The AIT and scholarship

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Introduction

1. In this paper I examine some additional aspects of the Aryan Invasion Theory and scholarship or academic study and practice in general. In the end I suggest means for a resolution of this rather unproductive controversy.

Before proceeding I wish to apologise to indologists, especially to Prof M Witzel for – *mea culpa* – connecting him to A B Keith in §§10, 11 in the older version of my "The *RV* Date: a postscript" which was posted on the Indian Civilization List. (Another version was sent by me for publication in a Journal in India, but was cancelled.) The Indian List version had, apart from the said error, several others. There were simple spelling mistakes like "millen[n]ia" in §2; "pinpoint[s]" in §4; "1000[s]" in §8; and so on. There were also distortions or omissions: in §9 (end) "Mesopotamia and the [Mehrgarh-] IVC [culture], it…"; in §13 "current, flow [, rapids]"; in the Bibliography the SGD, that is the Stamatakos *Dictionary of Ancient Greek*, and two titles by A Parpola – 'The Coming of the Aryans…' (1988, Helsinki) and *Deciphering the Indus Script* (1994, CUP). All these are inconsequential.

The reference to Keith is more serious. The notion Keith criticized was that of Hillebrandt who thought that Sarasvatī in the *RV* was the Iranian Haraxvaitī. This has little to do with Prof Witzel's notion that the indo-Aryans brought the name Sarasvati from Afghanistan and gave it to the Indian river; consequently there is no question of resuscitating a defunct theory. Again, I apologize for this almost indefensible error. In my notes on Sarasvatī I had one about Keith and Hillebrandt's hypothesis of transfer of the river and the reference 1922: 224, which is Rapson's edition of the *Cambridge History of India* vol 1. This note must have been made in a library in England. My own book is the 1935 reprint and the pages are 86-7. I should have checked but, because of overconfidence in my own memory, of idleness and pressure of time, I did not. It was **a blunder** and I beg that the (ir-)relevant statements in §§10, 11 in the old version be erased or ignored.

However, this in no way affects the rest of my criticism of Prof Witzel's views on Sarasvatī and of his indiscriminate attack on non-invasionists in his article with S Farmer in *Frontline* Oct 13, 2000. In addition, his insistence that much of the action in the *RV* hymns looks back to Afghanistan (Witzel 1995b) is indeed a repeat of the 19th century notion which Keith knocked on the head. Much more drastic is R Kochhar's effort to transfer the geography of the *Rgveda* and the *Rāmāyaṇa* to Afghanistan. I shall not be dealing with this science-fantasy. Prof Witzel does not go that far: in his most welcome recent study he states that the geographical horizon of the *Rgveda* is limited to the Panjab and its surroundings (2001: §§3,10), much as others have said before him. It is interesting that he does not criticize Cochhar for not considering at all the linguistic evidence whereas he excoriates all non-invasionists who disregard it.

Sarasvatī and the AIT.

2. In his 2001 study Prof Witzel re-examines the Sarasvatī material (§25). He thinks the Viśvāmitra hymn III 33 "already speaks of a *necessarily smaller* Sarasvatī" since it "refers to the confluence of the Beas and Sutlej (*Vipāś*, *Śutudrī*)" which means that the "Beas had already captured the Sutlej away from Sarasvatī, dwarfing its water supply." Rightly he points out that book III is an early one (earlier than Bk VII) and so Vasiṣṭha's praise (VII 95,2 'flowing from the mountains to the *samudra*') may echo praise of the *ancient* Sarasvatī or may be a hyperbole. All this would be acceptable, if the data were limited to III 3 and VII 95 – but, as I showed (2001a:§15), they are not: there are more, and Prof Witzel ought not to ignore them.

To begin with, people do not go to the confluence of rivers, where the current will be at its most turgid and vehement, to cross over; they go either higher up and cross two rivers or lower down where the flow would be smoother. That it is a confluence is not absolutely certain, but it is more probable than not. However, in the late X 75,5 *Vipāś* Beas is not mentioned, only *Śutudrī* Sutlej: this suggests that the latter absorbed the former rather than the other way round. Then, adding to the confusion, III 53, 9 says that Viśvāmitra induced only one river (no name) to stop. It is not a very clear situation. Be that as it may, in II 41,16 Gṛtsamada, calls Sarasvatī *nadītamā* 'most river-like' (*ambitamā* 'best mother' and *devitamā* 'best goddess'). If this is taken to be earlier than III 3, then we can accept that this is indeed the ancient river itself, before it dried up. But then, in the later VI 52,6 Bharadvāja says Sarasvatī is swollen *pinvamānā* by many rivers *sindhubhiḥ*; so many rivers still feed Sarasvatī. And VI 61,8-13 lauds the river – endless, swift-moving, roaring... most dear among the sister-streams, and, together with her celestial aspect, nourishing the 5 tribes. I don't think we can say that Bharadvāja also lauds twice the ancient Sarasvatī or uses hyperboles. In the definitely later VIII 21,18 the ṛṣi Kāṇva says that many kinglings dwell along [the banks of] Sarasvatī (and mighty King Citra?). Vasiṣṭha says the same in VII 96,2 and prays for sustenance and good fortune (as does X 17,7).

Sarasvatī is lauded in both early and later Maṇḍalas – in several hymns after III 3. We can hardly say that all references are to the ancient river, or are hyperboles, and deduce only from III 3 that Sarasvatī has dried up. The situation in III 33 and III 53 is unclear (moreso in conjunction with X 75) whereas the statements in the other hymns are quite clear. Prof Witzel takes as facts the unclear situation and disregards the rest. No, Sarasvatī is a large river and "most river-like" *nadītamā*, flowing mightily from the mountains to the ocean and nourishing the tribes dwelling along its banks – not in the past but even as the poets compose. No other river receives such praise so often.

3. The notion that the Aryan immigrants (as the AIT goes) gave the name Sarasvatī to a river in Saptasindhu **is indeed implausible** and this applies to other names of rivers or places: "River names in northern India are thus principally Sanskrit…" doubtless because "there has been an almost complete Indo-Aryanisation in northern India" (Witzel 1995a: 106).). The Aryans, even according to the AIT, moved not into a vacuum but a region thickly populated by a people who may have been on the decline and moving eastward but who were also more numerous, more advanced in urbanized civilization and literate. Now, would the immigrants be in a position to give new names and would the natives accept them for their rivers? … I doubt it. When thousands of Greeks were forced out of Turkey in the early 1920s and immigrated into the Greek islands and the mainland, they formed new communities and often gave to these the names of their former habitats (eg New Ionia, New Smyrna, New Halkidona etc). This is as far as their "nostalgia" went. They did not rechristen nearby mountains, plains or rivers, but accepted the existing geographical nomenclature. And here we are speaking of immigrants and natives who were equally civilized and one people, united in language and religion. As for the native Harappans – why would they accept new names for their rivers just because some illiterate barbarian intruders (as the AIT goes), different in language and religion, felt "nostalgia" for rivers they had known somewhere in their previous habitat 1, 5 or 50 years earlier? They would not, of course. The intruders would have been able to rename the rivers only if *they were conquerors* with the power to impose this. And, of course, the same is true of their Vedic language: since no people would bother of their own free will to learn a difficult, inflected foreign language, unless they had much to gain by this, and since the Aryan immigrants had adopted the "material culture and lifestyle" of the Harappans (Allchins 1997: 223; also Witzel 1995: 113) and consequently had little or nothing to offer to the natives, the latter would have adopted the new language only under pressure. Thus here again we discover that the substratum thinking is invasion and conquest.

One historical example of dominance-model Prof Witzel uses is the Normans invading Anglo-

Saxon England in the 11th century CE (2001: §12·1, n 85; also in 1997: xxii; in his latest study he uses also the spread of Swahili but about this I know nothing.) The example of the Normans is inappropriate because they were literate, unlike the IndoAryans. But there are other striking differences. William and his 12000 Normans invaded England in the autumn of 1066 (Trevelyan, 1972: 102ff) and at the battle of Hastings hewed down king Harold and his loyal thanes. Then, while various English potentates offered their loyalty to William (to keep, as they thought, their feuds and offices), the Conqueror proceeded to destroy villages in Buckinghamshire and Hertfordshire until London accepted him as their lawful king. On Christmas day, during his coronation at Westminster, his knights rushed out with the whisper of treachery and burned houses of natives (ibid, p 106). There were peaceful periods but in 1069 he attacked in the North. “Between York and Durham he left no house standing and no human beings alive that his horsemen could search out” (ibid, 107). The native aristocrats (and others) who wanted to maintain their property and position, of course, hastened to learn French and adopted the ways of the Normans. “Nevertheless there was never any period during which the majority of the country’s population did not speak English” and eventually the Norman descendants came to consider themselves English; about 50% of all French loanwords came into English at that period (Pyles and Algeo 1993) but no place- or river-names are mentioned. This then is the sophisticated model of immigration and élite dominance. (I can see why C Ehret was mentioned by Prof Witzel but Ehret’s views, formed mostly with regard to Africa, are quite different and, in any case, I would like to see some real paradigms, especially where he says that the “linkage of pottery and ethnicity breaks down in class societies…” (1988: 572).) The quote from Anthony (1995) in the same §10, end, I couldn’t trace: there is no Anthony 1995 in the Bibliography.)

Let me not be misunderstood. I am not suggesting that Prof Witzel, Parpola, Erdosy, et al (in the West), see any merit in the AIT and promote it with subterfuges. No, I don’t believe this at all. Nor do I believe that there is a grand conspiracy to undermine Indian history. (If anything, many Indians themselves do their best to achieve this end in various ways.) But invasion is the substratum of all such theories even if words like ‘migration’ are used. There could not have been an Aryan immigration because (apart from the fact that there is no archaeological evidence for this) the results would have been quite different. Immigrants do not impose their own demands or desires on the natives of the new country: they are grateful for being accepted, for having the use of lands and rivers for farming or pasturing and for any help they receive from the natives; in time it is they who adopt the language (and perhaps the religion) of the natives. A good example is that of the Parsi immigration into Gujerat in the 8th century CE to escape from the Moslem persecutions and massacres in Iran: there was one wave in 718-9 and another 60 years later. The Gypsies settling in various European countries and, while retaining their own language, learning the local language, provide additional example(s). The Aryan Migration Theory which V Agarwal so painstakingly charted with all relevant quotes (2001b) is a non-starter. Indologists of all hues can use as much as they like the term “migration” or its equivalents. There was none. You cannot have a migration with the results of an invasion. If there was an entry, it was an invasion (see §**19**, below).

Nor do some indologists care much where the IndoAryans came from – so long as they are not indigenous. Witness Prof Witzel’s reply to Subhramanya on 24/5/1998: “I would not care, eg, if the IA-s could be shown to emerge from the proto-Masai culture of E-Africa.”

4. We can take the discussion further examining some more notions and arguments habitually employed by invasionists. I shall again refer to Prof Witzel since his views are readily accessible. He is quite admirable both for his knowledge of these matters (even though I consider some ideas misguided), and for his indefatigable willingness to answer questions at all times.

I don’t use the computer at all and don’t follow events in the various Indology sites. Quite by chance I got in touch with Dr D Wujastyk whom I knew briefly in the early 1970s. In the course of our correspondence, he pointed out (March 2001) that Prof Witzel had expressed this notion of

Sarasvatī being really and originally a river in Afghanistan. After some digging (by my assistant, Mr Oikonomou) I obtained the relevant material, and wrote 'The *RV* date: a postscript' to supplement the information in my 1999 and 2000 articles.

A version of the present paper was complete (end of May) when I was given Prof Witzel's latest opus 'Autochthonous Aryans?...', so I had to make some revisions, but retained the original form. It may not read smoothly in places as I have been concerned more with the data and arguments than the literary style and format. Mr V Agarwal helped with some suggestions and references and Mr Oikonomou printed the text.

5. On 30/11/1999 Prof Witzel informs enquirer J Silk that horses are "not found in South Asia before 1700" and directs him/her for confirmation in R Meadow's papers (whereas R S Sharma's *Looking for the Aryans*, 1995, gives dates for horses c5000) then adds a note: – "All other claims, such as 'astronomical data' do not apply. – Long story." Before dealing with astronomical data, it is worth casting another glance at horses and chariots.

R Meadow and A K Patel argue that no *clear* examples of horse-bones have been found in North India before c 2000 (Witzel 2000: 7). This is repeated in Witzel 2001, §21. But what we are not told is that this paper by Meadow & Patel (i) seeks to refute S Bokonyi, who actually *does accept* that horse-remains were found at Surkotada, and (ii)) was completed in 1994 (publication in 1997) and therefore *does not cover* data presented in late 1994 and subsequent years. On the other hand, BB Lal (1997: 285-6) presents sufficient evidence for horse in the IVC; other archaeologists too. RS Sharma (who favours the AIT) also gives data of remains of domesticated horse from the Ganges area: these were reexamined and redated at c 5000-1000; I also pointed out that many prayers for horses in the hymns suggest the animals were not perhaps plentiful at all times and places (Kazanas 2001a: §8; also §**13**, third paragraph, below). D Antony also suggests there may have been few horses brought to the Indus by land or sea; hence the small number of bones (1997: 316). This view is also not mentioned by Prof Witzel even though he refers to Anthony's paper.

As for the chariot, why assume that the rigvedic *ratha* 'chariot' was like the chariots of the Near East or Europe in the 2nd millenium?… With the rigvedic car we face a double problem: first, most of the references to *ratha* and its related aspects are mythological and we cannot be certain that the descriptions given apply to human physical realities; second, more obviously realistic details found in the Brāhmaṇas or Upaniṣads are far removed in time to be of indubitable relevance. Many interpretations of rigvedic issues suffer from this drawback: because of insufficient information in the *RV* hymns scholars seek help from later texts, and even non-Indic material under the general belief that the IndoAryans entered NW India c1500 and the *RV* was composed c 1200 – . Such procedures have, I think, generated assumptions that may not be true and arguments that are circular, and thus adulterated the rigvedic evidence and its interpretation. Here I shall use only rigvedic evidence and such references from later texts as do not affect it.

To refer to an Egyptian chariot of the 15th century (now in Florence, Italy) with parts of it made of elm, ash, oak and birch (all imported from northern regions, i.e. South Russia) and assume that the Vedic vehicles are similar to this (Witzel 2000: 6) may be legitimate but not very relevant or helpful since the Vedic vehicle is made of *śalmali* (X 85, 20: also *kiṃsuka*?) or of *Khadira* and *śiṃ śapā* (III 53, 19) and its axle of *araṭu* (VIII 46, 27) – all these woods being native to India. We must remain with the evidence in the hymns.

Much of the evidence is collected in the *Vedic Index* under *Anas* and *Ratha*. It is said (under *Anas* , vol 1) that the cart (ie *anas*) is "sometimes expressly contrasted" with "the chariot (*ratha*) for war or sport" and the reference *RV* III 33,9 is given. This reference, however, *does not* present any differentiation or contrast: it says simply (twice, in fact, stanzas 9 and 10) that Viśvāmitra "has come from afar *ánasā ráthena*", that is 'by means of *anas/ratha*'.We could translate 'by means of a cart [and] a chariot' but it seems much better to translate 'by means of a chariot [which is] a cart' (or vice versa); one wonders why a high piest would use a *ratha* "for war or sport". There are present "Bhāratas" in the plural, so there may have been many cars, both *anas* and *ratha*, but the statement in the *Index* is not justified. This statement is corrected (under *Ratha,* vol 2) when it is said, "*Ratha* in the Rigveda and later denotes 'chariot' as opposed to *Anas* 'cart', *though this distinction is not absolute*" (emphasis added). For example, Uṣas has *ratha* in (the late hymn) I 48, 10 (100 in stanza 7!) and (the early) III 61, 2, and others, but she has *anas* in (the early) IV 30, 11 and (the late) X 73, 6, and others. Indra is said to be *anar-viś* (rather late I 121,7) 'seated in a cart' and not in a chariot. The references are by no means exhaustive but enough to justify the statement (under *Ratha*), "Of differences in the structure of the two we have no information." (That the navehole in the chariot-wheel is larger than that of the cart-wheel, as the *Index* says, is a mere conjecture from a single passage, VIII 91, 7 which in fact makes no such statement but only mentions three naves.) We can add that there does not seem to be any difference in function either.

RV VI 75, 8 has *rathavā́hana*, mentioned by Prof Witzel (2001: §21, note 192) and usually translated as a platform or vehicle for transporting "the quick but fragile, light weight (30 Kg) chariot" (Witzel, ibid). The word occurs also in *AV* III 17, 3 in a list of "cow, sheep, *prasthāvad rathavāhanam* and a lusty fat girl", all of which a plough should dig up (*lāṅgalam … ud-vapatu…*). (The passage is found also in some Brāhmaṇa texts with slight variations.) If we put aside any notions about Near Eastern (entirely hypothetical) parallels and modern racing-cars (Witzel: ibid; he might have added the transport of tanks or helicopters over very long distances) it is very difficult to see how "the chariot-transport that-has-a-support or -platform" (surely pleonastic? Whitney translates 'on going chariot-frame' fits with the cow, sheep and girl. On the other hand, the whole phrase could be a metaphor for *a horse*, "the chariot-puller that-has-stability". The word occurs also in the later prose texts and there it may have the meaning of 'platform, conveyor' though in some places, eg the much later Baudhāyana Śrauta Sūtra XI, 6, 72, 8 it may be used metaphorically: *athaihi yajamāneti*: *ratha eṣa dakṣiṇe śroṇyante rathavāhana āhito bhavati* "'Come, O sacrificer! he says. This chariot is placed on the conveyor [which is] the southern hip (*śroṇi-*) [of the altar]'. (For the mainstream view, see Sparreboom 1985 *passim.*) All such references are too late. Be that as it may, the use of the word in *RV* VI 75, 8 can be taken differently. First of all, if the *ratha* was dismantled, as is generally thought (Sparreboom, p 30), this would have been an ideal place to refer to it. But the hymn does not refer to wheels and carriage as being separate; in fact it does not refer at all to the car itself. Therefore, since *rathavāhana* is equated here with the oblation *havi*, since the chariot is often used figuratively for the hymn-oblation (in other hymns) and since the car itself is not mentioned and lauded (as the bow is in st2, the good charioteer in 6, horses in 7, the whip in 13, the mail or armour in 18-9), it would be very reasonable to translate *rathavāhana* as 'vehicle/transport that is a chariot', taking st 8 as lauding the chariot itelf. There is no need at all to assume that there was a platform that carried the chariot. W O' Flaherty here translates (1981: 237) pāda 8c "on it [i.e. the platform] let us place the working chariot" for *tátrā rátham úpa śagmáṃ sadema*. This rendering is not correct. The causative *upasādaya-* could be translated thus ('place on'), otherwise the verb means 'sit on or sit by, approach respectfully' and the like. So the phrase should be rendered "let us revere/honour/pay-respects-to the chariot".

The dimensions and measurements of the chariot are given in the much later Śulba Sūtras, so I shall disregard them. But there is a passage in the *RV* that is helpful: in VI 61, 13 the river Sarasvatī is likened to a chariot: *rátha iva bṛhatī́* 'big like a chariot'. So if a large river is compared to a chariot for size, the chariot must be big – much bigger than a lightweight car of 30kg.

These vehicles, *ratha* or *anas*, were pulled by 1, 2, 3 or 4 animals, usually horses. As oxen were also yoked to cars for races (X 102) we need not dwell on this point (see also III 53, 17-8; etc). Even in later texts we read of races where animals other than horses are yoked (Kazanas 2001a: §8). So "Horses were normally used for chariots but the ass (*gardabha*) or mule (*aśvatarī*) are also mentioned" (under *Ratha*). What is surprising is that while in the Upanishads the cars are said to have two wheels, in the *RV* they have one wheel (I 53, 9 and 164, 2; VI 54, 3; VII 63, 2, where obviously the sun is meant), sometimes 3 wheels (eg the Ṛbhus' one in IV 36, 1), sometimes 7 (II 40, 3: obviously mythological) and only once 2 wheels. This two-wheeled car (all-golden) is that of the Aśvins (VIII 5, 29). Everywhere else (in fact 6 times) this car is said to be 3-wheeled *tricakra*. In X 85, 14 the car is again described as *tricakra* but in the next stanza *one of the wheels is missing*! It is not clear to me whether the Aśvins had a 4-wheeled car, now left with three, or a 3-wheeled car, now left with two. These cars have another curious aspect in the *RV*: the chariot *ratha* has normally 'seats' or space for two, the driver and the warrior, but sometimes the rigvedic *ratha* has space for three; I am not referring only to the Aśvins' car which carries the Sunmaiden too, but also to III 6, 9 and VI 47, 9 where the *ratha* carries three and more (on its *váristhe… vandhúre* 'widest seat/box').

All these details constitute the picture of a vehicle that is not at all like the (war) chariots appearing in the 2nd millenium in the Near East – particularly as presented by Hollywood technicolour films. One wonders if in various scholars' connecting of the *RV* vehicle with the Near Eastern one there is anything other than wishful thinking. In my 1999 paper (p 33) I discuss the subject of vehicles and spoked wheels indicating that the technology for them was available earlier in India than in the Near East, and quote S Piggott to the effect that a more sophisticated type of vehicle with "one or two pairs of wheels with their axles" is known "from the Rhine to the Indus by around 3000 BC". A less sophisticated type of *anas*/ *ratha* obviously could have been known in our area much earlier since such fabrications get improved in course of time. The evidence for the development in the Near East of the first light chariots for war (Littauer & Grouwel 1996: 936-9) as against the pontic Steppe (Anthony & Vinogradov 1995: 40-1) seems fairly convincing. But the Near Eastern chariot can't tell us anything useful about the *RV* date.

It may be thought that my interpretation of the *rathavāhana* is far-fetched and ignores the traditional meaning given by scholars. This may be so, but the more I read the hymns the more I wonder whether the traditional, generally accepted meanings that I have learnt are true. Consider one well-known passage. Every sanskritist knows the *Nāsadīya Sūkta* X 129: pādas c and d read *kím ā́varīvaḥ kúha kásya śármann ámbhaḥ kím āsīd gáhanaṃ gambhīram*. All translations that I know give 'water' for *ambhas* (Geldner: 'Wasser'); even Jeanine Miller, who always approaches the hymns with great sensitivity and strives to bring out some spiritual significance, here translates 'water' and conncets this with other mythologies (1972: 68). As most mythologies/religions speak of "water" at a very early stage of creation (Judaic, Egyptian, Mesopotamian, etc) it is assumed that *ambhas* (connected with *abhra*, *ambara*, *ambu*, all denoting 'cloud, sky, rain, water') also does the same. But this cannot be justified: it is negated by the first hemistich which states unequivocally "Then there was no existence nor non-existence, nor space (*rajas*) nor the upper heaven". How, when there was no existence of any kind (known to us), could there be "water"? There couldn't. Here the word *ambhas* means 'potency' (Mayrhofer: *Gewalt*). Behind the traditional (Western) thinking is the notion "Why shouldn't the rigvedic mythology be like the other primitive mytholgies?" And so this preconception will continue to produce translations of "water", ignoring what the text says and what plain logic enjoins.The hymn here is significantly different from other mythologies (see also n 5, end).

A similar preconception operates and, I think, misinterprets the data about chariots and many other rigvedic matters.

Prof Witzel's discussion of *ratha* and *cakra* 'wheel' (2001: §14, p47) seems to me unclear. The linguistic data he provides do not support a Steppe-Urals origin. The specialized meaning of *ratha* 'chariot' is certainly found in Indo-Iranian but it is not confined there; the Germanic branches have *reith*/ *reita* and the Balts *ratai*/*reita* 'car, cart' (Buck: *cart*). The stem *rota, roth, rad* etc is fairly widespread in the other IE branches in the senses 'wheel', 'running' and the like. The supposedly older *anas* appears only in Latin *onus (-eris)* 'load'. Most commonly attested is the stem for 'wag-on, veh-icle': S *vah-*, Gk *och-*, Old Slavonic *vez-*, Old High German *wag-*, etc (SGD *ochos*;

Mayrhofer *vahati*). But Old Slavonic has another stem for 'car' or 'chariot' *kol-* which is also the 'wheel' *kol-o*. This *kol-* stem for 'car/chariot' *stands alone in Slavic*; no other IE branch has this stem to designate 'car, chariot'. Yet this same stem *kol-* is fairly common in the other branches for 'wheel': S *cakra*, Gk *kuklos*, Old Norse *hwel/hjol* etc (SGD *kuklos*; Mayrhofer *cakra*). If the chariot had indeed spread from the Urals (and gone east with the Indo-Iranian branch, as Prof Witzel maintains), we would expect this stem *kol-* to denote 'car', and not only 'wheel', in some (be it one!) other of the IE branches. Since it does not, we cannot assign origin or primacy to the Steppe/Urals (and Proto-Slavonic). We shall find something similar happening with the stem for horse (see §**13**, third paragraph).

Such details as we have of the rigvedic car do not suggest any similarity with the early one-man, two-wheeled chariot of the Near East (as shown in Littauer & Crouwel 1996: 936-7). Even if there was one, among all the forms suggested by the textual evidence, this would not mean that the rigvedic *ratha* came from the Near East, since the Harappans already had the technology for its construction – and, still less, from the Urals: at least not according to the actual textual and linguistic evidence. The invasionists will need to furnish much better evidence to begin to sound convincing.

6. Let us now look at Astronomy. One wonders why astronomical data should be excluded from any consideration of the *RV* date. After all Prof Witzel himself states that scholars should have "a willingness to confront data in all relevant fields" (2000: 14). Ultimately of course, there is no reason and Astronomy is brought in, but with caution (Witzel 2001: §29). All dates that specialists arrive at (even with the most conservative estimates) are rather early and upset the mainstream chronology pushing the dates back in time (Elst 1999: esp 101ff; Kak 2000). Prof of Archaeology B B Lal does not adopt the early date for the Vedic texts suggested by astronomical data only because he himself feels unable to judge such calculations (1997: 286).

Astronomical data disturbed rather violently the stagnant waters of Egyptology particularly with regard to the Giza pyramids. Egyptologists have believed for about 150 years that these splendid monuments were tombs for the Pharaohs – despite the absence of any inscriptions to this effect and, more important, of corpses or mummies. An engineer, R Bauval with the help of A Gilbert showed (1994) that the 3 Giza pyramids are aligned in the exact position of the 3 stars forming the belt of the constellation Orion as it would have appeared in the sky at 10500 and that the Sphinx, this strange, most impressive lion-man sculpture near the pyramids, stares at the lion-constellation with the sun rising before it at the same date. Then the story was taken up by the best-selling writer G Hancock (1995). I Edwards, an acknowledged mainstream authority on pyramids (1993), was convinced by these finds. These independent researchers (including Anthony West) brought American experts on rock-erosion and they concluded that the Sphinx was eroded by water (not wind and sand as egyptologists had conjectured) during heavy rainfalls between 7000 and 5000. This independent group of researchers suggested then that the Sphinx was in fact sculpted c 10500 (Bauval, Hancock 1996) and not c 2500 as mainstream egyptologists had assumed. Moreover, another establishment egyptologist, Mark Lehner, had 15 samples of mortar from the Great Pyramid analysed in Dallas and Zurich. These analyses yielded dates 3809-2869 – and not c 2500 as the standard theory teaches (Bauval, Hancock 1996: 306-7). Other mainstream egyptologists dismissed out of hand all these finds. The controversy continues.

The new view of the Giza complex was obtained through Archaeoastronomy by suitably programmed computers, which recreated the star formations above Giza (and Egypt) at different dates all the way back to the 11th millennium. (Be it noted that the new discovery was due to an engineer's insight and not mainstream academics).

Something similar has been done by Prof Narahari Achar for the star formations above northern India during ancient times. He concluded: "A simulation using the software SkyMap Pro in conjunction with Pancang2 has been used to verify that the statements in Ś[atapatha] B[rāhmaṇa] about the Kṛttikās [=Pleiades] never swerving from the east and about Saptarṣis [=the Bear, Ursa Major] rising in the north [relate(?)] to **events that could have been observed aroung 3000 BC**" (EJVS 5·2, 1999 emphasis added). Even if this date is out by 1000 years, the ŚB at 2000 BC is still more than 1000 years earlier than mainstream estimates.

One interesting reflection that will serve for many other future instances. Achar mentions that SB Dikshit had, about 100 years earlier, propounded the very same idea but later Western sholars rejected it by saying that the ŚB phrase "never swerve from the east" does not mean what it says (ie they rise "heliacally precisely at the east point"). Prof Witzel too writes an introduction to Achar's paper suggesting that we should not really believe what the ŚB is saying and adds a longer piece of his own on the Pleiades again "correcting" any "wrong" impressions we may have had from Achar's article. Thus we are (again) asked to ignore simple straightforward statements and get enmeshed in fictional possibilities. Achar's paper is quite clear and needs no introduction or cautionary comments.

Equally clear is Achar's provision of evidence that the date of the *Vedāṅga Jyotiṣa* (or that of astronomical facts described therein) is not 400 BC, as some scholars claim, nor 1200 as others would have it (with the identification of Dhaniṣṭha with β Delphini), but c1800 BC when Dhaniṣṭha, identified as δ Capricorn, receives together the sun and the moon for the winter solstice (2000: 177). This not only demonstrates that Indian Astronomy was independent of Babylon, as Achar observes (ibid 178), but also lends much greater credibility to Seidenberg's estimate of the *Śulbasūtra's* being older than Near Eastern, and in fact the origin of both Babylonian and Greek, mathematics (1978).

7. Prof Witzel (again 30/11/99) informs us: "The speakers of IA do not need to be *genetically identical* with those in the Ural areas! "Aryan bones" are not required". It may be claimed that this view is adopted because at least some evidence is against the AIT, but I doubt it. In any case, Prof Witzel does not give any explanation.

Certainly some evidence by experts goes against the AIT. B E Hemphill and A F Christensen found (1994) that there was no flow of genetic traits from Bactria into Saptasindhu c 1800: "Parpola's suggestion of movement of Proto-Ṛg-Vedic Aryan speakers into the Indus Valley by 1800 BC is not supported by our data. Gene flow from Bactria occurs much later, and does not impact Indus Valley gene pools until the down of the Christian eara". Dr Elst, who quotes this passage, explains that this later inflow is apparently that of the well-known Shaka and Kushana invasions (1999: 232). K A R Kennedy (in Erdosy 1995) concurs with the above view: "[T]here is no evidence of demographic disruptions in the northwestern sector of the [Indian] subcontinent during and immediately after the decline of the Harappan culture" (again in Elst, 1999: 233).

Personally, I distrust such finds. I examined the study of Cavalli-Sforza, Menozzi and Piazza (1996, repr of 1994: xii, 5, 29, 32, 88, etc) and found it to be full of difficulties and several inconsistencies (Kazanas 1999: 18-9). Lord 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. Nonetheless, "So far archaeology and palaeontology, based on multivariate analysis of skeletal features,have not found a new wave of immigration into the subcontinent after 4500 BCE (a separation between the Neolithic and Chalcolithic populations of Mehrgarh), and up to 800 BCE" as Prof Witzel himself admits (2001: §7). Do we need other testimony? Do we need, in fact to continue the discussion? Surely if Prof Witzel's statement is correct, then there was no invasion and the IndoAryans are indeed indigenous.

8. Prof Witzel thinks (again 30/11/99, though it has been mentioned on other occasions) that he and others, have discovered a trail of immigrants connected with the BMAC (=Bactria Margiana Archaeological Complex) dated 1950-1700 BCE. He states: "In addition, the trail of the speakers of

Indo-Iranian increasingly becomes clearer" as it comes into the BMAC – and thence of course into Iran and Saptasindhu. What is of real interest here is the start of the trail. It is not mentioned in this context but it was mentioned two statements earlier (§**7**, above) – *the Ural areas*. So in the year 1999 Prof Witzel finds a trail that runs from the Urals to Bactria (or the other way back).

What I find interesting here is simply the fact that other invasionists have a somewhat different view of things. First, B Sergent (1997) also shows considerable interest in Bactria and finds all kinds of connections east and west, north and south, but, surprisingly and reversing the direction, states that the Kurgan people originated in Central Asia (1997: 440). I make only this brief mention because Dr Elst has already dealt with Sergent showing very adequately that he fails to prove any immigration (1999: 238ff). Sergent, then, who has been trained in Anthropology and Archaeology, looking at the archaeological evidence in that region of Central Asia and with the same aim (ie to usher the Aryans into Saptasindhu), reverses the direction ignoring the Urals-trail to Bactria. But otherwise, according to Sergent, yes, the IndoAryans came from the N-W into Saptasindhu.

In year 1999, again, V Sarianidi (Institute of Archaeology, Moscow), examining much the same archaeological evidence at BMAC, finds also a trail leading away from Bactria – but not to the Urals. Now, Sarianidi should have, one might think, a vested interest in the Urals or the Pontic steppe since they are Russian grounds; but his trail leads westward to the area south of Caucasus (east Anatolia and north Mesopotamia), the IE homeland favoured by T Gamkrelidge and V Iranov and, in respect of Anatolia, by Lord Renfrew (Sarianidi 1999). I shall not pursue this particular trail further as I am writing a separate paper on Sarianidi's and Renfrew's views. Here suffice it to say that the Urals and South Caucasus (or eastern Anatolia) are very far apart and can't possibly both be the starting point of the "trail" to Bactria. At least one of them, if not both, must be wrong. The only point of agreement between the two views is the AIT – and while Sarianidi writes openly of invasion, Prof Witzel does not.

In 1999 Prof S S Misra published a new study with "fresh linguistic evidence".I have been unable to obtain this, but I presume it carries forward Misra's thesis in his 1992 work that the Aryans are indigenous. However, in 1997 Johanna Nichols presented her own linguistic case whereby, as is said in her Introduction, "the locus accounting of the distribution of loanwords, internal innovation, and genetic diversity within IE could only have lain well to the east of the Caspian Sea" (1997: 123). I shall not reproduce here J Nichols's detailed analysis of the evidence. Suffice it to say that all the different aspects of the evidence point to a location, not far from the one given by Sergent – a location "far out in the eastern hinterlands … a locus in western central Asia," which is then, in her "further implications", specified with precision: "The locus of the IE spread was … somewhere in the vicinity of ancient Bactria-Sogdiana" (ibid: 137).

Lord Renfrew – also in 1999 – claimed that the more recent trends in IE linguistics support his own theory of an Anatolian origin of the IndoEuropeans and their dispersal in the 7th millennium (1999: 258-93). He admitted that there are difficulties in "dating linguistic phases" and correlating them "with archaeological ones" and that he himself lacks "an appropriate training in the methodology of historical linguistics", but his study offered "a framework with a palpable time-depth for Proto-Indo-European instead of the 'flat' Proto-Indo-European commensurate with … the hypothesis of a steppe dispersal" (ibid: 286).

Gareth Owens supports lord Renfrew's general view but proposes the hypothesis that Minoan is "the so far oldest example of Indo-European" being a satem language (1999: 46, 49), and, taking into account certain paleaeontological finds at Petralona (northern Greece), suggests tentatively that "the Aegean is possibly the cradle of mankind" (ibid, 46, n 33).

Prof Witzel repeatedly emphasizes that the AIT depends primarily and largely on linguistic evidence (2001: §§10, 11.5, 12ff) and cites, among many other studies (§12·1), a paper by H H Hock, the eminent comparativist. He is right in that Hock disproves much of Misra's 1992 evidence but seems to ignore the seminal fact that **Hock finds no *linguistic* difficulties in the proposition**

that the IE branches moved out of India (Hock 1999: 16). So in the end, contrary to Prof Witzel's claims, *it is archaeological and similar evidence that counts, not linguistic* (see also my §**13**, fourth paragraph, below).

It has always been for me a cause of wonderment how scholars look at much the same evidence (at least so it seems), yet manage to produce different results and reach different conclusions. This happens not only in Indology but also in all academic fields (and even in ordinary life where two or more interested parties will view one and the same situation each in their own separate way). However, in the academic realm scholars are trained in part precisely to be more objective and to avoid the personal subjective factors that limit the untrained ordinary person's view and estimate of a situation.Yet, to use a hackneyed example, you find that scholars are divided about the 'coming of the Greeks' giving dates ranging from 2600 down to 1600. And some find that the Greeks are almost autochthonus or arrived onto the shores of the Aegean before 6000! (Owens 1999: 43-4 with bibliography.)Thus it seems that the training in objectivity is not all that successful.

Plato's Academy

9. I doubt whether today, apart from some scholars of Greek, other academics (or academicians) have any idea of what this term signified originally. The term 'academy' (or 'academia') comes ultimately from Gk *acadēmeia* which was the school Plato established in the grove of Acadēmos (an ancient hero) just outside Athens – much as Marsilio Ficino did at the villa given to him by Cosimo de Medici at Careggi, just outside Florence, early in the 15th century for the (Florentine) Platonic Academy. The grove was sacred to goddess Athena with 12 olive-trees supposedly derived from the original one she had caused (in the myth) to spring from the Acropolis rock.

An "academic" was originally a member of Plato's Academy, or, in other words, a disciple of the Athenian philosopher. It would be very presumptuous to attempt to present here, or even adumbrate, the many and varied aspects of Plato's teaching since, as has been said, "Western philosophy is a series of footnotes on Plato" (Raju quoting Whitehead 1971: 15). Even this description is not adequate nor does it indicate Plato's main concern. Very briefly, Plato's philosophical paideia aimed at the cultivation of the ethical virtues and Self-knowledge (*Charmides* 169E; *Alcibiades* I, 130E-133; *Phaedrus* 229E) and the elevation of the human condition to a supramundane godly state (*Theaetetus* 176A; Laws 716C). This was to be achieved through training in dialectic and the development of the reasoning faculty (*Republic* passim, esp 534D). This training in dialectic had nothing to do with Hegel's or Marx's dialectic and was not, of course, arguing in the abstract, as is usually thought and as it has become in modern times; it was rather discriminating between the transient and the eternal, the inessential and the essential, the unreal and the real or true. And in this training towards wisdom the academician practised some kind of meditation, an withdrawal from the material world of the senses, that ever changes, to the inner world of the spirit or true being, that is immutable and everlasting (eg *Phaedo* 79C; *Phaedrus* 247C-E). Such, moreorless, was originally "academic" study and practice – not substantially different from practical Vedānta (or Rājayoga)[1]. At the same time and long before Plato there were also the sophists, who sold what may be regarded as "ordinary education", and various other vendors of knowledge like prophets and purifiers, healers and magicians and charlatans.

Such was the "academic" study and practice of the members of Ficino's (Neo-)Platonic Academy also, where, under Cosimo's encouragement, Ficino revived Plato's philosophical paideia. At the same time in Florence there was the University; elsewhere in Italy and Europe were other suchlike institutes of learning. In these were taught the "humanities", that is Rhetoric, Poetry, Law and the like. These had developed from the studies of learned monks, schoolmen like Abelard, Albert Magnus, Thomas Aquinas, Roger Bacon, Duns Scotus, William of Occam (hence "Occam's razor") et al. But by the 15th century literacy and learning was no longer the privilege of monks and churchmen. Many members of Ficino's Academy were also distinguished men of letters and teachers in Universities, like Benivieni, the greatest physician of Florence in the 15th century; Buonincontri, poet and astronomer; Collucio, Professor of Poetry and Rhetoric at Bologna University; Landino and Naldi at Florence University; and so on (OM, 1983; Kristeller 1965, 1972).

Thereafter University staff appropriated the term "academic" to themselves, when Platonic Academies (there was one in Rome also, under Bessarion) ceased in the 16th century, and later new Academies (but not Platonic) were established in various countries. The aim now was not the Platonic aim of Self-knowledge and union with immortal True Being through the cultivation of *aretē* 'virtue', meditation and the development of reason or discrimination; it turned into collecting and transmitting information, much of which becomes obsolete and useless after two generations (with the exception of basics in various disciplines like Maths and Sciences, History and Ancient Languages, though even the teaching of inflected languages changes every few years now). With few exceptions that prove the general rule, academics and scholars continue in this activity of collecting, and transmitting information (much of which will be discarded) and "doing research" which again produces new information, like the car industry which produces vehicles with in-built rust, that will be discarded after a few years' new research. By its nature this has little to do with wisdom. "Scholarship is an *ongoing* dialectical process" (Witzel 2001: 11.1) – not Platonic "dialectic". Academics have appropriated this Platonic term also and brought it down to their own non-Platonic practice.

Modern scholarship

10. All this scholarship is, of course, legitimate and necessary in modern conditions of life. But academics seem to have lost, or at any rate do not diplay, the capacity to discriminate between the transient and the eternal, between the inessential and the essential, between fiction (ie theory etc) and fact, with very unhealthy consequences.

Even today the aim of scholarship in all its aspects is declared to be truth and the promotion of true knowledge. It is natural and inevitable that scholars should form a theory (a hypothesis or a model) based on the available facts and data, and then use this theory as a tool for further research and collection of fresh data. In this manner we enrich our knowledge about, and come closer to the truth of, the subject we investigate. However, we often forget that others may construct different theories (or models) and use them for the same purpose. For various reasons – mostly self-interest – we get intensely attached to our own theory claiming that this alone is right and all others wrong or harmful. This is bad enough, but the next stage is worse. Those who have authoritative positions proclaim their own theory as the "mainstream" view or "orthodoxy", condemn other dissenting views and ostracize them and their holders from universities and similar establishments. Many cases in the (known) history of the world and the totalitarian states of the 20th century illustrate this all too plainly – with ostracism turning into persecution. But while we readily recognize the intolerance of the 'Holy' Inquisition, or of the various despotic rulers and of the totalitarian regimes of Hitler's Germany and Stalin's Russia, we do not recognize and condemn with the same rigour the milder but equally real intolerance that prevails today in most spheres of learning (and, of course, within ourselves).

11. This situation generates yet another unsavoury product. The degradation of scholarship does not stop with the absence of the pursuit of supreme wisdom and its substitution by the accumulation of ever-changing information. I doubt whether most academics realize, or care, just how degraded the modern system is in comparison with the Platonic one. The situation is summed up in the German saying *Drei Professoren, Deutschland kaput* 'three professors [and their theories] – and Germany is finished'; or the British anecdote where two University Dons are travelling by train and seeing outside some sheep one says “They are shorn of their wool” and the other replies “Yes, the side we see, at any rate”. There are signs, unfortunately, that academics are hardly interested in any knowledge at all. No doubt there are some scholars, both within and outside Institutes of learning, who are devoted to their subject and try as best they can to promote knowledge. But the vast majority are careerists interested primarily in income and position just like the workers in any syndicate or commercial enterprise. This is understandable since academics, like any other class of men, must earn an income to maintain themselves and their family. (Even as I am writing – middle of May 2001 – the Provosts of Universities and other Heads of Higher Institutes here in Greece are preparing to come out on strike suspending all work so as to safeguard their hard-won economic rights, threatened now by some impending legislation.) Beyond this, within the academic world itself we find all the sordid aspects of antagonism and self-interest, one-upmanship, hatred, back knifing, bigotry and similar ugly realities. Because of the tacit motto “publish or perish”, hundreds of useless papers are being produced, often without any interest in the subject, often with dishonest references to publications that have not been consulted, often borrowing the work of others without acknowledgment but with the necessary modifications, and not infrequently the supervisors present as their own the work of their students.

Another feature is the emphasis on the study of secondary material, often at the expense of the primary. So, one finds sanskritists who know all the latest translations of texts, and related studies in other fields (Greek or Celtic or whatever), but do not know that *pitárā* is as valid a dual as *pitárau* or that in *RV* I,32 Indra rehabilitates the Sun and the Dawn upon killing Vṛtra. I wonder where we would all be without the translated texts, the dictionaries and all the various reference books. All this, of course, becomes very easily pedantry, quibbling or hair splitting (as in the anecdote above).

12. In Indological matters, as in all fields, we have the established mainstream orthodoxy or standard model. A brief exchange between Prof Witzel and Dr Wujastyk (Indology, 8/3/2000) is informative. Dr Wujastyk writes: “I am afraid that I neither trust nor believe anything said by atomic scientists about humanistic subjects (or most other subjects). Let’s try to keep this list scholarly, shall we!” And Prof Witzel agrees.

I don’t know how atomic scientists would take this, but it does seem, from every point of view, a remarkably sweeping statement. Undoubtedly, atomic scientists may have nothing interesting to say about *some* “humanistic subjects”; but then “humanists” also may have nothing of interest to say about humanistic subjects. Surely atomic scientists are not more divorced from humanistic (and human) matters than specialist humanists? What is baffling in this steam-roller statement is that scientists are considered incompetent or irrelevant for “most other subjects”. This would seem to indicate a non-humane humanistic arrogance. To be scholarly, then, is equated with the indeterminate “humanistic subjects” (and a supercilious attitude?).

Unfortunately, this does not stop here, for the two scholars demonstrate all too blatantly that dissenting views also are “unscholarly” or, at any rate, that only those sharing their own views are “scholars” of Indology. This intolerance is displayed clearly in Prof Witzel’s wholesale attack on non-invasionists in *Frontline* Oct. 13, 2000. (I return to this in detail below, §**18**). We find it present also in his dismissal of E Leach’s rejection of AIT, E Leach who, as an anthropologist “of another generation... could not judge much of the relevant evidence independently: his is another non-specialist’s opinion” (27/4/01). *The only specialists competent to judge the AIT are, then, sanskritists like Prof Witzel.* Anthropologists like J Shaffer and D Lichtenstein are non-specialists and so are numerous archaeologists like J M Kenoyer and B B Lal (who, though Indian, had a distinguished career in USA Universities). This is clearly absurd. And its absurdity is shown by the fact that even S S Misra, a *bona fide* sanskritist and comparativist, Professor at Benares, was included in that *Frontline* list! No, Prof Witzel seems to say, that only *invasionist sanskritists* are competent to judge. Yet, I don’t believe that a man of such ability and love of Sanskrit would allow his mind to get entrapped in so preposterous a proposition.

Here it is worth reminding all mainstream academics that many important discoveries in specialist fields have been made not by mainstream scholars wrapped up in their oppressive orthodoxies, but by non-specialists. R Bauval was not, as mentioned earlier (§**6**), an academic egyptologist but an engineer, who, yet, loved and pondered deeply the ancient Egyptian culture. Michael Ventris, who deciphered the Mycenaean linear B writings, was not an academic classisist but an architect who loved the ancient Greek culture. As for Heinrich Schliemann, who unearthed the Mycenaean "new world for archaeology" – he was a retired merchant with great faith in Homer's *Iliad*, unlike "the Classical pundits of his day" (Taylour, 1983: 9) and their 'mainstream' views.[2]

Then, many great discoveries were made by scientists in various fields not because of the mainstream doctrine or theory but in spite of it. W Beveridge lists many such cases in his study including E Jenner's vaccination and Sir A Flemming's penicillin (1957/68: ch 3). Einstein himself was not an academic when he published his important papers in 1905.

So let mainstreamers be not too hasty and arrogant to think they alone hold the keys to the kingdom of correctness. They hold some keys but only to control who enters academia or who publishes in academic Journals, and who doesn't. Surely some flexibility would harm nobody.

13. In his *Frontline* article Prof Witzel postulates 4 standards for writing history – with which no rational scholar would disagree. These are: "(1) openness in the use of evidence; (2) a respect for well-established facts; (3) a willingness to confront data in all relevant fields; (4) independence in making conclusions from religious and political agendas" (p 14).

Yet even no 2, the fundamental necessity to respect facts, gets coloured, distorted or ignored in the conditions of contemporary academia. Not being a saint of a scholar myself, I often catch myself presenting hypothetical elements as though they are facts or emphasize unduly facts favourable to my thesis; and I observe this in the work of all scholars. The AIT itself is a case in point: it is a *theory* that remains to be proved by unambiguous evidence (not by further theories). As Lord Renfrew put it, the AIT "comes rather from a historical assumption about the 'coming' of the Indo-Europeans" (1987/89: 182). But as archaeological evidence has not showed up for the last 30 years, the invasionists are now working out a new hypothesis whereby theAryan entry is accomplished *without leaving archaeological traces*. The fact is that the AIT is now *sub judice* and indologists and indoeuropeanists of all denominations should realize that from the moment we have two or more proposed urheimats – as indeed we have – then all are equally controvesial. The ProtoIndoEuropean and other Proto-reconstructions also are presented as facts, whereas, of course, they are conjectures. Comparativists of the older generation were cautious about the value of these reconstructions, that cannot be verified (eg Lockwood 1969, 1972), but 10 to 15 years later the new ones (Baldi 1983, Hock 1991 et al) began to lose restraint. Now they use the conjectural forms as if they are real words, the way other people chew gum or chain-smoke. It is not that new material has been discovered – a manuscript with Proto-Germanic or a rock-inscription with genuine PIE: no, it is just habit taking over. The confidence these asterisk-users exude is no different from that of the heavy smoker (and I have been one) who feels that his acquired habit is *most natural*.

I do not hesitate to use linguistic facts. I did so earlier in §**5**, end. Here, again, I do not doubt that the horse was domesticated and trained fairly early in the pontic steppes (Witzel 2001: §21, n 188; Anthony & Brown, 2000; Kazanas 1999: 34, n 16) at c 4000 and earlier, but I reject the suggestion that the horse was taken from there to India, on linguistic grounds as well (for other grounds, §**5**, above). Sanskrit has *aśva*, Avestan *aspa*-, Greek *hippos* (Mycen *iqo*-), Latin *equus*, Gothic *aihva*, Gaulish *epo*-, Old Irish *ech*, etc. But Old Slavonic has *Konji* and all Slavic languages have variants thereof. Now since, if we take the Urals as the urheimat, it would be ludicrous to suggest that all these people (west and east) innovated using the selfsame stem, the reasonable suggestion would be that the common IE stem travelled (with perhaps the horse) from a different centre and got established in all these regions but was not accepted by the people of the steppes because they already had a long tradition of domesticating the horse and so retained their own word for it (Kazanas 1999: 24, 34 n 14). This is an example of a hypothesis based on linguistic facts as distinct from one based on linguistic hypotheses (i.e. conjectural words with asterisks).[3]

H H Hock has examined and illustrated (1999) the difficulties involved in the non-invasionist view and its corollary, that the other IE branches emigrated out of India. As one would expect from one of the foremost specialists in the field of comparative linguistics, this paper is instinct with words with asterisks (pp 5-10, 14-5). Prof Witzel presents this paper (2001: 29, etc) as though it proves the invasionist thesis that there was no emigration out of India – although he does admit (p 30) such a possibility *theoretically* (as if the opposite notion is not *theoretical*). This is not quite so. Hock shows that some of the notions S S Misra used (1992) to demonstrate the origin of the IE languages from Vedic are wrong. I agree partly with Hock's general drift, though not because of his asterisk-word arguments which I distrust wholly (my n 3). Elst had anticipated much of this, without referring to Misra in his *Indigenous Indians…* (1993: 80-137), where he argued, among other things, for the irreversibility of palatalization. Hock relies entirely on "comparative reconstructions" (p 9), which are pure hypotheses, and on "regular sound changes" (ibid), which (as shown in my n 3) are anything but regular and mostly unreliable. He also seems totally unaware of J Nichols's evidence (see §**8**, above) which requires a locus of dispersal at Bactria-Sogdiana – unlike Hock's own conjectural and vague "vast area from East central Europe to Eastern Russia" (p 17); in this Hock omits also the South-east Anatolia of Lord Renford and South-of-Caucasus of Gamkreligze and Ivanov (et al). Be that as it may, Hock makes two important points. (a) He states (p 12) that apart from the Gypsy emigration there are *three* other emigrations of Indo-Aryan languages out of India: "Gandhari Prakrit (in medieval Khotan and farther east), and Parya (in modern Uzbekistan)... and Dunaki (close to present-day Shina)... to the outer northwestern edge of south Asia" (also in Hock 1996: 82). (b) He states also that the PIE could "a priori" have been "originally spoken in India" (p 11) and rejects the idea *not on linguistic* but archaeological (!) grounds (p 13-7) of the kind usually employed by invasionists (horse and chariot). He also invokes the "principle of simplicity" (p 16): one migration into India as against many out of it. But here he ignores the fundamental fact that *there is plenty of evidence of IE branches invading the areas they occupy but there is none for India*. This makes considerable difference, surely. Also, most European nations and the Iranians preserve very early traditions of migrations but not the IndoAryans: this too must count just as much as an (ill-conceived) idea of simplicity. In his usual fashion, Prof Witzel does not mention these points. What is most important here is that according to Hock **there are no substantial linguistic arguments against the proposition that IE branches moved out of India**.

Most interesting is postulate no 4. This sounds fine at first, but is terribly ambiguous if you think about it. Is atheism or agnosticism "independence" from religion? Or are such attitudes "religious" also?… What of conclusions formed in a deceitful, greedy, lascivious or similar frame of mind? … And what of "objective" scholars' worship of their own pet theories? ... Can an irreligious mind fully appreciate a religious hymn, lik a Psalm or an *RV* hymn?… To a religious person historical writings by an irreligious/atheist/agnostic mind would seem highly prejudiced or false and vice versa, of course. Which one is right or objective?

The political aspect is just as problematic. We can set aside marxist, fascist and similar political attitudes. But what of the prevalent capitalist attitude?… It may be called "free-enterprise" or liberal and the like, but how much freedom is in reality enjoyed by the large masses of people? … All economists from Smith and Ricardo to M Friedman and J Stiglitz agree that a tax on land-values creates no distortions. Yet such a tax is not levied because of entrenched interests; so the current capitalist system is taken to be "natural" reality, and history is written from capitalist premises. Why, moreover, assume, as is generally done these days, that the democratic system is the best?… Plato and Aristotle placed it at the bottom of their lists of political systems. Yet like capitalism, democracy forms an unquestioned premise of most thinking and of historical writing in modern times.

Then there are surely personal and family prejudices as well as national or racial – all embedded in our mind.

Is then objectivity or "truth" (uncoloured by preconception and prejudice) impossible?… In the ordinary conditions of contemporary life, yes, this would seem to be the case. But the situation is not entirely hopeless, since mystics, yogis and artists (more obviously poets) appear to reach unusual states of awareness transcending all ordinary contents of the mind and all usual prejudices, habits and limitations. But such a state of mind does not appear to be common or to come easily and when it does come, it does not concern itself with debates of this kind – as is obvious in Krishnamurti's *Notebook* (1978: passim), to mention one example.

The Platonic schooling aimed at this state: "Of all good things, both for gods and men, truth alone stands first; every man then should partake from his early days of truth, if he wishes to be blessed and happy and so live his life as a true man" (*Laws* 730 C). Plato modified very considerably in the *Laws* his ideal state that was controlled and closed in the *Republic*: he stressed the need for freedom and pointed out – rightly or wrongly – that the Persians degenerated because their governors "had removed to a great extent the freedom of the people imposing despotism" (697 C-D). Nonetheless, he still would not allow poets and painters (and presumably historians) into the State if they do not express the truth (*Laws* 817). For Plato, the truth or highest reality included the presence of gods – something most strange to most people of our times. Plato describes clearly in the *Phaedo* how one's mind (literally 'soul' *psuchē*) reaches this higher state by withdrawing from the senses and the physical world into itself, into the realm of pure being, at rest by itself, without movement and change (79 C-D): this alone would appear to be freedom or true objectivity.

With such criteria not many scholars of today would be admitted into Plato's Academy or his ideal State. Most scholars would shrug their shoulders with indifference, and perhaps rightly so. But then why consider Plato such a great thinker, write so many books about him and persist in studying him – if his thought is inferior to our own? … And what of our much vaunted "objectivity" and "promotion of knowledge"?

In one case, the editors of a UK University Journal accepted, after the due processes of refereeing etc, an innocuous paper on Indology for 1999 and even invited the writer for a Conference, but when they found out that he was a non-invasionist they cancelled the pubication of his paper and the invitation, *without explanation*. Such is the prejudice of "orthodox" thinking (and such its "advancement" of knowledge) that even one's word is not kept.

It would seem then that scholars who want to reach conclusions untainted by prejudices and preconceptions will need first to return to the old, original study and practice in Plato's Academy (above §**9**) or something very like it. Otherwise, even with the best will in the world, our habits, assumptions and prejudices, of which we are mostly unaware, will keep interfering.

I am sure Dr Wujastyk wants his INDOLOGY site to be very "scholarly" and free of error and paranoic notions. Most of the material in it is extremely useful and the whole conception deserves praise. But it is also heavily prejudiced. *The non-invasionist view is not presented at all nor the spiritual aspects of sacred texts*. It is curious how all classes of people extoll the democratic processes, justice, fairness for the underdog and the like (the Presidents of the USA are quite adept at this), but as soon as their own interests are threatened all these slogans go by the board (mark how any USA administration reacts as soon as american businesses are threatened anywhere in the world).

14. I mentioned in passing political/economic matters (§**13**, middle). The degradation in the field of Political Economy is quite startling. In 1994 were published two books – *The Death of Economics* by Prof P Ormerod and *The Corruption of Economics* by Prof M Gaffney (and F Harrisson). It should be first noted that the discipline is now called *Economics* thereby showing that it is no longer concerned with the ‘Polity’, ie the larger body of the Nation and its organisation, but ony the increase of profits.

In his Preface, Prof Ormerod wrote: “Even to the intelligent of the public Economics is often intimidating. Its practitioners pronounce with great confidence in the media, and have erected around the discipline a barrier of jargon and mathematics which makes the subject difficult to penetrate for the non-initiated." This, of course, applies to most academic disciplines today. The distinguished economist wrote also: "Yet *orthodox economics* is in many ways an empty box. Its understanding of the world is similar to that of the physical sciences in the Middle Ages. A few insights have been obtained which will stand the test of time, but they are very few indeed, and the *whole basis of conventional economics is deeply flawed*" (emphasis added). Commenting on the economic crises afflicting the world, Prof Ormerod says: "The orthodoxy of economics, trapped in an idealized, mechanistic view of the world, is powerless to assist" (1994). Be it noted that other eminent economists like Joan Robinson and JK Galbraith wrote earlier in a similar vein

To discover perhaps the cause for this malaise one should go back to the turn of the 19th into the 20th century, when the fledgeling discipline was effectively and thoroughly corrupted by academics, probably under the behest of vested interests in land (railways and other corporations) and venal politicians in the USA.

In 1908 SN Patten (Univ of Pennsylvania) wrote: "If the new group of thinkers called themselves sociologists or historians they *might be disregarded*. But they openly claim to be economists; and the worst of the matter is, they have, so far as statement goes, the mass of the older economists on their side. Nothing pleases a socialist or a single taxer better than to quote authorities and to use well-known economic theories to prove their case." "Socialists" were not socialists of the type of Proudhon or Marx, who were almost totally unknown in the USA; “socialists” and "single taxers" were those who favoured a single tax (on land values) and the abolition of all other taxes, following certain ideas of Adam Smith, Ricardo and others, culminating in Henry George, the most vociferous exponent (1879). Patten continued: "The economists rubbed their eyes in surprise when this assault first began; but they soon realized that their favourite authors were not so perfect as they supposed and that **economic doctrine must be recast** so that it would rest wholly on present data. This, I take it is the real meaning of the **present movement in economic thought**. It will not accept socialism; and to free itself from the snares into which it has fallen through the careless statements of its creators, **it must isolate itself more fully from history, sociology and other disciplines** that give undue weight to past experience" (1908, emphasis added).

Note that the great classical economists (Smith, Ricardo and JS Mill) made, according to these new “scientists” of Economics, "careless statements" necessitating a new "movement in economic thought" that would (and eventually did) recast the mainstream economic doctrine. This was done primarily and chiefly by JB Clark who was Professor at Columbia and, in a series of publications, had laid down the Neo-classical economic theory, isolated from history, sociology and other disciplines and "past experience"! This degradation of Political Economy came about through Professors but under the influence of holders of very large land-property: Wharton, who financed the Wharton School at Pennsylvania, had over 100.000 acres and Seth Low, who was Provost at Columbia, had property of similar proportions. Details with extensive bibliography are given in the *Corruption of Economics* (Gaffney and Harrison, 1994 passim). A great number of academics, who refused to follow the mainstream doctrine, were ostracized or fired from their posts as a result (ibid, pp 51-2). After Clark and Patten, a series of academics cemented the new economic theory – economists like ERA Seligman, FA Fetter, R Tely, M Clark and F Knight. The oppressive situation has not changed much, as Prof P Ormerod shows: "The obstacles facing academic economists are formidable, for tenure and professional advancement still depend to a large extent on a willingness to comply with and to work within the tenets of orthodox theory. It is a source of encouragement that more and more economists are willing to look at alternatives, despite the risks they take in doing so" (1994: xx). Frankly, I think Ormerod is over-optimistic.

That Political Economy should have been corrupted by vested interests in finance and property is not perhaps so strange. But we meet a similar phenomenon in Palaeoanthropology which deals with the culture and evolution of early man (and hominids).

15. In 1993 came out a voluminous study of more than 800 pages, *Forbidden Archeology*, by two researchers, MA Cremo and RL Thomson. With total candour, the two men say that they are members of the Bhaktivedanta Institute (for Krishna consciousness) and that they were encouraged by their spiritual master Swami Prabhupāda "to critically examine the prevailing account of human origins and the methods by which it was established" (pxxxvi). This occurred because in Vedic literature is found the idea that the human race is of great antiquity whereas the current "mainstream" teaching is that human beings (*homo sapiens*) are of comparatively recent origin, about 100.000 BP. The research began in 1984 and the two were aided by Thomson's assistant S Bernath (Thomson and Bernath being mathematicians).

They found much published material that man is in fact in existence for million of years but that all this has been suppressed or dismissed so that only the current view (in many cases based on inadequate data) should prevail.

The two writers quote many authorities in the field. For instance, CE Oxnard who, referring to the uproar created by the (temporary) victory of one opinion over another, pointed out that: "This may well have resulted … in the burying of *that part of the evidence* upon which the contrary opinion was based … [but] … it should be possible to unearth it" (p 718, emphasis in the original). And this is exactly what Gremo and Thomson (and Bernath) have done: they unearthed vast amounts of buried and forgotten evidence which indicated that human beings are much much older than the current mainstream ideas state (chs 2 – 6 and Appendix A2).

They also quote G Carter: "When a new idea is advanced, it necessarily challenges the previous idea. This disturbs the holders of the previous ideas and threatens their security. The normal reaction is anger. The new idea is then attacked, and support of it is required to be of a high order of certainty. I have never been able to accept this. *It assumes that the old order was established on high orders of proof and on examination this is seldom found to be true*" (p 24, emphasis added). This aspect is examined extensively in chap 7 and after, and it is shown that some mainstream evidence is established on anything but a high order of proof. This aspect is, of course, most relevant to the controversy round the AIT[4] . For instance Old Slavonic is not as old or as near the PIE as Vedic is (see §**19**, below); the Slavonic mythology does not retain even a fifth of the common IE stock preserved in the *RV* (Kazanas 2001d); the language does not have the common IE stems for 'horse', 'dog', 'bear' etc (see Kazanas 1999: 24) but its own isolated *kunji, pis, medved* etc: yet most invasionists do not seem to be disturbed by these blanks, accept the Steppe-Urals as the urheimat, and then make all sorts of demands in respect of Vedic and the Indian indigenous homeland view.

Another relevant passage concerns the suppression of "evidence that contradicts current ideas about human evolution: this evidence has been systematically suppressed, ignored or forgotten, even though it is qualitatively (and quantitatively) equivalent to evidence favouring currently accepted views on human origins. When we speak of suppression of evidence we are not referring to scientific conspirators carrying out a satanic plot to deceive the public. Instead, we are talking about an ongoing social *process of knowledge filtration* that appears quite innocuous but has a substantial cumulative effect. Certain categories of evidence simply disappear from view, in our opinion unjustifiably" (p xxvi, emphasis added). One mechanism of this filtering process is given with a quotation from another eminent scientist, S Zuckerman: "Over the years I have been almost alone in challenging the conventional wisdom about the australopithecines… but I fear to little effect. The voice of higher authority had spoken, and its message in due course became incorporated in text books all over the world" (p 717). This too is very relevant to the AIT controversy.[5]

We need only mention another aspect of suppression of 'anomalous' findings – the rejection of research-articles, sometimes without any explanations, and the subsequent "personal abuse and professional penalties for those who dared to present and defend them in scientific literature" (pp 346-50, 361-6). This also is common practice now in Indology. Academic journals in the West will not publish any articles advocating an indigenous homeland for the IndoAryans or a date 3000+ for the *RV*.

Naturally, the establishment struck back with various negative reviews like that of J Marks, Prof of Anthropology at Yale, in the *American Journal of Physical Anthropology* vol 93 (140-1), 1994. Marks describes the book as "Hindu-oid creationist drivel" but he is *guessing* (wrongly) here because the book contains nothing of the author's view of the creation of the world and man; the frequent long extracts from anthropologists, zoologists et al, are hardly "drivel". There were many more reviews, some positive, most of them negative.

I end this section with yet another incidental reference to Prof Witzel – "Rajaram's 'Piltdown Horse'" (2000: 5). This is a reference to the famous Piltdown skull found in Sussex, England c1910 (the exact date remains unknown). The skull was partly human partly ape and caused great furore when it was discovered in the early 1950s that it was a deliberate hoax (Walker and Shipman, 1996: 45, 75; Reader, 1988: ch 4). Suspicions for the culprit(s) of this almost successful forgery have fallen not on someone of the "lunatic fringe" as G Erdosy insultingly refers to another Indian writer, S Talageri, but on highly respected figures of the establishment, like AS Woodward, G Elliot Smith and PT de Chardin (Reader, p75), W Soldas and even Sir A Keith (Cremo and Thomson, pp 522-3).

16. I conclude this look at the unhealthy condition of academic disciplines with a brief mention of the Classics and particularly Greek Studies.

In the second half of the 20th century several scholars of Greek turned increasingly to the Near East and found there the origin of many elements in the Greek civilization. There is no doubt that both in pre- and post-Mycenaean times the Near East exercised considerable influence on Greece (and vice versa to some extent) through frequent contacts and exchanges. But, as often happens when a fashion gathers momentum, this trend ignores other aspects, like the IndoEuropean descent of many elements in the Greek tradition and ascribes these also to NearEastern influences (see Kazanas 2001b). The publications are many but I shall take only one of the more impressive examples.

Prof M L West examined many incidents and motifs in the archaic Greek literature and adduced parallels from Near Eastern traditions. Many scholars (eg Arora 1981; Baldick 1994) have noted the parallel between Penelope' s and Draupadī' s archery contest in the *Odyssey* (19, 17) and the *Mahābhārata* (I, 175-180), but West's position on this is very curious (1998: 431-3). He quotes two Egyptian inscriptions of c 1420 that praise Amenophis II for his prowess in archery. He then mentions iconographic material that shows pharaohs Ay (c 1320) and Rameses II (c 1350) shooting with their bows at targets that "might at a casual glance be taken for double axes" and (following W Burkert and P Walcot) finds it very plausible that *misrepresentations of such scenes "may have given rise to the idea of shooting through a line of axes*" (p 432: my emphasis). All this, of course, is just as possible as so many other things in daily life are possible – misreading something, being witness to a murder, being struck by lightning and similar accidents or coincidences – but no more. Then West, in his passion for piling up parallels even if they are irrelevant, mentions a Hittite narrative about an archery contest for which *there is no prize whatever but only the king's satisfaction in winning*. Not one of these citations has the slightest affinity with Penelope's situation – a queen thought to be a widow who is pressingly courted by a bunch of repugnant idlers and manages, with Athena's help, to think out various devices to protect herself and her young son and keep the vile suitors at a distance while hoping for her husband's return. One can't help wondering why West mentions them. One's wonderment increases when West refers to the *Rāmāyaṇa* and

Mahābhārata and the epic of *Alpamysh* (a Turkic poem that arose in Central Asia c 8th or 9th century CE), admits the existence of parallels but states “it is something that we cannot pursue here” (p 433)! Why not in heaven’s name?… One can only suppose that (along with Burkert, Walcot and other scholars cited, p 432) West decided that archaic Greek literature must derive only from NE sources. Why the Greeks, a people obviously fond of fighting, piracy, pillage and conquest, who obviously used bows and axes in their fighting as well as swords and spears, could not think for themselves of archery contests and needed diverse NE sources (some misrepresented), is a mystery none of these scholars bothers to consider.

This example has a rather unfortunate aspect in that Prof West does in some of his other works refer to Indic parallels, unlike many other hellenists who care little for the IndoEuropean (and Vedic) connections of the Greek culture. And here I end my brief and rather one-sided survey of examples from modern academic study and practice.

The supramundane state

17. From the cases examined we can draw many conclusions, beyond the general one that scholarship or academic practice (barring individual exceptions) is no better and no worse professionally or ethically than any other area of human activity.

There is scholarship and scholarship and scholarship. In the humanities today most of it is arguing about dates and textual parallels, more and more theories and secondary material (who said what when). And so scholars write about ancient systems of Astronomy without any practical knowledge of Astronomy; or of Āyurveda or Acupuncture without ever having practised any Medicine; or of Plato’s philosophy or Vedānta or Rājayoga or Buddhism without ever having put into practice the ethical injunctions of these systems and without having tried meditation. Obviously all such “studies” are of very limited value, if any. There is a subhāṣita that sums it up nicely: *pustakasthā tu yā vidyā parahastagataṃ dhanam; kāryakāle samutpanne na sā vidyā na tad dhanam* (‘knowledge in books is like money in another’s hand; when the time of need comes we have neither the knowledge nor the money’).

A careful observer will find not only obvious religious and political motivations but also subtle ones – common attitudes and motivations that are taken for granted and are never questioned.

We pretend to know that which we don’t know, not only in the sphere of the ultimate reality but also in much smaller areas and specific subjects. We know that this happens but, again, we pretend it doesn’t happen. So we have dishonest scholarship – as also when evidence is deliberately suppressed or ignored.

Ignorance is not only an absence of knowledge, a lack of information. Often, it is an active disregard or avoidance of things we know too well but prefer to ignore or pretend they are not here.

We know but forget, or ignore, and don’t want to consider, that there are other states of experience or being, of mentation and perception – like the one described by Wordsworth: "And I have felt/ A presence that disturbs me with the joy/ Of elevated thoughts; a sense sublime/ Of some thing far more deeply interfused,/ Whose dwelling is the light of setting suns,/ And the round ocean and the living air,/ and the blue sky, and the mind of man;/ A notion and a spirit, that impels/ All thinking things, all objects of all thought,/ And rolls through all things" (*Tintern Abbey*).
Or by Shelley: "The One remains, the many change and pass;/ Heaven’s light forever shines, Earth’s shadows fly;/Life, like a done of many coloured glass,/ Stains the white radiance of Eternity…" (*Adonais*).
Or by William Blake: "To see a world in a Grain of Sand,/ And a Heaven in a Wild Flower,/ Hold Infinity in the palm of your hand,/ And Eternity in an hour…" (*Auguries of Innocence*).
Or by Henry Vaughan: "When on some gilded cloud, or flower/ My gazing soul would dwell an hour,/ And in those weaker glories spy/ Some shadows of eternity/…/ But felt through all this fleshy

dress/ Bright shoots of everlastingness" (*The Retreat*: spelling modernized).

One could gather many more examples from other poets and from mystics, from many countries. Krishnamurti's *Notebook* of 1961 has 200 pages of descriptions of this extraordinary state of ecstasy, immensity, power and beauty. Here is given one passage: "Woke up early this morning with an enormous sense of power, beauty and incorruptibility…in which nothing could exist that could become corrupt, deteriorate. It was too immense for the brain to grasp…limitless, untouchable, impenetrable. Because of its incorruptibility, there was in it beauty. Not the beauty that fades… One felt that in its presence all essence exists and so it was sacred. It was a life in which nothing could perish…With it all there was a sense of power – strength as solid as that mountain. (...) Yesterday, driving through the narrow valley … there was this benediction. It was very strong and everything was bathed in it.The noise of the stream was part of it and the high waterfall… It was like the gentle rain … and one became utterly vulnerable; the body seemed to have become light as a leaf, exposed and trembling. This went on … talk became monosyllabic. The beauty of it seemed incredible" (pp 30-31).

Naturally, questions arise: – In such a state, what would a fact or a succession of facts be like?… How would one look at, interpret and evaluate, say, a hymn of the *Rgveda*, or any other text?… What sort of history would one write then?… Are perhaps many of the *RV* hymns "history" conceived or envisaged in such a state? ... Can there be real understanding in any humanistic discipline without some experience of this state? And so on.[6]

It may be easy to answer 'yes' or 'no' to these questions, but, obviously, only first hand experience would furnish valid replies. And just because most people do not have, do not know how to reach or do not wish to reach, such states, it does not mean that we should ignore the experience of those who do reach them and the testimony given by the state itself. After all, our usual state is not all that remarkable. We don't need expert psychologists to tell us that we are lost mostly in dreams and mental dialogues and streams of circling day-thoughts and desires, attractions and aversions, with our attention flitting uncontrollably from object to object (within or outside) and our feelings and moods constantly changing. To claim that we have objectivity in this state sounds like a very bad joke.

Prof Witzel again

18. Prof Witzel made on the Internet (27/4/01) some critical comments on my 'The *RV* date: a Postscript'. I don't doubt he is right in many of his remarks, even in that I "overlook contradicting evidence" (presumably within my thesis). My mental baggage is inded a "surprising mixture" of knowledge and ignorance, with ignorance having the upper hand for the most part.

I have no personal quarrel with Prof Witzel – only with the idea of AIT, (which I consider wrong for the reasons given), and with his mode of attacking those who dissent from his view. If he or anyone supplies adequate evidence for the AIT, acceptable to, say, anthropologists like J Shaffer or archaeologists like J Kenoyer, I would not hesitate to embrace it.

However, Prof Witzel, through force of habit perhaps, makes some curious slips in his strictures of me. He writes: "It would have served Dr Kazanas' credibility better if he had not let down his guard and had shown his anger about our deconstruction of Rajaram et al and our comparisons with other "patriotic" writers' fantasies. To defend the undefendable does not add stature to one's own."

First of all, I was not angry at that time nor am I now. Then, it should be plain that I was not defending anyone's fantasies but criticizing sharply Prof Witzel's ill-informed mode of attack: –
(a) I pointed out that he has no right to bracket *all* opponents of the AIT with some writers who make inordinate claims: there are writers like Devi Chand, S Kulkarni and P Choudhury, who express very strange notions and/or exorbitant claims, yet these were not mentioned (but are mentioned in Witzel 2001). Among those mentioned, DK Sethna, SS Misra, S Kak and K Elst,

whose writings I know well, display only level-headed scholarship within all acceptable academic standards. Sethna is no nationalist nor Hinduist, but a Parsi who follows Shri Anrobindo's transcendental teaching. As for SS Misra – he was Professor at Benares and wrote many academic studies. Kak makes no extravagant, nationalist or religious, claims in his publications on Indology and Elst is a Belgian Roman-catholic whose matter-of-factness might well annoy his opponents. (Now, yes, I have defended these scholars.) (b) I pointed out there is great confusion among different invasionists who claim different locations for the IE urheimat and therefore cannot be using *well-established* facts since such facts would lead to identical conclusions. (In his 1999 article, Hock acknowledges this difficulty, p 12.) (c) One of his comments I called"irrational". Indeed, his statement "Ironically, many of those expressing these anti-migrational views are emigrants themselves, engineers or technocrats like NS Rajaram, S Kak and S Kalyanaraman, who ship their ideas to India from U.S. shores" is a *non-sequitur*: I find nothing ironic about contemporary Indian emigrants expressing views against an invasion that allegedly took place 3500 years; there is no semantic connection. It is also a false statement in that Sethna, Misra et al did not wait for Rajaram's or Kak's ideas to be shipped from the USA; on the contrary, it is very obviously from India that these ideas were brought into the USA. The issue is very old and, in any event, Sethna's *Karpāsa...* came out in the early 1980s. (d) I also said that Prof Witzel does no know what "New Age" writers are – since this label does not fit D Frawley, G Feuerstein, K Klostermeier and Dr Elst, as V Agarwal shows succinctly (2001a). All these statements are true. True was also my assertion that he prevaricated about Sarasvatī since he did not mention some seminal references to the river (II, 41, 6 *ambitamā* etc; VI 52, b *pinvamānā* etc; VI 61, 8-13 'mighty, swift-moving…'; etc), which could not possibly apply to a river drying (or having dried) up, as shown in §**2**, above.

He says I defend all those people (27/4/01). In the opening statement of his brief critique of my Postscript (27/4/01) he refers to my other "related papers", i.e. my ABORI article of 1999 and my JICPR paper of 2000. Obviously one can't criticize a postscript unless one knows the text to which the postscript is attached – in this case my two papers 1999 and 2000. The second one he could not possibly have consulted because, due to various delays, *the volume has not come out yet*; in any case, this is not verry different from the 1999 one. If he had consulted the ABORI 1999 (2000), vol 80, he would have seen that I use more than 35 titles of Western scholars and *only five non-invasionist ones* and that I actually criticize both SS Misra and Sethna. I wrote: "Some scholars (Misra 1992 and Sethna 1992) assign the *RV* to the 5th millennium and even earlier. This seems far-fetched. Misra's linguistic evidence is unreliable on its own – while Sethna follows him adducing some not entirely convincing data" (Kazanas 1999:32-3). I wrote also that the spoked wheel (mentioned in *RV* V, 58, 5, VIII, 7, 3, etc) provides a sure check against claims for such very early dates (p 33). So much then for defending. (It other respects, some of the notions there need revising.)

I have no doubt that Prof Witzel is a brilliant Vedic scholar and I admire him for his patience in answering *On Line* the same questions again and again. At least he does not indulge in the "ostritch act" displayed by most other mainstreamers who pretend that dissenters (non-invasionists, in this case) do not exist, and, discarding even elementary professional courtesy, do not bother to reply to factual practical queries. Regrettably I shall not follow him hereafter. I can put up with neologisms like "deconstruction" and "undefendable", even expressions like "boy, do they keep us busy" and fantasies like the "reconstructed" original name of the river Sarasvatī *Viš̌ampaḷ/ž̌ or Vipaḷ/ž̌ (2001: §25, end: note the uncertainty in the alternative forms). But as regards his persistence in undergraduate bravado, in "battling" against enemies and crusading for the salvation of India and indological scholarship, I can only say "Good luck". I am too old for such capers. Battles may give temporary excitement and an illusory sense of triumph in victory or disappointment in defeat, but at the mental level they are utterly unproductive.[7] And, frankly, I much prefer reading with my students an Upanishadic passage or an *RV* hymn to all this argumentation.

Dr Elst presents in some detail a different case where Prof Witzel dismisses and pretends to know a book he has not, in fact, read (1999: 55): it is S Talageri's 1993 book. (It is obvious he has not read attentively S Talageri's 2000 book also and so his criticism is often off the mark.) This is a great pity – as is his derisive arrogance. Unfortunately, practically all academics now indulge in this kind of dishonest scholarship – pretending to know books (or data or ideas) which they don't know in fact. It is a great pity that they do not display a sense of responsibility finer and wider than the strict letter of a superficial law. All standards of scholarship should always be safeguarded, of course. But obviously implicit in this is the safeguarding of the ethical foundation upon which scholarship, as indeed every other human activity, rests. When we turn our back to this foundation by pretending to know things we don't know in order, as we think, to maintain a position and reputation (and income) or to save face, or maintain a puffed-up fantasy of our own Papal infallibility, we undermine and corrode, as it were, the entire structure of scholarship. To err may be reprehensible but it is forgivable since it cannot always be avoided in our imperfect condition. To refuse repeatedly to admit the error(s), to pretend to be or know or do what one is not or does not know or do – this is not merely reprehensible but condemnable. It is a pity we all, and our Harvard Professor, do not realize that the example we give to the rest of the world from our position and function as bearers of knowledge is not a bright one and not at all conducive to the essential improvement of scholarship.

19. To return to the AIT, the demonstrable facts are very simple. There is no archaeological evidence at all for any mass intrusion; on the contrary, even invasionists, agree that "there is very clear evidence of a continuous civilization" down to 1400 (Witzel: 22/5/98). In fact, the continuity of this civilization remains unbroken until c 600 BC. J Kenoyer sums it up: "there is no archaeological or biological evidence for invasions or mass-migrations into the Indus Valley between the end of The Harappan phase, about 1900 BC and the beginning of the Early Historic Period around 600 BC" (1998: 174). And Prof Witzel echoes: "archaeology and palaeontology … have not found … immigration into the subcontinent after 4500 … and up to 800 BCE (2001: §7).

This is the inescapable and overruling fact. All other data should be examined under this light and arranged accordingly. Linguist-invasionists have other notions. They abandon the term "invasion" and adopt "migration" but since archaeologists will not have this either, they evolve a theory of "more gradual and complex phenomena" which archaeologists do not fully understand thereby "denying the validity of any migrational model" (Erdosy, 1995: pp xiii, xv). Let us leave aside the implicit charge that archaeologists are obtuse (whereas invasionist linguists are sharp and imaginative) which is derogatory sophistry. This complex migrational model has two and more waves of immigrants, not massive, and already acculturised to the Harappans (see §**3**, above). The gradual entry and the acculturisation prior to the entry are aspects of the new refined model. However, this version of the AIT strikes against two at least impediments: (a) Place-names and particularly river-names are, with negligible exceptions, Sanskrit. One explanation given for this is that "the Indo-Aryans could not, apparently, pronounce local names" (Witzel, 1995b: 326). And how is this known? Well, obviously, because the intruders retained and imposed their own names. A nice circular argument. Anyway the intruders did not preserve the local names – despite the fact that they had stayed long enough in neighbouring regions and had achieved acculturisation. Ah, continue the invasionists, the acculturisation concerns only material culture, it does not extend to language and the geographical nomenclature. (But on the other hand they were mostly bi-lingual: Witzel 2001.) So these immigrants slip into Saptasindhu (in several waves, over years and decades) and ignoring the local names begin to christen places and rivers with their own favourite and agreed names without creating any confusion and having the cooperation or consent of the natives. I can't believe all this, but invasionists apparently do. (b) The second obstacle is much harder to surpass: the *RV*, being the first literary document of the invaders, should reflect the material culture of the Harappans, but it doesn't; this is done by post-rigvedic texts, mainly the Brāhmanas and the Sūtra-texts. I dealt with this extensively (1999: 28-31) pointing out that it is not just rice or cotton that are missing from the *RV* – when the argument *e silentio* would not signify much, as Prof Witzel states rightly (2001: §23) – but most of the features of the IVC: urbanisation, large buildings, brick, silver etc.. I also showed (p 31, n 14) that *RV* I 133 does not refer to a ruined (Harappan) city – and so did Sethna (1992: 130-5). That later texts refer to actual ruins and *kapāla* 'pots herds' (Falk 1981: 167ff; Witzel 2001: §22) is rather to be expected. Thus the refinements in the model convince only those already convinced. The primary fact remains unshaken; its opponents have to produce hard evidence and more realistic arguments.

This primary fact is supported by several secondary facts like the native tradition, which knows nothing of an invasion/migration and places the *RV* (its arrangement) c 3100. True, the tradition (Epics, Purāṇas, astronomers) appears to be late, within 1st-6th centuries CE, but we must not ignore the weighty evidence of the classical sources (the Megasthenes report c 312-280 BC) giving related chronologies. Arrian (*Indika* 1, 9), Pliny (VI, 21, 4) and Solinus (52, 5) – all give dates of 6000+ BC for Indian royal genealogies: so this aspect of the tradition is at the *very latest of the 4th century BC*. I am not claiming here that the tradition is necessarily correct in all (though it could be in most of) its aspects but only that it is not as late as it seems at first sight.

Supportive of the primary fact is also what may be called the preservation principle. It is well known in History and Historical Linguistics (Hock 1991: 467-9) that, other things being equal, the culture and language of a people on the move suffer changes and/or lose elements faster and more than those of a people at rest (if nothing else, because with the latter the older generations have time to teach the new ones more fully). (a) We have Burrow's statement, "**Vedic is a language which is most respects is more archaic and less altered from original Indo-European than any other member of the family"** (1973: 34). Vedic is superior (and older) in respect of its inner organic cohesion: from roots (*dhātu*) by simple processes are generated.primary (*krit-*) and secondary (*taddhita-*) derivatives in nominal and verbal forms. Burrow again, "Chiefly owing to its antiquity the Sanskrit language is more readily analysable, and its roots more easily separable from accretionary elements than... any other IE language" (p 289). No discovery of new IE linguistic material has been brought forth to refute, and the presense of dialectal variants and innovations in Vedic (and Sanskrit)does not invalidate, Burrow's judgments. (b) In the realm of Mythology "the *RV* contains a decisively greater proportion of the common IE mythological heritage. In fact there is hardly a major motif common in two or more of the other branches that is not found in the *RV*" (Kazanas 2001d, sect III, 1). This is true also of the poetic devices used in the early poetry of the IE traditions. Here again the *RV* seems to contain all that is partially found elsewhere (Kazanas, ibid). Prof C Watkins writes: "The language of India from its earliest documentation in the Rgveda has raised the art of the phonetic figure to what many would consider its highest form" (2001: 109). The language and culture of a people on the move for many decades if not centuries, and their mingling with alien peoples, could not possiblly retain so much more of the common heritage than other peoples who moved little or not at all. (The Iranian *Avesta* is not so rich in retentions.)

A third important fact is that the Vedic tradition carries no memory of an immigration. Some invasionists quibble about this and claim various forms of "indirect" evidence of this in the hymns (Witzel 1995b) but when we see the very clear recollections in other early traditions, even if some of them are not correct, such quibbling is rather dishonest. The Iranians recall passing from 15-16 places, including the Saptasindhu; the Irish record 5 or 6 waves of migration/invasion; the Scandinavians thought they came from Troy; the Jews remember the early migration of Abraham from Ur; even the Greeks, whose epics have no such memory, do mention through Herodotus the fact that the Pelasgians (in Athens) had never moved whereas the Dorians did move about before settling; and so on (Kazanas 1999: 27). Surely this consideration is not to be dismissed lightly.

Devoid of such clear, fundamental facts, the AIT stands on two hypotheses. One is the coming of the IEs (§**2**, above) into various countries of Europe (Greece, Italy etc.) and its hypothetical extension into Saptasindhu despite the absence of any evidence. The other is the hypothetical arrival and stay of the Indo-Iranians in Afghanistan having come as one people from the North-West (or elsewhere) and then, for unknown reasons, splitting into two, the Iranians going westward and the IndoAryans eastward: this is necessitated, it is claimed, by the close kinship of Vedic and Avestan – though of course, this kinship can be explained in other ways just as easily.[8] In any case, as we saw in §§**8** and **13**, even the linguistic evidence does not support exclusively the AIT but can be, and has been shown to be (Hock 1999), capable of supporting emigrations out of Saptasindhu and the adjacent area. Two hypotheses do not, of course, make a fact: they make a bigger hypothesis that is even farther removed from reality and, as opponents find its flaws, will require more and more complicated and "refined" hypotheses to be maintained. The AIT results moreover in a startling paradox: we have an archaeologically attested culture of many centuries if not millennia with undoubted literacy but without any traces of religious texts, legal codes, scientific works and even simple secular fables (except most laconic legends on indecipherable seals), and, in quick succession, even as the older culture declines, an intrusive illiterate people with no archaeological attestation at all who yet produce within a few centuries (according to the AIT) all the literature that was missing from the previous culture. This is a unique situation that makes little sense.

Linguists don't seem to be able to stand back a little, out of the sweltering sands upon which they have erected the AIT and realize how much shifting confusion lies at its base. I pointed out that different linguists, examining the body of linguistic evidence, reach different conclusions regarding time-depths, place of origination and dispersals (Kazanas, 1999:17-8; 2001a: §3; above, §**8**). This diversity and disagreement can arise only from disregarding fundamental facts or from interpreting the evidence in a mode not consonant with its nature. I suspect both reasons operate in the case of the AIT. A third reason may be found in the words of Edmund Leach back in 1990: "Indo-European scholars should have scrapped all their historical reconstructions and started again from scratch. But that is not what happened. Vested interests and academic posts were involved." They still are.

Adjudication

20. "The burden of proof squarely rests on the shoulders of the advocates of the new autochthonous theory" (Witzel 2001: 32).[9] This is not so. The facts of the case assert that there was no invasion: in all such situations archaeologists and anthropologists are far more competent judges than linguists, or linguists assuming the role of archaeologists and anthropologists in order to preserve their linguistic theories. Consequently the burden of proof rests with the linguists: they have to convince all other experts that their theory is correct and superior to the (total absence of any) archaeological or anthropological evidence. Perhaps indigenists with the appropriate training should consider the study, research and provision of data demonstrating an Out-of-India trail (Witzel 2001; Elst 1999). But this could be done by invasionists as well – in sporting spirit if nothing else. After all, the entire situation is most unequal. While there is no archaeological, anthropological and palaeontological evidence for a mass entry into Saptasindhu between, say, 4500 and 600 BC, there is plenty of such evidence for all the other locations where an IE language was/is spoken. J Nichols (§**8**, above) proposed, on linguistic grounds, Bactria-Sogdiana as the locus of genetic diversity – a location very close to Saptasindhu and much closer than any other proposed urheimat. But we must not ignore the possibility that we may never be able to pinpoint exactly a PIE urheimat but only a fairly broad zone where the PIE language had already differentiated itself into several dialects. (cf also Sethna 1992: 75).

Invasionists every so often come up with new theories and evidence (that stops short of the Indus) while non-invasionists counter all this with fresh arguments. The controversy will go on for many years. If the Indus script gets deciphered in a way acceptable to most indologists (as Linear B was to hellenists) then the conflict will be resolved, though diehards may resort to new scenarios. But the decipherment may take many years or decades, since the legends on the seals are so brief. Meanwhile the attitudes of the opposing parties are hard, fixed, immovable. The one side hardly ever pays attention to what the other is saying and each one continues mostly a monologue. This situation obviously is unproductive.

Let us therefore go to Court.

Let both sides, invasionists and non-invasionists, agree to a truce and publish nothing for a fixed period – unless startling new evidence comes up. Let both sides prepare their brief and present it to a number of jurors (6, 10, 12 or whatever number) who will then decide the case. These must not be linguists or archaeologists or academics and should know as little as possible about the AIT, IVC or archaic Indian history: they should, in other words, have no preconceptions or prejudices. In all court cases the jurors are ordinary people, not necessarily experts in law or in fields related to the case under judgment and certainly not well-informed and therefore prejudiced. The experts are the lawyers and the witnesses for the prosecution and the defence. Thus the jurors should be selected with the agreement of both sides, from the sphere of Business and Management (private sector), Engineering, Architecture, and Medicine and should be successful in their fields. They should not be academics. It may be claimed that such people will not be able to understand the specialist terms of the disciplines involved. This difficulty arises in ordinary cases as well; the counsellors take care to present their specialist evidence and arguments in simple terms so that the jury understand. The same could be done here. These criteria will ensure that the adjudicators have the competence to distinguish between nebulous armchair theories and actual facts, as is required in this case.

I do not entertain the illusion that, even if such an event should take place, the resultant decision will be accepted by all as binding. But at least we shall have an indication of how disinterested professional people, practical and reasonable, view the affair.

This should not be difficult to organise.

NOTES

1. In fact, Plato differs somewhat from Vedānta in that in his view man neither originates in the Supreme Being nor returns to It; he is created at, and returns to, the somewhat lower though immortal level of the star-zone (Kazanas 2001c).

2. I would not be surprised if the IVC writing was deciphered by an engineer or similar non-specialist. Linguists and archaelogists have not succeeded so far. From the examples cited, it would seem that such cases require minds free from the burden of rivetted theories and preconceptions.

3. This aspect is a great nuisance because scholars have forgotten that they are dealing with *conjectures that cannot be verified* and by putting an asterisk think they can now deal with them as facts, and actually do so. One example (Witzel 2001: §11.5) is “the correct equation, sound by sound of Skt *dvā(u)*, latin *duo* = Armenian *erku* < IE **dwō(u)*: here **dwo(u)* can in no way be verified and, moreover, shows uncertainty with the alternate form of the backeted (*u*). Another example, culled by chance, is the hypothetical derivation of Gk *mousa* (Aeolic *moisa*) ‘Muse’ from **monsa* from Proto-Greek **montwa* from PIE **mon-tu-h₂* and then being compared to Vedic *mántu-* ‘mindful’ (Watkins 2001:73, 110). The Doric form is *mōsa* (and *mōa*) which is connected to a hypothetical verb **maō* (and *maomai*) ‘seek’ and this connected to PIE **men* and Vedic √*man* ‘thinking etc’. (H Frisk connects *manthanō* ‘learn’ and *menthērē* ‘care’ with it all.) All this is possible but cannot be verified; since Gk has also *mainomai* ‘rage’, *mania* ‘(inspired) frenzy’, *mantis* ‘seer’ etc, all of which are obviously cognate with V √*man*, the muse-example (with the loss of ‘*n*’ and the appearance of the labial vowel ‘*ou*’) seems most uncertain and may well have quite a different derivation. Let us try an experiment with this backward trail. *Mousa* has the form of the feminine participle, present, active ‘she who inspires, cares’ etc. Let us take the parallel form *ousa* ‘she-who-is’ (from verb *eimi* ‘I am”): this would come from **onsa*, from P-Gk **ontwa* from PIE *on-tu-h2*. This now should, if our law operates, have a Vedic correspondence *antu-*, but there is no such form. We find stems *anta* ‘end’,

antar 'within' and *anti* 'in front of' but no *antu*. The equivalent fem participle in Vedic is *satī* (from root *as* > *asmi* 'I am' giving masc *sa(n)t*): this shows no close phonetic correspondence either. In fact, if we go back to *mousa* with its normal fem participial form, the Vedic equivalent should be not *mantu-* (which is an epithet) but *manyamānā* or *manvānā* 'she who thinks, cares' (< *man*, *manyate*/ *manute*): both forms show no phonetic correspondence with Gk *mousa*, PGk **montwa* or PIE **montuh2*. One might try *dousa* 'binding' – (in LSJ under *deō A*) and end up with PGk **dontwa*, PIE **dontuh2* and Vedic *dyantī* which is not attested but quite normal. Thus we have a very thoroughly chaotic situation, but linguists don't mind fishing in these muddy waters, where murkiness and confusion are the only regular laws. The simple fact is we don't know anything beyond the forms Gk *mousa*, *ousa*, *dousa* etc and Vedic *mantu-, satī*, *dyantī*, etc: everything else is idle speculation.

In the various IE languages what we **actually** have is cognation based on phonetic and semantic proximity, nothing more. To pretend, as many do, that in this field there are operating strict phonetic laws is another instance of dishonest (and pretentious) sholarship. I give some examples where strict phonology is irrelevant.

(a) Sanskrit / Greek cognations: *śatam/he-katon* and Latin *centum* 'hundred'; *aśva/h-ippos* and L *equus* 'horse'; *caturtha/tetratos* (Homer) and L *quārtus* 'fourth' but *cakra/kuklos* (and L *circus*?) and *cinoti, cayate/ poieō, poinē* and L *poena* 'gather, make' etc and 'punishment'. Some reduplicating verbs: *dadāmi/didōmi* 'give' but *dadhāmi/tithēmi* 'put'; then *piparmi/pimplēmi* 'fill' but *juhomi/cheō* 'sacrifice, pour'. We accept all these cognations not because of strict phonological laws but only because there is *suficient* (a subjective degree) phonological and semantic correspondence.

(b) Without documentation no philological laws could ever take us back from many Mod Gk words to the ancient forms: we have *tuptō* < *tuptō* 'hit'but *rhichnō* < *rhiptō* 'throw' and *skubō* (pronounced *skivo*) < *kuptō* 'bow'; *denō* < *deō* (*didēmi*, only epic) 'bind' (cf Sanskrit *dyāmi*) but *chunō* < *cheō* 'pour' and *lunō* < *luō* 'release' and *ntunō* < *enduō* 'dress'; *teicho-* < *teicho-* 'wall' but *gleiphō* < *leichō* 'lick' (S *lih*/*rih*).

(c) For Latin and Romance languages, it should be sufficient to quote E Pulgram: "since all Romanic languages name a certain animal *cheval*, *caballo, cal*, etc and have words for 'war' like *guerre*, *guerra*, the Latins called the horse *caballum* and the war *guerram*" (p 147) whereas of course in Latin 'horse' is *equus* and 'war' *bellum* (poetic *duellum*).

In (b) and (c) we are dealing with changes within a single language, involving no dispersal – except for the Romance languages, though the examples of 'horse' and 'war' are found also in Italy; in addition these changes occur within a few centuries and, more important, in times when literacy was widespread. It seems to me much too presumptuous and arrogant to assume, as many linguists do, that from the remnants of IE languages, when for the most part there was no literacy and the periods involved are perhaps millennia, we can reach back and reconstruct any proto-languages and especially PIE.

d) Let us look at a different aspect: innovations or archaic features? It is assumed by many that the feminine gender in S, Gk, Italic etc, is an innovation whereas its absence in Hittite etc is original. It is also thought that the isogloss '*r*' indicating passive verbs in Italic, Celtic Hittite, and Tocharian (but not S, Gk, Germanic, etc), is also considered an archaism, lost in those IE branches that do not have it. It is further thought that the voiceless aspirates *kh*, *th*, *ph* are shared by Greek and Italic alone (S retains these as well as the voiced aspirates *gh*, *dh*, *bh*), because the indigenous people in Greece and Italy were unable to pronounce the original voiced aspirates (*gh*, *dh*, *bh*) and so the sounds became voiceless. Al three examples are pure conjectures without any basis in fact. We don't know, for instance, what exactly the Proto-Greeks brought with them nor what the indigenous people could or could not pronounce. The isogloss '*r*' could be an innovation while the feminine gender could just as easily be an archaic feature.

Comparative Linguistics has some regularity in some areas and has produced some good results but it is not, and cannot be, a science since there are no regular laws and reproducible results: it is a game in which star-players are good guessing and, as was said, approximate phonetic and semantic correspondences. I doubt whether scholars would indulge so extensively in this game if they had to pay out of their own pocket instead of spending other people's money.

4. The non-invasionists are called "revisionists" (Witzel, 2000), but it is forgotten with characteristic insouciance that before 1850 the IE urheimat was thought to be N-W India, and this on linguistic grounds that were later revised; then followed Lithuania and N Europe, Midle Europe and so on. The story is narrated by J P Mallory (1973). In all cases the evidence was flimsy or non-existent – one confident linguistic conjecture being superceded as soon as a new one was concocted. The science of Archaeology was then in its gestation period. The term 'revisionist' was also much loved by the diehard communists who stuck it pejoratively on all who tried to improve the system.

5. One of my papers was recentlly (late May 2001) rejected by a (Western) academic Journal – as I expected since I emphasized the *RV* date 3100. The referee's relevant note reads: 'The date of the *RV* is ludicrous: 3100 BCE (Kazanas) _ _ _ before horses in India, and when the horse-drawn chariot was not even invented??? See criticism of all such data in Witzel 2001 http ... ejvs". (See §**5**.) He/she is quite aghast and comes again: "The date of *RV* is outrageous. Kazanas, as many of the present "indigenists", simply has not studied the matter in context. The *RV* at 3100 BCE would be long before any chariots (which develop at c 2000 BCE) and before any domesticated horses in India (1700 BCE)".

As Zuckerman wrote, "The voice of higher authority had spoken" – in this case Prof Witzel. Now why should our "indologist" referee bother to look into the *RV* (or the *Vedic Index*) and see for himself what the text *actually* says? It is so much easier to run sheepishly behind the high authority and repeat its pronouncements appearing very knowledgeable about it all. The referee excoriates me also for writing that Bhaga is the 'Provider/Dispenser' whereas, according to him/her, the word means 'share, portion'. Obviously he/she has not read much *RV* nor (attentively) secondary literature like Mayrhofer. I am also criticized for saying that the *RV* alone of all early IE texts asserts a Unity (*tad ekam* in X 129, etc) as the Primal and Supreme Cause of all: the referee informs us that Ahura Mazda is the same Unity with the opposed forces added(!): here he/she has not noted that the high authority (ie Witzel 2001: §4) says succintly that Zarathustra establishes a "*dualistic religion of a fight between the forces of good and evil*". It is so safe to hide in anonymity. (The paper, incidentally, has been accepted elsewhere.)

6. Recently a German classisist (who for reasons of his own wishes anonymity) in a Conference organised by the American School of Archaelogy in Athens told me that, despite long study, he began to understand some Greek texts only after meditating for some years using a Buddhist method of meditation. (Yes, I too practise meditation for many years – and I am a devout Greek Orthodox with no enmity towards non-orthodoxy.)

7. Apart from "revisionists", the indigenists (all, without distinction and with Choudhury as the only exception) are said to copy from one another "in cottage industry fashion" (Witzel 2001: §11, n 74; §12·4). I do not know if this is true as I do not follow the Internet, but if the phrase "cottage industry" is meant to indicate contempt (in contrast to the more serious, organised industry of adacemia), it has an unfortunate aspect. Cottage industries flourished in England until the Agrarian and Industrial revolutions, in the second half of the 18th century CE. The destitution of the peasants caused by the Land-enclosures and the misery of the workers in the conditions of the new large factories are well known. To quote Trevelyan: "On the whole, Britain flourished greatly in the Eighteenth Century, and her civilization struck roots both deep and wide (…) The Industrial Revolution …new forces of machinery and capitalized industry worked their blind will upon a loosely organized, aristocratic society". And so "the quiet and self-contented England … slid unawares into a seething cauldron of trouble" (1972: 379-80). Biologically tampered foodstuffs and increasing atmospheric pollution (perhaps not reversible) are two modern gifts of the post-cottage-industry system of production. I am sure Prof Witzel had no such implications in mind when using the term "cottage industry". What I find alarming in *some* of these writers is their arrogance; but then chauvinists display only an intensification of a very common trait (or traits) and are found in all countries.

8. Other linguistic theories,like the substratal (Dravidian and Austric) entries into Vedic/Sanskrit, the isoglosses, the Anatolian antiquity etc etc, can be interpreted in different reasonable ways. Note 2, earlier, shows how uncertain this region is. One usual claim is that there are no Dravidian and Austric stems in *other* IE branches, only in Vedic (Witzel 2001). Hock, again, citing B Tikannen, tells us (1996: 103) that "vocabulary evidence favours Indo-Aryan contact with neither Dravidian nor Munda but possibly with some unknown northwestern language". But even if linguists can't agree about such basic data (it is the same vocabulary they examine, surely), nonetheless we can envisage the Dravidian/Munda influences coming from the South-west, affecting the Vedic speakers in the middle and not influencing the other IE peoples further (North-) West.

9. Prof Witzel has rendered Indology an important service, undoubtedly, by his critical examination of the non-invasionist literature. He has thus acknowledged the existence of a protest group of a view that is different to that of maintream sanskritists, one that has existed for a long time but has now spread more widely and has become more vociferous. We shall now have to wait for new finds in the field of Archaeology. Refinements in linguistic theories add

lttle to the old arguments. Below I examine briefly some of the many minor questionable points in 'Autochthonous Aryans?'.
a) §8: "the Indo Aryans, as described in the *RV*, represent something definitely *new* in the subcontinent." This is precisely the matter under discussion. Only the *RV* shows complete ignorance of the IVC. The post-rigvedic texts, particularly the Brāhmaṇas, reflect many elements from the IVC. If the *RV* is taken to have been composed before the IVC, there is no such problem.

b) Mention of Lord Renfrew (§8, p12, and n29) has inaccuracy. His Hypothesis A (1989: 178-97) finds nothing to demonstrate that "the Vedic-speaking population were intrusive to the area", ie Saptasindhu (182); nothing that implies invasion" (188); "it is difficult to see what is particularly non-Aryan about the Indus Valley civilization which on this hypothesis would be speaking the Indo-European ancestor of Vedic Sanskrit" (190); 'the continuity is seen to follow unbroken" (196). His difficulty is that all this does not fit with his general theory of farming dispersal from Anatolia, so he abandons this Hypothesis and concocts a Hypothesis B of waves of mounted nomads invading Saptasindhu and establishing themselves there with élite dominance (197-205) – and this he adopts in all his subsequent publications, despite the fact that "the balance of the evidence … is in favour of the presence of an Indo-European population" in the IVC (209).

c) §9:"nowhere in the Vedas do we hear of a *westward* movement". This must be so, since those who left (Out of India) did not remain there to write about it. But *RV* VII, 18 and VII, 6 say how Indra scattered the opponents of Sudās over the earth (this includes the West) and how Agni chased the Dasyus out to the West (Kazanas 1999: 28). See also Talageri's analysis of the facts in *RV* (2000: ch 4) showing a movement westward; I am not fully convinced by this, but it seems reasonable.

d) §11·4: "The autochthonous theory … maintains that there has not been *any* inflex at all, of Indo-Aryan or of other people from outside, conveniently forgetting that most humans have emigrated out of Africa only 50000 year ago". Dr Elst does leave open the possiblility of earlier immigrations (1999) and so do others. What non-invasionists reject is an immigration within the period, say, 5000-600 BC.

e) §19 (p 57, n 170): No "Kings" in the Gangetic plains of the 7th millennium "when this area was populated by a few hunter and gatherer tribes". What is so remarkable about a tribe having a "monarch" in 6000+ or 60000+? If there is a group of men someone must be "first among equals", and if his leadership proves good he is bound to pass into history/legend.

f) "The core 'books' of the *RV* (2-7) are arranged from short books to long ones". Presumably here is meant length in total number of verses. In his 1995b article Prof Witzel had said "number of hymns" (p 309); S Talageri showed this to be wrong (2000: 442) and that the 6 Family Bks increase in number of verses (ibid, 74)..

g) In §28 is discussed Prof Kak's "astronomical code". Some of Prof Witzel's strictures are correct here, but some of the details and the general scheme of Kak's thesis seem to be just as correct. In their 500-page *Hamlet's Mill*, Santillana and Dechend examine many examples of symbolic numeration (connected with astronomy) from all over the world. If Kak's work had been available in the 1960s I am sure it would have found a place in that study(1969). Kak's more serious offence is his emphasis (in other writings) on Indo-Greek contacts in pre-Hellenistic times and before 1000 BC (see Kazanas 2001b, §VII, n11).

h) On p 58, n 177, archaeologists buried the remains of 5 different linguistic communities and after some years they dug them up and found "the same (material) culture". First, it would be nice to know more details about this undocumented report. Second, we must take it then that archaeologists are near-imbeciles to think they can map out the "history" of prehistoric periods without written records and linguistic evidence. I doubt Prof Witzel really believes this.

i) In his Summary, Prof Witzel expresses some anxiety in case the established model in Indology and other Studies gets upset. This is understandable, of course. But I am sure he agrees that the establishment of the true situation is far more important than the inconvenience resulting from this.

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