

A Reply to Michael Witzel's 'Ein Fremdling im Rgveda'¹ (Journal of Indo-European Studies, Vol. 31, No.1-2: pp.107-185, 2003)

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Background:

Recently, N. Kazanas published a sixty page article titled 'Indigenous Indo-Aryans and the Rigveda' in *The Journal of Indo-European Studies* (JIES), vol. 30, Numbers 3&4 (2002), pages 275-334. The article argues that the speakers of Indo-Aryan (IA) languages did not enter the Indian subcontinent around 1500 BC, as the conventional Aryan Invasion Theory (AIT) and its euphemistic versions hold. Kazanas further argued that the Rgveda, the oldest IA and Indian text, was composed mainly in the 4th millennium BC, and therefore, these peoples may have arrived into the Indian subcontinent around or before 4500 BC, not later. The article provoked nine comments, of which eight² were quite short and were published in the same issue of the journal.

The ninth comment, by Professor M. Witzel of the Harvard University, was published in the next issue (*JIES* vol. 31, No. 1-2 (2003), pages 107-185. Witzel's comment is thus much longer than Kazanas' article itself. In the latter issue of JIES, Kazanas wrote a 54 page 'Final Reply' (*JIES*, vol. 31, No.1-2: pp. 187-240, 2003) to all his nine reviewers. He has responded to most of Witzel's important academic objections. My own critique here is meant to supplement Kazanas' response to Witzel. I will not repeat what Kazanas has already included in his final reply.

In my critique below, the page numbers within (...) refer to those in Witzel 'comment' as published in the journal, unless a different bibliographic reference is given by me. Witzel's actual statements from this 'comment' are in **brown** font, everything else is in black colored text.

Irrelevant examples in support of the Aryanization Hypothesis:

Witzel argues (pp. 108-109) that there is nothing extra-ordinary in the thesis that migrating IA speakers could have imparted their language and culture to the non IA indigenous population of the Indian subcontinent via acculturation. He gives the examples of pastoral Gurjara into India and of Brahui speakers from Central India into Baluchistan (p. 108, fn. 4) and then states (pp. 108-109) that

'frequently in such cases, immigration has been followed by acculturation of key parts of the pre-existing population'.

¹ The present article is a truncated version of an earlier article, which is available in its complete form online at <http://vishalagarwal.bharatvani.org/JIES2003-MW.html> An MS Word version of the original may also be downloaded from <http://vishalagarwal.bharatvani.org/ReplytoWitzelJIES.doc>

² These eight comments were by Richard Meadow, Martin Huld, Edwin Bryant, D. P. Agrawal, Asko Parpola, Stefan Zimmer, J. P. Mallory, Elena Kuz'mina.

a) Gurjaras -

However, the two examples he gives contradict his claim of acculturation of pre-existing Indian populations. The Gurjaras were all absorbed into the local population in India (whether in Gujarat, Rajasthan, Panjab or in other parts of India) to the extent that though they maintain their 'caste' identity at places, they speak local dialects, practice local religious beliefs and wear the attire of other locals of their respective regions³.

b) Brahuīs -

The Brahuī speakers number a mere 400000, and are found only in parts of Baluchistan and southern Afghanistan. The Brahuīs have got Islamized, and their customs are virtually indistinguishable from other tribes in the region. The 'Dravidian' content of their language is just a few hundred words. Moreover, the Brahuī areas are some of the most sparsely populated regions in South Asia, and have undergone language changes several times in historical times. Contrast this with Witzel's claim that a few IA speakers from Central Asia trickled into the Indian subcontinent and managed to Aryanize almost the entire population of an area of 3 million sq. km. without themselves getting absorbed into the native population.

Many other irrelevant examples are given by Witzel, but the above discussion should be sufficient.

Kazanas' Preservation principle and Polynesians:

In his article, Kazanas had argued that Vedic literature has preserved the maximum linguistic and cultural elements of all IE cultures, which would have not been possible if the Vedic peoples were always on the move. Witzel counters this argument (page 134) by pointing towards the example of Polynesian peoples who have preserved their oral lore despite being on the move for several millennia. However, there is a crucial difference between them, and the IA speakers, as he notes himself. The Polynesians moved into hitherto uninhabited areas, whereas the IA speakers moved into areas that were already inhabited. The Polynesians could not have come under the influence of any 'indigenous' inhabitants, they did not necessary have to 'invade'. So, the two scenarios do not parallel each other at all. Moreover, the example of Polynesians is somewhat anomalous, and it is not a norm for all migrating peoples. Examples that are *exceptional* merely open the possibility that such a thing could have happened in India at 1500 BCE, but the probability of that actually having happened remains low.⁴

'There are no Invasions, only Migrations and Acculturations'-

³ Even though they gave the name 'Gujarat' to a state in India, to a city in western Punjab etc., and even though they ruled large parts of Western India along with the Pratihara dynasty. The Gurjaras speak Hindi in Uttar Pradesh, Gujarati in Gujarat, Punjabi in Punjab and so on. The Gurjaras in India are largely Hindus (except in Kashmir) and those in Pakistan are largely Muslim.

⁴ Another crucial difference of course is that the Polynesians have retained memories of their voyages, but Indo-Aryan speakers have not.

Witzel alleges (page 116, fn. 19) that Kazanas has misinterpreted him in pointing out the confused nature of his elite dominance model in his 1995 papers. Any reader can verify that “elite dominance”, which is a subset of invasionist models, forms a necessary precursor to this ‘acculturation’. Witzel repeats the importance of ‘elite dominance’ in another later publication [WITZEL *et al* 1997:xxii, note 54], illustrating it with the example of the Norman invasion of England in 1066 AD and the ‘arrival’ (in reality, *invasions*) of Sakas, Hunas and Kushanas into N. W. India:-

“The immigrating group(s) may have been relatively small one(s), such as Normans who came to England in 1066 and who nearly turned England into French speaking country- while they originally had been Scandinavians, speaking N. Germanic. This may supply a model for the Indo-Aryan immigration as well.....However, the introduction of the horse and especially of the horse-drawn chariot was a powerful weapon in the hands of the Indo-Aryans. It must have helped to secure military and political dominance even if some of the local elite were indeed quick to introduce the new cattle-based economy and the weapon, the horse drawn chariot, - just as the Near Eastern peoples did on a much larger and planned scale. If they had resided and intermarried with the local population of the northern borderlands of Iran (the so called Bactro-Margiana Archaeological complex) for some centuries, the immigrating Indo-Aryan clans and tribes may originally have looked like Bactrians, Afghanis or Kashmiris, and must have been racially submerged quickly in the population of the Punjab, just like later immigrants whose staging area was in Bactria as well: the Saka, Kusana, Huns, etc.....”

Elsewhere, Witzel [1995:114] elaborates on the role played by the chariot (‘Vedic tank’) and the horse in enabling the Aryans secure elite domination over the descendants of Harappans:

“The first appearance of thundering chariots must have stricken the local population with a terror, similar to that experienced by the Aztecs and Incas upon the arrival of the iron-clad, horse riding Spaniards.”

He elaborates further [1995: 114, n. 74]

“Something of this fear of the horse and of the thundering chariot, the "tank" of the 2nd millennium B.C. is transparent in the famous horse 'Dadhikra' of the Puru king Trasadasya ("Tremble enemy" in RV 4.38.8)The first appearance of thundering chariots must have stricken the local population with terror similar to that experienced by the Aztecs and the Incas upon the arrival of the iron-clad, horse riding Spaniards.”

These are clear-cut invasion scenarios, which Witzel now wants to deny, and obfuscate with ‘acculturation’.⁵ By his methodology, any invasion can be converted into ‘acculturation’ and ‘migration’.

⁵ Since Witzel who brings in the conquistadors for explaining the Aryanization of northern India, one can therefore hardly blame his pet-hate LEACH [1990] for saying that –“ Common sense might suggest that here was a striking example of a refutable hypothesis that had in fact been refuted. 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. Almost without exception the scholars in question managed to persuade themselves that despite appearances, the theories of the philologists and the hard evidence could be made to fit together. The trick was to think of the horse-riding Aryans as conquerors of the cities of the Indus civilization in the same way that the Spanish conquistadors were conquerors of the cities of Mexico and Peru or the Israelites of the Exodus were conquerors of Jericho.”

It is only in his recent writings, such as WITZEL [2000a:291], where he has practically abandoned the thoroughly invasionist ‘elite dominance’ scenarios, fantasizing an Ehret elite kit model to explain the Aryanization of Northern India instead.⁶

Witzel criticizes (page 117) Kazanas for branding Erdosy as an invasionist. Anyone can however read his introduction to ERDOSY [1995] to see clearly that he initially starts with numerous promising statements, but soon takes a somersault and relapses into the old ways⁷. That Erdosy may have written different things in other publications is another matter. The fact remains that the publication that Kazanas had in mind does give the impression that Erdosy is an invasionist.

Is AIT dead?

Witzel claims (pages 119-120) that ““invasionist” views were first challenged by Vedic philologists such as Kuiper (1955 sqq.)....”
This claim is specious, because Kuiper⁸ was still writing on Aryan invasions twelve years later [1967: 81] –

" A German scholar of a former generation once remarked that there can be no more important task for the Sanskrit philologist than to describe changes that have taken place, in the course of the ages, in the mentality of the inhabitants of India. What he referred to was the slow but steady cultural process of Indianization of those Aryan tribes who had once, in a prehistoric period, invaded India from Iran."

Clearly, invasions are a precursor to acculturations in Kuiper’s model. In fact, one of Kazanas critics, Stefan Zimmer himself subscribed to the Aryan Invasion theory very recently. He writes [ZIMMER 1991: 328] –

“In India, all possibly non-Aryan mythical and religious material most probably stem from contacts of the invading Indo-Aryans with local populations. These contacts cannot be dated earlier than c. 1500 BC, and have therefore nothing to do with the period discussed here. It should be mentioned here that the Indra-Vrtra myth has earlier been interpreted as a reflex of historical combats rather as a cosmogonical myth comparable to the separation of earth and water in other mythologies.”

⁶ For a summary of various views proposed by Witzel in recent years, see section II.H in AGARWAL [2001a], available online at http://vishalagarwal.bharatvani.org/what_is_AMT_2.html The entire article is accessible at http://vishalagarwal.bharatvani.org/What_is_AMT.html

⁷ In fact, this flip-flop by Erdosy within a span of a few pages is so obvious that even an anonymous reviewer in October 1998 says the following at <http://www.amazon.com> - “The book has some excellent articles by the archaeologists but, on the other hand, it has a rehash of the failed philological theories regarding the Indian linguistic area. Overall a very uneven package where the editor raises some good questions in the beginning but soon after lapses back to old ways of thinking.” See the URL http://www.amazon.com/exec/obidos/tg/detail/-/3110144476/qid=1046992911/sr=1-2/ref=sr_1_2/002-4123873-5784068?v=glance&s=books It is strange that an intrepid internet researcher like Witzel missed this out.

⁸ F. B. J. Kuiper is one of Witzel’s teachers.

Numerous Vedic philologists still subscribe to the most racist and rabid versions of the Aryan invasion theory, contrary to Witzel's claims. For instance, ELIZARENKOVA [1995:41] says⁹ –

“The role of forests in the RV might also have bearings on the studies of the pre-history of the Aryan tribes that invaded India. ”

The fact that AIT is fairly mainstream in academic circles can be concluded from the fact that it is included as axiomatic truth in influential texts on Indian history (e.g. WOLPERT 2000: 24 pp.), works on Indian Philosophy [e.g., REAT 1996: 4-8], socio-cultural studies [e.g. DONIGER [1992], decipherments of Harappan script [AALTO 1984] and so on. The wide-prevalence of AIT in academic circles is precisely due to the fact that all the so-called migration and acculturation models proposed by Vedic philologists are but euphemistic versions of AIT. Witzel's own models are but a version of the AIT, as shown above.

Vedic Ratha = Witzel's 'Vedic Tank' –

Witzel emphasizes that the 'real' Rgvedic chariot necessarily has (two) spoked wheels, is a light 'Vedic tank', i.e., a war machine and is always pulled by horses (page 109). Archaeological evidence from other parts of the world however shows us otherwise. A seal impression from the Late Minoan period in Crete shows a 'real' spoke wheeled chariot being pulled by a pair of goats [ZEUNER 1963:144]. In ancient Mesopotamia, bovids were used for pulling 'real' chariots as early as the Late Uruk period [ZARINS 1976:225]. Chariots in Mesopotamia were also pulled by mules [ZARINS 1976:457-461]. It is not really necessary that equid pulled chariots should always have spoked wheels. The copper models of equid drawn chariot unearthed from Diyala [ZARINS 1976:579] show solid wheels, as does the limestone plaque [*op.cit.*, p. 583] from the same site. It is not that Witzel is unaware that other animals are also said to pull chariots in the Rgveda. However, Witzel's insistence that the horse pulled chariot in the Rgveda must have spoked-wheels is not attested by the text itself.

To drive home the idea that the Vedic chariot was a real, light, spoke wheeled war machine that seated two people and had two wheels, Witzel brings together an assortment of vocabulary related to the *ratha* from the Rgveda and other late texts such as the Kathaka Samhita, the Jaiminiya Brahmana and the Baudhayana Grhyasutra (pages 157-162). I will ignore the late Vedic texts, focusing on the Rgveda. The occurrence of these terms does not prove the existence of Witzel's Vedic tank throughout the chronological period associated with the Rgveda. Talageri¹⁰ explains, for instance –

⁹ Apparently, Elizarenkova is the principal Vedic authority and source for Elena Kuzmina (both are Russians) and therefore one is hardly surprised that the latter should still subscribe to AIT-like paradigms in interpreting archaeological record.

¹⁰ Email sent in July 2001 to Steve Farmer and a few others. This email was sent to Michael Witzel too, but he never replied.

('ara', and perhaps 'shanku') in the Rigveda are found *only* in the Mandalas and upamandalas of the Late Period:

I. 32.15; 141.9; 164.48;

V. 13.6; 58.5;

VIII. 20.14; 77.3;

X. 78.4

Steve Farmer finds it necessary to infer the presence of spoked wheels on the basis of words *other* than the actual words for spokes: “numerous references in RV..... to parts *only* existing on or in conjunction with spoked chariot wheels: the metal tire/rim (pavi) nemi (felloe or possibly wheel/felloe combination) felloe/felly again (pradhi, vartani), chariot carriers ... and other parts linked to spoked chariots... a *mass* of evidence – ‘hundreds’ of references – not a couple of random passages that you can wish away”, All these “hundreds of references”, however, refer to wheel parts which *in later times* were associated with spoked wheels because later wheels were spoked wheels. Inferring backwards from this that these words (nemi, pavi, pradhi, etc.) presuppose spokes runs in the face of the solid fact that spokes are emphatically *not* mentioned in the Early and Middle Mandalas, and equally emphatically *are* mentioned in the Late Mandalas (a conspiracy on the part of the composers?). On such grounds, even the bare word for “wheel” should necessarily presuppose the existence of spokes.

Let us examine the specific words cited by Farmer:

a) “vartani” almost everywhere means “pathway” or “track” and not “felly” as alleged by Farmer.

b) “pavi” simply means the rim or edge of a wheel (spoked or otherwise). In fact, of the three references in the Early Period, in the two in the oldest Mandala (VI. 8.5; 54.3), the word refers to the sharp edge of the weapons of the Gods (Indra’s thunderbolt and Pushan’s discus respectively). The third (VII.69.1) does refer to the bright rims of the wheels of the Ashwin’s chariot, but “spokes” are not even implied. The word does not occur in the Middle Mandalas IV and II; and in the other Mandalas (I, V, IX, X) it occurs 13 times; but even here once it means the sharpened point of an arrow (IX. 50.1), and once, again, the sharp edge of Indra’s bolt (X. 180.2),

c) “nemi” appears to mean “felly” in the Late Period, since 3 (I.32.15; 141.9, and V.13.6) of the 9 references here use the word in conjunction with spokes. But even here it once (VII. 34.3) refers to the rim of the stone which is used to crush Soma (so, surely, spokes are not inbuilt in the meaning of the word). The word occurs once in the last Mandala of the Early Period (VII.32.20) and once in the Middle Period (II.5.3); and in both it definitely represents the outer part of an elaborate wheel, but spokes are not mentioned: in fact, the first reference is specifically to a *wheel of solid wood* (which hardly indicates spokes).

d) “pradhi” first occurs in the Middle Period (IV. 30.15; II.39.4) and while it certainly represents a part of the wheel, it does not automatically imply spokes (except when one reasons backwards from the references to spokes in the Late Period).

Commenting on even a late text such as the Brhadaranyaka Upanishad I.5.15, OLIVELLE [1998: 415] shows how the word ‘*pradhi*’ most likely means a part of a solid wheel –

““Wheel Plate: The meaning of Pradhi here is uncertain. It is generally translated as "rim, felly" and the grammatical subject of the final saying "pradhinagat" ("He went with the rim") is generally taken to be a man who has lost his wealth. To make any sense of this, one has to add (following the commentator Shankara, but I believe unjustifiably) the word "lost" and translate the saying as "He has come off with the loss of a felly!" (Hume 1931). I think the subject of the saying is not the man who lost the wealth but the robber who plundered it. Thus, if the robbed man is still alive, his friends might breathe a sigh of relief and say: "Thank God! The robber got away with just the wheel plate [i.e., the

external things that can be replaced], but at least you [i.e., the hub] are all right." This is probably the meaning of the pithy saying. The term Pradhi, moreover, probably, means not the rim but the section from the hub (wheel head) to the rim of a wheel, that is, the wheel plate, and the wheel in question was probably solid rather than made with spokes. The solid wheel plate, moreover, was made of several sections, and it is possible that pradhi refers to these sections, especially to the half moon shaped sections at each end (see examples Sparreboom 1985). Another interpretation is offered by Joel Brereton (personal communication). If the wheel consisted of many pieces called pradhi, then the meaning could be that the person robbed escaped with just one such piece. This makes sense within the context of a race; even if the entire chariot is destroyed, a man may win the race if he just attaches a piece of the wheel to the horse or bull and crosses the finish line (see, for example, the story of Mudgala in RV 10.102). The expression then would be like our "on a wing and a prayer.""

In summary then, the mere occurrence of words such as *pavi*, *nabhi*, *ani*, *cakra* etc., do not automatically imply a spoked wheel war machine because all these terms are common to solid wheel carts and chariots as well. The specific words for spokes, and other parts of the classical chariot appear only in late books of Rgveda, in the middle books and in the latest Family book, i.e., Mandala 5 (per Talageri's scheme, which was devised in ignorance of the chariot argument, and therefore quite innocent of it).

Witzel says (page 158) that the word for chariot in Sanskrit is derived from older IE word for "wheel" and gives some cognates. This would rather prove my point that the Vedic *ratha* originally meant simply a vehicle, or a cart or just a wagon. Why would someone name their innovation, the chariot, after the word for 'wheel'?

What Witzel does not point out is the fact that there is no archaeological evidence of the existence of chariots in the period when the IA speakers are supposed to have arrived in north-west Indian subcontinent. Absolutely no remains of chariots (or any depictions) are found in the Saptasaindhava region from post 1900 BC right down to Mauryan times (~250 BCE).¹¹ In any case, chariots came to many cultures such as ancient Egypt and ancient China from outside without any change of their language or culture. Such adaptation of technical inventions need not be confused with linguistic or other changes.

An Archaeological Hunt for Aryans:

Witzel argues (page 148) that the Cemetery H artifacts at Harappa indicate an intrusion of a new group people because one sees a change in the burial practices which no longer involve inhumation but reburial. He also cites POSSEHL [2002:170] to the effect that a different anthropology is also indicated. However, when I examined this book, I noticed that Possehl makes no such claims that Witzel attributes to him. Finally, Witzel states that the burial urns are adorned with pictures of bird souls, which are suggestive of later Vedic ideas. KENOYER [1998:174-175]

¹¹ Literary evidence exists of course, but considering that the invasionists are so keen to deny any horse bones in Harappan sites even when they are found, how come they are silent on this matter? If chariot-racing and their use in battles was common amongst Vedic Aryans, then how come we see no evidence in the archaeological record?

discusses Cemetery H burial artifacts in some detail, and does not see any sudden intrusion, but only a gradual transformation of culture, thus contradicting what Witzel seems to imply.

Witzel also claims that in the late Harappan period, some new ‘**extraneous elements**’ are seen in the northwest, but that these are evident throughout the northern part of the Indian subcontinent. As an example, he points to the spread of some Pirak/Baluchistan motives (sic!) into Uttar Pradesh (PGW pottery). However, the excavator himself, JARRIGE [1985] interprets the situation in a totally different manner –

“Another element of continuity between ceramics of the third millennium Baluchistan and those of the second millennium can be found in the decoration. While the geometric painted designs on pottery from Pirak may be quite different from those on Harappan pottery, they are very much in the older ‘Quetta-Amri’ tradition. In our report on Pirak we pointed out similarities which we feel are too close to be explained merely as a result of coincidence. We postulated that such traditional styles of decoration survived in regions which were at the periphery of the principal zone of Harappan influence...

...Should the origins for these transformations of the second millennium be sought in exogenous events, in colonization of the area by new peoples, by a sudden influx of refugees bringing new crops and animals with them? Probably not, since the processes which I have briefly described are too complex to be attributed to the arrival of invaders who at the same time would have had to have introduced rice from the Ganges, sorghum from the Arabian Gulf, and camels and horses from Central Asia. It is also not likely that the newcomers, whether they be a ruling elite or refugees, would have had the impetus to change an agricultural system still capable of being intensified without the introduction of new crops and, for rice, new irrigation practices.”

It is important to point out that this ‘evidence’ is however rejected as proof for ‘intrusive Aryan elements’ by archaeologists like CHAKRABARTI [1999:201] and Indo-Europeanists like MALLORY [1998:192] as well, although for mutually different reasons.

Huns as the Aryans of Europe -

To account for the absence of distinctly ‘Aryan’ elements in archaeological remains, Witzel argues (pages 150-151) that the Avars (Huns) themselves have not been attested archaeologically until recent times. Therefore, it is quite possible that an Aryan migration happened even if they are not attested in archaeological record. This analogy is false. The differences between Avars and Aryans may be summarized as follows –

1. There is no literary evidence to prove that Aryans migrated into India, whereas historical sources detailing the invasions of Avars are plentiful. Their invasions of Huns were carried by armies (accounts give numbers ranging from 300000 to 700000)
2. Avar settlers in Europe lost their language and culture practically everywhere except in Hungary, where the people speak a non-IE language. In contrast, the Aryan speakers are said to have Aryanized the language, culture and religion of entire populations over 3 million sq km.

3. They withdrew rapidly and mysteriously from Europe (attributed to the death of their king) from Europe and are known to have carried their dead back with them. They had a very poor material culture. Their invasions lasted a little over a 100 years. In contrast, the Aryan speakers are said to have just come in a one way traffic into India in several generations and via a slow process of acculturation. Parpola also sees at least two such waves covering several centuries.
4. The homeland of PIE, i.e., the ancestors of Aryans, is unknown, and the PIE people still remain a non-proven group. In contrast, the homeland of Avars has been located with a fair certainty in eastern Siberia. While the ancestry of proto- Huns is known to 200 BCE, the Proto IE speakers are still a hypothesized group whose existence is yet unproven, although speculated at several places.
5. The Avars launched massive invasions all over Eurasia, causing a lot of bloodshed and destruction, but the IA speakers are said to have arrived largely in a peaceful manner, and achieve what Avars could not.

In any case, archaeological remains of the Avars have been unearthed by now, but such remains are absent for migrating IA speakers. Witzel appears to argue that by the time the IA speakers arrived in the Indus valley, the area was practically deserted and that they dealt mainly with a 'remnant population'. Such a scenario would have lead to a significant change in the genetic make up of the depopulated areas, something which has not been demonstrated so far. Secondly, such new fangled ideas clearly indicate that linguists such as Witzel only pretend to take the archaeological data in their stride, but actually keep modifying their philological/linguistic theories to circumvent the new archaeological findings, instead of co-opting them. Just a few years ago, when the Harappan culture was understood to have died out by 1900 BCE, Witzel (see for instance his 1995 papers) proposed that RV should be placed sometime after that date. Subsequent excavations increasingly brought to light certain 'Aryan' features in Harappan sites, such as fire altars. This lead certain scholars such as Parpola suggest that the Aryans might actually have been present in the last phases of the Harappan culture. Now archaeologists inform us that the Harappan culture did not die around 1900 BCE, but lingered on for several centuries after 1900 BCE, and that the interior of the Indian subcontinent had several other contemporary or derivative chalcolithic cultures sharing some affinities and connections with the Harappan culture. These cultures (e.g. Kayatha, Jorwe) also show 'Aryan' characteristics such as fire altars and horse bones.

This raises great problems for invasionists. So now, the presence of Aryans in the late Harappan period (i.e., around the middle 2nd millennium BCE) must be denied or minimized, and the RV be postdated further. Therefore, attempts are being made now by Witzel et al to deny completely the existence of such 'Aryan' features (such as the horse, spoked wheels, fire altars etc.) at Harappan sites. Secondly, the entry of Indo-Aryan speakers into India is being post dated and it is being proposed that by the time they came to North West India, the local populations had been

famished culturally to such an extent that they could be dominated very easily by the intruders.

Archaeometallurgy and Vedic texts:

One of the arguments made by Kazanas to suggest that Vedic texts could date to 3000 BC or earlier is that the astronomical data in these texts indicates stellar positions from that period. In ancient times, it was almost impossible to back-calculate the positions of various constellations etc. over a period of 1000 years, and therefore, the astronomical data in these texts represents actual astronomical observations by the composers of the Vedic texts. Witzel counters this by arguing that Satapatha Brahmana belongs to a ‘full-blown Iron age’ (page 174), i.e., to a period slightly before 500 BC. This seems to be incorrect. Referring the *Vaidik Padanukramakosha* (Vedic Word Concordance) of Pandit Vishvabandhu, the following occurrences of words *syaamam*, *syaamaayas* etc., can be noted in the Satapatha Brahmana –

Satapatha Brahmana 5.1.3.7; 5.1.3.9; 5.2.5.8; 5.3.1.9; 5.4.1.2; 6.2.2.2; 13.2.2.6; 14.9.4.15

Let us examine the occurrences of these words in the Satapatha Brahmana –

5.1.3.7: Here, the word *syaama* does not refer to any metal. Rather, it refers to the color 17 victims for Prajapati, which have to have a color that is a combination of white and black, i.e., dark grey (Eggeling’s translation), or a mixture of black and white (as Sayana explains).

5.1.3.9: This passage actually explains that *syaama* is a combination of light color and black.

5.2.5.8: Here, *syaama* is the color of the bull, that is the fee for a ritual.

5.3.1.9: Here again, the word is used as an epithet for a bull.

5.4.1.2: This text states that ‘*lohaayasa*’ or red metal (=copper?) is neither gold nor *syaamam*. This text merely contrasts the red metal with a bright, and a dark metal. Again, no clear evidence that iron is meant. The contrast could very well have been with bronze and gold.

6.2.2.2: Here, the word *syaama* is an adjective for a goat meant for sacrifice to Prajapati. The text clearly says (Eggeling’s translation) – “It is a dark grey one; for the grey has two kinds of hair, the white and the black.....”

13.2.2.6: This, and other occurrences in the vicinity also deal with characteristics of sacrificial animals. Again, no connection with any metal.¹²

Assuming that Vishvabandhu missed 1 or 2 genuine occurrences of ‘black metal’ in his concordance, we still have at the most 3 occurrences (and just one in the locations pointed above by the Concordance) in this large text. Just three! And none compels us to accept the meaning of the word as ‘iron’. So Witzel’s claim that the Satapatha Brahmana is an iron-age text through and through is a pure bluff, and his entire argument for dismissing the archaeoastronomical evidence collapses.¹³

¹² I could not trace the last occurrence in the 14th book of Satapatha Brahmana.

¹³ The claim that the Satapatha Brahmana is an iron age text ‘through and through’ has been made by Witzel on various Internet lists also, but I will let it pass here.

Witzel alleges that Kazanas' interpretation of *syaamaayasa* as bronze or something different from iron is based on some discussions in Internet lists (page 175, fn. 112). Kazanas does not have to do so. The *Vedic Index* (Volume II, page 398) says that *syaamaayasa* in the Atharvaveda Samhita denotes iron 'in all probability', which clearly indicates that it was a conjecture made by the authors of the Index¹⁴. In a study on gold in Vedic texts, even Jan GONDA [1991] treats the equation '*syaamasa* = iron' with reservation, and in fact, suggests that the word could mean bronze. Finally, Witzel's pet-hate K. D. Sethna [1992: 235-236] has already discussed the question in detail and has argued that there is no compelling reason to believe that *syaamaayasa* has to mean iron. Kazanas is well aware of this book. Witzel's frequent appeal to the authority of Wilhelm RAU [1974] is of no avail – there is simply no evidence to prove the assumption firmly that *syaamaayasa* or *syaamam* denotes iron.

Witzel does not stop at this. He bluffs (pages 174-175, fn. 112) that iron that is occasionally found in India and surroundings before 1200/100 BCE is meteoric iron. In reality, there are no available chemical analysis results showing that this is indeed the case.¹⁵ In fact, POSSEHL [2002:93] notes very clearly that the iron artifacts predating 1000 BCE from various sites in South Asia have not been analyzed to ascertain whether it is meteoric iron or not. While Witzel derives all his knowledge of metallurgy from the works of Rau, he forgets to see the aforementioned reference, which mentions in the next page [POSSEHL 2002: 94] that iron can be produced as a by-product during the smelting of copper, and that this is, in all likelihood, the source of Harappan artifacts made from iron. What this means then, is that unless Witzel can show a very widespread use of iron from Samhitas and Brahmanas, none of these texts can be dated to the 'iron-age'. In any case, even if the Satapatha Brahmana mentions iron, the text has no information on whether it was meteoric or terrestrial, a fact that is accepted even by Edwin Bryant in his own comment to Kazanas' article in *JIES* 2002.

Witzel then counters Kazanas' high chronology for the events of Mahabharata by arguing (page 174-175; 176-177, fn. 115) that the text itself is very late because it even mentions the Yavanas, Parthians, Shakas and the cities of Rome and Antioch (which was occupied by Romans in the 1st century BC). The argument is spurious because Kazanas never denies that the Mahabharata is a stratified text, and the verses mentioning these peoples and these cities may well be late interpolations. They do not really form an integral part of the central story in any case.

¹⁴ The Saunakiya Samhita mentions 'dark' to denote a dark metal at two places -9.5.4; 11.3.7. In his translation, although Whitney glosses 'dark metal' as 'doubtlessly iron' for the latter occurrence, nothing compels us to accept this meaning. It could very well mean bronze (knife). He does not comment on the identity of the dark metal at 9.5.4. although the context again refers to a knife made out of the same. It may be noted that bronze and copper knives and blades have been found in the Harappan sites.

¹⁵ Meteoric iron has a higher nickel content. None of the standard works on Archaeometallurgy of ancient India, including recent ones by Vibha TRIPATHI [2001], and by D. P. AGRAWAL [2000], contain any such information which enables to decide if these ancient iron artifacts in a bronze age context are derived from meteoric iron or not.

As an illustration, let us consider the case of the mention of Antioch and Rome in the Mahabharata. Obviously, Witzel has the following half-verse in mind –

antaakhiim caiva romaam ca yavanaanaam puram tathaa |

The above words occur in the Sabha Parvan of Mahabharata, which was critically edited by for Bhandarkar Oriental Research Institute (Pune, India) by Franklin Edgerton, a predecessor of Witzel at Harvard University.

Now, scholars accept the fact that the critical texts of Mahabharata and Ramayana are not infallible. In fact, an examination of the critical apparatus – various readings, different recensions, parallel passages from other texts, etc., can yield a text different from the one fixed by the editor of the critical editions. In this particular case, SURYAVANSHI [1986:20-32] examines the issue in great depth by looking at the wording of the various recensions of Mahabharata, considering textual variants, and keeping in mind the geographical context of the adjacent passages and he concludes that Edgerton's choice was rather injudicious. He demonstrates that *antaakhiim* should rather read '*aaTaviim*' and '*romaam*' should be read as '*ramyaam*'.¹⁶ Thus, Witzel's argument evaporates.¹⁷

Muddying the holy waters of Sarasvati:

a) Indus-Sarasvati, or Indus Valley Civilization (IVC) -

In recent decades, archaeologists in India and Pakistan have discovered more than a 1000 sites along the dried up Ghaggar-Hakra plains. Literary data from the Vedas indicates that the Ghaggar – Hakra river system is indifferent from the Sarasvati river extolled so highly in the texts. Since the earliest sites of IVC were discovered along the Indus river and its tributaries, the civilization was named thus. However, the current situation is that fewer than 25% of the sites lie along the Indus river and its tributaries, more than 50% along the Ghaggar-Hakra system, and the rest are scattered in Gujarat, Ganga-Yamuna doab and other regions. Witzel laments (page 165) –

"There is a move by some Indian archaeologists and indigenists to call the Harappan civilization, against common archaeological convention,... the "Indus-Sarasvati Civilization."

Witzel then postulates a reason for this 'move' (page 165, fn. 89) –

"Note that nearly all sites of the Harappan Civilization, found early on, are situated in the Indus area. Hence the popular designation as "Indus Civilization". That means they are now in Pakistan; the Sarasvati area came to the rescue of

¹⁶ Unfortunately, BROCKINGTON [1998] does not look at this issue in detail.

¹⁷ I do not want to get into the controversy regarding identification of *yavanas* with Greeks. Even though the equation has become an Indological dogma, there is really no firm evidence to prove that the *yavanas* in the Mahabharata, Ashtadhyayi and the Gautama Dharmasutra are indeed Greeks. For a contra view, I refer the reader to SHRAVA [1981].

those who wanted to see the Harappan Civilization represented inside India, in the mythical heartland of Kurukshetra...”

The insinuation is that those who want to rename the IVC as Indus-Sarasvati Civilization are either Right Wing Hindu, indigenist, or Indian nationalists. However, archaeologist Jane McIntosh, who is neither of these, also recognizes the importance of Sarasvati sites in the entire IVC area [MCINTOSH 2002:24]-

“...Suddenly it became apparent that the “Indus” Civilization was a misnomer – although the Indus had played a major role in the development of the civilization, the “lost Sarasvati” River, judging by the density of settlement along its banks, had contributed an equal or greater part to its prosperity. Many people today refer to this early state as the “Indus-Sarasvati Civilization” and continuing references to the “Indus Civilization” should be seen as an abbreviation in which the “Sarasvati” is implied.”

She further adds [2002:28] –

“The now-dry Hakra River forms part of this river system. Surveys along its dry bed revealed that this was one of the most densely populated areas of the 3rd millennium, the agricultural heartland of the civilization, although it is now virtually desert.”

In short, there are purely academic reasons to suggest a change in nomenclature, and one should not see political ideologies or religious motivations in this ‘move’.

b) Afghani Sarasvati -

Witzel criticizes (page 164, fn. 87) Kazanas for allegedly misrepresenting his views –

“Kazanas criticizes Witzel for having “explain[ed] why the Sarasvati is not really the Sarasvati but some river....in Afghanistan....or Milky Way”..But, Witzel has even printed a map of the Sarasvati of Haryana/Panjab...”

In fact, it is Witzel who has misrepresented Kazanas’ intentions. Kazanas was merely referring to Witzel’s critique of TALAGERI [2000] in which the latter has demonstrated that all the rivers in Mandala 6, the oldest book of the Rgveda, are eastern rivers (Ganga, Yamuna, Sarasvati). WITZEL [2001: §7] however argued that there is no reason to believe that this Sarasvati in book 6 of Rgveda is in Punjab, and said that it could be a woman, or an Afghani river (modern Helmand) or even a river in the Milky Way. His actual words are-

“The River Sarasvati found in book 6 (T. p.102) may be discarded just like T.’s Gangetic Jahnvi ... in 6.49.7 the Sarasvati is a woman and in 50.12 a deity, not necessarily the river (Witzel 1984) (At 52.6, however, it is a river, and in 61.1-7 both a river and a deity – which can be located anywhere from the Arachosian Sarasvati to the Night time sky, with no clear localization)” (§7).

The reply of Talageri to Witzel is available online.¹⁸ Let me also point out that the Old Avesta does not mention the Harahvaiti, considered cognate to Sarasvati, at

¹⁸ See section III.1.b) at http://www.voi.org/general_inbox/talageri/ejvs/part3.html

all. It is only the later parts of Avesta that mention the river goddess for the first time. These parts of Avesta are chronologically very late in comparison with Rgveda.¹⁹

c) *The older name of Sarasvati -*

Witzel refers (page 164) to an older article of his [WITZEL 1999a, § 4.3,5] wherein he argues that the older name of Sarasvati was *Vaisambhalyaa*, mentioned in Taittiriya Brahmana (and Bharadvaja Siksa etc.). Witzel classifies this word as of Austro-Asiatic origin, and then suggests that this indicates that the Kuruksetra region was initially inhabited by speakers of para-Munda languages. These people were apparently displaced by IA speakers, who then Aryanized the name of the river to ‘Sarasvati’. The reader can easily check Taittiriya Brahmana 2.5.8.6 and verify that nothing of this sort is stated or implied therein. The word actually has a very transparent IA etymology as explained even in the *Jnanayajnaabhashya* of Bhatta Bhaskara. Sarasvati was so called because it nourished and sustained masses of people. This is a meaning which fits the ritual context of the sections very well. As for his reference on the occurrence of the word in Bharadvaja Siksha, the reader should note that the Siksa is a *late* text and is merely an index of words in the Taittiriya Brahmana. So its occurrence in the Siksa is of no independent utility.²⁰

d) *Vinasana*

Witzel (pages 164-165) states –

“The river [Sarasvati] has been mentioned frequently in the RV and subsequent texts and survives as a small river, the Sarsuti-Ghaggar-Hakra in Haryana, that quickly disappears in the desert, as the Brahmana texts already tell us.”

The statement is completely wrong. First, Sarsuti is a tributary of Ghaggar, and the relationship between Sarsuti and Ghaggar is not the same as that between Ghaggar and Hakra as Witzel seems to imply. Secondly, Hakra is not in Haryana. In fact, it is that stretch of the river system that traverses Bahawalpur a lot downstream from Haryana. And finally, the Brahmanas do not say that the river ‘quickly’ disappears in the desert! Rather, the Tandya Brahmana 25.10.16 says²¹ that the spot of disappearance of Sarasvati is more than a month’s journey from its origin on horseback! This means that in the time this passage of Tandya Brahmana was written, the length of the Sarasvati river was several hundred miles and the river did not ‘quickly’ disappear into sands in Haryana.²²

¹⁹ In fact, many Iranists like Mary Boyce argue that the Avestan cult of the river goddess Anahita (linked with Sarasvati-Harahvaiti by Indologists such as H. Lommel) is actually derived from Mesopotamian antecedents. A detailed discussion on this matter is beyond the scope of my critique.

²⁰ Witzel argues that the word occurs with variant spellings in Apastamba Srautasutra, Bharadvaja Siksa and Taittiriya Brahmana and that these spelling variations are ‘proof’ of the word’s foreign origin. The argument is curious and not sustainable.

²¹ The text says that the distance from *Plaksa Prasrvana* to *Vinasana* is ‘44 *asvinas*’, which, according to one calculation, could be 880 miles. Other interpretations of ‘*asvina*’ would still yield a length of several hundred miles for the river.

²² It may be noted that the advancement of sand-dunes towards Sirsa and Hissar districts of Haryana is a fairly recent phenomenon and happened just a few centuries ago. The Thar desert extended over a much smaller area in Harappan or in Vedic times.

In fact, we have literary evidence that shows that the Sarasvati was a perennial river several hundred miles long even to the times of *late* Srautasutras. The following passages from the late Latyayana Srautasutra may be cited²³ –

10.15.1 The consecration for the Sarasvata Sattrā is performed at the Southern shore of Vinasana

10.17.10 'They should not even once approach the Sarasvati river for the Avabhṛta rite. This is, indeed, their sacrificial ground.'

10.17.11 If no other water-place is available (for the Avabhṛta) they should collect water from the Sarasvati and create a water-place in its neighbourhood for the Avabhṛta rite.

10.18.3 'The Brahmana states- 'there are settlements called Naitandhavana near the river Sarasvati. One of them is known as Vyarna. 'One should kindle fire for one year at this place' implies that one should perform worship by means of the (Aupasana) single fire.'

10.17.1 'If the river Drsadvati is full of water, they should perform the Aponapriya Isti near its confluence (in the Sarasvati)

10.17.2 Dhananjaya maintains that it may be performed there, even if it (the Drsadvati) has no water.

An important point to note here is that the river Sarasvati is implied to be always full of water till Vinasana, which is placed *west* of its confluence²⁴ with Drshadvati. The Drshadvati appears to be a seasonal stream from the sutras cited above, but its connection with Yamuna in older times is clearly hinted in the subsequent sutras –

10.19.8 'He should move by the southern bank of the river Drsadvati.'

10.19.9 'Having reached the settlement at the origin of this river and having performed this Isti (to Agni), he should move to the region called Triplakshaharana on the Yamuna River for the Avabhṛta rite.'

10.19.10 'He may even perform this Isti at any point where the Yamuna is at a long distance, and then proceed to the Avabhṛta place either while chanting the (Avabhṛta) Saman by himself or not.

Archaeologists and geologists have equated Chautang²⁵, a seasonal stream in North India, with Drshadvati. But at one time, it was a perennial stream, till its waters transferred the present day Yamuna.

It must be emphasized here that nowhere does the Rgveda say or even hint that the Sarasvati river ends up in a desert at a place named Vinasana or Adarsana. Such a notion starts appearing only in Brahmana texts, and is absent in all the extant Samhitas of Vedas.

²³ I have used the recent translation by RANADE [1998]. Similar passages occur also in Asvalayana Srautasutra, Sankhayana Srautasutra etc.

²⁴ The confluence itself corresponds to the Harappan site of Kalibangan in Rajasthan. Clearly then, Vinasana was most probably in Ganganagar district of Rajasthan or in Bahawalpur area of Pakistan even at the time of the late Latyayana Srautasutra. This fact itself upsets the entire late chronology assigned to sutra texts by mainstream Indology.

²⁵ Variants of this name are Chitang, Chutang etc.

e) In the Aryan world, Ocean = Pond

Rgveda 7.95.2 says that the Sarasvati flows from the mountains to ‘*samudra*’. Witzel suggests (pages 168-171) that in this verse, *samudra* could merely mean the playa near Fort Derawar in Bahawalpur (Pakistan). Relying on some papers by the Konrad Klaus written between 1986-1989, Witzel says that oceanic imagery and realia are absent in the Rigveda, the text speaks only of small river boats, and there is no mention of tides therein. Witzel also claims that Klaus is the last person to study the manifold meanings of ‘*samudra*’.

All these assertions are totally false. First, Klaus is not the ‘last’ person to have studied the various meanings of ‘*samudra*’, and its various occurrences in the Rgveda. Recently, KAZANAS [2002a] and FRAWLEY [2002] have studied the matter in detail, in articles written and publicized well before Witzel wrote his comment for the *JIES*. In fact, these two articles were specifically written in response to Witzel/Klaus²⁶ claims that the Rgvedic Aryans had a very meager knowledge of the ocean. Witzel too is well aware of the existence of these articles, having been involved in a prolonged debate with Kazanas and Frawley on this issue in various Internet lists and in Indian newspaper *The Hindu*. The non-mention of these articles by Witzel, coupled with a parrot-like repetition of Klaus’ name for the readers of *JIES* has obvious reasons.²⁷ The two articles clearly demonstrate that the Rgvedic peoples were well aware of ocean, maritime trade and so on.

f) The Unfaithful Sisters of Sarasvati -

Witzel wants to argue that even in Rgvedic times, Sarasvati did not reach the ocean. This is possible only if both Yamuna and Satlaj had ceased to flow into Sarasvati completely by the time Rgveda was composed, and the Sarasvati had no other perennial glacial source of water. By the time of Brahmana texts, this does seem to have happened to some extent and at least the Yamuna²⁸ had ceased to flow into

²⁶ The extremely pedantic nature of Witzel/Klaus arguments can be judged from the needless hairsplitting they do in examining Rgvedic passages that say that the Satlaj, Beas rush towards the ocean (as in RV 3.33), or the frequently occurring Rggvedic clause ‘as all the rivers rush towards the ocean’. These scholars argue that the tributaries of Indus and Ganga do not really meet the ocean directly but fall into these two rivers, therefore the word *samudra* in all such passages should mean either the confluence of the tributaries with Indus/Ganga (or with each other) or it should mean the lower broad reaches of Ganga and Indus! The clause ‘all rivers meet the ocean’, is however a commonplace expression in Indian languages, and is also used in various scriptural contexts (such as Prasna Upanishad VI – “As all rivers meet the ocean losing their name and form”). In all these cases the word ‘*samudra*’ uniformly means ocean, even though we know quite well that Yamuna, Satlaj, Ravi, Beas and many other rivers do not meet the ocean directly but via Indus and Ganga. It is only the heavily conditioned mind of scholars burdened with AIT-related notions, that interprets the Vedic texts in such a tortuous manner and non-obvious manner.

²⁷ Witzel should of course be well-aware that the ordinary reader of *JIES* is neither aware of this newspaper debate, nor about the online articles written by Kazanas and Frawley. So Witzel is willing to gamble, and keeps mum about these articles.

²⁸ And also perhaps much of Satlaj waters. Note that rivers do not necessary change their paths completely at one time. They may first get braided, with different channels flowing in different directions. In fact, in later literature, Rgvedic Sutudri is called Shatudri – meaning a river with 100 flows. This indicates that as the river emerged from the Himalayas, its course split up into numerous channels. Even down to historical times, Satlaj has flowed in several parallel channels simultaneously. Therefore, Satlaj may have transferred

Sarasvati. That is why Tandya Brahmana 25.10.11-12 mentions *Plaksa Prasravana* as the source of Sarasvati. This site is still known, and is represented by a small muddy spring arising from the foothills of the Sivalik mountains. In the Rgveda however, the source of Sarasvati is not a muddy spring arising from foothills of Siwaliks (which do not have glaciers), as Witzel seems to imply (page 172). Rather the text (Rgveda 6.61.2) says that the river cuts through the mountains and emerges through them with a tremendous roar. But Witzel wants to consider this mantra and also Rgveda 7.95.2 as hyperboles, and rather give them a convoluted interpretation (pages 167, 168, 172).

The second argument that Witzel advances to prove that the Sarasvati did not reach the ocean in Rgvedic times is that Rgveda 3.33.1-2 which clearly indicates a confluence of Satlaj with Beas. Witzel therefore argues that by the time this early book was compiled, Satlaj had already been captured by Beas and therefore could not have fed the Sarasvati. Although Witzel shows acquaintance with the works of various archaeologists and other scholars who have written on Sarasvati, his argument is too simplistic to be compelling. As early as 1886, OLDHAM has suggested that that Sutudri was initially the name of a tributary of Beas which eventually captured (by headwater erosion) the trans-Himalayan river (now called Satlaj) as it emerged from the hills near Rupar in Punjab. The Satlaj, in ancient times, flowed straight south beyond Rupar, to meet Ghaggar (Sarasvati) near Shatrana. Even after it took a right angle turn at Rupar to meet Beas, Oldham argued that this does not mean Satlaj abandoned its old channel permanently. He suggested that even if Rgveda 3.33 meant a confluence of the two rivers (as suggested also by the Brhaddevata and Sarvanukramani), there is no guarantee that the Satlaj did not revert to its old course again to reinforce Sarasvati again.

Witzel's fears of a 'depleted Sarasvati' seem to be influenced by the current location of channels of Beas and Satlaj, and their confluence at Harike (near Jalandhar) in Punjab. In reality, this course was adopted by the two rivers only around 1796 AD. The three eastern tributaries (Beas, Satlaj and Ravi) of the Indus have frequently changed their courses. Before 1796, Beas and Satlaj met beyond Fazilka and split into 4 channels (which united again) before meeting the Chenab.

A look at the *Imperial Gazetteer of India* [New Edn., 1908, vol. 23, page 179] will show that the Greek geographers accompanying Alexander even noted that the Satlaj, Ravi and Beas drained together independently through a different channel east of Indus.²⁹ M. L. BHARGAVA [1964] has also examined the literary evidence and

just enough water to Sarasvati for it to flow up to Bahawalpur, with the remaining water flowing via different channels towards the Beas.

²⁹ "After it leaves the hills the river is never called Sutlej by the people and it has changed its course more than once in historical times. The history of those changes can be traced with considerable probability and detail. In the time of Arrian, the Sutlej found an independent outlet into the Rann of Kutch. In the year A.D. 1000 it was a tributary of the Hakra, and flowed in the Eastern Nara. Thence the former bed can be traced back through Bahawalpur and Bikaner into the Sirsa *tahsil* of Hissar, until it is lost near Tohana. From Tohana to Rupar this old bed cannot be traced; but it is known that the Sutlej took a southerly course at Rupar, instead of turning west, as now, to join the Beas. Thus the Sutlej or the Hakra – for both streams

the geological data and suggests that in the Vedic period, the Ravi and Beas both fell into Satlaj (whereas currently, the Ravi meets Chenab, and in medieval period, the Beas fell directly into Ravi and not into Satlaj). And finally, WILHELMY (1969) shows that the Ravi and Beas fell into Nara, a little south-west of Marot (in Bahawalpur) whereas Satlaj emptied into Sarasvati close to Fazilka.

The Satlaj itself has alternately merged with Beas and Ghaggar even in the last two millennia, and has often drained into Ghaggar or the Nara, instead of uniting with the Beas and the Indus system of rivers. At other times, the Satlaj united with Beas and the combined flow fell into the Ghaggar channel, a little beyond Fazilka. [vide the *Imperial Gazetteer of India*]. Consequently, the confluence of Beas and Sutlej does not necessary imply a 'depleted' Sarasvati because the united stream of the two Punjab rivers could yet have met the Sarasvati further downstream (while the upper course of Sarasvati was still fed by the Drishdvati and Apaya). Moreover, no amount of linguistic exercise can show how the name '*Shutudri*' (= 'swiftly flowing') in the Rigveda changed to '*Shatadru*' and '*Shatadhara*' (both meaning 'a hundred flows/channels' – a meaning also attested texts like the Mahabharata) in later times, unless we assume that before the Satlaj first took a right turn at Rupar, it split into numerous channels some of which still drained into the Sarasvati at various locations, one by one. This possibility is confirmed by the presence of numerous palaeo-channels spreading out like a fan from the spot where the Satlaj emerges from the mountains close to Ropar. Many of these channels do extend all the way to Ghaggar.

g) Link between Hakra and Nara-

Witzel emphasizes (page 170-171) that the Sarasvati never flowed to the sea because the link between Hakra in Bahawalpur and Nara in Sindh through the sand-dunes has never been established. VALDIYA [2002: 27-32] however shows otherwise [pace POSSEHL 2002: 239-240]. It may be noted that the Nara is still called the Sarasvati by rural Sindhis and its dried up delta in Kutch is still regarded as that of Sarasvati by the locals.

h) Omission of Beas in Rgveda 10.75.5 -

Rgveda hymn 10.75 extols the Indus river, and in the process, it enumerates several rivers verse 5 onwards. In verse 5, the rivers are enumerated from east to west, starting with Ganga. Surprisingly, Beas is not mentioned even though Jhelum, Chenab, Ravi and Satlaj are.

flowed in the same bed - is probably the lost river of the Indian desert, whose waters made the sands of Bikaner and Sind a smiling garden. By 1245 the Sutlej had taken a more northerly course, the Hakra had dried up and a great migration took place of the people of the desert - as it thus became - to the Indus valley. The course then taken by the Sutlej was apparently a continuation of the present course of the Ghaggar. About 1593 the Sutlej left the Ghaggar and went north once more. The Beas came south to meet it, and the two flowed in the same channel under various names - Macchuwah, Hariani, Dand, Nurni, Nili and Gharah. Then the Sutlej once more returned to its old course and rejoined the Ghaggar. It was only in 1796 that the Sutlej again left the Ghaggar and finally joined the Beas." Page 179 of the *Imperial Gazetteer*.

Witzel proposes an ingenious reason for the exclusion of the name for Beas in Rgveda 10.75.5. He says (page 171, fn. 103) –

“...normally the name of the bigger stream is used for the united one. When the great, glacier fed Sutlej joined the Beas, it would become the Sutlej, as is indeed seen in 10.75”.

This explanation is incorrect for several reasons. First, Beas is as much ‘glacier fed’ as is Satlaj. Second, the Vedic Rishi would not have known that the Satlaj is longer than the Beas and rises from southern Tibet, whereas Beas rises from the Himachal ranges. Tibet and Himachal would have been *terra incognita* for the Rishi, in all probability, and both the rivers would seem to appear from the Himalayas. Third, Satlaj does not carry significantly more water than the Beas. Both carry practically the same volume of water in modern times and the situation may be assumed to have been the same in olden days as well [MISRA 1970]. Fourth, historically, the combined channel of Beas and Sutlej has historically been called Beas, as pointed out by numerous scholars such as Aurel Stein.

There is a simpler explanation for the omission of Beas that is consistent with the references cited by Witzel, and also with the viewpoint of Kazanas. The hymn, which seeks to glorify Indus as the greatest of all rivers, enumerates two types of rivers –

First, those which flow into the Indus, directly or through a tributary

Second, those which do not flow into the Indus and reach the ocean or a desert lake independently.

My hypothesis is that the hymn mentions each and every river in the first category, because of which even the smallest tributaries and sub-tributaries of Indus are mentioned. However, many rivers in the second category are not mentioned. Thus, the Apaya and Drsadvati (flowing into Sarasvati), Sarayu, Rasa (Oxus?) are not mentioned. But even the tributaries of Chenab (e.g., Marudvrdha³⁰ = modern Maruvarthavan), Jhelum (Arjikya and Sushoma?) and small tributaries of the Indus such as Kurram are mentioned. According to this hypothesis then, the Beas should not be a tributary or a sub-tributary of the Indus. This is possible if the Ravi and Beas merged with each other, and then flowed together into Nara, as has indeed happened according to WILHELMY [1969].

The Hunt for ‘Pure Aryan Genes’ –

Witzel (pages 152-153) is very hopeful that genetic studies will eventually unveil the genetic tracks of Aryan immigrants to India. He cites several recent articles, notes that they do suffer from some deficiencies, but concludes nevertheless that –

³⁰ In EJVS 7.3, Witzel makes the totally absurd suggestion that Marudvrdha in Rgveda 10.75.5 could mean Beas.

“Recent work by Bamshad, Majumder, Underhill, Sells, and many others has uncovered preliminary evidence that not only points to prehistorical movements into India from Africa and the Middle East, but movements in later periods as well from Central and even East Asia.”

One of these papers, BAMSHAD [2001] really seems to have revived the sagging hopes of invasionists,³¹ and even the author of one of the ‘comments’ to KAZANAS [2002], namely D. P. Agrawal quotes its conclusions approvingly and uses this paper as proof that an Aryan invasion into India around 1500 BC is attested by genetic studies. This paper by BAMSHAD *et al* is obviously cited by Witzel (page 152, fn. 72) approvingly. So as an illustration, let us examine how sound the conclusions and methodology used by BAMSHAD *et al* [2001] are. We observe -

1. The sample size is very small, and restricted to one district in coastal south India, to where migration of upper-castes from North India is attested even by Vedic texts (for instance the legend in Aitareya Brahmana mentioning that descendants of Visvamitras moved east and south to become Pundras, Sabaras, Andhras and so on). No statistical justification is given by the authors for what is *prima-facie* an insufficient sample size.
2. The authors do not take into account the mobility of caste and sub-caste groups in social hierarchy. They just assume that present day Ksatriyas were Ksatriyas in 1500 BCE as well.
3. The European-ness of Ksatriyas, per the data in that paper, is greater than that of Brahmins, which is odd. If we adhere to invasionist scenarios, Brahmins should resemble the ‘Europeans’ most closely.³²
4. The genetic distance tables actually show that the ‘genetic distance’ between Indians as a group, and East Europeans is LESS than that between East Europeans and South Europeans. This puts a question mark on the very basis of the ‘genetic’ category ‘European’ employed by Bamshad *et al*.
5. The paper is silent on *when* these ‘Eurogenes’ entered the various castes of India. These genes could have well come during Shaka, Greek and Persian invasions and thus have nothing to do with the Aryans at all. The authors of

³¹ The American journal *Archaeology* [September/October 2001:13] summarizes the results of BAMSHAD *et al* and says -

“**DNA does tell tales**, according to researchers who studied from the genetic data of 250 unrelated men from the eight social castes of southern India. Y-chromosome analysis, which identifies the genetic material passed along the paternal line, reveals that members of the upper castes are more genetically similar to Europeans, while lower caste members share more genetic similarities with Asians. The study, by researchers from the University of Utah, Louisiana State University, and Andhra University, India, confirms literary and archaeological evidence for a Vedic invasion of the subcontinent from the northwest between 3,500 and 3,000 years ago. This “new” population is generally considered to have occupied higher positions within India’s caste system.”

Next to the news item is an uncharacteristically fierce looking, semi-naked Brahmin with a very ferocious look in his eyes. The caption next to the picture reads – “The blood of this Brahmin priest may hold evidence of a Vedic invasion”. (!)

³² There is a view however, proposed by Marxist historian D D Kosambi, and accepted by a few other scholars, that the Aryans co-opted with indigenous priest-hood, that became the Brahmana caste, while the invading Aryans themselves became the Ksatriyas, Vaisyas (and also supplied some Brahmanical genes). I think that the study by Bamshad *et al* is just too ‘quickie’ to be of any academic use.

the paper however assume that these genes were brought in by Aryans around 1500 BC.

In short, the study has several fundamental flaws and cannot be accepted as ‘proof’ of an Aryan invasion or immigration around 1500 BCE. In short, the authors have forcibly retrofitted their skimpy data into the invasionist hypothesis that ‘European’ Aryans invaded India around 1500 BC and formed the upper castes because of which these castes will have greater affinity to Europeans than lower-caste Indians. When a request was sent to the authors to clarify the term ‘European’, they responded by saying that the term merely meant populations west of Indus!

Miscellaneous

Witzel states that ‘the persistent tendency, even in modern India, is to begin geographical lists of places in the East....and then to proceed clockwise’ (page 115). This is completely false. The list of 52 Pithas of Shakti cult, occurring in the Puranas, starts with Hinglaj in Baluchistan, i.e., the westernmost site is named first. The verse³³ listing the seven holy rivers, recited by Hindus even today, mentions them in the order –Ganga, Yamuna, Godavari, Sarasvati, Narmada, Sindhu, Kaveri. This is hardly east to west or clockwise. The list of seven salvation granting cities, traditionally recited, also hardly goes from east to west – Ayodhya, Mathura, Haridwara, Varanasi, Kanchi, Puri, Avantika (Ujjain).

Witzel speculates that Daimabad could have been a Dravidian settlement (page 132, fn. 40). He does not explain why. The site is typically linked to Late Harappan culture, and if Harappan culture was Para-Munda (as suggested by Witzel’s 1999 article in *Mother Tongue*), then Daimabad should also have been para-Munda and not Dravidian.

Calumny, Half-Truths and Double-Standards:

a) Is Sethna’s Chariot pulled by Rajaram’s horses?³⁴

In his initial article, KAZANAS [2002: 310, fn. 19] had pointed out –

“In yet another misrepresentation Witzel writes: “The spoked wheels that Sethna wants to find on the Indus seals turn out to be in most cases oblong – resulting in singularly bad transport for Indus merchants” (2001:n.194). However, K. D. Sethna makes it quite explicit (1992: 50-51) that these indentifications were first made by Parpola and other Finnish scholars; he merely followed! Parpola is an invasionist and co-editor of Witzel’s EJVS!”

What Kazanas is clearly hinting at is Witzel’s double standards in ridiculing the views of an Indian scholar but refraining from making any negative remarks against Parpola even though it is Parpola who is the source of Sethna’s views. However, Witzel now feigns innocence, and in his comment (page 123, fn. 3), he claims that –

³³ *Gange ca yamune caiva godavari sarasvati....*

³⁴ The title of this subsection derives from the title of a post on this issue left by Steve Farmer on the Indian Civilization list, of which Witzel is also a member. See also the webpage in the following footnote.

“...merely mentioned Sethna’s oblong chariot wheels in the context of Sethna’s fantasies of a late Vedic, Sutra style Indus language.”

Witzel’s claim of innocence is deceitful considering his argument [WITZEL 2001: n. 194] is clearly derived from a cheap webpage created one year earlier by Steve Farmer³⁵ with whom Witzel had collaborated in attacking Rajaram, Sethna etc. through their articles in the Frontline magazine and elsewhere. The point is that if Sethna’s wheels are wobbly, then so are Parpola’s. But would Witzel (or Steve Farmer) ever ridicule Parpola in such a manner?

b) *Who is Rgvedic ‘rajan’?*

Witzel himself uses ‘battle of ten kings’ for *dasarajna* in WITZEL 1995a. So it is ridiculous of him to criticize Kazanas for using words like ‘king’ when referring to Vedic chieftains (page 124, fn. 4). In fact, even Geldner frequently uses ‘könig’ for ‘*rajan*’ in his translation of Rgveda.

c) *Insulting B. B. Lal -*

Witzel also alleges that B. B. Lal has been operating on the fringes of the Right-Wing Hindu movement for two decades now (page 127) merely because he is annoyed that Lal supports the Vedic-Harappan connection hypothesis.³⁶ Witzel has

³⁵ See <http://www.safarmer.com/sethna/pseudochariot.html> . Farmer had earlier made similar sarcastic remarks on this issue on the Indology (Liverpool) Listserv. In this discussion list, Witzel was also a prolific writer and had teamed up with Farmer in ridiculing and criticizing Rajaram, Sethna etc., culminating in their article in the Marxist biweekly ‘*Frontline*’. Some remarks from Farmer’s webpage – “What is ‘perfectly evident’ to Sethna is dubious at best -- and that’s being generous -- to anyone who bothers to check out the evidence, with which Sethna plays fast and loose...If confronted with this evidence, Sethna could potentially argue that the Harappan artists were incompetent and incapable of drawing round wheels. This would let him ‘save his text,’ to use the scholastic phrase for this sort of hermeneutics, but it would be a tough argument to support given the high level of artisanship seen elsewhere on Indus inscriptions. In any event, Sethna doesn’t use this argument, but is satisfied with letting the reader think that the ‘wheels’ are perfectly round -- not showing the original evidence, which tells a different story. Hunter’s diagram of the same seal from the 1930s isn’t nearly as regular as Sethna’s. To put it bluntly: Sethna’s ‘wheels’ aren’t round -- as is immediately evident when we look at the originals of his imaginary ‘chariot,’ which he transforms in his neat little diagram. I propose that Sethna’s ‘chariot’ exists only in a world where it can be pulled by Rajaram’s Harappan ‘horses.’ No other animal could get the job done.

³⁶ Ironically, Lal has been threatened with physical violence by a section of Hindu orthodoxy for upholding the fact that there is no archaeological evidence that the Hindu holy city of Ayodhya was settled before 700 BC. This runs counter to the Hindu belief that makes Ayodhya one of the oldest cities in India. Lal maintains that he cannot deviate from what his digging spade tells him, because he is a professional archaeologist. Recently, there has been a great controversy over the a site ‘Babri Masjid-Ram Janmabhumi’ in the town. Again, Lal gave his archaeologist’s opinion that the site was initially occupied by a Hindu Hindu temple which was replaced by a mosque. Overnight, he was dubbed as a Hindu fundamentalist by Indian Marxist and Islamist circles, precisely the groups that also uphold AIT and its euphemistic versions for political reasons. If Lal and Kazanas draw support from Hindu fundamentalists as Witzel insinuates, the he himself perhaps draws support from Indian communists/Marxists, Islamists and Christian missionaries by the same yardstick.

characterized Lal's earlier publications too as 'examples of Hindu exegetical or apologetical writings' in his own papers published in ERDOSY [1995] volume. Such charges have been rebutted in a very polite manner by LAL [1998], just as a gentleman and a scholar ought to have had. It is therefore unfortunate that Witzel should continue to insinuate all kinds of calumnies against reputed scholars.

d) Debunking Talageri in dreams -

Witzel boasts (page 144-145) how he has debunked TALAGERI [2000]. The objective reader can refer to the online response of TALAGERI [2001] to judge the matter himself. Witzel completely ignores Talageri's reply, not even referring to it for the benefit of the readers of JIES. Instead, continues to repeat the same claims that he made in his review of Talageri's book. For instance, Witzel criticizes Talageri for connecting Jahnāvī in Rgveda 1.116.19 and 3.58.6 with Ganga because the context also mentions dolphins, for which Ganga is famous. I will not recount Talageri's response [2001, section III.1] because it is available online³⁷. Witzel's assertion that connection of Epic Jāhnavī with Vedic Jahnāvī 'is not allowed' (page 145) is a dogmatic assertion, nothing else. It is better to draw as evidence direct data from later texts belonging to the tradition, than to propose something purely speculative, with no evidence to stand upon.

Likewise, Witzel repeats his objection to Talageri's argument that Rigveda 6.45 is an old hymn and that it mentions Ganga. About Witzel's current insistence on hymn 6.45 (which refers to the Ganga) being a late interpolation (or a composite hymn with different sections belonging to different periods), there are very categorical statements to the *opposite* effect in another of his paper [WITZEL 1997a: 257-345]. On p. 262 of this paper, he accepts Mandalas 2-7 as generally being earlier than the other four Mandalas (1, 8, 9, 10). Then, on pp. 292-293 of this paper, where he gives us the chronological levels of the hymns, he specifically places the poet of hymn 6.45 in the "Early Rgvedic level" – incidentally the *only* individual hymn specifically named by him here.³⁸ In any case, the reader can note that the 'unsuspicious' hymn has now become a 'suspicious' hymn, perhaps because it stands in the way of AIT.

Witzel also alleges (page 146-147) that both Kazanas and Talageri have misrepresented him in attributing to him the antiquity of RV 6.45. He claims (page 147) –

"What Witzel 1995 in fact said was that Book 6 "once mentions even the Ganga in an unsuspecting hymn (though in a trca section)." [6.45.31].

The comment on the trca status of 6.45.31, whose sense would be recognized by any competent RV scholar, was lost on Talageri and Kazanas....Oldenberg also distinguished other hymns which were in part compiled out of independnt trcas

³⁷ See the relevant section at at http://www.bharatvani.org/general_inbox/talageri/ejvs/part3.html for details. Witzel knows very well that the general readership of JIES would be ignorant of TALAGERI [2001], and so hopes that his gamble of omitting the mention of his opponent's reply will pay off.

³⁸ See also TALAGERI [2001: Section IV.6] available online at, http://www.bharatvani.org/general_inbox/talageri/ejvs/part4.html, where he has clearly shown why Witzel's objection is not applicable.

(and pragathas); the trcas out of which these hymns are compiled were of varying age. 6.45.31-33 is one such Trca addition, tacked onto 6.45, as the reader is again alerted in Witzel 1995.”

This explanation of Witzel is specious. First, in WITZEL 1995a, paper referred to here, Witzel gives a list of hymns which *or parts of which* are later additions. This list does not include RV 6.45. In fact, fn. 86 in WITZEL 1995a specifically refers to longer hymns (such as RV 6.45) *without* making a reference to the late date of the *trca* additions. The only thing that can be inferred from Witzel’s paper is that that the *trca* section is of uncertain data. It may be a ‘late addition’ but need not be ‘late’ chronologically in comparison with the rest of the hymn³⁹.

Even worse is the fact that in a paper presented by Witzel at a conference in Madison (Wisconsin, USA) in 1998⁴⁰, he categorically [WITZEL 2000:10] classifies the entire hymn 6.45 in the ‘Early Rgvedic period’ category, and further clarifies that the occurrence of word ‘*gaangya*’ in Rgveda 6.45.13, next to the name of Brbu indicates that in the early Rgvedic period, the IA settlement extended from Afghanistan to Yamuna/Ganga’ [Page 10, fn. 14].

So we see that WITZEL [1995a] contradicts WITZEL [1997a], which contradicts WITZEL [2000], which in turn contradicts WITZEL [2001] and now all of this contradicts WITZEL [2003]! Clearly, Witzel’s own take on the issue depends on who his audience is, and whether some opponent needs to be ‘decimated’ (see section g below) or not. Witzel’s frequent somersaults in discussing the same issue with different scholars are reminiscent of his behavior when the errors in his translation of Baudhayana Srutasutra 18.44 were pointed out [AGARWAL 2001].

e) Kazanas’ supposed ignorance of Aitareya Brahmana -

Witzel (pages 113-114) objects to Kazanas’ characterization of the Aitareya Brahmana as ‘early’ because he refers specifically to a passage in the 7th *panchaka*, which was tacked on to the first 5 *panchakas* at a later stage. However, can Witzel say for sure that the seventh *panchaka* of the Ait. Br. is later than the other Brahmana texts (Satapatha, Jaiminiya, Gopatha etc.). If no, then Kazanas’ general characterization of the text as ‘early’ stands. The passages Ait. Br. 7.33.6 or 7.18 do not refer to the ‘late Vedic inclusion into the Kuru orthodoxy of by the dwntrddn

³⁹ Now it turns out that in Spring 2003, Witzel taught a course titled ‘Indian Studies 117’ in which a revised version of this paper [WITZEL 1995a] is required reading. The revised version was available online at http://www.courses.fas.harvard.edu/~indst117/Source_materials/Historical_Evidence_from_Vedic_Texts/RgvedicPeriod but is not accessible to the public now. On page 51 of this version, Witzel adds the following revision after ‘unsuspicious hymn (even though in a *trca* section)’ – [i.e., a ‘hymn’ later on pulled together out of *trca* fragments of unknown age]. One wonders what is so ‘unsuspicious’ about RV VI.45 if it is composite, and has fragments of unknown date attached to it? Clearly, this addition is a result of afterthought subsequent to discussions with Talageri.

⁴⁰ A ‘tentative’ draft of the paper, dated 17 February 2000, is available online at <http://www.people.fas.harvard.edu/~witzel/IndusLang.pdf> It will be published eventually as ‘**Early linguistic data and the Indus civilization**. In: J. Kenoyer (ed.) *Proceedings of the conference on the Indus civilization*, Madison 1998’ according to Witzel’s CV available online at <http://www.people.fas.harvard.edu/~witzel/mwbib.htm>

Iksavaku lineage of Kosala of eastern (Bihar) tribes’, as WITZEL [1997a:308, 308, 327] has argued elsewhere. Rather, it seeks to explain the ‘downgraded’ nature of the Angas, Pundras and so on.⁴¹

f) *shatranaashaka Oldenbergaastra*:

Witzel considers an knowledge of OLDENBERG [1888] as a pre-requisite for any ‘revolutionizing’ research on the RV, but elsewhere [WITZEL 1995:311] he refers to Oldenberg’s principles as being based on “formal characteristics”, and, on the very next page, he writes:

“To begin with, it is surprising how scholars have persisted with formal characteristics which cannot be independently evaluated – unless we already know the distribution and mutual influence of Rgvedic dialects and poetic diction per book, clan and poet. This, however, remains to be done.” [WITZEL 1995a: 312]

In other words, Witzel has himself suggested the *limited* utility of Oldenberg’s principles in his earlier publication, but now argues that their knowledge is indispensable. Why? Because it suits his immediate purpose of dealing with Kazanas. It may be noted that other than this casual name-dropping exercise, Witzel has not really shown how Oldenberg’s work invalidates Kazanas’ thesis.

g) *Professor Rahul Peter Das’ Viewpoint is ‘Decimated’!*

Witzel boasts that his teacher F. B. J. Kuiper has ‘decimated’ R. P. Das’ objections (page 131, fn. 38). In another earlier publication [WITZEL 1999a, fn.2], Witzel similarly calls Kuiper’s response to Das ‘rather scathing’ and ‘well deserved’. I find this manner of defense of Kuiper rather childish, and any reader can refer to the respective articles of KUIPER [1995] and DAS [1995] to see who is being dogmatic. In fact, DAS [2000: n. 2] responds to these attacks in the following words -

“Cf. DAS (1995) and the response by KUIPER (1995), which WITZEL (1999: n. 2) polemically calls not only ‘rather scathing’ but also ‘well deserved’, without giving any further reasons. In fact, KUIPER’s response is dogmatic in that it refuses to consider anything except a linear evolution from Indo-European to Vedic by means of mostly clear-cut phonetic and morphological developments, so that what does not conform to such transparent developments cannot but be ‘foreign’. My contention was that it is much more probably that we are dealing with a multi-linguistic reality with different synchronic and diachronic developmental forms of Indo-Iranian and Indo-Aryan spoken side by side, as well as at different times, most probably with interferences and alterations due to varying languages not only of redactors, but also of recitors (especially before any final redaction), so that before labeling anything ‘foreign’ it is imperative that one examines all such factors in detail. Since much in this realm can only be conjectured upon, this clearly may result in a situation in which one simply cannot reach any satisfactory conclusion. Of course, one can brush aside all such considerations as nonsense and insist that the tradition view is the only one permissible, but that will not make the problem go away. WITZEL’s own sophisticated study is heavily influenced by his teacher KUIPER’s axioms, and is as such in the final analysis probably going to convince only those already convinced and leave the skeptics as skeptical as they were.”

⁴¹ In fact, Witzel’s own understanding of the Aitareya Brahmana is questionable. See section IV.12 in TALAGERI [2001] available online at http://www.bharatvani.org/general_inbox/talageri/ejvs/part4.html

Anyone who disagrees with Witzel becomes a target of his barrage of abuses, sarcasms, and cheap remarks, and is thereby ‘decimated’!

Concluding remarks –

Practically everything is possible on this earth. It is even possible that human beings are descendant from extra-terrestrial aliens. But how probable is it that this did really happen? *Almost zero*. My point is that a mere demonstration that the occurrence of a historical is possible is not a sufficient proof that it did occur. One also needs to demonstrate that the probability of that possibility having become a reality is high. In short, to establish a case, one must make it likely, not simply ‘not impossible’.

The conventional Aryan Invasion/Migration Theories and their new Siamese twins that are delivered each passing day imagine a unique situation in South Asia around 1500 BCE. It is a situation that is a *simultaneous combination of several independent and improbable factors or events*. Statistically speaking, such a resultant situation is even more unlikely to have happened than the individual events could have occurred individually. These theories basically advocate that –

1. The IA speakers preserved their voluminous literature, heritage and religion despite being on the move, even when passing through vast inhabited territories, something that is contrary to norm (with a few exceptions such as that of Polynesians).
2. The IA speakers managed to Aryanize the culture, religion, language of the indigenous population of an area of 3 million sq km., without leaving any literary, archaeological, genetic, anthropological evidence. This is against the norm and very few exceptions exist.
3. The process is said to have been achieved without much violence or use of force. The ‘acculturated’ or ‘conquered’ peoples have no memory of this having happened. This is again against the norm.
4. A culturally inferior people are said to have overwhelmed a more advanced civilization. This is again against the norm.
5. Evidence from Geology and Archaeoastronomy contradicts the soft linguistic evidence.
6. South Asian cattle (zebu) appear around the same time in the Middle East that Aryans supposedly enter South Asia – movements in opposite directions.

Many other reasons could be cited to argue why the Aryanization of much of the Indian subcontinent around 1500 BC is a highly implausible scenario. At least, the existing body of evidence from various fields does not compel us at all to accept such a thing. This does not mean of course that the Indo-European speakers were indigenous to the Indian subcontinent and that this area is the original homeland of IE languages. Again, the existing body of evidence does not compel us to accept such a possibility being real. One rather needs to examine the data afresh, with an open mind, as Kazanas urges, and determine if the coming of IA languages into the Indian

subcontinent could have happened much earlier than when Indo-Europeanists and Indologists believe this had really happened.

With these comments, I end my critique.

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THE END

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