A Detailed Reply to a Joker's "Review" of my Book.

Shrikant G Talageri.

Niraj Mohanka has, on 10th April 2010, sent me, presumably to elicit some reaction from me, the following comments by Arnaud Fournet made during the course of a discussion on an internet discussion site IndiaArchaeology@yahoogroups.com.:

> "**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 and keeps on dreaming on never-exist fairytales**".

Fournet refers here to a "review" he had published on www.scribd.com on 22nd May 2009 ─ that is nearly a year ago, of my third book "The Rigveda and the Avesta ─ The Final Evidence" (Aditya Prakashan, New Delhi, 2008). I had read this "review" at that time itself; but, after the initial reactive indignation that I naturally felt after reading a pointless and pompous diatribe against my book written in a jeering and sneering tone, I soon realized that there was really nothing to "reply" to in that "review": it was so utterly pointless and irrelevant. [Later, I was informed that another, even more vicious and vindictive, review had been written in a Bangalore journal by an Indian writer who has had his knife in me since quite some time. I did not think that other review even worth procuring and reading]. I decided at that time that I really could not waste my time replying to every Tom, Dick and Harry of a writer who chose to vent his spite and venom on my book or on myself just to satisfy his itching fingers, *unless he really had something concrete to say about the data, facts and evidence contained in my book*. Sad to say, *Fournet's review had nothing concrete at all to say about my book*, and did not really merit any serious reply.

But it appears Fournet is under the impression that his "review" has silenced me and others like me who choose to keep on "**dreaming on never-exist** [sic] **fairytales**". And perhaps friendly readers would like or expect me to give some reply. So I am writing this "reply" in order to clarify once and for all *as to what would constitute a genuine review of my book which would merit a reply from me*; and the best way of doing this is by giving a counter-review of Fournet's "review" of my book, to demonstrate how there are *absolutely no* "**real issues**" *at all* "**contained in the review**", however fondly Fournet, egged on by the Farmer-Witzel pack of jokers, may be under the impression that he has managed to fool everyone into believing that there *are*. In fact, Fournet's review really shows him up as being a joker par excellence.

First, let me clarify what my book is all about. *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. The second section of the book only dots all the "i"s and crosses all the "t"s (often repeating material from my second book along with an array of new evidence and logical arguments) in order to show how the OIT alone fulfils all the requirements and solves all the problems of the IE Homeland question. Any discussion on the second section *can only follow* a discussion on the first section of my book.

The first section of my book proves *beyond the shadow of any doubt* that 1) the period of composition of the latest parts of the Rigveda (latest not only according to my criteria but according to the internal chronology accepted by consensus among western academicians) goes back into the late third millennium BCE at the latest, 2) that the proto-Iranians and the proto-Mitanni emigrated from India during the period of composition of these latest parts, and 3) that the proto-Iranians and the pre-Mitanni Indo-Aryans, in the periods preceding this late period, i.e. in the periods preceding the late third millennium BCE at the latest, were inhabitants of areas to the *east* of the Sapta Sindhava region with little or no prior acquaintance with areas to the west.

This is proved, *not* on the basis of empty rhetoric of the kind which characterizes Fournet's 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.

Only <u>*and only*</u> after this evidence in the first section of my book is discussed, and either conclusively proved wrong (with the help of an alternate, and equally detailed and complete, analysis of the chronological and geographical data in the Rigveda and the Avesta), or accepted but within an attempted alternate AIT hypothesis, can any discussion spill over into the second section of the book.

This reply to Fournet's review" of my book will have three sections:

I. The Real Issues contained in the first section of my book.
II. The "Real Issues" in Fournet's "review".
III. Postscript: How to write a review.

First, let us see how Fournet deals with the core "**real issues**" contained in my book.

I. The Real Issues contained in the first section of my book.

The first section of my book is loaded with detailed masses of concrete data covering all the possible occurrences of a large number of categories of words in the Rigveda, relevant to the historical analysis of the Rigveda and the Avesta, complete with hymn and verse numbers. This is solid data, arranged systematically in tables, charts and lists, *the*

veracity of which can be verified or disproved with very little effort. The text of the chapters very systematically explains the logical significance of the detailed charts and lists, and the very precise conclusions that can be drawn from this data. *This data, and the conclusions which automatically and logically flow out from it, constitute the crux of the first section of the book*, but Fournet totally fails or refuses to even glance at this data and evidence: in fact, he finds that there are "**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** […] **All these textual and typographic features are hindrances for the reader to understand what the writer wants to say and sometimes to find the text itself amidst the references**" (notwithstanding that "the reasoning and the text of the author" and "what the writer wants to say" are based *solely* on these copious references and wordlists rather than on empty rhetoric!). And, again, about chapter 1, "**About half the pages are references which could be synthesized and organized otherwise as annexes**", and about chapter 2, "**Most of this part is references or tables**".

But, in spite of having all these concrete masses of references and data, along with detailed explanations about their meaning and import, virtually *thrust* on him in the main body of the text rather than in extraneous and avoidable annexes, Fournet resolutely ignores it all, and sums up his conclusions about the chapters on the basis of vague, impressionistic and opinionated comments which totally fail to make even the pretence of an examination of any part of the data or even to take it into consideration:

<u>Chapter 1</u> gives a complete analysis of the names and name elements common to the Rigveda and the Avesta, and shows how the major body of these names and name elements (and, incidentally, even various categories of compound word types which form these name elements), which form the common cultural elements in the two books, are found right from the *earliest* hymns of the Avesta (the Gathas) but are found in the Rigveda *only* in the *Late* Books and hymns: precisely, in 386 hymns in the *Late* Books I, V, VIII-X, but in only 8 hymns in the *Early* and *Middle* Books II-IV, VI-VII (*all 8* of which are classified by the western scholars as *Late* hymns within these *earlier* books!). Fournet sweeps aside this overpowering data, without examination, with the remark: "**We have no particular opinion about the conclusion and the method used to reach it. We tend to think that this point is not as crucial as the author seems to believe**";

<u>Chapter 2</u> 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. Fournet, again sweeps aside this concrete data, without examination, with the remark: "**This chapter is abstruse and it is hard to figure what these statistics actually prove**";

<u>Chapter 3</u> examines the geographical data in the Rigveda in complete detail, and shows how the Vedic Aryans in the periods of the *Early* and *Middle* Books of the Rigveda, i.e. in the periods *before* the development of the common Indo-Iranian culture which took

place in the period of the *Late* Books of the Rigveda, were located to the *east* of the Punjab, with little, if any, knowledge of areas to the *west*. Again, without examining any of the copious data given, Fournet dismisses the inevitable conclusions arising from this data with the evasive remark: "**Ultimately, the conclusions drawn from the Rig-Veda depend on the relative chronology chosen or determined for the books. Circularity is a permanent risk**".

Thus, Fournet sweepingly dismisses the copious data in chapters 1 and 2, *without examination*, on the ground that it is *not* "**crucial**" or that it is "**hard to figure out**".

Worse, he dismisses the copious data in chapters 1, 2 and 3 on the additional ground that the conclusions drawn are not acceptable since the veracity of these conclusions "**depend on the relative chronology chosen and determined for the books**", and that different scholars have proposed different chronological orders for books II-VII from the one proposed by me in my books (which is VI, III, VII, IV, II, V). Fournet simply refuses to examine, or even to consider, all that copious data, and simply dismisses my conclusions with a contemptuous Gallic shrug, and the escapist remarks: "**We do not have the expertise to determine which order (or if another one) should be preferred.** [….] **These philological technicalities should be addressed and discussed by competent specialists of the field**",

Here, he deliberately ignores the fact that Chapter 4 of my book makes it very clear that the veracity of the conclusions drawn by me in the first section of my book *does not in any way* depend on my chronological order for books II-VII. The conclusions actually stand confirmed purely on the basis of the *consensus among academic scholars* (the "**competent specialists of the field**") that the family books II-VII are older than the non-family books I VIII IX X, and that, of books II-VII, book V is closer to books I VIII IX X than to the other family books, so that we get *two* distinct groups of books on the basis of a near consensus among academic scholars: an *earlier* group consisting of books II III IV VI VII and a *later* group consisting of books V I VIII IX X. Fournet *himself* confirms the major part of this consensus classification: "**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**". And the fact is that all the "**copious references to the hymns and verses of the Rig-Veda**" and all the "**lists of names or nouns**" which Fournet regards as "**frequent interruptions of the text**" in my book, and as data to be ignored or dismissed, fall into two distinct and clear cut categories in their patterns of distribution in the Rigveda in line with *these very* two groups of books. Therefore, even without the help of "**competent specialists of the field**", even Fournet should have been able to verify whether my conclusions are right or wrong by simply checking the veracity of my data.

Fournet's remarks on Chapter 5 are even more surprising. In Chapter 5, I have clearly shown how all the Mitanni name types are found only and exclusively in the *later* group of books (V I VIII IX X in 112 hymns) and missing in the *earlier* group of books (II III IV VI VII, except in 1 hymn classified *by western academic scholars* as a *late* hymn in these *earlier* books). Fournet does not just find my conclusion (that the data shows that the Mitanni IA language is younger than the earlier parts of the Rigveda) unconvincing,

but he finds that "**If any conclusion can be drawn out of these data, we 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**"! How on earth, given that even *he* accepts that "**all agree**" that books I VIII IX X are "**the most recent**", does he find that "**these data**" — which clearly show that the "**Mitanni Indo-Aryan-oid**" names are found *only* in this "**most recent**" group of books, and are *totally missing* in the books which are "**admittedly the oldest**" — without any examination to disprove the veracity of the data, lead to the conclusion that "**the Rig-Veda, as a whole, is younger than this Mitanni Indo-Aryan-oid language, contrary to the author's claim**"? Just how does this joker's brain function?

So far, discussions on the Indo-European question have been based only on rhetoric and airy assumptions. When references from the Rigveda have formed any part of the evidence presented by either the OIT side or the AIT side, they have consisted mainly of stray references picked up from the text, interpreted by adding all kinds of values absent in the actual words, and made the starting points or first links of chains of similar interpretations one leading to the other and ending in momentous conclusions which bear no direct connection with the original references cited. Many of the astronomical interpretations of Vedic references cited by OIT writers fall in this category. The textual "evidence" for the AIT as a whole is almost entirely based on such interpretations: the most telling example is the way one stray word, *anās*, occurring just once in the whole of the Rigveda and never again after that in any other text, was taken as *a-nās* rather than *an-ās* which it actually was, translated as "nose-less" and further interpreted as "snub-nosed", and consequently treated in countless scholarly works over two centuries of western Vedic scholarship as evidence that the alleged native non-Aryan Indians, whom the alleged Aryan invaders/immigrants encountered when they allegedly entered India, were "snub-nosed".

The data and statistics which fill the first section of my book to the overflowing — the "**copious references to the hymns and verses of the Rig-Veda**" and all the "**lists of names or nouns**" which Fournet regards as "**frequent interruptions of the text**" in my book — form the very crux of my book and of the evidence presented by me. They consist of *complete* lists of *concrete* words (i.e. words taken in their accepted literal meanings, rather than with symbolic or value-added meanings) of different categories (including personal names, and names of animals, rivers, etc.), and the particular picture consistently depicted by the *very regular* pattern of distribution itself, of these words (as also of other data like meters), forms the crux of the evidence.

The summary of this evidence is spelt out so clearly (in the section entitled "What the Evidence Shows", pp. 43-49 of my book) that even a half-witted person, if he took care to actually read the section instead of writing an abusive "review" based only on his predetermined agenda, should have been able to understand it. And the inevitability of the conclusions drawn by me from this evidence is also spelt out so clearly (in the section entitled "Can this Evidence be refuted?" on pp.135-142 of my book) that any reviewer without sand in his brains (*if*, of course, he had bothered to read and understand what I

have written) would have thought ten times before being so summary in his dismissal of the evidence without examination.

There is only one Rigveda (as there is only one Avesta, and one known and limited treasury of Mitanni words), so it is not really possible to challenge this evidence by citing *alternative* equally complete lists of words showing a *different* regular pattern and therefore a different picture; but *a genuine critic would have examined the actual lists given by me in detail to check the extent to which they are genuine and complete, and to which they do indeed show the pattern of distribution claimed by me and justify the historical and geographical conclusions reached by me, and would have based any criticism on such an examination*. However, Fournet completely shuns examining this copious data which conclusively establishes the chronology of the composition of the Rigveda as going back into the late third millennium BCE and beyond for the beginnings of the latest parts and, almost like a joke, merely reiterates the incredible (in view of all the data in the first section of my book) proposition: "**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**"!

Fournet, like Witzel before him in his criticism of my earlier book, shows the same utter contempt for concrete references, data and statistics, and the same total reliance on mockery and on empty rhetoric. What Fournet proves in this review, as we shall see in detail, is that the *only* way in which writers like him, including Witzel before him and other likely critics after him, can afford or *dare* to deal with my book is by *completely ignoring the copious references, data, statistics, and other hard evidence actually presented by me, and the conclusions which unavoidably proceed from this material, and by substituting jeering rhetoric for analytical reasoning*. The fact is not that "**nobody addressed the real issues contained in the review**"; the fact (to put it crudely but accurately) is that polemicists like Fournet and Witzel just simply do not have the guts in their balls to address the "**real issues**" in my book.

Any review which steadfastly avoids dealing with the concrete data overflowing on every page of the first section of my book ─ avoids examining all the data and either showing that significant portions of that data are false, or showing convincingly that the data leads to conclusions other than those drawn by me ─ is a *Big Zero*, howsoever much the reviewer may pat himself on the back (and have his back patted by like-minded jokers) that he has effectively made mincemeat of my book merely on the basis of a barrage of rhetoric, polemics and derision. Fournet's "review" is nothing but a joke played by a sick joker to win the gleeful applause of other like-minded jokers.

It is up to the reader to read both my book (the reading of which Fournet claims his review renders unnecessary) as well as Fournet's "review" and to decide for himself:

a) what exactly the "**real issues contained in the review**" are, and whether they really require to be addressed at all; and also whether or not Fournet himself has in fact addressed the *very real issues* in *my* book in his "review", and

b) whether it is I who do not understand "**how to write a book**" (and have to learn "**how to write a book**" from this joker), or whether it is Fournet who does not understand how to read a book, or how to understand what he is reading even when it is set out in plain English.

II. The "Real Issues" in Fournet's "review".

Fournet steadfastly refuses to examine the *masses and masses of concrete, complete and verifiable data in the form of references, data, facts, statistics and evidence* given in the first section of my book, presumably on the ground that they do not constitute "**real issues**". So what exactly are the "**real issues**" he is "reviewing" in his "review"?

The "**real issues**" in Fournet's "review" are all *purely pedantic and polemical* issues, and the review by and large consists of a series of monologues consisting of long, convoluted and *extremely confused* polemical discussions on different subjects: e.g. the phrases "AIT" and "OIT", the concept of "Indo-Iranian", the concept of "Indo-European", the phrase "develop", and the concept of cultural change and transformation. The rest of the "review" is devoted to a pedantic criticism of the book as a whole. The monologues, as well as the rest of the "review", consist mainly of detailed semantic discussions on the meanings of different words and concepts and Freudian psycho-analyses of my alleged basic misuse or misunderstanding of these words and concepts.

Before examining the "**real issues**" raised by Fournet, it is necessary to understand *two very basic aspects* of Fournet's "review" which become clear from every word and line written by him:

First of all, it is clear that Fournet's "review" is not written with the intention of seriously examining what I have written in my book: it is written with the *sole and only* aim of sneering and jeering at anything and everything written in the book, and ridiculing and deriding my hypothesis and my person. This will become clear as we proceed with our examination.

Secondly, it is also clear that Fournet's "review" is based on the principle that "ignorance is bliss"; or rather, that "ignorance is power", since it removes all ethical, moral and logical inhibitions and constraints in criticizing and deriding.

Thus, Fournet sees no need to acquaint himself with any of the basic background material behind the book, and proudly proclaims his ignorance almost as a qualification: to begin with, he has not only not read my earlier books, but he finds that "**The book does not require any prior reading of the two other books by the same author, which were on the same topic**". In the same vein: "**We are not a specialist in Vedic or Indo-Iranian studies**"; "**Before reading the book, we 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**"; "**we would have appreciated to see what evidence in the Rig-Veda substantiates the claim of 'a mighty Sarasvatī in full**

powerful flow'. Be it right or wrong, and we have no opinion, such a claim requires to be duly documented and proved by a philological analysis, and this analysis is lacking"; "**the tribe names, Druhyus, Anus and Pūrus — we have not checked that point —** […] **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'. We are not familiar with these items and we cannot describe what added value this section of the book might bring.**"; "**The book ends with the evocation of the 'Battle of the Ten Kings' (p.370). We must confess to having never read or heard what this epical event is**".

Can a person who has not read the two earlier books "**on the same topic**" by me, who knows little about Vedic or Indo-Iranian studies, who knows virtually nothing about the OIT, who knows so little about the Rigveda that he does not know that the Rigveda speaks of a mighty Sarasvatī in full powerful flow, and has never heard about Druhyus, Anus and Pūrus, or about the Battle of the Ten Kings, presume to write a review of my third book which claims to be the Final Evidence on the subject of Vedic and Indo-Iranian history (within four days of receipt of the book: he received it on 18/5/2009, while the "review" was first posted on 22/05/2009) — a "review" claiming to be so accurate ("**accurate enough for people to assess what the book is, when they have not read it themselves**") that it can eliminate the need for his reader to expect anything more substantial or illuminating from a direct perusal of the book?

As we proceed with our examination of his critique, it will be clear from his criticism not only a) that he is proudly ignorant about *all* the background issues which form the topic of my three books, b) that he has *not* read what I have written in my two earlier books with which this third book forms a continuum, and c) that, even as far as this third book itself is concerned, he has completely ignored all the masses of "**frequent interruptions of the text**" in the form of references, data and statistics; but also, d) that he has not bothered or seen the need to really read even the "**text**" of this third book, beyond searching for passages for quotation, or scouring the text to count the number of times I have used certain words, or checking out which words are "missing" in my book, or hunting out words which he can subject to a long discussion in order to allege a semantically wrong use of those words by me — *the most telling testimony to this is the fact that he comes across any reference to the Battle of the Ten Kings for the first and last time only in the last paragraph of my book (p.370)*! e) that even the portions he has quoted often include only parts of sentences, wherein his criticism shows that he has not read the other parts of the very sentences that he is actually quoting, and f) that even when he quotes full sentences, he is not able to understand what he has read and quoted.

All this makes it all the more of a joke when he tries to copy Witzel's tactic of listing out things which *I* "do not know" and "have not mentioned" in order to show *my* alleged ignorance about the subject or my alleged failure to understand the issues involved.

Now an examination of Fournet's "**real issues**", which will help us to understand his agenda and his methods, as also to comprehend the psychological and intellectual level of his "review":

1. **<u>The smell and colour of my book</u>**: The first "**real issue**" for Fournet, is the smell and colour of my book: "**The first contact with the book has reminded us of a Sanskrit grammar we bought in China some years ago and which is our main source on that language: Fan Yu KeBen. The size, the smell, the pages, both whitish and yellowish, have kindled the same impression**". The smell and colour of the book (which I at least do not find notably different from the smell and colour of the books published by any normal western publishing house: if anything, Aditya Prakashan books are notably better than most of them) are obviously "**real issues**" more worthy of notice and comment than the copious "**interruptions**" in the form of references, data and statistics.

2. **<u>Review-politics</u>**: Even the very fact that the book was sent to him for review by Koenraad Elst is a "**real issue**" worthy of snide comment. Fournet takes care to inform us at the very outset of the "review" that the book has been reviewed more or less as a favour to Koenraad Elst: "**The copy, received 05/18/2009, was sent by Koenraad Elst, a personal friend of the author, after we accepted his proposal to (try to) review it. For the sake of courtesy, we had proposed that our review could be read by the author before being made public, but this proposal has been rejected by K. Elst. We have never had direct contact with the author**." Fournet ends his "review" with the remark: "**We are still wondering why K. Elst has proposed that we (try to) make a review of Mr. Shrikant Talageri's book. We are not sure that our review is what they have expected.**"

Koenraad Elst, at my own general request in the first flush of publication of the book, proposed sending my book for possible review to various people. That is the standard procedure when a new book is published, when a debate or discussion is sought to be initiated on the contents of the book. The proposed reviewer, naturally, always has the right, for whatever reason or even without assigning any reason, to refuse to review the book; or, if he reviews it, to criticize it in all legitimate terms (and even, I suppose, if that is his nature, in illegitimate terms). What distinguishes Fournet is his unique and peculiar code of "**courtesy**" whereby he reviews the book, but at the same time takes care to suggest in the body of his review a) that the review is more or less being undertaken almost as a favour, b) that the author was indirectly offered the chance to read the review before it was made public (perhaps in the expectation that the author would be so terrified on reading his devastating critique that he would desperately plead for a kinder review, and this plea could also then be jeeringly publicized in the body of the "review" when finally published?), c) that the author and his friend confidently expected a glowingly favourable review and would probably be embarrassed at it turning out to be a critical one after all, and d) that he himself is ultimately mystified as to why he was ever approached at all to do the review (but not, apparently, about why he did ultimately review it!). In truth, I am equally mystified on this point. On being asked, Koenraad told me that Fournet was a writer with "unconventional" ideas, and therefore he (Koenraad) felt that he would be more receptive to "new ideas". Apparently Koenraad felt that having "unconventional" ideas was a qualification of an open and honest mind, and also

that this assumed qualification was sufficient to automatically eliminate the need to have the ability to read and the brains to understand what one is reading!

3. **Fournet's mental trauma**: The tumultuous emotions that raced through Fournet's breast as he ploughed his way through the book is also another "**real issue**" eloquently placed before the readers. A sample: "[…] **In the course of reviewing the book, in the middle of the reading of section 2, we realized that the self-imposed goal of remaining neutral made increasingly no sense. We 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. We then opted from the somehow psychoanalytical anamnetic, which expresses our distantiated conviction that we 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, we have been successively disconcerted, assiduous, amazed and frightened** [...]".

The above, incidentally, is a representative sample of the style of the entire review, like that of an essay written by a school student for an elocution competition: pedantic and flowery language, with verbose and pompous words, phrases and sentences to be delivered with the right melodramatic pauses, intonations, expressions and gestures.

4. **Pedantry in academic writing**: After his outpourings on his feelings while reading my book, Fournet turns to my bibliography, followed by my preface. A little later, he turns to the textual organization of the book and the fonts used by me. Still later, he refers to the maps in my book. At the end of his review, he refers to my index. We will take up these issues here ─ bibliography, preface, textual organization, fonts, maps and index ─ as they all fall in one broad category of incidental aspects of the book as distinct from the direct subject matter of the book in the form of data, facts, evidence and conclusions.

Since the facts, data, statistics and evidence given by me are to be ignored as non-issues, these become the "**real issues**" in his review. As in Witzel's "review" of my second book, every failure on my part to follow *the reviewer's views* on the proper table manners and etiquette of academic writing (i.e. *academic equivalences*, in my writing, to a failure to use the right knife, fork or spoon while eating different dishes, to keep the cutlery and napkin in the right place, to start and to stop eating a particular course at the correct moment, to open and close my mouth in the right manner while eating, to chew the food the requisite number of times, to follow the correct rules of table conversation, to sit in the right position and at the correct angle, etc.) becomes a major "**real issue**", and every comment by the "reviewer" on each of these "failures" becomes a *devastating* indictment of my book, of my theory, of the evidence presented by me, and of the OIT itself. It

shows not only that I do not know "**how to write a book**", but automatically also that I do not "**understand what the issues are about**"!

Since the criticisms are mainly pedantic or polemical, my reply to them will be on the same level:

My bibliography: Fournet begins by noting that the bibliography is "**very short for such an issue as the PIE homeland**". This comment is superfluous since I have made the following clear statement in the preface: "**I have not adopted, and will never adopt, the fraudulent system of providing long bibliographies containing the name of every single book ever read by me (not to mention books not read by me but culled from the bibliographies of other books). The *only* books in my bibliography are those books actually quoted by me, and those referred to in any significant context**". Fournet quotes only the last part of this statement, and takes comfort in thinking he has discovered the following which gives the lie to my claim: "**It must nevertheless be noted that Oldenberg. 1888. Prolegomena, are discussed and cited in the chapter 4 but do not appear in the bibliography**". While it is true that Oldenberg's Prolegomena not being included in the bibliography is an omission, it does not really give the lie to my claim: if Fournet had understood English, he would have realized that what would have given the lie to my claim is not *omissions*, but *inclusions* in my bibliography of books neither actually quoted by me nor referred to in any significant context.

Fournet continues: "**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**". Here we see the familiar tactic of continuously demanding what is *not* in the book instead of examining what is actually there! Fournet shows clearly that he has totally failed to understand what my book is all about: the very title of the book indicates that 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. There is hardly any place for general linguistic discussions in this section. The second section of my book also has little place for books containing *general* discussions on linguistics, even Indo-European linguistics (indeed, the writings on every single technical aspect, and item of data, concerning every single branch of study of Indo-European linguistics, could fill out a number of encyclopaediac volumes or even a small library), *except where they contained data, discussions or arguments pertaining to the debate on the geographical location of the Indo-European homeland, and relevant to the subjects under discussion*. So, in view of my ethical refusal to fraudulently list out in my bibliography long lists of books read and unread just to show my erudition (take any article or paper by Witzel, for example, and see how many of the endless number of books listed in the bibliography really have any place in the concerned article), my bibliography contains just the right number of books dealing with (the *relevant* aspects of) linguistics.

After a critical reference to the book by Chang, 1988, quoted by me, Fournet resorts to the following year-wise analysis of the books in my bibliography: "**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. We 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 from Talageri himself or from Witzel". Fournet clearly has no idea at all what my book is about, not having seen the need to read it before reviewing it. Naturally, the majority of the books quoted are after 1986, since it is in the last twenty years that the Indo-European homeland question has hotted up, and all the various pros and cons of the AIT-vs.-OIT debate have been vigorously debated (and the linguistic aspects mainly by Witzel and myself, and also Hock as quoted in my book), *including points and arguments made in earlier publications*. The early foundations of Indological study go back mainly into the late nineteenth and early twentieth century, so again some books of that period are likely to be quoted. Given the subject matter of this book, very little indeed "**worth quoting has been written during the 80 years from 1906 to 1986 on the issue of the PIE homeland**". In any case, I was not aware that scholarly etiquette demands that when quoting from different books, a writer is supposed to meticulously allot an impartial quota of an equal number of books for every year or decade!

Further: "**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 avoided. It amounts to pro domo propaganda**". Nothing exposes the bias and hostility behind this fake "review" more than these comments. To begin with, *not one single* OIT writer has been quoted by me throughout the entire book: *all* the quotations without exception are from the scholarly writings of AIT scholars i.e. scholars who would implicitly or explicitly be on the AIT side in any debate (although I have given due credit to two OIT supporting writers, on pp.102 and 338, when I have made certain points; but I have not actually quoted these two writers, both of whom are non-Indian and both hostile to me, and nor are they a part of the bibliography under criticism). If the writings of these *AIT* scholars "**agree with the author**", surely it is something for Fournet to ponder over seriously instead of branding it as "**propaganda**". But these "**agreeable**" quotations are not the only ones quoted by me: I have also quoted and exposed the fallacy of almost as many AIT arguments which do not "**agree with**" me (Witzel, Hock, Lubotsky, etc.). All this is apart from the fact that the overwhelmingly largest number of references in my book are not from any writers, AIT or OIT, but *directly from the original sources*: the Rigveda and the Avesta ─ and it is these original references that polemicists like Fournet and Witzel dread the most and avoid like the plague.

<u>My preface</u>: The first thing Fournet points out about the preface is the following: "**The Preface (21 p) actually starts on page XVIII and not XV as indicated in the contents**". Obviously, I cannot answer for this printer's or publisher's error.

He then notes: "**the preface includes a listing of 'errors' and 'mistakes' made in the author's previous works** […] **This could have been preferably located somewhere else, after the bibliography for example**". So far, this criticism is legitimate: I, in hindsight, would go further and say that this list of errors was really an unnecessary

"**interruption**" not only in the preface but in the book itself, and could even have been dispensed with altogether. But Fournet does not stop here; he goes on to make the following pointless and petty comment: "**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**"! Freud? Holmes? No, it's Hercule Fournet! [Fournet tells us a little later on that the book "**can be read in a** [sic] **several ways: a surface reading of what the writer writes explicitly and deeper readings of what he assumes and thinks but does not write**". Clearly, this master psychologist cum detective has no place for the explicit data given on the "**surface**" and his whole "review" is based on these brilliant "**deeper**" pieces of Hercule Fournetian mind-reading, as we will see many times in his review!].

About my claim in the preface that my book would prove conclusively that India was the original homeland of the Indo-European family of languages, Fournet makes the following profound observation: "**It can be underlined that the wording is 'homeland of the Indo-European family of languages' and not '(Proto-)Indo-European homeland'**"! In continuation of this diversionary play on words, Fournet continues: "**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**" (note again the profundity of the last sentence!). Apart from scouring my book to find out which words are missing in my book which he feels should have been there, or in examining the semantic sense in which I have in his opinion misused certain other words, one more aspect of Fournet's "review" consists in counting the number of times I have used certain words. But he does not seem to have been very meticulous even in this utterly pointless venture: the phrase "Proto-Indo-European" is found at least 25 times in my book in this full form, and at least 40 times in the form PIE, and the word "Indo-European homeland" is found at least 8 times (notably even in the very title of the second section of the book)!

Fournet's criticism of my preface also includes a polemical monologue on the phrases AIT and OIT, apparently provoked by my references to the AIT-vs.-OIT debate in my preface. This we will examine separately.

<u>The textual organization of my book</u>: Fournet tells us at the very beginning of his review: "**The book does not have an explicit conclusion**". Later, he goes into more details about how "**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. The section 1 includes subchapters with titles like Appendix 1 and 2 and Footnote that are in fact incorporated in the body of the text.** […] **The book does not begin with a programmatic presentation of what the author plans to state or prove in the section 1.** […] **The multiple goals, compounded with**

the defective textual organization of the book, contribute to the opacity and lack of fluidity of the section"

He writes that it is difficult to know "**what the author plans to state or prove in the** [sic] **section 1**" since I do not "**begin with a programmatic presentation**" of it, but immediately tells us that his *own* "**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 argument** [sic] **in favor of 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**". Now obviously Fournet does *not* get all this "**understanding**" from his brilliant detective abilities but from the very title of the section itself, as well as from the titles of the chapters and sub-chapters and headings and sub-headings, *quite apart from the fact that the first few paragraphs of every chapter state very clearly* what "**the author plans to state or prove**" in that chapter, and the conclusions arising from the data in each chapter and sub-chapter are repeatedly hammered into the readers' attention throughout the concerned chapters and sub-chapters.

Each chapter is a step-by-step progression from one point to the next: the first two chapters show that the common "Indo-Iranian" culture originated in the *Late* Rigvedic period; the third shows us where the Indo-Aryans and proto-Iranians were (i.e. deeper inside India, and not in Central Asia) in the period *preceding* this period of development of a common culture; and the fourth clarifies how the chronological basis behind all these conclusions is *not* just the internal chronology of the books postulated by me but the one agreed upon by a consensus of western scholars from Oldenberg through Witzel to Proferes. The fifth chapter analyses the Mitanni Indo-Aryan names and shows how this analysis parallels the analysis of Avestan names in chapter one; and the sixth one shows how this Mitanni data now allows us to arrive at a rough absolute chronology for the Late books of the Rigveda. And, as Fournet himself puts it, "**repetitions and refinements of some key points provide a helpful guideline as to where the author is ultimately going**". Obviously, no amount of (more) spoon-feeding could have sufficed to prevent these determinedly querulous complaints.

About my preface, yes, I could have included the transliteration conventions elsewhere, and, as already stated, dispensed altogether with the errata. But, my inclusion of a Footnote as a subchapter in chapter one, and Appendices 1 and 2 as subchapters in chapters 3 and 4, was very logical: those subchapters pertained only to the particular chapters concerned and not to the section as a whole. And yet, they needed to be distinguished from the main point of the chapters concerned: e.g. the main point of chapter 4 was that the internal chronology of the Rigveda, on the basis of which one *inevitably* arrives at the conclusions reached in the other chapters of section 1, *is based on the consensus of western scholars*, and that these conclusions *simply cannot* be rejected without rejecting altogether this consensus of two centuries. The matter in the appendices consisted merely of additional discussions on this internal chronology, so they were

distinguished as appendices. The failure of a pedantic critic to understand this logic cannot be construed as a failure or shortcoming on my part.

The fonts used by me: "**another feature is the letter fonts, sizes and cases which often vary within any given page.**" This is counted as "**one of the hindrances for the reader to understand what the author wants to say**". Now, Fournet cannot be referring here to the "**fonts**" used for writing Vedic and Avestan words, since those are absolutely essential. He is therefore obviously referring to my use of italics and bold letters.

I have used bold letters only in titles and sub-titles and also in two special circumstances: one, in every quotation from other writers, to distinguish what is being quoted from what I myself am writing, and two, in distinguishing the hymn number from the verse number in giving references from the Rigveda. Also, in chapter one, they are statedly used to highlight names common to the Rigveda and the Avesta. I think all these uses of bold letters should in fact be *useful* in helping the reader to understand *better* what I want to say.

Likewise, the different "**sizes**" of the fonts are also used only in titles and sub-titles; and as for "**cases**", capital letters are likewise used in titles and sub-titles, and in giving references of books, e.g. WITZEL 1995b:35. Italics are also often used for specific purposes: in chapter one, they are used to distinguish the common (to the Rigveda and the Avesta) half of the names from the other parts. Again, all this should be *useful* to readers, rather than a "**hindrance**".

In the case of italics, perhaps I have the habit of using them a bit too much to emphasize words (apart from the fact that the printers have wrongly used italics in subtitles in chapters 2 and 3 where I had indicated bold letters), but that happens to be my style of writing, and I think, like every other writer, I too have the right to my own way of writing. Some of it may be very irritating to many readers; but if any of this actually prevents the reader from understanding what I want to say, it can only be if the reader, like Fournet, has set out determined not to understand what I want to say.

My maps: About the maps in my book: "**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, we 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**".

To begin with, if he is able to immediately tell us that the "**maps are centered on Afghanistan with present-day borders of the different states surrounding Afghanistan**", what was the need to first claim that he was not able to understand what the area on the maps was?

He describes the functional maps as "**low-quality**", and earlier in his review, he jibes that "**a map like the one Talageri's book displays on p.226 could have been printed in Pictet's book in 1859**". Complete with the borders of post-1947 India, Pakistan and Afghanistan, and with the inclusion of Anatolian and Tocharian (both identified as Indo-European only in the early twentieth century)?

The above comments are not only cheap, they are also cowardly: would Fournet have had the guts to say the same thing about, for example, the map depicted on pps.294-295 of H. H. Hock's article "Historical Interpretation of the Vedic Texts", in the Volume "The Indo-Aryan Controversy: Evidence and inference in Indian history", Routledge, London and New York (Indian edition), 2005? They are not only as functional ("**low-quality**") as my own maps, they are also much, much less accurate: *in the maps, the Indus throughout seems to flow from well within the borders of present-day India before flowing out through Gujarat, to the east and south of the gulf of Kutch, rather than through Pakistan and out through Sind*. Further, Fournet complains: "**The borders of the former Soviet republics (Uzbekistan, Kirghiztan, Kazakhstan) are missing**" on my map. All borders are missing in Hock's map, including those of India, Pakistan and Afghanistan.

My 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, PreRigVedic (but not PostRigVedic). K.Elst is cited in the index in bold type with no page number.**"

Criticisms of structural things, like the preface, bibliography, maps, fonts, index, and the names and arrangements of the chapters and sub-chapters (sections) of a book, must, in general, necessarily be subjective, since in most of these matters the author must be the natural person to decide what is best suited for his purpose in each of these respects.

Moreover, such criticism is always grossly disproportionate and dishonest (besides being totally inadequate as a substitute for criticizing the actual data and logic presented in the book). About his petty criticism of my index: I can genuinely say my index is the most complete index possible necessary *for any analytical study of the material presented in my book*, unlike my two earlier books whose indices had not been prepared by me and in which many key words in those books are missing in the index. Of course words like "**AIT (but not OIT), PIE, proto-language, PreRigVedic (but not PostRigVedic)**" are absent from the index, but so are words like Aryan (but not ārya), Indo-European, Rigveda and Rigvedic, and most of the (Rigvedic and Avestan) personal names in the book except those discussed or mentioned in the book in a distinctive or important context. Words which refer to the central theme of the entire book and are therefore not reference-specific, as well as words not mentioned in my book in any important quotable or referable context, are obviously excluded from my index. Such criticism for the sake of criticism can be made of any book: I challenge Fournet to send me a complete book written by him, and I will produce a long, and *much* more relevant (than the words cited by him) list of words from his book which are "**conspicuously absent from the index**".

[Incidentally, Elst in the index in bold type with no page number is a printer's or publisher's error for which I am not answerable].

5. **AIT-vs.-OIT**: Included in the preface is a polemical monologue on the terms AIT and OIT which contains many profound gems. But first, a look at two instances in this monologue where Fournet tries to show up my ignorance, by citing things of which I am supposed to be "**unaware**", and only ends up showing his own ignorance:

One: "[…] **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, and from Gamkrelidze and Ivanov, who support Eastern Anatolia as original homeland. Talageri seems to be unaware that his short bibliography includes two works proposing two theories**". If Fournet had done his homework, he would have seen repeated references in this book, as well as in my second one, to Gamkrelidze's Anatolian homeland theory as a distinct one from the Pontic-Caspian homeland theory: in this very book, notably on p. 222-23 (where in fact, in a sense, *the Anatolian theory is even bracketed together with the OIT rather than with the Pontic-Caspian theory*!) and on p.246. This is apart from the different homeland theories referred to in my first book, and the detailed analysis of Tilak's Arctic theory in my second one.

Two: "**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**". Again, if Fournet had done his homework, he would have known that this fact, that the Indian Homeland theory was one of the earliest theories which was later dismissed, is one of the favourite talking points for those writers from the OIT side who, like Fournet from the AIT side, concentrate only on polemics and rhetoric, and therefore only a particularly naïve or stupid person would assume that I could be "**unaware**" of it. It is, moreover, referred to by me in my first book which discusses the history of the homeland debate. As for the word "revolutionary", it does not simply mean "new" or "for the first time"; it means "something which introduces radical change", *even if it is the revival of an old idea or system*; and the OIT, when it is accepted, will certainly introduce a radical change in the writing of world history.

Fournet objects to the word "revolutionary" above, and later on also to the phrase "new hypothesis": "**the OIT is not a 'new hypothesis' (p.XIX) but one of the oldest theories dismissed more than a century ago**", and even quotes in detail two eighteenth-nineteenth century European writers who need not concern us here (incidentally, for some unknown reason he chooses to quote a writer who advocates the "**vast plateau of**

Iran" rather than India as the homeland!). Here Fournet deliberately obfuscates the meaning of what I have written: I have not claimed that the OIT itself is a "**new hypothesis**" but that the *particular* OIT hypothesis presented in my book is one: the full sentence used by me on p.XIX, which Fournet does not quote, is as follows: "it is easier to attack the nonsensical notions and wishful writings of more casual or biased OIT writers than to deal with a logical and unassailable new hypothesis backed by a solid phalanx of facts and data". My 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.

The monologue on AIT-vs.-OIT contains many such "time pass" comments and objections [It also contains a longish illustration of the writings of some eighteenth century French writer, which we can safely ignore]:

Fournet basically objects to the very terms OIT and AIT. He attributes this "**creation of an alternative between OIT or AIT**" to the OIT writers: he calls the AIT a label "**created by the OIT supporters**", and refers to the OIT as "**what is called the 'Out of India Theory' 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**". So far as the term OIT is concerned, it was actually coined by the AIT writers themselves (perhaps to rhyme with AIT): it was not used by me even once in my two earlier books. So I cannot answer for this term.

But, the phrase "Aryan Invasion Theory" ─ shortened to AIT again by the AIT writers themselves ─ was first used, in the present debate, by me in the title of my first book in 1993, "The Aryan Invasion Theory and Indian Nationalism". So let us see Fournet's querulous objections to this term:

Firstly, Fournet objects to this alternative between OIT and AIT since it lumps together all the other homeland theories other than the Indian homeland theory "**as if there were only one non Out-of-India Theory**", clearly because it gives the Indian homeland theory a special position vis-à-vis the other homeland theories. But he deduces the answer to this objection himself in his Hercule Fournetian manner: "**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**". Fournet does not realize how valid this explanation is (although his use of the phrase "**daughter languages**" shows he has not read pp.281-288 of my book, and is unaware of or oblivious to the complexities of so-called "Indo-Aryan" linguistics): while the homeland debate on the linguistic side is primarily concerned with linguistic change and development and not with geography-specific data, the debate on the textual and inscriptional side is based primarily on the data in the Indo-Aryan Rigveda and the Iranian Avesta and secondarily on the data in the Hittite and (again Indo-Aryan) Mitanni-Kassite records, all of which are geography-specific. The Rigveda has been interpreted throughout as the record of the Vedic Aryans moving into the Vedic territory from the northwest/north/west. In this alleged movement, whether they originally, *before they*

allegedly entered this territory from the northwest/north/west, came from South Russia, Anatolia, Eastern Europe or the North Pole, or somewhere else, is a negligible point in the data analysis, so all these homeland theories fall in one category. But if it is shown that they actually moved into this territory *from the east/southeast*, then the only homeland theory indicated, i.e. the Indian homeland theory, or OIT, obviously falls into a distinctly second alternative category.

But Fournet also objects to the term AIT because of the word "invasion" inherent in it. He tells us the AIT label "**created by the OIT supporters**" is "**not far from being a libel**" when it is "**used to describe present day scholarship", since "this kind of invasionist schemes was very much fashionable in the good old days of European colonialism** […] **it has become unpalatable to everybody at the beginning of XXIst century**". This kind of objection is only to be expected from Fournet, who has clearly not read the numerous internet debates in which the tendency of AIT writers to use terms like "migration" and "trickling-in", even while they describe blatantly invasionist scenarios in detail, has been repeatedly exposed. He could read pps.317-322 of my book, for starters, very, very carefully ─ particularly p.322.

Like a naïve child, Fournet also puts forward this objection: "**India did not exist thousands of years ago as a state and did not have (its present-day) borders**", so we cannot describe an invasion "of India" in that remote period, nor perhaps talk of an "Indian" homeland. So until we can specify with documentary proof what exactly every place in the world was named in the remote period under discussion, every geographical statement by us about that period using present-day geographical terms becomes redundant and wrong! If we prove that the original homeland was within India, we are of course wrong because there was no "**India**" with "**(its present-day) borders**" at that time. Of course, when Fournet talks about "**Southern Russia**", "**Anatolia**", "**Balkans**", and so on, all these territories existed since eternity with their "present-day" borders and names!

Fournet further fine-hones his objection: "**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)**". How innocent and idyllic! Fournet is of course, unaware that the recorded history of West Asia ─ even before the date of 1500 BCE postulated for the alleged Aryan invasion ─ is full of descriptions of established states (Egypt, Assyria, Persia, etc.) invading the territories of other established states. Or of the detailed descriptions in the Bible of the Jews coming from Egypt and invading established states in Palestine. And, certainly, of the fact that the Rigveda itself, in the description of the battle of the ten kings (which Fournet only encounters on the last page of my book), describes Sudas' invasion of the established states of the Anus. The city-states of the Indus Valley, whatever their identity, were certainly "established states" before 1500 BCE, and it is their alleged invasion that the AIT definitely describes.

Fournet uses the word "**libel**" to describe the use of the term AIT by the OIT writers; but indulges in genuinely libelous allegations himself without any compunctions: "**the**

reader is faced with the Orwellian threat5 that all the researches on the PIE homeland for centuries amount to an attempt to 'stifle the truth' (p.XXXIV) […] **5 or 'an all-out Goebbelsian campaign' (p.116)**". Actually, on p.XXXIV, I have written: "however much the entrenched AIT scholarship may succeed in stifling the truth today, they will not be able to do so for too long". I am talking of the *present day* pack of jokers like Witzel and Farmer (and now Fournet) and the entrenched political "scholars" in Indian and western academia who will try to stifle the truth written in my *present* book (published in the year 2008). And, on p.116, I am talking about the all-out Goebbelsian campaign (a very mild term in the circumstances) in *present* day India to deny the very existence of a Sarasvati river which flowed through ancient India. Neither of the two instances refers to "**all the researches on the PIE homeland for centuries**". The second expressly cannot, since I expressly point out on p.116 that this Goebbelsian campaign goes *against* what "**all the researches for centuries**" have unanimously upheld!

Further, I have not only *never* accused two centuries of scholarship of trying to "**stifle the truth**", but I have frequently expressly dissociated myself from the tendency of many OIT writers to see a colonial conspiracy in the writings of the early AIT scholars . In my first book (1993), I have given the history of the AIT without even hinting that it was anything but a purely academic theory in its origins. In my second book (2000), I have expressly pointed out that the western Indological scholars "were, by and large, reasonably honest; and although they were often wrong, they were *naturally* wrong and not *deliberately* so" (p.404). In fact, I am quite certain that most of these Indologists, if they had been alive today, would not only have been deeply interested in, but even genuinely *excited* about, the masses of original data and conclusions given in the first section of my third book under discussion; and many would have accepted my conclusions and treated my book as a starting point for new lines of research. Even if this sounds unlikely to the reader, it should at least be clear from all this that Fournet's accusation that I have claimed that "**all the researches on the PIE homeland for centuries amount to an attempt to 'stifle the truth'**" is nothing but pure libel.

6. **<u>The Mitanni evidence</u>**: After the above monologue on the terms AIT and OIT, Fournet takes up his casual dismissal of the massive data and evidence in the first section of my book, already dealt with in the first section of this reply. We will only take up here some specific grouses about the Mitanni evidence.

Fournet dismisses the unassailable evidence of the Mitanni names in chapter five, *which fits in perfectly with the identical evidence of the Avestan nanes in chapter one*, with the hypocritical and escapist charge that these are "**limited lexical items written in a fairly obscure graphic system**". Typically, he expects to get away with a vague and unsubstantiated objection, and does not have the guts to point out exactly which, and how many, of the Mitanni names and name types listed by me are *not* actually there in the Mitanni data, and have only been invented by me or have been wrongly imagined as IA names by various western academic scholars due to a wrong reading of the "**obscure graphic system**" in which they were written.

Fournet tells us: "**One of the few clear features is /azda/ attested in -1500BC in this Mitanni Indo-Aryan-oid language versus /eda/ attested one thousand years later in Vedic**". He later adds: "**The phonetic change from Indo-Iranian */azda/ to Rig-Vedic /eda/ was already completed when the Rigveda was composed**". Apart from Fournet's new implied dating of the Rigveda to 500 BCE, this statement shows that he has not read my chapter very carefully. Unlike Fournet, who must have traveled back in time in a time machine to observe the Rigveda as it was pronounced when it was being composed, most other scholars believe that the Rigveda underwent phonetic changes between the time its various parts were composed and the time the text received its final form. Witzel puts it succinctly: "**certain sounds — but not words, tonal accents, sentences — have changed**". Therefore, it is the "**limited lexical items**" in Mitanni, and not the sounds written in "**a fairly obscure graphic system**", which can help us in placing the chronological position of the *ancestral* Mitanni "**Indo-Aryan-oid language**" vis-à-vis the Rigveda.

Fournet has one more grouse: "**Moreover, the author does not address the issue of how Indo-Aryans coming from India could have moved through Iranians until reaching eastern Anatolia**". Fournet is apparently unaware of the fact that, in the history of mankind, countless peoples and tribes have migrated from one part of the world to another, "**moving through**" countless other peoples and tribes in the process. It is not clear exactly how and why this joker wanted me to "**address**" this "**issue**". But, in the process, Fournet provides an alternative answer to his main objection about the /azda/ in Mitanni: this phonetic feature may, alternately, have been borrowed by the Mitanni ancestors from the Iranians during the period of sojourn and interaction with them as they "**moved through**" them. [Yes, phonetic features *are* borrowed by languages from other languages, although Fournet may act naïve and innocent on this point as well. Even very unlikely features can be borrowed even from unrelated languages, like the tonal accents borrowed by the Vietnamese branch of the Austric languages from neighbouring Sino-Tibetan, or the clicking sounds borrowed by some non-Khoisan languages of South Africa from the Khoisan languages].

The lexical evidence, if "**limited**", is *total* and *uni-directional*, and is supported by the *exactly identical* and massive evidence of the Avestan names in chapter one. And it is *unassailable* evidence. And no amount of blustering can change this fact.

7. **"Indo-Iranian"**: Next in line in Fournet's review is a longish monologue on the term and concept of "Indo-Iranian". It starts with a rejection of my 2000 year period for the composition of the Rigveda. Now the western scholars and I are both in agreement that the *final* point of composition of the Rigveda was somewhere in the mid-second millennium BCE. But the western scholars place the *beginnings* of the composition of the Rigveda also somewhere in the same period, and have a total span for the period of composition as a few centuries in the second half of the second millennium BCE. I have shown in Section I of my book that the *beginnings* of the composition of the hymns of the Late Books of the Rigveda go back deep into the third millennium BCE: I will not repeat all the data and evidence here since it is unassailable evidence which has been presented

in full detail in my book for anyone to see. The composition of the Middle Books and before that of the Early Books must therefore go back much further. My period of 2000 years is therefore closer to the truth than the few centuries of the western scholars. There is no sense in bandying polemical arguments on this subject with a polemicist like Fournet.

The rest of the monologue is a vicious and mindless diatribe against what Fournet wants his readers to believe is my "**fixist and anti-evolutionist**" portrayal of the "Indo-Iranians" [Taking a leaf out of Fournet's book, I counted the number of times this picturesque polemical phrase is used in the review, and counted no less than seven occurrences, in which my "**mind-set**" (twice), my "**framework**" (thrice), my "**stance**" (once), and my "**approach**" (once), are all classified as "**fixist and anti-evolutionist**"]. His main claim is that I deny both the existence of the Indo-Iranians as a "**unique ethnocultural community**" as well as the inherited Indo-European heritage. But all this repetitive ranting and raving is best read in his own colourful words:

"**What is constructed in the 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', 'shared(d)', "common' and 'spread'.**[…] **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.**[…] **the author negates the very fact that the ethnocultural Indo-Iranian community could ever have existed** […] **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, they have no shared ethnocultural heritage.**[…] **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** […] **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 neighbours, 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. […] **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**". Phew!!

Now what has provoked all this passionate ranting and raving? The fact is that Fournet is totally unacquainted with the Indo-Iranian question ("**We are not a specialist in Vedic or Indo-Iranian studies**"). And at the same time he has certain dogmas on this matter installed in his brain. Like all people with half-baked knowledge and a dogmatic disposition, any blasphemy against his accepted dogmas drives him into a frenzy of passion; and facts and data which go against his dogmas, even more so. And when you add, to all this, his tendency to "review" and condemn *without reading*, the effect is explosive.

His main dogma is that the Proto-Iranians and Proto-Indo-Aryans were *one* "**unique prehistorical human community**" rather than "**two ethnocultural entities, the 'proto-Iranians' and 'the Vedic Aryans', which have been in 'continuous interaction'**". I have quoted two scholars who have stated the facts very clearly: Meillet in 1908, who pointed out that "**Indic and Iranian developed from different Indo-European dialects, whose period of common development was not long enough to effect total fusion**", and Winn in 1995, who also pointed out that there are "**ten 'living branches'** [….] **Two branches, Indic (Indo-Aryan) and Iranian dominate the eastern cluster. Because of the close links between their classical forms — Sanskrit and Avestan respectively — these languages are often grouped together as a single Indo-Iranian branch.** [….] **a period of close contact between the Indic and Iranian people brought about linguistic convergence, thus making the two languages misleadingly similar**". [Incidentally, note the word "**developed**"/"**development**" as used by Meillet above, which is also a word which drives Fournet into a frenzy of passion, as we will see later]. Even Witzel's partner in his BMAC theory, Lubotsky, concedes in 2001: "**In the case of Indo-Iranian, there may have been early differentiation between the Indo-Aryan and Iranian branches, especially if we assume that the Iranian loss of aspiration in voiced aspirated stops was a dialectal feature which Iranian shared with Balto-Slavic and Germanic (cf. Kortlandt 1978:115)**". Apart from this feature mentioned by Lubotsky, there are other important isoglosses which separate Vedic Indo-Aryan and Iranian, and place Vedic Indo-Aryan as distinct from a dialect group consisting of the other "Last Dialects", Iranian, Greek and Armenian: the conversion of certain particular positions of <s> into <h>, and the change of PIE *tt to ss.

Childish outbursts like "**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**", and "**they have no shared ancestors, they have no shared homeland, they have no shared ethnocultural heritage**" are also totally unwarranted. If I point out, *with data and evidence*, that the common cultural heritage in

the Rigveda and the Avesta represents mainly the culture acquired in a late period, i.e. in the Late Rigvedic period, it cannot be interpreted to mean that I say that they had *no* common ancestors etc. at all, except by a myopic polemicist like Fournet. Of course, as two branches of Indo-European languages, they have inherited a basic vocabulary and culture from the ancestral Proto-Indo-Europeans in common with the other branches; as geographically particularly close branches, they have developed many other features in common separate from the other branches. But these common features did not all come into being in one go at the time of the Big Bang: different periods saw different common features.

It is this myopia that provokes pathetic digs like "**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**", which are totally out of place. As I have pointed out in detail in all my books, the two "**entities**" were always in contact: the proto-Iranian priests, the Bhrgus, introduced fire-rituals to the Vedic Aryans in the pre-Rigvedic period, and Soma rituals in the early Rigvedic period. The two shared a common history in the Kurukshetra region, and as components of the broad "Indo-Iranian" Harappan culture, in the Early and Middle Rigvedic periods. By the Late Rigvedic period, the centre of the Proto-Iranians had in fact shifted westwards to Afghanistan, but it was the period in which they developed a common name-culture; and the evidence of this name-culture, among other things, shows that this *Late* Rigvedic period was the period in which the *earliest parts* of the Avesta were composed. And, even post-Rigveda, the two cultures continued to develop common features like the *upanayana/navjot* ceremony and other ceremonies common to the Vedic and Zoroastrian religions, as well as some mutually antagonistic mythological or theological concepts (like the deva-asura opposition), all of which, as Humbach, quoted in my book, points out, "**suggests a synchrony between the later Vedic period and Zarathustra's reform in Iran**" (i.e. *eastern* Iran or Afghanistan).

Fournet's has objections on more specific aspects of the Indo-Iranian evidence analysed by me:

To begin with, he complains: "**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**". Here, Fournet arbitrarily opens up the book, and then examines the page on which it opens up to see if he can find on that particular page any statement to the effect that "**the Indo-Iranian culture inherited a considerable number of ethnocultural and mythological items from the original PIE community, apart from words and grammar**"; and when he does not find it this means, of course, that I know nothing about these things. Since Fournet, as we know, has not done his homework, he is "**unaware**" that both my earlier books contained separate chapters on mythology which deal with this common mythological heritage in detail. The question of discussing this mythology, or any other common Indo-Iranian "**ethno-cultural items**" (like fire-worship, soma, the thread ceremony, etc., whether "**inherited from the original Indo-European**

community" or developed, i.e. "**created ex-nihilo**", by the Indo-Iranians) just did not arise in this book in the context of discussing the chronology and geography of the texts based on the distribution of different categories of names and words, although some of the "items" are mentioned in the second section.

Even in respect of the words and grammar, Fournet finds that I have been lax in detailing the Indo-European "**heritage**": after some diligent checking of my book with a magnifying glass (e.g. "**a careful search has made nearly sure the** [sic] **section 1 never uses the word inherit(ed)**"), which must have taken up the major part of the four days which it took him to read my book and complete and upload his review, he writes: "**The book starts with an analysis of the person names found in the Avesta and the Rig-Veda, as listed by Mayrhofer.** […] **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 protoform. 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.**" It is not clear why Fournet wants to find words "**like cognate word, change, phonetic, correspondence, proto-language, etc.**" This book is not an introductory book to the Indo-European problem ─ Fournet can not complain about the fact that I have not reproduced the entire contents of my two earlier books in this one just for his benefit (and not that he would have read it if I had). And nor is it an etymological dictionary of the Proto-Indo-European language or a comparative dictionary of the Indo-European languages. I am analyzing the chronology and geography of the Rigveda and the Avesta in the first section of this book, and the various strands of evidence for locating the original homeland in India in the second. Technical discussions on the *etymologies* of words have no place at all in this analysis. But for these gutless "reviewers", keeping up a litany of querulous complaints about things I have not mentioned or written about is a clever and diversionary polemical substitute for having to deal with things that I have *actually written about in detail*. There was no reason for me to include unnecessary words, data or subjects in my book, unless as a pastime, or to increase the bulk of my book, or just to show my erudition.

In this particular case cited by Fournet, for example, the "**reconstructed PIE protoforms**" of the common Indo-Iranian name elements were totally irrelevant: the relevant issue was not even whether the individual names were Indo-European or not, whether as "**inherited from PIE**" or as unique "Indo-Iranian" words developed (="**created ex-nihilo**") by the Indo-Iranians, or whether they were borrowed from some supposed BMAC language or some supposed "language X" of the Indus Valley or from Semitic, Burushaski, Sino-Tibetan, Munda or Dravidian. The relevant issue was that this overwhelming mass of common name-culture, *whatever its individual origins*, is found among the Mitanni as the remnants of a dead *ancestral* heritage, and is found right from the *oldest* parts of the Avesta, but is found only in the "**most recent**" parts of the Rigveda and is *completely missing* in the parts which are "**admittedly the oldest**". Clearly, there is method behind this joker trying to divert the discussion from the relevant issue.

Fournet shows his total inability to comprehend what is placed before him, or even to use his brains and think, when he complains: "**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)**". It is clear that this joker cannot even comprehend the difference between inherited roots and inherited names. Is it Fournet's contention, for example, that the Rigvedic name Shyavashva (patronymic Shyavashvi, Avestan Siauuaspi) is actually a personal name inherited from the Proto-Indo-European community: i.e. that the Proto-Indo-Europeans had a personal name like *khy-e-H-ekhwos, which has been "**inherited**" by the Indo-Iranians? Since the components of the words tele-phone and tele-vision are also traceable to their Proto-Indo-European roots through Greek and Latin, are these words and concepts "**inherited**" from our Proto-Indo-European ancestors? Personal names *do* indeed come into existence, "come into vogue", and "go out of vogue", even if the root component parts of those names may have been in existence from long before. Fournet's objection only shows up the pointlessness and inherent stupidity of his review.

8. **The Evidence of the isoglosses**: Fournet next moves on to the second section of my book, and starts out on his "review" of chapter seven, "The Evidence of the Isoglosses". This part of his review covers three full sheets out of less than twelve sheets which constitute his review. Here Fournet is in his element since he does not have to face massive masses of unassailable data and references, and this leaves him with greater scope for glib polemical bluster and semantic hair splitting. Having abused the first section of the book all he can, Fournet now tells us: "**On the whole, the 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 the section 2**".

Chapter seven contains seven sections (or sub-chapters as Fournet calls them), of which the first five sections deal with the main subject embodied in the title: the first two A and B deal with Hock's case for the evidence of the isoglosses (of which B deals with my examination of Hock's case), and the next three sections C, D and E present my case for the evidence of the isoglosses. We will deal with Fournet's "review" of this main part of the chapter here, and will examine his "review" of sections F and G, which represent two different aspects of the linguistic evidence, separately.

These five sections of chapter seven deal primarily with the evidence of the isoglosses, and I show very clearly and logically that Hock's case for the evidence of the isoglosses is wrong, and that the Indian homeland theory alone can explain all the isoglosses. Further, all the corroborating evidence is also detailed in full: the fact that the earliest historical locations of the Early Dialects (Hittite and Tocharian) are most logically explained by the OIT, the fact that the Early Dialects and the Last Dialects both share isoglosses with the European Dialects but not with each other, the fact that the linguistic evidence detailed by Johanna Nichols (ancient loanwords from Semitic and Sumerian words in Indo-European, the geography of the *centum-satem* split, etc.) show that the locus of the IE spread was in ancient Bactria-Margiana, the evidence of Chinese,

Yeneseian and Altaic loanwords in Germanic, the one-way traffic of borrowings from Indo-Iranian into Finno-Ugric, etc., apart from literary evidence from the Rigveda and Avesta.

Fournet completely ignores all this evidence, and resorts to his usual tactics. To begin with (after a brief discussion of the maps in my book, and some polemical comments we will examine presently), he chooses to discuss the word "isogloss" rather than the actual evidence of the isoglosses: according to him, my entire scenario is underlined by "**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 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).**".

Hock is probably successful, so far as his "review" goes, in diverting the discussion from the evidence of the isoglosses to the meaning of the word isogloss. But he also succeeds in showing up his stunted intellect, and the fact that he never sees the need to go beyond the most basic one-liner commonest-meaning dictionaries meant for primary school children. Yes, most dictionaries would definitely define the word isogloss merely as "**a boundary line between places or regions that differ in a particular linguistic feature**" (Merriam-Webster's Online Dictionary). But it *also* means the area enclosed by this line: "**In dialect geography, an area within which a feature is used predominantly or exclusively** […] **more commonly, the line on a dialect map which bounds the area of a certain usage**" (Concise Oxford Companion to the English language, 1998, Tom McArthur). And finally, it *also* means *the feature itself*: "**An isogloss refers to a specific type of language border** […] **Within the field of linguistics (including historical linguistics), the term 'isogloss' describes a distinctive feature of a language or a dialect (see volumes such as *The Cambridge Encyclopedia of the World's Ancient Languages*, ed. Roger D. Woodard).**" (absoluteastronomy.com).

In any case, Fournet realized that *I* at least have used the word isogloss in the sense of "**a shared innovation, a shared conservation and an areal feature**" (and therefore naturally also "**a kind of permanently inheritable and transportable feature**"). So he could have examined my analysis of the evidence of the isoglosses on that basis. But he escapes doing so by claiming that I have misunderstood Hock's representation of isoglosses, and that therefore my case makes little historical or linguistic sense. But Hock also describes "**shared features**", and the whole point of his representation is that these "**shared features**" in the homeland were "**a kind of permanently inheritable and**

transportable" features, which were inherited and transported by the various IE dialects from their common homeland to their various earliest inhabited historical locations. So basically, apart from the diametrically opposite conclusions, and the more complete range of "**shared features**" presented by me, there is no difference at all in what Hock represents and what is represented by me. The only difference is in name: Hock uses the word isogloss in its most common sense as a line demarcating areas with shared features ("**a dialectological approach that maps out a set of intersecting 'isoglosses' which define areas with shared features**" in his own words) while I use it in the *also valid* sense as the feature itself (not, as Fournet suggests, from an "**erroneous interpretation**" of Hock's use of the word, but because I had already used it in that sense in my second book in 2000). Conveniently, Fournet uses this as an excuse to avoid having to deal with the evidence of the isoglosses as presented by me.

Fournet compounds his discussion on the word "isogloss" with a discussion on the words "development" or "to develop". The climax of Fournet's "review" of chapter seven is his passionate monologue on my use of the words "development" and "to develop" in my book, words which seem to drive him into a frenzy of uncontrollable fury or madness. We must take note of it in full, tedious though it is. To distinguish clearly Fournet's own words from places where he quotes my book, I will underline the parts quoted by him from my book, and place in bold type Fournet's own words of wisdom:

"**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. We have 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-Rigvedic'. (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 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 places, 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 223-226, 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 Colorless green ideas sleep furiously of Chomsky. Being a structuralist, we shall leave to generative-transformists the task of turning the above sentence into the passive voice. On the whole, this chapter of the Section 2 reveals the multiple inadequacies and flaws of the author's fixist and anti-evolutionist approach. In contrast with the Section 1, which contains stimulating elements, potentially requiring further analysis, this chapter of the Section 2 can be rated to be a near complete intellectual wreckage. About nothing (< 5%) has any scientific value or status.**"

What does one call all this: a philosophical discourse, a semantic dissertation, a Freudian psycho-analysis, or just the rantings and ravings of a maniac? The last part of the monologue is certainly nothing but pure venom and hate. But the one outstanding aspect of the whole monologue is the masterful way in which Fournet completely diverts the

discussion and attention from all the masses of material data and evidence to the purely semantic issue of the meaning of one word.

The word "develop" has many meanings: "create" or "invent" is one of them (e.g. "he developed a new machine/system"). But in *not one* of the instances given by Fournet does the word "create" (much less the gratuitous phrase "create ex-nihilo") fit in with the sense of the sentences where he replaces ""develop" with "create ex-nihilo". The word "evolve" would be a more correct replacement (except in the one instance where he translates as "began to feel"). But neither "evolve" nor any of the other synonyms (acquire, grow, build up, alter, change, expand, generate, become, modify) would express the composite meaning of the word "develop" which goes beyond all its synonyms and is the absolute *mot juste*.

It is clear that Fournet has not understood Hock's hypothesis *at all* (*if*, that is, he has even read any of it outside the references to it in my book!). The main point of the hypothesis is that the various IE branches (originally "Dialects" of PIE) share, among each other, certain linguistic features (which I, *not incorrectly*, call isoglosses). Different branches share different isoglosses with different sets of branches. The logic is that, in the original homeland, the original Dialects, which gave birth to the later branches, shared these isoglosses with each other *when they occupied contiguous areas*: in short, the isoglosses are "areal features" in origin (in the original homeland). Although the branches occupied distant areas in historical times, the isoglosses give testimony to the fact that the original Dialects *were* in contiguous areas. But there are different isoglosses which cover different dialects: i.e. Dialects A, B and C may share one isogloss in opposition to Dialects D, E and F; while A, B, D and E may share another isogloss in opposition to C and F. Hock purports to present a dialectological arrangement which shows all the Dialects sharing isoglosses with each other in contiguous areas, in such a way, or ways, as to explain *all* the isoglosses. Hock's contention is that his arrangement shows the hypothetical geographical positions of the different Dialects to each other in the original homeland to be more or less the *same* as the actual geographical positions of the respective branches to each other in their earliest attested historical periods.

I, on the contrary, show that Hock's dialectological arrangement does *not* explain all the isoglosses, and, in fact, leaves many important isoglosses unexplained. In order to explain all the isoglosses, three things are required: a homeland to either the north or the south of the broad historical east-to-west Indo-European belt, a common exit point from this homeland onto this belt where exiting branches would remain in contact with each other after exiting the homeland (*this* alone explains the isoglosses shared by far apart branches like Hittite, Tocharian and Italic), and a shifting series of movements which would bring different branches in contact with each other in different periods. Such a scenario from an Indian homeland explains all the isoglosses, as well as a host of other linguistic features and phenomena, of the different branches more logically and fully than any other homeland scenario; and all this is fully corroborated by the actual recorded textual evidence of the Rigveda and the Avesta.

If Fournet is too busy searching out the most basic and simplistic meanings of individual English words (isogloss, develop, etc.) from the substandard dictionary used by him, and concentrating only on rhetorical and polemical arguments on the meanings of these words, rather than trying to understand the real issues involved here, I at least have better things to do.

9. **Minor points on the evidence of the isoglosses**: Fournet's discussion of the evidence of the isoglosses is littered with all types of petty or time pass comments of a polemical nature:

At the very beginning of the discussion, I point out that there are two versions of the OIT (correctly distinguished by Hock): the ""Sanskrit-origin" hypothesis and the "PIE-in-India" hypothesis, and that I represent the "PIE-in-India" hypothesis. Fournet has the following snide comments to make on this point: "**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**". Now does Fournet himself believe that the Vedic language is the ancestor of all the Indo-European languages? If not, does it mean that he also believes that the "**Rig-Veda Aryans have always been different from anybody else**", and that the Indo-European languages have "**no ancestor at all**"? If not, then why should it mean that *I* believe it? Here we have a prime example of petty criticism for the sake of criticism. But this is the childish theme which reverberates through Fournet's time pass "criticism":

"**It must be emphasized that the scenario proposed by the author is not a homeland for the Proto-Indo-European family, nor a protoIndo-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 Indo-European languages (or branches) as having contiguous individual homelands. In other words they have never been anything but neighbours. 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-Europeans became what they aiways were, but they did so further away.[…] The scenario described in the book is teleological […] 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 has always existed, at least as far back as 'originally' may go, and he has to**

posit that all Indo-European languages were equally existing from the same point, to when 'originally' goes back."

There is more repetitive ranting and raving in the same vein. What Fournet's dim brain fails to comprehend is that, whether he likes it or not, all these foolish and myopic charges would apply equally well to the presentation of the isoglosses by Hock in his article, "Out of India? The Linguistic Evidence" (1999), to which my own presentation in this chapter is a reply. Hock also does not start the Proto-Indo-European story from the embryonic and foetal stages, but starts it with the various Dialects ancestral to the various branches already basically differentiated from each other, "**having contiguous individual homelands**", with "**all the components of the Indo-European family in a reduced and telescopically concentrated format**" etc.. Or perhaps, it does, and he thinks a simple assertive denial will do the trick: "**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**". Well, if wishes were horses, Fournet would ride. My presentation, except in its more complete survey of the isoglosses, and in its diametrically opposite conclusion, is of the same type as that of Hock, and if Fournet does not have the guts to deal with the evidence, he could at least refrain from opening his mouth too wide and making a fool of himself.

Fournet indulges in more semantic hairsplitting, or criticism for the sake of criticism: he refers to my "**decided contention (for political reasons) that Indo-Aryan is not a 'dialect' (p.236) but a 'branch' (p.223). 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 between** [sic] **Anatolian**". So all the scholars and linguists who have referred to Indo-Aryan as a "**branch**" (including Meillet and Winn quoted earlier), and all those (and this includes the *overwhelming majority* of western linguists and other academicians) who refer to Tocharian, Hellenic, Germanic, Celtic, etc. as "**branches**" rather than as "**sub-branches of the non-Anatolian branch of the Indo-European family**" all have their "**political reasons**" for doing so? Fournet seems to be unaware that the "Indo-Hittite" theory he is upholding here is almost as dead as a dodo, and Anatolian is now treated as one more branch of Indo-European, albeit a special one. [Incidentally, I have treated, in this context, the words "**dialect**" and "**branch**" as almost synonyms, the original IE "**dialects**" later developing into the different "**branches**". Where does this joker catch me claiming that Indo-Aryan is "***not* a dialect but a branch**"? I do so neither on p.236 nor on p.223, nor anywhere else].

About my very logical contention that the one-way traffic of borrowings from Indo-Aryan and Iranian into Finno-Ugric proves that the Indo-Iranians did not pass through the Finno-Ugric areas in a migration towards the east, but that west-migrating groups of Indo-Iranians imparted those words to Finno-Ugric, Fournet calls it an "**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'**". This criticism falls in

the same category, call it what you will, of OIT writers who reject the very existence of the Proto-Indo-European language because it is "**unattested**". Fournet fails to realize that the very fact, that the Indo-Iranian languages of the south do not "**attest**", with Finno-Ugrian borrowings, their having passed through the Finno-Ugrian areas, itself "**attests**" to the existence of west-migrating Indo-Iranians lost to history.

Later, Fournet again shows his total inability to comprehend written English, in the following simplistic criticism. He quotes my statement denying that "**the 'sequential movement of different groups' Out-of-India hypothesis (postulated by *no-one*, so far as I know)**" (p.306 of my book) forms any part of my hypothesis, and writes: "**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 upto 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**". Only a person with a brain like Fournet's could have asked such foolish questions. *Nowhere* have I said that the emigrating branches of Indo-European did not move out in sporadic sequential movements. Obviously, they were not running a relay race that they all started sprinting out the moment the whistle was blown. Fournet quotes different sentences to show that I describe sequential movements. But, when he quotes my allegedly *denying* it, he deliberately quotes only half the sentence: I am *not* denying sequential movements per se, I am denying "**the 'sequential movement of different groups' Out-of-India hypothesis (postulated by *no-one*, so far as I know) argued against by Hock (HOCK 1999a), which would treat the various Indo-European Dialects as moving, one by one, *out of the bottle-neck routes leading from northwestern India to the outside world, after having developed all the isoglosses within India*"**. Note the key words I have placed in italics here, which Fournet *deliberately* avoids quoting. As I have shown, all the isoglosses developed, in stages, *outside* this bottleneck area.

Fournet also quotes my references to the Druhyus, Anus and Purus, but as he is admittedly totally ignorant about these matters, his views, that my homeland hypothesis is "**hallucinatorily absurd**" and that my "**intellectual framework is flawed to a (possibly) hopeless extent**", and (note the sheer poetry of this) that "**this part of the book has a kind of incoherent and unworldly flavour that borders on Nostradamus' predictions**", I will treat as merely the rantings of a vicious mind.

Fournet also writes: "**For reasons that 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 we tend to consider his rejection of these two points unacceptable in the first place**". As we

have been seeing throughout this reply, and will see more clearly in the next point (9. A fake review) Fournet criticizes without reading, and we can safely say that he has *not read*, or has *totally failed to understand*, what I have written on p.222, where I have explained why a common exit point is necessary. [Incidentally, note his confused logic or confused English: in the first sentence above, he correctly states that I *reject* one point, about a homeland "situated in a central area", while I *require* another point, "a common exit point". In the second sentence, he suggests that I *require* both the points (a homeland "situated in a central area as well as "a common exit point"?). In the third sentence, he says that I *reject* both the points!].

Funnily, Fournet also complains that, in my hypothesis, I portray Indo-Aryan as a branch which "**has never moved from India: 'Indo-Aryan, the Dialect which remained in the homeland after all the others had left' (p.277)**". But why does this surprise Fournet, or why does he find it worthy of comment? Since my hypothesis portrays India as the original homeland, isn't it understood that it is intrinsic to the hypothesis that Indo-Aryan, which is historically native to this area, automatically stands portrayed as a branch which "**never moved from India**" (although two *groups* from among the Indo-Aryans did do so: the proto-Mitanni, and the Indo-Aryans who moved westwards through the Finno-Ugrian areas)? A time pass comment to beat all time pass comments.

9. **<u>A fake review</u>**: Fournet's "review" of sections F and G of chapter seven of my book is particularly interesting, since it exemplifies more clearly than anything else the fake nature of his whole review, and the fact that Fournet compulsively "reviews" without reading, or else that he is pathetically unable to comprehend *at all* what he is reading. [While section G alone is entitled as an Appendix by me, actually these two parts should have been entitled Appendix 1 and Appendix 2 respectively, since they represent two aspects of the linguistic evidence different from the rest of the chapter].

Fournet has the following to say about section F, "The linguistic roots in India": "**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. Our 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** […]"

Incredibly, this section, far from being a summary of them, has nothing whatsoever to do with "**the preceding pages from the start of the book**", and deals with linguistic evidence totally different even from those discussed in the sections dealing with the evidence of the isoglosses. It deals with the "two-waves-of-migration" theory and its incompatibility with the literary and linguistic evidence, with the question of non-Vedic dialects of "Indo-Aryan", with the l-and-r phenomenon in "Indo-Aryan" linguistics and how it totally shatters the AIT paradigm, with the evidence of Bangani and Sinhalese, and with suggested affinities between Indo-European and Austronesian. None of these topics has even been touched upon in "**the preceding pages from the start of the book**". Fournet unilaterally deduces, from the title of the section and perhaps a glance at the first

paragraph or two, that this section represents "**a kind of summary of the preceding pages from the start of the book**". Hercule Fournet does not stop there; from this first deduction he draws another one: "**that a first version of the book may have stopped here and that new chapters were added later on**". Then he draws a third deduction from the second one (and the giant leap in logic involved here is totally beyond me, and would probably leave even Sherlock Holmes, Hercule Poirot and Miss Marple foxed): that this might "**explain the erroneous reference: page XVIII and not XV for the preface**".

Fournet's summary treatment of section G, "Witzel's linguistic arguments against the OIT", is even more hilarious. This is what he has to say about this section: "**The next sub-chapter (p.290-307) is focused on previous exchanges with the author's bête noire aka Michael Witzel. There is nothing new in this part of the book. The part was probably added to the book because some of its content has not been published elsewhere as the author had wished (p.290)**".

It is clear that Fournet has not even glanced at this section, since every word written by him here is factually wrong. I state at the very beginning of this sub-chapter or section that it represents *my point-by-point reply* to the summary of the linguistic arguments against the OIT made by Witzel *in his article in the Bryant-Patton volume published in 2005*. It has nothing whatsoever to do with any "**previous exchanges**" with Witzel. This is the first time that I replied to this article by Witzel, and I do not know that anyone else had done so before me; so this part of the book should have been *absolutely* "**new**" to any reader, and certainly to Fournet, who, as per his own admission, knew nothing about the OIT: "**Before reading the book, we 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**".

But Fournet attempts a Hercule Fournetian deduction here too: "**The part was probably added to the book because some of its content has not been published elsewhere as the author had wished**". Where does Fournet get this idea? I explain on p.290 that the article by Witzel was published in the Bryant-Patton volume published in 2005, which also contained an article by me. But while I was expressly not allowed to update my article (which had been given to the publishers in 1998, seven years before it was published), Witzel was allowed to update his article almost to the last minute. This explanation by me is interpreted by Fournet to mean that *this present* sub-chapter by me (pp.290-307 in my book published in 2009) contains material which I wanted to publish in the 2005 Bryant-Patton volume but was not allowed to do so! Now this sub-chapter consists of *my point-by-point reply* to Witzel's article published in the 2005 Bryant-Patton volume, which of course I could only have read *after* that volume was published and I received a copy of it much later. So how could it be possible that this reply to his article could contain matter which I wanted to publish in that very volume itself but was not allowed to do so?

After all his bluster about the lack of linguistic discussion in my book (and he means of course etymological discussions about Proto-Indo-European roots!), Fournet not only

completely ignores all the linguistic discussion in these two sections, but shows up his total inability to even comprehend what the sections are all about. But none of this stops him from "reviewing" and making masterful deductions, and arriving at condemnatory conclusions!

10. **The archaeological case**: Fournet then moves on to review chapter eight of my book, "The Archaeological Case". Since this chapter is based more on logical arguments, and less, or almost not at all, on hard data and evidence (which makes Fournet uncomfortable and nervous), Fournet is a bit more comfortable with this chapter: "**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** […]**. Moreover the content of this chapter is considerably better than the rest of the section 2**". But his "kindness" stops at this point.

He starts out by a repetition of his earlier Fournetian deduction: "**As mentioned before, we suspect that this part of the book was probably added to the book in a second (or third) phase of its composition**". […]. Hercule Fournet even provides the two clues which pointed the way towards this deduction: first: "**There are some lexical differences with previous chapters**" (meaning the above "**fully-grown-up**" use of certain words), and second, "**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.**" Therefore, one more deduction: "**The first page is a kind of apologetic transition for the addition of the chapter.**"

To Fournet's myopic vision, nothing seems to be what it actually is: earlier, he deduces that "**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,**" when it was nothing of the kind. Now, when I actually state, on the first page of chapter eight, that the aim of this "final chapter [is] to sum up the case and present it in final perspective", it looks to him like an apology for the "**addition**" of this chapter. Actually, right from my second book, I have constantly been pointing out that there are three disciplines involved in the study of the Aryan question: linguistics, archaeology and textual analysis. As the entire first section is devoted to the unassailable masses of textual references which conclusively prove the OIT, it should be clear to anyone with a modicum of sense that a second section with two chapters on the linguistic and archaeological aspects of the case was inevitable from the beginning. But having made a deduction, Fournet must find clues for it, ranging from "**lexical differences**" to an "**apologetic**" attitude.

While it is true that "**the case is settled once and for good in favor of the OIT**" on the basis of the unassailable textual evidence and a consideration of all the linguistic evidence, it is unthinkable that the discussion could ever have been sought to be concluded without considering the archaeological (including anthropology, etc,) position.

The aim of this chapter is threefold:

First, to show very conclusively that this third discipline also, although based only on logical arguments rather than hard data (since archaeology has failed so far to yield any hard, concrete evidence for *either* the AIT *or* the OIT), "is *not* neutral in the debate so far as the AIT case is concerned: archaeology stands in sharp *opposition* to the AIT and conclusively disproves it. At the same time, archaeology is more or less *neutral* so far as the OIT case is concerned: although there is obviously no conclusive archaeological evidence for the OIT scenario, this circumstance does not disprove the OIT. There are many basic reasons why archaeological evidence is *vital* for the *AIT* to be accepted as valid, but archaeological evidence is *not* vital for the *OIT* to be accepted as valid, and we will see this in detail in this chapter." (pp.311-312 of my book).

Second, to show that the AIT contention that a "non-Aryan" Harappan civilization was replaced by an "Aryan" one is *untenable* since it consists of a *combination of several totally incompatible postulates* ─ a combination which has no parallel anywhere else in the world, and therefore represents an extremely unlikely to impossible scenario.

Three, to show that, because of this combination of several incompatible postulates, the AIT writers are compelled to concoct different scenarios to explain away the facts which contradict these postulates, but these scenarios also end up contradicting each other sharply

It is these three points that are explained *in great detail* in chapter eight of my book with facts and illustrations. And it represents a logical conclusion to the rest of the book.

Fournet so utterly fails to understand (assuming he has really read any portion of the chapter) the inevitable logic of the detailed case presented in this chapter, that he writes: "**Another point is the reluctance of the author to accept an ethnocultural and linguistic shift in a short period. There exists** [sic] **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 Turkic to Russian in three generations**".

Fournet here illustrates the second point that I am making in this chapter: that the contention that a "non-Aryan" Harappan civilization was replaced by an "Aryan" one is untenable since it consists of a *combination of several totally incompatible postulates* ─ a combination which has no parallel anywhere else in the world, and when AIT writers like Fournet (see also Witzel on pp.318, 320-322, and Hock on pp.326-27 of my book) try to show parallels based on only one or two postulates they only succeed in highlighting the unparalleled nature of the contention *in respect of all the other postulates*.

Thus, taking only the single postulate of "**an ethnocultural and linguistic shift in a short period**", Fournet cites the alleged parallel of the Gauls becoming Gallo-Romans in "**fewer than five centuries**" and a section of Uralic people changing from Samoyedic to

Turkic to Russian in three generations. But he ignores the *sharp opposition* between these cases and the alleged AIT case in respect of all the other postulates:

Firstly, there can be no comparison between the Gauls and the Samoyedic groups (with full respect for their culture) and the Harappans in respect of civilizational status. However much it may irk Fournet ("**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**"), the Harappan civilization was one of the great ancient civilizations, on par with the Egyptian, Mesopotamian and Chinese civilizations and even superior to them in a few respects (while they may have been superior to it in some others). Even when it was "**past its heyday**", its inhabitants continued to be the inheritors of that great civilizational tradition.

Secondly, the unorganized Gauls were conquered, overrun and ruled by the Romans, whose massive and organized military apparatus was unparalleled in the world at that time and is still an object of respect. Likewise, the unorganized Samoyedic groups were repeatedly conquered by Altaic people, and finally came under the control of one of the most totalitarian states, the basically Russian USSR. On the other hand, the multitudes of Harappans are alleged to have been completely transformed ethnoculturally and linguistically by a few people, less culturally advanced than themselves, "trickling" into their midst or (according to Witzel) by "**just one 'Afghan' IA tribe that did not return to the highlands but stayed in their Panjab winter quarters in spring**".

Thirdly, there are ample textual records and archaeological evidences, and strong traditional memories, which testify to the earlier languages and ethnocultural identity of the Gauls and the Samoyedic people, and to their conquest and linguistic-ethnocultural conversion by the Romans and the Russians respectively. The alleged AIT case has left not the faintest textual record or archaeological trace of any earlier language and culture, or of this alleged total linguistic-ethnocultural conversion from non-Aryan Harappans to Aryan people, and not the faintest traditional memory of it either among the local people or in the oldest Aryan texts.

Fourthly, the conversion of the Gauls and the Samoyedic people was on a relatively superficial or outer level in comparison with the alleged conversion of the non-Aryan Harappans, which was so deep, total and complete that even Witzel remarks on the unparalleled nature of this totality. He describes as "**relatively rare**" what we (allegedly) see in the Harappan transformation "**with the absorption of not only new languages but also of an entire complex of material and spiritual culture, ranging from chariotry and horsemanship to Indo-Iranian poetry whose complicated conventions are still actively used in the Rgveda. The old Indo-Iranian religion, centred on the opposition of Devas and Asuras, was also adopted, along with Indo-European systems of ancestor worship**". Even more startling is the fact there was even the rivers have purely Aryan names even in the very oldest texts, with no evidence to suggest that those rivers ever had different names earlier. Witzel describes it as "**especially surprising**" since it is totally without parallel anywhere else in the world.

Fifthly, as I have pointed out in my book, the Harappans are supposed to have been converted and transformed not by the original Indo-Aryans who are alleged to have originally emigrated from South Russia, but by a group of people who so represented the final result of a continuous admixture of different races all the way from South Russia to India that they bore almost no ethnic affinity at all with the original Indo-Aryans, but were actually (in Witzel's words) an "**Aryanized**" section of the "**local population**" of the "**Turkmenian-Bactrian area which yielded the BMAC**", and (in the words of Hock) were therefore "**fairly similar to the population of that area**" (the Harappan area) "**in terms of their physical appearance**". And the "Aryan" ethos transmitted by these highly diluted carriers of the Indo-Aryan culture to the Harappans resulted in the Vedic civilization in the formerly Harappan areas, which produced a text (well after these Vedic people had lost all memories of this whole process of transformation, as also of any extra-territorial associations), the Rigveda, which seems to contain the seeds and essence of the reconstructed and reconstructable language and mythology of the original Proto-Indo-Europeans in the original homeland, and bears closer affinities with the language and mythology (in a more primitive form) of each of the other Indo-European branches than any of them bear to each other! Is there any comparison *at all* on this point with the Gauls or the Samoyedic groups?

To compound his errors, Fournet quotes me in full as follows: "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' mass of chaotic scenarios or postulates which explain nothing and lead nowhere: except that they are all intended to somehow prove the AIT case" (p.331), and writes: "**After having read the (section 2 of) the book, our conclusion is that this description best suits the OIT**". Fournet does not realize that just retorting "you too" or "not me, *you*", like a child involved in a juvenile quarrel, makes no sense. I have exposed all the contradictions in the AIT scenario (e.g. advocating peaceful "**trickling-in**" scenarios to answer objections about the lack of archaeological evidence, while advocating violent invasionist scenarios to answer objections about the total transformation). At the same time I have presented a single, coherent and cohesive scenario in my book where all the points fit in with each other and there is no internal contradiction whatsoever: I challenge anyone to show me any contradictions.

Apart from this, Fournet's review of this chapter contains his usual silly time pass comments:

He writes that "**the interactions between the new and the old populations are described in an "erase and rewind" mode**" in my description of the Aryan transformation of the Harappan area. What he does not realize is that, in my hypothesis, there are no "new" and "old" populations in that area. It is the AIT which holds that there were two different populations or civilizations in that area, before and after the alleged

Aryan transformation. What I am describing, or exposing, is the AIT descriptions of the case.

Further, Fournet writes: "**we do not understand this statement of the author: 'The Vedic Aryans are the People of the Book in the Rigveda'. (p.368)**" Fournet does not understand it because he has not read pp.260-264 of my book, or for that matter, most of the book as a whole. He writes: "**The book does not require any prior reading of the two other books by the same author, which were on the same topic**". But he could have correctly added: "**The writing of this review did not require any prior reading of this book under review either**".

In giving his decided views that the system of signs depicted on the Harappan seals is "**most probably not a writing system**" (the weight of logic is on the side of the view that it definitely *is* one, but there is no sense in pointless quibbling until it is finally and conclusively deciphered), Fournet produces this gem of profundity: "**The author has the prejudice that the absence of writing is tantamount to idiocy and cultural vacuum**"!

And Fournet ends his review of this chapter with his blessings: "**Well, our most sincere suggestion would be that the long defunct OIT should now be allowed to rest in peace**"! Well, our most sincere suggestion would be that Fournet step out of his cocoon and learn to face unpleasant (to him) facts: it is the AIT which is being put to rest, sooner than it would have otherwise, by inadequate protagonists like himself.

11. **Identities and prejudices**: Fournet finally moves on to the last part of my book "**Postscript: Identities Past and Present**". This postscript contains basically two parts. The first part, like the first section of the book, consists of detailed data and references from the Rigveda, here proving that the term *arya* in the Rigveda refers to the Purus, and that the terms *dasa* and *dasyu* refer respectively to non-Puru tribes and their priestly classes. Beyond noting that "**the author agrees that there were two different populations simultaneously inhabiting north-western India but he does not identify the dasa with the pre-existing non Indo-Aryan population**", Fournet does not deal with this data (any more than he deals with any of the hard data in any other part of my book). [It may be pointed out, incidentally, that I do not agree "**that there were two different populations simultaneously inhabiting north-western India**". There were *many* tribal conglomerate populations inhabiting northern India as whole, but *in the eyes of the Puru composers of the Rigveda*, the Purus were one entity, and all non-Purus were another].

The second part of this postscript deals with ancient vis-à-vis modern identities. In this part, I make it clear that my analysis of the Rigveda "refers to people who lived, and events which took place, thousands of years ago" (p.363), and that "the history of Vedic times is just that: *the history of Vedic times*. It has to do with the history of civilizations and language families, and must be recognized as such; but it does not have *anything whatsoever* to do with relations between different ethnic, linguistic, caste or communal groups of the present day. The biases and the conflicts of ancient times are the biases and

conflicts of ancient peoples *with whom present day peoples have no direct ethnic connections"*.(pp.365-6). Hence I plead that I "can only hope that nothing written in the book is used as a fodder for manipulative politics of any kind seeking to revive supposed biases, prejudices and putative identities of the past" (p.368).

As the saying goes, to a person with severe jaundice, the whole world looks yellow. Where any sane reader would have heard the voice of sanity, sobriety and harmony in this chapter, Fournet finds the whole chapter eerily dark, satanic and sinister and finds in this chapter "**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 we will not step beyond that point, all the less so as we have recently admired la Porte de l'Enfer by Auguste Rodin in the Musée d'Orsay in Paris and we have some uncanny forebodings about thresholds**"! So this postscript is the doorway to Hell! A strange kind of phrase and sentiment, this kind of medieval Christian attitude towards heathen traditions, to hear in this day and age in a so-called academic debate!

Another aspect of this second part of the postscript is that I plead for a non-partisan attitude even in our treatment of the people and events in these ancient texts, and show how I have adopted such an attitude in my own analysis. This seems to irk Fournet, who comments: "**the author's insistence on the otherness of Indo-Aryans results in a very unfavourable 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. We cannot help thinking that the Indo-Aryans, and their neighbours as well, do not deserve these characterizations**"

The particular quotation from my book which provokes this outburst is "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" ─ but this is a view with which almost every single scholar of comparative studies of Vedic and Avestan traditions would agree!

Furthermore, if Fournet finds an "**unfavourable portrayal**" of the Vedic Aryans in my analysis, his findings are not based on Hercule Fournetian deductions drawn by him from indirect references in my book, but on direct quotations of phrases used by me, and I have myself made this "unfavourable" portrayal clear in unambiguous terms (p.368-369). So, here, it seems as if *he* is ending up defending the Vedic Aryans from *my* calumnious portrayal of them! On the one hand, Fournet accuses me of glorifying the Vedic Aryans and portraying them as a unique superhuman community, "**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**" ─ *all this simply because I advocate an Indian homeland*! And when I simply portray them as normal human beings in their morals and actions, he turns round and accuses me of giving so "**unfavourable**" a portrayal of them that even *he* is compelled to rise to their defence.

Fournet's words show his total lack of understanding. I have *nowhere* included "**and their neighbours as well**" in these characterizations so far as I can see, since my book is an analysis of the Vedic Aryans, whose attitude is portrayed in their *own* books by *themselves*; and I cannot characterize *other* people on the basis of the way they are portrayed by the Vedic Aryans.

But, an examination of the Avesta will show that these are characteristics of the Avestan Iranians as well. And an examination of the characteristic texts and literature of almost every single civilized human community in history will show that, except to the eyes of the idealistic and Utopian or partisan viewer, this insularity and disdain for other peoples and areas is sadly characteristic of almost all human societies in general. As I have repeatedly written: "Rigvedic history, which forms the backdrop of the Rigveda, is like the history of any ancient civilization" (p.369).

It is only people with mind-sets like Fournet who will insist on interpreting the very idea of location of the PIE homeland in India itself as being *directly and automatically* tantamount to treating the Vedic Aryans as a unique, superhuman community. Incidentally, does Fournet also feel that all the writers and scholars who advocate different homelands for the Indo-Europeans also treat the present-day Indo-European language speaking inhabitants of the geographical area advocated by them to be "**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** […] **homeland**"? Or are contemptuous descriptions of this kind reserved only for dealing with the OIT hypothesis?

Fournet's comment, "**The reader is left to wonder what 'thoroughly South Asian' means**", is typical. Whether the reader would "**wonder**" about it or not is a doubtful question. If Fournet "**wonders**" about it, it may be because he does not understand English beyond the limited meanings of English words yielded by his favourite dictionary. More specifically, it is because he has not read p.100 of my book. If he had, he would at least have known that his mystification about this perfectly logical and simple phrase would have to be clarified by Witzel, whose use of this phrase has merely been repeated by me.

Fournet ends his review of the postscript with his reference to my "**evocation of the 'Battle of the Ten Kings' (p.370)**", and his admission of his "**having never read or heard what this epical event is**" (although it is referred to many times in the course of this book itself, as the Index could have enlightened him) already referred to earlier in this reply. Then this profound gem: "**According to the author, this may have been a kind of Big Bang of the Indo-European history. We tend to think this more the Big Crunch of the OIT**".

After a few words on my Index (already dealt with earlier on in this reply), Fournet finally ends his "review" with the biggest joke cracked by him throughout this entire

"review": he tells us that his "review" "**has not been 'done with unfriendly or hostile intent' (p.XXXII)**"!

III. Postscript: How to write a review.

As we saw, the entire "review" had nothing to do with the data, evidence and conclusions in my book, and consisted of nothing but polemics: long diatribes and confused masses of comments and opinions on a variety of topics (from AIT and OIT to Indo-Iranian to Proto-Indo-European), interspersed with jeers, nasty pieces of psycho-analyses, and semantic discussions on the meanings of specific words used by me. If large parts of the review, many of them quoted above (the prime example being Fournet's monologue on the words "development" and "to develop"), were to be described in one phrase, it would be a phrase used by Arun Shourie in one of his books: "**verbal vomit**".

A pedestrian response to my book of this kind would have been understandable from a lay person, who would have neither the interest, nor the time nor the ability to take the trouble to try to understand the details of the subject under discussion. Many lay people who liked my first book found the second book more tedious, and many who liked the second book find my third one extremely tedious and tiring, because of the masses of references, data and statistics which they naturally cannot be expected to read in detail or subject to a critical examination. But people presuming to write a review of my book and judge the contents as scholars cannot escape with this pedestrian approach. Unfortunately, that is just what Fournet does in this "review" and which other possible reviewers are going to try to do in respect of my book, when they do not have the guts to address the real issues in my book. It is a technique which was demonstrated by Witzel, in his review of my second book, and in the enthusiastic reactions to it from his comrades in arms and many lay (but partisan) readers, as a tactically effective one in derailing serious discussion.. Any objection to the pedestrian and irrelevant nature of the criticisms is dismissed as a failure to understand how clinching the criticism is in debunking my arguments and evidence!

I had to descend to Fournet's low level in order to reply to his "review" of my book. But, henceforward, I will not take the trouble of giving detailed replies to any so called review of my book which concentrates on polemics or on *anything* other than factual criticism.

While I can have nothing to learn from jokers like Fournet on "**how to write a book**", Fournet certainly can learn from me "how to write a review" or "how to review a book" on the basis of factual criticism rather than polemics. Fournet's whole attitude throughout his review was that of a monkey who has been given a stick in his hand (*bandar ke haath mein laathi*, as the Hindi version of an all-India saying goes): he decided that Koenraad Elst, by asking him if he could review my book, had given him a licence to give me the thrashing of my life, and like Witzel, who has a special word for this kind of Indian-bashing, "**fun**", Fournet decided to have "**fun**" as never before. "**Fun**" it may have been, but a "review" it was not.

As illustration of what a genuine critique should be like, he can examine my own criticism of Witzel in my book under review. I have also criticized Witzel (whom Fournet calls my "**personal favorite duelist**" and my "**bete noire**") sometimes in sharp words, but my criticism is everywhere based on *factual* criticism of what he has *actually written*, rather than, like Fournet, on polemical monologues about my *personal opinions* of what he represents, what he does not know, and what he has not mentioned but should have (and in fact, on p.xxx and xxxii of my preface, I have even given him credit for one important instance of factual criticism by him of my second book). My criticisms are unanswerable, and have therefore remained unanswered and steadfastly ignored by him:

On p.50, I point out how Witzel takes up VII.33.3 in the Rigveda, which (as confirmed by VII.18.9) is a reference to a battle on the Yamuna, and treats it first as a battle on the Indus, then into a reference to a west-to-east movement across the Indus by Vasishtha and the Bharatas, and finally into a testimony of Vasishtha and the Bharatas being "**self-proclaimed immigrants**" from Iran. Can Witzel show *one* reference or *one* item of acceptable evidence (and tell us how many western Vedic scholars will accept it as valid) to show that this verse refers to the Indus and not to the Yamuna, and that Vasishtha and the Bharatas, as per this or any other reference from the Rigveda or any other Vedic or even Puranic text, are "**self-proclaimed immigrants**" from Iran?

On pp.50-51, I point out how Witzel again claims that since absolutives (gerunds) are not found in Iranian, and since the Vasishthas don't use absolutives in their compositions, this is evidence of their being from Iran. Witzel does not provide a complete family-wise or book-wise list of absolutives in the Rigveda to prove his point. But I do, and show that his claim is blatantly false. Can Witzel provide a complete list of absolutives from the Rigveda to show that he is right and that I am wrong?

In the same context I point out, on pp.51-52, that while the non-family books (1, 8-10) as well as the book of Atri (book 5) are *overflowing* with personal name types in common with the Avesta, the book of Vasishtha (book 7) is not only completely lacking in such names, but, in fact, the only Iranian names in the whole of book 7 are the names of the persons and tribes arraigned as the *enemies* of Vasishtha and the Bharatas! Can Witzel explain this in the context of his claim that Vasishtha and the Bharatas are from Iran?

On pp.52-53, I point out that Witzel, in two different pages of one and the same paper written by him in 1995 (and later reiterated in all subsequent papers), claims on the one hand that Vishwamitra was the leader of the coalition which fought Sudas, the Bharatas and Vasishtha in the Battle of the ten kings and was completely defeated and humiliated by them, and on the other that hymn III.53 was composed by the Vishwamitras to commemorate and glorify the victory of the Sudas in this battle. Can Witzel clarify this point?

On pp.108-110, I point out how Witzel claims that the Ganga and Yamuna are "**already mentioned**" in the Rigveda, as if they are new sights on the Rigvedic horizon, and that to book 10 (which even Witzel accepts as the last and latest book of the Rigveda) "**most of Afghanistan** […] **is already out of sight**", as if the Vedic Aryans, coming from the west,

have moved away from it eastwards by the time of composition of book 10. And I point out that, on the contrary, Afghanistan (and even the Indus) is totally unknown to the three oldest books of the Rigveda (books 6, 3 and 7) and even to a large extent to the other family books (4, 2 and 5, of which 4 and 5 alone mention some rivers, but none of the lakes, mountains, place-names and animals of Afghanistan), which refer to all the rivers, place-names, lakes and animals of the east. And far from Afghanistan being "**already out of sight**" in the last and latest book 10, this book not only contains references to the lakes, mountains, place-names and animals of Afghanistan (totally unknown to the family books), but also to every single river of Afghanistan named in books 4 and 5, and even some more totally unknown to the family books. Can Witzel show, from the references in the Rigveda, that he is right and I am wrong?

On pp.115-122, I show how Witzel, in his papers on Rigvedic history written in 1995, categorically identifies the Rigvedic Sarasvati with the Ghaggar-Hakra of Kurukshetra, categorically and repeatedly locates *every single reference* to Sarasvati in books 6, 3 and 7 with the Kurukshetra region, and categorically treats the reference to the Sarasvati which flows from the mountains to the sea in VII.95.2 as a reference to the river of Kurukshetra (and even dates the verse to *before* 1500 B.C. on the ground that the river of Kurukshetra had dried up by that date). But after reading my book, Witzel suddenly discovers that the Sarasvati of books 6, 3 and 7 is either the river of Afghanistan, or the name of a woman or a deity, or a reference to the "**night-time sky**", and that the reference in VII.95.2 is to the river of Afghanistan (with the sea in the verse turning out to be the Hamun-i Hilmand). Can Witzel justify this opportunistic volte-face?

Likewise, on pp.125-128, I show how Witzel, in *all* his papers written from 1995 to 2000, categorically treats the reference to the Ganga in VI.45.31 as belonging to the "**early Rgvedic period**" and as a reference found in one of the "**oldest hymns**" in one of "**the oldest books**", categorically older than at least books 1-3 and 7-10 of the Rigveda, and as evidence that "**early Aryan settlement**" extended "**upto Yamuna/Ganga**", and he even takes up issue with other western scholars who feel otherwise. But after reading my book, Witzel suddenly discovers that this reference "**occurs in a trca that could be an even later addition to this additional hymn**", and finds that he has to "**immediately throw out the reference to the Ganges that appears at RV 6.45.31**". Can Witzel justify this second opportunistic volte-face?

On p. 170-173, I show how Witzel arbitrarily decides that the Mitanni language had no retroflexes, and, therefore, that this is evidence against their migration from India. I have shown how the data available is *totally insufficient* to give any logical ground whatsoever for deciding that the Mitanni IA language had no retroflexes (apart from the fact that even proven absence of retroflexion in Mitanni IA would have been no evidence that they had not migrated from India). Can Witzel produce convincing logical evidence to prove his two points?

On p.191, I show how Witzel, on two different pages of the same paper, treats the word *armaka* in the Rigveda as the "**ruins**" of the (according to him non-Aryan) Indus

civilization, citing Falk 1981, and as "**shallow remnants of the IA settlements**", citing Rau, 1983. Can Witzel clarify his position on this?

On pp.192-197, I show how Witzel treats a group of words, which he categorizes as BMAC words (words allegedly borrowed by the Indo-Iranians from the alleged language of the BMAC of Central Asia) as representing a *pre*-Rigvedic heritage. However all these words are missing in the *earlier* group of books (2-4, 6-7) of the Rigveda, and are found only in the *later* group of books (1, 5, 8-10) ─ *earlier and later as per a chronological consensus among western scholars* ─ and in all post-Rigvedic texts, and in the Avesta. Can Witzel logically explain this contradiction?

On pp.295-297, I have shown how Witzel tries verbal jugglery and trickery in order to argue against the migration of the Indo-European dialects from India: he tells us that the ancestral forms of the western Indo-European dialects could not have migrated from India since if they had they "**would have taken with them a host of 'Indian' words ─ as the gypsies (Roma, Sinti) indeed have done** […] **the Gypsies, after all, have kept a large IA vocabulary alive, over the past 1000 years or so, during their wanderings all over the Near East, North Africa and Europe**". He thus gives us the impression that his criterion is a group of "**Indian**" words which have been preserved by the Gypsies but not by the western Indo-European languages. Such a criterion would itself have been an unfair and illogical one, since the early development of the *non-Indo-Aryan* Indo-European dialects took place *in the northwest* (*beyond Pakistan*) ─ see maps on pp.226-234 of my book ─ and they emigrated from there in an *early prehistoric period*, while the Gypsies emigrated as speakers of an *Indo-Aryan* dialect *from the interior* of India *just over a 1000* years ago; so the Gypsies could well have retained typically "Indian" words not found in the western Indo-European languages. But what makes Witzel's analogy really fraudulent is the fact that he gives two *different* groups of words to show that the Gypsies preserved "Indian" words with them while the Indo-European dialects did not: he tells us that the Indo-Europeans did not preserve "**Indian words such as those for lion, tiger, elephant, leopard, lotus, bamboo, or some local Indian trees**" ─ *but then, neither did the Gypsies*! And he tells us that the Gypsies have preserved words such as "**e.g. *phral* 'brother', *pani* 'water', *karal* 'he does'**" ─ *but then, so have the western Indo-European dialects preserved words for "brother". "water" and "he does"*! Can Witzel explain this trickery? [Incidentally, the western Indo-European dialects in fact actually *have* preserved words for elephant (Vedic *ibha*, Greek *el-ephas*, Latin *ebur*), monkey (Vedic *kapi*, English *ape*, Irish *apa*) and leopard (Vedic *prdaku*, Greek *pardos*, Hittite *parsana*)!]

All these are concrete *factual criticisms* based on *hard, concrete data*, of things *actually written* by Witzel, and the correctness or otherwise of each of these pieces of criticism can be verified by examining this hard, concrete data. See also my criticism of Witzel on pp.112-113, 189-190, 255, 263, 290-307, and elsewhere. Fournet could have taken a leaf out of my book, and presented a logical critique based on factual criticisms of the hard facts, data and evidence presented by me, rather than presenting a mass of *verbal vomit* consisting basically of vindictive polemical monologues.

But polemicists like Witzel and Fournet are incapable of honest and decent debate. They do not have the guts to present an honest and factual critique of the data and conclusions presented in other people's writings any more than they have the guts to reply to honest and factual critiques of the data and conclusions presented in their own writings. If they choose to merely comment on this reply, expect name-calling or haughty dismissals. If they choose to reply at some length, expect more verbal vomit, and plenty of dot-bashing. But do not expect honest and serious debate and discussion. ["Dot-bashing", from the earlier "dot-busting", is increasingly replacing anti-Semitism and Ku-Klux-ism as the favourite form of ethnic bashing in certain western academic circles since it is also the safest: firstly it is still "politically correct", and secondly even many co-opted and willing-to-be-co-opted Indians can be induced to join in the "**fun**" (as pointed out earlier, Witzel's favourite word for it) or will even do so willingly]

In the end, to genuinely unprejudiced and impartial readers of this reply, I can only repeat what I wrote earlier on in this review: please carefully read both my book (although I admit it is rather dry and technical) as well as Fournet's "review" and decide for yourself:

a) what exactly the "**real issues contained in the review**" are, and whether they really required to be addressed at all; and also whether or not Fournet himself has in fact addressed the *very real issues* in *my* book in his "review", and

b) whether it is I who do not understand "**how to write a book**" (and have to learn "**how to write a book**" from this joker), or whether it is Fournet who does not understand how to read a book, or how to understand what he is reading even when it is set out in plain English.