

1. The official pro-invasionist argument at last

(This chapter is a review of the Aryan invasion arguments in J. Bronkhorst and M.M. Deshpande: *Aryan and Non-Aryan in South Asia*, written immediately after its publication in 1999.)

On October 25-27, 1996, the University of Michigan at Ann Arbor hosted a conference on “Aryan and Non-Aryan in South Asia”. Its proceedings are now available as vol.3 of the *Opera Minora* in the *Harvard Oriental Series*, with some updates and a related more recent paper by Prof. Hans Heinrich Hock added, and edited by Prof. Johannes Bronkhorst and Prof. Madhav M. Deshpande: *Aryan and Non-Aryan in South Asia. Evidence, Interpretation and Ideology*. Some of the papers have but little bearing on the question of the Aryan Invasion Theory, e.g. Pashaura Singh's paper on the 19th-century Hindu reform movement Arya Samaj, or Asko Parpola's paper on a Kerala folk deity. Here we will focus only on the arguments relevant to the Aryan Invasion debate.

1.1. Invasion, the concept

1.1.1. Invasion, not just immigration

To start with a clear understanding about the terminology used, please allow me to explain why I have chosen to retain the term “Aryan invasion” where most contributors to this volume use “Aryan immigration”. Some of them have, in other forums, insisted that I drop the term “invasion” as this represents a long-abandoned theory of Aryan warrior bands attacking and destroying the peaceful Indus civilization. Well, I disagree.

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. Yet invasion should not be confused with military conquest; it may be that, but it may also be demographic *Unterwanderung*. What makes it 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, they are also in charge, just like after a military conquest.

In today's immigration debate, we can vividly see the contrast between the two terms. Those who expect Mexicans in the US to blend in, use the neutral term “immigrant”, even when prefixed with “illegal”. Some people, however, speak of a “Mexican invasion”, by which they mean that the Mexicans, whether “wetback” or legal, have no intention of becoming Americans, of respecting the existing system, but want to impose their identity on Texas or California, making them Spanish-speaking rather than English-speaking states. Likewise, some French opinion leaders, including former president Valéry Giscard d'Estaing and former actress Brigitte Bardot, have spoken of a “Muslim invasion” in France, though most Muslims concerned are perfectly legal “immigrants” who entered France without any violence. What raises alarmist cries of “invasion” is the perception that these North-Africans want to impose a Muslim identity on French society.

It is the end result which decides whether an “immigration” can be called an “invasion”. 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 (like Native Americans in the US) become strangers or second-class citizens in their own country.

In the case of the 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 better part of India. It is therefore entirely reasonable to call such development an “invasion” and to speak of the prevalent paradigm as the "Aryan invasion theory" (AIT).

1.1.2. Was there a military conquest?

There is a hard variety of the AIT, mostly upheld by Dalit activists and Dravidian separatists (and their American Afrocentrist allies) which insists on such a conflictual scenario, with Aryans devastating the cities and civilization of the natives. But most scholars deny or avoid such a scenario, if only because the Harappan cities don't show trace of such military conquest. However, if there was no conquest, the question should be answered: how in the world could the Aryan “immigrants” have aryanized the Harappan area if not by military conquest? Those who are so particular about “immigration” as opposed to “invasion”, like Prof. Michael Witzel, ought to explain how Central-Asian immigrants could impose their language on a far larger and culturally advanced Harappan population?

As far as I can see, the supposedly invading Aryans could only initiate a process of language replacement by a process of elite dominance (that much is accepted by most invasionists), which means that they first had to become the ruling class. Could they have peacefully immigrated and then worked their way up in society, somewhat like the Jews in pre-War Vienna or in New York? Moreover, the example given illustrates a necessary ingredient of peaceful immigration, viz. linguistic adaptation: in spite of earning many positions of honour and influence in society, the Jews never imposed their language like the Aryans supposedly did. So how could these Aryan immigrants first peacefully integrate into Harappan society yet preserve their language and later even impose it on their hospitable host society? Neither their numbers, relative to the very numerous natives, nor their cultural level, as illiterate invaders relative to a literate civilization, gave them much of an edge over the Harappans.

Therefore, the only way for them to wrest power from the natives must have been by their military superiority, tried and tested in the process of an actual conquest. Possibly there were some twists to the conquest scenario, making it more complicated than a simple attack, e.g. some Harappan faction in a civil war may have invited an Aryan mercenary army which, after doing its job, overstayed its welcome and dethroned its employers. But at least some kind of military showdown seems necessarily to have taken place at some point. If invasionists now shy away from Sir Mortimer Wheeler's robust conquest scenario for the Aryan invasion, it is up to them to first of all think up and eventually prove the unlikely non-military alternative. As things now stand, the Aryan “immigration” theory necessarily implies the hypothesis of military conquest.

1.2. The racial interpretation of the AIT

1.2.1. Awareness of colour difference

Two participants have dealt with the old assumption that *Ārya* and *Anārya* are racial categories. Thomas R. Trautmann ("Constructing the racial theory of Indian civilization") acknowledges that ancient Hindus already had an awareness of skin colour, as when Râjashekhara in his *Kâvyamîmânsâ* describes the people of northern India as *gaura*, "fair", those of eastern India as *shyâma*, "dusky", those of southern India as *krsna*, "black", and those of western India as *pandu*, "pale". No colour discrimination is implied there, merely a description of facts, yet a preference for fair complexion in brides is reportedly attested as early as *Vâsistha Dharma Sûtra* 18:18. Most famously, Patanjali the grammarian described Brahmins as *gaura*, "fair". Trautmann asks: "How can one argue against the racial theory in the face of such facts?" (p.289)

First of all, none of these quotations pertain to an opposition between natives and newcomers from the northwest, only one between different sections of the population of India, which even today varies in skin colour from almost white in the northwest to purely black in the south, just as in Europe, hair colour varies between white and black. Long before the 19th-century wave of race theories, Europeans have described one another as red-haired or dark-haired or flaxen-haired, without implying inferiority or foreign origin.

That Brahmins were once recognizable as *gaura* is only logical when you consider that the heartland of the Vedic tradition was to the west of the Yamuna river, and that people in that northwestern region were (and could still be) characterized as *gaura*, this being the skin colour of Panjabis rather than of Europeans. Of course, at that time already Brahmins were swarming out over India, eventually getting invited by kings to settle in Tamil Nadu or Bengal, intermarrying with local women, and ending up as dusky Bengal Brahmins or blackish Tamil Brahmins. Which is why Patanjali's early commentators already rejected his confinement of Brahmins to *gaura*-skinned people as absurd.

The preference for white skin is very widespread among populations which certainly have no Aryan invasion history, e.g. the harems of the Caliphs and Sultans were full of captured white women. More generally, we find white and black having positive c.q. negative connotations among non-white populations, e.g. white is the sacred colour for many Indian tribals, which is why they select white goats or white chicken for sacrifice. Conversely, they do not necessarily have these connotations among white people, e.g. the *gaura* North-Indian Hindus have white as their colour of mourning, as do the whitish Japanese, for whom black is an auspicious colour. At any rate, even the preference of Indians for any one colour need not prove that this was their own colour, much less that they originated in a non-Indian region where everyone wore that colour on his skin.

1.2.2. How the racial interpretation was popularized

Trautmann goes over the historical record of early Indology to show how the race theories of the 19th century forced racial interpretations on text fragments which had never been read in that sense before, e.g. how the single reference to the enemies as *an-âs*, “mouthless” (i.e. “of defective speech”, meaning “not groomed in Vedic culture”, Sayana's reading consistent with the traditional cultural interpretation) was read as *a-nâs*, “noseless”, i.e. “flat-nosed”, by Max Müller, then cited by anthropometrist H.H. Risley as a racial description which the Vedic Aryans often made, and finally adopted in that version by most textbooks. (p.287-288)

Trautmann likewise points out that there is no contextual evidence supporting the nontraditional interpretation of *varṇa*, “colour, caste” as “skin colour”: “On the evidence of use it appears that *varṇa* here simply means ‘category, social group’.” (p.288) Until recently such criticism of the racial interpretation of the ethnic data in the Rg-Veda was only made by the non-invasionist school.

Hans Hock (“Through a glass darkly: modern ‘racial’ interpretations vs. textual and general prehistoric evidence on *ârya* and *dâsa/dasyu* in Vedic society”) also points to the genesis of the racial interpretation in the context of the “scramble of the European powers to divide up the non-European world”, in which “the British take-over of India seemed to provide a perfect parallel to the assumed take-over of prehistoric India by the invading ‘Aryans’” (p.168). He argues that “such notions as ‘race’, defined in terms of skin color, are an invention of (early) modern European colonialism and imperialism and thus inappropriate for the prehistoric contact between *ârya* and *dâsa/dasyu*”, citing as example the absence of racial considerations in the Roman empire. (p.159)

1.2.3. The evidence

Prof. Hock provides a detailed survey of the Vedic verses which have been cited as proof of a racial antagonism between the Vedic people and their enemies (verses containing terms like *asikni* and *krsna*, “black”), and concludes that the racial interpretation “must be considered dubious”. (p.154) He points out that “early Sanskrit literature offers no conclusive evidence for preoccupation with skin color. More than that, some of the greatest Epic heroes and heroines such as Krshna, Draupadi, Arjuna, Nakula and (...) Damayanti are characterized as dark-skinned. Similarly, the famous cave-paintings of Ajanta depict a vast range of skin colors. But in none of these contexts do we find that darker skin color disqualifies a person from being considered good, beautiful, or heroic.” (p.154-155)

About Patanjali's description of Brahmins as *gaura*, he notes that “there is nothing in the passage that forces us to view this difference as one of inherited skin color” (p.155 n.29): it may be the difference between one who lives indoors or sits teaching his pupils in the shadow of a tree, as opposed to labourers or soldiers whose occupation exposes them to the elements. In Europe too, the distinction between the sun-tanned peasant woman and the lily-white lady of the castle was familiar until the recent vogue of sun-bathing.

But as already noted, since the North-Indians have been described by Rajashekhara as *gaura*, the *gaura*-skinned Brahmins do not really stand out by skin colour. Since *gaura* does not strictly mean European-white, we would need something more specific to indicate northern origins. Reference to fair hair would certainly qualify, but according to Michael Witzel, there is in Sanskrit literature exactly “one ‘gold-haired’ (*hiranyakeshin*) person that is not a god, the author of HShS”, i.e. the *Hiranyakeshin-Shrauta-Sûtra* named after him. (p.390, emphasis in the original) Quite possibly, even the author called *Hiranyakeshin* or Gold-Haired was not gold-haired at all, but had one of the epithets of the solar deity Vishnu as his given name, just as people called *Nilakanth*, “blue-throated” like Shiva after he swallowed poison, are not blue-throated at all.

But suppose that we have at last found one Nordic-looking Vedic Aryan here. Considering that fair hair and blue eyes are recessive traits (meaning that in mixed genetic settings, they tend to diminish and disappear) and that fair hair is still common among certain communities in Afghanistan and even in Pakistan among the Kalash Kafir and ex-Kafir populations around Chitral (also known as *Arya-e-Koh*, “Aryans of the mountains”), it stands to reason that before the Christian era, fair hair was not all that uncommon along the Indus and farther northwest. But the Vedic texts make no mention of fair hair as a mark of friend or foe, of ethnic us or them.

Loss of pigmentation seems to be a selected trait of northern latitudes, because it favours the intake of ultraviolet rays needed for the production of vitamin D (in tropical latitudes, by contrast, pigmentation protects against excessive intake of ultraviolet). The neat picture of whiteness and fairness originating in the north is upset somewhat by their common occurrence among the Australian aborigines, but let us still assume it for now. Then, the white and fair people have most likely descended from the north to the subtropical latitudes during the Ice Age, when the northern regions were even more inhospitable. There is no trace of their descending on India after 8000 BC, since when the skeletal record shows the same population living in the Indus basin as lives there today.

Perhaps there was a kind of white penetration of north-western India sometime in the Glacial age, but that immigrant group need not at all have been IE-speaking. Given the known fact that IE has crossed racial frontiers during its expansion, it remains perfectly possible that a darkish group of PIE-speaking Indians moved out to Central Asia and beyond in ca. 5000 BC, mixed with ever-whiter successive groups of natives, imparted their language through elite dominance, but lost their genetic distinctiveness after a sufficient number of generations of expansion and thinning out.

1.3. The political dimension

1.3.1. Racism vs. Hindu nationalism

It may be noted in passing that before arguing for the non-racist interpretation, Prof. Hock apologizes “if some of my findings are superficially convergent with those of the nationalistically inspired groups”. (p.147) At least he has grasped that in this debate, it is the hated Hindu nationalists who take the non-racist position. Indian polemicists like Yoginder

Sikand (“Exploding the Aryan myth”, *Observer of Business and Politics*, 30-10-1993) had tried to amalgamate Hindu nationalism, through the term “Arya(n)”, with “racism”, and many gullible Westerners have fallen prey to this deliberate confusion. Which brings us once more to the inevitable political dimension of the debate.

Most ivory-tower philologists have little understanding of the political aspects of the AIT debate. I will not try to support that statement with examples giving names and references, for enough bad blood has flowed already, so instead I will just indicate a general tendency. Most of them think that in the West, the chapter of Aryan politics has been closed for good in 1945, thus exonerating themselves from a charge still frequently made by Indians, viz. that their acceptance of the AIT is motivated by racism.

In their own case, I will gladly assume that none of them is motivated by racist doctrines, though they do work within a framework which is still indebted, through inertia, to ideas developed in an age when racist or colonial or missionary motives did play a significant role. They also have a personal stake in maintaining the status quo, for their own previous papers would suddenly look obsolete if the AIT were discarded,— and not only AIT-related papers, for all Indological papers contain innocuous chronological references, of the type: “Upanishads (ca. 800 BC)”, which would be far off the mark if the AIT-based low chronology were replaced with something closer to the indigenous high chronology.

Most Western scholars also know little of the contemporary Indian politics concerning the AIT. They generally link the questioning of the AIT to Hindu nationalism, of which they turn out to have a very exaggerated and demonizing image, without noticing that the Hindu-nationalist aversion to the AIT is in fact a reaction to an already old, widespread and well-entrenched political use of the AIT by its believers and propagators: Christian missionaries, Dalit separatists, Dravidian separatists, and to a large extent the Marxists. The latter case requires some explanation.

1.3.2. Two faces of Indian Marxism

Prof. Hock has once, just once, encountered an expression of Dravidian chauvinism in the AIT context (a Tamil professor walking out from a lecture in which it was shown that there is no pre-Vedic Dravidian substratum in Indo-Aryan, as Dravidian influence trickled into Sanskrit only from the later-Vedic stage onwards, p.146), illustrating once more how near-innocent Western scholars are of the intense and widespread political use made of the AIT by various separatist movements in India. But he is quite aware of the polarization on Marxist/anti-Marxist lines in the AIT debate. He was consequently surprised to find “a highly positive review” of anti-invasionist books by David Frawley, N.S. Rajaram, Subhash Kak and Georg Feuerstein “on the neo-Marxian Postcolonial List, in spite of the fact that authors such as Frawley heavily inveigh against the Marxists”. (p.147)

This admittedly unusual phenomenon really only restores the natural order of things: in most Third World countries, Marxists are nationalists to the extent that they want to revalue the contributions of non-European peoples and deconstruct colonialist views, i.c. the AIT. Earlier Marxists like S.K. Chatterjee only accepted the AIT because it was the dominant paradigm and seemed satisfactory enough, and they were by no means as militant about it as

their present-day successors prove to be. Today's Indian Marxists have joined the broad anti-Hindu front, allying themselves with ideological forces of which they used to be as critical as of Hinduism in the days when they were more self-confident.

In the 1950s and 60s, when the Soviet and Chinese models were in the ascendant, Marxists didn't mind offending everyone else. In parallel with Chinese propaganda against Tibetan "feudal society", Dev Raj Chanana could freely expose the numerous social injustices in which Buddhists participated, including the use of slave labour by monasteries and wholesale collaboration with feudal aristocracies and militaristic dictatorships, making a mockery of the pious claims by neo-Buddhists that Buddhism stood for social reform and equality. Today, the demoralized Indian Marxists flatter Buddhism ("revolt against Brahminical oppression") as well as Islam ("egalitarian mass movement against Meccan trade monopolists", later "welcomed by the oppressed masses in India") and all other possible enemies of Hinduism.

Now, these anti-Hindu forces are exploiting the AIT to the hilt, infusing crank racism in vast doses into India's body politic. Read e.g. Kancha Ilaiah's book *Why I Am Not a Hindu* (Calcutta 1996), sponsored by the Rajiv Gandhi Foundation, with its anti-Brahmin cartoons: move the hairlocks of the Brahmin villains from the back of the head to just in front of their ears, and you get exact replicas of the anti-Semitic cartoons from the Nazi paper *Der Stürmer*. This crank Dalit tendency is strongly patronized by the Christian missions, witness the distribution of one of the Bahujan Swayamsevak Sangathan's anti-Hindu pamphlets at the Indian Catholic bishops' Delhi press conference just before the Pope's visit in November 1999. Many of V.T. Rajshekar's brochures (Dalit Sahitya Academy, Bangalore) are transcripts of speeches given at Christian conferences. Like pure Indian Marxism before, this lumpen anti-Brahminism is also well-liked and even patronized by Western academe. Thus, Ilaiah was invited to contribute to the book *Democracy in India, a Hollow Shell* (American University Press) edited by Prof. Arthur Bonner.

Another movement which puts Marxism in the shadow is Islamic militantism. Its rise goes against Marxism's predictions and sympathies, and has taken the lives of thousands of Marxists in Iran and Afghanistan. But it is so undeniably successful, even gaining a lot of sympathy and patronage in Western academe (witness e.g. Asghar Ali Engineer's contribution to Bonner's book, or Syed Shahabuddin's contribution to John Esposito's book *Islam in Asia*) that Marxists have decided: if you can't beat them, join them. It is no surprise, then, to find Marxist professors Irfan Habib and R.S. Sharma pleading the anti-temple case in the employ of the fundamentalist Babri Masjid Action Committee during the Ayodhya controversy.

Against this background, it is not so strange anymore that Indian Marxists have become zealous defenders of a colonial-originated thesis about ethnic movements of four thousand years ago, simply because that thesis is functioning as the war-horse of the united anti-Hindu forces. The greying Indian Marxists are trying to widen their shrinking base by uniting with forces they would once have denounced as obscurantist and populist-retrograde.

But then, not all Indian Marxists have followed their vanguard in taking this new ideological turn. One of the important contributors to the anti-invasionist case is a Marxist, Bhagwan Singh, author of *The Vedic Harappans* (Aditya Prakashan, Delhi 1995), who has predictably focused on the economic and industrial aspects of the Vedic and Harappan evidence. During the past half-century, in most Third World countries, Marxism and nationalism have often gone hand in hand. That is why some Marxists and some RSS activists have recently joined hands in protesting against new footholds of the multinationals in India and other aspects of economic globalization. It should, then, also clear up Prof. Hock's puzzlement at occasional displays of Marxist sympathy for the non-invasionist argument.

1.3.3. Non-invasionism and Hindu nationalism

A wholly different political element in Prof. Hock's contributions concerns his characterization of the non-invasionist school. He repeatedly identifies it as the "Hindu nationalist" school. But this mistakenly attributes a political identity and motive to a scholarly hypothesis about ancient Indian history. I don't call the AIT party "the European racist school" or the "Dravidian chauvinist school" even though those terms do explain the motives behind at least a part of the pro-AIT polemic, past or present.

A number of anti-invasionist authors are not Hindu nationalists. I don't know if Klaus Klostermaier, Georg Feuerstein, François Gautier or Michel Danino considers himself a Hindu, but if so, he would (like David Frawley, a formal convert to Hinduism) definitely not be a Hindu nationalist: a non-Indian Hindu is a living denial of the Hindu nationalist identification of Hinduism with India. To whom it may concern: the present writer, at any rate, is neither a Hindu nor a nationalist. Even among those non-invasionists who are Hindu Indians, there may be some who are not "Hindu nationalists", as Prof. Hock acknowledges of BHU linguistics professor Satya Swarup Misra (p.1).

From the identification of a historical theory with a much-maligned political movement, worse things follow: "But given the nature of the politics of authenticity and of its practitioners, I doubt that a meaningful dialogue with any of the nationalistically motivated groups is feasible, just as I doubt that any meaningful dialogue is possible between evolutionists and creationists." (p.147) It is to Prof. Hock's credit that he acknowledges the existence of Tamil-chauvinist and tribal-separatist "nationalistically motivated groups" alongside the alleged "Hindu nationalists" as interested parties in the Aryan invasion debate. After Western academics have been effectively supporting the first two while demonizing the third one, it is perhaps a welcome progress that he puts them indiscriminately in the same bag: "In fact, given the unfortunate consequences in my native country, Germany, of a nationalism that sought authenticity in non-scholarly interpretations of history and prehistory, I am extremely uncomfortable with any of the modern Indian nationalisms briefly referred to earlier." (p.148)

But what evidence does Prof. Hock have for the "non-scholarly interpretation of history" by the non-invasionist school? It is not given, but it ought to be strong stuff, for he goes as far as to exclude all the "nationalistically motivated groups" from the history debate by equating them with Biblical-fundamentalist creationists. The one similarity he cites is that scholarly disagreements between different invasionists (e.g. identifying the Harappan language as Dravidian vs. Munda vs. Burushaski etc.) are exploited by non-invasionists to discredit the whole invasionist paradigm: "Creationists and various types of nationalists look upon such disagreements as indictments of scholarly methodology; but such disagreements are the very foundation of scholarly enquiry." (p.147)

Well, I haven't seen that polemical tactic in most non-invasionist publications. Probably the reference is to Prof. N.S. Rajaram, who has very substantial scientific contributions to the debate to his credit, but who once pointed out the controversies between linguists as an extra argument for not taking the soft evidence of linguistics too seriously. I repeat that I (along with Hindu authors like S.S. Misra and Shrikant Talageri) have always disagreed with the Indian skepticism vis-à-vis the linguistic evidence, yet here, Rajaram has a point.

If invasionist linguists still disagree on the IE Homeland by a margin of 5,000 km, from northern Germany (Lothar Kilian, Jean Haudry) through Anatolia (Thomas Gamkrelidze & Vyacheslav Ivanov, along with archaeologist Colin Renfrew) and South Russia (Gordon Childe, Marija Gimbutas and most *Journal of IE Studies* contributors) to Xinjiang (A.K. Narain), it is not unreasonable to question the capacity of the linguistic data to yield decisive information on the location of the IE Homeland. Indeed, Prof. Hock himself accepts that pinpointing the exact location in this vast stretch of land is a question which “may, in fact, never be settled”. (p.17) But if it is too early to exclude any part of this territory from possible Homeland status, is it so crazy to suggest that the exclusion of India may have been premature as well? In this case, the plurality of opinions does prove that there is as yet no solid evidence basis for deciding the Homeland question, which does not preclude that decisive evidence will be found in the near future. And which does not amount to asserting that faith and faith alone should decide the Homeland question.

1.3.4. Why there is so little genuine debate

In various discussion forums, I have repeatedly seen the same pattern of Aryan invasion debate. Invasionists, supposedly sobre scholars with no axe to grind, attack non-invasionists as religious obscurantists, flat-earthers, “lunatic fringe”, Atlantis freaks, Nazis (apparently unaware that Hitler was an AIT believer), creationists, and what not: terms they would never use among each other. Even more than most people, invasionist academics prove to be *mimophants*: like the touch-me-not mimosa, they are extremely sensitive,-- at least when they themselves are criticized; and like the elephant, they are extremely insensitive,-- at least when it comes to throwing insults at others.

Then, when someone draws attention to their unusual *ad hominem* language, so unbecoming of scholarly company, they blame the non-invasionists: with such “Hindu nationalist” freaks, what else do you expect? Though quite untrue, that analysis is not really mendacious, merely prejudiced: Western Indologists are so conditioned by a wild enemy-image of Hindu nationalism that they genuinely associate anyone rightly or wrongly counted as a Hindu nationalist with trouble and fury. So, when they see a debate degenerate into a slanging-match under their own impact, they wrongly yet sincerely attribute this to the input of the non-invasionists. At best, they pick up the quarrel in the middle, muse about how “the AIT debate doesn't bring out the best in people”, and decide that it is best to suspend the debate.

Let me put on record here that in my 9 years of close involvement in this debate, I have seen time and again that it is the invasionist school which, when it did not refuse the debate, has spoiled the debate by replacing argument with mud-slinging. There are exceptions, of course, and the publication of the volume under discussion is a great step in the right direction.

[Postscript, November 2006: This was written seven years ago, and I am sorry to note that in recent years, a few internet cowboys from peninsular India have given the AIT

champions tit for tat. While their foul language and false allegations against some leading US-based academics (including Madhav Deshpande, co-editor of the volume under discussion) have been bad enough, the worst about their interventions was the misrepresentation of their opponents' views and of the whole tradition of AIT-supporting scholarship, obviously stemming from their own total ignorance about the science of historical and comparative linguistics. Intelligent people are aware of the limits to their own knowledge, but duffers can crow around without ever doubting their own infallibility. In the process, by sheer association they do great damage to the position of anyone else who questions the AIT orthodoxy.]

1.4. The Saraswati river

1.4.1. Which Saraswati ?

The frequent Rg-Vedic references to the Saraswati river are seen by both sides as a key to the solution of the Aryan question. Non-invasionists have pointed out that the biggest concentration of Harappan cities was along the Saraswati river, and that it nearly dried up synchronously with the decline of Harappan city culture. Therefore, the Rg-Veda cannot be post-Harappan, though it may be pre-Harappan.

To this, Prof. Hock argues that there is a lot of ambiguity about this name: Saraswati can be the goddess of learning (a personification derived from the river's status as the feeder of the heartland of civilization?), so the number of references to the river may be smaller. Moreover, even as a river name, it has two referents: the Indian Saraswati river and the Afghan river known in Iranian as Harahvaiti, now as Helmand. Clearly, we are witnessing the result of a typically colonial process of name transfer by colonists (cfr. from "Paris, France" to "Paris, Texas"). Question is: did Indians bring the name to Afghanistan, or did Afghans bring it to India?

The last alternative is a favourite among invasionists. They suggest Afghanistan-based Aryan explorers reached that river in India and gave it the name of their familiar river in the old country. Some of them, e.g. Rajesh Kochar ("Learning about India's past", *The Hindu*, 7-11-1999), practically ignore the Indian Saraswati and assert that Afghanistan was the real setting of Rg-Vedic history, so that Rg-Vedic Saraswati references are to the Harahvaiti. Most, however, accept the contextually obvious location of the Rg-Vedic Saraswati in Haryana-Panjab-Rajasthan after the Aryan invasion, and place the Afghan origin of the Saraswati name in the pre-Vedic and pre-invasion past. But most of these scholars fail to make the obvious deduction from the Vedic references to this dried-up Indian river, viz. that it places the Rg-Veda in the Harappan or pre-Harappan period, as the drying of the Saraswati was connected with the decline of the Harappan cities.

From the etymology of the name, the location of the river cannot be deduced: “the river name [Saraswati] must mean ‘provided with (many) ponds’. This describes the Iranian and Panjabi Sarasvatî much better: both rivers end in the desert, in a series of meandering branches, with lakes and ponds. The etymology is clear (*saras*, IE *selos*, no connection with *sar*, ‘run, move speedily’ < IE **sar*)”. (Michael Witzel, p.376) While the ending in the desert has little to do with this etymology, and may not have applied in the case of the ancient Saraswati, the “series of meandering branches with lakes and ponds” seems to be common to both the rivers. This similarity at once explains why Indo-Iranian settlers in Afghanistan chose the Saraswati from among the rivers they remembered as the one to impart its name to the Helmand.

1.4.2. The Helmand in the Rg-Veda ?

Prof. Hock thinks that while “Saraswati” in the Rg-Veda mostly refers to the Indian river, in some instances it may be the Afghan river. Firstly, RV 7:95:2 describes the Saraswati as flowing “from the mountain to the sea”. While seemingly trivial, since all rivers ultimately carry their water to the sea, this innocuous phrase becomes an important argument, if not against the AIT, at least against the identification of the Helmand as the Vedic Saraswati, because the Helmand, exceptional among rivers, does not carry water to the sea but to a lake on the Iranian plateau. To this, Hock replies that *samudra*, “sea”, may well also mean a “lake”, such as the *Hâmûn-i-Helmand* lake into which the Helmand flows. (p.165) True, it is not uncommon for terms for “lake” and “sea” to overlap, cfr. German *die/das See*, or the West-Germanic word *meer*, Dutch for “lake” but German for “sea”. But is there any indication that this is the case of *samudra* in Vedic Sanskrit? Even if so, all that is proven is that the Afghan identification of “Saraswati” is a possibility, not that it is likely or contextually warranted, let alone certain.

While conceding that the Saraswati is described as the most divine among the rivers and other superlatives in RV 2:41:16, Hock reminds us that the Sindhu is also glorified in superlatives in RV 8:26:18. The 8th book of the Rg-Veda is the most northwesterly book, the one which mentions Afghan flora and fauna (8:5, 8:46, 8:56). From that perspective, the Sindhu is the greatest nearby river, even in the heyday of the Saraswati which was at any rate far more to the east, beyond even the five main tributaries (*Panj-âb*) of the Indus. But the 8th book is younger than the family books (2 to 7), which are unambiguously located in India and near the Indian Saraswati. If the Sindhu becomes more prominent than the Saraswati at some point, this amounts to a movement from east to west, from Panjab to the frontier (Indus) to Afghanistan. Incidentally, the superlatives for the Saraswati in RV 2:41:16 are an unlikely description of a relative backwater like the Helmand except for absolute provincials who had never seen the nearby Oxus or Indus.

Hock also points out that the Sindhu is once described as the mother of the Saraswati (RV 7:36:6), in a verse about rivers “coming together longingly”. Indus and Saraswati both flowed into the Arabian Sea, more or less forming a common delta. It seems that before drying up, the Saraswati had changed its course and flowed into the Indus at a more northerly location. At any rate, in the case of the Indian Saraswati, this imagery of the Sindhu being its

mother and coming together with it makes sense. That is not the case for the Helmand, which forms a separate basin from that of the Indus and does not flow into the same sea.

Finally, Prof. Hock has found a possible reference to the Helmand outside the Rg-Veda: “Identification of at least some of the Vedic occurrences of the river name Sarasvatî with the Iranian Helmand/Harahvatî may further help explain a puzzle in the Vedic tradition which, to my knowledge, has so far resisted explanation. A passage in the Vâjasaneyi-Samhitâ (34:11) states that five rivers empty into the Sarasvatî, contrary to what is known about the Sarasvatî of Kurukshetra. As it turns out, according to some of the more detailed maps that I have seen, this is exactly the number of major tributaries of the Helmand/Harahvatî.” (p.166)

The number of tributaries is a sometimes changing and at any rate very relative matter: it depends on what size of tributary you consider big enough for mention on the map you are making. I have a Bartholomew map of South Asia which shows seven permanent tributaries to the Helmand plus some seasonal rivers; *The Times Atlas of the World's* mini edition shows three. But if we accept this count, and if we accept that the Vâjasaneyi-Samhitâ is really referring to the Afghan Helmand, and if we remember that most of the Rg-Veda with its numerous mentions of the Indian Saraswati is older than the Vâjasaneyi-Samhitâ, then that would indicate an India-to-Afghanistan movement. Here again, and contrary to what the AIT predicts, we do not find the Afghan references in the earliest parts of the Veda, but in the later parts.

1.4.3. When did the Satlej change course ?

Prof. Michael Witzel has thought up an original objection concerning the Saraswati, which supposedly dried up when tectonic movements redirected some of its tributaries elsewhere: “The Beas, however, is mentioned in the somewhat older hymn 3:33 [i.e. older than the river hymn RV 10:75], together with the confluence of Satlej and Beas. This will provide, incidentally, a date *ad quem* for this part of the RV, once the relevant geological and geographical data have been confirmed (and it speaks against the current revisionist fashion of assigning a pre-Harappan date to the RV).” (p.371)

Prof. Witzel is referring to the hypothesis that the Satlej, which now joins the Beas in southwestern Panjab to jointly flow into the Indus, originally flowed into the Saraswati, and that the Satlej's changing course was responsible for the drying up of the Saraswati. That a reference to the confluence of Satlej and Beas would turn this change of course into a *terminus post quem* for the third book of the Rg-Veda, would indeed seem logical. But things may be more complicated.

While the shifting course of the Satlej may have played a role at some point, Prof. Witzel is mistaken if he thinks that that is where non-invasionists have sought the key to the decline of Harappa. N.S. Rajaram and David Frawley have noted that “it appears that the Yamuna stopped flowing into the Saraswati at an earlier era some centuries before 1900 BCE, perhaps before 2300 BCE, which is what current archaeology suggests. As this westward flowing Yamuna would have made up most of the waters of the Drshadvati, which is

identified with this region, it suggests that much of the Rg-Veda reflects a period when the Yamuna flowed west, which places it yet earlier into the Harappan or Pre-Harappan era.” (*Vedic Aryans and the Origins of Civilization*, Voice of India, Delhi 1997, p.96)

Later, the Yamuna slightly changed course, joined the Chambal and started throwing its waters into the Ganga at Prayag, which had already been the confluence of Chambal and Ganga for long,-- meaning that the lower course of the Chambal, now joined to the Yamuna, was henceforth called Yamuna. Something similar must have happened to the Satlej. This was the name of a river joining the Beas, or possibly the lower course of the Saraswati (near present Bahawalpur), but a change of course at that point would make little difference to Vedic and Harappan history, because it was downstream from the Vedic Kurukshetra region and from the greatest concentration of Harappan cities along the Saraswati, so it made no difference to them. So, the Satlej may have started flowing into the Beas before RV 3:33 was composed, without adverse effect on the Harappan cities upstream. What is more, the Satlej may (and as we shall see, must) always have joined the Beas, yet may have replenished the Saraswati with its waters at the same time. How can that be?

The Satlej gets its water from several tributaries, and can definitely continue to exist and join the Beas even at a time when one of its tributaries goes missing. The tributaries did not adapt their course to that of other tributaries, but simply obeyed the law of gravity and flowed from higher to lower. So, their waters already flowed more or less where they flow now, joining each other to form a single river, now called Satlej, and ultimately meeting the Beas at some point. In the densely populated region where Beas and Satlej meet, both rivers obviously had a name, and there is no reason why the Satlej was not already called the Satlej (or Shutudri) back then. Later, in its upper course, it was joined by a new tributary, a river originating near Mount Kailash in Tibet and originally flowing into the upper course of the Saraswati. When you follow a river upstream, you come across confluences where it is a matter of choice which tributary to consider as the river itself, and which one as a mere tributary. In this case, when the Satlej was enriched with a new and mighty tributary, the Kailash-originated former tributary of the Saraswati, the tendency may have caught on to continue the name Shutudri/Satlej upstream along that river rather than its original source.

The place where the mountain river which now counts as the upper course of the Satlej once joined the Saraswati has reportedly been identified: “Satlej too, owing to tectonic movements, took a sharp westward right-angled diversion and this is evident from the sudden widening of the Ghaggar paleochannel south to Patiala.” (V. Prabhu Kumari: “Antiquity of the Vedic river Saraswati”, in Bhu Dev Sharma, ed.: *Revisiting Indus-Saraswati Age and Ancient India*, WAVES 1998, p.120) At any rate, even if this change of course took place in post-Rg-Vedic times, the Beas must have been joined already before that by a Satlej river consisting of its other tributaries.

This issue will only be definitively decided by serious geological research, and possibly the technical answer is already available in some geology journal. Meanwhile, it is remarkable how scholars who fail to acknowledge the main implication of the Saraswati's centrality in the Rg-Veda, viz. that the Rg-Veda cannot be post-Harappan (the decline of Harappa being connected with the drying of the Saraswati), can make so much of a related but minor issue.

1.5. Inconclusive types of evidence

1.5.1. Archaeological evidence

In a joint paper, “Migration, philology and South Asian archaeology”, two of the participating archaeologists, Jim Shaffer and Diane Lichtenstein, confirm and elaborate their by now well-known finding that there is absolutely no archaeological indication of an Aryan immigration into northwestern India during or after the decline of the Harappan city culture. It is odd that the other contributors pay so little attention to this categorical finding, so at odds with the expectations of the AIT orthodoxy.

The absence of archaeological evidence for the AIT is also admitted, with erudite reference to numerous recent excavations and handy explanations of the types of evidence recognized in archaeology, by outspoken invasionist Shereen Ratnagar in her paper: “Does archaeology hold the answers?” It then becomes her job to explain why the absence of material testimony of such an invasion need not rule out the possibility that an invasion took place nonetheless. Thus, there are parallel cases of known yet archaeologically unidentifiable invasions, e.g. the Goths in late-imperial Rome (p.222) or the Akkadians in southern Mesopotamia (p.223). So, in archaeology even more than elsewhere, we should not make too much of an *argumentum e silentio*.

To quote her own conclusion: “We have found that the nature of material residues and the units of analysis in archaeology do not match or fit the phenomenon we wish to investigate, viz. Aryan migrations. The problem is exacerbated by the strong possibility that simultaneous with migrations out of Eurasia there were expansions out of established centres by metallurgists/prospectors. Last, when we investigate pastoral land use in the Eurasian steppe, we can make informed inferences about the nature of Aryan emigration thence, which is a kind of movement very unlikely to have had artefactual correlates.” (p.234)

Agreed, but this agnostic position does at least cut both ways: if there is no evidence for a migration, then migration theories for either direction are without proof. Obviously, if the Aryan invasion does not stand disproven by the absence of definite archaeological pointers, then neither does an Aryan emigration from India. However, there is one difference. Because several generations of archaeologists have been taught the AIT, they have in their evaluation of new evidence tried to match it with the AIT; in this, they have failed so far. However, it is not at all certain that they have explored the possibility of matching the new findings with the reverse migration scenario. At least psychologically, they must have been much less predisposed to noticing possible connections between the data and an out-of-India migration than the reverse. Perhaps evidence of an Aryan emigration was staring the archaeologists in the face but wasn't recognized as such because they weren't intellectually ready for it? Therefore, a scrutiny of the evidence in the light of an alternative hypothesis is called for.

We must thank Prof. Ratnagar for her authoritative hint at one of the possible scenarios of out-of-India aryanization: “expansions out of established centres by metallurgists/prospectors”. An economic metropolis has a tendency to establish trading outposts (cfr. ancient Greek outposts as far as the Crimea peninsula or Massilia/Marseilles), settlement colonies (cfr. Carthage founded by Phoenicians, and then Carthagenia in Spain founded by the Carthaginians, or the Greek settlement colonies in Sicilia), military outposts to protect trading routes, mining and production outposts. Therefore, it is completely logical to suspect pre-Harappan and Harappan expansion into Central Asia. This may have been how Aryan expansion out of India started.

A half-serious aside: in her acknowledgments (p.234), Prof. Ratnagar thanks her colleagues Deshpande, Trautmann, Witzel, Hock and Bronkhorst for their “ethics and honesty, rare amongst scholars working on the Indian past”. So there you have it from the horse's mouth: a JNU professor daily surrounded by Indian Marxist historians flies out to attend a conference in Ann Arbor with US-based scholars, and is pleasantly surprised to find scholarly “ethics and honesty” at last.

1.5.2. The horse evidence

In his paper “Out of India? The linguistic evidence”, Hans Hock gives the two arguments which have, all through the 1990s, kept me from giving my unqualified support to what he calls the “out of India” theory. These are the dialectal distribution of the branches of the IE language family (see below) and the sparse presence of horses in Harappan culture. About the horse, he says very little, but summarizes the problem very well: “no archaeological evidence from Harappan India has been presented that would indicate anything comparable to the cultural and religious significance of the horse (...) which can be observed in the traditions of the early IE peoples, including the Vedic Aryas. On balance, then, the ‘equine’ evidence at this point is more compatible with migration into India than with outward migration.” (p.13)

Let us first get one thing straight: a number of horses have been identified in the bone record of Harappan and pre-Harappan sites, a few even to the southeast of the Harappan area, in India's interior. Against Prof. Michael Witzel's curt and sweeping claim that “horses were introduced from Central Asia only ca. 1700 BC” and that “all reported earlier finds are hemiones (half-asses)” (p.353), it may be noted that international experts have certified the earlier existence of true horse in India. Thus, the precise identification of equid remains in Surkotada has been conducted by Hungarian expert Prof. Sandor Bokonyi: “The occurrence of true horse (*Equus Caballus* L.) was evidenced by the enamel pattern of the upper and lower cheek and teeth and by the size and form of incisors and phalanges (toe bones).” (quoted by Prof. B.B. Lal from Bokonyi's letter to the Director of the Archaeological Survey of India, 13-12-1993, in *New Light on the Indus Civilization*, Aryan Books, Delhi 1998, p.111; Lal took the trouble of quoting Bokonyi precisely because the latter's expertise had falsely been cited in favour of the opposite view, viz. that the horses found were really hemiones.)

Lal mentions other finds in Rupnagar, Kalibangan, Lothal, Mohenjo-Daro, and terracotta images of the horse from Mohenjo-Daro and Nausharo. Many bones of the related onager or half-ass have also been found, and one should not discount the possibility that in some contexts, the term *ashva* could refer to either species.

Nevertheless, all this is still a bit meagre to fulfil the expectation of a prominent place for the horse in an “Aryan” culture. I agree with the “out of India” school that such paucity of horse testimony is explainable (cfr. the absence of camel and cow depictions, animals well-known to the Harappans, in contrast with the popularity of the bull motif, though cows are usually more numerous than the concomitant bulls), but their case would be better served by

more positive evidence. On the other hand, the evidence is not absolutely damaging to an Aryan Harappa hypothesis, both outcomes remain possible.

1.5.3. The linguistic horse

The word **ekw-o-s*, "horse", is a later formation in PIE. The oldest vocabulary had athematic stems (e.g. Latin *lex* from *leg-s*), the thematic stems (e.g. Latin *corv-u-s*, "raven") belong to a later generation of PIE words. This can be taken as an (admittedly very small) indication that the horse was not part of the scenery in the PIE homeland. To be sure, there are many newer-type formations for age-old items, e.g. the species *lup-u-s*, "wolf", was most certainly known to the first PIE-speakers. But in the present case, another argument for the late origin of *ekw-o-s* has been added, viz. its somewhat irregular development in the different branches of IE, e.g. the appearance from nowhere of the aspiration in Greek *hippos*.

If this is not really a compelling argument, at least the converse is even more true: the clinching linguistic evidence for a horse-friendly Urheimat is missing. We should now count with the possibility that the Proto-Indo-Europeans only familiarized themselves with the horse towards the time of their dispersion. A possible scenario: during some political or economic crisis, adventurers from overpopulated India speaking dialects of PIE settled in Central Asia where they acquainted themselves with the horse. More than the local natives, they were experienced at domesticating animals (even the elephant, judging from RV 9:47:3 which mentions an elephant decorated for a pageant), and they domesticated the horse. While communicating some specimens back to the homeland, they used the new skill to speed up their expansion westward, where their dialects became the European branches of the IE family.

The horse became the prized import for the Indian elite, which at once explains both its rarity in the bone record and its exaltation in the Vedic literary record. The rumour of new and undreamed-of possibilities in horse-rich Central Asia attracted many more fortune-seekers from India: "Go northwest, young man!" Which in turn explains the dialectal diversity of the PIE expanders and their large numbers which proved sufficient for the linguistic conquest of the natives of Central Asia and Europe. Just a possibility...

1.6. Linguistic evidence

1.6.1. From PIE to Sanskrit

Most linguistic arguments given here by Prof. Hock and Prof. Witzel cause no serious problem to the theory that the IE family originated in India.

Prof. Hock updates the old arguments against the fairly popular Indian (and late-18th-century European) belief that Sanskrit is the mother of all IE languages. Thus, you can explain Skt. *jagâma* from PIE **gwegwoma* as a palatalization of the initial velar (before *e/i*) followed by the conflation of *a/e/o* to *a*, implying that the kentum forms and the forms with differentiated vowels as attested in Greek and other IE languages represent the original situation, while the Sanskrit and other Indo-Iranian forms represent an innovation. This means that Sanskrit is not PIE, that it has considerably evolved after separating from the ancestor-languages of the other branches of IE. I entirely agree with those arguments, and expect the same of most readers, so I need not elaborate on them here.

Hock addresses this part of his paper to the Sanskrit-as-PIE argument of Satya Swarup Misra (*The Aryan Problem, a Linguistic Approach*, Delhi 1992), practically the only non-invasionist who has discussed the linguistic evidence in some detail. While Misra's book contains valuable points, his equating Sanskrit with PIE is untenable and would probably make him the butt of ridicule if his work were better known in the West. Hock's polite and technical handling of the argument, otherwise a normal matter in scholarly debate, deserves some praise in the present case of the AIT controversy.

Note that Misra is one example of a scholar who originally believed in the AIT but developed doubts: his Ph.D. thesis on (meaning *against*) the laryngeal theory in the 1960s entirely accepted the AIT and the low chronology (1200 BC) for the Vedas. Likewise among archaeologists, a number of the present critics of the AIT used to be believers in the AIT, and this includes top-ranking experts like Prof. S.R. Rao and Prof. B.B. Lal. I am not aware that the AIT can boast of such converts. But I agree that winning converts is not a criterion to decide questions of history.

1.6.2. Dialectal distribution of the IE proto-languages

Even when I learned about findings which indicate that something is wrong with the AIT, one nagging doubt which has always kept me from simply declaring the AIT wrong was the geographical distribution of the branches of the IE family. This argument has been developed in some detail by Prof. Hock, who shows his mastery by skipping obsolete arguments like glottochronology and linguistic palaeontology (still brought up by too many scholars in this debate) and going straight to this crucial point. He explains that “the early Indo-European languages exhibit linguistic alignments which cannot be captured by a tree diagram, but which require a dialectological approach that maps out a set of intersecting ‘isoglosses’ which define areas with shared features (...) While there may be disagreements on some of the details, Indo-Europeanists agree that these relationships reflect a stage at which the different Indo-European languages were still just dialects of the ancestral language and as such interacted with each other in the same way as the dialects of modern languages.” (p.13)

Isoglosses, linguistic changes which are common to several languages, indicate either that the change was imparted by one language to its sisters, or that the languages have jointly inherited it from a common ancestor-language. Within the IE family, we find isoglosses in languages which take or took geographically neighbouring positions, e.g. in a straight Greece-to-India belt, the Greek, Armenian, Iranian and some Indo-Aryan languages, we see the shift $s > h$ (e.g. Latin *septem* corresponding to Greek *hepta*, Iranian *hafta*). In the same group, plus the remaining Indo-Aryan languages, we see the "preterital augment" (Greek *e-phere*, Sanskrit *a-bharat*, "he/she/it carried"). Does this mean that the said languages formed a single branch after the disintegration of PIE unity for some time, before fragmenting into the presently distinct languages?

No, for this group is itself divided by separate developments which the member languages have in common with non-member languages. Best known is the kantum/satem divide: Greek belongs to the Kentum group, along with Italic, Celtic, Germanic, Anatolian and Tocharian, while Armenian and Indo-Iranian share with Baltic and Slavic the Satem isogloss (as well as the "ruki rule", changing s to sh after r, u, k, i). So, like between the dialects of any modern language, the IE languages share one isogloss with this neighbour, another isogloss with another neighbour, who in turn shares isoglosses with yet other neighbours.

The key concept in Hock's argument is *neighbour*: the remarkable phenomenon which should ultimately support the AIT is that isoglosses are shared by neighbouring branches of IE. Thus, the Kentum languages form a continuous belt from Anatolia through southern to western and northern Europe, and the Satem isogloss likewise covers a continuous territory (only later fragmented by the intrusion of Turkic) from central Europe to India. To be sure, there are serious exceptions here, e.g. there are Kentum languages far removed from Europe, viz. Tocharian in Xinjiang and proto-Bangani in the western Himalaya; and there is a later satemizing tendency within the Kentum group, viz. in the Romance languages (none of which pronounces its word derived from Latin *centum* with a k sound), Swedish and English (where *wicca* became *witch*). But we get the idea, especially after studying the map which Prof. Hock provides in Figure 2 on p.15. There, we see ten isoglosses in their distribution over the geographically placed IE language groups, all showing the geographical contiguity of languages sharing an isogloss.

Why is this important? "What is interesting, and significant for present purposes, is the close correspondence between the dialectological arrangement in Figure 2 (based on the evidence of shared innovations) and the actual geographical arrangement of the Indo-European languages in their earliest attested stages. (...) the relative positions of the dialects can be mapped straightforwardly into the actual geographical arrangement if (...) the relative positions were generally maintained as the languages fanned out over larger territory." (p.16) In other words: the geographical distribution of IE languages which actually exists happens to be the one which would, at the stage when the proto-languages were dialects of PIE, be best able to produce the actual distribution of isoglosses over the languages.

1.6.3. Dialectal distribution: compatible with Indian Homeland?

So, the relative location of the ancestor-languages in the PIE homeland was about the same as their location at the dawn of history. This, Hock proposes, is compatible with a non-Indian homeland. Thus, if the Homeland was in the North-Caspian region, the dialect communities spread out radially, with the northwestern proto-Germanic tribe moving further northwest through what is now Poland, the northern proto-Baltic tribe moving further north through Belarus, the western proto-Celtic tribe moving further west through Slovakia, likewise the Italic tribe through Hungary, the southwestern proto-Greek and proto-Albanian tribes moving further southwest through the Balkans, the southeastern proto-Indo-Iranians moving further southeast through Kazakhstan, etc. (One reason given by the early Indo-Europeanists for assuming such radial expansion is that there is little inter-borrowing between IE language groups, indicating little mutual contact. However, plenty of Iranian loans are found in Slavic, Celtic loans are found in Germanic, etc.) This way, while the distances grew bigger, the relative location of the daughters of PIE vis-à-vis one another remained the same.

If this is a bit too neat to match the well-known twists and turns of history, it is at least more likely than an Indocentric variant of Hock's scenario would be: "To be able to account for these dialectological relationships, the 'Out-of-India' approach would have to assume, first, that these relationships reflect a stage of dialectal diversity in a Proto-Indo-European ancestor language located within India. While this assumption is not in itself improbable, it has consequences which, to put it mildly, border on the improbable and certainly would violate basic principles of simplicity. What would have to be assumed is that the various Indo-European languages moved out of India in such a manner that they maintained their relative position to each other during and after the migration. However, given the bottle-neck nature of the route(s) out of India, it would be immensely difficult to do so." (p.16-17, emphasis Hock's)

I believe there is a plausible and entirely logical alternative. It remains possible that the isoglosses match a twofold scenario, part area effect and part genealogical tree, as follows. In part, they reflect successive migrations from the heartland where new linguistic trends developed and affected only the dialects staying behind or developing there later (vide e.g. T. Gamkrelidze and V. Ivanov's outlining in their magnum opus, *Indo-European and the Indo-Europeans*, Berlin 1995, the successive waves of emigration from Homeland X, leading to groupings like Celtic and Italic, or Germanic and Balto-Slavic). Thus, PIE in its Homeland was a kentum language, and its first emigrants retained the Kentum form: Anatolian (the oldest judging from its retaining the laryngeals), Tocharian, Celtic, Italic, Germanic. Later emigrants developed Satem features: Baltic, Slavic. Along with the stay-behind Homeland language, Indo-Aryan, the last emigrants had been completely satemized: Armenian, Iranian.

The second part is that the isoglosses not explainable by the former scenario are post-PIE area effects, which is why they affect historically neighbouring languages. Archaeologists (mostly assuming a North-Caspian homeland) have said that the North-Central-European Corded Ware culture of ca. 3000 BC was a kind of secondary Homeland from which the Western branches of PIE spread, again more or less radially, to their respective historical locations. Be that as it may, that or a similar culture may well have comprised a juxtaposition of IE-speaking communities before their further dispersal, living in close proximity to the next (though not to all), close enough to allow for the transmission of linguistic innovations.

Hock himself unwittingly gives at least one example which doesn't easily admit of a different explanation: "The same group of dialects [Germanic, Baltic, Slavic] also has merged the genitive and ablative cases into a single 'genitive' case. But within the group, Germanic and Old Prussian agree on generalizing the old genitive form (...) while Lithu-Latvian and Slavic favor the old ablative". (p.14) Clearly, Old Prussian and Lithu-Latvian lived in close proximity and separate from Germanic and Slavic for centuries, as dialects of proto-Baltic, else they wouldn't have jointly developed into the Baltic group, distinct in many lexical and

grammatical features from its neighbours. So, if the Baltic language bordering on the Germanic territory happens to share the Germanic form, while the languages bordering on Slavic happen to share the Slavic form, we are clearly faced with an area effect and not a heirloom from PIE days. The conflation of cases or case endings has continued to take place in many IE languages in the historical period, so the example under consideration may well date to long after the fragmentation of PIE.

A second example mentioned by Hock may be the split within the Anatolian group, with Luwian retaining a distinction between velar and palatal but Hittite merging the two, just like its Greek neighbour. But not knowing that corner of the IE spectrum very well, I will not press the point.

As far as I can see from Prof. Hock's presentation, the twofold scenario outlined above is compatible with all the linguistic developments mentioned by him. The one difficult case is Greek, which shares a number of innovations with Indo-Iranian, yet has also missed out on others just like its Western neighbours (non-merged *a/e/o* vowels, Kentum). Perhaps Greek was late to leave yet had retained its Kentum forms even when surrounded by increasingly satemized dialects, just as the Indian-but-Kentum language proto-Bangani seems to have managed until some time within living memory. Some dialects just happen to be more conservative than others, e.g. Greek is usually reckoned as the most conservative regarding the PIE vowels, more faithful to the old vowel distinctions than any of its neighbours at any time.

I leave it to more technically inclined linguists to look into this more closely. For now, I must confess that after reading Prof. Hock's presentation, the linguistic problem which I have always considered the most damaging to an Indocentric hypothesis, doesn't look all that threatening anymore. I do not believe that the isoglosses discussed by him necessitate the near-identity of the geographical distribution of the PIE dialects with the geographical distribution of their present-day daughter languages, which near-identity would indeed be hard to reconcile with an out-of-India hypothesis. Maybe other linguists, or a challenged Prof. Hock, could sharpen this line of argument and make it tougher to reconcile the distribution of isoglosses with an Indian homeland hypothesis.

1.6.4. Exit Dravidian Harappa

To Prof. Michael Witzel's recent work on the non-IE substratum discernible in Indo-Aryan, I hope to return for a fuller discussion worthy of this important new synthesis of the substratum evidence. It may be noted that in his contribution to the volume under consideration ("Aryan and non-Aryan names in Vedic India. Data for the linguistic situation, c. 1900-500 BC"), Witzel tries in passing to counter the AIT skeptics but formally does no more than work within the AIT paradigm, assuming it as a framework for his investigation. He is untroubled by anomalies which he himself mentions in passing, such as the apparent unfamiliarity with rice cultivation in "the first post-Indus text, the Rg-Veda" eventhough rice was first farmed in the northwest "during the late Indus period". (p.353) He unconvincingly interprets *Parshu*, the Iranian-related Vedic ethnonym of a "northwestern tribe" which is obviously related to "Persian" (the tribe which gave its name to the central Iranian province

Pars, now *Fars*), as the etymon of “Pashtu”, which he must do because the AIT is hardly compatible with a historical presence in (and emigration from) India of the Persians. (p.342)

For now, a few remarks. The most important thesis posited here, almost off-hand and unexplicitated, is: there is not a shred of evidence for the identification of Harappan culture as Dravidian. Like Vedic culture, the oldest glimpsable Dravidian culture was centred on transhumant herding. (p.349) Judging from the substratum of place-names, they once were located along the northwestern coast (Sindh, Gujarat, Maharashtra), while southern India clearly had a pre-Dravidian population including the Vedda people, which has lost its language but retained its ethnic identity in Sri Lanka. Witzel quotes David McAlpin as dating the settlement of the Dravidians from the west in the southern reaches of the Harappan civilization at as late as the 3rd millennium BC. It would seem that from there, some of them (the ones least affected by Harappan influence) moved south and easily assimilated the less advanced natives into their culture, while those staying on in the Harappan sphere of influence were assimilated into the dominant Indo-Aryan culture, bringing a few Dravidian elements into the Gujarati language (e.g. the double “we”, inclusive and exclusive) and Gujarati culture (e.g. the Dravidian system of kinship). (p.385)

Like the Gutians and Akkadians in Sumeria, the Dravidians settling in Gujarat may have been outsiders attracted by the economic opportunities offered by a rising metropolitan culture. Witzel acknowledges the “linguistic connections of Dravidian with Uralic” (p.349), which also point to the northwest outside India as the origin of Dravidian. While non-invasionists probably smile at this painful blow to the Tamil chauvinist view of Aryan barbarians invading the native Indo-Dravidian civilization, they should be the first to recognize the possibility of alternative explanations for the Dravidian-Uralic connection, though speculations on that topic cannot detain us here.

The oldest layers of Vedic literature do not contain loans from Dravidian, not even hydronyms. Dravidian loans appear only gradually in the next stages and are typically terms used in commercial exchanges, indicating adstratum rather than substratum influence. This, we may remark, fits neatly with a pre-Harappan date for the Rg-Veda, when the Dravidians shepherds were still at some distance, while an early-Harappan influx of the immigrant Dravidian language could make its mark in the later Vedic literature.

At any rate, Dravidian seems now to have been eliminated from the shortlist of pretenders to the status of Harappan high language. That Dravidian cannot be the main “pre-Aryan” influence on Vedic language and culture had earlier also been argued by F.B.J. Kuiper, Bernard Sergent and others. Prof. Asko Parpola's well-known decipherment of the Indus script as proto-Dravidian doesn't prove its own starting-point, and may turn out to be no more than an imaginative though admittedly masterly groping in the dark.

1.6.5. Pre-IE substratum in Indo-Aryan: language X

Unlike Dravidian, other languages have exerted an influence since the earliest Vedic times: chiefly a language exhibiting Austro-Asiatic features, hence provisionally called para-Munda, not the mother but at least an aunt of the Munda languages still spoken in Chotanagpur; and an unknown language with nonetheless consistent features, provisionally

called Language X. Non-invasionists strongly dislike the seeming fondness of Western linguists for “ghost languages”, but the simple fact remains that numerous languages have died out, and that the ghost of some of them can be seen at work in anomalous elements in existing languages. Thus, the first Sumerologists noticed an un-Sumerian presence of remnants of an older language typified by reduplicated final syllables, hence baptized “banana language”. Today, much more is known about a pre-Sumerian Ubaidic culture, which has become considerably less ghostly.

Witzel quotes Colin Masica as crediting language X with 31% of agricultural flora terms in Hindi. (p.339) I would caution, with Shrikant Talageri, against prematurely deciding on the non-IE origin of a word not having parallels in other IE languages, especially in the case of terms for indigenous flora and fauna. When Indian emigrants stayed for a few generations in the rather different climate and landscape of Central Asia, they would logically lose that term (or sometimes transfer it to similar-looking species), so its absence in the non-Indian branches of IE would prove little. For the rest, there is no objection to the impression that Vedic Sanskrit has absorbed some foreign words, especially after the emigration of the other branches of IE, so that these could not share in this process of borrowing.

The assumption of a language X in northwestern India will be welcomed by many as the solution to the vexing question of the origin of retroflexion in the Indian languages. Weak in Burushaski and Munda, strong yet defective (never in initial position) in Dravidian, strong in Indo-Aryan but unattested among its non-Indian sister-languages, retroflexion in its origins is a puzzling phenomenon. So, language X as the putative language of the influential Harappan metropolis might neatly fit an invasionist scenario for the genesis of retroflexion in Indo-Aryan as well as its spread to all corners of India. But an entirely internal origination of retroflexion within early Indo-Aryan (which then imparted it to its neighbours) has always had its defenders even among linguists working within the invasionist paradigm. No new hard data on this undoubtedly important question have been presented in this volume, so for now, we may let the matter rest.

6.6. How to decide on the foreign origin of a word ?

Linguists like Kuiper and now Witzel (in more detail in his paper “Substrata in Old Indo-Aryan” in the *Electronic Journal for Vedic Studies*, 1999) have proposed a clear-cut criterion for deciding whether a word attested in ancient Sanskrit is IE or not. They argue that a word in a given language cannot take just any shape, e.g. a true English word cannot start with *shl-*, *shm-*, *sht-* (though these German sound patterns are now becoming familiar to English-speakers through loans like *schnitzel*, often Yiddish words and names like *schmuck*, *schlemiel*, *Wasserstein*). Likewise, a Sanskrit word cannot contain certain sound combinations, which would mark a word as a foreign loan.

However, there are several problems with this rule. Firstly, and invasionists should welcome this one, if a sound is too strange, chances are that people will change it to something more manageable, resulting in a loan which differs in pronunciation from its original form, but which is no longer recognizable as a loan by the present criterion precisely because it has conformed itself to the more usual sound patterns of the receiving language.

Thus, in Sino-English (Hong Kong etc.), a boss or upper-class person is called a *taiban*, Chinese for “big boss”; there is nothing decisively un-English about this string of consonants and vowels. The one feature of this Chinese word which could have marked it as un-English, is its tones (*tai* fourth tone, *ban* third tone),-- but precisely that typically foreign feature has been eliminated from the English usage of the word. The same is true in Japanese, which has adopted hundreds of Chinese words after stripping them of tones and other distinctively Chinese phonetic characteristics. Likewise, Arabic has a number of sounds and phonemic distinctions unknown in European languages, which are systematically eliminated in the Arabic loans in these languages.

So, if a word looks Sanskritic, it may still be of foreign origin. With historical languages, better known than proto-para-Munda or “language X”, the assimilation into Sanskrit sound patterns is well-attested, e.g. Greek *dekanos* becoming *drekkaṇa*, Altaic *Turuk* becoming *Turuṣka*, Arabic *sultan* becoming *suratrāṇa*, etc. Sometimes this phonetic adaptation gives rise to folk-etymological reinterpretation, often with hypercorrect modification of the word; and the latter can also take place even without etymological interpretation, just for reasons of “sounding right”. Thus, it is often said (also here by Witzel, p.358) that *Yavana*, vaguely “West-Asian”, is a hypersanskritic back-formation on *Yona*, “Ionia”, i.e. the Asian part of Greece (ethnically cleansed of Greeks by Turkey in 1922). While I am not entirely convinced in the case of *Yavana*, I do accept that this principle underlies the Sanskrit looks of many foreign loans in Sanskrit.

Witzel uses this phenomenon to explain the Sanskrit looks of 35 river names: “Even a brief look at this list indicates that in northern India, by and large, only Sanskritic river names seem to survive”. (p.370) Quoting Hans-Jürgen Pinnow, he notes that over 90% don't just look IA but “are etymologically clear and generally have a meaning” in Indo-Aryan. He attributes this unexpectedly large etymological transparency to “the ever-increasing process of changing older names by popular etymology”. Note, however, that in some cases he is open to surprising IE etymologies for names usually explained as loans, e.g. *Sindhu* might be an “Indo-Iranian coinage with the meaning ‘border river, ocean’ and fits Paul Thieme's etymology from the IE root **sidh*, ‘to divide’”. (p.387)

Meanwhile, numerous words have wrongly or at least prematurely been classified as foreign loans on the basis of the said criterion. The point is: how do you decide what the standard shape of a word in a given language should be? Witzel calls *Bekanâta* “certainly a non-IA name” (p.364) citing as reason the retroflex *ṭ* (though in Vedic, the dental/retroflex distinction is sometimes merely allophonic, representing a single but phonetically unstable phoneme) and the initial *b-*. While *b-* may be rare in Old IA, there is no good reason to exclude it altogether from the acceptable native sounds of the language. What threatens to happen here, is that the minority gets elbowed out by the majority, that the majoritarian forms are imposed as the normative and only permissible forms.

Compare with the argument by Alexander Lehrman about accepting or excluding the rare sequence “e + consonant” as a possibly legitimate root in Hittite: “There is absolutely no reason why a lexical root of Proto-Indo-European (or Proto-Indo-Hittite) cannot have the shape **eC-*, except the wilful imposition by the researching scholar of the inferred structure of a majority of lexical roots on a minority of them.” (“Hitt. *ga-ne-esh-*+ and the Laryngeal Theory”, *Indogermanische Forschungen*, 1997, p.151, emphasis mine) The same openness to exceptions to the statistical rule is verifiable in other languages, e.g. Chinese family names are, as a rule, monosyllabic (the *Mao* in *Mao Zedong*), yet two-syllable names have also existed, though now fallen in disuse (the *Sima* in *Sima Qian*). As a rule, Semitic verbal roots have a “skeleton” of three consonants, yet a few with two or four consonants also exist. But admittedly, both examples also illustrate a tendency of the exception to disappear in favour of (or to conform itself to) the majoritarian form.

Another point is that there may be a covert *petitio principii* at work here. Many assertions on what can or cannot be done in Indo-Aryan are based on the assumption that Vedic Sanskrit is more or less the mother of the whole IA group, it being the language of the entry point whence the Aryan tribes populated a large part of India. Thus, Witzel is sure that *Kosala* must be a loan (from Tibeto-Burman) because the sequence *-os-* is “not allowed in Sanskrit”. (p.382) But first of all, what is more ordinary in dialectal variation than an *s/sh* shift? In the present Hindi dialects of “Kosala” and “Videha” (eastern Uttar Pradesh, Bihar) and in Bengali, Sanskrit *sh* routinely becomes *s* (*Subhâsh* > *Subhâs*, *Ghosh* > *Ghose*): this could be a substratum influence, following Witzel, or it could simply be a spontaneous dialectal variation.

What is more, *s* in such cases, and likewise other sound combinations deemed “not allowed in Sanskrit”, could also be the original form, in a dialect which did not descend from Sanskrit. Indeed, in Shrikant Talageri's reconstruction of ancient Indian history, Sanskrit is not the mother of IA at all, there being IA dialects developing alongside Vedic Sanskrit in India's interior. Just as Vedic tradition is but one among several Indo-Aryan religious traditions, the traces of which are found in the Puranas, Vedic Sanskrit is but one among a number of OIA dialects. The progeny of the latter consists of the Indo-Aryan languages, especially their so-called *deshî* vocabulary, words seemingly unrelated to Sanskrit, being neither *tatsama* (pure Sanskrit “citation” words) nor *tadbhava* (evolved words having a Sanskrit correlate). Thus, the deshi word *kuta*, “dog”, is used in most NIA languages, as opposed to the Sanskrit word *shvan* (cfr. Greek *kuon*, English *hound*), used only in Singhalese and Konkani. As Talageri has pointed out, the latter two languages both have a tradition of originating in the northwest, not far from the Vedic heartland; they also use *tadbhava* words for “water” and “horse” whereas most other NIA languages use deshi words (*pânî* c.q. *ghora*).

Rather than to say that these deshi words are non-IE loans, they may often be native IA coinages which just happen to be different from usage in the northwestern dialect which became Vedic Sanskrit, and which was in some respects closer to Iranian than to the IA dialects of India's interior. Likewise, these inner-Indian IA dialects may have had phonological characteristics different from those of Sanskrit and permitting certain combinations and sounds patterns “not allowed in Sanskrit”. By Witzel's reasoning, the name *Ghose* would have to be Tibeto-Burman or otherwise non-IE, while we know that it is Bengali and quite IE. This alternative possibility lessens the necessity of a non-IE origin for odd-looking words.

1.6.7. Pre-IE substratum in Indo-Aryan: “para-Munda”

Since remnants of extinct language isolates like the hypothetical language X don't tell us much except that they existed, the more interesting substratum influence is the one attributed to Austro-Asiatic, more specifically to its mainland Indian branch, Munda. In Witzel's view, this seems to be the main influence, reaching far northwest to and beyond the entry point of the Vedic Aryans in India, and definitely predominant in the whole Ganga basin.

The word *Gangâ* itself has long been given an Austro-Asiatic etymology, esp. linking it with southern Chinese *kang/kiang/jiang*, supposedly also an Austro-Asiatic loan. The latter

etymology has recently been abandoned, with the pertinent proto-Austro-Asiatic root being reconstructed as **krang* and the Chinese word having a separate Sino-Tibetan origin (vide Zhang Hongming: “Chinese etyma for river”, *Journal of Chinese Linguistics*, Berkeley, Jan. 1998, p.1-47). Witzel now proposes to explain *Gangâ* as “a folk etymology for Munda **gand*” (p.388), meaning “river”, a general meaning it still has in some IA languages. The folk etymology would be a reduplication of the root **gam/ga*, “moving-moving”, “swiftly flowing”.

In some cases, a Munda etymology is supported by archaeological evidence. Rice cultivation was developed in Southeast Asia, land of origin of the Austro-Asiatic people, who brought it to the Indus region by the late Harappan age. Therefore, it is not far-fetched to derive Sanskrit *vrihi* from proto-Munda **vari*, which exists in practically the same form in Austronesian languages like Malagasy and Dayak, and reappears even in Japanese (*uru-chi*), again pointing to Southeast-Asia as the origin and propagator in all directions of the both the cultivation of rice and its name **vari*. With para-Munda prefix, this may also be the origin of the Hindi word *câ-val*, “rice”.

For Hindu nationalists, and for serious historians in general, all this goes to confirm that the Munda tribals are not “aboriginals” (as Christian missionaries have called them in an effort to set them against their Indo-Aryan Hindu neighbours), but carriers and importers of Southeast-Asian culture. Witzel himself acknowledges that “Munda speakers immigrated”, as this should explain why in Colin Masica's list of agricultural loans in Hindi (which, in conformity with the invasionist paradigm, is very generous in allotting non-IE origins to Indo-Aryan words), Austro-Asiatic etymologies account for only 5.7%. In borrowing Munda words, the Vedic Aryans clearly did not behave like immigrants into Munda-speaking territory. This paucity of Munda influence in the agricultural vocabulary, soil-related par excellence, should also caution us against reading Munda etymologies into the equally soil-bound hydronyms, e.g. there is no compelling reason for a Munda etymology for *Shatudri* (Witzel diagnoses the usual Sanskrit interpretations as artificial “popular etymology”, p.374, but does not produce a convincing Munda alternative) or *Gangâ*, even when it is not impossible either.

The main pointer to a Munda connection seems to be a list of prefixes, now no longer productive in the Munda languages, and not recognized or used as prefixes by Vedic Sanskrit speakers. Thus, the initial syllable of the ethnonym *Ki-râta* seems to be one in a series of non-IA and probably para-Munda prefixes *ka/ke/ki/kr* (p.365). On this basis, all words beginning with *k-* become suspected loans from “para-Munda”, e.g. one might decide the question “which was first?” between Sanskrit *karpâsa*, “cotton”, and Munda *kapas*: the latter was first, signifying one of the textile manufacturing processes pioneered by the Southeast-Asians, and the former, with its typically Sanskrit-looking cluster *-rp-*, is but a hypersanskritized loan. This is not impossible, of course, but not quite proven either.

An interesting idea suggested by Prof. Witzel concerns an alleged alternation *k/zero*, e.g. in the Greek rendering of the place-name and ethnonym *Kamboja* (eastern Afghanistan) as *Ambautai*, apparently based on a native pronunciation without *k-*. Citing Kuiper and others, Witzel asserts that “an interchange *k : zero* ‘points in the direction of Munda’” (p.362), though this “would be rather surprising at this extreme western location”. Indeed, it would mean that not just Indo-Aryan but also other branches of Indo-Iranian have been influenced by Munda, for *Kam-boja* seems to be an Iranian word, the latter part being the de-aspirated Iranian equivalent of Skt. *bhoja*, “king” (Eric Pirart: “Historicité des forces du mal dans la Rgvedasamhitâ”, *Journal Asiatique* 286.2, 1998, p.542; he also gives an Iranian etymology to Vedic *Agastya*, from *a-gasti*, from Iranian *gasta*, “ill-smelling, sin”). Well, if the Mundas could penetrate India as far as the Indus, they could reach Kamboja too.

But the interesting point here is that the “interchange *k : zero*” is attested in IE vocabulary far to the west of India and Afghanistan, e.g. *ape* corresponding to Greek *kepos*, Sanskrit *kapi*, “monkey”, or Latin *aper*, “boar”, corresponding to Greek *kapros*. Gamkrelidze and Ivanov have tried to explain this through a Semitic connection, with the phonological closeness (somewhere in the throat) of *qof* and ‘*ayn*. But if the origin of this alternation must be sought in a Munda connection, what does that say about the geographical origin of Latin and Greek?

1.6.8. Munda and the AIT

Given Prof. Witzel's known invasionist presuppositions, we may suspect a pro-Munda bias in his discussion of Vedic words with unclear etymology. However, given the location of the different language groups in India, it is entirely reasonable that Munda influence should appear in the easternmost branch of IE, viz. Indo-Aryan. If both IE and Munda were native to India, we might expect Munda influence in the whole IE family (though India is a big place with room for non-neighbouring languages), but since Munda is an immigrant language, we should not be surprised to find it influencing only the stay-behind Indo-Aryan branch of IE. This merely indicates a relative chronology: first Indo-Aryan separated from the other branches of IE, and then it came in contact with Munda.

So, if we provisionally accept the presence of Munda loans in Vedic Sanskrit, we need not follow Witzel in accepting that this is a native substratum influence in a superimposed foreign language. Shrikant Talageri (*The Rg-Veda, a Historical Analysis*, forthcoming from Aditya Prakashan, Delhi 2000) has argued that the Rg-Veda shows a movement from the western Ganga basin to the Saraswati and Indus basins. This implies that as an eastern dialect of PIE (with the western dialects in the Indus basin soon to move out to central Asia and beyond, and to its east only other IA dialects), pre-Vedic and Vedic Sanskrit were more open to influences from the eastern immigrants from Southeast Asia, the Mundas.

Therefore, the recognition of Munda (or likewise of Dravidian) influence on Indo-Aryan is not really a problem for an Indian homeland theory. Vedic Sanskrit is attested only since well after the fragmentation of PIE. If it shows features including a vocabulary borrowed from other Indian languages, this doesn't prove that Indo-Aryan split off from the other IE languages in Central Asia, but may indicate influences which reached northwestern India only after the other PIE dialects had moved out to Central Asia.

1.7. Conclusion

From this all too brief survey of the most AIT-related arguments in the Bronkhorst & Deshpande volume, we may conclude that the Aryan invasion debate has really taken off at last. In spite of very minor traces of theoretical or political bias, the contributing scholars have really addressed the issue, and have really made an effort to present non-invasionists with material challenging the Indian homeland paradigm. The time for wailing about the colonial-missionary bias underlying the invasionist paradigm is definitively past, for this paradigm is

now being given an articulate scholarly defence increasingly based on first-hand knowledge of the rivalling Indocentric model. In some respects, especially the linguistic angle, the debate is also being taken to a very technical level, to which non-invasionists will have to get used while continuing to develop their own arguments.

But so far, any decisive evidence in favour of the AIT has not been given. Or if I have missed something, I invite readers to point out which fact has been presented in this book that is strictly incompatible with an Indian homeland scenario.

Finally, it must be kept in mind that the contributors to the Aryan invasion debate in this volume have generally sought to prove the AIT. If they have not succeeded, it only means that the AIT has not been proven, not that it has been disproven. Perhaps the proof is waiting to be discovered, and the first failed attempt to find it has merely postponed the discovery till such time when the invasionists really mobilize their scholarly and scientific skills. For non-invasionists, it would be wrong to sit back and enjoy the other party's failure, for their own theory is still in need of proof positive. Rather, the time has come to shift the focus from countering the argumentative basis of the AIT to building a corpus of positive evidence for the alternative out-of-India paradigm.