive and unrealistic claims (K IX, 2).

I may have overstated the case, I may have been polemical, but I wrote nothing unreasonable. The laws in a science like Physics or Chemistry have no exceptions: they are universal or invariable within their domain. In describing the changes in the IE languages linguists have disclosed neither regular and invariable phenomena nor universal laws. Apart from exceptions, the changes in the Vedic, Greek, Germanic and other languages are quite different (*eg* V *bha*, Gk *pha* and Gmc *ba*): they occurred in particular, if not precisely determined, places, periods and peoples, and stopped there. In contrast, the law governing the chemical constitution of water ($H_2+1/2O_2=H_2O$) or the law governing the attraction of bodies/masses ($F=m_1 \cdot m_2/R_2$) operates at all times and places. There is no regularity, constancy or universality in the various linguistic changes, which are of the nature of accident. Curiously, Huld does not mention this distinguishing feature; instead he cites the statement that "a scientific statement can be disproved" which can apply to pronouncements in all fields.

5. Let us look at this issue from another angle. Languages are changing constantly in one or more aspects. Can any linguist, using all the accumulated information of centuries, predict how any language will change in the next 50 years?... Here in Greece we had by an Act of Parliament in the late 1970s a Reform in the grammar and spelling of Md Greek. Nobody could have predicted these changes 20 years earlier, just as nobody in 1000 CE could have predicted the changes in English that came with the Norman Conquest or after Chaucer or Shakespeare. Such linguistic changes are subject to influences that originate in not strictly linguistic factors – like the education system, parental attitudes, foreign influences, political pressures, great writers and so on. If we can have no reliable predictions about future developments, we can have no reliable conclusions regarding changes in past periods before documentation. Some of the reconstructions may be true. But who can tell with certainty? There is no verification.

Linguists take offence and have strong objections here. To my question "why should we take this sort of 'evidence' seriously?" Witzel replies "*because it is there*" (original emphasis: §6.0). A, yes! but where exactly? And why the asterisks? For instance, Witzel quotes " IE **sel* 'to jump, propel forward' " (§6.8, end). Huld cites *k_we-k_wlo (=V *cakra*, Gk *kuklos*, etc). But in what IE language do we find *sel or *k_we-k_wlo? In none. We find them only in modern writings and the asterisk denotes precisely that they are not "*there*" in any language.

Huld mentions early on "Ernest Pulgram's skeptical rejection of Proto-Romance" to which I also referred (K IX, 2).[2] A paper by A. Dolgopolsky illustrates this aspect better. Utilizing various linguistic data in conjunction with Renfrew's (1987) archaeological evidence, in this paper (1993) Dolgopolsky argues (*contra* Mallory, Gimbutas *et al*) for an Anatolian IE homeland. This aspect I leave aside. He cites Pulgram's statement "Words cognate with French *bière*, tabac, *café* are common Romanic, evoking a picture of Caesar's soldiers guzzling bier and smoking cigars in sidewalk cafés", then writes (pp 232-233): "comparison between the Romance languages does not claim to reconstruct the classical Latin of Cicero's time, but only the Late Vulgar Latin of the Early Middle Ages... 'Since all Romanic languages name a certain animal *cheval*, *caballo*, *cal*, etc... the Latins called the horse *caballum*', continues Pulgram '[whereas the Roman word for 'horse' was *equus*]'... This is true of Classical Latin, but not of Proto-Romance, i.e. Late Vulgar Latin, in which *equus* was replaced by *caballus*... I cannot understand why Ernest Pulgram took the liberty of ignoring these simple facts (known perfectly well to him)." What Dolgopolsky does not understand is precisely the point. We have the documentation and therefore know about the developments and

2 Witzel's Comment also mentions Pulgram contemptuously.

distinctions from Classical Latin onwards. Remove the documentation of Proto-Romance and before, and we shall have the caricature that Pulgram sketches.

I dealt extensively with many examples from Md Greek (which have extant forms in OGk) showing that without the documentation we would end up with very absurd originals. It would have been nice if Zimmer actually demonstrated what is wrong in these examples and in the S and Gk correspondences instead of accusing me sweepingly of "complete ignorance of [my] mother tongue's historical grammar" (§5.2)[3] . Huld does mention the example of "OIn. *juhomi* vs Gk χέω 'pour'" (and also the IE reconstructed root **gheu*). But the point I was making is precisely that whereas there are correspondences of reduplicating verbs (with variations in the vowels) and we *might expect* a similar one in the particular example, we do not find this correspondence.

Nowhere, have I denied that the various linguistic disciplines are of immense value and have produced fine results. My objection is that since all conjectures contain an element of uncertainty, sometimes small and sometimes large, conjectural asterisk forms (like **gheu* etc) cannot be regarded as reliable evidence (hypotheses are not admitted as evidence in Courts of law). Theories, then, erected upon such forms are equally unreliable, if not more, as when one reconstructed proto-language is compared with another and conclusions are drawn therefrom. These asterisk forms remain "a perpetual possibility/ Only in a world of speculation."[4]

6. Huld discusses some of my cognations and "half-baked etymologies" and finds some of them "nearly correct". The list had been scrutinized by at least four (possibly six) referees (two for *Cosmos*, the Edinburgh Univ Journal, and at least two for the *JIES*) and I incorporated all the corrections they suggested. So all I can say is that the *JIES* and *Cosmos* need referees with Huld's criteria.

Zimmer writes that he can take my "intention as serious" but my article "hardly meets scholarly criteria". I can only cite the Editor's comments: "I indicated that I thought that it would be unlikely that any referee would agree with his [=Kazanas'] conclusions but that I would consider publication if one of the referees believed that the article had a case to answer; I requested the referees to view the article in that light." The Editor sent me the conflated comments of the referees and I made adjustments and corrections.

Here are two of the points the referees made that support "a case to answer". (a) "[T]he author gives different arguments against recent works by Witzel (and some other scholars) who oppose the idea of an autochthonous character of the speakers of Indo-Aryan in India. Some of these critical thoughts are quite interesting." (b) "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 data mentioned in the article may be relevant for the definition of this time." So let us turn to this.

7. Archaeoastronomy Both Huld and Zimmer reject or doubt strongly the archaeoastronomical evidence furnished by B. N. N. Achar (K V, 1-3), each on different grounds. Both linguists cite many authorities from their own field and experts from other fields, yet, strangely, do not trust the expertise of a professional astronomer.

Huld doubts the identity of the nakṣatra Dhaniṣṭha and seems unaware of Witzel's discussion of this subject in EJVS 7-3, §30. He also mentions "precession", which describes the changes that

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[3] Witzel also sidesteps this issue and, instead, talks at length about loan words in Old Greek (§6) - an irrelevant issue I don't touch upon.

[4] Beginning of 'Burnt Norton' in T S Eliot's *Four Quartets*.

appear in the disposition of the stars due to the change of the tilt of the axis of our planet. One wonders whether only non-astronomers like Huld take such phenomena into account whereas a seasoned scientist like Achar does not. Huld appears content with his comments on the astronomical references in the *"Śatapatha Br* and the *Jyotiṣa Vedāṅga* and says nothing about the numerous references in the *MBh* that converge, according to Achar, in the year 3067.

Zimmer admits (§ 5.1) that "heaven does not lie" but, he cautions us, "humans do lie, occasionally, and ... do in fact err very often. Modern astronomical computation is one thing, and its application another." Thus he doubts scientist Achar's competence. In support he cites Jos 10, 12-13, *Old Testament*, where both sun and moon stood still for a whole day, and asks, "Has Mr Achar already been able to date Joshua's battle agaist the Amorites in Gideon?" Presumably this is rhetorical not sarcastic. Then Zimmer gives another objection: "The three-hour eclipse of the sun reported in Mt 27, 45, Mk 15, 33 and Lk 23, 44-45 has never been confirmed by astronomy." This is due to the simple reason that it never happened. On the contrary Achar confirmed the celestial phenomena in the Indic texts because they did happen. And where does the "eclispse" come from? All three Gospels use the identical phrase "there was darkness (σκότος ἐγένετο)." Only Luke adds that the sun "was darkened (ἐσκοτίσθη)". But darkness can come as a result of heavy cloud and not necessarily a sun-eclipse, the effect of which is milder.

Perhaps non-indigenists can finance a similar project in an independent planetarium and see what results are produced. We may thus enter into a new version of star-wars. Now let us return to Agrawal and archaeology.

8. The Takṣaśilā excavations. Convinced as Agrawal is that IAs and Harappans are different peoples, he asks (me) "how do we explain that the substratum of Indian Culture is mainly based on the IVC"? I have nowhere so much as hinted that the situation is different. B. B. Lal, who, like me, turned to indigenism late in life, provides ample evidence for the Harappan-Hindu continuity (2002, passim).

Agrawal lists many items of this continuity, then gives evidence of change after 2000. Among the latter are listed millets and rice, which other scholars regard as items of the Harappan agriculture (Kenoyer 1998: 163; Lal 1997: 159-60, both with references), but also lentils and leguminous plants which were absent in the Harappan record. He also includes as "evidence" what are interpretations of actual evidence. Thus he mentions "Lack of control from the metropolitan centers" and writes "Between the urbanized societies of Harappa and Ganga civilizations, the interregnum (2000 and 600 BC) was marked by chaos and disorganization". The recent excavations at Takṣaśilā/Taxila may necessitate important adjustments to such interpretations.

In DAWN, a newspaper in Karachi, on June 2, 2002, appeared the following report by Mahmood Zaman: –

"*Taxila 600 years older than earlier believed.* Recent excavations at Taxila have pushed back the history of the ancient settlement by another six centuries to the neolithic age. Earlier, artifacts collected by Sir John Marshal had dated Taxila back to 518 BC. The new study also indicates the existence of cities in the valley between 1200 BC and 1100 BC. Potsherds and other terracottas, found at the lowest occupational level, 15 feet in depth, is the main evidence of the latest discovery which establishes that Taxila and the Indus Valley Civilization settlements of Mohenjodaro and Harappa existed almost simultaneously. Sir John, who excavated several Taxila sites between 1913 and 1934, had found four occupational levels. The latest study has unearthed six occupational levels which have been listed afresh as pre-Achaemenian, Achaemenian, Macedonian, Mauriyan, Bactarian Greek and Scythian. Archaeology Department and the United Nations Educational, Scientific and Cultural Organization, have also found for the first time an intergrated drainage system comprising open as well as covered drains. The discovery of several wells also establishes that fresh water was used for cooking and bathing." There are more details in this report about king Ambhi's palace (Macedonian period) and about later periods, but these fall outside our discussion.

In the absence of detailed studies by archaeologists, no secure conclusions can be drawn from this report. But, at first sight, it suggests that the chaos or disorganization may not have been so great as is generally assumed; for the 12th century falls in the middle of the interregnum 2000-600 BC. The 12th century is the period when, according to the AIT, the Aryans are moving in and are composing the *RV* – which knows nothing of urban structures or ruined towns.

9. Enter the Aryans? Agrawal admits that there is no clear "archaeological identity of the IA" and so he calls them "elusive IA", but finds circumstantial evidence for the entry of "new elements" (=new people speaking an IE language?). He adduces 5 different points: (a) the termination of the Bronze Age in Iran and the migration of speakers of Indo-Iranian; (b) agriculture and agricultural terms; (c) the horse and spread of pastoralism; (d) the evidence of the plant *Ephedra* (=Soma), suggesting that the Soma cult *may* have had its origin in southern Siberia; (e) new DNA evidence showing "the possiblility of the migration of "a male dominated population" (mainly Kshatriyas and Brahmins). I leave out (a) and (d) because they are so very circumstantial as not to carry any weight at all.

§10. In his brief, lucid Comment **Meadow** presents his own view of the evidence for horse. In his §4 he asks about my references to wild and domesticated horse in G.R.Sharma **et al** 1980 and Alur 1980. However I had not consulted this publication but a different one – G.R.Sharma 1980, as given in my Bibliographies (K 2002 and present).

He states that the bones from the Gangetic basin dated by Sharma at 6570-4530 (K VII, 1, end) were redated within the 2nd millennium. Here I must express a doubt because D.K.Chakrabarti (Cambidge, England), who is a prime authority on Gangetic archaeology, states that these bones have been subjected to further tests and the earlier date c5000 has been re-established (1999). For a non-specialist like myself it is difficult to decide between two so eminent scholars.

However, Meadow also states that Sahu 1988 (*non vidi*) is still a useful source for horse remains. This I must challenge because there have been several reports of horse-bones in various IVC sites after 1988. Even in Meadow's last sentence "As far as I am concerned, there are yet to be convincingly identified bone of the true horse (*Equus ferus* and *Equus caballus*) from South Asia that can be securely dated before about 2000 cal BC", the key phrase "that can be securely dated" suggests there are horse remains that *may be* older. The following reports are not in Meadow:

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

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

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

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

11. Agrawal regards the horse and its use as (circumstantial) evidence for the entry of the IAs. He writes: "It was in [?] the nature of pastoralism (2000 BC), which gave rise to repeated out-migrations. The mounted herder could cover long distances in a day, which would lead to yet more geographically extensive pastoral circuits and larger flocks." So, it is suggested, the Aryans enter bringing this kind of pastoralism and lentils and leguminous plants. Notice that invasionists stress the "nomad pastoralism" of the Aryans. But in the *RV* there is much evidence suggesting they were settled cultivators also. The hymn to *Kṣetrapati* 'Lord of the Soil/Field' (IV, 57) alone should suffice; then, the girl Apālā refers to her father's *urvarā* 'fertile field' (VIII 91, 5) and we find cultivation implements *khanitra* 'shovel', *lāṅgala*, *sīra* 'plough', *sṛṇī* 'sickle', etc. They were also weavers with loom, shuttle, warp and woof (*RV* I, 134, 4; II, 3, 6; VI, 9, 2-3; X, 130, 2; etc).

All this does not indicate the entry of a new people. The appearance of some new plants does not remotely necessitate an immigration: they could have been brought by traders. Agriculture was well developed in the IVC, as is attested by the granaries at Harappa and Mohenjodaro and jars in private houses (Kalibangan): there was cultivation of wheat, barley, cotton etc. So agriculture can be discarded. Pastoralists (with horse-mounted herders) must also be discarded for similar reasons. Even if we accepted Meadow's view and the horse (and the two-humped camel) were a new element, this, like seeds or plants, could have been brought by traders. Animal husbandry in the IVC was just as advanced as agriculture. Sheep, goats and cattle are amply attested. Then, the Harappans could have been practising mounted herding with animals other than the horse: sheep, goats and cattle move slowly. So the innovation of the horse would be no indication let alone proof of an immigration. But, as we saw, the horse is well attested now.

12. Kuz'mina's Comment introduces a different aspect in an attempt to show the Aryan entry. In its last section, it mentions a distinction between śudras who, as aboriginal inhabitants, produced wheel-made pottery, and the Aryans who, as new-comers, produce "hand-made ritual vessels". The validity of this distinction is questionable on the evidence of ancient Indic texts. In ch 16, st 27 of the White Yajur Veda (*Vājasaneyī Saṃhitā*) there is a reference to carpenters, car-makers, smiths and potters (*kulāla-*). In ch 30 of the same we find mentioned some forty professions and crafts; fine distinctions are made between, for instance, such professions as elephant-keeper *hastipa*, horse-keeper *aśvapa*, cowherd *gopāla*, goatherd *ajapāla* and shepherd *avipāla* (st 11). In the same ch 30, st 7, are mentioned the bow-maker *dhanuṣkāra*, the arrow-maker *iṣukāra* and the bowstring-maker *jyākara* as distinct craftsmen. But no distinction is made anywhere between hand-potter and wheel-potter: in this st 7 is mentioned the potter *kulāla-* and in st11 the son of a potter *kaulāla*: the potter is regarded as a unitary craftsman. Moreover, common experience shows that it is the hand-potter who needs to learn the difficult use of the wheel; any skilled wheel-potter can easily make pots with bare hands and does not need foreigners to instruct him. Indeed, Jarrige reports that at Sibri were found from Harappan levels an amulet with Indus script and "coarse handmade pottery [making] up about 30 per cent of the ceramic assemblage, the remainder... having been formed on the fast wheel" (1985 with references).

Kuz'mina mentions also "The adaptation of the Aryans to a foreign ecological niche and their borrowing of the aboriginal material culture (Allchins R and B 1997: 221-222)". This has no validity because, as I pointed out (K IV, 2), the Allchins adduce no evidence of entry: they had found that several Harappan elements are similar to elements mentioned in the "later Vedic Literature" (1982), now state that the Aryans' presence is not attested in the archaeological record and **hypothesize** that the acculturation took place **before** the Aryans entered (1997). Such conjectures may be convenient for the AIT but are totally unconvincing: they are scraping not the bottom but the outside of the barrel. (Loan words are considered in §20, below.)

13. Zimmer complains that K 's "only argument repeated throughout is that there is neither archaeological evidence for an invasion nor allusions to such an event in the *RV*;" he also sees "no sense in K's polemics around 'invasion' and 'immigration'." It is not true that this is the only argument I offer, but Kuz'mina makes a statement which shows the importance of this argument and explains my "polemics" around invasion and immigration. She writes: " N Kazanas *fairly underlines* the absence of traces of a mass invasion of the Indo-Aryans and continuity of the 'indigenous culture of India' " (my emphasis). Obviously, if there are no "traces" of entry how can we reasonably claim that the entry occurred?

Kuz'mina thoughtfully uses the term "invasion". For invasions imply battles and these invariably leave easily discernible traces, like destruction of buildings and skeletons with wounds from arrows or other weapons. Thus we come to another important argument – one which no Comment except Bryant's and Witzel's attempts to meet:

> If there was no mass-invasion, how did 2 or 3 waves (so Allchins following Parpola, 1997: 221-222) of peaceful immigrants accomplish the "Aryanisation" of the vast expanse of the IVC?

I gave many cases of small waves of peaceful immigrants, and even of invaders, that failed to achieve such a result (K IV, 3). I also mentioned that sometimes even with a complete conquest this result does not arise, as in the case of the Greeks who retained their religion, language and place names despite 400 years of Ottoman rule. It is not enough to show that there might have been a peaceful intrusion. Non-indigenists must also explain satisfactorily how these peaceful intruders effected a result which only conquest could accomplish.

The Harappans had agriculture, animal husbandry and wheel-made pottery (which produces a much larger variety of pots than the hands-only craft). So agriculture, pastoralism and pottery had nothing so very startling as to compel the Harappans to accept Aryanization.

In sum, there is no substantial evidence of an Aryan entry at c1500.

14. DNA. Agrawal writes: "The new DNA evidence shows that there is a significant male Eurasian element in the DNA of the Kshatriyas and the Brahmins, which again shows the possibility of the migration of a male dominated population". He gives no reference except a paper by M Bamshad *et al* (2001) in his bibliography. However two important questions remain here: at what date and in what manner entered the new DNA.

I accept the scientific value of the Bamshad *et al* paper (2001) but three points stand out. (a) It is not as neutral as Meadow's paper is on the horse evidence – just giving the facts. It seeks to find support for the AIT. It starts with the assumption that there *was* an entry at c1500 (see point c). (b) On p 999, Brahmin, Vaiśya and Kshatriya are given as "upper castes" while Yadava and Kapu are given as "middle castes" yet on p 1002 they are classified as "Sudra" (=*śudra*?). Now as far as I know, the Yadus were kṣatriyas and one of their scions was Kṛṣṇa of the *MBh*. What Yadus are these non-kṣatriyas? Then, it is assumed that today's kṣatriyas were kṣatriyas 3000+ years ago. In any case, the sample size is too small, restricted to one district in coastal South India. Finally, who are the "Europeans" responsible for the DNA entry? (c) On p 1000 we read of dates: "West Eurasian admixture in Indian populations *may* have been the result of more than one wave of immigration into India. Kivisvild et al. (1999) determined the coalescence (~ 50000 years before present)... Our analysis... is consistent with *more recent* West Eurasian admixture. *It is also possible* that haplotypes with an older coalescence were introduced by Dravidians... *Alternatively* the coalescence dates of these haplotypes *may predate the entry of West Eurasians populations into India*" (my emphasis). Obviously, the paper suffers from methodological faults and great uncertainty.

P K Manansala examines another paper (Sept 2001, signed by Ramana GV, Su B, Jin L, Singh L, Wang N, Underhill P, Chakraborty R) of the Human Genetics Center, University of Texas, Houston, and writes: "This study, along with a recent conference paper, both contradict other recent studies suggesting that there has been little gene flow between castes and between castes and tribes... It basically states that there is little evidence of caste integrity over wide regions in India...

[E]ven within a confined area there is much evidence of back and forth *male* gene flow between castes and between castes and tribes. Studies like Bamshad et al which support AIT depend on caste boundaries protecting the integrity of the genetic structure of the invaders over thousands of years" (Oct 2001).

Obviously, the plot thickens and leads to biological warfare.

In 2001 Witzel and I had a joust on the Internet; he refers to this in his Comment. I wrote then (much as in K IV, 1 n 9): "Personally, I distrust such [genetic] finds. I examined the study of Cavalli-Sforza, Menuzzi and Pizza (1996: xii, 5, 29, 32, 88 etc) and found it to be full of difficulties and several inconsistencies (Kazanas 1999: 18-9). Lord [Colin] Renfrew also had pointed out that there are 'difficulties of methodology not yet resolved' (1997: 88). Consequently it seems best to wait a few more years until these difficulties get resolved and the methods become fully reliable" (Kazanas 2001a: §7). To this Witzel replied: "K finds – as a Sanskritist – faults with one of the leaders of the genetic studies, Cavalli-Sforza. Fine. We will wait for K's first genetic paper" (2001b: p4, §7). Note here that nothing is said about Renfrew who also is not a geneticist.

However, in his Comment (§6.4),Witzel now writes: "The temporal resolution of these [genetic] studies [i.e. Bamshad *et al* and others] is currently too imprecise to say anything definitive about population movements in any given historical millennium". It is good to see this change in Witzel's thinking. Then he continues – "there is no doubt that a mass of evidence is accumulating, *pace* Kazanas, and his colleagues, that do nothing to support the fictional idea of "Mother India" being protected from normal migration movements..." I suspect here he confuses mine with some other paper; for I mention nowhere "colleagues" or "Mother India" and admit the possibility of an entry at c4500.

In conclusion, Agrawal's DNA evidence must also be discarded. That there were influxes on the Indian subcontinent at different periods is undoubted. The dates remain uncertain and, at present, there is no evidence for any influx c 1500. An additional difficulty is the mode of entry. This may have been due to migrations, traders, bands of marauders, prisoners, or whatever else.

E. Bryant

15. Bryant's Comment is quite different from all others in that it evaluates the evidence and arguments I presented rather than criticize them outright or circumvent them. "All in all, I cannot fault Kazanas for feeling the need to undertake a critique of the evidence supporting the Aryan Migration hypothesis. In my view, the Indo-Aryan invasion/migration theory, at least in its present forms, as well as the dating of the Vedic texts, remain unresolved issues that invite unbiased fresh scrutiny." Bryant requests that I outline my own rationale for opting for the Preservation Principle. I do so in my reply to J P Mallory's Comment, which raises specific objections.

I apologise to Bryant for my oversight of his remarks on Hock (on p 151) and for my charges against his own work. It does lay out the history of the ideas connected with this subject. I was carried away by his occasional judgment of the evidence, as when he writes: "This does not mean that the Indigenous Aryan position is historically probable. The available evidence by no means denies the normative view – that of external Aryan origins and, if anything, favours it" (p 11). But, of course, in a book of over 300 pages judgments are bound to slip in. I also take in good spirit his strictures about my lack of cordiality betrayed in phrases like "preposterous proposition" and the like. But I beg to differ when he says that I arrogate to myself the competence of archaeologists and astronomers. I did no such thing. The distinction I made was that whereas linguistics gives only relative chronologies, archaeology and astronomy can and do establish (near) absolute dates.

16. I also differ from some other observations made by Bryant. He writes: "As for Kazanas' second complaint, I stand corrected in my statement that the Greeks and Scandinavians do not preserve record of their early movements if, as he claims, they unambiguously do." Now, why use the phrase "if, as he claims" that casts doubt on my statement? After all, I give the few necessary references. The other Comments express no doubt.

17. Commenting on my treatment of the Hittite records, which preserve no mention of a migration, he writes: "Kazanas compensates for this by noting that eminent Hittitologists consider them intrusive on other grounds[5]; needless to say, whatever might be Kazanas' opinion of their arguments, eminent Vedicists likewise consider the Indo-Aryans to be intrusive despite a similar absence of migratory records." This is true on face value but I mentioned (K III, 3) not only hittitologists like the linguist J Puhvel but also historians like O R Guerney and W E Dunstan and this makes an important difference. Certainly, vedicists and many proto-historians (even in India) regard the IAs as intrusive. This is to be expected since the AIT is the established doctrine. But archaeologists specializing in the area do not – and they are many: J-F Jarrige 1980, 1985; D L Heskel 1984; J Shaffer 1984; R and B Allchin 1997; J M Kenoyer 1998; J Shaffer and D A Lichtenstein 1999. They all stress the cultural continuity and/or expressly deny any significant entry. Even Agrawal admits the Aryans to be archaeologically "elusive".

18. This links with another statement at the start of Bryant's Comment: "While the Indological consensus of an Indo-Aryan invasion or migration into the Indian subcontinent has long been taken for granted, at least in western academic circles, it has been ferociously contested among Indian scholars over the last decade or so (and on the margins of academia in India for well over a century)." But surely all the archaeologists mentioned above are also indologists and belong to Western academia. Why ignore them? This would be a quibble and I would bypass it if it concerned Bryant alone. Unfortunately many scholars (Huld, Kuz'mina, Zimmer) refer only to Indian nationalists/fundamentalists and do not mention all these western archaeologists. This omission indicates the subtle habit (long established) of thinking that Indology is the concern mainly of vedicists, linguists and comparativists.

19. In connexion with Hittite, I am aware of the Indo-Hittite school of thought, which Bryant mentions - and others, including Renfrew (1999). It is true, Sanskrit has both losses and innovations and these phenomena are already observable in Vedic. Some apsects of the Hittite culture make me extremely skeptical about the antiquity of the language as a whole and about Anatolia being the original IE homeland.

Undoubtedly, Hittite retains archaic features like the stems with alternate *r/n* suffixes (Misra 1968: 70-1; Burrow 1973: 127, 226). These are, of course, present in Sanskrit also (eg *ahar/n-* 'day', etc). Bryant mentions the laryngeals, which have gained general acceptance despite some few dissents. But the laryngeals have been traced also in Sanskrit (Szemerényi 1990: 143-4, 291; Hock 1991: 587-8).

With regard to the reduplicated perfect in Greek and Sanskrit – how is this an innovation? Yes, there are arguments for the innovation. But then Szemerényi traces several forms in other IE branches (1990: 289-291) and states that a "systematic elimination of reduplication took place in

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[5] Witzel gives a curious twist to this point about records (§6, n 33). I wrote, as Bryant notices, that the Hittite records mention no immigration, but Witzel criticizes this fact because "even in ritual the local 'foreign' language Hattic was still used."He equates records not mentioning an immigration with the use of Hattic (which indicates indirectly an immigration and which historians, whom I mention, do take into account). Mischievous?

Germanic" (p 291). Md Greek has lost this reduplication in conjugation retaining it only in passive perfect participles (eg πεπραγμέν- 'what has been done', etc). The perfect is now formed with the auxiliary 'have'. So I think here Hittite has suffered a loss – as also with the dual.

The absence of feminines in Hittite is considered by many an archaic feature, and, consequently, its presence an innovation in the other branches. But, again, who can tell with certainty?... J Puhvel wrote: "It is not impossible that such feminines once existed in Anatolian but failed to maintain themselves" (1991: 57). Others estimated that Hittite was on the way to becoming genderless (Lockwood 1972: 269). To explain the feminine gender as an innovation in the other IE branches we must suppose that they all moved out of Anatolia to another location, stayed there long enough to develop the feminines and then moved away to their historical habitats. This of course nullifies the Indo-Hittite idea. That every branch developed the feminines independently seems improbable. If Hittite and Ancient Egyptian (Gardiner 1957) and modern Lithuanian and French function so adequately with two genders why should all the others suddenly develop a third one? Then, we have the curious coincidence whereby the cognates S *ūrj-* 'power', Gk ὀργή 'implulse' and O Irish *fer-c/g* 'rage' are all feminine; also S *uṣas*, Gk ἠώς, L *aurora*, all 'dawn' fem. Finally, the distinction between male and female must have been observable in humans and animals even in most ancient times. So it is more likely that the feminine gender was present as early as the masculine one. Here again Hittite suffered a loss.

Anatolian lacks the common IE stem for the horse; by itself this would not matter. But it lacks also the stems for mother, father, son and daughter, which the other branches have – and some, like Sanskrit or Germanic, all four of them. Of the IE deities it retained the theonyms *D-Siu*, *Agnis* and perhaps *Inar(a)*. Otherwise the Hittite religion and culture, like the greater bulk of its vocabulary, is non-IE. C. Watkins sums up the situation nicely: "The Hittites were from the earliest times exposed to the influence of other languages each of which had literary tradition… [They] were profoundly influenced by Mesopotamian culture as mediated through the peripheral Akkadian.. and by the contact with the Assyrian merchant colonies of the 19th and 18th centuries... The major cultural influence, at least in religion and cult came from Hurrian... [resulting in] the Hurrianization of the Hittite pantheon" (1995: 52-53).

If the Hittites were indigenous how did this situation come about? When they emerge into the light of History, they are strong conquerors who form a mighty kingdom and then a large Empire. The heavy cultural losses they suffered probably occurred long before – I think – on their way to Anatolia. They were (I am speculating) an elit-dominance group that, like the Scandinavians in Normandy and Kiev, succumbed to the local culture(s).

20. One final point. Bryant writes: "There is thus no doubt (and Kazanas himself begrudgingly accepts) that the Vedic texts themselves attest the existence of Dravidian and Munda... therefore it is not quite correct to state that 'Vedic was an intruder when no other language of equal age was attested".

There is nothing "begrudging" about my acceptance of the possibility of loan words in Vedic. The acceptance is clearly stated. I simply took a long time to come to the point because I discussed other topics of interest. I gave some examples and also stated that I had examined this issue before: "As I pointed out on the Internet..." etc. If something is repeated, it cannot be said to be "begrudgingly" accepted. The Aśokan rock-inscriptions are of the third century BC. This is a secure date and from this it has been calculated and is now universally agreed that the Aryans came to India at the very latest c1500-1200 BC; so Vedic is of that period. Dravidian is not attested before, say, 200 CE (Marr 1973: 31-32) and Munda is much more recent. As for the unknown language, it remains unattested. So *it is correct* to say that "no other language of equal age [as Vedic] was attested". Then, I cited three Vedic words *khala* 'threshing floor', *bila* 'hole' and *lāṅgala* 'plough': the two are of uncertain derivation but have no cognates in Dravidian or Munda; *lāṅgala* was hypothetically linked with Munda in Mayrhofer's KEWA but in EWA is left with *Unklar*; *Fremdwort ?*. There is thus strong uncertainty and it is this that I strove to bring out. One could also argue that these words were PIE – lost in the other branches but retained in Vedic. The other languages could have been "intruders" and perhaps also borrowed from Vedic – eg V *bala* 'strength' (Gk *βελ-τίων* 'better'; L *de-bilis* 'strength-less'; Sl *bol-ij* 'greater') and Drav *bal-/val* .

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21. Parpola does not make it clear whether he himself believes or not the scholars of South India who claim that the Vedas were created there rather than in the North. From what I know, it does not seem that Vedic people in the South preserved much of the language or kinship rules. There have been of course Brāhmaṇa families that passed some ritual from generation to generation but the society generally followed its own language(s) and social customs, and worship local gods. Then, many major Brahmin communities in South India have a strong tradition that they originated from parts of the North (Nambudiri 1992: passim). I assume Parpola does not fully believe this claim since he states that the river Sarasvatī is the river in the North. But then why mention the claim?

With the river Sarasvatī I deal later under Witzel. Parpola connects it with the name Bṛsaya (*RV* VI 61, 3) which he thinks, following Hillebrandt, is that of Iranian chiefs comparable to Barsaēntēs, name/title of the Satrap of Arachosia in the times of Alexander. This is extremely doubtful, and Mayrhofer rejects it (1956-). In *RV* VI, 61, 3 Bṛsaya is described as *māyin*, one who has occult/magical powers: this suggests a demonic figure. The name Bṛsaya occurs also in *RV* I, 93, 4 (no connexion with Sarasvatī). This hymn is addressed to Agni and Soma and immediately after the destruction of Bṛsaya's brood, there is mention of finding the light for many (*jyótir ékaṃ bahúbhyaḥ*). So Bṛsaya is a demon of darkness.

Parpola doubts the archaeoastronomical finds I give on three grounds. First, he refers to the different dates assigned to the same textual references; but here we have to consider the big difference between computations done on paper and those done with the technology of a modern planetarium. Second, while it is true that a text may have been written (revised or completed, I would say) at a later date, an earlier date may determine its inception. His third reason is the absence of the Vedic nakṣatra calendar in other IE traditions – which means, according to him, that the IAs probably inherited this calendar from the IVC "after their arrival... in the second millennium BC". Is this so?... In *The Druids* Peter Ellis writes of the earliest Celtic astronomy having a system of 27 nakṣatra divisions as well as intercalary months (1994: 230-31). So this system was not unknown in other IE branches. (Chariots, the OIT and my Preservation Principle which Parpola also mentions are discussed later.)

22. I have already shown that **Kuz'mina's** attempt to get the Aryans into Saptasindhu was not successful. Her Comment presents other evidence, much discussed by many scholars – about chariots, horsebreeding, toponomy of the Steppes etc. Truth is I am not equipped to evaluate it all. But it cannot have escaped Kuz'mina's attention that some other scholars, aware of all this evidence and equipped to evaluate it, still insist on the Anatolian homeland (e.g. Sarianidi 1999 and Renfrew 1999; 2000: 28-30). So what we are dealing with is not simply evidence but evidence organized and interpreted in different ways.

I do not, as Kuz'mina states, "embrace the hypothesis of a group of nationalistic Indian intellectuals of the [19]90s." For me, it started in 1987 when, in trying to get a picture of Indian proto-History, I thumbed through the thick volume *Frontiers of the Indus Civilization* by B. B. Lal and S. S. Gupta (eds, 1984). There, writing of the Iran-Indus connections, D.L. Heskel had a statement that made a deep impression: "It is also evident that previous theories of wholesale population migration and invasions... are not acceptable in the light of archaeological evidence" (p

343). Then followed writings by other (Western) archaeologists (§ 17, above) who all declare – "No invasion". Of the Indian nationalists I learnt only 3 years ago from one of Witzel's publications (K XI, 1 also §32, below).

Kuz'mina writes: "References to archaeoastronomy and the date of 3067 BC of the battle of Kurukṣetra seem anachronistic in the age of radiocarbon dating." Radiocarbon dating and archaeoastronomy are not in opposition: the first deals with material artifacts and the second with star positions. Radiocarbon analyses cannot determine the dates of the celestial phenomena described in the Indic texts. Moreover, the date of the Mahābhārata War is still uncertain in mainstream circles (15th or 9th century BC?) and cannot be *directly* determined by radiocarbon analysis which dates artifacts that provide only circumstantial evidence. Archaeoastronomy dates directly the celestial phenomena mentioned in the epic.

23. Mallory's Comment falls into two parts. In the first Mallory (hereafter = M) refutes my Preservation Principle and postulates instead the Total Distribution Principle. In the second he examines the OIT, sets up some possible models of this and then knocks them down thus demonstrating the grave difficulties of the OIT. The Comment contains other objections as well.

(a) M states: "I am cited [by K] as a source (Mallory 1989) for demonstrating a foreign origin for the Balts, Celts, Romans and Slavs yet I am obviously not a reliable source when I write about the Indo-Iranians." The first part of this statement is correct (K III, 4) but I did not write anywhere that he is "unreliable" about the Indo-Iranians. I can only assume he refers to my statement regarding the absence of archaeological evidence for the entry of Aryans (K IV, 4), "Many have done much earth-digging and book-research but have found nothing (eg Mallory 1989: 46ff and 227-229)." In concluding his survey of the evidence M wrote (1989: 229): "it is not easy to make a simple appeal to the Andronovo culture to resolve all the issues of Indo-Iranian origins... When the archaeological evidence becomes so opaque then our only refuge... is probability and a little intuition". M himself thought the evidence uncertain. For my part, I regard his remarks quite reliable[6]. So, I am not, as M says, "more than a bit disingenuous".

His opening request that my thesis be rejected is unwarranted. This would have a basis only if he found evidence for an Aryan entry c1500.

(b) M rightly ridicules "native folk traditions" which speak of origins – "Romans derive from Aeneas at Troy, Germans come from... Noah, Irish was manufactured at the Tower of Babel". With this he seeks to undermine the fact that the Indic texts mention no migration. However, I did not write about "folk traditions" – excepting Virgil's Aeneas – and in fact rejected the Balts' late "tradition" of descent from India (K III, 3, n8). The Hebrew, Avestan, Greek and Irish records (speaking of "5-6 waves of immigrations but no place of origin") are hardly "folk traditions"; nor do they seek, as M writes citing Poliakov, "to re-invent themselves to enhance their status". Even Virgil's tale and Sturluson's mention of Troy (probably in imitation of Virgil) indicate a dim memory on the part of the Romans and Scandinavians respectively. After all, both peoples *did* move.

Fact is that the Indoaryan records contain no memory of a migration. We certainly cannot rely on this *only,* especially since the Anatolians also preserve no memory of immigration. But just as there are other data and arguments supporting an Anatolian entry, there are also data and arguments against an Aryan entry at c 1500. The silence of the Indic texts is, as Bryant puts it, "a valid ingredient in a complex picture".

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[6] In a later publication M wrote: "While a good case can be made for an expansion of Pontic-Caspian pastoralists onto the Asiatic steppe, and perhaps also into the belt of central Asian urban centres (Parpola 1988), it is still difficult to demonstrate movements from the steppe into the historical seats of the Indo-Aryans and the Iranians of Iran itself" (1997: 113). Here again M himself finds no evidence.

24. M is right in attacking my Preservation Principle and in pointing out that we should take into account, apart from migration and substratal/adstratal influences, factors like social change and time differential between sources. But his assumed corollary correlating retention/loss to small/great distance of travel is unwarranted. I implied no such correlation because even if the distance is very small the losses may be numerous on account of any number of factors, such as the receptivity of the people concerned, actual subjection, dislike of own traditions, attractive influences, etc. I shall disregard this aspect. Now it would be foolish to deny that both social change and the time differential between sources are significant factors for determining changes and losses in the culture of any people. The Zarathustra reform in Iran and the influx of Christianity in Europe had far-reaching effects which cannot be ignored. On the other hand, how important in reality are these factors in the case under discussion? I shall show that, they are not all that important.

We must note two more considerations. First, in supporting the antiquity of Sanskrit with the Preservation Principle, I appended the inner organic coherence of the language (i.e. roots generating primary and secondary derivatives) and the preservation of elements that "explain lacunae in other branches and are derivatives of roots having other verbal/nominal cognates within Sanskrit itself" (K VI, 4).[7] *No Comment refers to this.* The second consideration is the oral tradition which I did not discuss in my essay.

25. That the IE peoples maintained an oral tradition is beyond dispute since, otherwise, we would not know much about them before the period of literacy, say 600 BC in Greece and Italy and 500 CE in the rest of Europe. The systematic oral transmission of its voluminous sacred lore is a most impressive characteristic of the Vedic tradition: it was the sacred duty of certain families to transmit this knowledge from generation to generation (Winternitz I, 29-32, 51-2). Caesar reports a similar tradition among the Celts: "In the schools of the Druids they learn by heart a great number of verses, and therefore some persons remain twenty years under training. And they do not think it proper to commit these utterances to writing, although in almost all other matters... they make use of Greek letters" (*De Bel Gal*, VI, 14). The Greeks too maintained an oral tradition in the period of non-literacy (12th – 8th cent) and, in some 'esoteric' cults like the Orphics and the Pythagoreans, well into Hellenistic times (Murray 1993: 100; Kingsley 1995: 322ff). Even the Hittites must have had one before they emerged fully with their own records from c1620. But their tradition must have been extremely eroded since it preseved few IE elements.

Very few Indian manuscripts survive from before the 10th century CE. So in this respect the Indians are not better off than the Europeans and are certainly worse off than the Greeks and Romans. The Vedic texts were preserved largely, if not wholly, through the oral tradition (Winternitz I, 31-34).

The question arises: how and why did the IAs manage to organize their own oral tradition in such a superior mode? There may be several factors involved but one of them must have been immobility. People on the move for thousands of miles have little time to organize and/or maintain such a systematic tradition.

26. In Gaul and/or Britain the Celts had IE *Ariomanus* (=Irish *Eremon*), *Epona* and *Andarta/Andrasta*; also perhaps *Brigantia* (=St Brighid later in Ireland) who may be cognate with

[7] Here again Witzel gives (§6.1, n 42) a trickish twist to my words and those of Burrow. I quoted not only Burrow's "[Skt is] in most respects more archaic and less altered", which Witzel cites, but also Burrow's "Chiefly owing to its antiquity the Sanskrit language is more readily analysable, and its roots more easily separable..." (K VI, 3) which Witzel ignores. **No linguist challenged this.**

Rigvedic *Bṛhaddivā* (Kazanas 2001: 281, n11). But at that same period they had many non-IE theonyms (eg *Belenos*, *Cernunnos*, *Esus*, *Taranis*, etc) and some loans from the Romans, like *Mars*, *Mercury*, etc. So, while it is true that if we had written records we might have more inherited theonyms, it is obvious that the Celts had started adopting deities from other cultures very early.

M suggests that the horse-deity Epona may be an independent development and not an inherited element. This might apply also to the Mycenaean *Iqeja* (=horse-deity) since the Mycenaeans had the stem *iq-/ik-* for 'horse'. In this latter case, however, in the later literature we find the twin horse heroes Castor and Poludeukes who have many traits common with the Vedic Aśvins; we also find in Arcadia the mythologem of Demeter Erinys who took the form of a mare to escape from Poseidon's pursuit much as Saraṇyu did in *RV* X, 17, 1-2, *Nirukta* XII, 10, and *Bṛhaddevatā* VII, 1-6, and gave birth to the Aśvins. So the Mycenaean Iqeja would seem to be inherited. In the Celtic case, no independent stem *epo-* for 'horse' has survived in any of the sub-branches, the only IE one being Old Irish *ech*. Then, in the later Irish literature we find again many mythologems that are similar to Vedic and other IE horse-motifs (O'Flaherty 1980). So here again the available evidence suggests inheritance; but I do not insist.[8]

In sum, the perusal of any publication on early Celtic mythology shows that it is not names of deities that are lacking but information about them. Theonyms are as many as in India or Greece: the few IE ones are those I have given; the vast majority are non-IE or borrowings from the Romans.

27. Like the Celts the Greeks too absorbed many non-IE theonyms. This is evident in the Mycenaean Documents where we find some of the later non-IE names like Athena, Hera and Poseidon but also a host of others like *Drimios* (son of Zeus), *Emaa*, *Ipemedeja*, *Manasa* (Chadwick 1976; Burkert 1977). Here also we find the IE theonyms *Areimene*, *Erinus* (=V Saraṇyu), *Iqeja* and *Zeus*. One cannot protest that the Greeks are at a disadvantage since the works of Homer, Hesiod, Pindar and the dramatists, constitute a bulk very much larger than the *RV*.

In the case of the Balts and Slavs, certainly, the sources are late. The influx of Christianity must have swept away much early material and we cannot draw definite conclusions from the extant evidence. But even here many theonyms are pre-Christian non-IE or Roman loans, like *Andaj*, *Teljavel*, *Markopotis*, *Svantovit*, *Rujevit,* etc (Blažek 2001; Puhvel 1989: ch 12); so the changes started early.

Like the Greeks and the Celts, the Germans (all sub-branches) had numerous names but few IE ones. To my list of 15 names we should add two more – *Mannus* and *Twisto* (Tacitus, *Germania* 2): Mannus is obviously cognate with V *Manu* and, against the general opinion, I link Twisto, father of Mannus, with V *Tvaṣṭṛ*, who begets mankind *RV* III, 55,19. Thus from a new total of 17 the Germans have 7 names. The non-IE theonyms are more numerous: Thor the Thunderer (in Scandinavia; Donar in Germany; Thunor among the Saxons); bright Balder and his consort Nanna; Loki of mischief; Ull(r) the archer; Aegir of the sea, goddess Idunn with the apples of immortality, and so on. All these can hardly be said to owe to Christianity their presence in the German pantheon. Thor with his magical hammer is the Germanic equivalent of Greek Zeus and Slavic Perenu (=Baltic *Perkunas* and V *Parjanya*): apart from anything else, all three of them perform the function of the dragon-slayer (like V Indra and Hitt Inar). But long before Christianity arrived the non-IE name 'Thor' displaced the IE ones due to adstratal/substratal influences. In Tacitus's *Germania* and *Annals* already we find, apart from the Roman Mercury, Mars and Hercules identifications with (probably) Wodan, Tyr and Donar, more non-IE theonyms like *Nerthus*,

[8] The cognates for the Firegod (which M also mentions as an independent development) are found in Vedic, Hittite *Agni-* and Slavic *Ogon*. Since 'fire' in Hitt is *pa-aḫ-ḫur* while the theonym is *Agnis*, the cognates indicate an inherited form, otherwise the theonym would have been related to *paḫḫur*.

Tamhana, *Nehalennia*, etc. (See also Davidson 1964.) Thus, although the Germanic sources are late, the evidence of the theonyms indicates that losses of IE ones occurred early.

My choice of the names of deities is not quite so arbitrary as M suggests.

28. M prefers the Total Distribution Principle. In a way, in certain circumstances, so would I. But this principle can be misleading. In the total vocabulary of any language there are *variables* and *non-variables*. I use the terms in a relative sense. For instance, pots are made from different substances (clay, wood, metal) and in different shapes (bowl, jug, pitcher, urn, etc). The words describing them can over a period shift in meaning and the word “bowl” may come to denote an urn or vice-versa. To the same category belong food-stuffs, clothes, tools etc: (different) people in different circumstances eat different foods, wear different clothes and so on. On the other hand, man’s eyes and feet are non-variable and the words denoting them would not shift in meaning as easily as pots and pans. M mentions “arboreal terms”. But plants and trees are variable as one moves from one landscape to another: some pinetrees look like cypress-trees, these again like cedars or firs and so on. When people are on the move, the name of one plant could be applied after a period to a similar but different plant. Thus the evidence of “arboreal terms”, as indeed of all variables, would not be very reliable.

The list of theonyms belongs to the non-variables since people do not change their religion easily, except with coercion, duress or dissatisfaction. A Reformation will certainly bring about changes in dogma and ritual, but the religion will retain some or many older elements. The Iranians present Indra as a demon but the sungod Hvare (=V *svar-*) has swift horses and is the *eye* of Ahura Mazda as in the *RV* Sūrya has swift horses and is the *eye* of Varuṇa, while Yima is the son of Vivanhant as in the *RV* Yama is the son of Vivasvat. When peoples migrate the situation is different. If the migrants have literacy, there will be some changes but not highly significant (as with the Jews in Diaspora after the first century CE). If they have no literacy, as was the case with peoples in the second millennium and before, then their religion would undergo significant changes, however strong their faith or their oral tradition might be; for they would not have the leisure to pass onto the next generation all the details and they might easily absorb new alluring elements from other cultures . This is probably what happened to the various IE branches.

29. Non-variables would be generally words denoting things or activities that are constantly with people: parts of the human body (head, nose, shoulder, hand, foot etc); other people like man, woman and family (clan, tribe); relationships like father, mother, son, daughter, friend, enemy, etc; external objects like water, fire, stone, sun, moon, and phenomena like day, night, etc; animals like the bovine and equine species, goat, sheep, dog, worm etc; verbs like being, breathing, drinking, eating, excreting, speaking, hearing, seeing, standing, turning, going, dying, etc; states and qualities like new, young, old, heavy, light, etc. Obviously the list is not complete, but all such items should be constant, hardly affected by religious or social changes. Numerous losses or semantic shifts would be due to other reasons, one being migration.

As I mistrust reconstructed forms I wanted to test the figures M quoted for Greek retentions (2444), for Baltic (2376) and Indo-Iranian (2139). I examined only just over 50 of “non-variable” lexical items in Buck (1949), Pokorny (1956), Mann (1984/87) and Mayrhofer (1996). These authorities do not agree on the common stem of some few of these words (eg darkness, drinking) so I left them out. I left out some words (eg face, finger, hand, etc) which had no clear common stem in at least three of the major IE branches (ie Sanskrit, Greek, Latin, Celtic, Germanic, Slavic and Baltic; Celtic, Germanic etc include all sub-branches; Avestan was omitted because it is close to Vedic, and Hittite because it is not in the race). Words common to Sanskrit, Greek and Baltic were also left out since they would prove nothing (eg blood, jaw, tooth, horse, dog, water, sun, eat, sleep, etc). In the end I was left with 21 words that showed significant variations, i.e. absences in Sanskrit, Greek or

Baltic.

30. List of non-variables.

Respective abbreviations used are: S, Gk, L, C, Gmc, Sl and B. I give the form of the stem found in the various branches (at least 3) and finally state that it is not found in S or Gk or B, whatever be the case. In some instances I use the stem in Alb(anian), Arm(enian) or Toch(arian).

1. bone: S *asthi* (*asthnaḥ*); Gk *ὀστέον*; L *os(s)-*; Alb *ašt-* Not in B (Sl and Gmc).

2. head: S *śiras*; Gk *κάρα*; L *cere-brum* 'brain', *cer-nicus* 'head-first'; C *ker-n* 'top (of head)'. Not in B (and Sl).

3. ear: a) Gk *οὖς*; L *auris*; B *ausis;* C *au*; Gmc *eare*; Sl *ucho*. Not in S.

b) S (*śru-*) *śro-tra*; C *cloth*; Gmc *hliu-*. Not in Gk and B (but in both, verb-stems).

4. nose: S *nas-* ; L *nāris*; B *nosis;* Gmc *nasa*; Sl *nosĭ*. Not in Gk (ῥίς, ῥιν-ός)

5. mouth: S *ās-*; L *ōs*; C *ā*; Gmc *ōss* 'rivermouth'. Not in Gk and B.

6. shoulder: S *aṃsa*; Gk *ὦμος*; L *umerus*; Gmc *ams*; Arm *us*. Not in B.

7. knee: S *jānu*; Gk *γόνυ*; L *genu*; Gmc *kniu*. Not in B (Sl and C).

8. foot: S *pắd-*; Gk *πούς*, *ποδ-*; L *pēs*; Gmc *fōt*, Not in B (Sl and C).

9. man: a) S *nṛ-*, *nar-*; Gk ἀ-νήρ; Oscan *ner-um;* Alb *njer*, C *ner*. Not in B (Gmc and Sl).

b) S *man-u*; Gmc *man-n*; Sl *možĭ-*. Not in Gk and B (though both, as other branches, have the common stem *man-/min-* 'thinking').

c) L *homo*; Gmc *gum-*; B *žmogus*; Toch A/B *śom/śaum*. Not in S and Gk (but in both the stem *kṣām- and χθών* for 'earth').

10. corpse, dead and gone: S *naś-* , *naṣṭa*; Gk *νεκρός*, *νέκ-υς*: L *necare*, *nex* 'violent death'; C *ēc* 'death', Toch A *näk-*. Not in B.

11. fire: a) S *agnis; L* ignis; B *ugnis*; Sl *ognĭ*. Not in Gk.

b) Gk *πυρ*; Gmc *fōu/fȳr*; Toch A *por*. Not in S and B.

12. mouse: S *mūṣ*; Gk *μῦς*; L *mūs*; Gmc *mūs*; Sl *myšĭ*. Not in B.

13. worm: S *kṛmi*; C *cruim*; B *kirmis*; Sl *črŭvĭ*. Not in Gk.

14. day: S *dina*; L (*dies*?) *nun-dinae* 'ninth/market day'; B *diena*. Not in Gk.

15. moon: S *mās*; Gmc *mōna*; B *menuo*; Toch A/B *mañ/meñe*. Not Gk (where *μεις/μην-* ='month' only).

16. star: S *star-*; Gk *ἀ-στήρ*; L *stel-la*; C *sterren*; Gmc *stairnō*; Toch *śreñ*. Not in B.

17. beget: S *jan-*; Gk *γεν-* (*γίγνομαι*); L *genere*; C *-genāthar* (Welsh *geni*). Not in B.

18. breathe: S *an-*; L *an-ima* 'air, breath'; C *anāl*; Gmc *-anan*; Toch *añm*. Not in Gk (except ἄν-εμος 'wind') nor B.

19. awaken: a) S *jāgar-*; Gk *εγείρ-; Alb ngrë-he*. Not in B.

b) S *budh-*; Gmc *ana-biudan* 'command'; Sl *buditi*; B *budeti*. Not in Gk (which has *πενθ-/πυνθ-* 'learn').

20. dress: S *vas-*; Gk *ἕννυμι/ἕσ-σαι* (aor); L *vestire*; Gmc *wasjan*. Not in B.

21. carry: S *bhar-*; Gk *φέρω* L *fer-re*; C *berid*; Gmc *bairan*; Sl *bera* 'take'. Not in B.

With the additional stems we have a total of 26. Of these 26 stems Sanskrit lacks 3, Greek 10 and Baltic 16. The exercise furnishes only an indication, no more. Definite conclusions should be drawn only after a much larger number of items and stems is examined. Then the investigation should show whether each stem has other cognates in its own language or simply hangs isolated. This exercise would be more realistic and substantial than the theoretical pronouncements of "areal linguistics" with its framework of "marginal, central, isolated" categories. But, perhaps, this too might be considered another arbitrary statistical game.

31. In the second part of his Comment M examines the OIT. Realizing that I have no competence in this field he kindly provides some possible models. He would have been much kinder if he had not demolished them all. However, I cannot evaluate all this. I simply accept his word for it. He had done the same with other proposed homelands (1989, 1997) and stated that he preferred the Pontic Steppe only because it was "the least bad" solution (1997: 115). Apart from anything else, one basic difficulty with the Kurgan people is that we don't know what language they spoke.

I should not be at all surprised if eventually it is discovered that there was an IE continuum from Saptasindhu to the Pontic-Caspian region and there were movements of people, some quite indeterminate, within it and in and out of it. Nor would I be surprised if the IAs were shown to have immigrated to Saptasindhu in the early 5th millennium – whereby, of course, by 1500 BC they should be regarded as indigenous.

India may not have the *best* claim for the IE urheimat, as Bryant points out, but it "*may just as well* have been the IE homeland in theory since all other homeland proposals suffer from setbacks that are arguably just as problematic as those associated with a south Asian homeland".

32. Michael Witzel (hereafter, W).

First, I apologize to this my *priyatamaśatru* for omitting to read many of his works, for misunderstanding him at times and for misquoting him on the subject of *pur* (his §5, n2); indeed, I should not have stayed only with his remarks suggesting that the *RV* may refer to the ruins of the Indus cities (K IV, 4) but should have noted also his meaning "simple mud wall and palisade forts" (2001a: §22). The difficulty here is that *armaka* and *mahāvailastha* (*RV* I, 133) do not refer to Indus town-ruins but a ghost-scene with ghouls and goblins (*yātumati*, *piśaci* and *rakṣas*).

I also apologize for seeming to single him out as an object for attack but there is really nothing personal; he alone publishes frequently and volubly on these issues (and often impolitely on whoever he considers an opponent.

His writings contain many difficulties as I noted 4 or 5 times in earlier pages. Another difficulty is found in this same § 5 when he writes about his and Farmer's article in *Frontline*, "Kazanas claims not even to have seen the article ["non vidi"] drawing his conclusions, as so often, second-hand from his Indigenist friends' e-mail reports!" (square brackets original). This surprised me because I had quoted twice from that article. And, behold, lower down on that very page, in n 27, W gives my own words about his article "in which he [=W] assails some publication which allegedly used fraudulent material (*non vidi*)". Surely, by its very position "(*non vidi*)" could not refer to W's article: it is the publication assailed by W that I had not seen.

A different sort of difficulty comes when W describes R. P. Das (1994) as "one of Kazanas' favorite sources" (§ 6, end). A "favorite source" is one which we use often. Considering I refer to Das only once in K IX, 2 and never before, I don't understand in what way he is "favorite".

W's Comment is infested with similar problematic statements. It would be tedious to examine them all. I shall consider only those that are directly connected with the major ideas discussed hereafter.

33. New Age, Hindutva, Right wing. W has decided that in 1996 I was converted to New-Age/Hindutva beliefs, that various Indian fundamentalists are my colleagues and that *Omilos Meleton*, the Cultural Institute of which I am Director, is a New-Age cult and that we have right-wing leanings and associations (§1 and passim). On what are these allegations based? On four things: a) some of my papers have been published in academic Journals in India and are listed on an Indian website (given openly in K's bibliography!) as well as on that of the Omilos; b) one quotation from one of my published papers; c) another quotation without reference; d) a paper on Plato and a book on Marx. Let us see.

a) My papers have been published in four academic Journals *ABORI*, *JICPR*, *Yavanikā: Indo-Hellenic Studies* and *Adyar Library Bulletin* (the last two not mentioned by W). Incidentally, these and Prof Bh. Gupt, a hellenist in Delhi University, are the only people in India with whom I correspond. As W knows well, no Western Journal would publish articles advocating Indoaryan indigenism and a *RV* of the fourth millennium.

b) The quotation about all gods being manifestations of the One and the inner spiritual strength of the Rigvedic people comes from the last paragraphs of my 'Indo-European Deities and the Rigveda' published in *JIES* vol 29, pp 257-293. For the deities and the One, see *RV* I, 164, 46, VIII, 58, 2, X, 114, 5 and III, 55. For the inner spiritual strength, see VI, 75, 19, "My innermost armor is brahma". Now, by W's criteria we should dubb "neo-paganists" or "idolaters" all classicists who in the last 200 years praised the spirit of the Greek (and Roman) civilization.

c) W's quotation that we derive inspiration "from numerous spiritual traditions of mankind - Indian, ancient Greek, Buddhist, Christian, Gnostic, and so on" is trumped up for the occasion: W gives no reference.[9] The "New Age basket" derives only from W's imagination and if he had found anything "New-age" on the Omilos website he would have printed it in bold capitals. It is surprising that he does not mention items on our website like the study on Marsilio Ficino and the Renaissance or papers on Economics, etc. The very first Notice in the English section states we have courses in Philosophy, Christian and Buddhist Ethics , Sanskrit, Comparative Mythology, Political Economy and others. The Philosophy course includes "Plato, Aristotle, Spinoza and others, and lectures on Vedānta". The Economics Faculty is quite large and we contributed a paper on Taxation Reform to the Primeminister's Economics Advisory Committee. If all this constitutes New Age, then, yes, we are New Age. But a warning: we have no Astrology, Numerology, Palmistry, ESP, yogic postures, yantras and tantras, Tarot cards, the I-Ching, healing, séances and the like.

d) The paper on Plato's Economics is a summary of some lectures given by Lena Derou in Utah University in 1999. W's derisive remarks merely betray his ignorance of Plato and the classical economists. As for my study *Prodomenos Marx* (="Marx Betrayed",written in 1988-89) – it is an extremely sympathetic approach to Marx denouncing Stalinists and other self-styled "marxists" who do not know Marx's works. It traces the humanistic elements that unify Marx's thought; it mentions well-known errors in his economic formulations (eg the circulation of capital); it stresses the change in his last years when he began to think that the transformation of society could come (not necessarily by a revolution of the proletariat but) through the agrarian units of Russia. How from these two studies, one of which he could not have read, W deduces that we are a Right-wing organization (neo-fascist? crypto-Nazi?) is beyond my thinking. Frankly, his strident obsession with New-Age/Hindutva/Right-wing recalls right-winger MacCarthy who thought he saw in every closet conspiring communists, or left-winger Stalin hounding Jews, Gypsies, dissidents, revisionists and other "enemies of the State".

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[9] What is wrong in any case with people drawing spiritual succour from such traditions?

We have no association of any kind with any religious or political organization in Greece or elsewhere. I don't know what D. Frawley's Institute does or what the RSS is, nor will I waste time finding out.

34. My translation of *RV* VII, 6, 3 is free (K III, 2) but not as capricious as W claims (§6.2.1). First, the poet says in st 1 that he lauds the deeds *kṛtāni* of Agni. Then, in the first half of st 3, we find the Paṇis (plural Acc) as object of a verb but only the prefix *ni* and no verb; in the second half we find a verb (perfect *vivāya* 'chased') and the Dasyus (plural Acc) as object.W takes Paṇis and Dasyus to be the same, but the Paṇis too are said by some to be people, apart from mythological demons (*Vedic Index* under *Paṇi* and Mayrhofer under *Paṇayaḥ*). I think here the two are different and we have two parallel deeds. Agni drove down/back (=*ni*-) the Panis and chased/drove further on (*prá-pra*) the Dasyus. Then W translates 'the first one has made them the last ones': W's punctuation suggests that the 'first one' is Agni and 'them the last ones' are the Dasyus. Rightly he brings in much philology from secondary sources, but in what practical sense is Agni 'the first one' and the Dasyus are made 'last ones'?...Agni was not there at the start, because st 2 tells us he has been forced/impelled out of the stone (*adrer hi*-): he therefore is not 'the first one'. If Agni sheds light, as he does, and the Dasyus (whether demons of darkness or people or whatever) are driven off, then it is Agni's rays that now remain and are last.

Then, st 4 says that Agni (in another deed) made *prācīḥ* 'forward/eastern' some females (=Dawns?) 'rejoicing *madantīḥ* in the backmost/western darkness *apācīne tamasi* ' and st 5 says that Agni made the Dawns 'consorts of a noble-one (=Sun)'. Here surely the Dawns are in the east married to the rising sun and the darkness recedes westward. By analogy now, in st 3 we can take Agni to be in the east and the Dasyus to move, chased away, to the west (not as W says "eastwards"); after all, W admits (6.2.1 and n 43) that elsewhere *apara* means 'western'.

If all these are one deed as W implies, then the plural *kṛtāni* 'deeds' in st 1 is pointless. It is good to bring in secondary sources but better not to ignore the actual context itself. The Rigvedic poets were not incoherent imbeciles.

Are Dasyus people or mythological figures, demons of darkness? In different contexts, they seem to be sometimes the one and sometimes the other. In I, 33, 7 Indra burns the Dasyu down from heaven: here we have mythology. But in II, 12, 10 Indra strikes with arrows sinners, does not forgive the arrogant and slays the Dasyu: in this context they are people. Considering the Dasyus in VII, 6, 3 are *ayajyu*- 'without rites' (so W), we should here take them to be people since it is humans rather than supernatural beings that normally have rites.

Now I concede that my interpretation may be wrong and that a different one is possible. I leave it to the readers to decide. W's comments made me re-consider the passage and I thank him for this.

35. W criticizes me for not making allowances for future archaeological discoveries and this for him is a "deception". He adds : "Nobody can predict much about future discoveries in archaeology"(§6.3). A little earlier he wrote : "Kazanas does not allow even the possibility of archaeological discovery involving the IAs."

Considering that W reads English, I find both statements astonishing. For I wrote very early "at any time new finds may emerge...that will change radically whatever picture we now have" (K II,1). This is another difficulty in W's writings: suggesting Somebody has said (or not said) something which Somebody has not said (or said), then triumphantly refuting it.

I mentioned the new discoveries in Taxila in §8, above. Then there are the discoveries in the Gulf of Khambat (or Cambay) off the coast of Gujarat. On January 16, 2002, India's Minister for Human Resource Development stated that an underwater urban settlement had been discovered in that Gulf dating from c9500 BP. Most archaeologists doubt this dating. We have to wait for new studies by experts and these will probably cause adjustments in our views of India's Protohistory. Other discoveries will come, no doubt.

36 Aryan entry ? Early on in his Comment W invokes Renfrew's agricultural "wave of advance" and Ehret's "elite kit" model and various other migrations (§1) not realizing that these are not true parallels to the hypothetical Aryan entry c 1500.[10] To crown it all he asks "Why leaving aside the sensitivities of Indian fundamentalists or their New Age supporters, is the migration into India of speakers of early Indo-Aryan the *only* migration of which academic discussion is forbidden?" In § 6.1 he asks again: "Why is the immigration of Greek or Iranian into their later habitat 'allowed' by Kazanas et al, but not that of IA?"

Here again we meet with the difficulty mentioned in my §35, above – W's trick of suggesting I did not say something which in fact I have said, and then refuting the omission he has himself produced. For I did state clearly that there may well have been an Aryan entry c4500 or before (K IV,1; V, 2, n 12; X).

In 6.3 W writes about "cultural expansion" without "raids" or "military conquest" and reminds me of "the Greek settlements and their cultural influence in the Mediterranean and Black Sea regions". But these again are no parallels to the hypothetical Aryan entry. The various Greek colonists in historical times had literacy, had the constant support of their city-state (with some exceptions) and left plenty of archaeological evidence (apart from other documentation). The hypothetical Aryan entrants c1500 remain "elusive".

I remind W of something which he himself wrote a few years ago: "The Indo-Aryan influence [in Saptasindhu], whether due to actual settlement, acculturation, or, if one prefers, the substitution of Indo-Aryan names for local ones, 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, as has been the case in Europe and the Near East. At the least, one would expect a palimpsest, as found in New England, with the name of the State of Massachusetts next to the Charles River formerly called the Massachusetts River…[T]here has been an almost complete Indo-Aryanisation in northern India; this has progressed much less in southern India." (1995: 106-7).

How did the IAs manage to accomplish this enormous change in North India?... W's answer to this is extraordinary (§ 6.4). The Aryans, "'illiterate' Vedic speakers", had a "voluminous production of oral literature according to highly developed poetic techniques" whereas the Harappans had a "questionable 'literacy'… attested solely by symbolic inscriptions of unknown sense". So W almost reverses here the mainstream view of non-literate Aryans and literate Harappans. Be that as it may, we cannot assume that the Aryans brought this "voluminous…oral literature" with them because W told us that the "geographical horizon [of the *RV*] is limited to the Punjab and its surroundings" (EJVS 7-3, §3). Earlier we had agriculturalists, pastoralists and potters winning over the Harappans, who already had agriculture, pastoralism and pottery (§§ 11,12, above). Now we have Aryans presumably entrancing with poetry the almost illiterate Harappans into adopting the Aryan culture – although they did not know Vedic. But W then proceeds to explain that the IAs at first influenced the "few village level settlements…whose remaining inhabitants had reverted to a pre-Harappan style culture" while the Harappans themselves "were long gone from the scene" at Punjab (though Takṣaśilā thrived); but presumably the IAs caught up with them also eventually and brought them

[10] Incidentally, Renfrew's theories are now meeting much greater resistance from other scholars. "New archaeobotanical evidence suggests the spread of wheat and rice cannot explain language change in India and Southeast Asia" (Shouse 2001: 989).

under their irresistible spell. So, by the magic of their poetry the small waves of immigrants effected the Aryanisation of North India – but not the South. Well, yes, much stranger things happen – in fairy tales. (In fairness to W – he does not say that Aryanisation was effected by poetry, but neither does he explain how it came about.)[11]

Bryant writes in his Comment: "I agree that a plausible explanation has yet to be given as to how the newcomers could have completely eradicated the pre-existing language of the entire North of the subcontinent in the short interval normally allotted between their arrival and the composition of the Rg Veda, in which the local topography is Indo-Aryan. ... Kazanas has a right to wonder how and why would the Indus Valley dwellers have so thoroughly and completely adopted the language of these illiterate herdsmen if the latter were not invaders – a status denied them by archaeology?"

37. Horse and words in the *RV*. For the horse see what I wrote earlier in § 10.

While W in his §6.5 gives in detail reconstructed words for chariot and cognates found nowhere except in modern works, he writes about the horse in the end, but ignores the fact that in several hymns in the *RV* (eg. I, 34,9, III, 53, 17-18, etc) the actual word *vājin* 'horse, fast(?)' is used in conjuction with the actual word *rasabha* 'ass(?)' suggesting either that the ass is fast or that horse and ass are yoked together (KVII, 1). Since people do not usually yoke together horse and ass to a chariot and since the donkey is not fast like a horse, it occurred to me that the word *vājin* may denote some other equid. And if this is so, then why not *aśva* also?... These are possibilities that go against the grain of established tradition, but who can guarantee that this tradition is unimpeachable ?.. I pointed out (K VII, 7) that the word *ambhas* (*RV* X, 129, 1), traditionally translated as "water", cannot mean water in the context - but something like 'potency'. In a recent article Karen Thomson ascribes the "apparent poverty of sense" in many verses of the *RV* to "the legacy of the tradition" and adds "If we can dig beneath the assumptions about meaning that overlay the text... we shall uncover a very different *Rigveda* from the one that we have come to accept" (2001: 345).

Then W uses another subterfuge. He throws in "the retired bank official S. Kalyanaraman" as if I know him, and "the Indian *Equus sivalensis*" (which disappeared c10000 BC) as if I consider it to be valid evidence and should have mentioned it – but "mercifully"(!) do not in fact mention it. Do we need such a 'red herring'?

38. W. Lehman referred to H. Scharfe's demonstration that the Rigvedic *rāj-* means (not 'king' but) 'power, strength', stressed that the need now "is informed attention, often to individual items" (1993: 68), then added: "The example of... 'king' illustrates that generally accepted results may have been based on premature conclusions" (p 236).

In a philological, contextual study, S.A. Dange examined the incidence of *gaviṣṭi* 'battle, desire to gain cows' in 15 places in the *RV* (V, 63, 4-6 ; VI, 47, 20; VIII 61,7; etc) showing that nowhere does the word mean battle or cow-rustling as is traditionally translated (1967). I have not seen any references to this study but many to cattle-rustling. Will anyone examine Dange's paper?

[11] Here W throws a bombshell: "Kazanas is heavily influenced here by Frawley's most amazing paradox". (Elsewhere I borrow from Kak or some other "fundamentalist-colleague".) I was not heavily influenced by Frawley's paradox. I simply acknowledged (after some research) that Frawley had "first noted" this paradox, namely that the literate Harappans left no literature while the non-literate, archaeologically unattested Aryans left their voluminous productions. With this I suppose W tries to draw attention away from the fact that he has no explanation for the Aryanisation of North India. In W's mind Frawley is a fiendish figure of fundamentalist Hindutva, Astrology and the like, and therefore cannot utter any truth; if K is "influenced", K also cannot utter any truth.

In her penetrating article, Karen Thomson examines the fairly common *grāvan* which is traditionally translated as 'pressing-stone' and finds that in more than 30 instances the word means 'singer'. Her conclusion: "Tradition stands in our way" (2001: 345).

I examined the word *pur* (with the aid of Lubotsky's *Concordance*), since I was baffled by the *cariṣṇu* 'mobile' and the *āyasi-* 'metallic' purs (K IV, 4). I found that in at least 20 instances it does not mean 'citadel, fort, town' nor 'mud-palisade' as per W (my §32, above). No pur is constructed or destroyed by humans. In *RV* I, 166,8 Agastya prays to the Maruts (who are Storm-gods rather than builders) to protect from evil *agha* and injury *abhihruti* the man they favour "with hundredfold purs *śatábhujibhih pūrbhíh*". Geldner here translates *Mit hundertfachen Burgen*. But surely 100-fold forts (allowing for the hyperbole) cannot possibly protect a man from evil. Hymn II, 35, 6 has god Apām Napāt 'Offspring-of Waters' protected from malignities (*arāti-*) and falsities (*anṛ́ta-*) in *āmā́su pū́rṣu paráḥ* which W. O'Flaherty translates as 'far away in fortresses of unbaked bricks' (1981: 105). No bricks are mentioned here (nor elsewhere in the *RV*) so I take it she followed Geldner who translated *In den rohen Burgen* and realizing this does not mean much added a note (6c) *nicht wie die gewönlichen Burgen aus gebrannten Ziegelsteinen gemacht* 'not like the usual Burgs made from baked bricks' – but does not tell us where in the *RV* we can find "usual Burgs" made of "baked bricks". It is difficult to see how such "unbaked brick forts" can protect this deity from malignity or that he has any need of such protective means. Hymn VI, 48, 8 is a prayer to Agni to protect the fire-kindler from *aṃhas* 'anxiety' with100 purs – and one wonders how 100 forts (again allowing for the hyperbole) can protect from anxiety.Hymn X, 101, 8 (in connexion with Soma sacrifice) is a prayer to Viśvedevas to fashion inviolable metal purs: here again no material fortifications are meant. I can only suppose that *pur* denotes occult, supernatural means of protection.[12]

39. Bṛhat. In § 6.2.2 W makes this curious statement: "[K] does not know that *bṛhat* generally means "high" in Vedic (although he first gives it as an alternative). Thus, his translation of *bṛhad ratha* 6.61.[1]3 (VII 3) as a "large chariot" is misleading." Here again W is at his tricks. What I actually wrote is: "In VI 61,13 the river Sarasvatī is likened to a chariot: *rátha iva bṛhatī́* 'like a chariot tall/big/stately/bright'. So if a large river is compared to a chariot for size (*bṛhat-)* the chariot cannot be a small and narrow contraption". First, I don't translate *bṛhad ratha* as W twists my statement but, as the actual text says, a river "tall/big/stately/bright" like a chariot. Second, I do know that *bṛhat* means "high" (=my "tall"). Finally, I am translating poetry and try to bring out various feasible meanings in *bṛhat-* : tall waves; big in length and width; stately in flow (this best and most divine river); bright in the light.

Now, what does a simile "a river high/tall as a chariot" convey? In what sense is a river high/tall? In what sense is the poetic metre *bṛhatī* high/tall?... We know of a high mountain or a tall tree but a high river or a tall metre?... So I leave W's tall story.

40. Chariot. There is nothing in all the material amassed by W (in §§6.2.2 and 6.5) to show that the Rigvedic chariot came from the Near East or the Urals.

W accuses me of not using realistic details regarding the chariot but mythological. This is untrue. I do mention several realistic ones including the Mudgala/Mudgalānī race where the car is drawn by a bull (X, 102). I mention also the two-wheel car of the Aśvins and state on two occasions that this material is mythological (K VII,3).

[12] Cautious, unconvinced readers may cite dictionaries, other studies and historical semantics with Greek πόλις and Baltic *pilis/pils*. But I would refer them to *RV* I, 33,13; I,58,8; I, 189, 2; II, 4,6; II, 20,8; IV, 27,1; VI, 16,35; VI, 18,5; VII, 5,3; VIII, 1,28; IX, 48,2; X, 46,5; etc; also emphatically to *Aitareya Br* I, 23 and II, 11 and *Taittirīya Saṃhitā* II, 5, 3. In later texts *pur, pura, purī* do mean 'fort, town'. (Kazanas 2002b).

Then in his § 6.5 W gives a host of details that are supposed to be realistic and demonstrate the difference between *ratha* (=light, two-wheeled chariot) and *anas* (=heavy wagon): spokes... surrounded by wooden rim (!) ... bent by the carpenter (!)... made of suitable wood (!) and so on: it is such remarkably specific details that distinguish *ratha* from *anas*.

But W, paraphrasing W. Rau as he says, had already slipped into sloppiness or stealth. In saying that the "(light) chariot has two wheels (cakra)' he cites *RV* 1.164.13, 8.5.29 and 10.85.11. If any reader should look these up, he/she will find that they are as mythological as the 1-, 3-, or 7-wheel car. The first reference 1.164.13 mentions only ONE revolving wheel with five spokes upon which all creatures/worlds stand; the previous st 12 has 7 six-spoked wheels! The second 8.5.29 describes the chariot of the Aśvins with its two golden wheels (splendid realistic detail) – which I too mentioned admitting it to be mythological (K VII, 3). X.85.11 refers to Sūryā's bridal car which indeed has two wheels but also *the sky as its covering* and is in fact the 'mind' *manas* (in st 10)! Furthermore, if W had examined his last reference with only a fraction of the assiduity he uses to witch-hunt New-Age/Hindutva/Right-wing people he would have noticed that *stanzas 10 and 12 of this hymn have* ***anas***, *not* ratha. (It is always better to read the text rather than rely on secondary sources.)

Thus, with his own evidence, W unwittingly vindicates the good old *Vedic Index*, which states there is no "absolute" distinction between *anas* and *ratha*.

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

Thapar and Mughal mention a sherd depicting a canopied car with spoked wheels unearthed from early Harappan levels at Banawali (1994: 253). Bisht, the excavator at Banawali, mentions an additional pot sherd with similar depiction (1997: 252).

42. Rathavāhana. In our 2001 joust, W criticized my views on this issue and gave the following traslation for VI, 75,8: "on this (rathavāhana) we ***wish to put*** the useful/strong ratha" (my emphasis). This is also Geldner's translation (*auf den wollen wir der Wagen setzen*) and O' Flaherty's "on it let us place" (1981: 237)

Now, after I pointed out that *upa-sadema* is not a Causative (and therefore cannot mean 'wish to put'), W states that "Here K may actually have a point", shows that I do have a point and translates "We wish to approach the useful ratha". But, by another trick, traditional practice among translators, he introduces in the beginning also "We wish to approach" which is not in the text.[13] By this means he suggests that the chariot is on the *rathavāhana* 'platform'.

The correct translation is: "The conveying-by-the-chariot (or, 'The chariot-frame' as per Whitney) – offering is its name; where his [=warrior's] weapons and armor are laid, there, the useful chariot let us approach [respectfully], we who are ever full of good spirit."

[13] A good example of this introduction of words not in the original is found in my §38, above, where Geldner and O'Flaherty import "bricks" to explain *pur* in the traditional sense of 'fort'.

W then gives the general rules for the Vedic accent on compounds ("practically all... generally... generally") plus a pageful of examples – all unnecessary. I agree fully on this, but I said only that the accent is "not invariable", i.e. there are exceptions to the general rules, and it is this point that I stressed with my examples (K VII, 4, n 17).

What is more important, W concentrates on Accusative Tatpuruṣas and does not mention the Instrumental ones (and others) like the one I gave – *aritra-páraṇa* 'going-across with-oars' (not 'going-across oars'). This is the most salient point of this issue (MacDonell: 271-274). The compound *ratha-vā́hana* can be a Dative Tatpuruṣa 'conveying-by-chariot'. W gives also several examples of compounds where *-vāhana* means '-carrying/conveying': *kravya-* 'meat', *havya-* 'oblation' etc +*vāhana*. But these first members are not natural carriers like *ratha-*. So the parallels are not true.

Finally W mentions, as I did, Whitney's translation of *rathavāhana* in the *Atharva Veda*. He does not pause there but moves off tangentially to distract the readers' attention. Since he refers to Whitney's Grammar (1889, §1271 insinuating that here Whitney analyses *rathavāhana* which Whitney does not) he should have considered: "Whitney is a great grammarian. How does he in fact translate *rathavāhana*?" Whitney does not translate '-platform/conveyor' but '**chariot-frame**' (=chariot).

43. Sarasvatī revisited. The long and the short of this issue is that Sarasvatī is praised in early and late hymns as a great river flowing from the mountains down to the ocean. W seeks to counter (§6.7) this fact in his usual fashion: –

a) K's translation of *sindhubhiḥ pinvamānā* 'swollen by many rivers' (K VIII, 2) is not precise because it should be "due to the function of the plural in Skt. swollen by three (or more) rivers"! Despite this quibble, the river remains 'swollen', not shrunken: on this W is silent.

b) K admits that "Sarasvatī is also a goddess and has a celestial aspect... as the river Nile was also the Milky Way for the Egyptians (K VIII, 1)". The dots indicate the references I give where in the *RV* Sarasvatī is not the river but the goddess. W omits them and in an involuntary display of incoherence accuses me of contradiction.

c) Citing K. Klaus (1986/89), W writes that *samudra* can mean a "collection of waters" or "confluence" or "heavenly ocean"[14]. This is true, of course, but it is another 'red herring'. In VII, 95, 2 the river flows from the mountains: such rivers normally do not flow upward to heaven. Nor is there any hint in any hymn that Sarasvatī flows into the Indus. As for the "terminal lakes", only W believes this. In EWA Mayrhofer gives for *samudra* 'confluence' and 'sea' only. Vedic has two words for 'lake', *sáras* and *hradá*: if the intended meaning was 'lakes' (as per W), the poet would have used (not *ā samudrāt* 'to the ocean' but) *ā sarobhyaḥ/hradebhyaḥ* 'to the lakes' with the same cadence ¯ ˘ ¯ ; ¯ and with *sarobhyaḥ* he would have indicated also W's alleged etymological connexion of the name *Saras-vatī* (='having terminal lakes').

d) K is accused of being "hardly aware of the typical Ṛgvedic feature of lauding gods, chieftains, ... and rivers in hyperbolic fashion". (*But W refers also to my mention of "hyperbole" in K III, 2, and K VII, 1*!) However, the point is that, unlike the gods who all receive praise, with regard to the rivers, only Sarasvatī is lauded repeatedly. Only in X, 75 do we find praise for the Indus, if the word *sindhu* denotes the Indus river and not, as I wrote, the Spirit of the Rivers deified.

e) One more point on etymology. Historical semantics, invoked by W, is unreliable. Vedic *saras*

[14] If Klaus had analysed closely *RV* V, 55,5, VII, 6, 7 and I, 116, 4, which he merely mentions, he too would have seen that in these cases *samudra* denotes 'ocean'. Contextual analysis of the adjective *samudriya* (I, 25,7) reveals that it denotes 'ocean-going' boats not canoes on local lakes and rivers; cf also IV, 16, 7; IX, 62, 26 and 78, 3.

'lake, pool' does not mean 'swamp' as Gk ἕλος does, nor *manas* ('mind') 'force, passion' as Gk μένος. "K does not like the comparison with the Iranian Haraxᵛaiti," writes W, "as it smacks of an 'invasion' scenario". O, but I do like it – ardently. In Vedic the word *saras* does have a root *sṛ-, sar-* and derivatives *sara*, *saraṇyu*, *sarit* etc. The root has cognates in Gk ἅλλο-μαι (ἱάλλω), L *salire* and Toch B *salāte* (Rix: *Lexikon...* 527). In Iranian the word for lake is *vairi-*; the only cognate in Iranian is **Harah-* from *Haraxᵛaitī*. The loss in Iranian of these cognates and the change of IE *s* to Iranian *h* **do** indicate an "invasion" - not from Iran into Saptasindhu but the reverse.

44. The *RV* date. a) G. Possehl examined all the palaeoenvironmental and geological data relevant to the Sarasvatī river and concluded that this river could have flowed down to the ocean but only before 3200 at the very latest. The Allchins are more certain about this (1997); so is Francefort (1992) and Lal (2002). This obviously helps us to assign the *RV*, or at least the hymns that laud the mighty river, to a period before 3200.

b) The *RV* knows nothing of the IVC, of urban structures, fixed hearths/altars, cotton and rice, both of which were in cultivation in (post-)Harappan times; so it is not, as W would have it putting the proverbial cart before the horse, posterior to the IVC but anterior, i.e. before 3000.

c) Then, there are the lists of kings (=chieftains) and teachers. I do not refer to the Purāṇas which display prolixity and confusion but the accounts of Arrian, Pliny and Solinus which give a period of 6000+. W rejects them contemptuously - and so do others. But on what grounds?… Solinus may be doubted but Arrian is a historian and Pliny, apart from anything else, an observant naturalist. More important, all three agree on the figure 6000+. This means that their information derives from a common source, which is the Megasthenes report surviving mainly in the quotations of other classical writers. Is Megasthenes also to be rejected?… W himself refers to him, "the Greek ambassador to the Maurya court at Patna" c300 BC (EJVS 7-3, §16). If we reject every ancient source (even if not Indian) because it does not suit our theory, we may as well end the pretence of discussion.

45. d) Leaving aside the kings' lists, let us consider **the teachers' vaṃśa**. Citing M. Smith's works, W thinks these lists "rest on typically weak foundations" but is prepared to neglect the "small detail" that they "trace the line of teachers back to gods, to Prajāpati " (EJVS 7-3, § 19). It is true, as he says, that we don't know the dates of these teachers and that we must start with some assumptions. Fine, I start with the AIT assumptions.

According to the AIT the earlier *RV* hymns are dated c1200 while the later at c1000 or even 900. The *Atharva Veda* is somewhat later (1100 - 900) and the other *Saṃhitās* down to 800. Brāhmaṇas come shortly after at 800 - 600 including Upanishads like the *Bṛhadāraṇyaka*. Then come the Sūtras after 600 BC. The epics are after 400 BC and in their final version c 300 CE.

Now the *Bṛhadāraṇyaka Up* is not a unitary work but a collection of at least three different pieces, since adhyāyas 2,4 and 6 have a teachers' list at the end. The names of teachers are different (except for some that are common to the first and second): so we assume different schools or traditions. Each list contains about 60 names. The third has more than 65 with Brahmā and Prajāpati as the first teachers. Reducing them down to 60 we exclude the gods and any chance intruders – though it is more likely that names fell out.[15] By taking an average of 20 years we obtain a period of 1200 years.

The *RV*, even the late Maṇḍala X, knows nothing of the upanishadic teachings – ātman being

[15] In the Sunday liturgy in our Greek Orthodox Church are supposed to be recited the names of the Patriarchs from the present one back to apostle Peter. But only the more important names are actually mentioned; others are left out.

brahman, reincarnation, etc (but Prajāpati appears as a god in 4 hymns, X 85, 121, 169 and 184). If we assume that the 3rd list belongs to 500 BC (so the AIT), then the upanishadic teaching goes back to c1700. How does this tally with a late *RV* c900 and an early one c1200? It does not. The late *RV* should be c1700 and before. Even if we subtract 200 years we have a *late RV* at c1500.

46. The *Mahābhārata* Date. W writes: "It should be obvious to everyone but K that a text cannot be dated by the earliest date mentioned but only by its latest" (§6.9). Thus again he impishly ascribes to me a thought I neither expressed nor entertained. He then tells us that the *MBh* war would not "have had any horse-drawn, spoke-wheeled chariots …. nor names such as .. Greeks (*Yavana*), Parthians (*Pahlava*)… *Romā* and *Antakhī* (*Antiochia* conquered by Rome in 64 BC)", and concludes "Mercifully this little piece of counter evidence is passed over in silence." Here again is a trick whereby W subtly suggests that the entire epic was composed **after 64 BC** whereas, of course, this particular passage (*MBh* II, 28, 48-49) could be from the 3rd century BC, since the Indians could have known about Rome and Antioch and Greek cities 200 years earlier: the Greeks had settled in Bactria by 300, Antioch had been founded in 301 and Rome much earlier. By implication W further wants us to believe that the bards (or redactors/compilers) of the epic between 200 BC and 200 CE put in the epic the astronomical references in Bks III, V and XIII which by a most wonderful coincidence all converged in the year 3067; or, perhaps, they had detailed maps of the sky from previous years and decided that 3067 was the one to pick: another tall story. What W does not say is that Bhīma's club suggests a period of primitive warfare long before the sword came into common use by 64 BC.

In contrast, I wrote: "On the basis of the astronomical data the initial core of the MB belongs to the early 3rd millennium; the epic developed and grew in length in the subsequent years, when c1800, perhaps, a new change of style, language and rearrangement of contents took place leading to the final form in the last centuries BCE" (KV, 3). Obviously, there were accretions and interpolations into the epic text even in the early centuries CE.

47. People's Court. In several places W refers to "a public or people's court", which I allegedly proposed in our 2001 joust for the adjudication of the issue under discussion. Such phrases evoke pictures of Stalin's "people's trials" (the purges of the 1930s) or the tribunals of the French Revolution. I referred only to English Courts of law and trial by a jury of 12 true and honest men who know nothing about the case before them. It is the judge and the lawyers who know the niceties of the law and the lawyers (=scholars) of the contending sides who present the expert witnesses (=scholars) and evidence. Knowing nothing, the jurors themselves are utterly unbiased. I suggested that the 12 jurors be selected from among successful professional people (architects, engineers, administrators and the like) who would be "judges of fact". (For the development of this excellent system partly from ancient Anglo-Saxon customs but mainly from Norman practices that were old Frankish customs, see Maitland 1908: 120-136; 211-213.)

Why not expert scholars?... Very simply, because they are strongly prejudiced – whether invasionist or indigenist. Scholars should provide their expert evidence, but the jurors should know nothing and decide on the evidence adduced.

Huld has a point in saying that Copernicus was well prepared when he challenged the established theory of his day. But the point is blunt. What of the judges' condition?... Let us recall what happened.

48. The geocentric model of the cosmos dominated European thinking for 18 centuries. It was Copernicus (N Koppernigk: 1473-1543) who wrote of the Revolutions of the Celestial Spheres (*De Revolutionibus...* 1543) demonstrating the heliocentric arrangement of our solar system. 50-60 years later Kepler and Galileo promoted further this system. Did the mainstreamers of those days clap "bravo" and shout for joy at this? No, far from it. Galileo was made by the eminent scholars and churchmen of the time to renounce the heliocentric view in 1615 and in 1633 and was confined to a villa for the last 19 years of his life. Kepler's popularization work was condemned by the Church in 1622, printers would not publish anything of his and he lost job and income for a period. Giordano Bruno, who also advocated the Copernican system, the plurality of worlds and the circulation of blood (ideas that would become common after 50 years), was burnt at the stake in Rome in February 1600.[16] Copernicus himself escaped persecution publishing his book even as he was dying, in 1543. The book was placed on the Index 33 years later.

In the last 50 years of the 16th century Copernicus's *De Revolutionibus…* had only one reprint whereas in the same period the books of mainstream orthodoxy (Clavin's *Treatise…*, Melachthon's *Doctrine of Physics*, attempting to refute Copernicus, Paucer's textbook on geocentric Astronomy, Ptolemy's *Almagest* and Peuerbach's *Planetary Theory*) had altogether about 100 reprints (Koestler 1964: 194).

The odd thing about this story is that the heliocentric view was known in Europe long before Copernicus but was ignored by the "established" dogma. Leonardo Da Vinci (1452-1512) stated in his Notes that "The sun does not move" (Gombrich, 1995: 294). So this information was known – unofficially – in Italy before Copernicus, who studied in Bologna, Padua and Ferrara 1495-1503. In fact, much much earlier, *Aristarchos of Samos had made the heliocentric discovery in the 3rd century BC, ie 1700 years before Copernicus*, but none of the savants of his day (including the great Archimides), believed it or bothered to verify it. The geocentric view was formulated by Apollonius of Perga (also 3rd century BC following Aristotle and others, of course), developed by Hipparchus of Rhodes and completed by Ptolemy of Alexandria in the 2nd cent CE (hence, the Ptolemaic system). Thereafter all kinds of absurdities were written about the heavens, the celestial spheres, the Empyrean, the Prime Mover and so on, which constituted the "established" view. And all the time the real knowledge was there and all those schoolmen, could, with some practical observation and application of Mathematics, have found out that the Ptolemaic system was not true. But they did not: they argued about such weighty matters as how many angels could sit on the point of a pin. (There were exceptions, of course.) Even when the proofs were presented to them, hard and irrefutable mathematical demonstrations, they rejected them preferring the comforts of the "established" dogma. Theology and Church interests decided what was acceptable.

I am not suggesting that I am like Copernicus. What I am saying is that a wrong idea can be dominant for centuries and that prejudiced "experts" are not the best judges.

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[16] For details and more precise analysis, see Yates 1969: 354-356.

49. In conclusion, there are no indications of any entry c1500 nor of a satisfactory explanation of the Aryanisation of North India. J. Shaffer and D. Lichtenstein put it bluntly: "the academic investment in this hypothesis [i.e. 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. Rather, the physical distribution of sites and artifacts, stratigraphic data, radiometric dates and geological data can account for the Vedic oral tradition describing an internal cultural discontinuity of indigenous population movement" (1999: 258). The Indic records mention no immigration. Arhcaeoastronomical data indicate that some Indic texts were composed long before 1500. The *RV* was composed before the IVC since it knows nothing of it. (There are clear indications that the IVC had spoke-wheeled cars and, for me, the presence of the horse in the IVC is now well attested.) On the available evidence there may have been an entry c4500. Such facts, not hypotheses and speculations, should constitute the starting point of an approach to Indian Protohistory.

Dixi et salvavi animam meam.

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