

## *A new date for the R̥gveda*

The *R̥gveda* (RV hereafter) is the most important document not only for Indology but most IndoEuropean (IE hereafter) studies in philology, religion or mythology, history etc. The date of its composition is since the mid-19th century given as c 1200-1000 (this and subsequent ancient dates are BC). This date is closely linked with the hypothetical Aryan invasion into NW India c 1500 (Burrow 1973: 1-34 & 1975: 20-9). However, the numerous new data brought to light by archaeological investigations in Pakistan and NW India from the 1960's onwards make it imperative that the RV date should be reappraised and pushed back to c 3100<sup>1</sup> while the old "Aryan invasion" theory should be expunged from textbooks altogether. The archaeological record furnishes no evidence whatever of any invasion or entry of IE or other peoples in the region before the 1st millennium. This of course coincides with the Indian literary tradition. Furthermore, studies from other disciplines suggest that elements in some Sūtra literature must have been present c 2500-2000 or the Mature Harappan period, as will be indicated below (section 3, end).

The "myth" of the Aryan invasion – as American anthropologist J Shaffer called it (1984: 77 ff) – is a strange theory invented by European philologists in the 19th century<sup>2</sup> in disregard of the native tradition and when Archaeology was in its gestation period. The theory received a strong boost by the excavation of the Indus Valley or Harappan civilization between the two wars and particularly by Wheeler's pronouncement that the destruction of these cities was the work of the invading Aryans – for which "Indra stands accused" (quoted in RAL, p 188). Briefly, the theory states that c 1500 the fair-skinned Aryans rode on horses and chariots down into the Saptasindhu, that is the region of the Seven Rivers (ie modern North Pakistan and NW India), destroyed the local civilization, slaughtered, enslaved or drove into the South, the dark-skinned natives and became masters of the country continuing their pastoral nomadic life. At c 1200-800 they composed the RV and the other three Vedas, then the Brāhmaṇa texts on rituals, and so on. The theory was modified in the 1960's after archaeologist G Dales's publications (1965, 1966) showed that Wheeler had misread the evidence and the Harappan cities had not been destroyed by Aryan or other invaders but had been abandoned by their inhabitants c 1800-1600 because of ecological, climatic and geophysical changes that compelled them to move eastward to the Gangetic area. The old theory persisted into the 1970's and 80's but eventually the "invasion" became "immigration", "arrival" and the like (Aklujkar 1996: 64; Witzel 1995: 323).

The "invasion" theory and the RV composition date had become something of a doctrine not to be questioned by philologists and scholars in related fields – with the notable exception of archaeologist Colin Renfrew, who questioned both matters in his *Archaeology and Language* (1987: ch 8). The position is explicitly stated by an eminent linguist: "At some time in the second millennium BC ... a band or bands of speakers of an Indo-European language, later to be called Sanskrit, entered India over the north west passes. This is our *linguistic doctrine* which has been held now for more than a century and a half. There seems to be no reason to distrust *the arguments* for it, in spite of the traditional Hindu ignorance of any such invasion." (M B Emeneau 1954: emphasis added.) Here the linguist, who accepts the RV date as 1200-1000, makes a judgment about a historical occurrence admitting it to be a received *doctrine* and using the word "arguments" in its support, not, as one would expect in a case of historical events, the terms "evidence" or "data". *It was a*

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<sup>1</sup> I am aware of other early dates, but it will be quite obvious that I follow an entirely different approach.

<sup>2</sup> See Shaffer J (1984) for a summary of different views and of recent archaeological evidence.

*doctrine supported by arguments alone and no actual evidence* .<sup>3</sup>

The literature-aspect of the doctrine originates in Max Müller's *History of Sanskrit Literature* (1859), as somewhat modified in his 1862 edition of the *Rgveda* (4th Volume). He argued that, since Buddhism (6th cent BC) presupposed the Vedic Corpus (Vedas, Brāhmaṇas, Āraṇyakas and Upanishads), the Sūtras, which linguistically seem to be subsequent, were composed from the 6th century and later. He then allowed 200 years for the composition of the Brāhmaṇas and Upanishads (ie 800-600), then another 200 years for the Sāma-, Yajur- and Atharva-Veda (ie 1000-800) and 200 years more for the RV (ie 1200-1000). All this is, of course, sheer conjecture without the slightest evidence of any kind to support the date of the Sūtra compositions or the 200-year periods for the other divisions. Here, parenthetically, should be noted some relevant facts that one rarely meets in studies subsequent to Max Müller and hardly ever in modern publications. After receiving criticism, that great pioneer of sanskritic studies explained that the chronology he had given was hypothetical – only as a *terminus ad quem* and a basis for discussion – and wrote, “No power on earth will be able to determine whether the Vedic Hymns were composed in 1000 or 1500 or 2000 or 3000 before Christ” (*Physical Religion*, 1892, repr 1901, p 91). And in the very last years of his life he wrote: “If we grant that they [ie the Vedas] belonged to the second millennium before our era, we are probably on safe ground, though we should not forget that this is a constructive date only, and that such a date does not become positive by mere repetition”. Then he added, “Whatever may be the date of the Vedic hymns, whether 1500 or 15000 BC, they have their own unique place ... in the literature of the world”. (*The Six Systems of Indian Philosophy*, 1898, 1903, Longmans 1916, pp 34-5.) His chronologies fell into *mere repetition* and became a doctrine.

Even before and some years after Max Müller's passing, several scholars argued for a date of the RV much earlier than 1500. Notable among them are H Jacobi and B G Tilak who, working quite independently at about the same period, and examining the astronomical data in the Vedic Corpus, arrived at very early dates. Jacobi (1894: *Indian Antiquary* 23) gave a date c 4500. Tilak (1893: *Orion: Researches into the Antiquity of the Vedas*) traced some Vedic texts as far back as 6000. In his monumental *History of Indian Literature* (revised ed 1927 *Geschichte der Indischen Literatur*), Winternitz summed up the available evidence and concluded for a date c 2500-2000. Nonetheless, Max Müller's conjectural chronologies prevailed in academic thinking.

The eminent philologist T Burrow, whose descriptive study of Sanskrit is still the standard work, restated the doctrine conflating the divisions of the Vedas into one period (1973: 1-34, 43). He attempted to justify it with some philological considerations, not free of

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<sup>3</sup> To avoid possible misunderstandings I should state that I am a sanskritist, not an archaeologist. For further complaints against linguists, see Renfrew (1997: 88) who, for his own theory of agricultural diffusion (see n 7 below and text), would like earlier dates for the IE language dispersion.

inconsistencies<sup>4</sup>, but admitted early on that the RV composition date is arrived at “by rough guesswork” (p 9) – which is a most curious admission regarding so important a document. On these philological considerations Burrow finds that the ProtoIndoEuropeans (PIE hereafter) inhabited central Europe before their dispersal and that the IndoIranians migrated eastward c 2000 (p 9) and stayed somewhere in central Asia before splitting up, whereupon the IndoAryans invaded India, while a subsidiary wave moved into the Near East (the Kassites and the Mitannis), and the Iranians afterwards moved into Iran.

These philological considerations by themselves are not reliable. The same philological data have been examined and interpreted differently by different scholars who reach thereby different conclusions. Thus the Russian philologists Gamkrelidze and Ivanov (1985, 1990, 1995), mostly on linguistic considerations, posit as the PIE homeland the region just south of the Caucasus and the date of the dispersal or migrations in the 3rd millennium. S S Misra, an Indian philologist, again, from the same data derives dates ranging in the 5th and 6th millennia and posits N W India as the *urheimat* (1992, *passim*). Mallory (1997: 98) examines summarily some of these philologists’ conflicting “estimates” and cries out “Will the ‘real’ linguist please stand up?” The noted sanskritist Aklujkar for his part does not consider the established dates as incontestable and states that “ony *relative* chronology has been well argued for” (1996: 66, n 14b).

Shaffer sums up this matter writing: “The Indo-Aryan invasion(s) as an academic concept in 18th- and 19th-century Europe reflected the cultural milieu of that period. Linguistic data were used to validate the concept that in turn was used to interpret archaeological and anthropological data. What was theory became unquestioned fact that was used to interpret and organize all subsequent data. It is time to end the “linguistic tyranny” that has prescribed interpretative frameworks of pre- and protohistoric cultural development in South Asia.” (1984: 88.) Shaffer’s main concern is (pre-)history and the establishment of the true situation by the removal of the “invasion” theory: he accepts the RV date as contemporaneous with the late Harappan period (c 1900).

Genetics has also been adduced to demonstrate the migrations of peoples or types of men in different areas. Renfrew points out that there “are difficulties of methodology not yet resolved” (1997: 89). In fact, two independent investigations showed that previous finds (through research by molecular genetics) are not correct in placing in Africa c 200.000 years ago the common ancestry of the human mitochondrial DNA (*Science* 1992: 737-9). Such studies, moreover, cannot say anything about language nor the actual manner in which genes of a particular type first appear in any area – whether, in other words, we have large or small groups of immigrants, a band of merchants, troops of conquerors or captives, or whatever else. All these difficulties are evident in the recent study by Cavalli-Sforza, Menozzi and Piazza (1996: xi-xiii, 5, 29, 32, etc), who, unaware of the *Science* 1992 reports, accept the finds about the ancestry of human mtDNA in Africa (88). Furthermore, the genetic material and associations of different groups in the Indian Subcontinent present “unexplained”

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<sup>4</sup> See below, n 6 and text. However, Burrow makes many interesting points; one of them is his warning, often unheeded, about reconstructing ProtoIndoEuropean (1973; 11): “... in the case of Indo-European it is certain that there was no such unitary language which can be reached by means of comparison. It would be easy to produce, more or less *ad infinitum* [,] a list of forms like Skt *nābhi-*, Gk ὀμφαλός ‘navel’, which although inherited directly from the primitive IE period, and radically related [,] are irreducible to a single original. In fact detailed comparison makes it clear that the Indo-European that we can reach by this means was already deeply split up into a series of varying dialects.”

difficulties (239-41); the authors state that the Harappan skeletons show “a substantial similarity with modern populations from a nearby area”, yet advocate a post-Harappan (ie 1700?) “Aryan invasion” (210); moreover, their “synthetic map of the third genetic PC [=Principal Component]” supports both the South Russian steppes and Anatolia as the original IE homeland (299). We shall therefore ignore such evidence.

Let us now turn to the archaeological data and see where they, in conjunction with other relevant evidence, lead us.

### 1. *The Sarasvatī river.*

This is probably the most startling piece of evidence.

Many hymns in all ten Books of the RV (except the 4th) extoll or mention a divine and very large river (*nadītamā*), named Sarasvatī<sup>5</sup>, which flows mightily “from the mountains to the [Indian] Ocean” (*girībhya ā samudrāt*: VII, 95, 2). This river gave sustenance to many kings and the five Ārya tribes that were settled along its banks (VI, 61; etc) and along the Indus, Dṛṣadvatī and other rivers. In historic times the river appeared to be a minor stream, Sarsūti (<*Sarasvatī*) or Ghaggar, which ended in the desert at Bhatnair, hundreds of miles away from the Ocean, so that modern scholars (Roth, Griffiths, et al, *Vedic Index* II, 434) thought that the RV poets referred, in fact, to the Indus which alone is large enough and justifies such references.

In the 1970’s and 1980’s, however, archaeological researches discovered the old route of the Sarasvatī from the Himalayas to the Indian Ocean and its various diversions due to tectonic and climatic changes and unearthed along this route hundreds of small and large settlements, including some sizeable towns like Kalibangan: in fact these constitute two thirds of the total Harappan settlements of 2500 while those along the Indus are only about 100 (Misra V N, 1992). According to palaeoenvironmental scientists the desiccation of Sarasvatī came about as a result of the diversion of at least two rivers that fed it, the Satluj and the Yamuna. “The chain of tectonic events ... diverted the Satluj westward [into the Indus] and the Palaeo Yamunā eastward [into the Gangā] ... This explains the ‘death’ of such a mighty river [ie Sarasvatī] ... because its main feeders, the Satluj and Palaeo Yamunā were weaned away from it by the Indus and the Gangā respectively” (Rao 1991: 77-9; also Feuerstein et al 1995: 87-90). This ended at c 1750, but it started much earlier, perhaps with the upheavals and the large flood of 1900, or more probably 2100 (Elst 1993: 70; Allchins 1997: 117). In preHarappan times, of course, the settlements on the Sarasvatī were far fewer. Future excavations may well reveal many more settlements.

The RV knows two very large rivers, the Sarasvatī and the Indus. At 1200 there was only the Indus, as in modern times. Clearly, then, when the RV describes the Sarasvatī flowing down to the ocean, it is referring to the river as it was long long before 1750. How did the poets know of this *nadītamā* Sarasvatī?

This suggests a date well before 2000. However, P H Francfort, utilizing images from the French satellite SPOT, finds (1992) that the large river Sarasvatī is pre-Harappan altogether and started drying up in the middle of the 4th millennium; during Harappan times only a complex irrigation-canal network was being used in the southern region. The Allchins (1997) seem to be unaware of Francfort’s research: they cite L Flam in J F Shroder’s *Himalaya to the sea, Geology...*, 1993, which must have been *written* before Francfort’s publication. With this the date should be pushed back to c 3800.

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<sup>5</sup> RV II, 41, 16: *āmbītamā* ‘most motherly’; *nādītamā* ‘greatest of rivers’; *dēvitamā* ‘most divine’; or, best mother, best river, best goddess.

The RV hymn X, 75, however, gives a list of names of rivers where Sarasvati is merely mentioned (verse 5) while Sindhu receives all the praise (verses 2-4 and 7-9). This may well indicate a period after the first drying up of Sarasvati (c 3500) when the river lost its pre-eminence. It is agreed that the tenth Book of the RV is later than the others.

## 2. Language and Religion.

Linguistic considerations by themselves are inconclusive regarding absolute dates but do show, contrary to current mainstream opinion, that Vedic is older than other IE languages. Some indications are given in different places in the discussion that follows. But apart from such indications we draw on other kinds of evidence.

A consideration of the different locations postulated as the PIE urheimats by various scholars will also help towards establishing a date for the RV. We have three main proposals, though several more have been suggested (Mallory 1989: 143-4). The three are: central Europe (Burrow et al); Anatolia and Caucasus (Renfrew; Gamkrelidze & Ivanov); the south Russian steppe (M Gimbutas, et al). The cultural evidence from the language and religion (=mythology) of the IE branches is against all three proposed urheimats and indicates that the RV is older than the most ancient written texts, which are those of the Hittites, thus giving a date of, again, well before 2000.

It is a generally acknowledged principle of Historical Linguistics that “changes [of language] are quicker in unsettled communities than in more settled ones” (Lockwood 1969: 43; cf also Hock 1991: 467-9). Since according to the “Indo-Aryan immigration” theory the IndoAryans were on the move over many thousands of miles (from the Russian steppe, Europe and/or Anatolia) over a very long period of centuries encountering many different other cultures, they were “unsettled” and their language should have suffered faster and greater changes. Indeed, this is what happened to the Tocharians whose language, according to Burrow, underwent “profound ... changes strongly suggestive of alien influence” since it had “travelled far from its original home” (1973: 10)<sup>6</sup>. But the facts do not bear this out in respect of the IndoAryans: as Burrow says, “Vedic is a language which in *most respects* is more archaic and *less altered* from original Indo-European than any other member of the family” (34: emphasis added); he also states that root nouns, “very much in decline in the earliest recorded Indo-European languages”, are preserved better in Sanskrit, and later adds, “Chiefly owing to its antiquity the Sanskrit language is more readily analysable, and its roots more easily separable from accretionary elements than ... any other IE language” (123, 289). This being the case, Vedic is much older than Hittite, which is attested in writing in the 17th century (Gurney 1990: 17), thus giving us a lower limit of c 1800.

Before proceeding let us examine one example that bears out Burrow’s claims for Sanskrit (or Vedic). This is the word “son” and its cognates in other IE languages and the word “sow” (she-swine). Apart from Skt *sūnu*, it appears in all Germanic branches with the stem *sun-* (also O Norse *son-r*), Gk *huio-* (and dialectal variants), Avestan *hunu*, Slavic *synŭ* (and variants), Toch A *se* and Toch B *soy*, etc. It does not appear in Latin, in the Celtic branches or in Hittite. The stems for “sow”, which are generally accepted on philological grounds as cognates of the forms of “son”, also have widespread incidence; Gmc *sū-(gu)*, Gk *sus/hus*, L *sūs*, Av *hū-* etc. Curiously, in no language do we find other cognates, nouns or verbs, nor an explanation of the relation between “son” and “sow”: apart from late developments the two words hang isolated. Sanskrit provides both a plausible explanation

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<sup>6</sup> One of Burrow’s inconsistencies is that he does not apply this principle uniformly and ignores all other cases. All IE branches suffered changes and losses far greater than Sanskrit.

and several cognates. Skt *sūnu* “son” is a derivative from root  $\sqrt{sū} > sūte$  “beget”: this is quite a regular formation, as with  $\sqrt{grdh} > grdhnu$  “eager, greedy” or  $\sqrt{bhā} > bhānu$  “shining-one, sun”, etc. The root *sū* gives in Skt not only a full declension for the verb “beget” (*sūte*, *sauti*, etc, pr; *asāvīt* etc, aor; *suṣāva* etc, prf; *pra-sū* etc) but also a host of nominal forms: *sū*, *sūti* (fem “birth, production”), *sūtu* (fem “pregnancy”), *sava* (m “instigation”), *savitṛ* (m “impeller”) *savitṛī* (fem “mother”), *sāvaka* “generative”, etc. In Skt ‘hog’ or ‘swine’ is *sū-kara*, which some interpret as “making the sound ‘sū’ ” while others connect it with  $\sqrt{sū}$  “begetting” – and the latter sounds the more probable since hogs/swine make grunts not hisses. Thus, when the nominal *sū-* “begetting, begetter” is taken also into account, Sanskrit furnishes a plausible link between “son” and “sow”. There are many other similar examples, like skt *duhitṛ* “daughter” or *muṣ* “mouse” which stand isolated in IE languages but are seen to have both verbal and nominal cognates in Sanskrit. This phenomenon, that may be termed inner cohesion of a language and is usually ignored by comparative philologists, shows indeed that Vedic is much closer to the PIE language and older than its relatives.

It is a well known fact of History that people first lose their religion and then perhaps their language: eg Greeks, Romans, Egyptians, Iranians et al, lost their old religion but retained to different degrees their old language. Religious elements and terms change more easily. Now when we look at the Table of Deities, we see that the IndoAryans have retained the names of more deities than any other branch. In fact, no major motif of the old religion is found in two or more IE branches that is not preserved in the RV also. (The golden apples of the Hesperides in Greek Mythology may have been borrowed by the Teutons and turned into the apples of immortality kept by goddess Idunn; or they both may be independent innovations, or the only exception. Greek *Hestia* and Roman *Vesta* could also be added, since they have no cognate deity in Vedic – but only the root  $\sqrt{vas}$  .)

According to the above principle of change in language and culture, the people of the urheimat should have retained many more elements of the inherited religion than the other branches which moved away. None of them had writing in the relevant period from c 3500 to 2000: despite the Harappan script on seals which may or may not be IE (sanskritic), it is well attested that *until the 7th cent AD* at least, the Vedas were transmitted orally (Winternitz: I, 31). Writing in India, apart from the Harappan script, is clearly suggested only in the Sūtra texts and Buddhist scriptures (Basham 1961: 33, 43, 394) and emerges fully into the open in the Prakrit inscriptions of the 3rd cent BC. Thus, although there is no evidence of the Vedas being written down, some written copies should not be ruled out. Caesar informs us that the Celts also preferred the oral teaching tradition although the druids “made use of Greek letters” (*De Bello Gallico*, VI, 14). On the other hand, in the Near East Hittite writing is abundantly attested from c 1620 onward; yet, as the Table of Deities shows, it preserved far fewer IE names than either Vedic or Celtic and only as many as Slavonic or Baltic, which have a late CE attestation in writing. The factor of writing, therefore, can be ignored. What do we find when we look at this aspect with the aid of the Table of Deities? ...

*Table of Deities*

<i>Vedic</i>	<i>Other IE branches</i>
<i>Agni</i> :	Slavic <i>Ogun</i> . L <i>ignis</i> , Lth <i>ugnis</i> , Ltt <i>uguns</i> . (Note: even the Iranians who had Fire-worship did not preserve this name, not even as a demon like Indra, Sauru etc; it is found only in proper name <i>dāštāgni</i> .)
<i>Aryaman</i> :	Mycen <i>Are-mene</i> and Greek <i>Ar-ē-s</i> ; Celtic <i>Ariomanus</i> (Gaul)/ <i>Eremon</i> (Ireland); Scandinavian <i>Irmin</i> . The <i>ar</i> -stem in most IE languages.
<i>Bhaga</i> :	Kassite <i>Bugas</i> ; Slavic <i>Bogu</i> ; Phrygian <i>Bagaios</i> (Zeus, Gk).
<i>Dyaus</i> :	Hittite <sup>D</sup> <i>Siu-s</i> ; Gk <i>Zeus/Dia-</i> ; Roman <i>Ju[s]piter</i> ; Germanic <i>Tîwaz</i> ; Lth <i>dievas</i> (usually ‘god’ cognate with S <i>deva</i> , √ <i>d̥iv</i> ).
<i>Indra</i> :	Ht <i>Inar(a)</i> ; Mitanni <i>Indara</i> ; Kassite <i>Indaš</i> ; Celtic <i>Andrasta</i> . Gk <i>anēr/andr-</i> , Av <i>indra</i> (a demon).
<i>Marut-as</i>	Kassite <i>Maruttaš</i> ; Roman <i>Mars</i> ; Irish <i>Morrighan</i> . The stem <i>mar/mor/mer-</i> etc is common in most IE branches.
<i>Apām Napāt</i> :	Roman <i>Neptunus</i> ; Celtic <i>Nech-tan</i> (Irish). Gk <i>nep/a-nep-sios</i> ; OHG <i>nevo</i> , OE <i>nefa</i> , OLth <i>nep-</i> , etc.
<i>Parjanya</i> :	Slavic <i>Perenu/Pyerun</i> ; Baltic <i>Perkunas</i> (and variants); Scand <i>Fjörgyn</i> (-n, Thor’s mother).
<i>Ṛbhu</i>	Gk <i>Orpheus</i> ; Gmc <i>Elf</i> (and variants). Gth <i>arb-aips</i> (?); OSl <i>rabu</i> , Rs <i>rabota</i> ; L <i>orbu</i> (S <i>arbha</i> , Gk <i>orphanos</i> ); etc.
<i>Sūrya</i> :	Kassite <i>Šurias̄</i> ; Gk <i>Hēlios</i> ; Roman <i>Sōl</i> . Gth <i>savil</i> , ON <i>sol</i> , W <i>haul</i> , OSl <i>slunice</i> , Rs <i>solnce</i> , Baltic <i>Saule</i> .
<i>Uṣas</i> :	Gk <i>Ēōs</i> ; Roman <i>Aurora</i> ; Gmc <i>Ēostre</i> . Lth <i>aušra</i> , Ltt <i>ausma</i> , W <i>gwawr</i> , etc.
<i>Varuṇa</i> :	Ht <i>Wurun</i> (?); Mitanni <i>Uruwna</i> ; Gk <i>Ouranos</i> ; Baltic <i>Vēlinas</i> (and cf <i>jur</i> =sea). L <i>ūrīna</i> , ON <i>ver</i> (=sea).
<i>Vāstoṣ-pati</i> :	Gk <i>Hestia</i> ; Roman <i>Vesta</i> . Gth <i>wisan</i> ‘to stay’; OHG <i>wist</i> ‘inhabiting’; Toch A/B <i>wašt/ost</i> ‘house’.
<i>Yama</i> :	Scand <i>Ymir</i> . L <i>gemi-nus</i> , Gk <i>zēmia</i> (=damage), Av <i>yam</i> , <i>Yima</i> .

Here the upper line shows the incidence of the deities and the lower shows cognate stems that occur in branches where the deities are not preserved. It is obvious that in many cases the PIE stem remains in the language but not the cognate name of the deity – as with *ar-*, *mar-*, etc. A few more names could have been added (eg S *Aśvin* , Clt *Epona* ) but the

distribution pattern would not change. Some names like Gk *Ouranos* may be disputed and might be omitted, but, again, the pattern would not be seriously disturbed. The superiority of the Vedic data is all too apparent. (Avestan deities have not been included in every case because Avestan is very close to Vedic.)

First the Balto-Germanic region. The Germanic and Baltic branches have not, even if put together, preserved as many mythological elements as the RV. The Teutonic subbranches together preserve few of the original motifs and in the cases of *Tîwaz*, *Irmin* and *Fjorgyn*, only the bare name; the various languages can scarcely be said to be archaic with the enormous phonetic changes, the absorption of non-IE vocabulary, the near loss of the dual (preserved in Gothic) etc. The Lithuanian language is more conservative (retaining the dual to this day) but, even so, it lost the ablative and the neuter gender, it reshaped its verbal system and “regularized the forms of comparison” of adjectives (Lockwood 1972: 130-1). The Balts retain even fewer mythological motifs. The Celts, the Romans and the Greeks, may be excused since they travelled further before settling down, but the Teutons, so conveniently ensconced near the *urheimat*, have no such excuse.

Taking the Russian homeland, we see that the Slavs fare even worse. They preserve *Ogun*, *Bogu*, *Perenu* and *Svarog*, but little else. The Slavic languages do not have even the stem *aśva/equus* ‘horse’ which is so widely distributed in the other IE languages: this is a remarkable absence when one considers the important role of the horse in that early period and its early domestication (Piggott 1992: 43) in the southern steppes, the Slavonic word being *kunĵi* and variants. Equally remarkable is the absence of the IE stem for ‘bear’ *rkṣa/arkto/ursus*, while the Sl is *medved* ‘honey-stealer’. Yet this location carries the greatest favour among scholars, who remain undisturbed by this strange hiatus. Gimbutas, on the contrary, makes this curious statement: “The formation of Proto-Germanic, Proto-Baltic and Mycenaean Greek cultures largely depended on the influence of the substratum culture: in some areas it was considerable (Greece), in others not at all (Byelorussia, central Russia). The development of their languages also depended on the same conditions” (1985: 200). Yet here in Russia the mythological and linguistic facts belie sharply the archaeological data Gimbutas uses to support her theory that this location is the *urheimat* (1970: 170). Slavonic Mythology has preserved very few of the names of deities of the inherited stock, as shown above. Then, apart from the horse *kunĵi* and the bear *medved*, can be cited ‘dog’ *pis-*, ‘goat’ *koza*, ‘town’ *grad-*, ‘copper’ *med-* etc etc, that stand alone in Slavonic against the common stems *śvan/can-/kuon-*, *aja/aig/ožy*, *pur-/pol-/pil-*, *ayas/aes/aiz*, respectively in other IE branches. To suppose that the substrata in India and in south or west Europe had identical lexical influence upon the immigrants from the Russian steppe is beyond all credibility. But it is credible and very probable that the substratal words *kunĵi*, *pis-* etc did not give way to the onset of the IE corresponding words when the latter arrived at south Russia, and succeeded in dispelling them.



The Anatolian situation is most peculiar<sup>7</sup>. The Hittites emerge into history c 2000, mentioned in the Old Testament, Egyptian records and one tradition from 15th cent which places them at c 2200 calling them “kings of Hatti”. (Here one might pause and wonder whether *hatti* is a derivative of *kṣatriya*, however far-fetched this may sound). Their “earliest authentic inscriptions” are from c 1650-1620 (Gurney 1990: 1-17). An eminent scholar in Hittite and (comparative) philology also regards them as intrusive in the area (Puhvel, 1994). They were conquerors and rulers establishing an extensive kingdom that developed into an Empire. Yet they failed not only to impose their own culture on the other peoples around and under them but even to rescue it from sinking almost wholly under the diffusion of Hurrian, Sumerian and other foreign influences. Apart from the myth of Inara’s slaying of the dragon, their religion or mythology is non-IE.<sup>8</sup> Their language contains some very archaic features<sup>9</sup> like the stems with alternate *r/n* suffixes (Misra S 1968: 70-1; Burrow 1973: 127, 226), but it has only two genders and no dual while the greater part of its vocabulary is of non-IE origin (Lockwood, 1972: 262, 269). Since there is no evidence that they had been conquered and held in long subjection, this state must have occurred *because*

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<sup>7</sup> C Renfrew accepts (1987, ch 8) an indigenous, unbroken IndoAryan culture from Mehrgarh onwards in N W India but also posits a second hypothesis whereby the Aryans, *having originated in Anatolia, enter India c 1800-1600*; this hypothesis B (élite dominance) he adopts in subsequent works (1990, 1991 etc, and 1997). For his “hypothesis B” Renfrew finds support in D McAlpin’s Elamo-Dravidian affinities and the (hypothetical) split of the Dravidians between 5500-3000 (probably 5th millennium: McAlpin 1981: 134) who moved into India, perhaps, bringing agriculture and mud-bricks. McAlpin, of course, in preparing his “reconstructions” of Proto-Elamo-Dravidian, did not know of Jarrige’s and Meadow’s work (1980) showing that at Mehrgarh (on the Bolan, NW of Mohenjodaro) agriculture began c 6500 and this culture gradually spread to the south-east to develop into the Harappan, or Indus Valley, civilization c 3000 – without any evidence of intrusion. Doubts have been expressed on McAlpin’s linguistic data: eg, acknowledging some affinities, Diakonov, the eminent Leningrad linguist – “Some of the similarities may be fortuitous... Elamite is not a Dravidian language” (p3, *Cambridge History of Iran*, Vol 2, 1985). McAlpin does not deny (p 135) that Dravidian may be cognate with Uralic, as claimed by Burrow, Andronov, et al. For recent Uralic-Dravidian comparisons, see also V Shevoroskin, *Protolanguages & Proto-Cultures*, 1990. SS Misra stresses Dravidian affinities (not borrowings!) with Sanskrit: “Dravidian is more comparable to Indo-Aryan than to any other language family in the world” (1992: 71); so also Emeneau (pp 119-120, 1980). Thus linguistic data alone are unreliable again. Archaeological data do not support any intrusion: see n 18, below, and text.

<sup>8</sup> Gurney, chs VII and VIII; *PCM*, 21ff, esp 24-6. *DANEM*, passim. LEM, 84.

<sup>9</sup> The laryngeal(s) *h*, considered by Burrow and others as an archaic feature, is hotly contested by some scholars: eg Lockwood 1972: 269; also SS Mirsa, *New Lights on Indo-European Comparative Grammar*, Benares 1975, and *TheAryan Problem*, N Delhi, 1992; etc: it is interesting that it occurs out of all the IE languages only in Hittite, which flourished in a region of languages with a strong incidence of the laryngeal(s) *h*. Burrow gives also the absence of the feminine gender in Hittite as an archaism and considers its presence in other IE members as an innovation, ignoring the fact that, as Lockwood points out (1972: 269), Hittite was becoming genderless! Why and how the other IE branches, including Tocharian, should suddenly in the late 3rd millennium develop the feminine gender, when ancient Egyptian and modern Lithuanian, Spanish and French function so adequately with only two genders, is a mystery which Burrow does not attempt to explain. See J Puhvel: “It is not impossible that such feminines once existed in Anatolian but failed to maintain themselves...” (1991: 57).

*they had forgotten or no longer understood their own IE heritage; they became strangers to their own original traditions because – there is no other alternative – they had travelled a long way for a long time mixing with, and adopting elements from, non-IE cultures in their wandering.* The same holds for the Kassites and the Mitanni who appear as rulers in the Fertile Crescent c 1700 and 1500 (Roux 1992: ch 15-16); the scanty IndoAryan vocabulary in their language declares that their original IE tongue was by then very thoroughly *dead*. (Also Mallory 1989: 42.)<sup>10</sup>

On this count the RV is much older than the Hittite culture and therefore well before 2000. As for the original homeland, it is noteworthy that Mallory rejects the locations of Central Europe, Balkans, Anatolia and Pontic steppes and accepts the last one as “merely the ‘least bad’ solution” (1997: 115)

### **3. The Literary aspect.**

In the RV (or later texts) there is no hint of the Āryas coming into the Saptasindhu from elsewhere, no trace of memory of previous habitats. It is unlikely, given the prodigious oral tradition they developed and the mnemonic power supporting it, that they would not have brought recollections and interwoven them, even unwittingly, in their hymns. So Keith wrote (1922: 79): “It is certain ... that the Rigveda offers no assistance in determining the mode in which the Vedic Aryans entered India ... the bulk at least [of the Rigveda] seems to have been composed rather in the country round the Sarasvati river.”

In his noteworthy detailed examination of several names and phrases in the RV, Witzel finds references to an immigration but admits them to be “indirect” (1995: 321). The names of rivers and peoples cited (even if these were outside the Saptasindhu region), known to Iranians also (321), do not necessarily show a migration but only information about them – and possibly a migration of Iranians westwards. As for the phrases in the RV hymns that indicate movement of tribes across rivers, over floods, through a pathless region, narrow passes etc, they can hardly be regarded as evidence of immigration in “several waves” (323). When we consider that these people inhabited a country having mountains, valleys and ravines and at least seven major rivers that flooded, and that groups or tribes moved about, often in military campaigns, we need not speculate about immigration(s) from distant, unremembered lands. After all, Witzel himself tells us that even today tribes on the Panjab-Afghanistan borderland move up and down the mountains due to seasonal changes (322). And to say that the Brāhmaṇa texts “manage to garble the evidence” for an immigration (340) is an indirect admission that there is no evidence. As we shall see below, other people do remember their immigration and record it clearly, if not always correctly.

Apart from its silence on a former homeland the RV contains some positive indications about the Āryas’ very long presence in the Saptasindhu (as against the notion of a recent immigration). Hymn X, 75 gives a list of names of rivers not in the order west-to-east, as one would expect from invaders advancing in that direction, but from east-to-west, as of a people long settled and having the east as a starting point of reference. Then there are passages expressing the Aryas’ strong sense of being rooted in their land when they recall

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<sup>10</sup> It is worth noting that the IE elements, present in the Mitanni language were of IndoAryan provenance, not Iranian or Indo-Iranian (Burrow 1973: 27-30 and Misra S 1992: 8-11.)

Surprisingly neither in this publication nor in that of 1997 does Mallory show more than a cursory acquaintance with the literature about Indian prehistory.

For the Balkans as a possible homeland of PIE, see I Diakonoff (1985). The situation here is not dissimilar to that of central Europe.

their ancestors taking their place in the sacrifice ‘here’, like the ancient Angiras-family (IV, 1, 3) or the Vasiṣṭhas (VII, 76, 4), etc.

The Greek Epics, of course, betray no memory of immigration and earlier habitats. Here the timespan is incomparably bigger: even if we take 1600, the lowest date of the ProtoGreeks’ settlement, (Wyatt 1970: 107; Drews 1988: 170), we have a gap of some 800 years to the Epics (9th cent) – while the Mycenaean texts consist of inventories and no literature (Ventris & Chadwick 1959). Otherwise, even late traditions retain memories of migrations: the Irish Celts recall 5 or 6 waves (MCM, 54-63); the Scandinavians, according to Sturluson, came from Troy (*Edda*, 1-5, 57-8); the Anglo-Saxons composed poetry showing clearly that “The consciousness of their origin from and their strong links with the North West Europeans continued long in the new land” (Branston, 22). The Judaic tradition, too, remembers the very early migration of Abraham from Ur (*Genesis* 11).

The Iranian *Avesta* provides similar recollections in Fargard I (DZA, I, 4-10) naming 16 lands, not in a strict geographical order. But here we find also a direct reference to *HaptaHendu* which is the Vedic Saptasindhu, ie the region of the Seven Rivers. This indicates that the Iranians recall having lived in the Land of the Seven Rivers before settling in their new homeland. *Thus at least one IE people recalls leaving from NW India.* (We ignore the nineteenth century Baltic people’s “tradition” of descent from India since it is so late, and perhaps based on ideas current in the early 1800’s, despite arguments to the contrary (Chatterji, 1968).)

The Avestan testimony is supported in a way by various passages in the Vedic Corpus. The early *Aitareya Brāhmaṇa* (VII, 33, 6 or VII, 18) writes of sage Viśvāmitra exiling his 50 sons so that, in later periods, “most of the Dasyus are the descendants of Viśvāmitra” mostly in the east (*Vedic Index*, ‘Andhra’). RV VII, 18, again, tells how Indra helped king Sudās defeat his numerous opponents, many of whom were of Aryan tribes, and “scattered them far over the earth” (*pārā sárdhantaṃ nunude abhí kṣám*: 16). And in RV VII,6,3 “Agni assailed repeatedly those Dasyus and from the east turned the unholy ones to the west.” (...*pūrvaś cakāra-áparām*). Now since the Dasyus are, according to the “invasion” theory, native Dravidians, they should have been driven south; yet our text states unequivocally “west”! Are we to suppose that the natives were driven west and then somehow managed to return and travel south? This does not sound very plausible. Surely, it is not out of the bounds of possibility that such bands of exiled or scattered warriors moved west to Iran and the Near East or North and then westward to Europe. On the linguistic evidence, the Mitanni at least were of IndoAryan descent, not Iranian<sup>11</sup>. And Benveniste (1973: 260-1) after considering the Vedic *dasyu* and Iranian *dahyu* concludes, “Thus the connexion between the sense of *dahyu/dasyu* reflects conflicts between the Indian and Iranian peoples”: this is

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<sup>11</sup> See n 10 above and text. We must not ignore, however, that often in the RV the *dasyus/dāsas* are presented as demons or demonic forces opposing the quest for heavenly light: eg “Indra should help the Ārya worshipper in battles... in contests for heaven (*svarmīlheṣu*)” I, 130, 8; “Agni shone when born, with light (*jyótiṣā*) killing the dasyus and darkness (*tāmas*)”, V, 14, 4; etc, etc. For a more detailed examination of “Good Guys” and “Bad Guys” regarding Aryans and *dāsa/dasyus* see Hock who rejects simplistic conflicts between Aryans & natives (1996: 54-5). For the history of the racial aspect developed by British sanskritists and ethnologists in the 1850’s, see T R Trautman’s *Aryans and British India*, 1997 N Delhi, pp 172-216. The Brahuis in Baluchistan speak a Dravidian type of language but as we have no ancient attestation of their presence (like the Iranians or Mitanni, et al) they could represent a migration from the South in late times (just like the Parsis from Persia in Maharashtra): they can hardly be regarded as evidence for Dravidians in the prehistoric era.

supported by the RV passage, where the *dasyus*, perhaps ancestors of the Iranians, are driven west.

The common theory of the IndoAryans' entry c 1500 and the composition of the RV c 1200 is well outside the bounds of reasonable probability. Since the Vedic Aryas preserved to a higher degree than the other IE branches the inherited forms of PIE language and mythology, they could not have remained speechless for 300 years as the theory demands: (grand-)parents must have told tales to their (grand-)children and priests must have invoked in ritual prayer and in thanksgiving their gods (Agni, Indra, Sūrya, Varuṇa et al). It is hardly likely that memories of their former habitat(s), and of adventures and perilous threats during their long trekking to Saptasindhu, would not have found their way into the stock of legendry they carried and not been expressed in the RV Hymns. But of this there is not a single trace in the Vedic Corpus.

Moreover, it is extremely unlikely that the light war-chariots of the Aryans could have climbed up then down those difficult mountain passes or that the incoming Aryans fashioned them after their descent onto the plains and then proceeded to subdue the natives. That the illiterate barbarians expunged so fully the language and culture of the literate natives throughout the vast and populous area from the Indus to the Ganges plain is an event without parallel in the known history of man.<sup>12</sup> The area involved is "1.5 million square kilometers" (Rao 1991: 1).

An additional curiosity is that the illiterate barbarians produce shortly afterwards a large collection of hymns, some of great beauty and some of penetrating philosophical inquiry, while the literate natives leave nothing but some seals. Thus we have the startling paradox of a literate, archaeologically well attested civilization without literature, and a literature without a literate and archaeologically attested culture to support it, both in the same region in quick succession. Furthermore, these natives built planned cities with straight streets, large storehouses, baths and other structures involving various geometrical forms and exact calculations, yet left no corresponding documentation of such knowledge, while the Mathematics necessary for such constructions are contained in the *Śulbasūtras* of Āpastamba and Baudhāyana, two millennia later according to the standard theory, ie in the sūtra period after 600 BC. However, A Seidenberg, the late American mathematician, did not hesitate to assign the *Śulbasūtras*, or a work like it, to well before 1700 or even 2000 seeing it as the source for Egyptian, Babylonian and Greek Mathematics (1962: 515, 519; 1978: 318-9). Seidenberg writes of this original work:

"its mathematics was very much like what we see in the *Sulvasutras* [*śulbasūtras*]. In the first place, it was associated with ritual. Second, there was no dichotomy between number and magnitude ... In geometry it knew the Theorem of Pythagoras and how to convert a rectangle into a square. It knew the isosceles trapezoid and how to compute its area ... [and] some number theory centered on the existence of Pythagorean triplets ... [and how] to compute a square root. ...

The arithmetical tendencies here encountered [ie in the *Śulbasūtras*] were expanded and in connection with observations on the rectangle led to Babylonian mathematics. A contrary tendency, namely, a concern for

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<sup>12</sup> A possible parallel may be the disappearance of the Minoan culture after the advent of the Mycenaean. The facts here are not as clear as one would like; moreover, the Minoan civilization on Crete was contained and of smaller extent than the Harappan.

exactness of thought ... together with a recognition that arithmetic methods are not exact, led to Pythagorean mathematics. (1978: 329)

We can therefore safely assume that the knowledge in this Sūtra literature was extant in the late 3rd millennium, at least, and perhaps even in the extant sūtra formulations.

#### 4. Items not in the R̥gveda.

(a) The RV mentions no rice *vrihi*, as the *Vedic Index* shows. Rice was found in at least three Harappan sites: Rangpur (2000-1500), Lothal (c 2000) and Mohenjodaro (c 2500) as Piggott (1961: 259), Grist (1965) and others testify (Rao 1991: 24, 101, 150 etc). Yet, despite the importance of rice in ritual in later times, the RV knows nothing of it. Sankalia points out (1979: 109) that “this grain was unknown to the *Rigveda* as well as to the *Avesta*”.

(b) The RV mentions no cotton *karpāsa*, although wool and skins, shirts, garments and undergarments, loom, warp and woof and the like are abundantly evident (*Vedic Index* under ‘Clothing’). *Karpāsa* is the only word for ‘cotton’ in Sanskrit and it first appears in Gautama’s (I,18) and in Baudhāyana’s (XVI, 13, 10) *Dharmasūtra*. Yet the cultivation of cotton is well attested in the Harappan civilization and is found at many sites thereafter. B B Lal (1975: 17) writes: “Perhaps the most remarkable agricultural achievement was the cultivation of cotton. Even Egypt did not produce it until several centuries after it was grown in the Indus Valley”.

Here we can pause admitting that an argument *e silentio* is not actual evidence, much less proof. When a second important item is not mentioned, the silence, we feel, is indicating something. When a third important item is not mentioned, then the silence can hardly be regarded as accidental. The third item is silver.

(c) The RV knows no silver. It knows *ayas* ‘metal’ or ‘copper/bronze’ and *candra* or *hiranya* ‘gold’ but not silver. There is a generally accepted demarcation line for the use of silver c 4000 and this metal is archaeologically attested in the Harappan Empire (Allchins 1969: 285; Rao 1991: 171)<sup>13</sup>. The word *rajata*, used for ‘silver’ in later literature, appears once in RV VIII, 25, 22 in a context of horses and means ‘shining-white’. Silver is denoted by *rajatām hiraṇyam* literally ‘white gold’ and appears in post-Rivgvedic texts. But the cognate stem *arg-/arc-* (*S rj-/raj-*) significantly appears in other IE languages connoting both ‘white’ and ‘silver’, eg G *argēs* ‘white, shining’ and *árguros* ‘silver’.

(d) The stem *ayas* in RV denotes ‘metal (or bronze)’ not ‘iron’. Iron appears as *śyāmāyas* in *Atharva Veda* XI, 3, 7, *Yajurveda* XIII, 13, etc. Yet iron is in use by 1300 (conservatively Allchins 1997: 227) and perhaps in Mature Harappa: the Harappans “had access to, or knowledge of, iron technology” (Shaffer 1983).

On this set of evidence the whole of the RV, except for some few passages which may be of later date, must have been composed by 3100, when the Harappan civilization began to develop. This is supported by the evidence that follows.

#### 5. The Harappan culture.

This is also unknown to the RV. Other characteristic features of the Harappan culture, apart from the four items mentioned above, are urban life, large buildings, permanently erected fire altars and bricks. There is no word for brick in RV and *iṣṭakā* ‘brick’ appears

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<sup>13</sup> But see Hicks A A and Anderson R N, ‘Analysis of an Indo-European Vedic-Aryan Head – 4th millennium BC’ in *Journal of Indo-European Studies*, 18, p 425-45, 1990. The casting with high silver content known as “Vasiṣṭha’s Head”, found near Delhi in 1958, was dated (both in California and Zurich), with radiocarbon and metallic-crystallization tests at between 3800-3700.

only in *post-Rigvedic* texts. In fact all these cultural elements appear unmistakably in the Brāhmaṇas. B and R Allchin wonder whether “ritual hearths” carry with them “an indication of an Indo-Aryan presence even from so early a date” (ie 2900-2000 at Kalibangan) and note that they are of the kind “described in detail in the later Vedic Literature” (1982: 203).

Certainly, there is mention in the RV of *pur* ‘fort, city (?)’, sometimes belonging to Dasyus and sometimes to the Āryas (I, 166, 8; etc). But these *puraḥ* are always made of metal (eg IV, 27, 1 *āyasi*) or of stone (eg IV, 30, 20: *aśmanmāyin-*); there is not a trace of brick anywhere in the hymns.<sup>14</sup> The Rigvedic sacrificial altar is little more than a shallow bed (dug out in the ground and) covered with grass (eg V, 11, 2 & VII, 43, 2-3; Piggott 1961: 286 and Parpola 1988: 225). Brick-built altars are mentioned extensively in the Brāhmaṇas (eg *Śatapatha* 7, 1, 1, 37; 10, 2, 3, 1; etc).

In addition, as was mentioned above (sect 3, end), a strong feature of the Harappan culture is town planning, which would have been impossible without substantial knowledge of Geometry. This knowledge, as was indicated, was very probably the formulations contained in the Śulba Sūtras. But other sūtras are also cogent. Bṛhaspati’s lawbook, for instance, states “A privy, a fireplace, a pit or a receptacle for left-overs of food and other [rubbish], must never be made very close to another man’s house” (XIX, 26: Sacred Books of the East, vol 33, p 354). This rule is consonant with town-planning allowing every household to enjoy its dwelling without disturbances from others. This rule would become necessary only when population increased and communities became congested: this would correspond with the rise of the mature Harappan. Here we have an additional indication that some sūtra texts may have been formulated at that period and it is only our attachment to the mainstream chronologies that prevents us from seeing other possibilities.

It is impossible that the aforementioned cultural elements were forgotten or ignored throughout the composition of the RV Hymns, which would be, according to the invasion theory, closer in time to the Harappan civilization, and then, after four centuries (RV 1200; Br 800), were resuscitated in the prose texts. The evidence shows compellingly that the RV is pre-Harappan while the Brāhmaṇas and perhaps some Sūtra texts are contemporaneous with that culture.

### 6. *The Exegetical Texts, Brāhmaṇas .*

We must further take into consideration the fact that many sections of the Brāhmaṇas (and later texts) comment on or give explanations of passages or whole hymns in the RV. Such are, for example, the *Aitareya Br* VII, 13, 33 on the allusions to the Śunaḥśepa story found in RV I, 24, 12-13 and V, 2, 7, and the *Śatapatha Br* XI, 5, 1, on the Pururavas and

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<sup>14</sup> Burrow thinks (*Journal of Indian History*, vol XLI, Apr 1963, pp 159-68) RV I, 133, speaks of ruined cities (*arma-kā*). The Allchins use this as evidence for “Aryan invasions” (1968: 155). There is a devastated landscape certainly, but whether *Mahāvailastha* is a (Harappan) city is at least dubious. The text speaks of “unfriendly ones” who are she-fiends and demons (*yātumāti*, *piśāci* and *rakṣas*). No picture of a ruined city emerges and there is no mention of “city”, of bricks or any other building material. In sharp contrast, the Old English poem *The Ruin* contains such persuasive details of the ancient remains that some think it describes the Roman city of Bath: pp 253-5, B Mitchell & F Robinson, *A Guide to Old English* (1994), Blackwell 1996.

Rao too writes (1991: 32) of “*arma* and *armakas* ... ruins of Harappan towns ... well known to the Rigvedic Aryans”, obviously ignoring the actual text of RV.

Cf also Śuṣṇa’s (drought-demon’s) *pūr* which is *carīṣṇu* ‘mobile’ (!) in VIII,1,28. What is this mobile *pur*?

Urvaśi lovestory in RV X, 95. It is well known that such exegetical texts are not composed until the full understanding of the older writings has lapsed. This implies centuries.

Now additional evidence for dating the Brāhmaṇas comes from Archaeoastronomy. Subhash Kak recently<sup>15</sup> examined the position of certain nakṣatras (= constellations: eg the Pleiades) and rituals as described in the Brāhmaṇas and “*anambiguously*” dated “the rites described in the Brāhmaṇas to the second millennium BC”. This nullifies the usual dating of these texts at 800-600 BC. But Kak’s date is a lower limit which does not preclude an earlier date in the 3rd millennium – where they should be assigned since they reflect the Harappan culture. In fact, in another work, (1994: 35), Kak writes: “the *Śatapatha Brāhmaṇa* (2.1.2.3) has a statement that points to an earlier epoch where it is stated that kṛttikā (ie the Pleiades) never swerve from the east. This corresponds to 2950 BCE.” A little earlier he gives a classification of *yama nakṣatra* (twin-constellations) and comments: “This classification in Taittiriya Brāhmaṇa (1,5,2,7) corresponds to 2300 BCE.”

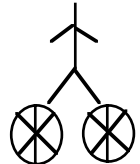
B B Lal, the eminent archaeologist, writes that astronomical calculations (re Aitareya Brāhmaṇa) “would place the Rigveda in the fourth millennium BC”; he sees no reason to reject such a date and he does not adopt it outright only because he feels in no position to judge such calculations (1997: 286).

The RV then must be c 3100 or earlier.

### 7. Horse, chariot and spoked wheel .

Taken all together, the evidence compels us to assign the RV prior to the Indus Valley civilization, ie 3100. Some scholars (Misra 1992, and Sethna 1992) assign the RV to the 5th millennium and even earlier. This seems far fetched. Misra’s linguistic evidence is unreliable on its own – while Sethna follows him adducing some not entirely convincing data. Some hymns may well originate from much earlier periods – perhaps even earlier than we dare think – but not the RV as a whole, as we have it.

The RV itself provides at least one sure check against claims for very early dates. Some hymns mention the spoked wheel (eg V, 58, 5; VIII, 77, 3; etc) in a way that definitely indicates the common wheel with nave, separate spokes and felly. Now the six-spoked wheel appears on seals and signs of the alphabet (Sankalia 1974: 363; Rao 1991: 192). At Lothal, Rao found “terracotta wheels ... with diagonal lines suggesting spokes” (1973: 124). This representational practice seems to have been widespread, for Piggott mentions similarly marked wheels found in the Karpathian Basin from the Earlier Bronze Age (1983: 91-2). A most telling piece is the figure on Seal No 3357 representing simplistically a man with outstretched arms standing on two six-spoked wheels. Parpola, who favours the “Aryan invasson” theory, identifies them as wheels “while realizing full well that the spoked-wheeled war chariot was a later invention of the Aryans” (1969: 24). The Harappans certainly had the technology for the spoked wheel: they “were technically in advance of their contemporaries [Sumerians] – they had devised a saw with undulating teeth, which allowed the dust to escape freely from the cut, and much simplified the carpenter’s task” (Basham 1961: 21); they had moreover the twisted bronze drill, discovered by Rao at Lothal, the occurrence of which “at so early a date is of great moment in the history of civilization” (Sankalia 1979: 61). However it would be rash to assume that such a technology operated before 4000 BC.



As for the cart, a more sophisticated type of vehicle with “one or two pairs of wheels with their axles” is known “from the Rhine to the Indus by around 3000 BC” (Piggott,

<sup>15</sup> ‘Archaeoastronomy and Literature’ in *Current Science*, Oct 1997. S C Kak is Professor of Electrical and Computer Engineering in Louisiana State Univ with numerous publications on the sciences of the Vedas in various Journals, including the *Quarterly Journal of the Royal Astronomical Society* : vol 36, 1995 and vol 37, 1996.

with their axles” is known “from the Rhine to the Indus by around 3000 BC” (Piggott, 1992:18).

The Harappans had also “domesticated sheep, horse and cattle” (Rao 1991, 109 etc). There is evidence of earlier horse-domestication, before 3000, in Phase I Rana Ghundai (Peake and Fleure 1956: 228; Piggott 1961: 121; Wheeler 1953: 60). Startling evidence comes from a Report, published by Allahabad University, by G R Sharma who had as co-workers, apart from Indians, Dr M Williams and K Royce, members of the team under Prof J Desmond Clark. The palaeolithic sites of the Belan and Son valleys (Ganges area) yielded fossils of cattle, horse, stag, elephant etc. The neolithic sites of Koldihwa and Mahagara yielded evidence of domestication of animals (cattle, sheep, goat and horse) and cultivation of plants (rice). With radiocarbon and C-14 tests were obtained the following dates: the upper palaeolithic sites (wild horse) 23840 - 17765 and probably earlier; the neolithic Mahagara (domesticated horse)  $6570 \pm 210^{16}$  and the neolithic Koldihwa  $5440 \pm 240$  and  $4530 \pm 185$  (Sharma, 1980: 110-112).

Thus with the domestication of the horse in Harappan times, and earlier in the west and east, and with knowledge of the cart and the spoked wheel, c 3000-2800, the archaeological evidence can well accommodate the composition of RV sometime before 3100.

#### **8. The Indus Script.**

Although much has been written about the Indus Script, the fact is that it remains undeciphered. Some claim it is Sanskrit (Rao, et al); others that it is Dravidian (Parpola, et al). Lal’s survey (1997: 203-14) shows clearly that all claims are unconvincing and have serious faults in their methodology. R and B Allchin cite Possehl, who in 1996 made a thorough study of some 35 different attempts at the decipherment of this Script; Possehl’s conclusions concur with Lal’s view. Should the Indus Script be deciphered (particularly with new finds with longer legends), then clearly new light will be thrown on IVC and its period. At present there is no point in making conjectures about this aspect.

#### **Conclusion**

The date 3100 is the one given by the native tradition of India for the compilation of the RV. The tradition seems to be correct. I have also adduced Seidenberg’s independent evidence suggesting that the Mathematics contained in the Śulba sūtras was known in the latter half of the 3rd millennium. Beyond this I do not propose to go.

This dating of the RV will assuredly be doubted and criticized. But any valid criticism will have to be directed against certain points. Philologists, for example, who insist on remaining entrenched in the internal linguistic evidence of the RV and its relation to

<sup>16</sup> This early date matches the early date of horse-domestication given by M Gimbutas, ie 6500, for the Volga Neolithic ‘in ‘Accounting for a Great Change’ in *Times Lit Suppl*, 23-30 June, 1988. This coincidence may explain why Slavonic has *kunji*, but other IE languages *aśvarequus* (and variants), for ‘horse’.

In *Looking for the Aryans* (Orient Longman, Madras, 1995) by R S Sharma, this Mahagara horse is described as “an isolated species of horse distinct from the one inhabiting areas in the USSR, Iran, Afghanistan etc, and associated with the Aryans” (p 17): obviously the author and those he follows do not realize they have prejudged the case.

At these sites in the neolithic period also has been found ample evidence of rice cultivation, “the earliest evidence ... so far in any part of the world” (Sharma 1980: 110). This sharply contradicts V Shnirelman’s claim that rice was introduced into India from the East in the late 3rd millennium and its alleged connection with the Dravidian languages (1997: 162).



Avestan etc, will need to explain satisfactorily how the RV poets, whom they place at 1200-1000, knew of the Sarasvatī river as it was 800-1000 years earlier at least, yet they knew nothing of iron, silver, rice and cotton, which were in contemporary use and known in post-Rigvedic texts; why the Harappan elements – towns, buildings, altars, bricks – are reflected in the Brāhmanas but not in the RV which, being earlier, should be closer to them in time; also why the RV preserves so much more of the inherited PIE language and culture, while other branches do not, even though certain of the latter have supposedly moved much less or not at all. Otherwise reactions will be emotional rather than rational.

Despite Dales's published discovery of the true causes of the collapse of the Harappan civilization, Sir Mortimer Wheeler did not relinquish his view of the Aryan invasion in his last work (1968). Few of us are able to apply to ourselves Darwin's dictum "I have steadily endeavoured to keep my mind free so as to give up any hypothesis (and I cannot resist forming one on every subject) as soon as facts are shown to be opposed to it" (Beveridge 1968: 59). And ten years after Dales's first publications, followed by several others by other archaeologists (eg Rao 1973), Burrow wrote: "The Aryan invasion of India is recorded in no written document, and it cannot yet be traced archaeologically, but it is nevertheless firmly established as a historical fact on the basis of comparative philology" (1975: 21). The little word "yet" in this statement shows how right Max Plank was in saying that often only death can separate scientists from their cherished theories (Kak 1994: 25). Yet, some 22 years later, R and B Allchin, again, follow Parpola and conjecture that the "Vedic Aryans moved eastward ... in the valleys of the Punjab and the Sarasvatī... Their presence *should* therefore be in evidence archaeologically... *But as yet* it is *scarcely* attested in the archaeological record *presumably because* their material culture and lifestyle were already indistinguishable from those of the existing population" (1997: 221-2).<sup>17</sup>

The simple fact is that archaeological data do not support any immigration into NW India prior to the 1st millennium. Jarrige and Meadow (1980) established the indigenous Mehrgarh culture with cereal cultivation c 6500 on the Bolan, north-west of Mohenjodaro, and its gradual spread south-east to the Indus developing into the Harappan, or Indus Valley, civilization c 3000; in a later study (1991), Jarrige placed the beginnings of Mehrgarh somewhat earlier than 7000.<sup>18</sup> Furthermore, according to Shaffer (1984), the Painted Grey Ware that had been associated with the supposed Aryan entry have been shown to be "an

<sup>17</sup> Emphasis added. The Allchins are evidently unaware of the subtle contradiction in their statement. And of course, they don't explain how the intruders (1700-1300 BC) knew of Sarasvatī as it was before at least 2000!

<sup>18</sup> Mallory (1989) disregards Jarrige's researches and Shaffer's which he has met in Renfrew. Like Renfrew (1991: 13-4), he cites D McAlpin's Elamo-Dravidian affinities and the Dravidians' migration (pp 55-6) to the Saptasindhu. McAlpin's "reconstructed" linguistic evidence was mentioned in n 7, above. His archaeological evidence is cultivation of barley and wheat (1981: 133). Both cereals were cultivated at Mehrgarh since c 6500 as Jarrige has shown, so that the putative Elamo-Dravidian entry in the 5th millennium comes too late. In the 540 pages of *Frontiers of the Indus Civilization*, (eds B B Lal & S P Gupta, Indian Archaeological Society, 1984) there is no mention of Elamite migrations. Contributors in this 'Sir Mortimer Wheeler Commemoration Volume' are Allchins, Jarrige, Konishi, Possehl, et al, and D L Hessel writes: "previous theories of wholesale population migrations... are not acceptable in the light of archaeological evidence" (p 343). For a similar view see also Allchins (1997: 191). Konrad Elst (1993: 127-9) hesitantly accepts a Dravidian migration and offers the possibility that it moved along the Gujarat coast and down into South India, without intruding into the Indus Valley civilization, which he regards as Aryan.

indigenous cultural development [that] does not reflect any cultural intrusion from the West, that is an Indo-Aryan invasion. (...) [I]t is possible to document archaeologically a series of cultural changes reflecting indigenous cultural development from prehistoric to historic periods. The early Vedic literature describes not a human invasion into the area but a fundamental restructuring of indigenous society that saw the rise of hereditary social elites. (...) Linguistic reconstructions for the area are no longer independently supported by the archaeological data, and even if one is reluctant to disregard these reconstructions completely, the present data nonetheless suggest critical reevaluation of earlier interpretations.” (Shaffer 1984: 85, 88; cf RAL: 209).

Eleven years later the fact has not changed: “The shift by Harappan groups, and, perhaps, other Indus Valley cultural mosaic groups, is the only archaeologically documented west-to-east movement of human populations in South Asia before the first half of the first millennium BC” (Shaffer & Lichtenstein 1995: 139). Two years later, even R and B Allchin do not adduce one shard of evidence (1997: 221 ff).

To expect, as some scholars do (eg Mallory 1997), that one should demonstrate how various linguistic developments or/and archaeological data harmonise, is something of a dream. The PIE language was as full of dialectal variants as was Old English, or Proto-Italic, Proto-Greek, etc: thus, although many changes have been explained, no absolute pattern for all changes can be formulated. Thus while the Skt *bh* correponds generally to Gk *ph* as with Skt *bhū* Gk *phuō* or *bhaga phoibos*, yet Gk has also *bag-* in Phrygian and Skt *labh-ate* correponds to Gk *lambanō* (but perfect *eilēpha* with *ph*).<sup>19</sup> With the archaeological evidence the difficulties are probably insurmountable since tools, weapons and other finds cannot furnish linguistic facts nor provide clear patterns of diffusion. The difficulties are acknowledged by Mallory (1997: 117).

The RV Saṃhitā as we have it (barring some few passages of later dates) must have been compiled just before the rise of the IVC at about 3100 BC. The Aryan origins are firmly rooted in NW India. The source of the PIE language and culture is in the ancient Saptasindhu. Since according to the archaeological record no other peoples entered into the Saptasindhu where the RV got composed in an IE language in the 4th millennium, and since other IE-speaking peoples entered according to archaeological and/or linguistic data into south, west and north Europe and in the Near East, the latter must have originated in the Saptasindhu. Those indologists, archaeologists et al who, generation after generation, seek to discover evidence for the Aryan entry, could, perhaps more profitably, re-examine and reappraise the data and trace the lines of movement *out of the Saptasindhu* westward and northward all the way to the western extremities of Europe. Philologists on their part can set Sanskrit studies and Comparative Philology on an entirely new footing acknowledging the antiquity of the *Ṛgveda*. In Indology at least, the earlier date for the *Ṛgveda* accommodates earlier dates for the Brāhmaṇas, Upanishads and Śrauta and Gṛhya Sūtras, as we saw; the early grammatical works including Pāṇini and the works on which the philosophical systems

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<sup>19</sup> Many other cases of exceptions to the established rules can be cited. Two common examples should suffice. Skt *dvār* ‘door’ is rejected from the IE cognates for “door” (Gk *thúra*, OE *duru*, Sl *dvīrī* Av *dwar*, etc) since the rules require a Vedic form *\*dhwār* that would correspond with Gk *thura*; *dvār* is explained as being associated with the two (*dvā*) folds or planks that make a door or gate (Mayrhofer II, p 83). This may be right but it seems more likely that at that early period a door would have a single fold or plank and that the *dvār/thura* cognation is just another exception to comparative rules. Skt *hr̥d/hr̥daya* also (apart from the generally accepted *śrad-*) could be one of these exceptions among the cognate forms for “heart” as Gk *kardia*, L *cor*, Av *zər̥d-*, OE *heorte* etc.

are based (for which I claim no expertise), will also need to be moved further back. Problems will arise, no doubt, but so will solutions, affecting many areas beyond Indology. What magnificent opportunities for fresh research!

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