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The Rig-Veda and the Avesta, The final evidence.

Talageri, Shrikant G. 2008. New Delhi: Aditya Prakashan.
(http://www.vedamsbooks.com/no59975.htm)

Reviewer: Arnaud Fournet.

[Editorial note: This review was originally written from May 18 to May 22, 2009. It was first published on *scribd* where it was read more than 1500 times. Afterwards it was proposed to a major Indo-Europeanist Journal, which first declared to be interested but later on remained unresponsive. This version of the review is marginally modified in order to follow the style sheet of the *TMCJ* and is included here as a *free paper*.

The book actually does not fit the scope and purposes of the *TMCJ*. On the whole the book and the review are interesting because the theory developed by Talageri is creationist and fixist. It negates the most basic features and principles of historical linguistics and of Indo-European studies in general, in spite of claiming to address the issue of the Proto-Indo-European homeland. Its virtues are therefore indirect: it helps figure out how a fixist and creationist theory tries to address issues that are usually dealt with in an evolutionary and historical framework.

As regards the contents of the book, needless to say that the theory developed by Talageri fails to deliver any insight on Indo-European issues and that the more one gets acquainted with so-called "out-of-India" theories (OIT) the less they appear to present any credibility.]

PART 1 *Original review*

1. *Introduction*

The book is the third one written by Shrikant G. Talageri and is dedicated to analyzing the Rig-Veda, its contents and its relevance, as regards the original location of the Proto-Indo-European (PIE) homeland. The book contains 379 pages and the following sections : contents, bibliography, preface, two main sections, postscript, index. The book does not have an explicit conclusion. The flaps of the cover could be considered to be part of the book as they provide some information, such as a partial inventory of the Indo-European languages and the issue of the PIE homeland, something that is done indirectly in the preface of the book itself (p.XXXII-XXXIII). The book is intended as one more piece added to the idea that the PIE homeland should be considered to be (the north-western parts of) India [Editorial: in fact Pakistan], according to what is called the 'Out of India Theory' (OIT) by the author and the other OIT supporters. It can be added that the same name is used by the non supporters to describe the OIT. The book does not require any prior reading of the two other books by the same author, which were on the same topic. The copy, received 05/18/2009, was sent by Koenraad Elst, a personal friend of the author, after I accepted his proposal to (try to) review it. For the sake of courtesy, I had proposed that my review could be read by the author before being made public, but this proposal has been rejected by K. Elst. I have never had direct contact with the author. Apart from being one more piece of pleading in favor of the OIT, the book is at the same time another intellectual *passe d'armes* between the author and his personal favorite duelist, Michael Witzel, one *Notoire* of Vedology, as de Gaulle would have put it. Citations of the book are written in the exact typography of the author, especially as regards the words stressed in italics. The only change applied is 'Rigveda' >

Rig-Veda, which seems to be a more regular spelling. Mr. Shrikant G. Talageri is called the author in the review, unless a disambiguation is necessary.

The first contact with the book has reminded me of a Sanskrit grammar I bought in China some years ago and which is still my main source on that language: FanYu KeBen. The size, the smell, the pages, both whitish and yellowish, have kindled the same impression. The readers of the present review must be aware that I am not a specialist in Vedic or Indo-Iranian studies, but my reckless and curious mind has prompted me to accept that perilous task. They must be aware that I am not an OIT supporter either. Before reading the book, I had about no expertise on the OIT, apart from the vague idea that the OIT tries to promote India as a possible homeland of the Proto-Indo-European language. I first started to read the book and simultaneously write the review. After a while, I realized that the task would be more complex than I may have thought. In fact, I realized that the book is much more than a pleading for India as PIE homeland and that it would require to be read more than once to achieve a real and thorough understanding of its contents, concepts, purposes and nature. Ultimately, this review of the book results from four waves of reading, during which I have successively discovered the explicit contents, the implicit framework, the key words and the political vested interests of the author's version of the OIT. During these four waves of reading, I have written in the present review that my self-imposed goal was to be, successively, as descriptive and neutral, as descriptive and empathetic, as descriptive and anamnetic, as possible, when one has to review a book on such a theme as the Proto-Indo-European homeland, which seems to have become highly polemical and politicized in India. In the course of reviewing the book, in the middle of the reading of section 2, I realized that the self-imposed goal of remaining *neutral* made increasingly no sense. I erased *neutral* and chose *empathetic*, because this word expresses open-mindedness without hostility or assent. After that, a deeper understanding of the way the author uses some key words and of their real meanings and implicit presuppositions made it clear that the word *empathetic* may be misinterpreted as a kind of implicit assent. I then opted from the somehow psychoanalytical *anamnetic*, which expresses my distantiated conviction that I have reached deeper and deeper layers of the mental construction of the author's OIT: the explicit contents, the implicit framework, the key words and the political vested interests. During that process of anamnesis of the author's version of the OIT, I have been successively disconcerted, assiduous, amazed and frightened. Because my initial and contractual task was to review the book, and nothing else, the present review is focused on the explicit contents, the implicit framework, the key words of the author's version of the OIT. For the same reason, I have avoided to make any comment or review of the citations of authors quoted by Mr. Talageri. I have also refrained from making a review of the author's self-appraisal of his own theory, which is often stated in the book. As far as is possible, I have avoided to resort to adjectives and adverbs, because they convey implicit value judgments, more often than is intended. I have also avoided to resort to [sic] in the quotations. I believe the readers of my review will be able to perceive the reasons of my selection. A possible fifth wave of reading could have been to suggest emendations to the version of the OIT proposed by the author. But as I do not support the OIT, and most probably never will, this would make little sense. I hope that my review is accurate enough for people to assess what the book is, when they have not read it themselves.

2. *About the Bibliography*

The Bibliography comes after the Contents and has two conspicuous features. It is very short for such an issue as the PIE homeland. This situation is explained by the author: 'The *only* books in my bibliography are those books actually quoted by me, and those referred to in any significant context.' (p.XXXVII) It must nevertheless be noted that Oldenberg (1888) *Prolegomena* are discussed and cited in Chapter 4 but do not appear in the bibliography. Another feature is that it contains very few works

with a real linguistic content. Paradoxically, (historical) linguistics is nearly completely absent in a book that claims to deal with the issue of the PIE homeland. Moreover the bibliography contains such works as Chang, 1988, *Indo-European Vocabulary in Old Chinese: A New Thesis in the emergence of Chinese Language and Civilization in the Late Neolithic age*, Sino-Platonic papers, whose scientific status is exceedingly low in my politely worded opinion. The years of publication of the 73 references listed in the bibliography are: before 1906 *7* books, between 1907 and 1985 *14*, after 1986 *52*. I cannot believe that so little worth quoting has been written during the 80 years from 1906 to 1986 on the issue of the PIE homeland. What is more, 23 out of the 52 modern references are either from Talageri himself or from Witzel. The bibliography is therefore an implicit indication of the goals and intrinsic limitations of the book. It can also be noted that some books have been selected and quoted more or less extensively because they agree with the author. From the textual and argumentative point of view, this practice adds nothing real and could be entirely avoided. It amounts to pro domo propaganda. On the contrary, I would have appreciated to see what evidence in the Rig-Veda substantiates the claim of 'a mighty Sarasvatī in full powerful flow.' (p.115) Be it right or wrong, and I have no opinion, such a claim requires to be duly documented and proved by a philological analysis, and this analysis is lacking.

3. *About the Preface*

The Preface (21p) actually starts on page XVIII and not XV as indicated in the Contents. From a sheer textual point of view, the preface includes a listing of 'errors' and 'mistakes' made in the author's previous works and the explanation of the graphic conventions for the transliteration of Sanskrit and Avestic words. This could have been preferably located somewhere else, after the bibliography for example. The meaning of these errata in the preface seems to be that the author has made his own mea culpa and that other people, presumably non OIT supporters, should do the same. The preface is mainly an autobiographical account of the author's framework, convictions and contacts with some other people, especially Kazanas and Witzel.

The first page of the preface contains the pivotal claim of the author 'I can *confidently* (opponents will say, *arrogantly*) say that this book will set the seal on the controversy, and prove beyond any *reasonable* doubt that India was the original homeland of the Indo-European family of languages.' (p.XVIII-XIX) This is what the OIT is about, as described above. It can be underlined that the wording is 'homeland of the Indo-European family of languages' and not '(Proto-)Indo-European homeland'. The reasons for that contortioned wording will appear gradually. The author mentions the word 'Proto-Indo-European' only once, when referring to Hock's works: 'the Proto-Indo-European language (as much ancestral to Vedic as to the other ancient Indo-European languages)' (p.210). This hapax word is not listed in the index. The author claims to have found the location of something that he about never describes by its name. On the second page, the author opposes the OIT theory with what he calls the AIT, the Aryan Invasion Theory. It must be first emphasized that there are several competing theories about the PIE homeland, other than the OIT, which differ both in datation (from the Paleolithic to the early Neolithic to the late Neolithic) and in location (from the North Pole to the Balkans to Southern Russia to Anatolia). What the author (and presumably the other OIT supporters) calls the AIT is to be understood as one of the mainstream theories: the one which describes a homeland in the Pontico-Caspian area in Southern Russia and a dispersal of the original community around -4000 BC. The bibliography includes two books: from Mallory, who supports this Pontico-Caspian homeland[1], and from Gamkrelidze & Ivanov, who support Eastern Anatolia as original homeland. Talageri seems to be unaware that his short bibliography includes two works proposing two

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[1] 'le foyer pontico-caspien' as stated in the French translation of that book.

theories which are in contradiction with one another. A feature of the book, and presumably of all the OIT literature in general, is the creation of an alternative between OIT or AIT, as if *there were only one non* Out-of-India Theory. There is in reality no dichotomy between 'people, from either the OIT side or the AIT side' (p.XXII). A plausible explanation is that the author lumps together all these divergent theories into 'the AIT side' because they all share the feature of having Vedic and its present-day daughter languages come from somewhere else than the present-day borders of India. Moreover it should also be noted that none of the existing theories refers to itself as being the Aryan Invasion Theory. This label is an artifact created by the OIT supporters. The emphasis on the word *Invasion* is inadequate to describe the ethnogenesis of early India and cannot account for the fact that India did not exist thousands of years ago as a state and did not have (its present-day) borders. For that reason, the concept of invasion, i.e. an instantaneous and conscious trespassing of an established state border, is absurd when dealing with Vedic times and the Antiquity (of whatever place). As underlined by the author, this kind of invasionist schemes was very much fashionable in the good old days of European colonialism. As a matter of fact, it has become unpalatable to everybody at the beginning of XXIst century. For that reason, the AIT label, not far from being a libel, coined by the OIT supporters to describe present-day scholarship is grossly inadequate. It can also be noted that the Indus Valley is located in Pakistan instead of India for the most part. So much for the accuracy of the OIT.

Another pivotal claim of the author is to 'think the OIT represents as revolutionary a change from the AIT in the field of Indo-European historical studies, and in fact world historical studies, as the helio-centric theory represented from the then prevalent geocentric theory.' (p.XXXIV) The author seems to be unaware that the OIT has *nothing* revolutionary *at all* and that the OIT theory is one of the first theories developed by European scholars in the XIXth century and one of the first to have been dismissed. Here is a citation from Emile Littré (1801-1881), a well-known French lexicographer:

> "De même que le français, l'italien ou l'espagnol sont, pour la plus grande partie, constitués par le latin, de même, le latin, le germanique et le celtique ont leur fonds commun avec la langue qui fut parlée sur les bords du Gange. Mais aussi, de même que, dans le français, l'italien et l'espagnol, il est des mots qui ne se rattachent pas ou ne sont pas rattachés aux trois langues mères, de même, dans le latin, le germanique et le celtique, il est des mots pour lesquels on n'a pas reconnu de congénères dans le glossaire sanscrit."[2]

Our translation : In the same way as French, Italian and Spanish are, for the most part, based on Latin, in the same way, Latin, Germanic and Celtic share lexical resources with the language which was once spoken on the banks of the Ganges river. But then, in the same way as, in French, Italian and Spanish, there are words which do not go back to the three mother tongues, in the same way, in Latin, Germanic and Celtic, there are words for which no cognates have been found in the Sanskrit glossary. A Swiss linguist, Adolphe Pictet (1799-1875), contemporary of Littré, wrote this:

> "ce qui précède me semble justifier suffisamment l'emploi du nom de *Arya* pour désigner, dans son unité, le peuple père de la grande famille appelée jusqu'à présent *indo-européenne*. C'est quelque part sur le vaste plateau de l'Iran que l'on s'accorde à chercher le commun berceau de la race arienne ; mais cet quadrilatère qui s'étend de l'Indus au Tigre et à l'Euphrate, de l'Oxus et du Iaxartes au Golfe Persique [...].'[3]

Our translation : According to me, what precedes justifies well enough the use of the name *Arya* to designate all the forbears of the great family of peoples called *Indo-European* so far. The common

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[2] Littré E. 1863. *Histoire de la Langue Français: études Sur Les Origines, L'étymologie, la Grammaire, Les Dialectes, la Versification Et Les Lettres Au Moyen Age*. Paris: Didier (p.79)
[3] Pictet, A. 1859. *Les origines indo-européennes ou les Aryas primitifs*. Paris: Joël Cherbuliez. (T1 p.34-35)

cradle of the Aryan people[4] is usually thought to have existed somewhere on the vast plateau of Iran; but this quadrilateral area which stretches from the Indus to the Tigris and the Euphrates, from the Oxus and Iaxartes to the Persian Gulf [...]. As exemplified, the French-speaking scholars of that period and most European scholars in general used to consider the PIE homeland to be located somewhere around the Indus Valley as the standard theory. A map like the one Talageri's book displays on p.226 could have been printed in Pictet's book in 1859.

In other words, there are several major inadequacies in the way Talageri portrays the present and past history of the issue of the PIE homeland in the preface of the book, apart from not calling the PIE homeland by its name. The alternative he describes between the OIT and another monolithic *alter ego*, labeled AIT, is fictitious. The academic situation is not this alternative between OIT and AIT but several competing theories. This may originate in the author's dislike of complexity and of the word 'complex' itself (p.331). Moreover, the OIT, i.e the India-as-homeland theory, is not a 'new hypothesis' (p.XIX) but one of the oldest theories dismissed more than one century ago. For that matter, the accusations in the preface sound wholesale and awesome. According to the author, the past scholars and present-day scientists working in the field of Indo-European studies 'have manufactured long and complicated stories out of thin air, based on zero evidence from the texts' (p.XXXIV), 'in over a century of mindless scholarship' (p.XXXV), 'backed as they are by an immensely powerful array of vested interests' (p.XX). The author is not only unaware (or oblivious) that the OIT was once the standard theory, but the reader faces the Orwellian threat[5] that all the researches on the PIE homeland for centuries amount to an attempt to 'stifle the truth' (p.XXXIV), i.e. the OIT.

Once these under-informed accusations have been pushed aside, the questions that are raised by the *revival of the OIT* are : why should we return to the OIT if this theory has been dismissed more than a century ago? and what has changed since that time so that the reasons to dismiss the OIT should have to be reconsidered? These are indeed the issues that the modern OIT supporters have to address and that the author claims to have addressed 'conclusively' (p.XVIII). The preface proposes on p.XXXII-XXXIII a hypothetical kind of family-tree of Indo-European peoples according to the author. Among other things, the peoples are not clearly distinguished from the languages.

The preface ends with words of gratefulness, mourning and love for the author's best friends and closest relatives.

4. *About Section 1 : Chronology and Geography of the Rig-Veda*

This section of the book has 200 pages and is divided in six chapters. As alluded before, the textual organization of the book is unusual and defective. There is no explicit conclusion, the preface includes errata for previous books and transliteration conventions. Section 1 includes sub-chapters with titles like *Appendix* 1 and 2 and *Footnote* that are in fact incorporated in the body of the text. Another feature of Section 1 is frequent interruptions of the text by copious references to the hymns and verses of the Rig-Veda and by lists of names or nouns. Many of these references should have been preferably dealt with otherwise, so that the reasoning and the text of the author would not be constantly chopped. Another feature is the letter fonts, sizes and cases which often vary within any given page. All these textual and typographic features are hindrances for the reader to understand what the author wants to say and sometimes to find the text itself amidst the references.

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[4] NB : the word 'race' is better translated as 'people', as Pictet is not especially racist in the modern sense.
[5] or 'an all-out Goebbelsian campaign' (p.116)

This section (and the book more generally) can be read in several ways : a surface reading of what the author writes explicitly and deeper readings of what he assumes and thinks but does not write. As will be shown it is sometimes necessary to group together quotations from different parts of the book to make clear what they actually mean, because a certain number of ordinary words are skewed and contrived by the intellectual framework of the author and this becomes apparent only after some key sentences have been regrouped in a linear and coherent paragraph.

The book does not begin with a programmatic presentation of what the author plans to state or prove in Section1. My understanding is that he wants to clear several issues at the same time: one is the relative chronology of the books and hymns of the Rig-Veda, another is their absolute chronology, another is the relative chronology of the Rig-Veda and the Avesta, another is to substantiate the supposed westward movements of the Rig-Vedic Indo-Aryans, one more is to expose the perceived fraudulences of the so-called Western scholarship, as exemplified by Witzel. Another thesis of the author is that the Rig-Veda should have taken at least 2000 years to be composed. The multiple goals, compounded with the defective textual organization of the book, contribute to the opacity and lack of fluidity of the section. Not infrequently the reader realizes the gist and purpose of a particular chapter at the beginning of another one. Fortunately, repetitions and refinements of some key points provide a helpful guideline as to where the author is ultimately going, an Ariadne's clew if I dare put it that way.

Section 1 contains the following chapters :

1. A first part (p.3-53) is dedicated to an analysis of the person names shared by the Avesta and the Rig-Veda, as listed by Mayrhofer. About half of the pages are references, which could be synthesized and organized otherwise as annexes. From that chapter, the author draws the conclusion that the Avesta must have been written later than most of the Rig-Veda. I have no particular opinion about this conclusion and the method used to reach it. I tend to think that this point is not as crucial as the author seems to believe.

2. A second part (p.54-80) is dedicated to a statistical analysis of the meters of the hymns of the Rig-Veda. This chapter is abstruse and it is hard to figure out what these statistics actually prove. Most of this part is references or tables.

3. The third part (p.81-129) is dedicated to a philological analysis of the implicit geography of the Rig-Veda as reflected in place-names and some typical animals throughout the hymns. This part is the most stimulating one. The author claims that a westward movement can be detected between the different sections of the Rig-Veda, provided that the relative chronology between the latest and earliest books as determined by the author should be proved correct. The opposite conclusion had been previously drawn out of the same data, as is aptly reminded by the author. Ultimately, the conclusions drawn from the Rig-Veda depend on the relative chronology chosen or determined for the books. Circularity is a permanent risk. Moreover the semantics of some items is controversial. I tend to think that more work on that interesting topic is needed, whatever the leanings of the specialists vis-à-vis the OIT may be.

4. The fourth part (p.130-167) deals with the relative chronology of the books of the Rig-Veda. That of the author differs to some extent from received chronologies. All agree that the books I VIII IX X are the most recent and disagree about the order of the other six ones, admittedly the oldest: the order of the author is (oldest >) VI III VII IV II V. The order of Witzel, as cited by Talageri in a previous book, seems to be (oldest >) II IV V V III VII. We do not have the expertise to determine which order (or if another one) should be preferred. The French scholar Filliozat mentions the books III IV V as the oldest layer. The author claims on the basis of some examples that the criteria proposed by Oldenberg

may not be as reliable as is usually considered (p.154). These philological technicalities should be addressed and discussed by competent specialists of the field.

5. This chapter (p.168-183) deals with the Indo-Aryan sounding words attested in Mitanni and Kassite texts. The author claims 'that the Mitanni IAs [=Indo-Aryans] were emigrants from India in the Late Rig-Vedic period' (p.168). I have reservations about any strong conclusion drawn out of these limited lexical items written in a fairly obscure graphic system. One of the few clear features is /azda/ attested in -1500 BC in this Mitanni Indo-Aryan-oid language versus /eda/ attested one thousand years later in Vedic. Thanks to its poetical structure and recitation, scansion and several complementary ways of decomposing the words and rhymes (samhitā, pada, krama-pāṭha, etc.) the text of the Rig-Veda, once composed, was preserved from (significant) alterations. As far as I know, nothing suggests that Rig-Vedic /eda/ has ever been pronounced */azda/ at any time. In other words, if one should accept the (possible) premise that Mitanni Indo-Aryan-oid were indeed a *spatial* variety of Indo-Aryan, then Vedic, as reflected in the Rig-Veda, is a *temporal* variety of Indo-Aryan younger than Mitanni Indo-Aryan-oid. The phonetic change from Indo-Iranian */azda/ to Rig-Vedic /eda/ was already completed, when the Rig-Veda began to be composed. Moreover, the author does not address the issue of how Indo-Aryans coming from India could have moved *through* Iranians *until* reaching eastern Anatolia. We tend to think that these lexical items are only remotely relevant and informative when it comes to the chronology of the Rig-Veda. But they are utilized by the author as a reference for his own absolute chronology. If any conclusion can be drawn out of these data, I would conclude that they prove the Rig-Veda, as a whole, is younger than this Mitanni Indo-Aryan-oid language, contrary to the author's claim.

6. The last chapter of Section 1 (p.184-200) is dedicated to the absolute chronology of the books of the Rig-Veda. The author makes the claim that 'by a conservative estimate, the total period of composition of the Rig-Veda must have covered a period of at least two millenniums' (p.185). Such a claim can hardly account for the state of language of the Rig-Veda, which is homogenous in spite of minor and detectable changes. Such a long stretch of time (> 2000 years) would necessarily entail major changes, such as those detectable between Latin and French, or Sanskrit and Hindi, which are different languages, not variants of a single state of language. The author provides his own estimates for the composition of the Rig-Veda: -3400 à -1400 BC (p.186). The theory of the author necessitates to shift and stretch the antiquity of the Rig-Veda (at least) in those proportions. Some other OIT supporters shift and stretch the antiquity of the Rig-Veda in even greater proportions. Considering the period of time when Pāṇini lived (circa Vth century BC) and the fact he was among the last speakers of that state of language represented in the Rig-Veda, the traditional bracket for the composition of the Rig-Veda from -1500 to -1000 BC seems to be the most reasonable.

Section 1 begins with the two following paragraphs, which are worth citing in extenso:

'The Rig-Veda and the Avesta are the two oldest "Indo-Iranian" texts. The joint evidence of the Rig-Veda and the Zend Avesta testifies to a period of common development of culture, which may be called the Indo-Iranian period.

According to the AIT (Aryan Invasion theory), this period preceded the period of composition of the Rig-Veda and the Avesta: the joint "Indo-Iranians", in the course of their postulated emigrations from South Russia, settled down for a considerable period of time in Central Asia, where they developed this joint culture. Later, they separated from each other, migrated into their historical areas, where they composed, respectively, the Rig-Veda and the Avesta, both representing the separate developments of this earlier joint culture. This joint Indo-Iranian culture is, therefore, pre-Rig-Vedic.' (p.3)

A detail, which intrigued me from the start, is <"Indo-Iranian"> with <" ">. The reasons for this graphic precaution gradually became clear while reading the rest of section 1. The next sentence is something that the author presumably accepts as true. Note that 'the Indo-Iranian period' is written without <" ">. The next paragraph, which the author presumably rejects as false, is his personal summary of the Indo-Iranian emergence. Note that <"Indo-Iranians"> is again written with <" ">. A key concept in the reasoning of the author is the idea of 'development of a joint culture'. The real meaning of 'development' and 'to develop' will be explained infra. Something that the author does not state in his summary is that the Indo-Iranian culture inherited a considerable number of ethnocultural and mythological items from the original PIE community, apart from words and grammar. As a matter of fact, my careful search has made nearly sure that section 1 never uses the word *inherit(ed)* and I will deal with the conception of *heritage* of the author infra. What is constructed in section 1 is an 'Indo-Iranian period' (just a period not a unique ethnocultural community) and two ethnocultural entities, the 'proto-Iranians' and 'the Vedic Aryans', which have been in *'continuous interaction'* (p.3) during that particular and specific period, but, as we will see, were previously completely independent. What the Avesta and the Rig-Veda share and have in common originates in this punctual rather than continuous interaction. From the very first page, the implicit model used by the author to account for the linguistic and cultural features shared by the Indo-Aryan and Iranian languages is an areal diffusionist model. The key words are 'interaction', 'share(d)', 'common' and 'spread'.

The book starts with an analysis of the person names found in the Avesta and the Rig-Veda, as listed by Mayhofer. At first, the reader may wonder why a book dealing with the PIE homeland should start with that ultra-precise topic, which is completely secondary in my opinion. Here is what the author states on p.34 :

> 'Many of the above names and name-elements common to the Avesta and the Rig-Veda may be common names and elements which originated among the Rig-Vedic Aryans and spread to the Iranians, or originated among the Iranians and spread to the Rig-Vedic Aryans.'

It should be noted that many of these name-elements are morphemes obviously inherited from PIE but a conspicuous feature of the book is that it contains nearly no mention of any reconstructed PIE proto-form. Most basic words, generally appearing in the works and articles dealing with the Proto-Indo-European issues, like cognate word, change, phonetic, correspondence, proto-language, etc. are absent from the book. The author nevertheless states: 'We have already examined the linguistic evidence and the textual evidence in detail'. (p.308) This claim is completely unsubstantiated because the linguistic evidence and issues related to PIE and Indo-European languages are not examined but conjured away in the book.

Some key sentences of the author's theory are :

> 'The proto-Iranians were originally inhabitants of Northern India - originally, in the pre-Rig-Vedic period, of the Kashmir region, and later, in the Early Rig-Vedic period, of the Punjab' (p.3)

> 'The Vedic Aryans were originally located to the East of this region [i.e. the greater Punjab cited three lines before] and only expanded into this region after the Early Rig-Vedic Period.' (p.93)

> 'In our examination of the relative chronology of the Rig-Veda vis-à-vis the Avesta, the common development of the joint "Indo-Iranian" culture represented in these two texts took place in the period of the *Late* Books of the Rig-Veda. [...] *In which area did this development*

of the joint "Indo-Iranian" culture take place ? [...] The *common* ground therefore lies in the area stretching from Punjab to Afghanistan.' (p.81)

In other words, as far back as 'originally' may go, Proto-Iranians and Vedic Aryans have *never* been *one* ethnocultural community and everything is the result of contacts and *'continuous interaction'* (p.3) limited to a specific and late period and nothing has ever been inherited from common ancestors. Person names built with the same Indo-European components appearing in the Avesta and the Rig-Veda are not inherited but 'came into vogue' (p.188) or 'have gone out of vogue' (p.44). The reason why <"Indo-Iranian(s)"> is written that way is because the author negates the very fact that the ethnocultural Indo-Iranian community could have ever existed :

> 'There is a sharp cultural, *and chronological*, dichotomy between the Early Books, *proper*, which do not contain a *single* name of the Rig-Vedic-Avestan type [...] and the Late Books.(p.48)

> 'In the period long *before* the development of this common "Indo-Iranian" culture, i.e. at the time of the composition of the Early Books of the Rig-Veda, the Vedic Aryans were inhabitants of areas *far to the East* of the area of development of this joint "Indo-Iranian" culture, *rather than its North*. The Vedic Aryans expanded from an Eastern Homeland (East of the Sarasvatî).' (p.98)

Apart from rejecting the northern origin of Indo-Iranian, the key point stated by the author is that Vedic Aryans have been something different from (and as we will see hostile to) Proto-Iranians as far back as 'originally' may go. In the fixist and anti-evolutionist mind-set of the author, they have no shared ancestors, they have no shared homeland[6], they have no shared ethnocultural heritage[7]. The words *heritage, common tradition, common culture,* are misleading and their real meanings are skewed and contrived, as the following sentences will show :

> 'The common "Indo-Iranian" culture visible in the two texts [the Avesta and the Rig-Veda] is a product of the Late Rig-Vedic Period.' (p.168)

This can be compared with :

> 'The two traditions, Vedic and Avestan, seem to represent two entities sharing a common tradition, but as *rival entities within this common tradition*. And echoes of this rivalry persist down to the later forms of these two traditions.' (p.259)

And with :

> 'All the areas to the west, including the Punjab itself, were originally alien, unfamiliar territory to the Vedic Aryans.' (p.106)

What the author calls *heritage* and *common tradition* are ethnocultural features recently acquired because the two entities: Indo-Aryans and Proto-Iranians have lately come to interact. But before they came in contact, i.e. 'originally', they were completely isolated and disconnected :

> 'A most significant indicator of the insularity of the Rig-Vedic Aryans within India, and strong evidence of their original unfamiliarity with the northwestern and western areas, is the fact that Vedic traditional attitude towards these areas has always been one of suspicion, disdain or even mild hostility.' (p.105)

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[6] The review of Section 2 will show how the author uses this word 'homeland'.
[7] There is three instances of the word 'heritage' on p.258-259. But this changes nothing to the fact that 'Indo-Aryans and Iranians have been neighbors to this day' (p.258). Neighbors and nothing more.

'The period of the Early Books (6, 3, 7) and even the period of the Middle Books (4, 2) *predates* the development of this common culture.' (p.43)

As these excerpts eloquently show, the concept of Proto-Indo-Iranian as a proto-language spoken by Proto-Indo-Iranian people as a unique prehistorical human community is completely negated by the theory of the author. What the author has in mind is an ethnocultural sandglass model: at a late period, after they had already started composing the Rig-Veda, Indo-Aryans, who originally had always been on their own in the east, came in contact, for whatever unknown reasons, with others, who were their north-western neighbors, i.e. Proto-Iranians, and they then acquired what they have in common and subsequently retained those late acquired features, the product of late contacts, which the author labels 'a common culture' and a 'heritage' resulting from 'continuous interaction' in a spurious and misleading fashion. A fortiori, the concept of Proto-Indo-European as a proto-language spoken by a unique prehistorical human community does not exist, because the sandglass model of the author is a *one-shot* sandglass model.

The logical conclusion is that the OIT as described in the book does not address the issue of the PIE homeland because it negates the most basic linguistic and ethnocultural concepts that this issue entails. The one-shot sandglass model invented by the author is a completely inadequate explanation of the features of the Indo-European languages. In that model, one is left to wonder what makes the Indo-European languages Indo-European in the first place.

On the whole, Section 1 of the book can be rated as decent, in spite of its negation of the basic concepts of historical linguistics and of its inadequate textual organization, especially when compared to Section 2.

5. *About Section 2: The Indo-European homeland in India*

Section 2 has 150 pages and is divided in two chapters. The first chapter (p.205-307) describes the scenario and hypotheses proposed by the author as regards the dispersal of the Indo-European languages and addresses several issues related to that dispersal, such as language contacts between Uralic and Indo-Iranian. The second chapter (p.308-354) deals, among others things, with archeological issues related to the dispersal within his scenario.

The first chapter of section 2 (Chapter 7) starts with a discussion of three arguments proffered by Hock against the different versions of the OIT. According to the author, Hock distinguishes two versions of the OIT: one is the 'Sanskrit itself as the original ancestral language' (p.205), the other one is 'the PIE-in-India' theory (p.206). As regards the first version, the author has 'very emphatically rejected the idea that the Vedic language was the ancestor even of the languages known today as the Indo-Aryan languages, let alone of all the Indo-European languages'. (p.205) This rejection is coherent with the general approach of the author, according to which Rig-Veda Aryans have always been different from anybody else: 'the other Indo-European Dialects were *different* from the Vedic dialect [...].' (p.298) In fact, the Indo-European languages are not far from having no ancestor at all in this version of the OIT. The next point addressed is the 'equine argument'. According to the Pontico-Caspian theory, the original PIE community knew the horse as a domesticated animal. The author claims that this argument is worthless and cites Blažek: 'The Indo-European homeland has to be located at the area where the wild horse did not live' (p.209). I tend to agree with this constraint. The third issue is the 'Evidence of the Isoglosses' (p.213). This requires a separate paragraph in the review.

The pages (p.213-258) are dedicated to a detailed description of the scenario proposed by the author, with 6 maps and their related comments. At the first look, I have not been able to understand

what the area on the low-quality maps was. The maps are centered on Afghanistan with present-day borders of the different states surrounding Afghanistan. The borders of the former Soviet Republics (Uzbekistan, Kirghiztan, Kazakhstan) are missing. The area proposed as contiguous homelands of the Indo-European languages (or branches) spreads from Kashmir to Afghanistan. The scenario is not exactly 'Out-of-India' but rather 'circum-Indus-upstream-valley'. It must be emphasized that the scenario proposed by the author is not a homeland for the proto-Indo-European family, nor a proto-Indo-European homeland. The scenario is a representation of (some of) Indo-European languages and branches concentrated in a reduced area. As the intellectual framework of the book negates the linguistic concept of Proto-Indo-European as a proto-language spoken by a unique prehistorical human community, the scenario illustrates some of the IE languages (or branches) as having contiguous individual homelands. In other words, they have never been anything but neighbors. The Proto-Indo-European homeland, in this version of the OIT, is a compaction of individual homelands, one of them being that of Indo-Aryan. This could be called the *PIE Homunculus Loquens* Theory. The compacted homelands area already contains all the components of the Indo-European family in a reduced and telescopically concentrated format. In this theory, the Indo-European languages became what they always were, but they did so farther away. 'The European dialects moved northwards from Afghanistan, and then, *in the same above order* [i.e. Italic, Celtic, Germanic, Baltic and Slavic], appear to have gradually migrated by a northwest path into Europe, and continued right up to Western Europe, [...]' (p.240).

The scenario described in the book is teleological. This situation originates in three main features of the general intellectual framework of the author. The first one is the lip-service acceptation of the linguistic concept of Proto-Indo-European, as seen in section 1. The second one is the decided contention (for political reasons) that Indo-Aryan is not a 'dialect' (p.236) but a 'branch' (p.223) of PIE. It must be reminded that Indo-Aryan is the Indian sub-sub-branch of the Indo-Iranian sub-branch of the non Anatolian branch of the Indo-European family, the other branch being Anatolian. Because the author does not accept the paleo-linguistic notion of Indo-Iranian and has a fixist and anti-evolutionist framework, he posits that Indo-Aryan always existed, at least as far back as 'originally' may go, and he has to posit that all Indo-European languages were equally existent from the same point to when 'originally' goes back. The third feature which underlies this scenario is an inadequate approach of the notion of *isogloss*. According to the author, 'an isogloss is a special linguistic feature which develops in any one language and then spreads to other languages and dialects over a contiguous area' (p.212). The regular definition is 'a line on a map that represents the geographical boundary (limit) of regional linguistic variants'. The erroneous definition of the author confuses a shared innovation, a shared conservation and an areal feature, among other things. An isogloss is a line on a map that illustrates existing variants of a particular phenomenon. The author transforms that descriptive tool into a kind of permanently inheritable and transportable feature: 'when, in some cases, some of the dialects or languages sharing the isogloss move geographically away from each other (into non contiguous areas), and continue to retain the linguistic feature, [...]' (p.214). This erroneous interpretation of Hock's representation of isoglosses misleads the author into thinking that a telescopic, homothetic or geometrical, modification of Hock's representation projected on his compacted homelands area could make any historical or linguistic sense. Another statement that makes little sense is 'the formation of the isoglosses covering different groups of dialects' (p.218). For reasons that remain obscure, briefly polished off, the author rejects the hypothesis of a homeland 'situated in any central area' (p.221) and states that 'a common exit point' (p.223) is necessary. These two points are obviously required by his scenario but they remain no less obviously unjustified in the book. And I tend to consider these two points are unacceptable in the first place.

Another feature of the scenario proposed by the author is that the dispersal of IE languages becomes a historical process, attested in the Rig-Veda (p.218). At this juncture, the reader understands what is at stake in the quest of an unfathomably ancient composition of the Rig-Veda. The standard traditional time bracket from -1500 to -1000 BC for the composition of the Rig-Veda disqualifies the OIT as constructed by the author. The tribe names: *Druhyus*, *Anus* and *Pūrus*, attested in the Rig-Veda - I have not checked that point -, are converted in the author's scenario into 'tribal conglomerates' that are supposed to account for the Indo-European family. The following paragraph is worth citing :

> 'As per our theory, the original homeland of the Indo-European family of languages was in India, and all the above twelve dialects were spoken by different groups of people who were referred to in Indian tradition as people belonging to the above three tribal conglomerates: the *Druhyus* (Hittite, Tocharian, Italic, Celtic, Germanic, Baltic, Slavic), and the *Anus* (Greek, Albanian, Armenian, Iranian) and the *Pūrus* (Indo-Aryan).' (p.224)

This paragraph can be considered the watershed of the book. The author seems unaware that his theory is inadequate in unspeakable proportions. The solution he proposes for the issue of the PIE homeland is hallucinatorily absurd. At this point of the reading and of the review, it becomes clear that the author hardly understands or takes into account any of the most basic concepts of the Indo-European issue and that his intellectual framework is flawed to a (possibly) hopeless extent.

The scenario tries to transform the gradual split of speech communities into smaller speech communities, according to the internal structure of the Indo-European family, into a temporal succession of pre-existing languages expelled from the same area in the same direction in a pre-determined order (p.236). On p.306, he claims not to do so. But he obviously does do so. This is another consequence of the author's misunderstanding of several linguistic concepts and of his fixist and anti-evolutionist mind-set. The pages (258-273) are dedicated to an outpour of considerations on typically Indian cultural items, among which the Druhyus, Anus and Pūrus 'tribal conglomerates'. I am not familiar with these items and I cannot describe what added value this section of the book might bring. To be frank, this part of the book has a kind of incoherent and unworldly flavor that borders on Nostradamus' predictions.

Afterwards, the author disposes of the lexical evidence contained in the Uralic languages of the previous location of Indo-Aryan, Iranian and Indo-Iranian languages (and of PIE in general) outside India with the ad-hoc and unparsimonious hypothesis of unattested Indo-Iranian people: 'the west migrating Indo-Aryans and Iranians are, unfortunately, lost to history, but their existence is vouched for by the borrowed words in the Uralic languages.' (p. 275) When compared with the preceding pages, this appears to be a minor epistemological breach.

The next pages (p.277-290) are a kind of summary of the preceding pages from the start of the book and a kind of provisional conclusion, before the author proceeds to the archeological chapter. My intuition is that a first version of the book may have stopped here and that new chapters were added later on. This might explain the erroneous reference : page XVIII and not XV for the preface and there is a somewhat distinct stylistic change in the following chapters. It ends with 'We have in this chapter, presented a complete linguistic case for the Indian Homeland or Out-of-India theory; and examined the linguistic evidence in all its relevant aspects starting with the Evidence of the Isoglosses. There is little scope left for claiming that the linguistic evidence is against the OIT: on the contrary, it fully supports the OIT case, and fits in perfectly with the irrefutable textual evidence presented in Section I of this book'. (p.289-290) For the time being, on the basis of that book, what can be stated is that there is little scope left for thinking that the author understands or takes into account what the issues of the Indo-European Proto-language and of the Indo-European homeland mean and entail.

The next sub-chapter (p.290-307) is focused on previous exchanges with the author's *bête noire* aka Michael Witzel. There is nothing really new in this part of the book. This part was probably added to the book because some of its content has not been published elsewhere as the author had wished (p.290). Some parts are worth citing because there are explicit statements of the core beliefs of the author :

> 'Our analysis of the relative chronology of the Rig-Veda and the Avesta, in Section 1 of this book, which I *again* challenge Witzel to refute, gives the lie not only to his insinuation that Avestan is older than Vedic, but consequently also to his claim that the two belong to one single branch of IE languages. As already pointed out, the other Indo-European Dialects were *different* from the Vedic dialect [...].'. (p.298)

This is in line with the fixist and anti-evolutionist stance of the author when it comes to Indo-Aryan, which is thought and constructed as being a kind of immanent panchronic (near Platonician) Entity, as old as 'originally' goes back. In that stance, Indo-Aryan was always *different* from the rest. And it has *never* moved from India : "Indo-Aryan", *the Dialect which remained in the homeland after all the others had left*'. (p.277)

The end of the chapter is not far from absurd :

> 'In sum, *all* of Witzel's linguistic arguments are basically directed against *three* hypotheses which are treated as the core of the OIT case, but *which form no part whatsoever of the case presented by us* : (1) the "Sanskrit-origin" hypothesis [...] (2) the "sequential movement of different groups" Out-of-India hypothesis (postulated by *no-one*, so far as I know) [...]' (p.306)

The reader is left to understand what the Early Dialects, the European Dialects and the Last dialects (p.236) mean. Is this not sequential? Not to speak of 'The European dialects moved northwards from Afghanistan, and then, *in the same above order* [i.e. Italic, Celtic, Germanic, Baltic and Slavic], appear to have gradually migrated by a northwest path into Europe, and continued right up to Western Europe, [...]' (p.240). And 'Hittite, Tocharian and Italic are the dialects which, in any generally accepted schedule of migration, were the first, second and third, respectively, to migrate from the original homeland.' (p.222) Is a schedule of respectively, first, second and third, not sequential? It seems that the author makes a distinguo between 'one by one' or in small groups, but this distinction is irrelevant. The stumbling-block is not the logistics but the sequential principle and the bottleneck route.

In the course of reading the book and writing this review, after I fully understood that the framework of the book is fixist and anti-evolutionist, I wondered how *linguistic change* was described in this version of the OIT theory. At that point, I realized that some words have acquired particular meanings under the pen of the author. This is the case of 'development' and 'to develop'. These two words are a key lexical tool to suggest *change* and *evolution* in the fixist and anti-evolutionist framework of the author. I made a survey of the main instances throughout the book and, most of time, the meaning is not 'to transform, to evolve' but 'to create ex nihilo'. The inherited features shared by proto-Iranian(s) and Rig-Vedic Aryan(s) from their common ancestor(s) are described in the book with these two apparently ordinary and innocuous words : 'development' and 'to develop'. The substitution of 'to transform' or 'to create ex nihilo' reveals the conveyed meaning :

> 'The Rig-Veda and the Avesta are the two oldest "Indo-Iranian" texts. The joint evidence of the Rig-Veda and the Zend Avesta testifies to a period of common development [= creation ex nihilo] of culture, which may be called the Indo-Iranian period.

According to the AIT (Aryan Invasion theory), this period preceded the period of composition of the Rig-Veda and the Avesta: the joint "Indo-Iranians", in the course of their postulated emigrations from South Russia, settled down for a considerable period of time in Central Asia, where they developed [= created ex nihilo] this joint culture. Later, they separated from each other, migrated into their historical areas, where they composed, respectively, the Rig-Veda and the Avesta, both representing the separate developments [= transformations] of this earlier joint culture. This joint Indo-Iranian culture is, therefore, pre-Rig-Vedic.' (p.3)

These two words 'development' and 'to develop' enable the author to *neutralize* the difference between the *transformation* of a bygone entity, which in the Indo-Iranian case is a split into two new entities, and the *acquisition* or *creation* of a new feature by an existing entity which remains unchanged. Most of the time, these two words are preceded or followed by 'joint', 'jointly', 'common', 'in common'. As the above example shows, this semantic neutralization is textually constructed from the very first words of the book. In most cases, the replacement of 'develop' by 'transform' or 'evolve' does not suit semantically, because this is not the purported meaning. Other instances are:

'The Indo-Iranian culture common to the two texts developed [= was created ex nihilo] *after* the composition of the hymns of the Early and Middle Books.' (p.45)

'In our examination of the relative chronology of the Rig-Veda vis-à-vis the Avesta, the common development [= creation ex nihilo] of the joint "Indo-Iranian" culture represented in these two texts took place in the period of the *Late* Books of the Rig-Veda. [...] *In which area did this development* [= creation ex nihilo] *of the joint "Indo-Iranian" culture take place* ? [...] The *common* ground therefore lies in the area stretching from Punjab to Afghanistan.' (p.81)

'The joint "Indo-Iranian" culture common to the Avesta and the Rig-Veda developed [= got created ex nihilo] during the period of composition of the *Late* Books of the Rig-Veda. [...] the area of development [= creation ex nihilo] of this joint "Indo-Iranian" culture [...] the development [= creation ex nihilo] of this common "Indo-Iranian" culture [...] the area of development [= creation ex nihilo] of this joint "Indo-Iranian" culture [...] the development [= creation ex nihilo] of this joint "Indo-Iranian" culture [...].' (p.98)

'The Rig-Vedic ritual traditions developed [= got created ex nihilo] in northern India.' (p.105)

'They actually developed [= began to feel] an all-pervading disdain [...]' (p.107)

'The emigrating Mitanni could have developed [= created ex nihilo] a few [Prakritizations]' (p.172)

'The Vedic Aryans [...] lived in a period *prior* to the development of this common culture' (p.188)

'The culture of the Last Rig-Vedic Period (the common elements of which are found in the Late Books 5, 1 and 8-10, in the Zend Avesta [...]) was already fully developed [=created ex nihilo]. Before this was the Middle Period, and before this the Early Period, both of which preceded the development [= creation ex nihilo] of this common culture.' (p.200)

'The common non-Indian word, in the OIT scenario, can have developed [= been created ex nihilo] in the region of Afghanistan and Central Asia.' (p.303)

In the pages 223-236, where the author describes his scenario of dispersal, this peculiar use of the word 'to develop' is compounded with the misunderstood word 'isogloss' and the nondescript phrase 'to develop an isogloss' (as of languages) is introduced. Thereafter, the book reveals the following sentence :

> 'The various European Dialects, on the other hand, developed isoglosses in common, separately, with both the Last Dialects as well as the Early Dialects'. (p.242)

This is how the author describes or explains the emergence of the so-called European Dialects. This set of words is undoubtedly benchmark and the reader is left to think whether Talageri has not outwitted the famous *Colorless green ideas sleep furiously* of Chomsky. Being a structuralist, I shall leave to generative-transformists the task of turning the above sentence into the passive voice.

On the whole, this chapter of section 2 reveals the multiple inadequacies and flaws of the author's fixist and anti-evolutionist approach. In contrast with section 1, which contains stimulating elements, potentially requiring further analysis, this chapter of section 2 can be rated to be a near complete intellectual wreckage. About nothing (< 5%) has any scientific value or status.

6. About Section 2 : The Indo-European homeland in India : The archeological case

This chapter (p.308-354) has 47 pages and deals with archeological considerations, among other things. As mentioned before, I suspect that this part of the book was probably added to the book in a second (or third) phase of its composition.

There are some lexical differences with previous chapters. In this part of the book, the word 'developed' has a more regular meaning (fully-grown-up) and the word 'transformation' is used, in contrast with all previous chapters of the Sections 1 and 2, where this latter word is unheard-of:

> 'This transformation is alleged to have taken place in the area of the Great Civilizations of the ancient world: a full-fledged, highly developed (in terms of technology as well in civic organization) and highly populated civilization.' (p.314)

Moreover the content of this chapter is considerably better than the rest of section 2. This explains that it is reviewed separately. There is a sort of contradiction in the very existence of this chapter. The author has very emphatically declared that the case is settled once and for good in favor of the OIT and then one more chapter is nevertheless added. This is one more oddity in the textual organization of the book. The first page is a kind of apologetic transition for the addition of the chapter. The content of the chapter is diverse and heterogenous. The main thread, as I have perceived it, is not exactly archeology but the ethnogenesis of the early Indo-Aryan community once settled in north-western India. This issue is indeed, in my opinion, a cornerstone and touchstone point in the understanding and description of the (pre-)history of north-western India, whatever one's theoretical background and leanings may be.

The author points at the scarcity of archeological evidence that could support (or equally disprove) the hypothesis of an intrusion of non autochthonous people at the time when Indo-Aryans are supposed to set foot in India during the second millenium BC. But as pointed at by Mallory, listed in the bibliography, about all the migrations and arrivals of Indo-European peoples are poorly attested from a sheer archeological standpoint. Another point is the reluctance of the author to accept an ethnocultural and linguistic shift in a short period. There exist clear examples of such processes: that of Gaulish people becoming Gallo-Romans in probably fewer than five centuries, and some Uralic people who changed from Samoyedic to Turcic to Russian in three generations. Plenty of other examples exist and could be cited.

The main arguments of the author are somewhat caricatured descriptions of the newcomers, of the pre-existing inhabitants and of the processes that resulted from their interactions. The newcomers are 'a pastoral, illiterate, nomadic people "on the move" (p.321). The author has the prejudice that the

absence of writing is tantamount to idiocy and cultural vacuum. But the Rig-Veda itself has been transmitted for centuries without resorting to writing. I do not understand this statement of the author: 'The Vedic Aryans are the People of the Book in the Rig-Veda'. (p.368) And the elaborate ethnoculture reconstructed for the original Proto-Indo-European community amply shows that these writingless people were neither stupid nor a cultural blank page. As a matter of fact, they managed to occupy half Eurasia before becoming literate. There is a kind of fetishist relationship among the OIT supporters when it comes to writing. It can be mentioned that the author considers the Indus symbols to be 'writing' : 'it [the Harappan civilization] had a writing system' (p.338). My own opinion since the early 1990ies is that this is most probably not a writing system. The last flap of the cover is more prudent and leaves that question open. Next, the author describes the Harappan civilization as a "local population", inhabitants of one of the world's largest, most organized and advanced civilizations of the time'. (p.320) It remains to be proved to which degree this panegyrical description suits the real state of that civilization at that time. It seems that this Harappan area was on the contrary in a kind of crisis and past its heyday. And thirdly, the interactions between the new and the old populations are described in an "erase and rewind" mode: 'The people of the Harappan areas [...] *completely* abandoned that language, or those languages, and switch over to speaking [...] Indo-Aryan. 'This switchover was *so total* that not a trace remains of the original language (except stray words in Vedic or Later Indo-Aryan [...]). (p.314) or 'resulting in a *complete* collective amnesia in the local population and replacing their earlier "language, poetry and spiritual culture" with the new Aryan ones.' (p.313) It seems on the contrary that numerous cultural and linguistic features of the pre-existing inhabitants got incorporated in the new synthesis. There happened no 'cataclysmic transformation' (p.320, p.346). An interesting issue which may require further investigations is toponymics. On the whole, I tend to think that the ethnogenesis of the early Indo-Aryan community is not fully understood at the present time and that more work is necessary on that crucial issue. In all cases, the OIT does not shed any new light on the Indo-Aryan ethnogenesis.

The chapter also contains this wonderful description :

'The AIT case is made up of a great number of different extremely unlikely to impossible scenarios and postulates which contradict each other hopelessly: each scenario or postulate is concocted in order to explain away certain valid objections to the AIT, but it ends up contradicting most of the other scenarios or postulates concocted to explain away various other equally valid objections. The net result is a "complex" mess of chaotic scenarios or postulates which explain nothing and lead nowhere: except that all of them are intended to somehow prove the AIT case.' (p.331)

After having read the (section 2 of) the book, my conclusion is that this description best suits the OIT. At this point of the review, the book is nearly read through and it is time to answer the questions raised by the *revival of the OIT*: why should we return to the OIT if this theory has been dismissed more than a century ago? and what has changed since that time so that the reasons to dismiss the OIT should have to be reconsidered? Well, the truth is that we I have not found in the book any reason to revive the OIT now defunct *de sa belle mort* for a century or so. The author concludes this chapter with this sentence: 'And on these words, we rest our case.' (p.354) Well, my most sincere suggestion would be that the long defunct OIT should now be allowed to rest in peace.

7. *About Postscript: Identities Past and Present*

This chapter (p.355-370) is the third addition to the core of the book. It deals with 'a few minor points' and 'side-issues' (p.355), as the author describes them, which are in fact the very purposes that motivate the author's enterprise. With these purposes, we stand on the threshold of the political vested

interests of the author's version of the OIT. And I will not step beyond that point, all the less so as I have recently admired la *Porte de l'Enfer* by Auguste Rodin in the Musée d'Orsay in Paris and I have some uncanny forebodings about thresholds.

The author states that he *'can only hope that nothing written in this book is used as fodder for manipulative politics of any kind seeking to revive supposed biases, prejudices and putative identities of the past'*. (p.368) This sentence definitely sounds surrealistic after hundreds of pages where Indo-Aryan or Vedic Aryan has been constantly constructed and sublimated as an immanent panchronic (near Platonician) Entity, as old as 'originally' goes back, that has always been different from anything else and that has virginally never moved from its supposed Indian homeland. As the author explains, this chapter has 'a connection with identities and identity-based biases'. (p.355) It could hardly be clearer what the OIT is about. Previously, the author had stated what sounds like his core beliefs:

> 'The eastern ethos is deeply rooted in the Rig-Veda.' (p.100)

> 'All this shows the Vedic Aryans to be thoroughly South Asian, and originally unfamiliar with the west of the borders of India.' (p.104)

The final chapter restates a leitmotiv of the author, reiterated throughout the book, which explains why the concept of Indo-Iranian is unacceptable and unthinkable to him: 'differences or conflict between the Vedic Aryans and the Iranians' (p.355) According to the author, the so-called *dāsa* are the Iranians and their main feature is to be other than Indo-Aryans:

> 'The words *dāsa* and dasyu, on the other hand, clearly refer to the *Others* in the Rig-Veda, i.e. to the *Other*-than-the-Pūrus. But it is clear, from two circumstances, that, the words *originally and primarily referred to the Proto-Iranians* (the Anus), even though used a general term for the non Pūrus.' (p.360)

The author agrees that there were two different populations simultaneously inhabiting north-western India but he does not identify the *dāsa* with the pre-existing non Indo-Aryan population. There are numerous references to that supposed difference or antagonism between Indo-Aryans and Proto-Iranians :

> 'A most significant indicator of the insularity of the Rig-Vedic Aryans within India, and strong evidence of their original unfamiliarity with the northwestern and western areas, is the fact that Vedic traditional attitude towards these areas has always been one of suspicion, disdain or even mild hostility. [...] The attitude of all traditional Vedic literature towards people and areas further west, including the Punjab, is one of disdain or even mild hostility.' (p.105-106)

> 'They [the Vedic Aryans] actually developed an all-pervading disdain for and hostility toward those areas and the people inhabiting those areas [the Punjab-and-northwest]'. (p.107)

> 'The two traditions, Vedic and Avestan, seem to represent two entities sharing a common traditions, but as *rival entities within this common tradition*. And echoes of this rivalry persist down to the later forms of these two traditions.' (p.259)

Paradoxically, the author's insistence on the otherness of Indo-Aryans results in a very unfavorable portrayal of these people, who were 'different', 'insular', 'developing an all-pervading disdain for and hostility toward' other people and areas, with a 'traditional attitude' of 'disdain or even mild hostility', etc. The reader is left to wonder what 'thoroughly South-Asian' means. I cannot help thinking that the Indo-Aryans, and their neighbors as well, do not deserve those characterizations.

Talageri. 2009. *The Rig-Veda and the Avesta, The final evidence.*

The book ends with the evocation of the 'Battle of the Ten Kings' (p.370). I must confess to having never read or heard what this epical event is. According to the author, this may have been a kind of Big Bang of the Indo-European history. I tend to think this is more the Big Crunch of the OIT.

8. *About the Index*

The index is divided in two: a General Index and a Sanskrit Word Index. Some words are conspicuously absent from the index : AIT (but not OIT), PIE, proto-language, Pre-Rig-Vedic (but not Post-Rig-Vedic). K. Elst is cited in the Index in boldtype with no page number.

The review of the book ends here. I am still wondering why K. Elst has proposed that I (try to) make a review of Mr. Shrikant G. Talageri's book. I am not sure that my review is what they expected. It has not been 'done with unfriendly or hostile intent' (p.XXXII). I hope it will be received in the same way as I have done it, in spite of my rejection of the OIT as described in the book, which was already well-known before I was proposed to read it. I am grateful to K. Elst for giving me the opportunity to better understand the ins and outs of the OIT.

Arnaud Fournet, le 22 mai 2009

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PART 2 *Unassailable*

The book is the third one written by Shrikant G. Talageri and is dedicated to analyzing the Rig-Veda, its contents and its relevance, as regards the original location of the Proto-Indo-European homeland. The copy, received on Monday 05/18/2009, was offered by Koenraad Elst, a personal friend of the author, after I accepted the proposal to read it. The review was published on Friday 05/22/2009, five days later. It remained unanswered until recently.

Mr Talageri finally decided to address the issues contained in the review and published his own reply in May 2010.

Basically, the original review contains: (1) a factual description of the book, (2) a description of the theories, words and framework of the author, (3) my reactions and assessment to these theories, words and framework, which on the whole are mostly negative, as noted by Mr Talageri. Quite strangely, since the very beginning a number of people have been complaining that my review is "a review without substance" or has "nothing concrete to say about the book". So I will summarize what the original review states concretely about the book.

To start with, it is funny that Mr Talageri lumps me with the "Farmer-Witzel pack of jokers" when it is clear that I have little in common with these people. Mr Talageri seems to be the only one on Planet Earth who fails to see this.

Mr Talageri does not appreciate my description of the book as having a smell. I never mentioned that smell was unpleasant. It is only his own personality that turns a neutral statement into a personal aggression. One year later the book still has traces of the original smell and this has never been a problem.

I must say that I read the book according to the premise that the name on the cover was the author. In his reply Mr Talageri states that '[he is] not answerable' for a number of problems in the book, such as wrong page numbers or 'Incidentally, Elst in the index in bold type with no page number is a printer's or publisher's error for which [he is] not answerable'. Who is the author of the book? I seriously doubt that the printer or publisher is responsible for these features of the book. Before saying that nobody "has the guts in their balls to address the issues" mentioned in the book, things would be clearer if Mr Talageri could indicate what parts of the book he considers to be answerable for.

So Mr Talageri wrote "a counter-review of [my] "review" of [his] book, to demonstrate how there are absolutely no "real issues" at all "contained in the review", however fondly [I], egged on by the Farmer-Witzel pack of jokers, may be under the impression that [I have] managed to fool everyone into believing that there are." So let's now proceed to the issues which allegedly are not there.

The first issue is the typographic roller-coaster of boldface, italics and cases, the defective textual organization of the book and some inadequate titles such as "footnote" being a chapter. Mr Talageri would like to characterize these features as his personal "way of writing". To put it bluntly I consider them to be thoroughly unprofessional. Good science is not about underlined yellings in boldface.

The next issue is the data and what they are supposed to prove according to Mr Talageri: "This is proved, not on the basis of empty rhetoric of the kind which characterizes [my] pathetic "review",

but on the basis of pages and pages and pages of detailed and complete (i.e. non-partisan) data, facts and evidence - concrete evidence which can be verified or else can be exposed if false." The problem is not that the data are false or need to be falsified or improved: the problem is more that they are either irrelevant or inconclusive. They just prove nothing per se and certainly not what Mr Talageri seems so adamantly convinced they prove. Yes there are lots of data, so what?

As Mr Talageri reminds us, "Chapter 1 gives a complete analysis of the names and name elements common to the Rigveda and the Avesta". Mr Talageri tries to draw chronological conclusions about these person names. This is like comparing the names of Catholic popes, French kings and English kings and queens and trying to draw any historical conclusion from that comparison. Any historian who would do that would be declared insane. I politely stated that I "tend to think that this point is not as crucial as the author seems to believe". To put it more bluntly the method is absurd. Moreover as I pointed out in the review, adding the Indo-European morphemes and inherited patterns of these person names would immediately reveal that the approach is doomed. All these names can be created at any given time and there is no conclusion at all to be drawn from these data, however extensively and corrrectly they might be listed and shown to the reader. Can one of his friends explain this to Mr Talageri? "Chapter 2 gives a complete typological analysis of all the meters used in the Rigveda, along with an analysis of the chronological evolution of the meters, and shows how the meters used in the Gathas, the earliest part of the Avesta, are meters which in the Rigveda had evolved only by the time of the Late Books of the Rigveda". Last year I wrote that "this chapter is abstruse and it is hard to figure what these statistics actually prove". Considering that very competent philologists like Calvert Watkins have spent all their lives studying Indo-European metrics and poetry and that they have only reached unclear and prudently worded conclusions, I definitely keep thinking that this chapter just proves nothing.

Mr Talageri very emphatically restates his own dream: "the core heart of this book is the first section which presents absolutely new and absolutely conclusive evidence about the chronology (relative, internal and absolute) and the geography of the Rigveda and the Avesta. This evidence itself is enough to smash the AIT into smithereens and to prove the OIT; or, at the very least, to make it clear that it would require complete and extremely radical amendments to the AIT to produce a new version of an AIT which would try to accommodate all these chronological and geographical factors into a non-Indian homeland theory." Let's put it simple: this section proves nothing and it does not impact the standard framework in any way, even remotely or marginally. The only thing that this section does is to cast very serious doubts on the clearheadedness of Mr Talageri. Apparently he is convinced that people should reach "the conclusions which unavoidably proceed from this material" listed in the first section. Unfortunately this material is completely irrelevant and inconclusive and the methods applied are absurd. There is just nothing unavoidable and unassailable in there.

As Mr Talageri reminds us "the central topic of the book is a textual exegesis of the Rigveda and the Avesta, and this is the subject matter of the first section, which constitutes the bulk of my book." So the conclusion I would reach is that this exegesis is irrelevant and does not teach us anything at all about PIE and the issue of the PIE homeland and that this kind of approaches as experimented by Mr Talageri should be better considered hopeless and be dropped.

In his reply Mr Talageri tries to brush aside a number of issues and features of his theories that are unacceptable: the practical if not overt negation of a PIE (and Proto-Indo-Iranian) originally unified speech community, the fact that basic linguistic concepts and notions, and even plain English words, are skewed and misunderstood, the fact that the basic and technical words that usually and repeatedly appear in the books dealing with the same topic are about never used in this book. The review dismantles, describes these issues and this clearly deals with the substance and contents of the book. The words that are used and that are not used, all this teaches us what the book is about, its contents, its purposes and its limitations. The same is true with the massively lacunary bibliography. This analysis has nothing to do with "empty or jeering rhetoric", "pointless venture", "escapist remarks", "determinedly querulous complaints" nor with being "an abusive review based only on a predetermined agenda", "myopic polemics", "pedantic criticism", "glib polemical bluster" or "the

rantings of a vicious mind". It is perfectly false to state that the review is "a substitute for criticizing the actual data and logic presented in the book". The review definitely deals with them.

Mr Talageri seems confident that "[his] hypothesis (as opposed to the "Sanskrit-origin" hypotheses of most OIT writers) is a new "PIE-in-India" hypothesis backed by a completely new and unassailable range of data, evidence and arguments." As indicated in the original review there is nothing new in Mr Talageri's hypothesis and it can be discarded altogether. There is no truth to be stifled in there.

The rest of Mr Talageri's reply does not add anything and does not address any of the issues raised in the original review. It is even amazing that Mr Talageri cites extensive excerpts of my review without even realizing that he is absurdly confirming rather than refuting what I originally stated. For example he dares cite this benchmark wonder of his which utterly means nothing: "The various European Dialects, on the other hand, developed isoglosses [sic] in common [sic], separately [sic], with both the Last Dialects as well as the Early Dialects (p.242)." There is not much to be said about most of his reply which matches the criteria of what he describes as "verbal vomit". Actually the reply is two and a half times longer than the original review.

The only point that deserves a word is his citation of Meillet (1908) *The Indo-European Dialects*: "Indic and Iranian developed from different Indo-European dialects, whose period of common development was not long enough to effect total fusion". This sentence is cited by Mr Talageri as if Meillet were supporting his theory but the truth is that this sentence is not Meillet's thesis but an ill-translated and truncated reservation about his general views which are clearly stated in the book: "L'indo-européen n'est de même rien autre que le système des correspondances entre les langues communes ainsi définies : grec commun, germanique commun, slave commun, indo-iranien, etc." [Proto-Indo-European is nothing but the system of correspondences that exists between the following intermediary proto-languages: Proto-Greek, Proto-Germanic, Proto-Slavic, Proto-Indo-Iranian, etc.] There is no doubt in Meillet's theory that Proto-Indo-Iranian exists, something that Talageri negates with his Indo-Aryans who have always been separated from anybody else. "Divers faits amènent ainsi à supposer une période indo-iranienne antérieure à la période indienne et à la période iranienne commune" [A number of facts therefore indicate a Proto-Indo-Iranian period preceding the Indo-Aryan period and the Proto-Iranian period.] This can be contrasted with Mr Talageri's approach: "In our examination of the relative chronology of the Rig-Veda vis-à-vis the Avesta, the common development of the joint "Indo-Iranian" culture represented in these two texts took place in the period of the *Late* Books of the Rig-Veda." (p.81) Even the citation (or translation) is incorrect: the original text of Meillet (1908) is "Ceci n'empêche pas que l'indien et l'iranien proviennent sans doute de parlers indo-européens différents, et dont la période de développement commun n'a pas suffi à déterminer la fusion totale." [All this does not prevent Indo-Aryan and Proto-Iranian from being probably derived from distinct Indo-European dialects and their period of common development does not mean total fusion.] And "all this" refers to "near identical vocabulary, near complete identity of grammar and morphology down to the tiniest details", etc. which are extensively described in the chapter out of which the citation was drawn. It is doubtless that Meillet unambiguously refutes and rejects Mr Talageri's theories and approaches of the Indo-Aryan, Indo-Iranian and (Proto-)Indo-European issues.

As reminded by Mr Talageri, I wrote in a post: "This book proves nothing but that Talageri still has a very long way to go before he understands what the issues are about and how to write a book […] I suggest you read again the review I wrote nearly one year ago. I read it again recently and I see little to change […] For the time being, nobody addressed the real issues contained in the review." Nothing changed with this reply. I did not fool anybody into thinking that I wrote a real review when the truth is the review is "fake". The review is real and no joke: it is descriptive, relevant and clear. I understand it is not pleasant to read but I'm afraid it is deserved. Actually other readers have apparently been even harder and harsher in their reviews. The book in its current state is ill-written, ill-worded, ill-founded, unprofessional, and most of the contents are irrelevant to the issues it is supposed to deal with. And this conclusion is the only one that is truly unassailable.