

WESTWARD HO !

The Incredible Wanderlust of the Ṛgvedic Tribes

Exposed by S.Talageri

(Saavadhaanapattra no. 2)

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Review of: Shrikant G. Talageri, *The Rigveda. A historical analysis.* New Delhi: Aditya Prakashan 2000, pp. xxiv, 520. ISBN 81-7742-0s10-0. Rs. 750; <http://voi.org/books/rig/>

Any new study of the historical content of the Ṛgveda is very welcome since the impression is common that little historical information can be found in the oldest Indian texts (Witzel 1989, 1995, 1997). One could therefore hope that Talageri's 500-page discussion would be of great relevance for Vedic Studies and for Indian prehistory in general. Any fresh evaluation of the data and of previous conclusions (including those of the present writer) could be expected to further advance Vedic studies. It is with high hopes, thus, that one opens Talageri's long and diligent study. It is a great disappointment that any close review of Talageri's "historical analysis" refuses to yield positive results.

What does Talageri (T.) want to achieve with this book? The study claims to present an unbiased, text-internal analysis of the historical data in India's oldest text. The book's claims are characterized by the blurb on the book's jacket (T.'s own words?), which accurately summarizes the tenor of the book. Talageri, we are told:

... has gone directly to the primary sources without any preconceived notions, and examined as well as analysed the available information with the help of traditional tools of interpretation.

... he has elaborated a historical analysis of the Rigveda on the basis of genealogies of composers of the hymns preserved in the Anukramaṇīs or indices of the Rigveda. The Rigveda consists of ten Maṇḍalas, each representing a different era of history. The interrelations between composers within the hymns, the references to Kings and Ṛṣis, the family structure of the Maṇḍalas, and system of ascription of hymns in the Maṇḍalas, go to show that the serial order in which the Maṇḍalas are arranged bears no relationship with their chronological order.

As a first step, therefore, Talageri has established the internal chronology of the Rigveda by classifying the Maṇḍalas as Early, Middle and Late. Next, he presents the geographical picture which emerges from the chronologically arranged Maṇḍalas, particularly the evidence of river-names, place-names and animal-names. The combined evidence gives a single unanimous verdict -- the Indo-Aryans were inhabitants of the interior of India, and their direction of expansion was from the east to the west and the northwest. Finally, he ... concludes that it was the Indo-Aryans who migrated to all lands covered by speakers of Indo-Iranian and Indo-European languages....

Talageri sums up his views further in his Preface (p. xviii), where he ties the aims of this book closely to his 1993 study: "...the Vedic Aryans were the Pūrus of traditional history. ... the Rigveda narrows down the identity of the particular Vedic Aryans of the Rigvedic period to a section from among the Pūrus - the Bharatas. This book ... shows that a detailed analysis of the Rigveda, far from weakening our [earlier 1993] theory, only makes it invincible."

There is enough evidence even here to demonstrate the frustrating contradictions in Talageri's book. Thus after being told that he goes "directly to the primary sources without any preconceived notions," we find that Talageri's analysis is made "with the help of traditional tools of interpretation" like the Anukramaṇīs -- which

(unknown to Talageri) have been recognized since the days of Macdonell (1886) at best as compositions of the late-Vedic period. (The text of the Anukramaṇīs that Talageri himself uses was actually redacted in the medieval period, as we will see *infra*!)

Talageri's 1993 book was rightly criticized for its heavy dependence on traditional sources like the Purāṇas. T. acknowledges that problem at the beginning of his new book (p. xvi) -- clearly suggesting that the same problem will not be found here -- only to tell us two pages later (xix) that his RV analysis depends on "indispensable and unassailable traditional information contained in certain basic texts" like the Anukramaṇīs, which reflect the same Purāṇic ideas found in his 1993 book! (He also slips a lot of Purāṇic materials in the backdoor in this work, as we shall see below.)

Talageri's reliance on traditional, post-Ṛgvedic sources in interpreting the RV, despite his claims to the contrary, is only the first of the countless errors that doom his endeavor from the start. Below I discuss a small cross-section of those errors. I begin with a summary of the ten main sections in my critique. For resilient readers wishing more data, further evidence on each of these points is given in the main body of the critique, drawn quickly from the notes on Talageri's book that I made in Paris.

CRITIQUE SUMMARY

§1 Stratification of the Ṛgveda. In his book, Talageri develops a transparently false stratification of the 1028 hymns of the RV, contradicting well-known evidence by claiming that composition of each of the 10 maṇḍalas (including the so-called family books) originated in its own unique time period -- one succeeding the other in tidy historical steps as the Vedic tribes supposedly marched Westwards. (He makes an exception of maṇḍala 1, whose hymns he distributes to three different periods; his march Westwards also involves a specific rearrangement of the 10 books; for details, see the web chart at the link given in §6, below.) Talageri does not acknowledge known details involving the redaction history of the family books, each of which contains late as well as early hymns. Nor does he mention known complications in the codification of the RV ascribed to Śākalya, made sometime near the middle of the first millennium BCE. (These issues are discussed in extenso in Oldenberg 1888, whose general position has been accepted by every serious RV scholar ever since.) Leaving aside his citations of a handful of studies available in English, Talageri does not mention any of the vast scholarly literature from the past 150 years that discusses the redaction of the RV; nor does he know of Oldenberg. Ignorance may be bliss, but not when it comes to revolutionizing current views of India's oldest text -- and much of ancient Eurasian history to boot.

§2 The Anukramaṇīs: Garbage in, Garbage out. As suggested earlier, in his "analysis" of the RV, Talageri depends heavily on the Anukramaṇīs -- late- and post-Vedic lists of RV poets (many of them clearly fictional), deities, and meters. These lists are closely related to other later and traditional sources, including the Purāṇas. The most common versions of them, including the one used by Talageri, were still being revised in the early Middle Ages.

Given Talageri's dependence on such sources, the venerable computer motto, "garbage in, garbage out," clearly applies to his book. Talageri begins his analysis with post-Ṛgvedic conceptions of RV chieftains, priests/poets, and the like, drawn from the Anukramaṇīs and (surreptitiously) the Purāṇas. Not surprisingly, this leads him to propose views of the historical contents of the RV that themselves reflect post-Ṛgvedic traditions. T. not only seems oblivious of these facts, but is unaware as well that competing versions of the Anukramaṇīs exist. Indeed, he makes the startling claim at the beginning of his book (p. 7) that "the Anukramaṇīs were part and parcel of the Rigvedic text from the most ancient times" -- claiming further that these lists must lie at the grounds of any serious analysis of the text.

Amateurish errors like this are compounded by the fact that the version of these lists that Talageri (unknowingly) depends on -- an early medieval redaction of late-Vedic Kātyāyana's Sarvānukramaṇī -- views things from the East, which was a prime area of late Vedic ritual reform and canonization (see Witzel 1997). The result is that T.'s claim that early RV books originated in the East and later ones in the West -- the grounds of his imaginary Aryan march to the West -- is bolstered by his unsuspected dependence on post-RV Eastern sources. In short, Talageri's new book, like his old one, is a Purāṇa-like fantasy, deriving from the same confusion of ancient and not-so-ancient sources that doomed his heavily criticized 1993 effort.

§3 **Victorian Sanskrit?** At no point in Talageri's book do we find any suggestions that he has a genuine working knowledge of Sanskrit -- let alone of the obscure Old Vedic forms of the RV. As noted earlier, Talageri relies throughout on Griffith's outdated Victorian translation (1889), which even in its own day was aimed at a popular (and not scholarly) audience. The translation is also marred by its heavy dependence on Śaṅkara's late-medieval scholastic commentary (cf. Griffith's preface to the first edition). Obviously anticipating heavy criticism for his dependence on Griffith, Talageri testily defends the accuracy of the translation, taking potshots at me in the process. He does not reveal what philological criteria he used in judging the translation, since it is clear from Sanskrit errors in the text (discussed *infra*) that he cannot read the original on his own.

The RV is one of the most obscure and problematic ancient texts known. It is not too much to ask that those who claim to reinterpret it radically -- and to reinterpret much of world history along with it -- be capable of reading it in its original form. At a minimum, one would expect Talageri to consult one or more of the modern scholarly translations, accompanied by critical philological notes, produced in the 20th century by Geldner (German), Renou (French), or Elizarenkova (Russian). But Talageri, who cannot read any modern scholarly language besides English, does not leave a clue that he is aware that these works exist.

§4 **Failures in the 'Petty Conjectural Pseudo-Science' of Linguistics.** Unlike his colleague and sometimes supporter, N.S. Rajaram, Talageri does not reject the results of what Rajaram styles "the petty conjectural pseudo-science" of linguistics. Indeed, the final chapters of Talageri's book haphazardly draw from a handful of linguistic works. Despite his scholarly pretensions, however, nothing in his book or its 1993 predecessor suggests that T. can claim anything approaching a serious grasp of the subject. His lack of philological knowledge deprives him of useful tools that he would need (if he could in fact read Old Vedic) in interpreting key items of pre-Pāṇinian grammar and of disputed Ṛgvedic words. Proper use of linguistics would also have helped him harness his undisciplined etymologizing, which results in his countless false deductions concerning pre-RV history; on all these points, see below.

§5 **Trita's View From Inside the Well? Other Missing Sciences.** Other humanistic and scientific fields critical to RV scholarship are neglected by Talageri as well, even though he has the rich resources of the Bombay University Library close at hand. Consulting a few standard scholarly resources would have saved T. from the myriad of factual errors that litter his book. Talageri demonstrates little knowledge of the realia of the RV period, displaying ignorance of the workings of tribal societies, early states, the habitat of the Gangetic dolphin, and many similar items noted in the following discussion.

§6 **Mythological Chronologies: Ṛsis of the Kali Yuga?** Due to the methodological problems noted above, one can hardly expect to find many reliable conclusions in Talageri's book. Some of his most ridiculous claims lie in his chronologies -- an old story to anyone acquainted with the works of other nationalistic "revisionist" historians. Talageri imagines that the whole of the RV took a minimum of 2000 years to compose, extending from roughly 3500-1500 BCE. (For an overview of Talageri's timeline, see the chart from his book, with critical notes by S. Farmer, posted at <http://www.safarmer.com/pico/talageri.html>). Talageri's impossible chronological ideas are tied to a number of obvious anachronisms. Thus in Talageri's historical fantasy we find Purāṇa-inspired Eastern Ṛgvedic "dynasties" and "kingdoms" in the Gangetic Plain, which was inhabited at the time in question only by Neolithic hunter-gatherers and early chalcolithic farmers; we discover fast Ṛgvedic horses-and-chariots nearly two millennia before any reputable archaeologist would place them there; and so on. Again, all this is familiar territory for Indologists acquainted with the works of Frawley, Rajaram, Kak, Kalyanaraman, and so on.

§7 **Talageri's Geography: A Moveable Feast.** Talageri's RV geography is similar skewed by Purāṇa-inspired fictions. A combination of bad RV stratigraphy, ignorance of Old Vedic, a neglect of standard scholarly research, an inadequate grasp of South Asian zoological facts, and a naive confusion of mythological and historical RV references help Talageri deliver the desired locations, rivers, and peoples required in his imaginary march of Vedic tribes to the West.

§8 **Have Words, Will Travel!** Free-form etymologizing worthy of the Indian nationalist P. N. Oak lets Talageri interpret the names of persons, tribes, and local animals as "Aryan" or "Eastern" (as his model requires) when the linguistic evidence points squarely elsewhere -- often to pre-Indo-Aryan substrates. Talageri's

embarrassing lack of scholarly linguistic and philological skills leaves him oblivious to these errors, some of which I discuss below.

§9 **Westward Ho! Talageri the Patriot.** The confidently asserted ("invincible") revolutionary conclusions that Talageri draws concerning the Westward drift of Vedic tribes -- which imagines them moving from the Gangetic Motherland into the Panjab, and from the Panjab to Iran and to Europe -- are familiar Hindutva fantasies. The expert knowledge of Old Vedic, Old Iranian, and other ancient Indo-European languages that would be needed to prove such surprising conclusions are not, as earlier suggested, the kinds of linguistic skills displayed in Talageri's work. Instead, he contents himself with citing a hodgepodge of linguistic facts (and fictions) -- often out of context -- from whatever studies in English happen to fall in his hands. All these are raised to back his fantastic claims that Vedic civilization was the "original homeland of the Indo-European family of languages" and ultimately of all Indo-European mythology and culture.

This, of course, is an exact reverse of the long-accepted view, supported by over a century and a half of research, that an Indo-Aryan migration, whose specific nature is still being studied, took place into the subcontinent. No matter that T.'s revolutionary insights are contradicted by a myriad of well-known linguistic, zoological and archaeological data: Westward ho!

All these fantasies are driven by the perceived need to prove the "hoariness" of ancient Bhārata, supposedly represented by unbroken traditions reaching way back to pre-Harappan times -- that is to 7th millennium Baluchistan(!). This, of course, would make them the oldest traditions on the planet: The RV, as Talageri declares on p. xix, "is the oldest and hoariest religious text of the oldest living religion in the world today: Hinduism." Underlying these claims is the familiar Hindutva agenda that suggests that all non-Hindus are ultimately "foreign" peoples in India, and a blot on the body politic. Bhārata über alles!

In short, T.'s book is driven by current political realities in India and not by a *l'art pour l'art* search for truth. This standpoint is reinforced by the uniformly bellicose style of the book, which throughout illustrates some of the least attractive aspects of pāṇḍita style. Talageri, like the medieval debater, is eager everywhere to score points for his thesis. Nowhere are obvious counter-arguments to his views seriously entertained.

§10 **Marching Backwards into History.** As I emphasized in the paper that initially triggered Talageri's polemics (Witzel 1995), reconstruction of the historical data locked in the RV demands the systematic collection and analysis of many diverse types of data scattered in the text. One powerful way to promote this end is to generate multidimensional computational grids, analyzable through sophisticated software, of all useful data concerning poets, chieftains, geographical locations, linguistic and dialectical variants, meters, substrate words, grammatical innovations, linguistic archaisms, and so on, found in different strata of the work. Points of convergence, divergence, and overlap in these multidimensional RV "maps" suggest new and reliable ways to stratify the text and to uncover the historical data locked in it. Researchers from all over the world have been invited to join in perfecting these tools, which have already begun to throw new light on the early history of (R̥g)Vedic peoples.

Ironically, in his new book Talageri has adopted something crudely approximating the methods first suggested in my 1995 paper, though not using computer programs. But he has approached the job without the requisite language skills, scholarly acumen, or historical and political objectivity to do a credible job. Moreover, rather than basing his analysis strictly on RV data, as he pretends he does, his analysis repeatedly confuses late-Vedic redactions and interpretations with what is found in the original RV text. The result is that his book manages at best to reassert familiar Hindutva myths using obfuscating claims about "new" methods, all of which is, as expected, uniformly praised by the "usual suspects" active in the Hindutva Cottage Industry.

The methodological and factual absurdities in Talageri's book will be obvious to any serious Vedic scholar once he or she tediously identifies the late- and post-Vedic origins or the data populating the morass of unverified charts and lists dotting the book. It will not at all be obvious to less specialized readers, including the Indian lay persons constituting Talageri's main audience. On this point, Talageri's book and the writings of other rightwing Indian propagandists like Rajaram perfectly coincide.

Serious attempts to understand ancient Indian history are unnecessarily diverted by the intellectual detours we are sent on in books like Talageri's. Solid reconstruction of the history hidden in the RV demands linguistic rigor, independence from Purāṇic-like worldviews, and above all a kind of political integrity not evident anywhere in Talageri's book.

DETAILED DISCUSSION

§ 1 Stratification of the Ṛgveda

In the rest of this review, the points I have made summarily in sections 1-10 will be discussed at some length. Due to their importance to the rest of his work, points 1 and 2 are outlined in special detail.

Like most ancient Indian works, the Ṛgveda is a heavily stratified text composed by many authors living in different eras. Like many such texts, whether Vedic, epic, Buddhist or classical, it has been arranged by its redactors according to the length of its subsections.

In the case of the Ṛgveda, the obvious and well-known subdivisions are those involving 10 maṇḍala (usually, though ahistorically, called "books"). Each maṇḍala is further broken into a number of sūktas (hymns), and each sūkta into a number of ṛc (verses).

This is the well-known division into 10 books -- containing 191+ 43+62+58+87+75+104+103+114+191 hymns and some 10,500-odd verses.

However, as soon as one searches beyond this obvious division, which is clearly distinguished in the manuscripts (and in the still available, superior traditional oral recitation), one discovers an older arrangement underlying the maṇḍala-sūkta-ṛc division. While to a large degree it mirrors the present division, on closer inspection certain deviations are evident. Obviously, books 1 and 10, which each contain 191 hymns, stand out from the generally ascending pattern found elsewhere in the length of books (from 42 to 104 hymns in books 2-7).

It has been long noticed that book 10 is linguistically younger and that it in part overlaps with sections of the Atharvaveda (AV). Pushing things still further back, it was already noticed some 130 years ago, by Abel Bergaigne and Hermann Oldenberg, that the so-called family books (2-7) form the old core of the RV. This finding has been taken over by T. without comment, quoting as his only witness the summaries of earlier research compiled by the promising, but ill-fated, Bh. K. Ghosh (who received a Ph.D. from Munich and a D.Litt. from Paris in the 1930s but died of leprosy sometime after returning to India).

If one prefers to add up the verse count for books 2-7 -- rather than counting the hymns -- one gets, according to T., 429+617+589+727+841 verses. (Cf. Satvaḷekar's RV edition, p. 809-826, which gives 2006 + 429+617+589+727+ 841+ 1108+ 1754 verses for the whole text.)

All this is not new. However, on further analysis, first carried out by Oldenberg in his groundbreaking Prolegomena in 1888, we find that the family books (RV 2-7) contain other organizational factors that involve the authors (ṛṣi), deities (devatā) and meters (chandas) of the hymns. Even today, all three are still uttered before any formal Vedic recitation of a hymn. The result is that each book in the "family" collection -- from the 43 hymns of book 2 to the 104 of book 7 or (with some deviations, even the 114 of book 8) -- is internally arranged as I summarized it in 1997:

...the RV is structured according to several clear principles best visible in the family books (RV 2-8): (1) the number of hymns per book increases, (2) the family books begin with a small Saṃhitā addressed to Agni, Indra and other gods, all arranged according to decreasing total number of hymns in each deity collection. (3) Inside a deity series the hymns progress from longer to shorter ones. The meter decides further: Jagatī, Triṣṭubh hymns precede those in Anuṣṭubh, Gāyatrī" (Witzel 1997).

Incidentally, similar arrangements are also seen in the Pali canon of early Buddhist texts, and elsewhere in Indian texts. Analogous principles are also found in the Zoroaster's Gāthās, pointing to formal links between Vedic and Avestan traditions that invite further investigation.

Any deviation from this strict numerical arrangement has to be explained. The reason, as demonstrated again by Oldenberg, is that various hymns or sections of hymns have at later points been interpolated into the text. This is found especially often in hymns of unusual length: small individual collections of 3 verses (Tṛcas) or 2 verses (Pragāthas) were added to certain hymns or were combined into a new hymn during the final standard RV redaction. This was carried out by Śākalya in the late Brāhmaṇa period -- in other words, shortly before the time of the Buddha (c. 500/400 BCE).

All such additions result in hymns that are too long and deviate from the strict pattern. Later on, after Śākalya, more hymns, such as the Śṛisūkta, were added to the text, some of them clearly reflecting medieval ideas.

They were gathered together in the Kashmir Khila collection -- and always stand out insofar as they are not found in Śākalya's Padapāṭha and reflect post-Ṛgvedic grammar and contents.

None of the well-known structural principles in the nucleus of the RV -- found in books 2-7 (and in a wider sense 1.51-8.66; see Witzel 1995: 309, 1997) -- are discussed by or even mentioned by Talageri. This gives him the freedom to propose his own additions, based, e.g., on some rather secondary and late evidence (these involve parts of RV 3 discussed in AB 6.18, which is found in a late stratum of that text; this issue is discussed below). Talageri also views as interpolations the Vālakhilya hymns of 8.49-59 (although these are, in fact, included and analyzed in Śākalya's Padapāṭha), some hymns mentioning the "Tṛkṣi dynasty" and a few minor but unspecified additions. All the rest of the text T. considers to be genuine creations of RV-time seers.

As already noted, the redaction history of the RV is well-known, but is nowhere discussed in Talageri's book. Summarizing this history, we can distinguish five major steps in the redaction of the text (for details, see Witzel 1995, 1997, etc.):

1. Stage one involved the original collection of the so-called family books, in the Kuru or Mantra period, which were organized using the numerical principles described by Oldenberg.
2. Stage two involved the addition of materials that now comprise books 8, 1, 9, and 10, which were added at several distinguishable moments (for details, see Oldenberg 1888 and Witzel 1995, 1997).
3. Stage three involved individual additions of whole hymns and of many Tṛcas and Pragāthās to various RV books. As again shown by Oldenberg 1888, these are often identifiable by the violation of the numerical principles found in the first redaction of the family books and/or on linguistic grounds.
4. Stage four involved the redaction and final ordering of the text by Śākalya in his Padapāṭha. (For simplicity, we can ignore some minor phonological changes that were later made to Śākalya's text.) The work ascribed to Śākalya occurred in the late Brāhmaṇa period, as is evident from his Eastern style, his grammatical misunderstandings of some RV forms (Witzel 1989, 1997), and from further evidence found in the ŚB tradition (ŚB 11.5.1.10).
5. The final stage included the addition of RV Khila that do not appear in the Padapāṭha.

Close analysis of these redaction stages demonstrates that the composition of the RV occurred in complex layers -- not in the tidy sequential patterns imagined by Talageri (cf. again the chart noted in §6). Once these complexities are recognized, it becomes obvious that many hymns can no longer be securely claimed to have been composed by the authors they are attributed to in many late traditions. In fact, many interpolated hymns (e.g., in the partial list given in Witzel 1995) clearly stand out as late additions due to their late book-10-style and AV-like grammatical forms and contents (e.g., sorcery hymns).

All of this is well known -- but not to Talageri (and apparently not to his proclaimed Western helper, Dr. K. Elst, who did not alert Talageri to the problem.) Even one look at Geldner's scholarly translation of the RV (Vol. I, xiv-xix) would have done the trick.

I have briefly explained these principles (discussed in my 1995 paper used by T.; also in Witzel 1997) to Talageri via email, in the summer of 2000, to no avail (rebroadcast in Indic Traditions, 11/12/2000, see: <http://groups.yahoo.com/group/indictraditions>). Far from being outdated or 'Romantic', as another self-proclaimed specialist of Old Indian history, N.S. Rajaram, in the Organiser, will have it, Oldenberg's analysis has not been challenged or refuted so far. We may hope, however, that the English translation of Oldenberg's 545 pp. book (Motilal Banarssidass, c. 2001), which is finally making it to English over 100 years too late, will help would-be historians like T. understand the structure of the RV.

The result of all this is that T.'s book is based on what is essentially the wrong Ṛgveda text -- the late Vedic compilation by Śākalya, which had already been subjected to several earlier redactions, and which mixed up materials from several eras in each of the books. Talageri, unlike all serious Vedic scholars after Oldenberg,

makes no attempt to reconstruct the more ancient text on which that compilation was based. The result is that all the far-flung historical conclusions that he draws regarding the time and location of individual books, their authors, etc., are totally unreliable. Many of his individual items of "proofs" (such as the designation of the Gaṅgā in RV 6, gāṅgya) immediately fall off the board as late, not as being part of the "earliest RV" as T. claims (see §7).

Amusingly, T. does not even exclude from his RV evidence stanzas that were added long after Śākalya's Padapāṭha. This includes 7.59.12 -- a Tryambaka verse, a late interpolation to the already older interpolated stanzas 7.59.7-11 -- and similar late additions found at 10.20.1; 10.121.10; and 10.190.1-3.

Instead, T. accepts as "major interpolations" (p. 74) only the well known Vākhilya hymns (RV 8. 49-59) and RV 3.21, 30, 34, 36, 38-39 (with 68 verses). His reasoning here follows from his observation of a discrepancy in the ascending number of the sums of verses for each of the RV family books 2-7, which he tells us (per his Śākalya and Anukramaṇī sources) contain 429-617-589-727-765-841 verses (p. 73-74). Noticing that book 3 has too high a number, T. explains this as an obvious result of an interpolation.

It is important to note that his claims concerning which verses were stuck in here do not come from any internal study of the text but from his reading (or misreading) of a much later text, AB 6.18. According to Talageri, AB 6.18 tells us that RV 3.21, 30, 34, 36, 38-39, together numbering 68 verses, were "misappropriated by Vāmadeva," the traditional composer of RV 4.

In fact, however, it is far from clear that AB 6.18 makes any such claim. The text only tells us that Viśvāmitra "saw" the Saṃpāta hymns in question first and that Vāmadeva actually "created" them (in RV 4). Viśvāmitra (of RV 3) therefore "created" counter-saṃpāta hymns. The whole section has other poets contributing to this endeavor as well: Bharadvāja (of RV 6), Vasiṣṭha (of RV 7), Nodhas (of part of RV 1). AB 6.18 continues by listing the beginnings of RV hymns 3.48, 3.34, 3.36, 3.30, 3.31, 3.38 (= 81 verses) -- not, as T. tells us, RV 3.21, 30, 34, 36, 38-39 (= 68 verses). The result is that the list in his own cited sources does not agree with the contents of his proposed "major interpolation."

A little countercheck of T.'s actual data is always useful, as shown further below. Dozens of cases that I have doublechecked show marked discrepancies of the sort found here.

Nothing in the AB passage in question speaks about the actual verse numbers of RV 3 or 4, as T. suggests. The discussion revolves instead about which Ṛṣi actually contributed some verses, not to the RV but to the Hotrakas' recitations. The story told at AB 6.18 comes from the late Brāhmaṇa period, from North Bihar, where Śākalya's codification of the RV took place (see Witzel 1997) and where the early layers of Kātyāyana's Anukramaṇī took shape (see below).

The underlying lesson to be learned from studying the representations of the RV in such late sources -- a lesson missed by Talageri -- is that the politics of later priests and competing Vedic schools (Śākhās) and redactors active at the Sanskritizing court of Videha often skewed the historical evidence found in the original RV. Note, e.g. the wrangling between the various types of Veda proponents in BĀU 3.3 (Witzel 1987); and cf. the "adoption" schemes among certain poets' clans (Witzel 1995) as well as a divergent Ṛgveda at ŚB 11.5.1.10.

On the issue of interpolations, it is critical to note again that Talageri never bothers to check his claims against the accepted findings of a long line of Vedic scholars. The 68 verses that he excludes in his "major interpolation" turn out not to be interpolations using Oldenberg's well-tested method (cf. Witzel 1995 : 311) -- while different nearby verses that T. assumes are authentic do turn out to be late additions (e.g., RV 3.26?-27?, 28, 29, 51?, 52-53, 62?). The importance of definitively identifying late and early strata in individual RV books is obviously essential to T.'s endeavor, since his announced aim is to unlock the historical truths hidden in this "hoariest" of all documents.

In passing, T. does mention other "incidental" interpolations in the text. Talageri writes (p. 68): "There are other actual or alleged cases of interpolations in the Rigveda (all interpolations made during different stages of compilation of the Rigveda before the ten-Manḍala Rigveda was finalized), but all of them are incidental ones pertaining to ritual hymns or verses" (p. 68).

Talageri does not specify which verses he has in mind besides (p. 70) quoting a note culled from Griffith's "hoary" translation (as T. might put it) claiming that RV 4.42.8-9 and 7.19 are later additions. Not surprisingly, both hymns turn out not to be interpolations when judged by Oldenberg's well-tested criteria. Evidence suggests instead that they belonged to the original, late Ṛgvedic "Kuru Saṃhitā" (Witzel 1997).

Even if T. were right in dismissing certain RV interpolations as "incidental," standard scholarly practice would demand that he identify those interpolations and explain how he identified them. Otherwise, he would

have a free hand in tossing out any verses in the text that conflicted with his "revolutionary" conclusions concerning the place and time of the composition of different RV books. This he has not done (cf. pp. 68, 73-4).

This is just one of countless examples of the methodological laxness that characterizes his entire book.

§ 2 The Anukramaṇīs: Garbage in, Garbage out

If all of this were not enough to discredit T.'s book, any examination of his use of the Anukramaṇīs would do an equally good job. Among his most startling claims is one found at the start of his book (p. 7), where we find that the Anukramaṇīs were "part and parcel of the Rigvedic text from the most ancient times." Nothing, however, suggests that the Anukramaṇīs (or any putative lost predecessors) existed in the RV period. In fact, this possibility is immediately ruled out due to the way that our present RV gradually developed out of a series of earlier redactions (see prior section).

It should be pretty obvious to most readers that Vāc ("Speech") is not likely to be the author of RV 10.125, as the Anukramaṇīs claim -- although the hymn does deal with speech (vāc). Nor is it very likely that all the hymns of book 4 (which includes known late hymns) were composed by one seer, Vāmadeva -- as they also tell us.

The Anukramaṇīs also give us the names of ancient poets, deities, and meters for many further hymns that are known to Vedic scholars (but not to Talageri) to be late. These include RV 6.75, a long AV-like sorcery hymn intended to make one's weapons victorious; the version of the Anukramaṇīs used by Talageri identifies the hymn's deities as "Bow, Arrows," etc. !

It is important to point out -- as Talageri does not -- that different Anukramaṇīs attached to the RV, SV, YV, and AV often disagree concerning the poets of the very same hymns or verses. This fact is more than enough to demonstrate the absurdity of his absolute reliance on these late- and post-Vedic texts, which he tells us (p. 4) must be the "very basis" of any analysis of the RV.

Remarkably, in his long discussion of these lists (pp. 3-20), Talageri does not bother to mention which version of the Anukramaṇīs he is following. Analysis of his book, however, shows that he is using the least ancient of the two extant versions of the RV Anukramaṇī text, which we will later see was compiled in the early Middle Ages!

What the Anukramaṇīs actually preserve, especially for the family books, are traditional attributions to a clan, or to individual poets (real or mythical), or to poets possibly assuming the name/title of a real or mythical ancestor. In many sections of the family books, it is only the relationships of certain hymns to each other that are correctly portrayed by the Anukramaṇī while the names of the poets involved are altogether unknown. This usually involves a (group of) hymn(s) by the same (small family of) known or frequently, by unknown author(s). Their names may occasionally be preserved inside the hymns themselves (or they may have artificially been derived by the Anukramaṇī from the text).

It is well known that traditional attributions of this sort may change over time: from the oldest surviving examples in Sumerian texts to modern Polynesian lists of chieftains, we can observe that there is no such thing as a fixed list. Changes occur in part when texts originating in one school or tradition are co-opted by late-coming rivals. This makes use of attributions like this an extraordinarily tricky business, as has again been known (but not to Talageri) since the 19th century.

In using this evidence, each case has to be carefully evaluated on the grounds of internal evidence before we can assume that poet X mentioned in the Anukramaṇīs was indeed the real-life poet of ascribed hymn Y. This careful approach is followed by Geldner in his translation of the RV (1951) and is further noted in my work (1995, 1997).

T., however, is quite innocent of any principles of the philological enterprise: he does not mind using a later text as a primary source to explain an older one in front of him, just as he does not use the Archaic Sanskrit text of the RV in front of him but only a Victorian English translation three thousand years more recent. But, all later texts always view older ones through the lenses of their own time (such as T. through his own peculiar ones -- of which color, we shall see by the end of this review).

Instead of depending on late sources like the Anukramaṇīs --with the motives of their composers remaining unstudied-- T. should have first carefully collected the poet/clan names mentioned in the hymns themselves. Of course, to get this job done, he would need to study the original Old Vedic text -- not Griffith's badly outdated Victorian translation. In the original, he would not only find references to authors who do not

appear in his Late Vedic list, but would also discover apparent poet or clan names hidden away in Sanskrit anagrams (see, e.g., RV 10.24.2, where we find vi ... made for "Vimada").

Despite his enthusiastic claims for the Anukramaṇīs, it should be noted that Talageri does not hesitate to change them when he is embarrassed by their contents (see pp. 19 ff.). We hence find that his list of supposed RV authors (pp. 7-19) varies considerably from what is found in any Anukramaṇī. Talageri writes: "There are obviously corruptions in the Anukramaṇīs in the form of ascriptions to fictitious composers we have replaced the fictitious names in the Anukramaṇīs with the names of the actual composers, ... the ṛṣi of the hymn or the ṛṣi of the Maṇḍala."

Talageri's free-form adjustments of such evidence, late as it may be, is still another distinctive mark of his book. The method by which he determines the "actual composers" in such cases remains a mystery.

Again, for unclear reasons he also wants to limit the clans involved in the composition of the Ṛgvedic hymns to ten families: "The composers of the R̥gveda are divided into ten families. These ten families are identified on the basis of the fact that each family has its own Apr̥i-sūkta" (read Āpr̥i-! - p. 21). However, the attribution of at least some of them to certain families depends on --what else could it be-- the Anukramaṇī; and there are more Āpr̥i hymns found elsewhere, such as in the Atharvaveda. Thus, the Āpr̥i hymn RV 1.142 is by an unknown poet, but traditionally attributed to the Āṅgīrasa Dīrghatamas, and 1.188 is attributed to Agastya. There are two Āpr̥i hymns in book 10: RV 10.70 is attributed to one Sumitra, and 10.110 is attributed to Jamadagni or his son Rāma. Obviously, T.'s statements are inconsistent and are a clear indication of how cavalierly he establishes his divisions of the RV. Incidentally, no study of the Āpr̥īs is mentioned, neither that by K.R. Potdar (1945) nor the last one by van den Bosch (1985).

I have already mentioned, as T. does not, that not one but a number of Anukramaṇīs are extant. Some particulars concerning these texts are found in A.A. Macdonell's specialized study from 1886 and in his succinct summary on pp. 272-4 of his History of Sanskrit Literature.

The best-known Anukramaṇī (the one used by T., though he does not mention it by name) is the Sarvānukramaṇī attributed to Kātyāyana, the alleged author of the White Yajurveda Śrautasūtra (the Kātyāyana ŚS) -- one of the latest texts of this genre (cf. also Kātyāyana in BĀU and Kātyāyana, the author of the Pāṇinean Vārttikas). These attributions suggest that Kātyāyana was a late- or early post-Vedic Eastern figure, implying that Eastern influences might be looked for in his Anukramaṇī as well. And indeed, we find that the Eastern countries of Aṅga (1.116, in S.E. Bihar, at the bend of the Ganges) and Kāśī (10.179, the Benares area) are prominently mentioned in it, as well as a poet Aṅga Aurava, 10.138. The language of the text (displaying late compounds, use of perfect, etc.) is certainly not Ṛgvedic, not even Upaniṣadic, but follows a terse Sūtra style. (In other Anukramaṇīs, Śloka even are the norm.) All this points to late/post-Vedic and Eastern origins as well. (For discussion, see H. Oldenberg, Zur Geschichte des Śloka, 1987: 1188-1215; Horsch 1966; and Macdonell 1886.)

Note, for comparison, the typical difference between the prose Dharmasūtras (early) and the metrical Smṛtis (later), or the older prose and the metrical middle Upaniṣads. The Ārṣānukramaṇī is a text of some 300 Śloka (Macdonell, 1886 p. v-ix). A hint about the general age of such texts may be contained in the fact that the Anuvākānukramaṇī of Śaunaka is quoted (Macdonell 1886: vii) by Āpastamba Dharmasūtra 1.3.11.6 -- which P. Olivelle (1999: xxxiv) now wants to date to the beginning of the third century BCE.

Since the Anukramaṇī used in Talageri's book knows of a king of Kāśī, Talageri -- who does not mention the country of Aṅga -- provides us with this curious analysis (p.118):

.... the Anukramaṇīs provide us with a priceless [geographical] clue: hymns IX.96 and X.179.2 are composed by a late Bharata Ṛṣi who ... attributes his compositions to his remote ancestor, Pratardana. He accordingly uses the epithets of his ancestors: in ... X.179.2, the epithet is Kāśīrāja (King of Kāśī) [read Kāśīrāja, Kāśī in Vedic! --MW]. Pratardana was a king of Kāśī, which is in eastern Uttar Pradesh. This can only mean that the Bharata Kings of the Early Period of the R̥gveda were Kings of Kāśī; and, in the

light of the other information in the Rigveda, the land of the Bharatas extended from Kāśī in the east to Kurukṣetra in the west.

Here we have T.'s historical logic in full bloom. Unfortunately for his views, Kāśī and other Gangetic lands mentioned here do not show up in the RV at all. When the term Kāśī first occurs, in AV 5.22 (Witzel 1980, 1987, 1995, 1997 n. 259), the Kāśī tribals were still regarded as despised outsiders to whom one sends illnesses. Even in the late Vedic ŚB 13.5.4.19, they still are not regarded highly. The rise of Kāśī comes only in much later periods. In Vedic times, they remained a small tribal area (which was conquered, by the time of the Pāli texts, first by Kosala and then by Magadha).

Predictably, T., makes much use of this late reference to the Kāśī in the Anukramaṇī to argue that the oldest parts of the RV are Eastern in origin. Here we get more vintage Talageri (my italics added):

The above conclusion is inescapable: the information in the Anukramaṇīs cannot be rejected on any logical ground (short of suggesting a conspiracy theory), and it fits in with all the other evidence: ... [Purāṇic and] even the rest of the Vedic literature. The geography even of the Yajurveda is clearly an Uttar Pradesh centred geography [-- however, as generally understood, the center of gravity shifted there in post-RV times -MW]. That the geography of the Rigveda is also the same has escaped the recognition of the scholars purely and simply because these scholars are so mesmerised by the Aryan invasion theory, and so obsessed with the vital need to locate the Rigveda in the northwest and the Punjab for the sheer survival of the theory, that their ideas and conclusions about the geography of the Rigveda are based on the tenets of this theory rather than on the material within the hymns of the text (p. 119).

Conspiracy theories created by whom? Ironically, of course, the text where the 'early' Kāśī appear is not in the RV itself, but in Talageri's late- or post-Vedic list, which he wrongly assumes is as old as the RV itself.

In order to evaluate the information in the Anukramaṇī, given its late/post-Vedic nature, it has to be compared with the various late-Vedic lists of Gotras and Pravaras in the Śrautasūtras, which trace the origin of Gotra names to many of the same Ṛṣis who show up in the Anukramaṇīs (cf. J. Brough 1953, not mentioned by T.). When making such comparisons, one can deduce some of the political and social reasons why such lists appear, and why they differ substantially among themselves.

More often than not, we find again that Late-Vedic Brahmanical rivalries underlie specific attributions -- as I have pointed out in relation to political "adoption" schemes (Witzel 1995, 1997; cf. Thapar 1984). Similar transformations can also be traced in the "historical" sections of the Purāṇas. It is therefore no surprise that genealogies suggested in different Anukramaṇīs agree with those found in the similarly late Śrautasūtras or even later Purāṇas. But all this has little bearing on genuine RV genealogies, since traditions obviously repeatedly shifted in the long centuries between the RV and late-Vedic periods (Witzel 1995: 339 sq.).

Against this background it hardly surprises us that Talageri finds in the Anukramaṇīs exactly what he hoped to find there: confirmation of Epic-Purāṇic data! By contrast, my own Ṛṣi lists in my 1995 paper were based on the RV itself, not on such outside information: the Anukramaṇī was cited there only as one, actually the last of the several means to assess RV traditions.

To crown it all, as earlier suggested, the Sarvānukramaṇī of Kātyāyana that T. uses throughout -- without identifying it by name or further discussion -- is the younger one of our two preserved versions of that text. This was shown way back in 1922 by Isidor Scheftelowitz (*Zeitschrift für Indologie und Iranistik*, 1, 1922, p. 89-90). Scheftelowitz also gave us the first edition of the RV Khilas, which is preserved today only in Kashmir. In his 1922 study, he demonstrated that the Kashmir version of the Sarvānukramaṇī as well preserves a version of Kātyāyana's text that is shorter and much older than the normal, received version. Scheftelowitz also provided evidence that the longer version used by Talageri --which the latter fantasizes goes back to RV times-- may date no earlier than the middle of the first millennium CE! (On this see further M. Tokunaga 1997: xv, xliv, lii.)

§ 3 Victorian Sanskrit?

I noted at the start of this review that Talageri shows no evidence that he possesses anything remotely approaching an adequate knowledge of Sanskrit -- not to speak of the archaic and enigmatic forms of Ṛgvedic Sanskrit. He depends instead on a dated English translation and a modern Sanskrit word list -- scarcely adequate tools to approach India's most ancient text. Talageri does not admit his linguistic deficiencies, of course, but they are nonetheless immediately evident in his frequent misreporting of Ṛgvedic phrases. Thus his book consistently gives us the Sandhi variant *vara ā pṛthivyā* (RV 3.23.4) instead of the correct non-Sandhi form *vara ā pṛthivyāḥ* ("at the best place on earth"), the way the expression is reported at RV 3.53.11-- but not by Talageri (cf. p. 115, 136, 210, etc.).

Similar mistakes are made (p. 117) in regard to *nābhā pṛthivyā* at 1.143.4, etc. for the correct *nābhā pṛthivyāḥ* -- which again occurs in non-Sandhi form in 3.5.9. Even more glaring are Talageri's *chanda* for *chandas* "meter" (frequently, e.g. on p. 3), and his misreporting of *Aprī-sūkta* for *Āprī-sūkta* (p. 21 ff.). And so on throughout his book.

The translation that Talageri adopts as his authoritative text, as already noted, is Griffith's Victorian version, which was first published in 1889. Next to the even more antiquarian one by Wilson, Griffith's is the only complete version of the work readily available to English speaking readers without Sanskrit. Serious researchers would be expected to consult the far more accurate scholarly translations made by K.F. Geldner (1951, German), L. Renou (1955-1969, French) and now T. Elizarenkova (1989-99, Russian). Talageri has not consulted any of these, nor (lacking German) Oldenberg's still unsurpassed *Noten* (1909-12), which deal with each hymn and verse in the book.

Every legitimate scholar knows that blindly using any translation -- let alone one as inadequate as Griffith's -- can easily lead one astray. Talageri, however, defensive about his dependence on the text, goes out of his way to praise it as "the best, most complete, and most reasonably honest English translation to this day" (p. 339).

As noted earlier, how someone who is incapable of reading an ancient text in the original is capable of making such judgments remains a mystery. Pace Talageri, the RV is a highly technical text composed in an archaic literary tradition that is still poorly understood -- and whose poetic forms are very imperfectly captured by Griffith. The forms of the RV are not those of the later *Kāvya* style but of those prevalent in the preceding Indo-Iranian and Indo-European periods. To date, those forms have not been adequately described as a complete system. Typically enough, Talageri ignores all the detailed work that has been conducted over the past 200 years in this direction. He dismisses all such research as the disdained product of "the scholars" -- the academic philologists and linguists who serve as straw men throughout his book. Meanwhile he pretends to proceed as his own man -- despite his dependence on Griffith's ancient translation! -- "invincible" in interpretation, as he often suggests.

§ 4 Failures in the 'Petty Conjectural Pseudo-Science' of Linguistics

Serious Vedic scholars need a thorough knowledge of the archaic forms of Sanskrit (Old Vedic) and closely related languages in order to correctly interpret obscure words, metaphors and similes, grammatical forms, etc., unknown in later Vedic texts. Frequently by comparing the RV with kindred documents, like the Avesta, or with other old Indo-European texts, new insights arise concerning its more obscure passages.

While Talageri obviously is no Old Vedic scholar, he is not altogether anti-linguistically minded (p. 309, 412, 415) -- unlike many of his colleagues in what he refers to as the "the Voice of India family of scholars." Rajaram, as noted before, denounces linguistics outright as a "petty conjectural pseudo-science," without understanding its theoretical basis and certainly without any knowledge of its procedures.

Linguistics is a "hard science, certain so far as sounds are concerned (the linguistic subfield of phonetics). Sounds are, after all, produced by physical instruments at various positions in the throat, mouth and nose. Grammatical formations and the syntax of particular languages are for the most part produced subconsciously by native speakers, but they follow certain abstract rules (so well defined by Pāṇini for Classical Sanskrit). It is strange that many Hindutvavādins want us to believe that linguistics (minus Pāṇini of course!) is

not a science. Deciding linguistic questions becomes more difficult when it comes to items of meaning and when linguistic reconstruction of pre-historic meanings are concerned. But even here, we can argue within the realm of probabilities and do not have to resort to fantasies of the sort found in Talageri, which are discussed below.

While Talageri's attitudes towards linguistics are more favorable than Rajaram's, he too has little theoretical grasp of its principles and is inept in carrying out linguistic investigations. His lack of linguistic training is devastating, since linguistics is vitally important to nearly all the questions that he treats in his book (see below on river, place and tribal names!).

Let us look here at a single example, involving his interpretation of the female name "Jahnāvī" (pp. 99-100; cf. also pp. 111-112), which he wants us to believe is another name for the Ganges river. Talageri's object is to use the word to support his claim that the RV originated in the East, in the Ganges region. This is one of the few places where he disagrees with Griffith's translation, which clearly conflicts with that view.

Talageri's discussion here is so revealing -- and convoluted -- that it is worth quoting in detail; my comments are added between brackets:

Jahnāvī, which is clearly another name of the Gaṅgā, is named in two hymns; and in both of them, it is translated by the scholars as something other than the name of a river: Griffith translates it as "Jahnu's children" (I.116.19) and "the house of Jahnu" (III.58.6). The evidence, however, admits of only one interpretation:

a. Jahnāvī is clearly the earlier Rigvedic form of the later word Jāhnavī: the former word is not found after the Rigveda, and the latter word is not found in the Rigveda.

The word clearly belongs to a class of words in the Rigveda which underwent a particular phonetic change in the course of time: Jahnāvī in the Rigveda becomes Jāhnavī after the Rigveda; brahmāṇa becomes brāhmaṇa in the Rigveda itself (both words are found in the Rigveda while only the latter is found after the Rigveda) [I fail to find brahmāṇa in the RV; T. or his source apparently misunderstood a plural form of brahman]...; and the word pavāka has already become pāvaka in the course of compilation of the Rigveda... the actual pronunciation of the word pāvaka must have been pavāka in the Rigvedic age [an old chestnut: see the 19th cent. discussion of RV diaskeuasis].

b. The word Jāhnavī and therefore also the word Jahnāvī ... literally means "daughter of Jahnu", and not "Jahnu's children" or "the house of Jahnu" [but see below!] and ... has only one connotation in the entire length and breadth of Sanskrit literature: it is a name of the Gaṅgā.

c. One of the two references to the Jahnāvī in the Rigveda provides a strong clue to the identity of this word: Jahnāvī (I. 116.19) is associated with the śiṃśumāra (I.116.18) or the Gangetic dolphin. The dolphin is not referred to anywhere else in the Rigveda. [But T.'s Gangetic dolphin is also found in the Indus river! And RV 1.116.18-19 are not as closely connected as T. wants us to believe; this is part of along 25-verse list of the miracles of the Aśvins.]

The three different examples given by T. have three different linguistic explanations and cannot be used to support his claims: his claim that RV Jahnāvī > Post-RV Jāhnavī is demonstrably false (see below); his second example involving brahmāṇa is fatuous, since that word is not found in the RV at all; and his third case, involving the phonetic change pavāka > pāvaka is a peculiarity of recitation schools. T.'s "linguistic explanation" of the word Jahnāvī thus is empty.

The meaning of that word can, however, be explained along simple linguistic and grammatical lines as follows: female derivatives of masculine names often have vṛddhi in the second last syllable; thus Manu : Manāvī, Agni : Agnāvī -- and consequently, Jahnu : Jahnāvī (cf. also analogous formations, such as Indra : Indrāṇī, Varuṇa : Varuṇānī, etc.) That is all there is to it. Consequently, "the scholars" who followed older translators or even Śāyana were closer to the truth than T. Jahnāvī was the wife or a female relation of Jahnu or otherwise connected to him or his clan. The "ancient home" (purāṇam okaḥ) specified here, which T. thinks is the "Ganges," is the territory of the Jahnu clan, whose location is unknown, that also figures prominently in later post-RV Vedic texts (AB, PB).

To turn the word Jahnāvī into a name for the Ganges can be done only by retro-fitting the RV evidence to Epic-Purāṇic concepts or to Talagerian conceits of a Gangetic (Uttar Pradesh) homeland of the RV and of the Aryans/Indo-Europeans (T., 1993). In short, Jahnāvī "Ganges" is not found in the RV. This robs T. of one of his important pieces of "evidence" for a Gangetic home of the RV (for more, see below, s.v. Pramaganda).

In sum, Purāṇic preconceptions, coupled with an obvious lack of grammatical and linguistic expertise, deliver for T. what he promised that he would deliver in the book's preface: "this detailed analysis of the Rigveda emphatically confirms our theory."

Not in this case, and not in the important ones mentioned above and still further below.

§ 5 Trita's View From Inside the Well? Other Missing Sciences

The quality of Talageri's research is no better when it comes to discussions of other fields that should have been included or consulted when he prepared his book.

Little use is made of the secondary literature, even with the rich Bombay University library at hand. T., however, mostly used what was readily available and -- as he tells us at the start of his book -- what his Western helpers, such as K. Elst, sent him. It is clear that he used a very haphazard selection of sources. As was early noted, those sources excluded the most important works for the present undertaking, including studies of the structure of the RV by Oldenberg (1888) and of the various Anukramaṇīs by Macdonell (1886) and Scheftelowitz (1922), not to speak of more recent ones such as Tokunaga (1997). Nor did he consult any post-Victorian translations of the RV -- not even the inadequate partial one by O'Flaherty 1981 (Penguin), or even any one available in various Indian languages!

In fact, leaving aside his sketchy and highly selective discussions in Chapter 8, the history of RV research does not exist for T. (In chapter 8, 'Misinterpretations of Rigvedic history', p. 335-424, he discusses a number of opinions, but the chapter is primarily arranged by figure or supposed "school" -- mainly focusing on supposed "invasionists" -- and not by subject, and is hardly comprehensive).

T. usually refers derisively to past and present research, blanket style, as the work of "the scholars" (implying that he is not one?!). When he actually mentions the work of earlier scholars he tends to jumble their research together with very recent work, as if the state of the art and the opinions of the 19th century were identical to those of the year 2000. This is a favorite tactic of the present rewriters of old Indian history -- meant to demonstrate "contradictions" in Indology.

In Talageri's discussions, large sections of relevant fields of the humanities and sciences remain untouched, although they have a direct bearing on the subject under discussion. A few examples here will illustrate the ways that T.'s judgment is hampered by a lack of information.

His discussion of the "kingdoms" of his time frame of 3500-1500 BCE is not tempered by discussion (or apparent knowledge) of semi-nomadic, transhumance life style or the workings of early pre-state tribal societies.

Nor does he display any familiarity with the realia needed to interpret a difficult text such as the RV. As noted earlier, all notions of RV poetics and their impact on interpretation are absent, since T. is only familiar with Griffith's Victorian translation. He does not know about geographical facts such as the nature of Panjab rivers and their constant building of natural dams (RV 7.18). In his interpretation of Jahnāvī (p. 100, RV 1.116.19) as a supposed name of the Ganges, he thinks that the matter is clinched by the fact that a dolphin is mentioned in an adjacent(!) stanza, RV 1.116.18, given in a long list of miracles of the Aśvins (see above). However, the river dolphin (*Platanista gangetica*) is not just found in the Ganges but also in the Indus river, as a simple check of any encyclopedia would show (see the Encyclopedia Britannica, which he actually lists in his references), s.v. dolphin, susu, e.g., <http://www.britannica.com/bcom/eb/article/0/0,5716,108400+14,00.html>.

Archaeology is largely ignored by Talageri as well. If he had consulted any standard studies he would have found that all through the time frame that he assigns to the RV (3500-1500 BCE) his supposed Aryan homeland around the Ganges was exclusively inhabited by hunters and gatherers and by some scattered chalcolithic agriculturists -- with no sign of great Pūru and Bharata "kingdoms." The same applies to the absence of horse and chariot in the Gangetic basin during this period -- a topic that Talageri wisely never brings up. Nor is the Indus civilization (2600-1900 BCE) discussed at length. It covered at least the Western extremities of his imaginary "Westward march" of the Vedic tribes during T.'s late RV period, 2100-1500 BCE. Since this does not fit, we are informed that the Indus people were Anu, -- Iranians, in T.'s opinion (p. 41), as we shall see below.

The section on comparative mythology, p.478 sqq. is seriously misinformed as well. Discussions of the problems in it would lead us far astray here, but the topic may be taken up at a later occasion. The list of such cases could be easily amplified.

Finally, there is the silly but infuriating use of irregular abbreviations of book titles which have to be learnt and re-learnt on any use of the book. This leads to obvious problems for anyone who wants to track down Talageri's amateurish uses of sources. What is ZTR or ZTH? ZTH is R. Gnoli's "Zoroaster's time and

homeland".... The same annoying baffle gab is also found, for example, in S.S. Misra's book "The Aryan Problem" (1992). In the age of computers, these acronyms could easily have been converted into something more recognizable, throughout the whole book, and within minutes. Amusingly, HINDUTVA (given just like this -- all in caps!), a book by the nationalist politician V.D. Sarvarkar, who closely worked with Italian and German fascists, is not further abbreviated in T.'s bibliography -- indicating, in an almost Freudian way, the bent of mind of the author under discussion here. *Nomen plenum est omen.*

A short list of obvious omissions relevant to study of the RV, a few of which we have already noted, includes the following. The list could easily be expanded:

- * The dates provided by the Old Indo-Aryan words in the Mitanni documents of c. 1400 BCE are not mentioned anywhere in Talageri's book; the forms of these words are slightly older than the corresponding forms in the RV (ma-aš-da [mazd[h]a] for medhā, vaj'hana > vašana- [važhana] for vāhana. They certainly do not support the fantastically "hoary" chronology developed in Talageri's book.
- * The evidence of archaeology is neglected for areas inside and outside the Indus Civilization. As mentioned, it is certainly difficult to picture "Vedic" hunter-gatherers and early chalcolithic agriculturists in an Gangetic Pūru "kingdom"! Talageri nowhere mentions the obvious conflicts in his work with accepted archaeological evidence.
- * Much other critical zoological and archaeological evidence is not discussed: horse and two-wheeled chariot are prominent at all levels of the RV, but are not found in South Asia before c. 2000/1700 BCE, more than 1000 years after T.'s early RV; river dolphins in the Indus are unknown to T.; and so on.
- * There is complete absence of discussion of social questions in the RV: "kings" in the semi-nomadic (only very partially village-based) society portrayed in early strata of the RV? Vedic "dynasties" reigning for millennia? What kind of "state" is represented by the RV hymns? Any search of the vast comparative literature on semi-nomadic peoples would have ruled out much of Talageri's fantasies.
- * There is no discussion of climate in the book. All of the RV indicates the presence of cold winters, a prominence of long dawns, and river flooding due to snow melt. All these conditions are typical of conditions in the Panjab, not of the more southern and warmer Gangetic plains under the heavy influence of the monsoon.

§ 6 Imaginary Chronologies: Ṛsis of the Kali Yuga?

Any evaluation of T.'s grand effort becomes even more devastating once the results of his discussions are pulled together and it is shown how procedurally erratic they are.

We can begin by looking at T.'s conclusions as to the time period of the RV. Based on his analysis of the post-Vedic Anukramaṇīs and related data, T. attempts to interpret each maṇḍala as a unified text (with the exception of book 1, which he subdivides into three phases, and to some extent book 10). Each book, on his view, can therefore be studied as an individual corpus of one or more clans belonging to one time period. References from these 10 books are then taken to establish the time and the place of the author(s) of the individual hymns and of the whole book. All of them taken together are used to delimit the area of the RV and of the relative time frame of the whole corpus and its parts.

It should be clear even to a superficial reader that T.'s rigid division of the RV into 10 books belonging to unique time periods contradicts well-known evidence, some of which we noted earlier.

Further, several Maṇḍalas overlap in time, as illustrated by the fact that they often mention the same chieftains (and sometimes, the same poets).

One is due for a surprise when one examines T.'s treatment of these facts. The "family reminiscences," such as RV 3. 53, and the similar late, additional family hymns (as per Oldenberg 1888) RV 6.47 and 7.33, clearly

show historical awareness in such clan traditions. However, the poets often do not say more than "we compose in the way of the Atri (Atri-vat)," or they may refer back to poets of the mythical past (Aṅgiras, Uśanas, etc.).

While we would expect, based on the internal evidence of the RV (Witzel 1995), a period for each family book that extends over roughly five generations -- with a few references thrown in to ancient predecessors and a few late (post-RV) additions -- T.'s schemes calculate the RV books each in term of many hundreds or even thousands of years (p.75 sqq.) :

It is clear that the Rigveda was not composed in one sitting, or in a series of sittings, by a conference of ṛṣis [echoes of my 1997 paper - MW]: the text is clearly the result of many centuries of composition. The question is: just how many centuries?

The Western scholars measure the periods of the various Maṇḍalas in terms of decades, while some Indian scholars go to the other extreme and measure them in terms of millenniums and decamillenniums.

A more rational, but still conservative [sic!- MW], estimate would be as follows:

1. There should be, at a very conservative estimate, a minimum of at least six centuries between the completion of the first nine Maṇḍalas of the Rigveda and the completion of the tenth.

2. The period of the Late Maṇḍalas and upa-maṇḍalas (V, VIII, IX, and the corresponding parts of Maṇḍala I) should together comprise a minimum of three to four centuries.

3. The period of the Middle Maṇḍalas and upa-maṇḍalas (IV, II, and the corresponding parts of Maṇḍala I) and the gap which must have separated them from the period of the Late Maṇḍalas, should likewise comprise a minimum of another three to four centuries.

4. The period of Maṇḍalas III and VII and the early upa-maṇḍalas of Maṇḍala I, beginning around the period of Sudās, should comprise at least two centuries.

5. The period of Maṇḍala VI, from its beginnings in the remote past and covering its period of composition right upto the time of Sudās, must again cover a minimum of at least six centuries.

Thus, by a conservative estimate, the total period of composition of the Rigveda must have covered a period of at least two millenniums [sic!- MW]."

It is, incidentally, altogether unclear how T. arrives at these estimates. If the composition of the RV stretched out over more than two millennia, the text would be in the same language (except for some innovations in the late book 10). This is a virtual impossibility as far as any living language is concerned. Since T. assumes "at least 600 years" for the period of composition between RV 9 and the end of 10 (p. 77), we thus would arrive at 3500-2100 BCE for the bulk of RV composition.

See here <http://www.safarmer.com/pico/talageri.html>, put together by Steve Farmer. Farmer attaches Talageri's own dates, provided in an online exchange on 21 July 2000 (for Talageri's own words, see the bottom, of the webpage) to an originally dateless chart that Talageri gives in his book. Here are the "conservative" dates and "lower limits" -- outrageous by normal historical standards -- that Talageri provided in that exchange:

Mandala 6: 3500-2900 BC,
 Mandalas 3, 7, early 1: 2900-2700 BC,
 Mandalas 4, 2, middle 1: 2700-2400 BC,
 Mandalas 5, 8, 9, late 1: 2400-2100 BC,
 Mandala 10: 2100-1500 BC.

In other words, the RV would be contemporaneous with the early village-like predecessors of the Indus civilization at Harappa itself. As far as T.'s U.P./Bihar "homeland of the RV" is concerned, the text would have evolved right among the hunter-gatherer bands of that area. That country did not see forts (pur), chariots, and horses for another 2000 years -- while the RV is full of them.

Further, by 3500 BCE (the date for his oldest book, RV 6) horse-drawn chariots were not even invented (even the bullock cart was only first appearing in Mesopotamia about that time). Chariots only appear more than a thousand years later both in Mesopotamia and in the S. Russian-W. Siberian steppes. There also was no horse in S. Asia then either -- the "indigenous" Siwalik horse (*Equus sivalensis*) by that time had been long extinct. The modern horse (*Equus caballus*) first appeared, imported from Central Asia (Bökönyi 1997; Meadow and Patel 1997), around 1700 BCE (Pirak, Kachi plain in easternmost Baluchistan).

But these glaring anachronisms do not pose a problem for T. All such historical, technological, zoological, and archaeological details do not detain him, since they are simply disturbing little facts; all of this will be discussed in further detail further below.

In the same vein, T.'s conclusions on "Kings and Ṛṣis" (p. 59 sqq.) must be regarded with utmost caution, based as they are on his impossible view of the RV's internal chronology.

For example, as soon as one applies Oldenberg's well-known principles in detecting late interpolations in the RV (i.e., looking for late violations of the original numerical order of the family books: see § 1 supra and my letter in the IndicTraditions list on 11/12/2000), one can simply forget the Purāṇa-assigned "Anu kings" of 6.45-46 -- where they appear in suspicious, additional hymns. (For T.'s Purāṇic mindset, cf. p. 138 sqq.). The same applies to the Yadu "king" Vītahavya(?) at 6.15.2-3, where he is not a chieftain but the poet of the hymn, and in 7.19.3 (T. has a misprint) where the word does not seem to designate a "King" but is an adjective referring to the Great Chieftain Sudās.

Not surprisingly by now, T. arranges his list of "Kings" by following the list found in the Purāṇas (cf. Morton Smith 1973: 504). Talageri tells us: "The names of these Kings are given above in order of their relative positions in the dynastic list" (p.60). Then, he sets out to prove that the Purāṇas, after all, are "right" as per his 1993 book. Tacitly following my 1995 historical paper, the close connection of Sudās with books 3 and 7 is accepted, as well as the tenor of book 6 where the prominence of Sudās' father Divodāsa points to a slightly earlier time frame. However, the conclusion that Talageri draws from these data (p. 62) -- that all Maṇḍalas after 6, 3, and 7 are "post-Sudās" is again unwarranted.

There is no way that any RV book can be that easily declared "post-Sudās," since each book contains heterogeneous material from several or even many generations of poets. Only after the multi-axial investigations mentioned early in this paper (Witzel 1995) will have been carried out, will statements of this sort have a chance to be substantiated.

Once again the principle "garbage in, garbage out" applies to Talageri's work. The chieftains of the RV are closely linked to certain priests' and poets' clans, according to T.'s traditional sources, and the hymns of these clans are concentrated in certain books. Therefore, the moment one (arbitrarily) orders these books in a fixed temporal pattern, guided by the Anukramaṇīs and Purāṇas, etc., the "kings" and poets must come out in the same order as that of the books. In consequence, T.'s whole "historical analysis" is wrong, based, as it is, on the use of such late sources and his rigid maṇḍala scheme, which ignores the temporal layering of individual texts (see above). Someone else has to do this exercise all over again, taking into account not just traditional Ṛṣi names and Maṇḍalas set in stone, but a multitude of parameters (Witzel 1995).

Not surprisingly, T. ends up with a list of kinds that more-or-less agrees with those found in the Purāṇas (p. 63). A little fudging helps, as a check of Sudās' ancestors in T.'s list (p. 63) indicates (cf. Witzel 1995). Morton Smith who worked in the footsteps of Pargiter's Purāṇic studies, often comes to conclusions radically different from the ones in T. Smith's comparison of epic, Purāṇic and Vedic sources (1973) is never mentioned by T. It is instructive to observe how T. has to twist and turn in order to explain, within his own erroneous framework, "the Ṛṅkṣi dynasty" (p. 66-72). Facts at all times in Talageri's work have to agree with his theory, not the theory with the facts!

§ 7 Talageri's Geography: A Moveable Feast

T.'s theory places Indo-Aryans impossibly in the Gangetic plains before they moved Westwards into the Panjab and beyond. It has already been shown (§ 4) that some of his "Eastern" data in the RV (Jahnāvī as Ganges) evaporate as soon as we take a close look at his historical ordering of the text and his faulty grammatical analyses. The same applies to other river names. I restrict myself here to the ones found in what T. claims, on flimsy grounds, is the RV's "oldest book," RV 6.

One can immediately throw out the reference to the Ganges that appears at RV 6.45.31 (gāṅgya). This reference occurs in a series of long hymns to Indra 6.44-46 which follow a shorter Indra hymn (6.43 of 4 stanzas) and precede the appendix hymn 6.47. Applying the principles pioneered by Oldenberg, RV 6.45 can be shown to be a composite hymn built out of Ṛcas at an uncertain period. The ordering principle of the old family books clearly points to the addition of all these hymns in mixed meters at the end of an Indra series. Such late

additions must not be used as an argument for the age of the bulk of book 6 (see my letter in IndicTraditions from 11/12/2000).

Incidentally, it should be noted that such Tṛca and Pragāthā hymns are not listed in the short list of additions that I gave in my 1995 paper. As I noted in the paper, however, they are prominently discussed by Oldenberg (1888). T. again falls prey to his lack of knowledge of Indological research -- not understanding my reference to Oldenberg -- when he accused me of inconsistencies in the nature of that list.

The River Sarasvatī found in book 6 (T., p. 102) may be discarded just like T.'s Gangetic Jahnāvī. In the hymns 6.49, 50, 52, 61, the order of arrangement is disturbed and especially the group 6.49-52 is very suspicious. According to the RV ordering principles, we expect 5 hymns for Sarasvatī, and do not obtain them even by a dissection of hymns 51 and 52. All this points to an addition of materials at an unknown time. Therefore, the Haryana River Sarasvatī (mod. Sarsuti) is not found in the old parts of book 6. Incidentally, it is entirely unclear that the physical river Sarasvatī is meant in some of these spurious hymns: in 6.49.7 the Sarasvatī is a woman and in 50.12 a deity, not necessarily the river (Witzel 1984). (At 52.6, however it is a river, and in 61.1-7 both a river and a deity -- which can be located anywhere from the Arachosian Sarasvatī to the Night time sky, with no clear localization).

Interestingly, however, the other -- i.e. Western -- rivers remain in the older parts of RV 6, such as the Yavyāvati and Hariyūpīyā: they still point to Eastern Afghanistan, to the river Zhob, and (perhaps) the Hali[-āb], as the location of these parts of the book.

It would lead us too far afield here to discuss all of T.'s identifications of rivers. Among the more ludicrous ones are precisely the last ones, the Hariyūpīyā and Yavyāvati. Talageri writes (p.98 sq.): "Hariyūpīyā is another name of the Dṛṣadvatī [in eastern Kurukṣetra, just west of Delhi - MW]: the river is known as Raupyā in the Mahābhārata, and the name is clearly a derivative of Hariyūpīyā. The Yavyāvati is named in the same hymn and context as the Hariyūpīyā, and almost all the scholars agree that both the names refers to the same river."

The only item agreeing here is hari "tawny, etc." = raupya "golden." RV 6.27 mentions the Hariyūpīyā in the context of the Vṛcīvants and the Pārthava, -- uncommon Western names for the Ṛgvedic E. Panjab/Haryana. Of course, Hariyūpīyā cannot be, as it is often alleged, the origin of the name of Harappa: medial -p- should have long disappeared, via -v- and zero, and cannot have resulted in double p.

Mercifully, T. does not mention this. In fact, Harappan Civilization is almost completely absent in his book, only noted in passing in discussing some other scholars' views. According to his time table of the RV, however, the Ṛgvedic period overlaps exactly with the later parts of the Harappan civilization -- which he fantastically describes in one of his few references to it as "a joint civilization of the anus (Aryans belonging to the same linguistic stock as the latter-day Iranians and some other Indo-European groups)... and the Pūrus (the post-Rigvedic Vedic Aryans), even perhaps more anu than Pūru, at least in the case of the more well-known western sites" (p. 419). This neatly shifts the evidence away from the RV -- according to T. a Pūru text of the Gangetic Plains.

Unfortunately for Talageri, the mature Indus civilization covered all of RV territory, as it extended from E. Afghanistan to Haryana and Western U.P, and from the Himalayas down to the Indus delta and Kathiawar in Gujarat. His knowledge of the Indus Valley Civilization is hence as misinformed as his knowledge of RV culture.

A few more curious points from his discussions of river names: Prayiyu and Vayiyu in 8.19.37 actually are (non-Indo-Aryan) men of the country of Suvāstu (mod. Swat, just east of Afghanistan) -- not rivers at all, as Talageri assures us (p. 102)! For Śveti RV 10.75.6 (in T. p. 103), read Śvetyā! Talageri lists Aśmanvatī as an "Eastern" river (p. 98), but in RV 10.53.8 (p. 103) it is probably not a river on earth at all but a river in the night time sky (Witzel 1984).

But what casual reader of Talageri's book could be expected to pick up on these points without spending weeks or months tracking down Talageri's spurious "evidence."

Incidentally, the Avestan river "harōiiu" (map, opposite p. 104; for details on these Afghani rivers, see Witzel 2000) is a nonce word, a pseudo-stem, derived by T. from the Acc. harōiium; the stem would be *haraēuua-, but to know that you would need some linguistics! The map opposite p.120 gives a few more Avestan names, often in wrong form: read Maryu for Mourv, Haraēuua for Harōiva, Airiianəm Vaējah for Airyana Vaējah; the latter Talageri improbably locates in Kashmir while the Avestan texts make it the central land of E. Iran -- i.e. the cool, central mountain pastures of Afghanistan (Witzel 2000). Also, note that Kīkaṣa, following 19th c. guesses, is located by T. in Bihar, while the RV Gandhāri -- here with correct spelling -- are located in Arachosia! However, on p.131 Talageri quotes the AV as identifying the Gandhāri with the Mujavants, which is

not correct as these two (and the Kāśī, Aṅga) are mentioned in parallel fashion as distant tribes, seen from the point of view of the central land of the Kuru (Witzel 1980).

T. thinks that such linguistic distinctions as mentioned just now are mere nitpicking -- again to his undoing. Speaking about Northwestern rivers, he tells us that "three of them, Kosala, Śutudrī and Kubhā are clearly Indo-European names ("the hairsplitting about the letter -s- in Kosala is a typical "linguistic" ploy....)" (p.248). However, even a brief look at the discussion of these words in Mayrhofer's etymological dictionaries (1956-1976, 1985-, unknown to T.!) and or my detailed study of Vedic river names (Witzel 1999) would have told him otherwise. There is nothing "clearly Indo-European" about them and words with *is*, *us*, *es*, *os* etc. are extremely rare in Vedic and stand out as loan words.

The summary of river names and their location given on pp. 103-112, and the inherent "Westward expansion" of the Indo-Aryans in RV times is of course based on the philological and linguistic mistakes criticized above (§1-4) T.'s conclusions (exemplified in his table and insert on p.104) are therefore moot. We need a fresh investigation of references to rivers in the RV. The results should be close to those found in Witzel 1995,1999 which was based on text-internal evidence.

T.'s lack of a linguistic background becomes painfully obvious when we consider his absurd analyses of RV place names. Since T. believes that all through the early Vedic period the Proto-Indo-Europeans were present in northern India, the Mundas in eastern India, and the Dravidians in southern India (but see Kuiper 1991, Witzel 1999a), he is not surprised that the northwestern part of the subcontinent shows predominantly Indo-Aryan names during the Vedic period. Relying solely on my short 1995 paper, which provided only a terse summary of this topic, he takes the prominence of Indo-Aryan names in North India as a proof for an Indo-Aryan (and Indo-European) origin there and for their early settlement in northern India. Along the way, he excoriates me for failing to note what he considers to be that obvious conclusion (p.248). However, like most of my 1995 paper, the views presented here were just a short summary of a broader base of evidence. A much fuller treatment of Vedic place names can be found in my 1999 paper, which came out one year before T.'s book.

The evidence assembled there suggests exactly the opposite of what T. assumes: Although the Northwest generally abounds in Indo-Iranian names, the remaining indigenous, non-Indo-Aryan names are of special interest. Their sounds and their word formations clearly show that they belong to a non-Indo-European, Greater Panjab substrate (Kuiper 1991, Witzel 1999a). This evidence renders all talk of an original Indo-European settlement of northwestern/northern India moot.

In addition, the recent discussion of the substrate words common to both Indo-Aryan and Iranian (Witzel 1999a, Lubotsky forthc.) adds substantial new evidence for this view. Such common non-Indo-Iranian words differ from the typical Ṛgvedic and post-Ṛgvedic substrate and indicate that both the Proto-Indo-Aryans and Proto-Iranians, perhaps even the speakers of Proto-Indo-Iranian, entered a Central Asian/Afghan territory that was already occupied by a previous population speaking a non-Indo-European language(s) (pace J. Nichols!) -- most probably the language(s) of the Bactria-Margiana Archaeological Complex (BMAC).

8. Have Words, Will Travel!

Talageri's lack of linguistic knowledge gives him a permit to accept whatever pleases his fancy when it comes to etymologies, using these Nirukta- or Kratylos-like fantasies as grounds for his fanciful "historical" deductions. These deductions are legion, especially in the later parts of the book, where unchecked etymological speculation abounds and stands proxy for legitimate historical information. A few examples should suffice.

On p. 119, the Kīkaṭa chieftain Pramaganda (see Witzel 1999, 1999a) is identified with the country of Magadha in S. Bihar. However, the hymn in question, RV 3.53.14, clearly speaks of Kurukṣetra and surroundings, some 750 miles to the west. It refers to the performance of the Aśvamedha (3.53.11) after Sudās' victory in the Ten Kings' Battle (7.18; cf. Witzel 1995), to which this late Viśvāmitra hymn of family reminiscences seems to look back.

But Magadha is not related in any way to Pra-maganda, at least not in Sanskrit, as T. tries to insinuate; for Austro-Asiatic possibilities, however, see Witzel (1999a). To equate magadha and -maganda in Sanskrit is folk etymology, as virtually no part of both words can be explained as Indo-Aryan (there is no root *mag* and no suffix *an-da* or *a-dha*, etc., see Witzel 1999). Yet T. draws wide-ranging historical conclusions from his folk etymologizing -- in the best style and tradition, incidentally, of his "scholars" of the 19th century!

On p. 113, T. connects the tribal name Gandhārī 1.126.6 (correctly: Gandhāri, and 1.126.7!) with that of the semidivine beings, the Gandharva, and just stops short of producing an etymology. On p. 133, T. produces the folk etymology Kaśyapa ~ Kaśmīra (< Kaśyapa-mīra!) and draws far-reaching conclusions from this: he makes the "Kaśyapas of Kashmir" (which is not mentioned before Patañjali, 150 BCE!) the original Soma priests who "entered the Rigveda at a late stage" (p.134). In reality, the name Kaśyapa "tortoise" is inherited from Indo-Iranian (Avestan kasiapa, cf. the modern Kashaf Rud on the Iran-Afghan border). This goes back to the Central Asian substrate word *kac'yapa (Witzel 1999a, Lubotsky forthc.) and has of course absolutely nothing to do with Kashmir.

In addition, Soma, a plant of the high Iranian, Pamir and Himalayan mountains -- especially that of Mūja-(vat); cf. Avestan Mūža -- originally had a Central Asian name as well (amśu). Indo-Iranian *sau-ma, with regular development (au > o) to Ved. so-ma and (s > h) to Avestan haoma, Old Persian hauma, is a simple and rather descriptive derivative of su "to press." (Incidentally, the discussion of Soma by T. also contains a lot of misunderstanding of Indo-Iranian mythology, and a page or so of free association on early Vedic history, p. 136.) In the face of all of this, then, how is it possible that "the evidence in the Rigveda thus clearly shows that the Vedic Aryans did not come from the Soma-growing areas bringing the Soma plant and rituals with them" (p.135)? T., as a linguist, does not notice that the name of Soma and its formation precede the Vedic period: they were already Indo-Iranian.

The following "etymologies" and "historical" conclusions based on them can only to be described as free association. The name of the Pūru tribe is explained by T. as the origin of the word for "man," puruṣa (p.147), like manu > manuṣa. A glance into Mayrhofer's dictionaries would have supplied T. with a legitimate range of explanations for the multiple Ṛgvedic and later Middle Indo-Aryan forms of puruṣa / pūruṣa, etc. Similarly, the name of the ārya he explains as being identical with Irish Eire, while this goes back to the equivalent of Ved. pīvarī "the fat (country)." Or, note the impossible Alina = Hellenes (but Gr. h- < *s- !) on the map opposite p. 264. T.'s equation of the names of the Paṇi and the Germanic gods, the Vanir, is of the same amateurish etymological "quality." Like S.S. Misra's reconstructions (1992), regressions to the early 19th century! These are outmatched in absurdity only by his treatment of comparative Indo-European mythology.

The animals of the RV do not fare better under T.'s charge. On pp. 120-136 he deals with words for the elephant, tiger, etc., all of which he declares to be "purely Aryan" (p.122) or to have "a purely Aryan etymology." He gives four words for "elephant," ibha, vāraṇa, hastin and sṛṇi (RV books 1, 4, 6, 8, 9, 10). Of these, ibha has been discussed for more than a hundred years, although T. is not apparently aware of the discussions. Geldner, Mayrhofer and others are not even sure whether one or all cases of ibha actually refer to the elephant. T., however, imagines even "perhaps a temple elephant" in the RV -- another first, "Vedic temples"! His kings, dynasties, and temples look as though they were taken from the popular comic books, Amar Chitra Katha, rather than from the Ṛgveda. The etymology of ibha is equally uncertain. The connection recently proposed by Gamkrelidze and Ivanov, with Greek ele-phas etc., suffers from irregular sound correspondences. What is i-bha in Indo-Aryan, "this + animal"?

Both vāraṇa and hastin are obvious new formations: vāraṇa (mṛga), if it refers to the elephant indeed, means "impetuous (wild animal)"; and hast-in clearly is the (animal) characterized (-in) by a hand (hasta). Finally, sṛṇi refers to a sickle, not an elephant. Does Agni have an elephant as his tongue (RV 1.58.4)?

Again, we notice that we have to countercheck T.'s data at every step. Further, the word for "peacock," mayūra, has been given various etymologies, from Dravidian to Munda. But neither its root (Skt. mā "to bellow?") nor its suffix (yū-ra??) nor its word structure (mā > ma? + yū-ra??) is Indo-Aryan, as T. has it.

Finally, on p. 298, T. declares the words for rhinoceros, khaḍgī [read khaḍga! --MW] and [the post-Vedic(!)] gaṇḍa to be "derived from purely Indo-European roots"! The retroflex sounds alone should have created enough suspicion of substrate origins to check an etymological dictionary before making statements as the ones above (see M. Mayrhofer's etymological dictionaries, 1953-1975, 1986-96).

In sum, when T. tells us (p.123) "The combined evidence of river-names, place-names and animal-names gives us a single unanimous verdict: the Vedic Aryans were inhabitants of the interior of India, and their direction of expansion was from the east to the west and northwest," his verdict is based on the erroneous approaches and methodologies, or rather the lack thereof, criticized above.

§9 Westward Ho! Talageri as Patriot

The conclusions in the final chapters of the book are based on the sorts of "evidence" critiqued above, and it would be pointless to discuss them at length. Much of them are simply repeated from T.'s 1993 book. However, a few glaring points will be selected for comment.

Chapters 5-7 of T.'s book ("The Historical Identity of the Vedic Aryans," "The Indo-Iranian Homeland," and "The Indo-European Homeland," on p.137-331) are the outcome of the preceding chapters 1-4. The results cannot be better than the those of the preceding chapters. In fact, those results only get worse.

To begin with, T. describes what he represents as the dominant model of the influx of Indo-Aryan and Indo-Iranian speaking semi-nomadic tribes into Greater Iran and Greater Panjab in simplistic 19th-century terms as the movement of "a people in South Russia; one branch of these Indo-Europeans, the Indo-Iranians, migrated towards the east and settled down in Central Asia; much later, one branch of these Indo-Iranians, the Indo-Aryans, migrated southeastwards into the northwestern parts of India; and thus commenced the story of the Aryans in India" (p.137).

Modern linguistics, archaeology and philology have modified this simplistic picture significantly, especially in the last few decades. We now tend to speak of transfers of ideologies, subsistence systems, language, and spiritual culture from one group to the other (Ehret 1988) as often as movements of people. Such processes do not necessarily involve large-scale migrations, although actual physical movement and intermarriage are not excluded. Various types of military interaction, such as cattle rustling (*gaviṣṭi*, RV), raids, actual war-like clashes (*saṃgrāma*), battles (*raṇa*, *yudh-*) and even the now much-despised incidental invasion of smaller or larger bands, groups or tribes may or may not be part of the picture.

Incidentally, since Talageri's preface expressively proposes the Hebrew Bible as an example of a violent invasion scenario, we can point out that such an image is a mirage. Palestine archaeology does not suggest that "invasions" and large-scale destructions took place in the way they are described in the Torah: Biblical scholars, like Indologists, long ago replaced 19th-century invasion theories with acculturation models. All of this, of course, is lost on T., who claims that Western scholars continue to apply what he mistakes as modern biblical models to the case of India. The case of a mythical Yayoi-time "invasion" of horse riding warriors into Japan and their subsequent spread up to Central Japan (Yamato) -- again, so clearly described in the old Japanese *Kojiki* and *Nihon Shiki* -- is similar. In all cases an acculturation scenario applies (Witzel 1997).

Further comparative studies in historical times, say of the movements of the Germanic or Slavic, Arab or Turkish tribes, would show that many variations of transfer and actual takeover were possible. In any study of early S. Asia the stress must be on a multiplicity of ways in which the transference of the parent Indo-European and Indo-Iranian spiritual cultures (mythology, rituals, poetics, etc.) and material cultures (chariots, horses, etc.) took place to other groups, including those that first appear in the RV as *Ārya* tribes.

Nothing of all of this is seen in T. Instead, he simply tells us: "But all this is the version of the scholars. As we have already seen, the scholars are wrong in their fundamental proposition that the Vedic Aryans moved into India from the northwest. They are also wrong in their conclusions about the historical identity of the Vedic Aryans" (p.136).

What Talageri would replace this with is his own Purāṇa-inspired "history." On p. 154 we get T.'s Eire "Ireland" deriving from *Ārya*, as "different Indo-European peoples were each, individually and separately, calling themselves by this particular name." However, he restricts the use of the word "*Ārya*" to the *Pūru* tribe, while all other Indo-Aryans are supposed to be the *Dāsa* of the RV. The chapter involves a lot of detailed attributions of tribes, dynasties, and priests that need to be tediously checked against legitimate evidence item by item, like every other detail in the book. The whole is a house of cards, being based on the skewed analysis of the RV analyzed above.

When T. comes to the home of the Indo-Iranians, he repeats: "the identification of Central Asia as the location of this common Indo-Iranian habitat and of South Russia as the location of this common Indo-European habitat are purely arbitrary hypotheses with absolutely no basis in archaeology or in written records."

In voicing this typically strong-worded opinion, T. does not take into account the evidence of the introduction into Proto-Indo-Iranian ritual of Soma, of its actual physical location in the Central Asian high mountains, or of its distribution among the Saka (*Haumavarga*), the Avestan Iranians, and the Old Persians as well as the *Ṛgvedic* tribes (Staal, *forthc.*). Also ignored are the copious data of a common Indo-Iranian language, mythology, ritual, etc., which have recently been supplemented by the discovery of a common Central Asian linguistic substrate both in Old Indo-Aryan and in Old Iranian (Witzel 1999a, Lubotsky *forthc.*). Sometimes, the

same words can be found twice in slightly different forms, which points to a persistent local C. Asian substrate -- not, as T.'s theory would require, just a local Panjab one (for details see Witzel 1999a, 2000, *forthc.*). The evidence suggests that various Indo-Iranian tribes entered a non-Indo-European speaking area, Bactria-Margiana, and brought new local loan words, taken over there, with them into Iran and the Greater Panjab.

Though T. uses Avestan texts in translation (p.178 *sqq.*), his use of them is far from sophisticated. Here too, his lack of linguistic training shows. We find the same kind of easy folk etymologies criticized above. The Vedic *Angiras* thus become Avestan *Angra* (correctly Avest. *aṅra*, Old Avest. *anra* "hostile" -- which would correspond to Ved. **asra!*). And so on.

T. also discusses the famous list of sixteen Iranian countries of the *Vidēvdād*, closely following G. Gnoli's interpretations (Gnoli 1980). However, these are flawed in many of their details (Witzel 2000), and so is T.'s conclusion (p. 199) that "the Iranians ultimately originated either in southern Afghanistan itself or in areas further east.... analysis and description of Avestan geography clearly suggest that the antecedents of the Iranians lie further east..." -- that is, in India, of course! T. opts for the Panjab, as *Haṭpa Həṇḍu* is mentioned in the *Vidēvdād* (actually, as the second least desirable of sixteen countries, since it is "too hot" for comfort!). Like Gnoli, he has not understood the structure of the *Vidēvdād* list. T. selects, out of the five major RV tribes, the Anu as the Iranians living in the Panjab side-by-side with their more eastern neighbors, the Indo-Aryans: "the Punjab (*Saptasindhu* or *Haṭpa-Həṇḍu*) was not a homeland of the Vedic Aryans, but was a homeland of the Iranians" (p. 203). But Iranian has none of the local Panjab and U.P. loanwords that are found in Vedic, which means that the Old Iranian languages just cannot come from the Panjab (Witzel 1999a, *forthc.* EJVS 7-3).

The discussion of the Iranians leads T. to that of the Ten Kings Battle, in which the Anu (his Iranians) took part. It is here that the transition from fantastic etymologies to fantastic "history" most fully takes shape. Of the participants in the battle, T. identifies the *Pṛthu* with the Parthians (attested only much later in Iranian history!); the *Parśu* with the Persians (although the word is the predecessor of *Pashtu*, and the *Parśu* are the neighbors of the *Gandhāri* and *Arāṭṭa*; see Witzel 1980, 1997); the *Paktha* with the *Pakthoons* (although this is just a modern dialect form of *Pashtu*, and *Paktha* is, per K. Hoffmann, the ordinal number "fifth"); the *Bhalānas* with the *Baluchis* (although these Western Iranian tribes appear in present Baluchistan only about a thousand years ago!); the *Śivas* with the *Khivas* (by which he understands the 19th c. Kingdom of Khiva in Uzbekistan!); and the *Viṣāṇins*, identified, for no good reason at all, with the "Pishacas (*Dards*)."

As if these identifications were not ridiculous enough, he goes on with even more fanciful etymological links with even more distant peoples: The RV *Bhrgus* are the *Phrygians* (in modern NW Turkey!); the *Śimyu* are the "Sarmatians (*Avesta Sairimas*)"; and the *Alinas* are the *Alans* (in the Central Caucasus)! In the same vein, he turns a poet, *Kavaṣa*, into the King of the Anu -- whom he further identifies with the famous Avestan figure *Kavi Kavāta* (correctly, *Kauui Kauuāta*) and the *Purāṇic Kekaya/Kaikaya*!

Give Talageri one consonant, and he will identify. How did Voltaire reportedly put it? "Consonants count little, vowels nothing."

Talageri then (p. 208 *sqq.*) lets the Anu migrate from the Panjab and Kashmir (= *Airiianəm vaējah*, actually Central Afghanistan, Witzel 2000) to Iran and Central Asia. In passing, he identifies the Iranian Yima's mythical underground *Vara* fortress with RV "vara ā pṛthivyā" (p.210), although the Old Iranian *Vara* myth has close links with the corresponding *Nuristani* tales and the *Ṛgvedic Vala* myth.

In sum, Talageri thinks that "the records of 'the later Vedic period' show that the Vedic Aryans and the Iranians were located in an area stretching from (and including) Uttar Pradesh in the east to (and including) southern and eastern Afghanistan in the west" (p.229). He laments that "the scholars, however, are not accustomed to deriving conclusions from facts; it is their practice to arrive at conclusions beforehand (the conclusion, in this particular case, being based on an extraneous, and highly debatable, linguistic theory about the location of the original Indo-European homeland), and to twist or ignore all facts which fail to lead to this predetermined conclusion."

Of course, T. (1993, 2000) himself does nothing of this kind!

The last 100-page chapter on the Indo-Europeans (p.232-331), mostly repeated from his 1993 book, has even stranger tales to tell. His motto appears to be: The further from Mother India, the more fantastic things become.

In brief, according to T., one of the five major tribes of the RV, the *Druhyu*, made it all the way into Europe: they constitute all other Indo-European peoples. He sums up his work in his inimitable style: The "case for an Indian homeland is so strong," he tells us, "and the case for a non-Indian homeland so weak, that, in spite of any number of academic dictators decreeing 'under pain of (academic) death' that the Indian homeland theory

be abandoned without serious examination, or with only perfunctory and determinedly skeptical examination, the academic world will ultimately be compelled, nevertheless, to accept the fact that the Indo-European family of languages originated in India" (p. 331). (For a brief refutation of this Hindutva "pet idea," see Hock 1999 and Witzel forthc.)

If T. wants to convince "the scholars" of his ideas, he has to be a trifle more rigorous than he is in this book. His book may have convinced the convinced, and he may even have convinced a large(?) section of the present generation of Indian scholars (cf. Bryant 1999) and of the Indian population inside and outside the country ("anti-Nehruvian", "anti-Marxist" and "pro-Hindutva", or whatever they may call themselves these days), but 10-20 years from now, all these views will be found in the dustbin of history. That is, unless the present trend continues of official support for such ideas, and a whole generation of school children is brought up on this unsavory nationalistic brew -- which continues with proposed Ph.D. programs in "Scientific Astrology."

This brings me to T.'s underlying political agenda. I have intentionally not discussed it so far, but have preferred to let his "facts" speak for themselves. Talageri's "scholarly" agenda is laid out clearly in both in his 1993 and 2000 books.

In our earlier book, we had taken up the subject of the Aryan invasion theory in all its aspects, and conclusively established that India was the original homeland of the Indo-European family of languages. However, this second book has become imperative for various reasons: 1. The literary evidence for our conclusion in our earlier book was based primarily on Purāṇic sources. ... the Rigveda is the only valid source for the period... this detailed analysis of the Rigveda emphatically confirms our theory... the actual data in the Rigveda shows that they [the Vedic Aryans] were in fact inhabitants of the area to the east of the Punjab, traditionally known as Āryāvarta. The Punjab was only the western peripheral area of their activity. ...

This book is, therefore, an answer to criticism: it shows that a detailed analysis of the Rigveda, far from weakening our theory, only makes it invincible. (p. xvii sq.).

In sum, T. is an amateur with an obvious agenda as well as a patently absurd thesis, expressed in his 1993 book, that he desperately needs to support. According to T., the RV is the oldest Indo-European text and contains the oldest Indo-European mythology. It was brought westwards by the Indo-Europeans who moved from the Gangetic Motherland out of India in several bands, the last of which were the Iranians. Much of T.'s two books merely restate, with the addition of Epic-Purāṇic legends, what S.S. Misra (1992) and many others before him had expressed. Talageri's work, like so much "revisionist" historical writing today, is nothing more than a cottage-industry exploitation of a current political trend. The over-confident, indeed "invincible," conclusions that T. draws concerning the Westward drift of Vedic tribes are familiar Hindutva fantasies.

T.'s scenario is, of course, not one suggested either by linguistics nor archaeology nor any other of the historical sciences. Talageri pretends to know the direction, time frame, and sequence of these imagined Indo-European emigrations from the subcontinent -- all drawn from a dated Victorian translation of a text that T. himself is incapable of reading in the original. Curiously, in speaking of his imagined migrations into Europe from the subcontinent, he again mixes Ṛgvedic information with Purāṇic data. The emigrants to Europe are given the Ṛgvedic name of Druhyu, which according to his reading of the Purāṇas were expelled from India: "The Purāṇas (Vāyu 99.11-12; Brahmāṇḍa III.74.11-12; Matsya 48.9; Viṣṇu IV.17.5; Bhāgavata IX.23.15-16) record: Pracetasah putra-śatam rājānāḥ sarva eva te, mleccha-raṣṭrādhipāḥ sarve hyudīcīm diśam āśritāḥ" (p. 260). Never mind that udīcī diś "the Northern direction, the north" usually refers just to the Northwest of the subcontinent (cf. AB, Pāṇini, etc.), not to Iran or Europe. Forget too that the criticism of the Northwest, especially the Panjab, is made from the point-of-view of the post-Ṛgvedic orthoprax Kuru-Pāñcāla center, and is prevalent already in ŚB, Patañjali and Mbh. -- and not only in the medieval Purāṇas.

Based on the Purāṇic statement quoted above -- expressed in the mid to late first millennium C.E.(!) -- T. constructs a detailed scenario of an invasion(!) of Europe, three thousand years earlier, by the Druhyu. (This word means, literally, "the ones who seek to cheat." Non-linguist as he is, T. missed a great chance for a "socio-ethnic" study based on an etymology!)

Talageri's historical fantasy is, not unexpectedly, the exact reversal of the century-old theory of an Aryan immigration into the subcontinent. Just as some Indian writers from the late colonial period found Indian "colonies" all over South-East Asia (again neglecting the various modes of cultural transfer!), the present generation finds Indians and Indian civilization all over the "West" (and occasionally at the root of Chinese civilization as well).

Post-traumatic stress and therapeutic writing, half a century after decolonialization? Westward ho!

Eke vadanti, vitathaṃ tv evāhaṃ bravīmīti ha smāha Śrīkaṇṭhaḥ...

I would be remiss if I failed to mention at this point the bellicose tone of Talageri's own book. The study is written throughout in a tone of "rightful outrage" against 19th-century colonialism, as though that were still an active force today. It shows, on every page, apocalyptic certainty and self-assured conviction -- as though the world had been saved by the "invincible" case he was making of an Indian home of all Indo-Europeans. Throughout T. expresses himself in a familiar scholastic pāṇḍita style. Every minor flaw in the wording of an "opponent" is leapt on -- hairsplitting and ignoring inconvenient arguments -- to "win" the case. In short, "winning" minor arguments appear to be more important for him than getting at the truth underlying it.

All this detracts from his announced undertaking, which supposedly involves a dispassionate attempt to uncover the history encapsulated in the RV's dominant ritualistic poetry. But a dispassionate search for truth is hardly part of Talageri's agenda.

More often than not, T.'s polemics border on the farcical. He accuses the Iranist Skjaervo of mentioning the "Seven Rivers" of the *Vidēvdād* but then of "pointedly avoid[ing the] mentioning anywhere that this refers to the Punjab ... since it runs counter to the Theory" (p.184). T. forgets the fact, mentioned later (p.230), that Skjaervo's teacher Humbach holds a different opinion about the location of the Seven Rivers (upper Oxus). Skjaervo apparently did not mention this simply because the work used by T. was a brief summary (perhaps also as to not commit *gurunindā?*). Instead of accepting the obvious, however, T. discovers conspiracy theories everywhere.

T.'s case of India as an "original homeland of the Indo-European family of languages" (p. 232 sqq.) often reads like a parody of the work of "the scholars." By "the scholars," as suggested earlier, he primarily has in mind "Western scholars" -- as if a division into "Indian" and "Western" is possible in the 21st century! He writes, in his typical polemical fashion: "The scholars, however, are not accustomed to deriving conclusions from facts; it is their practice to arrive at conclusions beforehand" (p. 229). In his biased view, "the scholars" are "staunch followers" of the (so-called Aryan Invasion) Theory who occasionally "admit" to certain facts -- that is when they honestly mention certain facts that may allow for other interpretations, or that may even speak against their own proposal or theory.

T.'s polemics against those who do not automatically treasure the "hoary" traditions that he imagines for ancient India come in for special scorn. An enumeration of such passages would fill many pages. Page after page we find ranting against "(Western) scholars" -- a term clearly built on the present Indian (right wing) denouncement of the "eminent historians" of Delhi. Occasionally, however, T. lapses into "a bored yawn" (p.344).

Some disagreeing Indian scholars, even K.D. Sethna, otherwise called "the *Bhīṣma Pitāmahā* of Indian historians," i.e. of the present wave of revisionist writers, are not spared either (p. 423). And note, "There is even an extreme lunatic fringe ... A political "scholar," Rajesh Kochhar, as part of a concerted campaign... He does this under cover ... This is the level to which "scholarship" can stoop, stumble and fall" (p. 96-97).

Talageri has reserved special invective for me (p.425-476, to which criticism I will come back later) as I had dared, in 1995, to denounce, in passing, his 1993 book as nationalistic. Indeed, it is a chauvinistic fantasy, on the same lines as the second part of the book under review here. Read, e.g., this characterization of my work (my italics, p.121): "a modern Western scholar, a staunch and even fanatical supporter of the Aryan invasion theory... tries generally to twist and distort the information in the *Rigveda*..." .

And so on and so forth.

All of these less-than-desirable traits further detract from a supposedly dispassionate study of the invaluable historical evidence locked away in the RV. T. may actually have thought he was setting out to find the

truth, but due to his obvious ideological leanings and his lack of expertise in many fields, he set himself up to find precisely what he wanted most to find: evidence of an Indian homeland for all Indo-European, Indo-Iranian, Indo-Aryan, and RV tribes. That vision may be psychologically and even spiritually satisfying for some, as visions of the sort found in *The Bible was Right After All* (Keller 1956) might be for some "Westerners." However, both visions remain fantasies.

While it is always justified to look again at the sources and begin to re-evaluate history, even of a remote period as that of the Veda, the present revisionist Hindutva wave has other aims in mind. An obvious goal is to display the "hoariness" and uniqueness of ancient-most Indian culture and its imagined importance for the rest of the world.

Against such a background, no cultural innovation and certainly no trickling in, immigration or "invasion" from the outside is allowed. Everything created by "Indian" civilization for the past 9000 years or so, beginning with the early agriculturists of Mehrgarh in Baluchistan(!), has been local and no (major) influences from the outside can be tolerated. This, of course, would make it the oldest tradition on the planet: The RV, to recall Talageri's words, "is the oldest and hoariest religious text of the oldest living religion in the world today: Hinduism." Underlying these claims is the familiar Hindutva agenda that suggests that all non-Hindus are ultimately "foreign" peoples in India, and a blot on the body politic.

One culture (Vedic), one language (Sanskrit), one people: Bhārata ueber alles!

And, in spite of certain well-attested cultural influences (e.g. in astronomy!), of repeated immigrations and of actual invasions -- from the Old Persians and Greeks to the Huns, Turks and Moghuls and the interaction and acculturation that all such political developments brought with them.

In other words, history is written with an ulterior motive in mind, that of "nation building." Facts count little, dates nothing!

The sources of the revisionist writing of history and of the present Hindutva boom generally go back to Dayanand Sarasvati, Shri Aurobindo and other (religious) leaders of the 19th century, but over the past ten to twenty years, the range and number of such productions has risen sharply. Publishing companies such as Voice of India or Aditya Prakashan specialize in such books, and more traditional publishers, for whatever reasons, chime in. There exists a closely knit, self-adulatory group now, members of whom often write conjointly and/or copy from each other. An incomplete but typical list would include: Choudhury 1993, Elst 1999, Danino 1996, Feuerstein, Kak, and Frawley 1995, Frawley 1994, Kak 1994, Klostermaier (in Rajaram and Frawley 1997), Misra 1992, Rajaram 1993, 1995, Rajaram and Frawley 1995, 1997, Rajaram and Jha 2000, Sethna 1980, 1981, 1989, 1992, Talageri 1993, 2000. Others fill the Internet with statements ranging from half-way scholarly opinions to inane accusations and outright slander. A cultural war is in full swing.

The various Hindutva-inspired proposals for a new history of ancient-most India do not present a cogent picture (see next issue of EJVS). They neglect the linguistic evidence, and they run into the serious chronological and geographical difficulties described above. They distort the textual evidence of the RV to make it appear to jive with Harappan town civilization with its stratified society and international maritime trade. They rewrite the literary history of the Veda to fit in improbable dates for the composition of most of its texts. They like to maintain an Indian homeland for Indo-Europeans, despite the fact that early South Asian loan words are entirely missing in all other Indo-European languages but are copious in (R̥g)Vedic.

In sum, in spite of his scholarly pretensions, Talageri remains an amateur historian with an obvious political and cultural agenda and a patently absurd thesis. As ancient history, his thesis is a failure; as a political attack on "Western" scholarship, it does not fare better. T.'s motives are revealed in this quote in his preface from Vivekananda: "the more the Indians study their past, the more glorious will be their future, and whoever tries to bring the past to the door of everyone is a benefactor of the nation." -- Foremost among these "benefactors," we find, "being the Voice of India family of scholars who will ever remain the intellectual focal point for exercises in rejuvenation of the innermost spirit of India" (p. xxii). Such "benefactors" include, of course, Talageri (1993) himself, who also figures in the ultra-nationalistic website HVK (Hindu Vivek Kendra), <http://www.hvk.org/> -- which is well worth a glance. Wehret den Anfaengen!

§10 Marching Backwards into History

It has become abundantly clear by now that T.'s book is more "historical fantasy" than "historical analysis." As such, wide acceptance of its ideas would hamper the study of the RV and of the oldest Indian history, at least in the Indian popular mind, for a long time to come. It certainly does not do what he pretends to do at the start: to exclusively follow the evidence of the RV wherever it goes.

This was my own course of action when I wrote the paper that initially triggered Talageri's polemics (Witzel 1995). As I emphasized then, reconstruction of the historical data locked in the RV demands the systematic collection and analysis of many types of data scattered in the text. That summary paper was a first attempt to give a comprehensive view of the historical conditions underlying the RV. In it I outlined some of the methodological principles underlying such a reconstruction. Some further principles have been added since, and are noted below.

A careful comparison of T.'s parameters will show that he surreptitiously took many cues from my 1995 paper and adopted something crudely approximating the methods suggested then. But -- angry as he was at me for criticizing, in passing, his 1993 book, -- he never clearly says so in the first part of his new book. On the contrary, in the very first sentence of his book he claims: "I have little to acknowledge to anyone (except, ... the modern scholars responsible for fundamental books on the Rigveda, such as Ralph T.H. Griffith and Viśvabandhu...), since this section is almost entirely a product of my study."

Surprisingly then, in the second part of his book, T. initially praises my approach (p. 434 sqq.), before he enters into an extended vociferous, and sometimes abusive, critique, based as it is on the wrong use of methodologies, as discussed above.

The many methodological and factual absurdities in Talageri's book are obvious now. They can be easily confirmed by any scholar who bothers to identify the late- and post-Vedic origins of T.'s data. But all this will not be so obvious to less specialized readers, including the Indian lay persons constituting Talageri's main audience.

That is why we cannot let books like T.'s slide by, as Indologists tended to do with similarly absurd works produced a few decades ago. The Rajaram's, Frawley's, and Talageri's of today, unlike most of their predecessors, are driven by a religious/political agenda that could poison serious historical discussions for decades to come. The potential effects on contemporary Indian society of that agenda evoke even worse premonitions and fears.

A better understanding of the history of the Ṛgvedic period is only possible when all the paths described elsewhere (e.g., Witzel 1995) have been diligently pursued. The path is a long one, but promising early results have already come in, confirmed as well by independent research -- e.g., by Th. Proferes (1999) and J.R. Gardner (1998).

In the future, what we really have to do is to apply the lessons learned over the past few centuries concerning the interpretation of texts in a large variety of cultures. We must learn the lessons of philology, which might be defined as "the study of civilization based on its oral and written texts." In doing so we have to be aware, as I stressed in 1995 (another point overlooked by Talageri), of our own individual biases or historical conditioning, whether reflecting a "Western," Indian, or other context. It is important, in the case of the RV, that we free ourselves of the "confident reliance on just a few translated Vedic texts and the unwarranted reliance on [the much later] Epic and Purāṇic texts... which also by their very nature (as bardic compositions) cannot correctly reflect the historical facts of a period thousand(s) of years earlier" (Witzel 1995). All this, of course, is precisely what Talageri never manages to do.

The parameters to be used in an approach to Ṛgvedic history were clearly spelled out in my 1995 paper. After listing the problems in approaching this archaic poetical text and its ritualistic and mythological background, I pointed out the several historical layers, the geographical and tribal divisions, and the great mobility of the authors. I also added a list of study parameters -- many if not all of which are "echoed" (if only in a distorted way) in T.'s initial chapters some five years later!

How does the saying have it? "Plagiarism is the best form of flattery"? I quote from my 1995 paper (307 sq.), which detailed these parameters:

A) The structure of the RV itself, with its relative order of hymns ...

- B) The relationship of the various tribes and clans to the books of the RV ...
- C) The authors of the hymns: deduced from occasional identification of themselves, from the patterns of refrains which act as 'family seals', and from the traditional attribution of hymns to certain authors in the Anukramaṇī.
- D) Geographical features, especially rivers and mountains.
- E) This information can then be combined in a grid of places, poets and tribes.
- F) Finally, this grid can be combined with a chronological grid established on the strength of a few pedigrees of chiefs and poets available from the hymns.

I continued that "to all this may be added data from linguistic investigations, as well as cultural data from the text on religion, ritual, material culture, local customs, etc...." in order to "construct a multi-axial grid with variables of time, space and social situation. Once that grid is plotted ... we may begin the writing of R̥gvedic history."

This still is where we stand now, due to the failure of T.'s book, and my final clause still applies as well: "To my knowledge this has never been attempted in detail; nor is it the principal aim of this paper: rather, I shall try to lay the essential groundwork for the undertaking of this exercise and relate some initial results in conclusion."

I have since collected more data in all these areas as well as in some others (meters, substrate words and linguistic influence, grammatical innovations such as the absolutive, dialect forms, archaisms, etc.). The investigation is ongoing and will take time. Writing criticisms, such as the present and similar ones, represents a tedious, if necessary detraction from the main goal. Modern computer programs, though, help in organizing and accessing such multi-axial data as described above (see J.R. Gardner, EJVS 6, 2000).

Only a full investigation involving not just poets, chieftains ("kings") and geography but also many other parameters will lead us to a fuller picture of the RV, its structure, time frame and geographical settings. Until then, we only have some incidental investigations, many of them misleading (Bhargava 1971, Chauhan 1985, and of course, T.). My mention of these writers does not mean that others not discussed here are infallible. I have a few problems with R. Kochhar's analysis (1999), and I certainly do not exclude my own past work from criticism. Apart from many printing mistakes in my 1995 paper (and its companion piece from the same volume; see my comments, 1997: 262), I have also since changed my opinion, based on new evidence, about the relative date of the bulk of RV 2, which I would now include in the mid-level texts. As I have indicated elsewhere, the 1995 papers were first drafts (stringently summarized and edited by G. Erdosy), based on many still unpublished notes; these papers will be reprinted in the future in corrected versions. A full treatment, based on some two dozen parameters, would probably differ in details, though I do not expect it, so far, to differ from the main outlines given in my 1995 papers.

To repeat it one last time: once the text-internal parameters, especially those of RV structure, have been studied, the rest should follow fairly easily -- that is as easily as this difficult, archaic, and highly poetical text will allow us. Solid reconstruction of the history hidden in the RV demands linguistic rigor, independence from Purāṇa-like world views, and above all a kind of political integrity. Serious attempts to understand ancient Indian history are badly diverted by the intellectual detours we are sent on by books like Talageri's. The facts should be allowed to speak for themselves and to lead us -- not a pre-enlightenment agenda.

Reconstruction of the historical world of the RV is being carried out individually and by a loose group of international scholars who share their data and the electronic tools that help us access them. Those who are interested in this kind of collaborative approach are welcome to approach the present author.

Even "ideology" must not be a hindrance. I recall with pleasure my meetings at Hoshiarpur with the late Acharya Vishva Bandhu, a "staunch supporter" of the Arya Samaj, whose great 16-volume Vedic Word Concordance is an excellent tool for research, created in the age of pencil, paper and type setting. It also is largely free of religious and traditional bias.

This shows that real cooperation is possible. It is possible for us to work together in interpreting Vedic texts objectively, without imposing on them extraneous ideas. We may hope that the increasing spread of

electronic media may allow more and more scholars and lay persons to share in this discovery and to check for themselves where grand claims are made and tall tales are told.

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