

## *Additional Comments*

N Kazanas, Jan 2011

This paper was written earlier this year to correct some violations (or errors) of scholarship by Mrs. Karen Thomson in an article published in *The Journal of IndoEuropean Studies* 2009; these were not spotted by the referees or the three commentators, fact which shows how little all these people know about Sanskrit and the archaeology of the ISC. My own paper has just been published in the same Journal, the issue of Dec 2010. With it is published Mrs. Thomson's reply, 'The plight of the *Rigveda* in the twenty-first century', which I shall deal with later in 2011. At the end of my paper I now add some comments which were not included in my published paper so as not to make it too bulky – §§8-10.

### ***Rgveda* 7.95.2 and Karen Thomson.**

1. In her paper 'A still undeciphered text' (2009) Karen Thomson (KT hereafter) deals with several rigvedic issues, continuing with her idea that the *RV* needs a new approach. I agree with much of what she writes but find some errors of methodology and of fact. She sent me a copy of her paper in 2009 (together with the three Comments). I made some notes then but had to put the matter aside due to pressure from many sides. I am surprised the *JIES* referees did not spot the errors. Just as surprisingly, the three critical Comments on her paper also did not spot them. I shall confine my observations to these errors – without intending to demean the rest of her good work..

Before I deal with the passage 7.95.2 and *samudrá*, a few words on *árma(-ká)*, *vailastha-* and ruins (in 1.133.3), a subject that immediately precedes KT's treatment of 7.95.2. I agree with her dismissal of Witzel's (and others') view that *armaká* means 'ruin'. How scholars (e.g. Witzel 1995: 3-4; Rao 1991:32; Burrow 1963 *passim*) came to this strange conclusion is not difficult to understand since it appears in that sense in some post rigvedic texts and as final in compound names of old villages like *guptārma* 'hidden, preserved'; also, initially, under the Invasion Theory, scholars thought the fiends and goblins mentioned in this stanza were the native enemies whom Indra had to destroy. However, hymn *RV* 1.133 has nothing that remotely suggests ruins. Moreover, stanza 6f states explicitly that invincible Indra 'does-not-kill-men' *ápūruṣaghna-* ! Certainly, there is a ghostly scene of frightful desolation with unfriendly she-fiends, goblins and demons (*yātumāti*, *piśāci* and *rākṣas*) but not a single mention of bricks, the chief building material of Harappan constructions, stone-slabs, fallen walls, beams or rafters and the like. In sharp contrast, the Old English poem *The Ruin* contains abundant persuasive details of the ancient remains (from Roman times?) so that some scholars think it refers to the town of Bath (Mitchell and Robinson 1996:252-5).

KT rightly cites Mayrhofer (EWA, 2, p120) who gives for *árma-* the meaning *wohl Brunnen* ‘perhaps *spring*’ and connects the word with Tocharian *ālme* ‘spring’ and names of European rivers like ‘Almus’ etc. This certainly seems to be so. The element of flow and moisture appears in the eye-disease called *arma* (in Suśruta). The word occurring in stanza 3 is very probably a (*guṇa*) development from √*r* (>*ár-ti*, *iy-ar-ti*, *r-ṇó-ti*, *r-cchá-ti*: 2nd, 3rd 5th and 6th class) with the sense ‘move’ (in the *Dhātupāṭha*, the native list of roots, √*r*=*gatau* ‘movement’). Similar formations are √*kr* > *kár-man*, or √*dhr* > *dhár-man* etc. The word would therefore entail movement in its denotation – like ‘spring’, ‘flow’, ‘up or down’ and the like, not a static state of ruins. And, of course, in the *RV*, *armaká* is *hapax legomenon*; but consider similar formations *anta-ka* ‘ending’, *karma-ka* ‘action’, *reca-ka* ‘out-breath’, etc. I will come back to this.

In the same stanza 3, we find *vailastha* - and cognates which are also *hapax legomena*: *vailasthāna(-ká)* and *mahā-vailastha*. These are left as “uncertain” by KT (p 28), following Mayrhofer. But surely this need not remain so! The stem *vaila-* is clearly a (*vṛddhi*) development from a primary stem *vila-* (> *vel-* then > *vail-*) and a possible dhātu √*vil*. Now, the native *Dhātupāṭha* has √*vil* twice and for both the meaning *bhedane* ‘breaking, cleaving’. It is surely paradoxical that we have primary *bila* and secondary *vaila-* but not primary \**vila* and secondary \**baila-*. This suggests that the two stems are connected. Indeed, the Dictionaries have √*vil* or *vila* but direct you to √*bil* or *bila* because, as we know, the consonants *b* and *v* are often interchangeable (e.g. *varh* and *barh*, *vala* and *bala* etc). √*bil* is not in the *Dhātupāṭha*, but is in the Monier-Williams Dictionary with the sense ‘breaking, cleaving’. The word *bila* means ‘cave, cleft, hole, opening, pit’ (*RV* 1.11.5 and 1.32.11 etc). So *vaila-* has to do with a cave or pit. Surely now, since we meet demons, fiends and sorcerers in this ghostly scene we can connect it with the great pit or chasm (*vavrá* in 7.104.3 and *párśāna* in 7.104.5) wherein are cast and destroyed evil-doers and fiends, as 1.133.5 says *sárvaṃ rákṣo ní barhaya* ‘[O Indra,] hurl down/in every demon’. So stanza 3 of hymn 1.133 prays to Indra (*maghavan*) to dash down (*áva-jahi*) this band of she-fiends/sorceresses in the downrush/sweep/vortex (?=*armaké*) that-is-within-the-pit (*vailasthānaké*: in the place/room of the pit), in the downrush (etc) within-the great-pit (*mahāvailasthe*)<sup>1</sup>. The scene now is the pit of hell wherein are cast and dissolved fiends, sorcerers, witches and other evil-doers. (I am happy to consider any other reasonable suggestion – but not ‘ruins’!)

If this delusion about ruins falls in ruination, then the *RV* is seen not to know either Harappan towns or their collapsed remains. Is it not then legitimate to assume that it is pre-Harappan? I should think so – especially if this aspect is taken in conjunction with others. Our investigation leads us to this issue unavoidably.

<sup>1</sup> Here I acknowledge my debt to Sethna’s comments in his 1992 publication, pp130-135.

2. While KT consulted Mayrhofer for *árma(-ká)*, she did not do so for *samudrá*. This is strange, surely. Had she done so she would have found that he gives for *samudrá* ‘confluence (*Vereinigung*) of two or more rivers’ and also (and for the *RV*) *Flut* ‘flood’ and *Meer* ‘sea’. She prefers her own ‘together-water’ which is etymologically correct but is not very helpful since it can denote anything from water in a cup, a puddle, a lake, to a cataract, rain, river, ocean. Here again she is right in exposing Witzel’s mistranslation and misrepresentation of “basic literary facts” regarding *samudrá* (Witzel 2001 §25, fn 204).

However, she abandons the awkward ‘together-water’ and translates 7.95.2 about the river Sarasvatī *śúcīr yatī giribhya ā samudrāt* as ‘pure, travelling down from the mountains, from the gathering-place of the waters’. And immediately one wants to ask how she knows that there was a “gathering-place of waters” up in the Himalayas. Why so?...A long line of vedicists, both Indians and Westerners, have invariably translated ‘pure, flowing from the mountains (*giribhyaḥ* ablative plural) to (*ā*) the *samudrá* (abl singular: confluence, ocean, sea and Witzel’s ‘terminal lake’). I am certain she knows that the Ablative does not require the *ā* either as preposition or postposition to express “movement from” as Pāṇini makes it abundantly clear in his fourth book: e.g. *RV* 4.51.8c – *rtásya devīḥ sádaso budhānāḥ* ‘the goddesses [are] waking **from-the-seat** (*sádaso* < *sádasah* ablative) of-Natural-Order (*rtásya*)’.

KT opts to differ because she thinks that the two ablatives are parallel and the preposition *ā* (which she calls postposition and adposition) governs the first one, i.e. *giribhyaḥ* with the sense ‘from’, and by extension the second one also with the same sense. She adduces the views of some comparativists in the Indo-European field (instead of Pāṇini) and avers: “Indeed, some linguists have argued that adpositions were *invariably* placed after the word they govern” (p 32; my emphasis). Now why does she do this when she knows perfectly well that such a view is utterly untrue? Why refer at all to indoeuropeanists and not to an acknowledged authority on Vedic?... All we need here is not pointless pedantry but a quick look at MacDonell’s *Vedic Grammar* which gives us the bare facts<sup>2</sup>. Writing of prepositions *āti*, *ádhi*, *ánu*, *ā*, *úpa* etc, this indisputable authority says: “As a rule these prepositions follow, **but also often precede** their case” (1916: 208; §176, 1: my emphasis) Then he adds: “*ā* with the abl., if following, means *from* (*on*); **if preceding, up to**” (p209, ¶176, 2b: my emphasis). But he points out also in a footnote here that *ā* sometimes precedes with the sense ‘from’ (cf my example c, below). Indeed, the *RV* corroborates this with many such instances. Obviously one must use one’s reason and discrimination in every situation.

<sup>2</sup> She refers to MacDonell’s *A Vedic Reader for Students* (1917 OUP), which cannot be understood without following up the teeming references to his own Grammar! She could also have consulted Wackernagel’s *Grammatik* which again she cites elsewhere! Thus she uses her sources and authorities selectively, as we shall see below, to suit her own notions.

KT does give a similar example where the  $\acute{a}$  seems to follow its case and then govern with the same meaning a subsequent noun. This is it, with four more nouns in the Ablative which are omitted for brevity's sake:

$\acute{a}$  *yatu índro divá  $\acute{a}$  prithivyā, makṣú samudrāt...*

‘Come hither Indra from the sky or from the earth, Swiftly from the samudrá...’ (4.21.3)

Yes, but one could argue with much reason that the first  $\acute{a}$  goes with the verb *yatu* as is very common, the Ablative *diváḥ* without the  $\acute{a}$  denotes the place from which emerges movement (and so do *samudrāt* and the subsequent Ablatives) while the second  $\acute{a}$  governs *prithivyāḥ*: the meaning would now be ‘Let Indra come from the sky to earth [where I am], swiftly from the *samudrá...*’ (and so on with the other Ablatives). This seems to me far more reasonable.

Here are some examples with the preposition preceding its case:

- a) 1.30.2 : *vayám hí te ámanmahi  $\acute{a}$  ántād  $\acute{a}$  parākāt* ‘we thought of you both nearby and at a distance’.
- b) 1.151.5 with similar construction :  $\acute{a}$  *nimrúca uṣasaḥ* ‘until evening and until dawn’ (or ‘at evening...’ etc).
- c) 7.6.7, which is early : Agni Vaiśvānara received treasures  $\acute{a}$  *samudrād ávarād  $\acute{a}$  párasmād...* ‘from the lower samudrá and the upper [one].’
- d) 3.53.11, also early : *svastí  $\acute{a}$  gṛhébhya ...  $\acute{a}$  vimócanāt* ‘wellbeing up to the houses ... until release/unyoking’.

In all these the  $\acute{a}$  precedes and in every situation one has to use one’s common sense and textual content. And with the aid of Lubotsky’s *Concordance* one could cite dozens of other examples with *adhi*, *úpa*, *pári* etc. Take some examples: ‘[The Maruts] like birds sat upon [their] beloved barhis-grass’ *váyo na sīdann **ádhi barhīsi** priyé* (1.85.7d); ‘Eloquent you repeat upon the waters’ *suvāco vādathana-**ádhy** **apsú*** (7.103.5d). ‘The eagle shook out from the mountain/rock the other’ *ámathnād anyám **pári** śyenó **ádreḥ*** (1.93.6) These examples should suffice. If the preposition invariably followed the inflected word it would be very difficult to have such compounds as *ádhy-akṣa* ‘eye-witness’, *anu-kāmá-m* ‘according to desire’, *pári-vatsará* ‘complete year’ etc, etc, etc.

To (wrongly) postulate invariable syntactic patterns (of this sort) for the *RV* is to show ignorance of Vedic and of poetry generally. (Consider the line from Eliot’s *Four Quartets* “In my end is my beginning” or the abrupt, startling reversal in John Donne’s ‘Thy beams so reverend and strong/Why shouldst thou think [O Sun]?’ from *The Sun Rising*.)

Since the position of  $\acute{a}$  in 7.95.2 is not anomalous, it is not surprising that generations of scholars have translated monotonously ‘from the mountains to the

*samudrá*'. Moreover, KT's rendering 'gathering-place of waters' has a serious difficulty with reality and common sense. There is no 'gathering-place' of 'together-water' on the Himalayan slopes. There are masses of snow, ice and glaciers and as soon as these melt they flow down as rivers. And the Vedics surely knew of the conditions on the Himalayas as they knew of the vast ocean far down south.

### 3. Many scholars doubt that the Vedics knew the ocean.

If KT had consulted the *Vedic Index* she would have seen that all the difficulties raised by modern indologists (e.g. Elizarenkova 1996-7; Klaus 1985) were dealt with adequately therein – except Witzel's 'terminal lake' which is really a non-starter. The conclusion was that the Vedics did know the ocean. Using common sense again, also in the Invasion scenario, we can see that even if the Indoaryans had entered Saptasindhu, the land of the seven rivers, in N-W India from a landlocked region, they would have found out about the ocean from the natives, who engaged in trans-oceanic commerce with Mesopotamia. Since, always in the AIT scenario, they had so intrepidly trekked thousands of miles, they would have travelled down south either on boats on the Sindhu or with carts; there they would have seen the ocean. Surely, there is nothing extraordinary in this. So why all this fuss and denial of the Vedics' knowledge of the ocean?... Reasoning must always be paramount.<sup>3</sup>

### 4. Could *samudrá* denote the ocean? Of course, frequently.

To speak of 'together-water' or 'gathering-place of waters' is unhelpful to say the least and, again, ignores common sense. A gathering place of waters in mass is a confluence or a river or a lake or the ocean. Now we know that there were several confluences, rivers and small lakes in Saptasindhu, but *samudrá* is usually in the singular, as all my citations herein show. Thus even if we accept "the gathering-place of waters", we would expect a plural for the Himalayas, the valleys and the plains simply because there were many gathering-places of waters. And when Agni received treasures from the lower and the upper *samudrá* (*ā samudrād āvarād ā párasmād...*), we understand only one lower on earth and one upper in the sky. If it is one terrestrial *samudrá* it can only be the ocean since, otherwise, there are plenty of confluences, rivers, lakes and general gathering-places.

<sup>3</sup> I don't really understand scholars who invoke scientism in the humanities ("scientific approach, method" etc). In Physics, Chemistry etc, scientists check their results against the realities of the material world, often with Maths and always with reasoning. We can't ignore the realities in our field (i.e. facts archaeological, grammatical and literary) and, most important, common sense.

However, I offer yet another passage – 7.49.2 :

*yā́ ápo divyā́ utá vā srávanti, khanítrimā utá á vāyā́ḥ svayamjā́ḥ; samudrārthā yā́ḥ śúcayaḥ pāvakā[h]* etc: ‘The Waters that are heavenly, or flow in channels, or arise spontaneously, [and] are clean and purifying, have as their goal the *samudrá* etc.

The heavenly waters are the river or watermass in the sky and, of course, the rains; the waters flowing in channels are natural rivers or man-made ditches; these that arise spontaneously are springs, lakes and wells. Ditches, lakes and wells do not in ordinary terms aim for a larger gathering place: these we can ignore. Obviously the one gathering place of waters which rains, rivers and springs have as their ultimate aim is the ocean. Confluences themselves move on as larger rivers to the ocean (or a large lake); lakes have themselves arisen spontaneously (if not created and fed by rivers) and there are many of them. So the only watermass (in the singular) left is the ocean.

Let us take a final example. 1.116.4 says that the Aśvins saved Bhujyu from drowning and carried him for three nights and three days (*tisráḥ kṣápas trír áha-*) to the ‘distant dry-shore of the watery ocean’ (*samudrāsya dhánvan árdrasya pāré*). Yes, we have a hyperbole which is common in the *RV*. But the poet has a specific intention here. Now, what “together-water” or “gathering-place of waters” is so large that the two gods, who fly on a car drawn usually by birds, would need three days and nights to traverse to reach its distant shore?...<sup>4</sup> Only the ocean.

**5** KT refers also (p33) to G. Possehl’s 1998 paper and cites the passage: “it seems unlikely that the ancient Sarasvati flowed to the sea during those times. The absence of a river scar suggests that the same is true for later periods.” (1998: 350) This is absolutely true. However, for reasons known best to herself, she does not divulge that “those times” are the centuries 1500-1000 BCE! She also does not tell us that with the very next sentence Possehl suggests “that the river once did flow to the sea, in very ancient times... (3800-3200 BC), but even this is not certain”. She refrained perhaps because she wished to spare us Possehl’s uncertainty. Nevertheless, the archaeologist thinks the river might have flowed to the sea but KT turns the archaeologist’s hesitancy into certainty that the river did not reach the sea! A more thorough investigation would have revealed to her that Bridget Allchin, a most reputable British archaeologist, expert on Indian protohistory and the Indus Valley Culture, expressed no doubt about the river reaching the sea taking the Nara Nadi river beyond the Derawar Fort as the natural continuation of the ancient Sarasvatī (1999). In the same year L. Flam, another expert published independently an identical certainty about Nara being the continuation of Sarasvatī (1999). French archaeologist P-H

<sup>4</sup> I discuss this issue very fully and give ten more examples in my 2009 publication, ch 5.

Francfort had reached the same conclusion with certainty back in 1992 giving dates 3600-3800 and before. The doyen of Indian archaeologists, B.B. Lal, also expresses no doubt in his 2002 publication that the ancient Sarasvatī flowed into the ocean through the Nara. And more recently, a team of reputable Indian scientists traced by satellite the course of the river (Possehl's "river scar") from the mountains to the ocean flowing into the Rann of Kachch or Kutch. (Sharma et al 2006).<sup>5</sup>

So the Sarasvatī, this *nadītamā* 'best of rivers' did flow into the ocean before 3500 (to give an average date), i.e. before, first, its tributary Sutlej was captured by the Sindhu and, later, the Yamuna was captured by the Ganges. This is the picture presented by the *RV* even in the hymns of the tenth and latest Book. So, again, when were these hymns composed?

6. Closely connected with the Sarasvati is the word *ándhas* which occurs in *RV* 7.96.2 and also engaged KT's attention (p24). The word is in the dual: *ubhé yát te mahinā śubhre ándhasī, adhikṣiyānti pūravaḥ* (her translation: ' Since through your might, O bright one, The Purus inhabit both *ándhasī* '). Dismissing Griffith's translation "grassy banks" for *ándhasī* and Geldner's *Getränken* "drinks" (echoed also by Renou in French and Elizarenkova in Russian), KT goes off to examine the controversy between indigenists and invasionists and refers to the Bryant and Laurie book, *The Indoaryan Controversy* (2005) but not to the debate in the *JIES* (2002-3), which is in fact later, i.e. more recent, since the papers in Bryant and Laurie were written before 2002! Thus she offers no translation of her own and so, as with the *vaila*-group and *armaká*, the text continues to remain undeciphered. Let us see if we can sort this out.

Of course *ándhas* is well attested in the sense of 'darkness (and blindness)' ; also 'bush, grass, plant, stalk' (esp 1.28.7) and '(soma-)juice'. Mayrhofer, as many another before him, links *ándhas* with Greek άνθος 'flower' and with Friesian *ändul* 'fine, tender grass'. (This is acknowledged by one Comment and KT in her second paper.)

The 'bright one' *śubhrā* addressed in the verse is Sarasvati. So we must ask what two things a river has: it has a beginning and an end; a surface and a bottom, the riverbed; and two banks. Common sense and the diction with the dual concord *ubhé...ándhasī* compels us to dismiss all notions except the "two banks". The two banks are the only places where the tribe of the Pūrus could dwell (*adhikṣiyānti*). Obviously the "two drinks" of Geldner, Renou and Elizarenkova, despite the massive notes that accompany this rendition, make no sense at all (not even metaphorically), since there are many, not two, drinks from the river-water and since the fluid denoted by *andhas* is the specific soma-juice.

True, *ándhas* as 'bank' is not attested anywhere. But why should this matter? Are we to abandon the reality of the material world and our common

<sup>5</sup> Most of this information except for the 2006 publication was presented in my 'Final Reply' in *JIES* 31, Spring (pp 228-9) but very few scholars bother to read nowadays, widely, attentively and impartially.

sense for the sake of philological pedantry? After all, *RV* 8.22.17-8 says that king Citra and lesser-kings *rājaká-* dwell along the Sarasvati (– along its banks, obviously). The enclitic *te* in 7.96.2 ‘thy’ covers both *mahinā́* ‘greatness, might’ and *ándhasī*.

Moreover, there is the figure of speech called ‘synecdoche’ which uses the part in order to indicate a larger whole, as in “I counted ten heads” instead of “ten persons” or “ten sheep”. Similarly here, we have “both bushes/grasses” to denote the “grassy banks” of the river. There is nothing extraordinary about it. Griffith, not for the first or only time, proves more faithful to the spirit of the poet<sup>6</sup>. And let us acknowledge the simple fact, that much of the *RV* is not just poetry, but great poetry!

7. Several more points could be made about KT’s article but I decided to keep this paper short. As I wrote earlier, I agree with most of what she says questioning various established but probably wrong meanings. One general point of caution. India has excellent archaeologists (Lal taught in California; Chakrabarti D. teaches at Cambridge, U.K.) and, of course, unrivalled sanskritists. Some Western indologists deride them with terms like “quacks, Hindutva hacks” and the like; almost all tend superciliously to ignore Indian scholars thinking that only Western indologists (mainly comparativists) transmit the truth. This is a sad mistake.

By way of conclusion I present seven items, apart from ruins, that are not found in the *RV* but are common in the Harappan culture and are found in post-rigvedic texts, especially the Brāhmaṇas and Sūtras.

- i) bricks – *iṣṭakā* ;
- ii) cotton – *karpāsa* ;
- iii) fixed altars or hearths ;
- iv) iconography – relief or statuary ;
- v) urbanization on a significant scale;
- vi) writing – *lipi* or *lekha(na)*;
- vii) dried up Sarasvatī. (There are more.)

<sup>6</sup> Consider 6.75.8 *tātrā rátham úpa śagmám sadema*. Geldner translated “...auf den wollen wir der Wagon setzen” and Witzel gave it in English “on this (*rathavāhana*) we wish to put the useful/strong ratha”; O’Flaherty too gives (1981:237) “on it let us place the working chariot”. But the verb *úpa-sad-* never means ‘place, put’; it means ‘sit by/near/on, revere, approach respectfully’ and the like. Only the causative form *úpa-sād-aya-* does mean ‘make sit on, place upon’! Here again Griffith got it right – “let us here...honour the helpful Car”. Of course, Griffith does make many mistakes but this is understandable when we consider that Vedic studies had only just begun.

Moreover, there is no evidence whatsoever to suggest that the *RV* was composed c1200-1000 BCE - none other than fanciful theory and mechanical repetition. All the tremendous arsenal used once upon a time to support the Invasion scenario has now been reduced to horses and chariots: chariots for war and races did not exist before 2000 and horses appear in *Saptasindhu* only after c1500 – according to Prof. Witzel who has become the main spokesman for this theory.

The meagre evidence for domesticated horse at the Harappan sites often adduced in discussions, is a red herring. There is no significant increase of horse-remains after the period 1500 BCE. If there was an entry of Indoeuropeans bringing horses and chariots at c 1500 BCE, there should be masses of such remains. There is no such evidence until the centuries of the Common Era. KT, as others before, rightly points out (p36) that horses are not quite so common in the *RV*, as many scholars claim (see also Kazanas 2002: §VII,1 with many more references). She also shows that the much mistranslated and thus maligned *rátha* is not a “war-chariot”. In fact, in his translations in his *Vedic Reader*, MacDonell never gives the word ‘chariot’ but always ‘car’. The “chariot” is a legacy of classicism (Greece and Rome). Moreover, rigvedic cars are made from native timber (*RV* 3.53.19; 10.85.20). They have space or seating for three *trivandhurá* (*RV* 3.6.9; 6.47.9 etc) and one is a minibus *rátha* having space for eight *aṣṭāvandhurá* (late 10.53.7): they are drawn by oxen, donkeys, antelopes and rarely by horses! There is not a single mention of one- or two-spaced *ráthas*. All this was discussed extensively in Kazanas 2002, §VII, 2-3. So the “war-chariot” is another red-herring.

I leave it to the readers to draw their own conclusion about the approximate date of the composition of (the bulk of) the *Ṛgveda*.

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#### *Additional comments*

8. When I finished my paper and J. Mallory, the Editor of JIES accepted it, I sent a copy to Mrs. Thomson since we have known each other and corresponded on and off for some ten years. She expressed some displeasure with some points and so I changed them to soften the criticism.

Our acquaintance began when she wrote to me after one article of hers and one of mine were published in the JIES in 2001; she thought we had similar ideas, but in my reply I explained that I was a rebel against mainstream indologists and sanskritists, like Witzel, Stephanie Jamison and so on. I also explained that, contrary to her belief that the *RV* could be translated in the spirit of its ancient composers and that she would accomplish this, I thought this could not happen unless the translator knew excellent Vedic but, in addition, reached the spiritual state of the ṛṣis who “heard” their *śruti*.

In 2002 and 2003 I wrote two papers for the *JIES* again, as part of a larger discussion-controversy on IAn origins, in which Bryant, Mallory, Parpola, Witzel, Zimmer and others also participated. As I cited her 2001 paper “The Meaning and language of the *RV*” I sent her a copy of my efforts. To my astonishment she wrote back an angry email forbidding me to quote from her paper and demanding that I excise the citation! I apologised but pointed out that since her study was next to mine and said some sensible things, other scholars would consider me daft not to refer to them. But in a peculiar paroxysm of unreason she came back insisting on her earlier demand and adding that she did not want to be dragged into this controversy! I pointed out again that she was not being dragged into any controversy just because I quoted a passage from her work that was published and fully accessible to anyone. But she would not hear of this. So our exchanges stopped until 3 or 4 years later when she wrote and apologized for her behaviour.

I mention this to help the reader understand perhaps why she hurls so hotly and repeatedly such intense negative criticism.

9. Mrs. Thomson presents a stance of neutrality claiming to be interested only in the text of the *RV* and its linguistic interpretation. This is merely a superficial pose and one of superciliousness at that.

Like most mainstream indologists, she too displays repeatedly the three banes that vitiate their studies: selectivity, distortion and mechanical repetition. Thus she examines at some length E. Bryant's publication *The Quest ...* (2001) and thinks it neutral but does not notice some crucial errors and omissions: for example, Bryant refers a lot to the linguistic aspect of the IA controversy but does not deal with the comprehensiveness of Sanskrit and its organic coherence, nor with its more archaic character and the matter of isoglosses. If Bryant or Mrs Thomson had bothered to examine in depth, say, the isoglosses, he or she would have discovered that only a place close to Saptasindhu could, as the source of all these phenomena, account adequately for their divergences.

She also refers at length to Max Müller citing among his other works and his *History of Ancient Sanskrit Literature* but does not deal with the matter of dating the Sanskrit texts. Yet Müller explains fully (pp 214-7) how he arrived at his chronological scheme (*RV* c1200-1000; *AV*, *Yajus* 1000-800; *Brāhmaṇas* 800-600; *Sūtras* etc down 200 BCE). He identified a certain Kātyāyana in a ghost-story in the *Kathāsaritsāgara* (c 1100 CE) with the sūtrawriter Kātyāyana whom he assigned to c200 BCE and thus worked backwards to the *RV*. He also had to take into account the chronology established by Archbishop Ussher or Ireland who calculated from the lives of the patriarchs in the Old Testament that the creation had started at 4004 BCE! So Müller's scheme is based on two fictions – plus the fact that, at that time, it was unthinkable that an eastern culture could be more ancient and comprehensive than the classical Greek and Roman cultures of Europe.

Another serious contradiction, emerging from her effort to appear neutral and attack now invasionists and now indigenists, is revealed in her attitude to sanskritists like Witzel, Jamison, Doniger, and to archaeologist Lord Renfrew. She castigates the sanskritists because, in her opinion their Sanskrit is not as good as hers or because they don't treat the texts, always in her opinion, as reverentially as she does, and so their interpretations are riddled with errors. On the other hand she cites with approval Renfrew's statements that he found nothing in the *RV* showing that the IAs invaded and slaughtered or enslaved natives and that they might have been native: such statements by 1987 when Renfrew published his book *Archaeology & Language* had become common place (though by no means general with mainstream indologists), after Dales's seminal article in 1966 showing that there had not been an invasion and conquest in Saptasindhu. But Renfrew knows no Sanskrit at all and relied on some dubious translation of the *RV* in contrast to the sanskritists. Moreover while scholars like Witzel, Jamison and others have a wider awareness of the protohistory of Saptasindhu, certainly much wider than Mrs Thomson, Renfrew knows again very little about that area (please see his poor bibliography) and, what is more, in a later part of his book he makes an abrupt 180° turn and has the IAs pouring into Saptasindhu in order to support his (now discredited) theory that all Indoeuropeans emigrated from eastern Anatolia transporting agriculture and its techniques together with the IEn languages. Renfrew is a brilliant

example of mainstream scholarship: he changes sides abruptly and totally and casts away truth in order to uphold and promote his own pet theory. This verges on schizophrenia and, in any case, it is quite dishonest. Why should anyone follow such erratic theorizing? Not once in his subsequent works does he refer to the *Rgveda* or the Indoaryans as indigenous. Anyway, his theory has been disproved and deservedly discredited. That anyone should cite him as a reliable witness seems to me incredible. But, of course, Mrs Thomson knows even less than Renfrew about Indian protohistory, and, as it seems, did not read the whole book (*Archaeology and Language* 1987) or, selected only what suited her momentarily; so such blunders should not surprise us.

Here again we witness either mechanical repetition (and negligence) or selectivity.

10. I suspect her assumed neutral pose and her claims of being concerned for the text and translation of the *RV* are quite hollow. I suspect also that her knowledge of the *RV* and its language hasn't much depth despite her published studies and the course she offers on the internet. If she were really concerned, she would not criticize so negatively others' translations yet give none of her own and leave uninterpreted so many words. One can understand that lexemes occurring frequently in the hymns need examination and study of all their contexts. But this does not apply to *hapax legomena*, i.e. words that occur only once, like *armaká*. These can be dealt with here and now since there are no other contexts to analyse and compare. It is the easiest thing on earth to criticize the work of other scholars but it is thoroughly dishonest not to give your own version(s).

What does *armaká* mean in 1.133? She criticizes but she hasn't enough knowledge or courage or honesty to supply some interpretation. How should we translate *ándhasi* in 7.96.2 ? Here again she maintains cold, superior silence on the correct meaning but criticizes all extant renderings.

Silence is golden, I fully concur. But it would be better to maintain it where negative criticism wells up so profusely. How can she translate the hymns when she has no idea how to translate these once-occurring words? This is a self-contradiction.

And, in the final analysis, what does it matter if one or two words are translated so as to fit the context but without secure knowledge of their meaning? It does not in the least, provided the interpretation complies with common sense and conforms to the realities of the material world. To deny constantly the usefulness of such approaches is a symptom of pretentious and conceited pedantry. It is a pity Mrs Thomson does not concentrate on the (re-)examination of lexemes – a task she accomplishes quite well – instead of rambling on with irrelevancies, criticizing and putting on a ponderous pontifical pose.

In due course I shall deal with the paper that purports to be a reply to mine in the same Dec 2010 issue of the *JIES*.