

17/9/01
To V Agarwal
Subject AIT

Dear Shri Agarwal. Thank you again for your observations and long note(s) on Sarasvati (22-3/8/01). The last piece I include in my reply to Prof Witzel together with your two notes to him (5/7/01).

Attached is my *Addendum*... together with your notes, which now form the reply to Prof Witzel. Please circulate and post the entire text.

Here, I explain briefly why I have not followed all your suggestions.

You wrote an excellent essay 'What is the Aryan Migration Theory' (1/5/01) but it is a "Love's labour lost" since there was no migration to India. The supposed imposition of river-names and the Aryan language and culture on the Harappans could only result from invasion and conquest: there is no historical case of a migration with such results. So we need not play the invasionists' game and argue about an alleged "migration".

We need not juggle about with piddly arguments re Sarasvati. It is not a controversial issue. Invasionists ignore the most obvious and practical aspect that *nobody praises in the present and to such an extent a dried-up river!* Only inhabitants of cloud-cuckoo-land would think or do so.

My view is and, unless new revolutionary evidence turns up, will be what it has been since 1996 when I abandoned the AIT: the Indian tradition is remarkably true. The Great Bharata War was c 3100, ie 35 years before the advent of the Kali Yuga and only just after the arrangement of the *RV saMhitA*. The bulk of the hymns are from the 4th millennium – though some few passages are later and some perhaps much earlier. The hymns are not contemporary with or later than the IVC; if so, we would find in them urbanisation, large buildings, fixed hearths/altars, bricks *iStakA*, rice *vrIhi*, etc etc. No, the *RV* is pre-Harappan and now the astronomical data (N Achar) establish this beyond doubt.

In any case, there has never been any serious evidence for the AIT (not even linguistic). It should have never been formulated – as the ptolemaic celestial system perhaps need have never taken root. There is only prejudice, pedantry and pretence – heavy, inherited, mechanical habits.

So, my young friend, I ignore your suggestions for diplomatic caution. Achar's early date for the Great War may cause controversy but the archaeoastronomical finds are much firmer than any other kind of evidence: you can't argue against heaven. We can't ignore Achar's data and thereby imply that this scientist fabricates evidence or that the ancient writers fished up these facts in order to mislead us – only the invasionists having sufficient perspicacity not to be deluded. Such thoughts seem paranoic.

My friend, you must see that here we have the vindication of the Vedic oral tradition – an issue of great significance. The date 3100 has momentous historical importance. At about this time writing emerges in Mesopotamia and Egypt and IVC, along with the construction of large cities. This is no accident, it seems now, but the very descent of the Kali Yuga with the decrease of memory and virtue, the increase of avarice and other dreadful qualities. This matter is not only of Indian (nationalist) concern; it impinges on the history of the whole humanity.

Furthermore, the *RV* alone of all the ancient IE and indeed other known traditions of that period c 3100 speaks clearly (X, 129; VIII, 58; III, 55; etc) of Unity being the primal substratum and cause of all phenomena, including the several deities that receive worship in the hymns. This is of inestimable significance.

So, my fellow-voyager, let us "Fare forward" as T S Elliot says in his *Four Quartets* ('The Dry Salvages' III, end): "Not fare well/ But fare forward voyagers". Best wishes –

N Kazanas.

Prof Witzel replied to 'The AIT & Scholarship' in two consecutive pieces on 5/7/01.

V Agarwal replied to him on the same date (5/7/01):

Date: Thu, 05 Jul 2001 17:08:40 -0000

From: VAgarwalV@cs.com

To: aroik@comvos.net

First, I clearly stated that Dr. Kazanas' article is a 'comment' on EJVS 7.2 and this was drawn from Mr. Kazanas' own description of his article. He told me that it is not a point to point reply to EJVS 7.3 (since much of it was written PRIOR to publication of EJVS 7.3) but nevertheless he said that he has tried to address most of the major points in EJVS 7.3. There is nothing incongruous in saying so, because EJVS 7.3 itself is a rehash of the old material put together in a tendentious manner. Much of what MW has written therein is contained in published works earlier, OR in his OWN posts on the Internet. Dr. Kazanas relied on these internet messages dating onwards from 1996 or 1995 and so happened to reply to similar points which were made in EJVS 7.3 automatically. And in fact, I also made it clear in a subsequent message in the IndianCivilization list that the article by K is NOT a point by point reply to EJVS 7.3 But unfortunately, as has been noticed by many others also, Witzel's anyamanaska nature prevents him from reading the words of others carefully and thoughtfully.

Yes, there are spelling errors in his article, and misspellings in the bibliography. But even your own articles in EJVS have such errors!

Witzel's grouse that a large portion of the article written by Dr. Kazanas does not deal with AIT is not justified if one looks at the name of the article itself - 'AIT and Scholarship'. Clearly, AIT is only one part of the article. In fact, it is more like a case study of Indology using Indology ('Vedic studies' in particular) as an example. Dr. Kazanas has CLEARLY stated in the beginning of the paper itself that it IS NOT written in an academic manner, because of its 'random jottings' nature and so on. So why is Witzel repeating what is stated by Dr. Kazanas himself? Besides K HIMSELF states that this is a revised version of a document posted earlier on the IC list. So has MW made a new discovery in this regard?

Much of Witzel's criticism below is general and dismissive in nature without addressing exactly what is wrong. He faults Dr. Kazanas for quoting Dr. Hock's paper selectively, whereas MW has done precisely the same (as amply demonstrated by K). Dr. Kazanas' main purpose was to examine whether Dr. Hock had successfully countered the OIT model on the basis of linguistics or not, and whether Dr. Witzel has used this paper properly or not. The results can be seen in K's article clearly and what Hock concluded or not is inconsequential. In fact, K DOES acknowledge Hock's conclusion but states that it is questionable. So why is MW complaining? List members will now note the crusading zeal of the Harvard professor in propagating his own Aryan fantasies as 'established facts' and then proceeding to denounce people who disagree with him as fascists, Hindutva followers and so on. This is the level of his discourse. Pity his students!

Date: Thu, 05 Jul 2001 17:09:08 -0000
From: VAgarwalV@cs.com
To: aroik@comvos.net

Since the confluence of Satlaj and Beas is mentioned in 3.33, it follows that the Sarasvati inferred from this hymn must be diminished in size (since its supposed tributary Satlaj is already captured by Beas).

However, this implied assumption of yours does not stand scrutiny because in several geological models, it is the Satlaj WITH THE BEAS MERGED IN IT, that meets the Sarasvati downstream, whereas large portions of source waters of present day Yamuna fall into Sarasvati further upstream via Drshadvati. Eg. include Yash Pal (1980), Wilhelmy (1969) etc. Not citing complete references because they should be known to you. [Note that the proponents for these models or scenarios had no axe to grind and did not base their thesis on RV 3.33)

In fact, Wilhelmy proposed that the combined channel of Ravi, Beas, Satlaj met Sarasvati somewhere in Cholistan at some point of time. At another point of time, it is likely that Satlaj (or at least a portion of its waters) met the Sarasvati much upstream at Shatrana.

Consequently, even if Beas meets Satlaj as in the mantra mentioned by you, there is no problem.

To suggest that the epithets nadiitamaa etc. is used for Satlaj and therefore it is at par with Rigvedic Sarasvati is special pleading. Even if we eliminate references to Sarasvati where a heavenly goddess or knowledge/Vac personified is meant, Sarasvati still is exalted above all the other rivers mentioned in the RV.

If you want to banish RV Sarasvati to heaven or to S. Afghanistan, that is clearly your problem. The fact is that most Vedic experts and virtually all archaeologists (not counting the Communist variety historians and 1-2 archaeologists in India) identify it with the Ghaggar-Hakra clearly indicates that a 'scholarly consensus' exists in this matter and yours is a non-mainstream, revisionist view.

On your speculation that the older name of Sarasvati was Vaishambhalya etc., one can only check the relevant sections of the Taittiriya Brahmana 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 Jnanayajabhashya of Bhatta Bhaskara. No need to derive Munda or para-Munda or Language X etymologies. Sarasvati was so called because it nourished and sustained masses of people. A meaning which fits the ritual context of the sections very well. As for your reference on the occurrence of the V word in Bharadvaja Siksha, one can only wonder in amusement. The Bh. Siksha is a LATE text and is MERELY AN INDEX OF WORDS IN THE Tai. Br.

So its occurrence in the Siksha is of NO INDEPENDENT utility. Your inclusion of that reference in the EJVS article is meaningless, except that it 'pads up' your publication.

On your own point that Samudra need not mean ocean, I suggest that yours is now a one sided, monolateral interpretation which cannot be sustained just because you choose to throw references like 'Klaus 19XX' and so on. One can check the references you cite and determine very easily (as Dr. Kazanas has indicated in a few cases) that you often

MISUSE them and DISTORT them. Not to mention that you overlook (or are ignorant) of articles that prove/state the opposite of what your cited references say. For 'Samudra' itself, I had cited two references to you earlier (incompletely) and I expect that I need not give their address in full since being a Harvard Professor and that too a linguist, you will know everything. These two references deduce the opposite of Klaus' article. And of course there are several studies on the oceanic imagery in IE literature (RV included) in journals like JIES which clearly assume 'ocean'. That you should choose to remain ignorant of all this is surprising.

From V Agarwal to N Kazanas (23/8/01)

----- Original Message -----

From: VAgarwalV@cs.com

To: aroik@comvos.net

Sent: Thursday, August 23, 2001 3:40 AM

Subject: The vagaries of Sutlej

After sending you my comments yesterday, I thought that matters can be made more understandable by including this following quote (reference at the end) that discuss the shifting courses of Sutlej even in the last 2500 years. The point to note is that the Sarasvati in the Vedic texts is a mighty river, fed by Sutlej and also tributaries that now fall into the Yamuna. The different perennial sources of Sarasvati sometimes came back to the old channel, BUT NEVER together and so when the matter is looked at from the long term perspective, the mighty river gradually got reduced to a seasonal stream (despite periods of a slightly more copious flow).

QUOTE

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 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] UNQUOTE

Reference: The Imperial Gazetteer of India (New Edition). 1908. Volume XXIII;
Clarendon Press: Oxford

Addendum to 'AIT and Scholarship': reply to Prof Witzel.

N Kazanas: Director Omilos Meleton, Athens.

I must thank Prof M Witzel (=W, hereafter) for his critical comments (7/5/01) on my 'AIT and scholarship' (=AS, hereafter). My intention was not to follow him further but some colleagues think I should reply. So I compromise by replying only to the few points I noticed as I read and checked Mr Agarwal's attempt to defend me. These gave me the impression that W doesn't really connect with what he reads, if the subject matter generates strong disagreement within him. This may sound harsh but will be demonstrated in detail. I shall not deal yet again with Sarasvatī. V Agarwal covered the matter adequately in his reply to W (5/7/01 and 23/8/01) which I include immediately above.

However, before proceeding I should mention that Dr N Achar has sent me a copy of a talk he gave in Montreal (2000) in which he confirms that some astronomical references in the *Mahābhārata* indicate 3067 as the date of that war (see § 12, below). Of course, this leaves no ground for the AIT to stand.

1. W ignores the title, the opening statement and the entire § 4 of my essay and criticizes me for indulging in "musings" and for not dealing with all the points in his most useful opus EJVS 7.3. Neither the AIT, nor the invasionists' or non-invasionists' views, nor IE reconstructions, nor philological, archeological etc pursuits, hold much interest for me. What essentially concerns me is the subject of Ethics (in Scholarship and daily life) because this will determine how we meet death. Everything else in the world can be avoided, hoodwinked etc, but not death. Ethics also infuses communal life with the quality of excellence.

P Kingsley writes at length of the "conspiracy of silence" with which classicists have treated the discoveries in Southern Italy regarding Parmeneides and the tradition of his followers – "of misrepresentations, of misuse and abuse" (1999: 258). It is hardly different from the way mainstream indologists have treated – and continue to treat – the indigenous origin of the Aryans in India.

To write of the usurpation and misuse of the term "academy" and its cognates may seem tedious "musing" to W but his view merely underlines the hard historical fact of the departure of so called academic studies from their Platonic origin and their corruption in modern times, as Kingsley also argues.

W is right in that I do selective commenting (not reading). It would be wasteful to indicate, comment upon, or disprove every point in the AIT and every hole in W's thinking. It is sufficient to show that the major points are wrong or irrational and establish the firm foundations of the view of indigenous origin.

2. My premise is the fact (which W accepts) that there are no actual archaeological (and kindred) data supporting an Aryan entry; now astronomical references also (§ 12, below) give a severe blow to the AIT. Invasionists start with a linguistic theory, then, anxious and panting, hunt to find facts to prove the theory and concoct further theories (refined and complex) while they wait for the archaeological data to turn up. I think the latter approach is less rational, less practical. W thinks my view is a "farce". So be it.

3. The word "legend" has in English also the meaning "letters on a coin/medal" or "writing accompanying a picture". It is the same as the German *Legende* (=Aufschrift, Begleittext, Bildunterschrift). But W takes my phrase "laconic legends" (on the IVC seals) and *expatiates with unconcerned irrelevance about Harappan lengthy myths and the like* (his p 7, § 13, b).

This is not some highly abstruse and controversial matter. God knows why W makes such a comment.

4. He dismissed my parallels to the *mousa/mantu-* example somewhat facetiously (his p 9). I examined 10 more cognations from *aj-/agō* to *hū-/cheō* and found no similar parallel. I don't think, and I don't think he thinks, that, in comparison with Greek, Vedic is *that* forgetful. Consequently the alleged cognation Gk *mousa/ S mantu* appears to be incorrect (AS p 25, Note 3).

Then W ignores completely the end of my Note 3, where I state clearly that Comparative Linguistics has "regularity in some areas and has produced some good results"; he suggests that I don't know about this (– Thank you very much, my good Professor). My main objections appear, again clearly, in the 4 or 5 areas which I mention earlier and which show gross irregularity – and W remains silent and pretends they don't exist. Perhaps *he* can tell us whether this is honest.

5. In his eagerness for strict correctness W criticizes (his p 3, bottom) my cognations and etymologies for *reith*, *ratai*, ('car, cart') and *rota*, *rad* ('wheel, running') etc (AS § 5, p 6). In this he uses J Pokorny but **wholly ignores** the fact, plainly stated by me, that I use C D Buck. Now Buck's *Selected Synonyms...* (1949) was reprinted by the Chicago University Press in 1989, that is 30 whole years after Pokorny's *Indogermanisches...* (3 vols, 1959), and several years after even Mayrhofer's KEWA. Now, the Chicago linguists either did not bother to consult Pokorny thinking that Buck had done a definitive job for eternity, or they did but thought that Buck's work was better or, at any rate, not so bad as to need correction. I do often consult Pokorny (and Mayrhofer and Burrow and my incomparable Stamatakos in Greek and have, for some years now, stopped pulling – metaphorically – my hair at their disagreements) but this time I did not, I thought Buck and his successors would be correct in this field. The real difficulty here is that there is no guarantee that in 20 or 30 years' time W's own information will not be dismissed as inadequate by some other Professor(s).

W is right to find "strange" my remarks on the ProtoSlavs in this linguistic context. Yes, this passage (AS § 5, p 7) is badly written and would make sense only in the context of my 1999 paper (pp 23-5) where I discuss the proposed PIE urheimats outside India. I apologise.

In the context of chariots and wheels, W brings his heavy linguistic artillery to crack the tiny nut of *rathavāhana*. My translation is absolutely necessary in the context which demands the praise of a chariot-oblation not of some "platform". Furthermore, W knows that there is no hint anywhere else in the *RV* or the *AV* that chariots were dismantled: this is an idea imported from later and non-Indic practices. He also knows that the compound-accent is not invariable (eg *āśvam-iṣṭi* 'horse-seeking' but *viśvam-invā* 'all-/moving/pervading'; etc, etc) and that *aritra-pāraṇa* means 'going across with oars' and not 'going across oars' (as he would have it in parallel with his *ratha-vāhana* 'carrying the chariot'); also that *rathavāhana* can yield 'the chariot's [function of] transporting'. (I suspect that is why W lists so many examples, which distract but have little force.) So he should turn his grammatical guns against himself and W O'Flaherty who translate the ordinary verb *upasad-* 'honour' as though it were causative *upasādaya-* 'place on' (please read ! my AS § 3 where I criticize O'Flaherty's translation, in a passage which W obviously did not read and then produced a similar translation. He should at least have criticized me!).

I would be prepared to grant that this *RV* VI, 75 hymn is a very late one and that *rathavāhana* indicates a platform – but for one practical detail. Stanza 8 states that upon this vehicle are already laid *nihita* weaponry *āyudha* and mail *varma* ; then, according to O'Flaherty, Witzel et al, the stanza says 'let us place on it the chariot...'. Is this really likely? Would they first place the weapons and mail on the alleged platform and *then* the chariot with the danger of damaging the weaponry? Would it not be more practical to place first the chariot and then the weaponry? ... Unfortunately, most modern academics live in their own cloud-cuckoo-land and think everybody else does so also, including the seers of the *RV*. If we take here *rathavāhana* as 'chariot' and the rest of the stanza as honouring or paying respect to it, we face no such difficulties.

The adjective *br̥ha(n)t* certainly has 'high' as primary meaning. So what?.. Much more certainly, it is absurd to translate "Sarasvatī high like a chariot" – which is bad poetry and means nothing. The poet says plainly his divine river is to be praised *upastutyā*; so (with added assonance) he presumably packs all possible meanings into *br̥hatī* – not just 'high': [with waves] *tall* like a chariot; [with a current] *broad* and *stately*...; [with water] *shining* like [horses-and-] a chariot [rolling in sunlight]; [a river] *large* and *mighty* [which nourishes many communities along its banks]. So Sarasvatī is compared to something large, bright and majestic, not a narrow, one-/two-man chariot of 1½m width.

My translation of the *AV* passage also makes much better sense in that context than the "chariot-platform". It is, by the way, plain that I *merely state* Whitney's translation: is the imaginary criticism, that W sees, due to his habitual urge to distort things, perhaps?

Mugdalinī's race (X 102) *may* be a "VERY SPECIAL case" (so W), but then W should direct us to some *common-case races* in the *RV*; the only other one I recall is *Aitareya Br* IV, 7-9, also not very common, where different gods' chariots have yoked to them mules, cows, horses and asses (which win: Aśvins). If he can't do this, why imply that I cheat by not referring to other races? I don't expect we shall get a rational reply here.

Incidentally, I did consult P Rauling's *Chariots, etc*. Its very admirable erudition sheds not one ray of light on *rigvedic* vehicles. The photo from de Mille's *10 Commandments* (1956) is truly indicative of how this IE issue is approached by all these experts. Details from NE vehicles are projected backward or to other regions and so *RV* is said to be composed not before these irrelevant chariots.

Furthermore, on p 6, § 5 of AS, I referred to evidence of spoked wheels in my 1999 paper (p 33). W wrote (27/4/01) that he had read this 1999 paper but it is obvious he has not and scornfully refers only to "Sethna's wobbly 'spoked-wheel' Indus signs": he ignores both my evidence and the fact that Sethna takes his "wobbly wheels" from Asko Parpola himself! Is *this* honest? Is it not very difficult to discuss anything in this fashion?

He informs us that several of my references to the *ratha* are mythological (one-wheeled chariot being the

sun, etc). Now, since I myself do this twice (AS § 3), is not his action, again, irrational if not deceitful?... Is it not rather fraudulent, to suggest, as he does by this method, that, once more, I don't know these facts?... Finally, it is plain to anyone who can read that I do not preclude the existence of light chariots (end of §3); the rigvedic evidence is simply not adequate for the assertions made by W (and others).

6. W is right in that I didn't mention his point about the male non-recombinant Y chromosome. Considering the uncertainty involved I didn't think it crucial. But, yes, I apologize for my omission.

In this context also he quotes me as though I found "*faults*" with Cavalli-Sforza. I can see he needs the word "faults" so as to squirt sarcasm, but the word I used was "difficulties" (and "inconsistencies"), and if he had read (as he said that he had) my 1999 paper, he would know what I meant. Lord Renfrew also, I write, found difficulties with the geneticists' methods. So sarcasm should be aimed at in that direction also - unless W practises discriminatory self-restraint.

7. Yes, W is right, I omitted H Hock's conclusion; but *not any of his points leading to it*. So where is the fault? W knows that writers make their conclusions smooth (sometimes rigged). At the outset we should be quite clear that S S Misra does not claim that Sanskrit (Vedic or Old Indo Aryan) is the PIE, as Hock suggests. Misra exhibits some sentiment, perhaps because of patriotism and religious feeling, but he is very careful throughout his study (1992) to keep Sanskrit (Vedic or Old IndoAryan) quite distinct from PIE. Thus he writes "the Sanskrit language has also suffered linguistic changes and the original Indo-European proto-speech was not the same as Sanskrit" (p2); also, it is "the oldest language of Indo-European" (p 84). This is very far from Hock's (false) statement that Misra supports a "Sanskrit -origin hypothesis" (1999: 2).

With regard to the "PIE-in-India" (or, better, Out-of-India) Hock notes two aspects of severe difficulty - "plausibility and simplicity". On p 13 we find the implausibility which is the scarcity of horse and chariot remains in Harappan sites and earlier; but this is not a real difficulty. On p 14 he gives Fig 1, the genetic table of the IE branches, which is generally accepted. On p 15 is Fig 2, the isoglosses - an area full of quick-sand uncertainties. On p 16 he states that if the model in Fig 1 is accepted, then the hypothesis of an Out-of-India migration would be "**relatively easy to maintain**". He then adds the "difficulties" which are chiefly what he calls "principle of simplicity" (i.e. one migration into India, many out of it), aggravated by the isoglosses. This difficulty I countered with other considerations. Now if there are grave difficulties then a view is **not** "relatively easy". (And "relatively" to what?... There is only the opposite view, the AIT!) Hock makes no other point. Here I should add that the difficulties of the "principle of simplicity" (one entry as against many exits) would apply to any other locus of dispersal.

Now if something is "relatively easy to maintain" then that is precisely what it is - and that is exactly what I stated 3 times. I omitted (and added) nothing. W accuses me falsely, again distorting my statements. His fight should be with Hock.

Also in the context of Genetics (Aryan bones etc) W denigrates me for being "simple-minded" because I seek simplicity. Hock invokes simplicity but gets no blame just as Renfrew got no sarcasm in § 6, above. Obviously I must accept the (illusory) fact that life is not fair.

8. Perhaps W might, if nothing else, explain what is wrong with my "elaborate" account, as he calls it, of the Norman Conquest of England. He mentions it twice, on his p 1 (§ 3) and p 9, describing it as "K[azanas]'s elaborate Norman example". It is not mine at all but one *he has been using* for many years! I refer to it only to show that this violent invasion is not a historical parallel to the supposed peaceful entry of the Aryans which W likes to advocate. A historical parallel for the Aryan entry is absolutely essential.

Before turning to Sanskrit, I read English (at UCL) and though I have forgotten most of it, I have memories of the effect of the Norman French on English (courtly and legal language). This effect W invokes now, but it has no bearing on the issue under discussion, i.e. **the mode of the Aryan entry** into Saptasindhu.

He invokes [Lord Colin] Renfrew as his source of the Norman French being an example of élite dominance. But apart from citing Renfrew's name (W's pp 1-2), W gives no reference - **because there is none**: Renfrew does not give it as an example! It looks again as if W is trying, with this lame excuse, to put the blame on another (as in repetition of an incident which we had better forget).

Lord Renfrew loves elegant models but gives no real, worked-out paradigm of élite dominance (1987: ch 6, II, etc); yet everybody parrots away this slogan without ever having an actual clear picture of it. Renfrew certainly knows enough English History not to cite as an example the Norman Conquest. Obviously, the Norman French in England form no historical parallel to the Aryans who, W repeatedly tells us, did not intentionally invade NW India, but entered in waves or "trickling" and the like. W insists on not seeing that such a mode of entry could not possibly produce results that could only follow from an invasion, that is imposing

names of rivers etc and the language and culture generally of the “immigrants” (as I made it quite plain in §§ 3 and 19 of AS).

He now introduces the Nepalese and the Turks. Of the former I know nothing. But as regards the Turks, my neighbours who brought some river-names and used them in Anatolia (so W), I fail to see their relevance as an example of the refined and complex infiltration (that becomes an élite dominance) which W, Erdosy, Parpola et al promote. The Turkic hordes (especially the Seljuks from the 11th century, and later the Ottomans or Osmanlis) butchered and burned their way through Central Asia, Iran, Mesopotamia, Syria, Armenia, Anatolia and the Balkans. They were undisguised invaders and conquerors, who, yet, were themselves conquered by the local religions (mainly Mohammedanism), the Byzantine cuisine and other cultural elements in these areas. If W doesn't really believe that this is how the Aryans entered into Saptasindhu, then why bring the Turks into the discussion?... Besides, I argued that if the Aryan invaders did give new names then they were not only invaders but also conquerors (like the Turks) – a point W conveniently ignores. But W does not think that the Aryans were invaders and conquerors like the Turks; so the matter gets very confused – and one wonders why?

9. W also reminds me of the Minoan - Mycenaean situation and all the literature etc, as a parallel to the Harappans-Aryans. Again let us look at the historical facts very summarily. In the generally accepted version (there are variations) the Mycenaeans arrive by the 16th century burning and conquering as they establish themselves. They learn writing from the Minoans, whom they also conquered on Crete, and in the next 150-200 years produce their "Documents" (tablets at Knossos, Pylos, Thebes etc) **which at all sites are mere inventories**: no religious or legal texts, no poetry or narrative or philosophy. (Meanwhile the Minoan writings remain undeciphered.) Then c 1200 new peoples invade Greece and destroy the Mycenaean centres. There follow about 300-400 years of non-literacy and (for us) darkness. We have thus two distinct, widely-apart waves of invasion. At c 800 writing reappears under the influence of the Phoenician (or other Near Eastern) script plus many other arts and crafts - which all together constitute one "Orientalizing Revolution". The epics etc appear c 750 and later. (Don't forget also that the Mycenaeans are archaeologically attested – very distinctly so – unlike the Aryans.) W may seriously suggest that this development in the Aegean has some resemblance to the alleged IndoAryan entry, but I doubt whether anyone will believe it. No, the AIT paradox of the literate IVC without literature and the non-literate Aryans with literature remains – unique and inexplicable.

10. It is difficult to see the parallel in W's reference to the Albanians also. Regrettably I must elaborate again. The Albanians (being Christians then) began their thrusts southward c1340-50 into Thessaly and in the 15th century down into Attica, the Peloponnese and some of the islands. At this period the Byzantine empire was collapsing fast. Most of the central and northern area of (what is today) Greece and Albania were initially under the Bulgarians but by 1350 the Ottoman Turks had invaded and captured these lands (except for Euboea, Attica, the Peloponnese, Crete and many islands that belonged to the Venetians, Catalans, Franks, etc). Albanians were fast becoming Moslems and their incursions occurred under the encouragement and protection of their fellow Moslems, the ruling Ottomans. The Albanian language was influenced by Turkish more than any other IE branch in the Balkans. Christians were slaughtered or driven off and many Albanian communities were established in Greece. Some of them (in Attica and Boeotia) exist even now (2001) but, while they still speak also an Albanian dialect, they became Greek (Christian and Greek-speaking) 5 or 6 generations ago. Turks and Albanians never quite ousted the basic Greek population element nor extirpated the Greek language and Christianity (nobody now believes the extreme claims of Fallmerayer). So here again the situation is hardly relevant to the alleged (trickling) Aryan entry.

11. W complains and criticises me for being impressionistic, not giving statistics (his p7, b; my §19). But if he had read my 1999 paper (as he claimed he had) he would have found statistics on pp 22-3: out of 14 IE deities' names (Agni, Usas, Dyaus, etc) that are attested in two or more branches (with cognate words like L *ignis* 'fire', in other branches that had not retained the gods' names) the RV alone has all 14 of them, Greek has 9, all Germanic branches together 6, Roman also 6 and the others less, with Hittite only 3; Avestan has no Agni, Uşas, Dyaus etc. Such a disparity is to be explained only by the preservation principle (AS § 19, p 23). I append the Table with 15 names (and we could have more, eg Śri, always to the advantage of Vedic). Some, like Varuṇa and Indra, may be disputed and left out, but the pattern would not change significantly. For obvious reasons I have not included Indo-Iranian cognates like *Mitra/Miṭra*.

The Table (It has been checked by 6 mainstream referees.)

Vedic	Other IE branches
<i>Agni</i> :	Slavic <i>Ogon</i> . L <i>ignis</i> , Lth <i>ugnis</i> , Ltt <i>uguns</i> . (Note: even the Iranians who had Fire-worship did not preserve this name, not even as a demon like Indra, Sauru etc, though the stem appears in the name <i>dāśtāγni</i> .)
<i>Aryaman</i> :	Mcn <i>Are-mene</i> and Greek <i>Ar-ē-s</i> (?); Celtic <i>Ariomanus</i> (Gaul)/ <i>Eremon</i> (Ireland); Scandinavian <i>Irmin</i> . The <i>ar</i> -stem in most IE languages.
<i>Aśvin</i> :	Celtic <i>Epona</i> (Gaul); Mcn <i>Iqaja</i> (horse-deity). Gk <i>hippos</i> (Mcn <i>iqo</i> , dialect <i>ikkos</i>), L <i>equus</i> , OE and Ir <i>eoh</i> , Baltic <i>ešva</i> .
<i>Bhaga</i> :	Kassite <i>Bugas</i> ; Slavic <i>Bogu</i> ; Phrygian <i>Bagaios</i> (Zeus, Gk); Gk <i>Phoibos</i> .
<i>Dyaus</i> :	Hittite ^D <i>Siu-s</i> ; Gk <i>Zeus/Dia-</i> ; Roman <i>Ju[s]piter</i> ; Germanic <i>Tîwaz</i> . Lth <i>dievas</i> (usually ‘god’ cognate with S <i>deva</i> , √ <i>dīv</i>).
<i>Indra</i> :	Ht <i>Inar(a)</i> ; Mitanni <i>Indara</i> ; Kassite <i>Indaš</i> ; Celtic <i>Andrasta/Andarta</i> . Gk <i>anēr/andr</i> (?); Av <i>indra</i> (a demon).
<i>Marut-as</i> :	Kassite <i>Maruttaš</i> ; Roman <i>Mars</i> ; Irish <i>Morrighan</i> . The stem <i>mar/mor/mer-</i> etc is common in all IE branches.
<i>Apām Napāt</i> :	Roman <i>Neptunus</i> ; Celtic <i>Nech-tan</i> (Irish). Gk <i>a-nep-sios</i> ; L <i>nep-</i> ; OHG <i>nevo</i> , OE <i>nefa</i> , OLth <i>nep-</i> ; etc (=nephew).
<i>Parjanya</i> :	Slavic <i>Perun</i> ; Baltic <i>Perkunas</i> (and variants); Scand <i>Fjörgyn</i> (- <i>n</i> , Thor’s mother).
<i>R̥bhu</i> :	Gk <i>Orpheus</i> ; Gmc <i>Elf</i> (and variants). Gth <i>arb-aips</i> (?); OSL <i>rabu</i> , R <i>rabota</i> ; L <i>orbu</i> (S <i>arbha</i> , Gk <i>orphanos</i>); etc.
<i>Sūrya</i> :	Kassite <i>Šuriasš</i> ; Gk <i>Hēlios</i> ; Roman <i>Sōl</i> . Gth <i>savil</i> , ON <i>sol</i> , W <i>haul</i> , OSL <i>slunice</i> , Rs <i>solnce</i> , Baltic <i>Saule</i> .
<i>Uṣas</i> :	Gk <i>Ēōs</i> ; Roman <i>Aurora</i> ; Gmc <i>Ēostre</i> . Lth <i>aušra</i> , Ltt <i>ausma</i> , W <i>gwawr</i> , etc.
<i>Varuṇa</i> :	Ht <i>Wurun</i> (?); Mitanni <i>Uruwna</i> ; Gk <i>Ouranos</i> ; Baltic <i>Vēlinas</i> (– and cf <i>jur-</i> = sea). L <i>ūrīna</i> , O N <i>ver</i> (=sea).
<i>Vāstoṣ-pati</i> :	Gk <i>Hestia</i> ; Roman <i>Vesta</i> . Gth <i>wisan</i> ‘to stay’; OHG <i>wist</i> ‘inhabiting’; Toch A/B <i>wašt/ost</i> ‘house’.
<i>Yama</i> :	Sc <i>Ymir</i> . L <i>gemi-nus</i> (=twin); Gk <i>zēmia</i> (=damage), Av <i>yam</i> , <i>Yima</i> .

In the Table the upper line shows the incidence of the deities and the lower shows the cognate stems that occur in languages where the deities are not preserved.

Burrow’s views about Vedic being older and closer to PIE than any other IE branch may be subjective (as W claims) but if W had an objective, or better subjective, view, he would have presented it. The only competitors to Vedic is Hittite and Avestan. Hittite *may* have *some* archaic features but its vocabulary shows a vast influx of Near Eastern elements, its “morphology is remarkably different ... in many crucial features”, one of them being the formation of the Perfect only in a periphrastic form with auxiliaries ‘have’ and ‘be’ (Baldi 1983: 160-1). As for Avestan, mention of the loss of aspirated stops like *gh* and *bh* should be sufficient. The table of the deities’ names tells its own tale. The inner organic coherence of Vedic is self-evident and needs no statistics to be demonstrated: no other IE or non-IE language displays such coherence in *dhātu*, primary and secondary derivatives in nominal and verbal forms. So W’s strictures are closer to prevarication than honesty. Incidentally, Burrow’s *Sanskrit Language* (1973) is still the only definitive study and the author was an ardent invasionist who awaited archaeological evidence for the invasion until his death .

12. Dr Achar sent me a copy of his talk in Montreal (2000) in which he demonstrates, with all appropriate sky-maps, that certain astronomical data in the *Mahābhārata* (V, 81, 6-7; etc) establish the date of the Great War at 3067 – one, which the mathematician-astronomer K S Raghavan had already suggested in 1969. This is only 70 years off the date given by the tradition of the Purāṇas and astronomers (ie, 35 years before the advent of the Kali Yuga at 3102).

Now W objects that post-rigvedic texts like the *AV* and *Brāhmaṇas* mention ‘iron’ and therefore should be dated not before 1100. W’s thinking here is defective since there is no reason why we should accept the references to “iron” (?*śyāma* = ‘black metal’; etc) as valid and the astronomical data as invalid at best, and forgeries at worst! Here again arbitrary and uncertain linguistic considerations are given precedence over other firmer facts. Two considerations nullify this: -

(a) Several objects made of iron have been found in Afghanistan and Baluchistan and some Harappan sites dated from 2600 to 1800 (Possehl & Gullapalli 1999: 159-61). Now the texts merely mention the *śyāma* and *kārṣṇā-krṣṇā-yasa* ‘swarthy metal’ and not the elaborate processes of extracting and smelting this metal. Since there were iron objects from the mid-third millennium, I don’t see why the texts must be assigned to the Iron Age proper, ie after 1100. I have argued that the *AV* and some *Brāhmaṇa* texts would range from c 3000-2500, some early Sūtra texts would belong to c 2600, ie the Mature Harappan phase, while later texts would come later. There is therefore no problem. (W knows all these facts.)

(b) Who can assure us that *śyāma* etc denote iron? In later periods, yes, they do so – but in those early times?... Some scholars have suggested bronze: this may be right, but there is another aspect. It is well-known that to harden copper, the metal is heated up to (but well below melting point) and then left to cool without using water. But this has the effect of also *blackening* copper: this is not soot that can be wiped off. A similar effect is produced through oxidation with the use of various sulphides (Hughes & Row, 1982). Thus the verse *AV* XI, 3, 7 speaks of flesh (*māmsa*) being *śyāma* (=swarthy metal) and blood (*lohita*) being *lohita* (=red copper). Since the ṛṣis probably knew that flesh is produced from and maintained by blood, the correspondence is apt: reddish copper for blood and (processed) black copper for flesh. Again, no problem.

Consequently the astronomical references in these texts can be taken to be what they are and help fix dates, exactly as Achar has done. W protests also about the *Vedāṅga Jyotiṣa*, which Achar fixes at c1800 (*AS* § 6, end), that because of style it should be very late. Here W’s thinking is even more defective and his “end of discussion” (!) is typical of his arrogance. How so, my good Professor? Are we to ignore totally Achar’s finds because of this invocation of “style”? We must then suppose that Achar too is fabricating evidence. Or perhaps the writer(s) of the *Jyotiṣa* cast back somehow and concocted an astronomical situation in order to mislead us. But then why not go even further back to 2500 or 3000?... What W will not consider is that the elaborate charade of mainstream dating of Sanskrit Literature has its foundations in fantasy; as A Aklujkar, Prof of Sanskrit (British Columbia, Canada), wrote, “Only *relative* chronology has been well argued for” (1996: 66). Style can be explained easily: a) Mainstream dates are late because of the general influence of AIT; thus this is no valid argument. (b) Even if the extant text *is late*, it may be only a late edition of an earlier version. The astronomical facts and dates are in no way influenced.

I wonder what excuses will be provided by invasionists in respect of Achar’s finds re the *Mahābhārata*. Even if other astronomical references prove to be of later date these will only indicate that the contextual passages are later accretions that in no way affect the earlier date. Achar’s work will now be as catalytic for indological dates as R Bauval’s was for Egyptology in the early 1990s (see *AS* § 6).

13. My suggestion that the matter AIT/OIT be put to an independent and impartial body of non-indologists is thought by W to be “silly”, a court not “of scholarly discourse but of public opinion”. I did not, of course, suggest “public opinion”. It should be obvious by now to any reader, including W, that my charges against him are very similar to his charges against me. Obviously mainstreamers will take W’s side (or maintain diplomatic silence) while indigenists will support my side. That is why I suggest an independent, non-indological, non-academic panel of reasonable and successful men/women of practical affairs. Be this as it may, W informs us that “public opinion” has not decided “on facts such as that the earth moves round the sun. It came from scholars...” Undoubtedly – eventually. W offers monotonously such unfortunate examples. Let’s look at this one, too.

Undoubtedly we all know that it was a scholar, N Koppernigk (=Copernicus: 1473-1543), who wrote of the Revolutions of the Celestial Spheres (*De Revolutionibus*... 1543), and that 50/60 years later Kepler and Galileo, two equally eminent scholars, supported and developed the Copernican new heliocentric arrangement of our solar system.

Most people know that Galileo was made, by the eminent scholars of the Church, ie the mainstream orthodoxy in Rome, to renounce the heliocentric view *twice*, first in 1615 and then in 1633 (even though by now

the Florentine Inquisition had sanctioned his work); for the remaining 9 years of his life he stayed confined to a villa near Florence. Many people know also that Giordano Bruno, who advocated the Copernican system, the plurality of worlds and the circulation of the blood (ideas that would become common after 50 years), was burned to the stake in Feb 1600 in Rome; but here it should be made clear that more damnatory were his philosophical and religious views which were largely inherited from Marsilio Ficino, Pico della Mirandola et al, of the Renaissance, 150 years earlier: they “stood for the dignity of Man in the sense of liberty, toleration, the right of man to stand up in any country and say what he thought, disregarding all ideological barriers. And Bruno, the Magus, stood for love, as against what the pedants ... had made of Christianity, the religion of love” (Yates 1969: 356).

But in the last 50 years of the 16th century Copernicus’s *De Revolutionibus*... had only one reprint after its publication in 1543 whereas in the same period the books of mainstream orthodoxy (Clavius’s *Treatise*..., Melachthon’s *Doctrine of Physics*, refuting Copernicus, Paucer’s textbook on Astronomy, Ptolemy’s *Almagest* and Peurbach’s *Planetary Theory*) had altogether about 100 reprints (Koestler 1964: 194). Copernicus himself escaped any persecution because he had the sagacity to publish his book even as he was dying (in 1543) and dedicated it to the exceptionally liberal Pope Paul III admitting in the Preface that his ideas might be wrong. Nonetheless, the book was placed on the Index 33 years after it was published.

Earlier than Copernicus, Leonardo Da Vinci, the famous artist, engineer, naturalist etc (1452-1519), stated in his notes that “The sun does not move” (Gombrich, 1995: 294). So this information was known in Italy – unofficially – before Copernicus, who studied in Bologna, Padua and Ferrara 1495-1503. In fact, much much earlier, *Aristarchos of Samos had made the heliocentric discovery in the 3rd century BC, ie 1700 years before Copernicus*, but none of the savants of his day (including the great Archimides), believed it or bothered to verify it and **for the next 18 centuries mainstream orthodoxy was the geocentric view** – established by Apollonius of Perga (also 3rd century BC), developed by Hipparchus of Rhodes and completed by Ptolemy of Alexandria in the 2nd cent CE (hence, the Ptolemaic system).

So much then for the notion that mainstream scholars (here we are talking of hard science verifiable by hard mathematics not some airy-fairy humanistic subject) can always decide a true idea. Eventually, yes, they do. But in the intervening years, something gets in the way so that even if they recognize the truth they quickly cover it up and reject it. This impediment is self-interest in the form of habitual thinking, attachment to opinions and, not least, attachment to position, reputation and income. Such were the motives of the mainstream pundits who were also pillars of the Church: to preserve their own power. Why today all this feverish fight against the indigenists? I don’t believe it is racist as many Indians think, nor any noble motive to keep Indological studies “scholarly” as W claims, though neither need be ruled out altogether. I think it is mainly the most shameful of all motives – to be on top and keep control of others. As soon as a Head of Dept or Professor feels strong (and rich) enough, they launch a Journal (with other people’s money) to promote their own pet theories, acquire kudos, confound opposition and control thinking thus perpetuating their own power and all the advantages this entails. And to this referred E Leach in a quotation I gave fully in AS § 19, very end, which W presented in a truncated form omitting the sentences saying that IE scholars did not scrap their theories because “vested interests and academic posts were involved”. I added “They still are”. And W ought to know because like Leach he has been up to his ears in the game for many years. (Sweeping the dirt under the doormat will not make it disappear.)

Scholars do not get burned now-a-days for their views – though Hitler’s camps and Stalin’s Gulag were very close variants and the victims’ numbers incomparably greater. No, the academic authorities of mainstream orthodoxy simply ostracize anyone who dares dissent. This is to be expected, I suppose. On the other hand, there is a large – and increasing – group of scholars of various grades that now openly defy the Harvard powerful authorities in Indology and the latter do not like this at all. If mainstream indologists were truly “humanist”, that is of a liberal and rational disposition, they would abandon their dictatorial rigidity, which in the end damages sanskritic studies alone, and would allow papers that present reasonably and decorously the indigenist view. As a result all interested scholars would have access to all views, all arguments and data (not just those that are presented through the mainstream filter and colouring) and so make up their minds on their own. A Hitze, for instance, writes in a note to a paper of his (1998: 139, n2) that the only exception to the mainstream AIT “is the view of the anthropologist E Leach that an Aryan immigration would never have happened. However, he admits himself ‘no one is going to believe this’ (Leach 1990, 245)”. Well, Leach of course wrote much more than this and castigated precisely the attitude and method exemplified by Hitze’s presentation, but few, if any, sanskritists would bother to look up this “anthropologist” reference; consequently they would be left only with the distorted impression made by Hitze’s mutilation of Leach’s paper. Hitze also ignores the numerous Indian scholars, academic and not, who have been writing against the AIT for many years and by his silence consigns them to Nirṛti’s lap. W’s EJVS does mention some of these contemporary writers but only to denigrate them (– legitimate?).

E Bryant's study (2001) is a good start for a new chapter showing clearly that modern Indian scholars who advocate the indigenous view are not "revisionists" (as W calls them pejoratively) but writers who (continue to) reassert ideas that were current in 1800-1850 and expressed by Indians continually thereafter. Doubtless, more similar studies will follow. But until now, scholars have been meeting in the relevant Journals only the slanted (defective) views of academics like W, Hitze, Parpola and the like – invested with the sanctity of papal infallibility. And in case these academics wonder, yes, I would allow a dissenting view if I edited a Journal: I am not afraid of truth and I have no position, no reputation and no income to maintain. (The Internet has now thrown a spectacular spanner in the academic works since with its aid all sides can now advertize their views. I am not in favour generally because Indology will be split right down the middle, and nobody knows how long the spirit of tolerance will last, but in this case – three cheers for channels of choice!)

14. In a different context, W thinks me a very bad scholar because I refer to N-W India and Pakistan as 'Saptasindhu' (adj, sing, nom) 'country with 7 rivers' whereas in the *RV* there is reference only to *sapta-sindhavaḥ* (plural) '7 rivers' (*RV* VIII, 24, 27). First, in the *RV* there is no name for that region. Then, Saptasindhu is a perfectly lawful formation and it is shorter than "N-W India and Pakistan" (or more correct than "Panjab"). Moreover, nobody is worried because Greeks call their country *Hellas* while others call it 'Greece' and Germans call theirs *Deutschland* while others call it 'Germany'. Indeed, other scholars use the term: Dandekar 1988: 23; Jha, 1998: 44; Thapar, 2000: 14. Finally, I would not mind calling the place by any reasonable short name.

Otherwise, yes, I am a bad scholar since I don't know the First and Ultimate cause of All nor that organ/power by which I know anything at all and therefore all my knowledge regarding different matters is to this extent defective.

15. Here I end my reply. Some of the other of W's comments may be right (in which case I would readily acknowledge the fact) but I doubt many will be of a very different quality. He distorts the statements of his opponent and tosses about very generously these numerous historical examples that have no relevance. All this seems to me another instance of how scholarship gets corrupted - with falsification, prevarication, sarcasm and scorn. Some of this may be fortuitous as it happens with most of us. But some of it seems to be deliberate. After all Prof Witzel is a sanskritist not an archaeologist, anthropologist or historian. He should accept the archaeologists' and historians' verdicts instead of inventing complex theories (without actual historical parallels) and trying to super-impose them on the other disciplines. Ultimately, it really is no concern of his if some Indians want to reappraise the ancient history of their country. Why does he attack this group of Indians and allies himself to a group of marxists, like the editors of *Frontline*? A Professor of his standing ought to be publishing his scholarly papers in the most prestigious academic Journals (like ABORI), not lend status to such popular magazines. Marxists of this sort have caused untold damage to every country – whether they usurped power (as in Czechoslovakia) or failed to do so (as in Greece). On the contrary, Hinduism has, throughout history and in contrast to Buddhism, Christianity and Mohammedanism, showed itself to be the most tolerant and least belligerent and expansionist of religions. In any event, if these Indians wish to reappraise their history, they have, in our democratic conditions, every right to do so and a Professor of Harvard has no business to insult them but only to disprove and admonish. If his remarks are not heeded, he ought to withdraw with dignity; *for in any event he will not stop them* – and indeed, they have already done it. (No, I am not really offering "gratuitous advice" here: I am only musing aloud, as it were.)

So, I shall continue to maintain - in my musings - that true scholarship can arise only from the Platonic *paideia* and meditation (and the Parmeneidian and Pythagorian antecedents): the rest is "such stuff as dreams are made of". Let me quote once more the hellenist P Kingsley: "in these modern times, what half-heartedly is described as mystical perception is always pushed to the periphery. When it's not denied it's held at arms' length... But what we haven't been told is that a spiritual tradition lies at the very roots of western civilization" (1999: 7). Later he adds: "Our wandering minds are so restless that they keep rushing this way and that, carrying us from theory to theory, from one sophisticated explanation to another. But they don't have the stillness that would ever allow the focus of our awareness to settle for more than the briefest moment on ourselves" (p 250).

I have no taste at all for this kind of coarse and unproductive polemics. If W does, I wish him fortunate fighting. Ultimately, however, he knows quite well that the AIT/OIT will be decided by the facts of astronomical, archaeological (and kindred) finds (and the fact of the Indus script) one way or the other. We can therefore cut out all the claptrap about linguistic theory. So far, Anthropology, Archaeology and (Archaeo-)Astronomy, all three are absolutely against the AIT; even linguistic theory can circumvent it satisfactorily. I personally will not respond at all hereafter, nor write about the AIT unless and until new and very significant

data emerge into the light of day. I am sure Prof Witzel too has other important engagements.

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