

# Petty Professorial Politicking in *The Indo-Aryan Controversy*

## A note on a Harvard professor's assiduous misrepresentation of my position in the Aryan invasion debate

It has taken a few years, but that's not unusual in academic publishing, and the result turns out to be well worth our patience. Edwin F. Bryant and Laurie L. Patton have edited a collection of papers arguing for or against the theory that the Indo-Aryan languages have entered India from outside in the so-called "Aryan invasion": *The Indo-Aryan Controversy. Evidence and Inference in Indian History*, Routledge, New York 2005. The book is a must-read for those who are interested in ancient Indian history, in Indo-European (IE) linguistics as related to the findings of other disciplines, and in a case study of the politics of history. It juxtaposes very divergent viewpoints, ranging from the total confirmation of the predominant Aryan Invasion Theory (AIT) to its total rejection in favour of an Out of India Theory (OIT).

### 1. Stalwart invasionism

One up-to-date instance of the full-fledged AIT developed in this book is by linguist Asko Parpola and archaeologist Christian Carpelan: "The cultural counterparts to Proto-Indo-European, Proto-Uralic and Proto-Aryan. Matching the dispersal and contact patterns in the linguistic and archaeological record", p.107-141. They confirm the widely accepted theory that the IE language family originated in Southwestern Russia and spread from there, with its Indo-Aryan branch penetrating India ca. 1500 BC.

On p.123-125, they propose a shockingly – or refreshingly – detail-happy identification of the PIE-speaking culture and its daughter cultures. Just next to the cradleland of Proto-Uralic, the Lyalovo (5000-3650 BC) culture on the upper Volga, it is the Khvalynsk culture (5000-4500) on the middle Volga that spoke PIE. It was itself the local continuation of the Samara culture (6000-5000), around the present-day city of Samara, which must have spoken an even earlier version of PIE. So now, at any rate, the Urheimat is known with some precision.

From Samara and Khvalynsk onward, the scholars identify some expansions and migrations in the archaeological record. This leads to the secondary joint homeland of all the European branches of IE in the Strednij Stog culture (4500-3350) in Ukraine, whence they follow Proto-Anatolian through the Ezero culture of Bulgaria (3300-2700) on its way to Anatolia, while another branch becomes the Corded Ware culture (ca. 3100) and spreads as far as the Netherlands, differentiating along the way into Proto-Balto-Slavic, Proto-Italo-Celtic and Proto-Germanic, under the impact of various substratum languages. On the eastern side, the Khvalynsk culture was taken to southern Siberia to become the Afanasievo culture by 3600 BC; apparently that's how Tocharian came about. Staying behind in Ukraine for another millennium or so, before they moved on to their respective destinations, the Proto-Greco-Armenians developed the Catacomb Grave culture, while their eastern neighbours in the Poltavka culture became the Proto-Aryans, or more unambiguously, the Proto-Indo-Iranians. The Proto-Iranians retained this region while the Proto-Aryans spread to the north to develop the Abashevo culture before turning southeast on their dramatic migration to the Andronovo culture of Siberia and Central Asia (1800-1500) and thence to India.

We can follow all of them from the one archaeological site to the next, located and dated with precision. And in parallel with this proliferation of IE, we are also treated to the exact genealogy of the branches and individual members of the Uralic family, complete with place and date so that the family astrologer can draw up their horoscopes. Well, that's the old school, not afraid to call a spade a spade, no pussyfooting with "hypotheses" and "probabilities" there. Nothing wrong with that: it is an attempt at the complete knowledge to which every researcher implicitly aspires.

The only question is whether this scenario is indeed the correct one. After all, there is a gap of more than 5000 years between the Samara culture and the first attestation of a known language in the region through written documents. And the migration from Central Asia into India, implied in this theory, has left no traces identifiable as such by B.B. Lal, Jim Shaffer and Diane Lichtenstein, specialized archaeologists contributing to this volume. At the present state of knowledge, and all the more after reading this new collection of contributions to the Aryan debate, I still feel more comfortable with the cautious modesty of Hans H. Hock, who writes about the astronomical evidence in the Vedas that "a few things can be established with certainty, others with a good degree of likelihood, and yet others remain entirely uncertain". (p.297) I believe that this still describes the over-all *status quaestionis*.

Meanwhile, politicians need not feel any of those scruples that restrain researchers. Having received the okay from a duo of top-ranking scholars, the local government of the Samara region may now grab the opportunity and open an Urheimat Theme Park, bringing to life our prehistorical linguistic ancestors in exchange for solid euros and dollars. Complete with fire altars, horse sacrifices, chariot races, bearskin-clad wolf hunters, a festive bridge across the Volga with horse heads on poles, staged thunderstorms featuring the Lightning God, and bouts of soma drinking. They may even concoct a special Aryan soma brew with three distinctively-coloured layers (dark, red and white) to represent the three functions of IE cosmology, a liquid version of the Russian tricolour. Yeah, Mother Russia, mother of all the Aryans!

## 2. A stain on the white shirt

Unfortunately, the aim of the present note is not to evaluate the various contributions to this important book on their scholarly merit and evidential strength. First another account must be settled.

In reading through Harvard Sanskritist Michael Witzel's contribution, "**Indocentrism: autochthonous visions of ancient India**", p.341-404, I noted a rather systematic misrepresentation of positions taken by me in the course of the AIT debate, especially in my book *Update on the Aryan Invasion Debate*, Aditya Prakashan, Delhi 1999. The book is extant in cold print, so everyone can verify for himself that what I have written and what Witzel claims I have written are often very different things. It is also available online at <http://www.voiceofdharma.org/books/ait/>. Let us have a look at the main instances.

## 3. What is an "invasion"?

On p.383 n.50, Witzel alleges: "Elst disingenuously insists on calling any migration or even a 'trickling in' an 'invasion'. However, immigration/trickling in and acculturation are entirely different from a (military) invasion, or from overpowering and/or eradicating the local population."

This is not a reference to my book *Update*, in which I haven't developed this point, but to (possibly earlier versions of) my contribution to the present volume: "**Linguistic aspects of the Aryan non-invasion theory**", p.234-281. It is simply untrue that I have ever called "*any* migration or even a trickling-in an 'invasion'", whether in that paper or even in an informal internet discussion. On the contrary, I have specified *when* a migration deserves to be called an invasion:

"The theory of which we are about to discuss the linguistic evidence, is widely known as the 'Aryan invasion theory' (AIT). I will retain this term even though some scholars object to it, preferring the term 'immigration' to 'invasion'. They argue that the latter term represents a long-abandoned theory of Aryan warrior bands attacking and subjugating the peaceful Indus civilization. This dramatic scenario, popularized by Sir Mortimer Wheeler, had white marauders from the northwest enslave the black aboriginals, so that 'Indra stands accused' of destroying the Harappan civilization." (p.234, incidentally with thanks to Prof. Witzel for pointing out, p.383 n.50, that this destructive imagery was coined not by Wheeler, as angrily repeated by many Indians, but by his native assistant, Prof. V.S. Agrawal)(...)

"Immigration means a movement from one country to another, without the connotation of conquest; invasion, by contrast, implies conquest or at least the intention of conquest. To be sure, invasion is not synonymous with military conquest; it may be that, but it may also be demographic *Unterwanderung*. What makes an immigration into an invasion is not the means used but the end achieved: after an invasion, the former outsiders are not merely *in*, as in an immigration, but they are also *in charge*. If the newcomers end up imposing their (cultural, religious, linguistic) identity rather than adopting the native identity, the result is the same as it would have been in the case of a military conquest, viz. that outsiders have made the country their own, and that natives who remain true to their identity (such as Native Americans in the US) become strangers or second-class citizens in their own country."

"In the case of the hypothetical Aryan invasion, the end result clearly is that North India got aryanized. The language of the Aryans marginalized or replaced all others. In a popular variant of the theory, they even reduced the natives to permanent subjugation through the caste system. So, whether or not there was a destructive Aryan conquest, the result was at any rate the humiliation of native culture and the elimination of the native language in the larger part of India. It is entirely reasonable to call this development an 'invasion' and to speak of the prevalent paradigm as the 'Aryan invasion theory'."

I believe that this observation was impeccable, and still is. If Prof. Witzel sees something "disingenuous" about this, it may well be in the eye of the beholder.

Having demonstrated that an unarmed immigration may still amount to an invasion, I would like to point out that most so-called immigrationists don't even insist on the unarmed nature of their Aryan "immigration". It is still very common to highlight the military advantages of the "Aryan" horse and chariot as a factor in the success of the Aryan settlement of India. On various internet forums, Witzel himself has spoken of the horse and chariot as the *Panzer* (tank) of the Aryans. Scratch a trendy immigrationist and you'll find an old-style invasionist.

#### 4. Beehive India

On p.384-385, n.66: "Elst 1999:159 sq. stresses, like many other autochthonists, that 'India was the best place on earth for food production' and that 'a generous country like India must have had a large population', both unsubstantiated articles of faith. The Indus Valley has only gradually been settled from the Baluchi/Afghani hills, and the Gangetic plain remained very sparsely settled for much longer. (...)"

Prof. Witzel is right in so far as the actual demography of a country should not simply be deduced from basic data of climate and soil fertility. Many parts of Africa have great potential for agriculture and hence for supporting dense populations yet are only thinly populated except by wildlife. So, for all its lush vegetation, India may have been thinly populated at one point. However, in this case we don't have to deduce nor accept "unsubstantiated articles of faith": we know for fact, on the basis of plenty of archaeological evidence, that the Indus basin had hundreds of cities, many of them of the same size of Babylon or larger. Maybe the region had indeed been settled from the Baluchi/Afghani hills in preceding millennia, but by the time period that concerns us in this discussion, the demographic predominance of the Indus Basin was an accomplished fact. If the Aryan invaders had to linguistically convert the Indus people, we know for fact that it was a very large population they had to deal with.

Witzel continues: "For Elst, however, 'the ancient Hindus colonized the world' while India, in reality, by and large, has been a *cul de sac*."

To start with the last part, it is indeed remarkable that for millennia, India seems to have received many more immigrants than it has sent out emigrants. Perhaps we may compare this with the European emigrations in the colonial age: while France had a much larger population than England, it sent out far fewer settlers to its colonies, with fatal consequences for its bid to long-term world leadership. France could offer its people a more pleasant life than England, giving them fewer incentives to leave. Such may also have been India's case, as compared to Central Asia which, though much less populated, sent ever new waves of emigrants as invaders into India.

But then this "quotation" Witzel claims to have read in my book: "The ancient Hindus colonized the world." The context was simply a restatement of the well-known "elite dominance model", quite mainstream:

"And just like a dominant Spanish minority managed to make its own language the mother-tongue of much larger populations which are genetically predominantly Native American, so also the slightly darker emigrants from India may have passed on their language to the white people of Russia and Europe. The view of some chauvinist Hindu writers that 'the ancient Hindus colonized the world' may have a grain of truth in it." (p.160)

The quote which Witzel attributes to me, is in fact my acknowledged restatement of the position of some chauvinistic Hindu writers, explicitly cited as their opinion and not mine, and evaluated as having "maybe a grain of truth", which falls seriously short of giving my approval. The grain of truth is that at an earlier age, India may indeed have been the wellspring of emigrations, with the emigrants benefiting from a civilizational lead over the people they encountered. That false quotation by the Harvard professor is an instance of crass manipulation in order to misrepresent me as one of those Hindu chauvinist eccentrics.

## **5. Proof positive, proof negative**

On p.388 n.106 "Elst 1999:126 sq. points, as 'proof' for his Indian *Urheimat* of IE, to some other asymmetric expansions."

Yes, there are asymmetric expansions of languages. Arabic has spread westwards to Morocco, but not eastwards. It was necessary to point this out against an unspoken tendency

to assume that expansions should be more or less symmetrical, or in other words, that the point of origin should be somewhere in the middle of the actual area of expansion, e.g. the Pontic region in the case of IE. Like Arabic, IE may just as well have spread in one direction starting from one corner, e.g. from India. That observation is not offered as *proof* of anything, only to *disprove* (or at least, to put in doubt) an innocent but prejudiced assumption that may influence some minds and condition them to preferring a theory that puts the homeland somewhere in the middle. The quote marks around the word “proof” are of course misplaced, Witzel is again trying to put words into my mouth. Add to this the second misrepresentation, viz. in the alleged object of this alleged “proof”. It is an insulting misrepresentation of my grasp of elementary logic to describe this modest observation on a single aspect of language history as an attempted “proof” for nothing less than the many-faceted theory of an “Indian *Urheimat* of IE”.

Again, in the Aryan invasion debate it is unfortunately not so rare to hear people make just such mistakes of logic, e.g. finding fault with one particular linguistic statement and thence “deducing” that linguistics is a pseudo-science. But that is their problem, not mine. Witzel wilfully misreads my position in order to lump it with the kind of discourse that isn’t worth replying to.

## 6. Not attested hence non-existent

On p.388, n.110, Witzel says: “Elst (1999) includes a long chapter on links of IE with other language families, with a curious mixture of correct and incorrect data.” Given that this part is largely based on mainstream linguistics publications, the data that Witzel would consider incorrect are really rather fewer than his even-handed division suggests. He actually mentions only two, and I may perhaps concede his first point, viz. that “Ved. *parashu*, ‘axe’, is not from Mesop. *pilakku*, ‘spindle’” though such a link has been accepted by many European academics (e.g. T. Gamkrelidze and V. Ivanov: *Indo-European and the Indo-Europeans*, Mouton de Gruyter, Berlin 1995, p.620) and is by no means an invention of Indian eccentrics.

But as to the second point, I disclaim “the logically/linguistically even more surprising statement that, because Drav. and Munda happen to be *attested later* than Vedic, there is no reason to assume early borrowing from these languages into Ved. (as if these languages did not have their own long prehistory, just as Ved.)!” There is no such position anywhere in my writings on the subject.

Though quite a few people on both sides in this debate have unwittingly fallen for the fallacy that something “didn’t exist because it hasn’t been discovered”, whether in linguistics or in archaeology, I make bold not to be one of them. I have always been aware that languages existed (and therefore may have left traces in other languages) well before being attested in writing, e.g. Prof. Witzel certainly knows that I accept the inferred existence of PIE, whereas some prominent Indian autochthonists reject this construction of a non-attested “ghost language”.

For another example, in the very book criticized by Witzel, I make inferences about the interaction between Indo-Aryan and Dravidian in the preliterate phase of Dravidian. If Dravidian was once spoken in Gujarat, as is commonly accepted, then “Indo-Aryan influence on Dravidian may be much older than usually assumed” (*Update*, p.146), and vice versa: whether Harappan or post-Harappan, at any rate pre-Sangam, i.e. before the actual attestation of any Dravidian language in writing. I also explicitly posited that the first written Tamil already shows the impact of Indo-Aryan, that many items of “the Dravidian core vocabulary as attested in

Sangam Tamil are actually very ancient *tadbhava* (evolved and sometimes unrecognizably changed) loans from Sanskrit or Prakrit” (p.147). Of course Dravidian and likewise Munda had a history, including a history of linguistic contact with Indo-Aryan, well before being committed to writing.

It is a different matter that the Dravidian impact upon Indo-Aryan was indeed remarkably limited as well as slow to appear, not of course because Dravidian was not materially attested yet, but simply because Dravidian formed no part of the linguistic substratum in the Indo-Aryan heartland of Northwestern India. Remarkable at least to those who had assumed that that area had been Dravidian-speaking, as implied by linguists like Walter Fairservis jr. and Asko Parpola who had tried to decipher the Indus inscriptions as a rendering of Dravidian sentences. As I have acknowledged in my contribution to *The Indo-Aryan Controversy* (p.254), Witzel himself has helped a great deal in breaking this Dravidianist spell over the Indus “script” and in recognizing that the large corpus of loans in Sanskrit and Hindi points to non-Dravidian substratum languages such as “language X” and “para-Munda”.

## **7. Return of the chariot-borne Aryans**

On p.372, concerning the difficult match between the archaeological chronology of chariots and the autochthonist chronology of their mention in the Vedic and Avestan record: “This is linguistically and archaeologically impossible, unless one uses the auxiliary, equally unlikely hypothesis that some Indo-Iranians left India and *reimported* the chariot into India (Elst 1999). All such arguments need very special pleading. Occam’s Razor applies.”

No wonder that no exact reference is given, for I have never taken this position. Nor have I ever thought up a reason why the people of the homeland of the chariot, whichever it was, would forget about such a useful technology just because some of their adventurous sons emigrated. That homeland certainly retained the know-how and did not need such a reimportation.

To be sure, return scenarios have taken place in history. The AIT itself implies such a return: one of the currently accepted theories on early human migrations is that all non-African human beings first moved to South Asia and thence spread to Europe, Northeast-Asia (and thence to America) and Australia; so any migrants from Europe to India merely returned to the land their ancestors had left tens of thousands of years earlier. Witzel’s scenario is reminiscent of Bhagwan Gidwani’s fiction book *Return of the Aryans*, which combines OIT and AIT by having the Aryans emigrate from India pre-Harappa and then return post-Harappa just in time for the good old “invasion”. It’s a nice story but it’s not mine.

## **8. Sense and nonsense of linguistic paleontology**

On p.391, n.137: “Elst (1999: 129 sqq.) simply denies the possibility of IE linguistic palaeontology and quotes the always skeptic Zimmer (1990) as his crown witness. However, it is precipitous to dismiss carefully applied linguistic palaeontology completely”.

Not quite. I noted and quoted Stefan Zimmer’s skeptical dismissal of linguistic paleontology with sympathy, because I had just discussed some treacherous pitfalls of this method. That criticism remains valid, not as a wholesale dismissal of the method (which may or may not be Zimmer’s position) but simply as a warning to be careful. It is a matter of

record that very divergent homelands of PIE have been deduced from this method. But if Witzel's readers are made to believe that I am some kind of extremist who cannot criticize one approach except by going to the opposite extreme, they may check for themselves that the chapter in question is actually titled: '**Positive evidence from linguistic palaeontology**' (p.130), featuring a great many classical instances of linguistic palaeontology in action. My contribution to *The Indo-Aryan Controversy* likewise contains a section of applied linguistic palaeontology, viz. ch.7.5: '**Linguistic palaeontology**', p.260-266.

Indeed, Witzel implicitly admits as much by mentioning, on p.141, n.391, my discussion of the application of linguistic palaeontology by T. Gamkrelidze and V. Ivanov in their classic *IE & the IEs*, already mentioned. In support of their Anatolian homeland theory, they have listed a number of lexical items referring to species typical of a warm climate. This breaks the spell of the received wisdom that the flora and fauna attested in the PIE vocabulary necessitate a cold-climate Urheimat. Since the species living in Anatolia generally thrive in India as well, the pro-Anatolian argument may perhaps be adapted into a pro-OIT argument.

Witzel argues briefly that the types of etymological relations involved indicate borrowing and not PIE origin, as I had surmised along with the original authors. Or perhaps he misunderstands the latter's argument, for some of the cases discussed concern words borrowed by PIE itself from languages of countries presumably in contact with Anatolia, so not cases of "borrowing and not of PIE origin" but of "borrowing into the original PIE from Kartvelian, Egyptian, Sumerian or Semitic, and thence PIE ancestral words for the IE languages". He misrepresents one of their key examples, notably Sanskrit *ibha*, "elephant", as corresponding to Greek *elephas* (which would be odd indeed) instead of to Latin *ebur* (*IE & the IEs*, p.443). I assume this mistake was caused not by malice but by writing in a hurry; Witzel's output is impressive in quantity but this may come at a price.

All this is fair enough, but then: "Elst (1999:131), however, concludes from the *same* materials that IE came from a *tropical* area, adding (1999:131-2) a few very unlikely comparisons on his own such as Latin *le-o(n)* from Skt. *rav* 'to howl' (!) – which is in fact IE *\*h3reu(H)*, Grk. *ōromai*, Lat. *rūmor*, demonstrating his lack of linguistic sophistication."

No, that connection with Sanskrit *rav-* was not my own little idea but part of my rendering of Gamkrelidze and Ivanov's argumentation (*IE & the IEs*, p.431). So, the authors of the acclaimed two-volume masterpiece *IE & the IEs* are herewith notified that by Harvard standards, they have demonstrated their "lack of linguistic sophistication".

A classical object of linguistic palaeontology is the IE term for the beech tree. On p.394, n.176, Witzel claims: "Elst (1999:130), while not mentioning historical climate, simply disposes of the *beech* argument wholesale." In fact, I have devoted more than half a page to it, arguing that its presence in the Western branches of IE and absence in the Eastern ones need not be interpreted as showing that the PIE homeland was in the beech-rich moderate zone of Europe. That ready deduction would precisely exemplify the typical "treacherous pitfalls of this method", as claimed above. Rather, that distribution may indicate the borrowing of the word by the early ancestors of the Western branches, then still only little differentiated from one another, upon entering beech-rich territory. I will not bet my life on the correctness of my argumentation, but it certainly is part of the typical and legitimate linguistic-palaeontological debate, not a wholesale dismissal of the discipline.

## 9. The Kassites

On p.380 n.12, Michael Witzel writes about the Indo-Aryan elements attested in West Asia: “However, many wrong data are found with the following authors: Elst (1999:183), (...)” Presumably, one of the allegedly wrong data is identified here: ‘Elst (1999:184) has the [Aryan] Kassites immigrate ‘from Sindh to Southern Mesopotamia’ as a ‘conquering aristocracy’ in a ‘planned invasion’. Actually, the Kassite language family belongs to an altogether unknown language group (Balkan 1954). From what sources did these writers derive their innovative insights?’

I certainly wouldn’t bet my life on my tentative hypotheses about the Kassites, somewhat beyond my field of expertise, but what I wrote is not all that far-fetched. It is a fact that the Kassites conquered parts of Mesopotamia from the East. It is also a fact that their nomenclature included some Indo-Aryan names and terms, just as the Mitannic language did. It is not true, however, that Kassite was an “Aryan” language. Nor is it true that I myself have written such a thing, as Witzel implies by inserting the square-bracketed word “Aryan” before the word “Kassite” in his rendering of my position.

On the contrary, I have written: “Non-invasionists have made much of the presence of Sanskrit names in the Kassite dynasty of Babylon. Yet, the reality revealed by this evidence may be more complicated than is usually assumed. We have information from Semitic Mesopotamians about the Kassite language, and it was not Indo-Aryan. A number of known Kassite words are apparently unrelated to any known language (...) Let the Kassites have spoken a non-IE language. This would be the same situation as in the Dravidian provinces: a non-IE-speaking population maintains its own language but adopts Sanskritic lore and nomenclature.” (*Update*, p.326-327) As to ‘from what sources’ I derived these insights, the main reference is to Wilfred van Soldt: “**Het Kassitisch**”, *Phoenix*, Leiden, 1998, p.90-93, a perfectly respectable mainstream publication.

At this point, I may thank Michael Witzel for pointing out (p.390 n.126), on Bjarte Kaldhol’s authority, that the reading on an Akkadian inscription ca. 2200 BC of surnames like *Arisena* and *Somasena* as Indo-Aryan loans is mistaken, there names being pure Hurrian. Not that I readily accept his criticism as correct, I just don’t know, but it is always good to cast doubt on borrowed opinions. In this case, though Witzel doesn’t bring my name in, I must say I was among those who had accepted these Indo-Aryan etymologies on the authority of Janos Harmatta as quoted by the Indian Communist historian and pro-AIT crusader R.S. Sharma (*Advent of the Aryans in India*, Manohar, Delhi 1999, p.82). I just thought it was safe to believe an establishment historian, especially when he cites data that can be used against the theory he himself upholds. But now the Western pro-AIT crusader has undermined the assurances given by the Indian one.

## 10. Substratum features

On p.391, n.144: “Autochthonists commonly decry the very concept of substrate, see Elst 1999, as this would necessarily indicate that Vedic had not been present in Northwest India since time immemorial.”

It is total nonsense that I have decried or otherwise rejected the concept of substrate. I am aware that some Indian autochthonists have taken that position, just as they have sweepingly rejected other linguistic concepts and methods (an attitude I have described and rejected in *Update*, p.119-120, and in *The Indo-Aryan Controversy*, p.238-239), and what is written here suggests that Witzel’s intention is to paint me with that same extremist brush. In the most charitable reading, “see Elst 1999” may only be referring to me as a reporter on



(rather than a case of) autochthonist anti-substratism. However, given the context, few readers will interpret it that way.

At any rate, there is no indication whatsoever that “decrying the very concept of substrate” is or ever was my own position. In *Update*, I have discussed substratum features on p.133-134, p.143-148 (ch.3.4.6: “Dravidian substratum elements”) and *passim*. In *The Indo-Aryan Controversy*, again, I have discussed substratum features in chapter 7.4, “Loans and substratum features”, p.249-260.

It is also untrue that acceptance of the mere concept of substrate would necessitate a non-Indian origin for Vedic Sanskrit. All kinds of scenarios are possible, such as the absorption of substratum elements during an expansion from a homeland inside India to other parts of India, a large territory by any standard. But it is true that some OIT extremists with little understanding of linguistics do sweep entire concepts off the table simply because they have been used in pro-AIT argumentations. They do indeed assume that “the very concept of substrate would necessarily indicate that Vedic had not been present in Northwest India since time immemorial”, finding an unexpected bedfellow in Professor Witzel.

## 11. “Speculation”

On p.388, n.110: “He may not regard himself as an OIT theorist but he constantly reflects and advocates this attitude in his writings; for example, he has a curious speculation of a Manu who would have led his ‘Indo-Europeans’ upstream on the Ganges toward the Panjab, ending with (p.157): ‘India as a major demographic growth centre from which IE (*sic!*) spread to the north and west and Austronesian to the southeast as far as Polynesia.’ If this is not autochthonist and Indocentric, what is?”

On one point, Witzel is quite right: the said musings were explicitly part of a “speculation”, elaborating on Isidore Dyen’s discovery of uncanny IE -Austronesian lexical commonalities (in I. Dyen & G. Cardona: *Indo-European and Indo-Europeans*, Philadelphia 1970) and on Stephen Oppenheimer’s discovery of civilizational beginnings in “Sundaland”, the land to the east of Vietnam drowned by rising sea levels at the end of the Ice Age (*Eden in the East: the Drowned Continent of Southeast Asia*, London 1998). Some things are certain, others are merely possible but deserve to be explored just to see what much truth there may be in them. This was the context in my own words: “For another alternative: (...) An entry of the Indo-Europeans into India from the east, arriving by boat from Southeast Asia, is an interesting thought experiment, if only to free ourselves from entrenched stereotypes. Why not counter the Western AIT with an Eastern AIT? Just imagine, a wayward Austronesian tribe sailed up the Ganga led by one Manu who, as related in the Puranas, started Aryan history in the mid-Ganga basin (...) This is of course a speculation (...).” (*Update*, p.156)

That Austronesian originated in India was proposed by the legendary linguist Suniti Kumar Chatterjee, a Marxist and by no means a Hindu chauvinist. That IE originated in India is the hypothesis we are exploring here. If there was a connection between the two language families, India could be the common location that made the interaction possible. But I am not attached to an Indian homeland theory, so I have pointed out that Southeast Asia, the more generally accepted homeland of Austronesian, might also be the common location, which implies a southeastern invasion scenario for IE rather than a northwestern one. I am not sure of any of these possibilities, but I like holding them against the light.

On p.384 n.56: “Elst (Ph.D. Leuven, Belgium) typically delights, in his *Update* (1999), in speculating about an Indian *Urheimat* of IE and a subsequent emigration, with ‘Indian’ invasions of Europe, neglecting that linguistic (and other) data speak against it”.

I can be accused of many things, but not of “neglecting” the data that speak against an OIT. I have discussed many of these data, sometimes refuting them, sometimes admitting that they are hard to refute, as the case might be. But I’ll admit that temperamentally, I do take a certain “delight” in exploring theories that go against the established consensus. At the very least, they provide good exercise to the brain, freeing it from entrenched prejudice.

As I put it in the introduction to *Update* (p.x): “The greatest hurdle has been my own anxiety in treading unsure ground, where every hypothesis which is now carrying the day may be blown away by a new discovery tomorrow. (...) But then, I am confident that this painful awareness of uncertainty has been the right attitude and the best starting-point for uprooting the false certainties of some and for clearing the bewilderment of others.”

And in the concluding chapter (p.331, paragraph titled: “Let us keep on doubting”):

“One thing which keeps on astonishing me in the present debate is the complete lack of doubt in both camps. Personally, I don’t think that either theory, of Aryan invasion and of Aryan indigenesness, can claim to have been ‘proven’ by prevalent standards of proof; even though one of the contenders is getting closer. (...) On both sides, I have seen so much self-satisfaction, the conceit of the academic establishment disdaining the contributions of ‘amateurs’, the bad faith among the Indian Marxists dismissing every word uttered by ‘Hindu chauvinists’, the triumphalism among the non-invasionists about having exposed ‘the myth of the Aryan invasion’. Many seem to think that all the questions have been answered, that only mad or evil people can still adhere to the rivalling school of thought, so that there is no need to listen to their objections; but what I see is that at least many parts of the question are still waiting for an answer.”

And some still are.

## 12. Why, oh why?

The thrust of all these misrepresentations is one and the same: to replace reasonable opinions with far-fetched or plainly nonsensical claims. Or in other words: to depict me as some kind of weirdo, fanatic and other ugly things besides.

I could have chosen to ignore this and let it pass, just as Witzel goes on ignoring all the swearwords hurled at him by some Hindu writers on internet forums. But then this is not a mere internet forum but an academic volume coming to us through a prestigious publishing house, and a Harvard professor’s word carries more weight than that of his outsider critics. Further, I have powerful enemies in academe, esp. in the US, and they will gladly exploit any slander they expect to get away with, in this case slander invested with Harvard authoritativeness. They have no scruples about using allegations that they know to be lies if these lies can do the job of harming. If I don’t contradict these lies, they will use that as an extra argument in their innuendo, “and Dr. Elst has never even denied it!”

For a well-established academic at a leading university, safe in his tenure and his creamy salary, approaching the completion of his career, Prof. Witzel’s behaviour seems odd to me. What is he afraid of that he thinks he must stoop to tackling me with these non-academic tricks?

The reason for this unpleasant pattern of falsely attributing silly opinions to me is probably not far to seek. It is the fact that I have exposed a mistake made by Witzel in a crucial part of his pro-AIT argumentation (*Update*, p.164-165). In his paper “**Rgvedic history**” (in G. Erdosy, ed.: *The Indo-Aryans of Ancient South Asia*, Berlin 1995, p.321), he

had mistranslated a verse from the *Baudhayana Shrauta Sutra* (18.44:397.9) to the effect that Ayu's clan "went eastwards" while Amavasus's clan "stayed at home in the west", meaning in Afghanistan or Iran. So, there at long last was the hoped-for Vedic testimony to the Aryan invasion from the west, the "missing link" between Vedic literature and the elusive invasion. Pro-AIT crusaders like R.S. Sharma (*Advent of the Aryans*, p.87-89) have gleefully invoked the Harvard professor's prestige in reproducing his OIT-shattering translation of Baudhayana, "the most explicit statement of an immigration into the Subcontinent" (Witzel p.340, Sharma p.87).

But the translation was wrong. Like the "missing link" between ape and man found in Piltdown, it was a hoax, though presumably a somewhat bonafide hoax. As Prof. George Cardona and other authorities have meanwhile confirmed, the sentence describes how from a middle position (which we can infer to be somewhere in Haryana, India), one clan went east to the Ganga basin and another went west into Afghanistan.

I have never accused Prof. Witzel of deceit or fraud. I prefer to live by Napoleon's dictum: "Never attribute to malice what can be explained through incompetence",-- or in this case, through over-enthusiasm for a long-hoped-for "discovery". When people are very very thirsty, they start to see an oasis on the horizon; no malice intended, just self-delusion. Only, after his innocent mistake had been highlighted, Witzel's reaction was rather unsportsmanlike. He claimed that it was all due to a printing error. That sounds a bit random for such a precise and sensational reading. As if you can put monkeys at a typewriter and let them produce an AIT-friendly translation by coincidence.

What's the big deal about standing corrected once in a while? Thus, in *The Indo-Aryan Controversy* (p.299-301), Hans Hock points out how I have followed (frankly, parroted) P.C. Sengupta's interpretation of an astronomy-related passage confusing *Brahma* (masculine, the god Brahma) and *Brahman* (neutral, the concept of the Absolute, in this context arguably an astronomical concept). OK, I had been careless and made a mistake, but I am glad someone checked it and set the record straight. That's how scholarship advances. Why should a Harvard professor be above this normal course of things? And why should he take his embarrassment out on an unimportant writer like me?

I happen to know that most contributions to *The Indo-Aryan Controversy* were written already a few years ago and only given a quick revision around the turn of 2005. In the intervening years, the atmosphere in this debate has calmed down a little, but in the final years of the second Christian millennium, scolding and shouting and smearing were the done thing on internet forum discussions of the Aryan invasion question. Ironically, most Western AIT champions have managed to come away with the impression that all the foul language was only their Indian opponents' doing, but the record shows that they too have given their best; Witzel's misrepresentation of my position is but a case in point. I will assume that it merely reflects the heated climate of those years, rather than his present attitude, and that during the last-minute revision his busy schedule has caused him to overlook this element in his lengthy footnote apparatus. Having set the record straight, I am now willing and eager to forget the whole episode and focus on the more useful elements in *The Indo-Aryan Controversy*

Dr. Koenraad Elst, 19 October 2005