2. The Rg-Veda in Afghanistan ?

(This chapter is a review of Rajesh Kochhar: The Vedic People, Their History and Geography*, Orient Longman, Delhi 1999, written immediately after its publication.)*

2.1. A decent book

Whenever a pro-AIT book is announced, I wonder with some apprehension: is this guy at last going to provide the evidence, turning my years-long effort to see the non-invasionist point of view into a waste of time? Is this going to be the end of the world of AIT debating? But then, when I get to read the much-acclaimed book -- such books are assured of compliments from prestigious quarters --, I find that the end of the world was once more announced too soon. It is the same story with astrophysicist Prof. Rajesh Kochhar's new book: *The Vedic People, Their History and Geography*.

First of all, this much must be admitted: though associated with the infamous crowd of "eminent historians" during the Ayodhya debate, Kochhar is not a dishonest man. He does not manipulate the evidence and he freely mentions data which on closer examination go against his own position, as we shall see. His fresh and independent approach in extracting a credible Aryan invasion scenario from the data makes him dissent from much standard AIT lore, e.g. he rejects the identification of Rama's antagonist Ravana as a non-Aryan champion of the natives against Aryan intruders, considering that by all classical accounts, Ravana himself was of Aryan lineage (p.212). Contrary to most of his allies in the Ayodhya debate, he accepts the historicity of Parashurama, Rama, Balarama and Krishna, and he asserts the historicity of the Puranic genealogies (p.91). Non-invasionists will agree with his criticism of the rather sloppy genesis of the prevalent Vedic chronology at the hands of F. Max Müller and A.A. Macdonell (p.11).

To be sure, he lets out his ideological agenda once in a while, e.g.: "The assertion that the Rgveda was composed entirely within the geographical boundaries of the Indian subcontinent neatly fitted the official view that Hinduism was the native to India while Islam came from outside. Imagine the damage to the two-nation theory that would have been caused by the assertion that the Indo-Aryans started composing the Rgveda when they were in central Asia, Iran or Afghanistan and completed it in India." (p.12-13) That assertion is the central thesis of this book, and Kochhar flatters himself by presenting it here as a bold stand against the "two-nation theory", all while discussing the 19th-century debate between European scholars about Vedic origins. But at that time, there was no "two-nation theory" except among orthodox Muslims who played no role whatsoever in this debate. The British saw India as a composite of many nations, and even without accepting Kochhar's assertion (then put forward by German scholars like Hermann Brunnhofer and Alfred Hillebrandt), they did believe that Hinduism came from outside: that was the whole point of the AIT, in any version. Nonetheless, his ideological commitments are never intrusive to the extent of replacing substantive argumentation with dogmatic pronouncements.

As an amateur to history, textual studies and archaeology, he also spends a lot of his allotted space on simply presenting the uncontroversial known facts pertaining to

parts of the relevant evidence, e.g. his brief introduction to the first musings about IE linguistic kinship (p.4-10) or to the history of Vedic literature (p.15-24) is quite helpful to the layman, as is his exhaustive presentation of the evidence for the by now well-established identification of the Soma plant as ephedra (p.98-112).

These parts contain a few very minor mistakes, e.g. not *kshetra*, "field", but *kshatra*, "rulership", corresponds to Avestan *xshathra*, "(domain of) rulership", whence *xshathra-pa*, hellenized as "*satrap*", and also related to *shah*, "king" (p.33). Also, it is not true that "if the Indian subcontinent were the Indo-European homeland, the Rgvedic language would be the mother of the IE languages" (p.205): no, what would more likely have happened within that hypothesis, is that after PIE-speaking tribes left the homeland to become Slavs, Greeks or Celts, the language inside the homeland kept on evolving and getting influenced by immigrant languages like Munda, to become Vedic Sanskrit many centuries after the break-up of the "mother" language. All the same, these parts prove that Kochhar has done his homework, earning him enough credibility to propose an at first sight far-fetched new thesis.

2.2. The Afghan Rg-Veda

Prof. Kochhar's thesis is briefly as follows. During the Harappan age, the Indo-Iranian tribes settled in Afghanistan, and started composing the hymns of the Avesta and Rg-Veda. The main lifeline of their civilization was the Saraswati river of southwestern Afghanistan, known as *Harahvaiti* in Avestan and *Helmand* (from *Haetumant/Setumant*) in modern Afghan Persian. In the declining centuries of Harappa, ca. 1900 BC, some non-Vedic Aryans moved into India, and their imprint is visible in a few Aryan elements in late-Harappan cities, such as fire-altars, which AIT skeptics have prematurely taken as proof of the predominantly Aryan identity of the Harappan civilization ("Features like the fire altars at Kalibangan and Lothal and the horse at Surkotada appear in the late Harappan phase thanks to the arrival of the Indic-speaking elements", p.206). By 1400 BC, a second wave of Aryans, equipped with the first half or so of the hymns which make up the Rg-Veda, entered India.

This hypothesis has a lot in common with the standard theory, e.g. the exaggerated attribution of Hindu practices to post-Vedic borrowing from the Harappans: "Concepts like Pashupati and Mother Goddess are obviously Harappan constructs that were incorporated into the Aryan fold" (p.207). Yet, a *Pashupati* figure is shown on the Celtic Gundestrup cauldron, and mother goddesses were quite important in the pre-Christian IE religions, suggesting in both cases a common heritage from PIE times. Note also Kochhar's faithfulness to A.F.R. Hoernle's and S.K. Chatterjee's proposition of two successive Aryan invasions, though on p.234, he rejects the contrived version that the first group went inland, leaving the northwest for the second, Vedic group. In his view, the first wave simply settled in the Harappan area: "The post-urban Harappans came in contact with the Indic speakers during the late Harappan phase. These Indic speakers, however, were not the Rgvedic composers", who arrived later, to compose the younger hymns, completing the Rg-Veda "in c. 900 BC". (p.206)

But his theory also differs from the prevalent paradigm in important details. Chronologically, it is more relaxed than the standard model which tries to cram the whole

of Vedic literature into a few centuries starting in 1200 BC or so. Indeed, though Kochhar does not insist on chronological detail, his hypothesis allows the earliest Rg-Vedic hymns to be dated as far back as the Harappan high tide, because the allegedly non-Harappan culture attested in the Rg-Veda would be located outside Harappan India in Afghanistan. And that then is the biggest difference: many scenes which both invasionists and invasion skeptics locate in India, have to be reinterpreted as referring to an Afghan setting. To match his hypothesis with the Vedic data, Kochhar has to impose a westerly interpretation on place and river names ordinarily located in the Saraswati and Ganga basins.

In his proofs, he follows in the footsteps of a P.L. Bhargava (*India in the Vedic Age*, 1955) and others by moving inconvenient locations around on the map, then using these new locations to justify yet other relocations, e.g.: “It is thus certain that the Hastinapura of the Mahabharata must have been somewhere to the west of the Yamuna; it is very likely that it was near the Indus.” (p.216) Follow some uncontroversial data which merely prove that “the Kuru were certainly familiar with the northwest”, which is not too distant in any case, e.g. Dhrtarashtra marrying a Gandhara (Afghan) princess. This “proof” of the more westerly location of Hastinapura is then used to drag Kurukshetra along: “The present-day Kurukshetra is unlikely to have been the battleground. It would have been too far away from the Kuru capital Hastinapura which we have suggested was on or near the Indus.” (p.217)

Kochhar is mistaken when he claims that “India does not figure in the Avestan and Pahlevi literature at all” (p.97). The land of seven rivers, Hapta Hendu, mentioned in Videvdad 1, is none other than Sapta-Sindhu, northwestern India (confirmed e.g. by Gherardo Gnoli: *De Zoroastre à Mani*, Paris 1985, p.26), and not the Afghan Farah-rud as Kochhar arbitrarily claims. Also, Airyanam Vaejo, mentioned ibidem, is a bit of a mystery to iranologists, but one serious candidate is certainly Kashmir, where summer does indeed last only two months, in conformity with the description given. The Persian word *Hindu* for “Indian” confirms that, contrary to Kochhar's claim, Hindu/Sindhu was their name for the Indus river and not merely a generic term for “river”.

He really goes overboard when he tries to counter the obvious objection that the whole surroundings of the Vedic Saraswati are unmistakably Indian (e.g. elephants), making its identification with the Afghan river Helmand difficult. Thus, in the famous River Hymn (RV 10:75), the rivers are enumerated from east to west, with the Saraswati coming after the Ganga and Yamuna and before the eastern tributaries of the Indus: here, like in the whole post-Rg-Vedic Sanskrit literature, the term “Saraswati” unmistakably refers to the Ghaggar/Hakra. So, Kochhar decides to move the whole Rg-Vedic setting along with the Saraswati into Afghanistan: Ganga and Yamuna become tributaries of the Helmand (p.131). Well, anything is possible, but this ad hoc solution really is far-fetched.

Also, he sticks to the Afghan identification even for the post-Rg-Vedic period which he has otherwise admitted as showing Indian locations (with the settlement of Northwest India taking place during the late Rg-Vedic period): “*We have shown* that the description of Sarasvati and Sarayu in the Rgveda and even sutra literature, fits the Afghan rivers Helmand and Hari-rud better than any river in India.” (p.222, emphasis added) Have we really?

2.3. Locating the Saraswati

The identification of the Vedic Saraswati is a matter of great consequence for Indian history: “From the late-Harappan evidence it is clear that the Ghaggar could have been a mighty river only before 1700 BC. If it was then the naditama [Vedic epithet: “rivermost”] Sarasvati, then all the settlements on its banks must have been Aryan settlements. In other words, if the Ghaggar is equated with the Rgvedic Sarasvati, then the Harappans must be equated with the Rgvedic people.” (p.137) That at least is clear. The mainstream invasionists ought to feel threatened by Rajesh Kochhar, for he gives them a choice between the non-invasionist scenario and his own invasionist hypothesis which upsets the generally accepted Vedic geography.

He distinguishes between two Saraswatis in the Rg-Veda itself: the mighty (or naditama) Saraswati is the Helmand, and the disappearing (*vinâshana*, “annihilation”, “disappearance”, place-name marking the dead end of the Ghaggar) Saraswati is the Ghaggar. Judging from the breadth of the old Ghaggar as suggested by the location of Harappan cities on its banks, it was bigger than the Helmand. Kochhar himself quotes RV 7:95:2 saying that the Saraswati surpasses “in majesty and might all other rivers", and RV 7:36:6 calling it the mother of rivers (p.120-121); that definitely doesn't apply to the Helmand, except perhaps to a local peasant unfamiliar with mightier rivers nearby, such as the Oxus and the Indus. Several references to the Saraswati having seven sisters (quoted ibidem) easily fit northwestern India, though Afghanistan need not be excluded depending on the scale of magnitude required of a worthy sister-river.

But let us give Kochhar a hearing. He argues that the Vedic Saraswati flowed in a mountainous area. The first Saraswati is described in the Rg-Veda as fierce (RV 6:62:7), swifter than others, roaring, and bursting the edges of the hills with its strong waves (all in RV 6:61), flowing from a threefold source in the mountains to the “samudra” (7:95) past many kingdoms (RV 8:21:18). Anyone who has seen the Ganga in Varanasi during the rainy season knows that a river can be “fierce” (few boatmen or swimmers dare to cross the river then), spectacularly eroding the banks and any constructions on it, eventhough flowing through the plains. The bigger a river is, the more violence it can exert, which again gives an edge to the Harappan Ghaggar over the Helmand. That the river's source is located in the mountains is true of most South-Asian rivers, including both Ghaggar and Helmand.

Note that Kochhar aptly delinks the loss of magnitude of the Saraswati in the early 2nd millennium BC from the change of course of the Satlej and the Yamuna, because according to his data, the time when these “flowed into the Ghaggar was long over before the Harappan times” (p.137). I reserve my judgment on the Saraswati system's prehistory until geologists speak with one voice about it, but I am in no position to deny the possibility that the present course of Satlej and Yamuna did indeed predate the late-Harappan period. This leaves the late-Harappan downsizing of the Saraswati unexplained for now (the drought crisis affecting Mesopotamia in the same period was not due to a shifting of rivers either), but a historical fact nonetheless.

As a non-philologist, Kochhar seems unaware of an argument which Western Indologists have thought up. On Dr. Dominik Wujastyk's Indology weblist, Prof. Michael Witzel has written that the etymology of the name Saras-wati, viz. from PIE **selos*, “pond”, hence “the river having many ponds”, is better compatible with the Helmand

than with the Ghaggar. The name would obviously be applicable to many rivers, quite possibly including the Saraswati/Ghaggar in the Vedic period, and is simply not distinctive enough to decide the matter. Perhaps we should count it as merit that the astrophysicist has remained aloof from the philological evidence.

He also leaves unused the passage in the Vajasaneyi-Samhita (34:11) which counts five tributaries to the Saraswati. This post-Rg-Vedic (hence, in Kochhar's and in fact in most scholars' scenario, definitely India-based) count is matched with a map of the Helmand basin by Prof. H. Hock (in J. Bronkhorst and M. Deshpande: *Aryan and non-Aryan in South Asia*, Harvard 1999, p.166). But in fact, the Helmand, like the Ghaggar, has many more tributaries. With the right criterion of magnitude, this unknown but large number can be reduced to five or three or seven, as is evident on various maps. Therefore, we may surmise that the Vedic poet meant something else than a complete count of tributaries along the whole river. The researchers L.S. Wakankar (brother of Saraswati "discoverer" Dr. V.S. Wakankar) and Dr. C.N. Parchure give a more likely explanation: "The five mouths can be identified as Jaisalmer/Badmer. It is significant to note that dried-up remnants of the following five rivers are presently observable near the holy place called Panchabhadra", viz. Luni, Guhya, Jojari, Lik and Dabariya. (*The Lost Vedic Sarasvati River*, Mysore 1994, p.45) So, five tributaries near a specific sacred place on a sacred river, viz. the Saraswati/Ghaggar.

Some of Kochhar's arguments only have force within invasionist assumptions, e.g.: "The Indo-Aryans, starting from the Eurasian steppes, would arrive on the Indus before they would reach the Ghaggar. Therefore, if Ghaggar were Sarasvati, one would expect the Indus hymns to be older than the Sarasvati hymns. As already noted, Sarasvati hymns are as old as, or older than, the Indus hymns." (p.196) Yes, in the Rg-Veda, the Saraswati appears before the Indus. But it is only after assuming an east-to-west movement that this necessitates, in contradiction with the textual evidence, a more westerly location for the Saraswati. It is equally possible to leave the rivers in place and deduce an east-to-west movement for the Indo-Aryans in the Rg-Vedic period.

All in all, I don't find any compelling reason, among those attempted by Kochhar, to relocate the Saraswati from India to Afghanistan.

2.4. Material evidence

Like most others of his school, Prof. Kochhar tries but fails to identify a trail of Indo-Aryan material culture leading from Afghanistan deep into India: "Archaeologically, the Vedic people have been a great disappointment". (p.89) Like Bernard Sergent, he finds clear Central-Asian elements in the post-Harappan border town of Pirak, west of the Indus, but this Pirak effect died down and had no substantial influence in more easterly regions, though these were certainly aryanized. So, the Pirak culture is not to be equated with the Aryan invader culture, merely one of the smaller intrusions which India has received from Central Asia all through history. Unlike Sergent, Kochhar also tries to revive the identification (once pioneered by Prof. B.B. Lal, now a convert to AIT skepticism) of the Painted Grey Ware with the Vedic Aryans, though they don't match chronologically even in his own theory.

Some facts to which he draws attention are, if verified, undeniably important. Thus, he argues that the production of large food surpluses is a theme frequently mentioned in later Vedic literature, starting from the 10th and latest mandala of the Rg-Veda, but absent from the earlier nine mandalas, because "by the time of the tenth mandala, the Rgvedic people were already residing in the plains where they could grow food in plenty, whereas the older mandalas correspond to the Hindu Kush regions without prospects of a food surplus". (p.241)

It is entirely new to me that the Rg-Vedic Family Books (2-7) show a scarcity economy, quite in conflict with numerous references to internal and international trade (vide Bhagwan Singh: *The Vedic Harappans*, Delhi 1995), but non-invasionists should take this claim as a challenge, rather than to smugly laugh it off.

Typologically, Kochhar's argument confirms their own approach of identifying matches and mismatches between the successive stages of Vedic and Harappan civilizations, e.g. how the absence of rice in the Rg-Veda matches the pre- and early Harappan period, before rice was introduced in the late-Harappan period, and not the post-Harappan period conventionally called Vedic. We should also take into account, against both the rice and the food surplus argument, the weakness of all *argumenta e silentio*. As Kochhar himself (p.11) quotes from Macdonell: "A good illustration of the dangers of the argumentum *e silentio* is furnished by the fact that salt, the most necessary of minerals, is never once mentioned in the Rgveda. And yet the Northern Punjab is the very part of India where it most abounds."

Some data clearly allow for a non-invasionist scenario eventhough Kochhar ties them to an invasionist one. Thus, the identification of Soma with ephedra does not necessitate locating the Indo-Iranian homeland in Afghanistan, for on Kochhar's own admission (p.110), ephedra is also found in North India (Kashmir, northern Panjab and Himachal). To be sure, in later Vedic literature, instructions are given for choosing substitutes for the soma plant, which had gotten out of reach. But this migration from soma-rich to soma-less territory is not one from Afghanistan to Northwest India, but from Northwest India to the Ganga plain.

That the soma plant could be plucked by the wayside (p.199; RV 8:80:1) would be a rarity in the plains where most of the Vedic action takes place, and more likely points to a mountainous area like Himachal or indeed Afghanistan; but the verse in question is from the 8th mandala, younger than the Saraswati-based Family Books, and the only book which unmistakably refers to an Afghan setting, with sheepdogs and camels. If this verse too is located in Afghanistan, it merely confirms the east-to-west expansion: in the older books, the Vedic Aryans live in the plains of Sapta-Sindhu, where soma has to be specially procured from the hills, but later they expand to the mountainous areas including Afghanistan where ephedra just grew by the wayside.

Kochhar himself writes that the Avestan "Yasna 10:10 mentions Haraiti Bareza as the Soma habitat". (p.101) "Haraiti Bareza" is the etymon of *Elburz*, now the name of a mountain in the Caucasus, but was, as Kochhar admits, the name of a whole range of mountains including the Hindu Kush. He might have added that it even includes the Himalaya. As leading Iranologist Prof. Eric Pirart recently told me, even in Firdausi's Shah-Nameh (ca. AD 1000) its meaning is Himalaya. The transfer of this name to a Caucasus peak typifies the east-to-west expansion of the Iranians from Bactria. At any rate, the Avesta confirms that soma could be found in India's border mountains.

That only the Indo-Iranians had a soma cult could be taken as proof that they were immigrants from a non-soma-producing PIE homeland. But the opposite scenario is equally possible: after reaching and bypassing the Aral Lake, where ephedra was no longer available, the migrating Indo-Europeans forgot about soma, or made do with alternatives. Thus, the Germanic Pagans used to offer beer sacrifices to their gods. The Hindu myth of the churning of ambrosia from the ocean by the gods is mirrored by the Germanic myth of Aegir churning beer from the ocean. In the unhospitable northern climate, they had to make do with a cruder beverage, but the general idea of the soma cult was preserved.

Meanwhile, if any excavation could confirm Kochhar's theory, it would be in the Helmand basin. The well-researched site of Mundigak (4th to mid-3rd millennium BC) near the lower Helmand is right where he should expect remains of the Vedic people. Bernard Sergent (*Genèse de l'Inde*, p.144) reports that this was a fortified town which underwent strong harappanization ca. 2600 BC, shortly before being abandoned. A centre of civilization emptied by the invading Indo-Iranians, who stuck to their shepherds' ways and spurned the city after capturing it? Or on the contrary, in the preceding centuries, a case of Indian influence spreading to the northwest along with the Iranians and Mitannians?

We can all join Kochhar in deploring that so many sites remain unexcavated. In Pakistan and Afghanistan, work has practically come to a standstill due to political and economic problems. In India, an archaeologist informs me, 99% of the identified Harappan sites are still awaiting excavation. So, that leaves us with a lot of room for speculation. In particular, Kochhar “would expect to find ruins of Rgvedic times along the Arghandab” (p.197), a tributary of the Helmand. Let us see.

2.5. Genetic evidence

Kochhar mentions a few data which hardly support his own theory. Most important is his reference to the genetic evidence: “Studies of genetic differences have shown that in about 3000 BC, migrations took place into Europe from the nuclear region of the Don river. This corresponds to the migration of the European branch of Indo-Europeans from the Eurasian steppes, which, then, must also be the starting point for the Indo-Iranians.” (p.205)

Well, exactly: part of the ancestry of the European IE-speakers can be traced back to the Euro-Asian border zone,-- but a similar genetic trail from the North-Caspian zone is missing for the Indian branch of IE. Kochhar has data for the European branch, but for the Indian branch he can only say that it “must also be” of North-Caspian provenance. True, such trail is so far also missing in the opposite direction, so we are watching for relevant new data to appear on the horizon.

And we get served at once. Dr. Nupam Mahajan, a medical doctor, recently drew the attention of Indology list members to two recent articles in *Current Biology* (London), vol.9, nrs.22 and 24, by T. Kivisild et al. (“Deep common ancestry of Indian and Western-Eurasian mitochonrdial DNA lineages”) and by Todd R. Disotell (“Human evolution: the southern route to Asia”), about genetic connections between India and “Western Eurasia”. One finding is that during or before the Ice Age, a group of human

beings migrated from Ethiopia and Somalia through Yemen and Oman to Gujarat and into India. The more important finding for our present purposes is that for the parameters studied, there is no north/south or Aryan/Dravidian divide in India; that one of the lineages showed a common origin between Indians and West-Asians in ca. 53,000 years before the present (this may be the spread of homo sapiens outside Africa, hence not very informative about more recent splits in the human family); that the West-Asian connection was highly minoritarian in the Indian gene pool, indicating only a small contribution by invaders from the West; and that the youngest split indicated by the genetic material dates to ca. 9,300 years BP.

This neatly fits the earlier findings of non-genetic (morphological) physical anthropology, viz. that the population type of northwestern India has remained the same for at least 8,000 years. Also: “Recent work suggests that the supposed Aryan invasion of India 3,000-4,000 years ago was much less significant than is generally believed.” In deference to established Indological opinion, the biologists make a perfunctory nod toward the “supposed” Aryan invasion, only to state that they have found no evidence for this popular supposition: “Their low frequency [i.e. of the West-Asia-related genes] but still general spread all over India plus the estimated time scale does not support a recent massive Aryan invasion, at least as far as maternally inherited genetic lineages are concerned.”

Of course, plenty of genetic material remains to be studied, new techniques are being developed etc., so I wouldn't think of pronouncing any definitive opinion on the genetic evidence. But for the time being, genetics joins morphology, archaeology, and Sanskrit text studies as yet another discipline which fails to yield evidence of that epochal Aryan invasion.

2.6. The testimony of the Vedanga Jyotisha

From an astrophysicist who freely writes about scriptural and archaeological topics beyond his formal competence, we had expected an in-depth discussion of a field in which everyone would accept him as an authority, viz. the astronomical evidence. Strangely, his contribution to that debate is limited to a few pages in which he discusses a single text, Lagadha's Vedanga Jyotisha. This text, in shloka form, is relegated by authoritative philologists to the post-Rg-Vedic age and specifically to the Iron Age (allegedly starting in 1200 BC). In a recent web discussion on the Indology list, a Dravidianist and invasionist scholar even proposed the first centuries AD as the date for this text. Yet, this is one text which is perfectly explicit about its own date, and Rajesh Kochhar is kind enough, or astronomer enough, to point this out.

When astro-chronological data are cited from Vedic literature, the invasionist defence invariably is that these must be “reminiscences”, verses preserved from an earlier time and describing a stellar configuration of the dim past, not the one visible to the poets in question. Somehow these Vedic oddballs always sang of things they didn't see (stellar configurations indication “pre-invasion” times, the mighty Saraswati in pre-Harappan magnitude), and not of the things they actually saw (stellar configurations of 1200 BC, the puny post-Harappan Ghaggar rivulet). Moreover, it is said that the supposed astronomical references are unclear, that they may be metaphors for something else, and

more such fog. While already contrived elsewhere, these excuses are simply unacceptable in the case of the Vedanga Jyotisha. As a manual for priests concerning the astro-chronological aspects of their rituals, the Vedanga Jyotisha obviously does not describe past constellations, but the sky actually visible to contemporaneous readers, in precise and technical language.

The important Vedanga Jyotisha verses for this discussion are verses 6, 7 and 22. Verse 6 gives the location of the winter solstice as 23 degrees 20' of sidereal Capricorn and of the summer solstice as 23 degrees 20' of sidereal Cancer. Taking the precession into account, Kochhar correctly tells us twice that this corresponds to "about 1400 BC" (p.26, p.112). It is a good thing that invasionist astrophysicist Rajesh Kochhar has now volunteered to confirm a date calculated before by so many Indian and a few Western scholars, but pooh-poohed by Western invasionists as nothing but an anachronistic "reminiscence".

Why their stubborn refusal to accept a solid astrochronological testimony? The Vedanga Jyotisha is in standard classical Sanskrit and in shloka form, which philologists have relegated to the post-Samhita period, a number of centuries after the Rg-Veda, which in turn they have dated to 1500 BC at the very earliest. This is a philological argument of which Kochhar seems unaware: in his view, the Rg-Veda and the Vedanga Jyotisha may well be contemporaneous, or the latter even older than the former. If the Vedanga Jyotisha really dates back to ca. 1400 BC, this implies either a drastic revision of the history of Sanskrit, or a shifting of the age of the Rg-Veda into the Harappan period or earlier.

Verse 7 of the Vedanga Jyotisha says that during the northward course of the sun (December to June) the daily increase of the day half is equal to one prastha (a measure of the water level in the clepsydra?), with the total increase in the length of the day half from winter solstice to summer solstice amounting to 6 muhurtas (1 day = 30 muhurtas), i.e. 6/30 or 1/5 of a day, or 4h 48' in modern time-reckoning. This is true at a latitude of ca. 35 degrees North: closer to the equator, the difference between the length of day in June and in December is less, closer to the poles it is more. This latitudinal belt approximately includes Kabul and Srinagar: "Making allowances for the margins of error in observations and for simplicity of calculation, any such place will serve as the seat of recorded observation" (p.113), but not the Harappan cities, which are between 20 degrees and 32 degrees North. Delhi or Indraprastha is at ca. 28 degrees N, where the difference between shortest and longest day is 4h 12'.

Verse 7 gives only a rather crude approximation of its stated object, viz. the increase in daytime length through the half-year from December to June. In reality the curve of the daily increase of daylight in terms of the time-distance from the winter solstice is approximately sinusoid (requiring a quadratic or second-degree equation) and not linear (first-degree equation), as is implied in this verse. The rate of lengthening is not constant, and the value for the day length calculated from the formula given will only be exact for the solstice and equinox days. Thus, at 35 degrees N and for 6 May, halfway between spring equinox and summer solstice, the formula would yield a daylight span of 13h 24', just midway between 12h on spring equinox and 14h 48' on summer solstice; the real value, however, is ca. 13h 35'.

Verse 22 again gives a linear formula to link a day in the year with the daytime span on that day: take the number of days elapsed since winter solstice on a given day,

multiply by 2, divide by 61, and add 12, to obtain the length of the daytime expressed in muhurtas. Thus, for winter solstice day itself, we get 0 x 2/61 + 12 = 12. For summer solstice day, we get 183 x 2/61 + 12 = 6 + 12 = 18. This again corresponds to 35 degrees N. For intermediate days, we get an inaccuracy, e.g. for 6 May we get 138 x 2/61 + 12 = 16.52 muhurtas = 13h 13'. One of the main points here is the symmetry through the year: the shortest day on 21 December (12 muhurtas) is exactly equal in duration to the shortest night on 21 June (30 - 18 muhurtas). This, I fear, cuts off one promising escape route from the conclusion that the place of reference was located near 35 degrees N.

However, there is a certain ambiguity about what exactly "day half" means. If the time-span from sunrise to sunset is meant, then the data are as just given: the longest day at 35 degrees N lasts 18 muhurtas. But could not the time from the first to the last daylight be meant? This would mean, from 20 minutes or so before sunrise until the same length of time after sunset, altogether more than half an hour longer. In that case, it is in Delhi or even slightly more south that the longest daytime would be approximately 18 muhurtas. However, in that case there would be no symmetry between summer and winter: the longest day would be about an hour longer than the shortest night, for the daytime gets half an hour on top of the time from sunrise to sunset, while the longest night loses half an hour from the time from sunset to sunrise because of the brief daylight periods before sunrise and after sunset. Now, the formula given in verse 22 (unlike verse 7) implies symmetry between winter and summer, ergo between night and day, so the meaning of "daytime" is clearly determined as the period from sunrise to sunset, not the longer period from first to last daylight.

The Vedanga Jyotisha is a notoriously dense and difficult text, so this will not be the last word about its verses 7 and 22. But for now, we must concede that there is a prima facie case for locating the observation underlying this text near 35 degrees N. Unless a more complicated explanation is called for, as possibly indicated by Kochhar: "Thus the ratio of the longest day to the shortest night is 3:2 [i.e. 18:12 muhurtas]. This statement is repeated in other texts also, for example in Kautilya's Arthashastra and Patanjali's Mahabhashya. Later, the ratio was spotted in Babylonian cuneiform records dating from about 700 BC." (p.113) Babylon is at ca. 32 degrees N, Kautilya's Pataliputra at ca. 26 degrees N. Did they merely copy a by then traditional verse, or should the text be reinterpreted in such a way that it becomes true for their localities?

We have already shown that twice, the formulas given for the length of day through the year are only rough approximations; could this not also affect the measurement of the longest day as "18 muhurtas"? In those days, measuring time was more complicated than the simple operation of measuring distance (e.g. the distance between stars), so perhaps they settled for the attractive approximation of a 3:2 ratio rather than exerting their utmost precision? Unlike the data on the solstice locations in the ecliptic, which had an immediate practical import for the priests and their rituals, this claim about the length of day and night had no practical implications, so perhaps less care was taken.

Well, I don't have all the answers, and I never said that we had reached the end of the debate. So, we will return to this topic when new data or insights come up. Meanwhile, locating the post-Samhita Vedanga Jyotisha in Afghanistan or Kashmir hardly proves that the Aryans came from Afghanistan. It adds just as likely to the number

of data showing a movement from early Vedic in Northwest India to late and post-Vedic in a much larger area, from Bihar to the Oxus and Helmand rivers.

2.7. Concluding remarks

A few important points remain to be explored. That Mandhatr, the pre-Vedic king of Ayodhya, went to subdue the Druhyus in Afghanistan, does indeed raise questions about the location of Ayodhya: should it not be in the far northwest rather than in the mid-Ganga plain? (p.209) Again, the traditional connection between some Ramayana characters and the northwest, e.g. Lahore being Lava-pur, "city of [Rama's son] Lava", could form a prima facie argument for a more northwesterly location of the original Ayodhya. (p.210) Unlike some of his friends in the anti-temple camp, Kochhar does accept a basic historicity for the Ramayana, placing it in the Copper Age (witness the absence of swords in its core events), but he rejects the traditional location. I expect Hindu scholars to have a reply ready somewhere, but they should effectively introduce it into the debate.

For the Vedic period, however, the arguments offered in this book aren't that enticing, even at first sight. There is no doubt about Rajesh Kochhar's competence in a few subdomains of the variegated evidence, nor about his bona fides. But decisive proof of the AIT, he too has failed to offer. If he wants to trump the case made by his opponents, the AIT skeptics, in a second attempt, I suggest that he start by reading their arguments. For, like in most similar publications, the text and even the bibliography of this book is blissfully innocent of the sheer existence of an anti-AIT argument.

2.8. An Atlantis in the South China Sea

(For good measure, the theory of a northwesterly homeland can be counterbalanced with an even odder theory of a southeasterly wellspring. This review of Stephen Oppenheimer's *Eden in the East* was written shortly after its publication in 1999. Here is a slightly rewritten version, leaving out the parts irrelevant to Indian and Indo-European history.)

One of the many insulting epithets thrown at AIT disbelievers is that they are no better than "Atlantis freaks". Actually, this is not entirely untrue. Some AIT skeptics who have applied their minds to reconstructing ancient history, have indeed thought of centres of human habitation in locations now well below sea-level. When Proto-Indo-European was spoken, the sea level was still recovering from the low point it had reached during the Ice Age, about 100 metres lower than the present level. It was in the period of roughly twelve to seven thousand years ago that the icecaps melted and replenished the seas, so that numerous low-lying villages had to be abandoned.

After all, it is a safe bet that more than half of mankind lived in the zone of less than 100 m above sea level. In the context of the present debate on global warming, it is said that a rise in sea level of just one metre would be an immense catastrophe for countries like Bangla Desh or the Netherlands. The Maledives would completely

disappear with a rise of only a few metres. But more importantly, most big population centres today are located just above sea level: Tokyo, Shanghai, Kolkata, Mumbai, London, New York, Los Angeles etc. If the sea level would rise 100 m, most population centres including entire countries would become a sunken continent, a very real Atlantis. Consequently, there is nothing far-fetched in assuming the existence of population centres and cultures, 10 or 15 thousand years ago, in what are now submarine locations on the continental shelf outside our coastlines.

In a recent book, *Eden in the East: the Drowned Continent of Southeast Asia* (Phoenix paperback, London 1999 (1998)), Stephen Oppenheimer has focused on one such part of the continental shelf: the region between Malaysia, Sumatra, Java, Borneo, Thailand, Vietnam, China and Taiwan, which was largely inhabitable during the Ice Age. Thinking that this was then the most advanced centre of civilization, he calls it Eden, the Biblical name of Paradise (from Sumerian *edin*, "alluvial plain"), because West-Asian sources including the Bible do locate the origin of mankind or at least of civilization in the East. In some cases, as in Sumerian references, this "East" may well be the pre-Harappan and Harappan culture, but even more easterly countries seem to be involved.

Oppenheimer is a medical doctor who has lived in Southeast Asia for decades. Like most of us, he is vaguely influenced by Marxism, e.g. where he dismisses religion as a means to "control other people's labour", with explicit reference to Karl Marx's *Das Kapital*. (p.483) His book is based on solid scientific research (genetic, anthropological, linguistic and archaeological), and is in that respect very different from the numerous Atlantis books which draw on "revelations" and "channeling".

The most airy type of evidence, in its massiveness nonetheless quite compelling, is comparative mythology: numerous cultures, and especially those in the Asia-Pacific zone, have highly parallel myths of one or more floods. These are not opaque allusions to Freudian events in the subconscious but plainly historical references to the catastrophic moments in the otherwise long-drawn-out rise of the sea level after the Ice Age. For, indeed, this rise was not a continuous process but took place with occasional spurts, wiping out entire tribes living near the coast. The last such sudden rise took place ca. 5500 BC, after which the sea level fell back a few metres to the present level.

According to Oppenheimer, the Southeast-Asian Atlantis, provisionally called Sundaland because it now is the Sunda shelf, was the world leader in the Neolithic Revolution (start of agriculture), using stones for grinding wild grains as early as 24,000 ago, more than ten thousand years older than in Egypt or Palestine. Before and especially during the gradual flooding of their lowland, the Sundalanders spread out to neighbouring lands: the Asian mainland including China, India and Mesopotamia, and (for their Austronesian-speaking branch) the island world from Madagascar to the Philippines and Melanesia, whence they later colonized Polynesia as far as Easter Island, Hawaii and New Zealand.

It is quite certain that some of these Sundalanders, Austronesian or other, must have landed in India, some on their way to Madagascar, some to stay and mix with the natives. Oppenheimer doesn't go into this question, but diehard invasionists might use his findings to suggest an Aryan invasion into India not from the northwest, but from the southeast. But he does mention the legend of Manu Vaivasvata saving his company from the flood and sailing up the rivers of India to settle high and dry in Saptasindhu. Apparently, the distant origins of Indo-European society and of Vedic civilization are

related to the post-Glacial flood, probably the single biggest migration trigger in human history.

The Tamils have a tradition that their poets' academy or Sangam existed for ten thousand years, and that its seat (along with the entire Tamil capital) had to be moved thrice because of the rising sea level. They also believe that their country once stretched far to the south, including Sri Lanka and the Maledives, a lost Tamil continent called Kumarikhandam. If these legends turn out to match the geological and submarine-archaeological evidence, our academics would be wrong to dismiss them as figments of the imagination. But the Indian or Kumarikhandam counterpart to Oppenheimer's book on Sundaland has yet to be written.

Another language family originating in some part of Sundaland was Austro-Asiatic, which includes the Mon-Khmer languages in Indochina but also Nicobarese and the Munda languages of Chotanagpur, at one time possibly spoken throughout the Ganga basin. It is the Mundas who brought rice cultivation from Southeast Asia to the Ganga basin, whence it reached the Indus Valley towards the end of the Harappan age (ca. 2300 BC). In this connection, it is worth noting that Oppenheimer confirms that “barley cultivation was developed in the Indus Valley” (p.19), barley being the favourite crop of the Vedic Aryans (*yava*). Unlike the Mundas who brought rice cultivation from eastern India and ultimately from Southeast Asia to northwestern India, and unlike the presumably Indo-European Kurgan people whose invasion into Europe can be followed by means of traces of millet and other crops they imported, the Vedic Aryans simply used the native produce. This doesn't prove but certainly supports the suspicion that the Aryans were native to northwestern India.

Concerning the political polemic, the usual claim that the caste system with its sharp discrimination was instituted by the invading Aryans to entrench their supremacy is countered by the finding that even the most isolated tribes on India's hills turn out to have strict endogamy rules, often guarded with more severe punishments for inter-tribal love affairs than exist in Sanskritic-Hindu society. Here, Oppenheimer confirms that in the Austro-Asiatic and Austronesian tribal societies, inequality is deeply entrenched: “Yet the class structure which cripples Britain more than any other European state, is as nothing compared with the stratified hierarchies in Austronesian traditional societies from Madagascar through Bali to Samoa. (...) This consciousness of rank is thus clearly not something that was only picked up by Austronesian societies from later Indian influence.” (p.484) Social hierarchy is not a racialist imposition by the Aryans, but a near-universal phenomenon especially pronounced among Indo-Pacific societies including most non-“Aryan” populations.

Stephen Oppenheimer makes a detailed and strong case for the importance of the culture of sunken Sundaland for the later cultures in the wide surroundings. India too must have benefited of certain achievements and human cargo imported from there.