

The Vedic Harappans in writing Dr. Koenraad Elst

Remarks in expectation of a decipherment of the Indus script

In the Harappan cities some 4200 seals, many of them duplicates, have been found which carry short inscriptions in an otherwise unknown script. There is not the slightest doubt that this harvest of Harappan writings is but the tip of an iceberg, in this sense that the Harappan culture must have produced much more copious writings, but that most of them disappeared because the writing materials were not resistant to the ravages of time, particularly in the Indian climate. This fatality can still be seen today, when the libraries of many impoverished maharajas' castles are full of manuscripts which are decaying under our very eyes, turning large chunks of India's national memory and heritage into dust. Even of the oldest and most popular texts, the extant manuscripts are seldom older than a few centuries, copies made as the only guarantee to save the texts from the ravages of time which were destroying the earlier copies. It is fair to assume that a text corpus proportionate in size to the enormous extent of the Harappan cultural space, has gone the same way into oblivion.

In 1996, Gregory Possehl published a survey of the extant decipherment attempts, which is essentially a catalogue of failures. Right now, I am looking forward to the publication of two rivalling and entirely independent decipherment proposals, both interpreting the script as Sanskrit or at any rate Indo-Aryan, one by Dr. Natwar Jha and Prof. N.S. Rajaram, and another one by the Portuguese indologist José Calazans. If only as a warm-up to an analysis of the merits of these decipherment efforts, I propose a few thoughts on some past attempts.

1. The Dravidian hypothesis

The Dravidianist hypothesis of the late Walter Fairervis jr., of Asko Parpola, of Iravathan Mahadevan and others assumes that the Indus script is totally isolated (e.g. unrelated to the Brahmi script of the Maurya age), so that the final control which the relation with other, known scripts might afford, is excluded. Unless and until a kind of Rosetta stone is found, a bi-lingual text juxtaposing a text in Indus script and its translation in another, known language, their hypothesis hangs in the air, not susceptible to passing any external test. To my knowledge, even the internal test has not been passed: deduce the decipherment from half the corpus and then apply it to the remaining half and see if it makes sense. They assume no relation with the later Brahmi script, a script used by the "Aryans" who, within the Dravidian hypothesis, had come later and had destroyed (or at least not continued) the Indus civilization. Nor is their interpretation such that it lends itself to the suspicion of a relation with known (deciphered) West Asian scripts.

What could help the Dravidian reading, is a relation with the undeciphered Elamite script, which was used in the same period. The thesis of a linguistic kinship

between Elamite and Dravidian has become fairly popular, possibly because few scholars are both competent and willing to test it. (See David McAlpin: "Toward Proto-Elamo-Dravidian", *Language*, 1974, 50, 1; "Elamite and Dravidian, Further Evidence of Relationships", *Current Anthropology*, 1975, 16; *Proto-Elamo-Dravidian*, Philadelphia 1981) It is well-attested that there were intense trade relations between the Indus area and its immediate neighbour to the west, Elam (southern Iran), so an influence in matters of script would have been quite natural, and all the more so if the languages were still to an extent mutually understandable.

Actually, there does seem to be a similarity between the early Harappan script, which was still largely pictographic, and the as yet undeciphered earliest script of Elam, which is assumed to be early Elamite: "The first [script] was in use from the fourth millennium BC (shortly after the beginning of writing in Sumer) to around 2200 BC. It has not been deciphered and is assumed to be Elamite due to its distribution and undisturbed overlap with the later [cuneiform] Elamite script. In form this older script is very similar to the Indus Valley script." (David McAlpin: "Linguistic prehistory: the Dravidian situation", in Madhav M. Deshpande and Peter Edwin Hook: *Aryan and Non-Aryan in India*, p.175-189)

However, the very nature of pictographic script makes it possible that it was borrowed without changes from one language to another (as Chinese characters were adopted to represent words in Japanese and Korean), so this doesn't inform us about which languages were involved. It is conceivable that the Indus script originally represented Elamo-Dravidian or another language, and was subsequently adopted by the Aryans to represent their own language; or that the creation of a phonetic (alphabetic) script was the result of the need to represent a new language with which the Dravidian scribes came in contact as a result of migration or conquest, viz. Indo-Aryan. Or the other way around: Elamo-Dravidian may have adopted an Aryan pictographic script, which was meanwhile being transformed into an alphabetic script by its native users.

A reliable chronology of the emergence of writing in northwestern India would be helpful to decide the matter, but that is what is lacking, unless you plump for the archaeological conceit of assuming that the oldest datable inscription discovered also happens to be the oldest one to have existed. But the least one can deduce from the combination of the Elamo-Dravidian hypothesis with the hypothesis of a common origin for the earliest Elamite script and the earliest Indus script, is that Dravidian was probably present in the Harappan culture. That is not at all in conflict with a Vedic Harappa theory: like Mesopotamian culture, Harappan culture may have been multilingual.

We will not try to give a detailed analysis of the ongoing attempts to interpret the Indus script as transcribing a Dravidian language. However, two remarks are in order.

Firstly, eventhough many pop books on the Indus culture give it as an established fact that the Indus language was Dravidian, the case for a Dravidian reading

is not that strong, judging from the results obtained. After half a century of being the official hypothesis, i.e. attracting most of the efforts and funds, its formulations are still clumsy and unconvincing. Consider two examples.

Walter Fairservis jr. explains that the Harappan script partly employs the rebus principle, meaning essentially that a more abstract word was written with a pictogram depicting a homophonous but more concrete word (as if you would write the English word *too* as two strokes depicting the homophonous word *two*). Thus, according to Fairservis, the word for "star" (which could well appear on administrative or commercial seals in the context of calendar data) would have been depicted as a fish, because the Old Tamil words for "star" and "fish" (*min-*) are homophonous. (Walter Fairservis jr.: "The script of the Indus Valley Civilization", in *Scientific American*, 1985)

Asko Parpola confirms: "It seems significant that painted Mature Harappan potsherds from Amri III A and C combine the motifs of 'fish' and 'star'. These potsherds suggest that the Indus people associated the two concepts and thereby support the hypothesis that the Harappans spoke a Dravidian language, where one and the same word is used for both of these things." (A. Parpola: *Deciphering the Indus Script*, p.181) He quotes the Sanskrit term *âkâsha-gangâ* for "ecliptic" to illustrate the ancient comparison between the sky and the water, with the stars as fish in the celestial stream (numerous times, Parpola takes his evidence for Harappan cultural motifs from Vedic literature, on the plea that the Aryans borrowed it all from the Harappans). But in fact, in that type of script, something as concrete and easy to depict as a star would never be expressed through a homophonous word, but rather through the shorthand picture of a star, as was effectively done in Sumerian.

A less contrived variation on this stellar decipherment is Asko Parpola's reading of the crab-like sign, suggesting the meaning "seize, grip", as "planet": Parpola points out that both Tamil *kol* and Sanskrit *graha* mean both "planet" and "seize, grip", with the Sanskrit word being a calque on the Dravidian, which shows a homonymy with a word *kolli*, "glowing ember". (A. Parpola: "Interpreting the Indus Script", in A.H. Dani: *Indus Civilisation*, p.117-132, specifically p.127) The explanation is consistent, but that is not enough to make it true.

For a second example, a particular sign looking like a grain stalk would mean "moon", because "*nel* means rice in five Dravidian languages and *nila* or *nela* means moon in three of the same five and in five others as well". (Fairservis: *ibid.*) However, no pictographic script would ever use the rebus principle to represent an easy-to-depict item like the moon. In Chinese, a moon-like character represents both "moon" and "month".

This problem is compounded by the fact that the whole approach of reading homophonous words into the script presupposes that we know the pronunciation of "Harappan Proto-Dravidian", but in fact the oldest Dravidian texts we have are a full two thousand years younger and were written in far-away Tamil Nadu. Without going into

further details, our impression with each one of the Dravidian readings is that in spite of the computer techniques used, a great deal of inspired guessing has gone into them.

A second remark concerns the procedure so far used to crack the Indus code. It over-confidently assumes that the structure of the language (agglutinative in the case of Dravidian, as opposed to flexive in the case of Indo-European) can be deduced from the series of signs available on the Indus seals. A few successes of this method have indeed played a role in the decipherment of Linear-B, but its failures in the attempts at decipherment of other scripts are far more numerous. It suffices to compare the different Dravidian interpretations proposed so far by the respective Finnish, Russian, American and Indian scholars, to find that starting from the same premisses, one reading takes a given sign to be a declension suffix while another takes it to be a preposition and yet another interprets it as a noun root. It is simply not true that with the present linguistic and cryptographic knowledge, the Dravidian-agglutinative structure of the Indus language has been demonstrated.

On the contrary, Subhash Kak has used the same method to "demonstrate" the typical Sanskritic and non-Dravidian use of the genitive case on many Harappan seals, hence the probable Sanskritic and non-Dravidian nature of the text represented. (Cited in Klaus Klostermaier: *Survey of Hinduism*, SUNY 1994, p.38) To me, this does not prove that Harappan was Indo-Aryan, but it certainly refutes the claim that computer-aided linguistic analysis of the Harappan inscriptions has *proven* the Dravidian hypothesis.

2. The Munda hypothesis

In the margin of the Dravidian hypothesis, we should mention the existence of an Austro-Asiatic hypothesis. N.K. Verma, a District Transport Officer of the Bihar State Government, claims to have demonstrated the link between the Harappan script and the Santhali language (belonging to the Munda branch of the Austro-Asiatic language family) of Chhotanagpur (reported in *Times of India*, 7 July 1992). S.K. Biswas reports on Verma's "discovery of Indus Valley script amongst the Santhals of Bihar-Bengal border, after an uninterrupted gap of three thousand and five hundred years time span and at a geographical distance of 2500 miles. (...) He solved this riddle by learning the unique symbols with sound-value which are used by the Santhals of Sahibganj area during their religious rites. (...) Controversy may arise on the point of accuracy or correctness of what Verma has deciphered in the symbols that have been used by the Santhals of Sahibganj. But none is to controvert the fact that the Santhals of the 20th century AD have been scrupulously using the scripts of the 16th century BC independently." (S.K. Biswas: *Autochthon of India and the Aryan Invasion*, p.13-14)

Assuming that this discovery is authentic, we maintain that it is easily compatible with an Indo-Aryan (or Dravidian) reading of the Harappan script. All that is required is that the Harappan script was adopted and adapted by some of the satellite populations

in the vicinity of the metropolitan area. The latter included eastern Uttar Pradesh, which borders on the region where the Santhal and other Munda-speaking tribes still live. Such phenomena of cultural transmission from a metropolis to its surroundings is the most natural thing in the world, as is evident from the adoption and adaptation of the Brahmi script by the Tibetans, Khmers, Thais, and other satellite cultures of the Hindu-Buddhist civilization of India, and likewise the adaptation of the Phoenician script as the Greek, Roman and Runic alphabets, transcribing languages unrelated to the language rendered by the original script.

The fact that the Tibetans use a Brahmi-based script does not, of course, prove that the original Brahmi was devised as a script by Tibetans and for rendering the Tibetan language. And so, the claimed use of Harappan-like signs by the Santhals does not prove that the Santhali language was the language of the Harappans.

3. The Easter Island connection

The same thing counts for the supposed connection between the Harappan script and some inscriptions found as far away as the Easter Island. We will not join the right-thinking academic establishment in dismissing as "ridiculous" the hypothesis that there is a connection between the Harappan and the Easter Island scripts all across two oceans. It is a historical fact that the Austronesian peoples populated the Pacific island world starting from Southeast Asia, and it is a real possibility that they had originally set out, if not from India, at least from places which were in contact with the Harappan sea-faring traders. The two scripts can be shown to be less than perfectly similar, but what would you expect after a journey of ten thousand miles and three millennia?

In case there is indeed a connection between the Harappan and the Easter Island script, it remains strange that of all the Austronesian peoples, the most distant ones would be the only ones to preserve a script which they had adopted at the opposite end of the Austronesian habitat. On the other hand, it would be in accordance with the spirit of the so-called *Lateral Theory*, which holds that the same ancient relicts of an original language can be found at the extremes of its area of expansion. At any rate, a possible relic of the use of the Harappan or a Harappan-derived script by an Austronesian population does not imply (though it leaves open the possibility) that the Harappan script must have been originally devised to render an Austronesian language. For the rest, the Easter Island debate is a sideshow without consequence for the decipherment of the Harappan script itself.

4. S.R. Rao's method

One of the latest decipherment attempts, and partially a convincing one, is Prof. S.R. Rao's Sanskritic reading, explained and illustrated in a lengthy chapter of his book *Dawn and Devolution of the Indus Civilisation* (Aditya Prakashan, Delhi 1992).

Convincing in the sense that it passes several tests which other proposed decipherments have not only not passed, but which they had implied to be impossible. It has ultimately proved to be another failed attempt, yet its methodology calls for a revised version of the same, not for a blanket rejection.

Asko Parpola and his followers cannot help it that no Harappan Rosetta stone is available, but that fact nonetheless makes the task they have set themselves extra difficult.

Of course, their starting-point may be right: it is quite possible that a script could develop without being modelled on other scripts, so that we will have to decipher it in complete isolation, without any helpful clues from related scripts. For another independent creation of a script: it seems obvious that the Meso-American scripts evolved independently from those of the Old World. Still, it would be a happy circumstance if a control were possible.

S.R. Rao's hypothesis seems to provide a double check: on the one hand, the language written on the Indus seals is a language roughly known to us (certainly better than a hypothetical North-Proto-Dravidian to be constructed from Old South-Dravidian of the Sangam era, at a distance of more than two millennia and a thousand miles), and on the other, it uses signs of which many are known from another place as well as from another time. The language is a dialect of Sanskrit, the script largely similar to the Semitic alphabets that appear around 1600 BC and to the Brahmi script attested since about 400 BC. Rao does not assume relations with Semitic and Brahmi scripts beforehand, but in developing his proposed reading of the Indus script he does end up discovering a good reason for postulating such relations.

Rao notices first of all that as the Indus civilization matures, the shapes of the signs do not change much, but strictly figurative signs tend to disappear, leaving a much more uniform set of much fewer cursive signs. These were characterized by ligaturing of individual signs into compounds (some of which look deceptively like figurative signs), and by accenting: small diacritic signs are added to the simple or compound graphs, like the vowel signs on the Devanagari syllabic compounds of consonant signs. It shares these characteristics with the later Brahmi script, out of which the modern Indian alphabets were developed, and with the Semitic alphabets in their advanced stage, when vowel marks were added for use in contexts where exact pronunciation was deemed of utmost importance, e.g. the Bible and later the Qur'an. These characteristics indicate that the mature Indus script was a phonological and in fact an alphabetic (rather than a picto- or logographic) script.

In a sense, the Devanagari and related scripts are syllabic, welding the markers of the sounds of a syllable into a single visual unit. Yet, the syllable-signs for *ka*, *ke*, *ki*, *ko* etc. all have the *k* sign in common, which is the defining feature of the alphabetic as opposed to the syllabic scripts. According to Rao, there were 24 basic phonetic signs (some of which had alternative forms), but 28 ideographs continued to be used, just like

Japanese combines Chinese ideographs with alphabetic writing. In Rao's reading, the diacritic marks have an identifiable vowel value.

As many as 17 of the 24 cursive signs are in common with the Semitic (Ugaritic and Phoenician, more remotely Hebrew and Arabic) alphabets, which are attested since the mid-2nd millennium BC. Admittedly, many fruitless attempts at deciphering have been made for many ancient languages on the basis of visual similarity with the signs of known scripts, and linguists tend to dismiss this approach, e.g. Asko Parpola: "One mistaken method is so common that it deserves special mention. The Indus signs have been equated with similar looking symbols in other scripts and read with their phonetic values. A comparison of signs that look alike in different pictographic scripts will demonstrate, however, that the phonetic values are different, since the different scripts for the most part reflect different languages." (Asko Parpola: "The Indus Script: a Challenging Puzzle", *World Archaeology* 17 (3): p.399-419, specifically p.407; quoted in Benille Priyanka: "Decipherment of the Indus Script: a New Attempt", in B.U. Nayak & N.C. Ghosh: *New Trends in Indian Art and Archaeology*, p.123-131) The remark is valid for pictographic scripts, which are not meant to render sound anyway, but is not equally relevant for phonological scripts.

It is quite logical, if you want to decipher the Runic or the Cyrillic script, to start by noting the similarities with the Latin c.q. Greek alphabets. To be sure, there is a danger of being misled by "false friends" (e.g. Cyrillic C and P do not correspond to the Latin C and P, but to S and R), but even so, it takes only an ordinary amount of skill to figure out the values of most Runic or Cyrillic signs through this approach. Nothing in Parpola's tested methodology would allow you to decipher Cyrillic faster than you could do with the common-sense approach of applying the Greek sound values to the visually most similar letters, on the correct assumption that both have a common origin (in this case, Cyrillic being a Slavic adaptation of Greek). So, there is nothing wrong with starting the decipherment of the Harappan signs by experimentally assigning to them the values of the corresponding Semitic signs. If this does not yield any sound combinations which make sense in any known language, then some other approach has to be tried; but in this case it has seemed to Rao that, yes, this approach yields a reading that makes sense.

5. Problems with S.R. Rao's reading

Unfortunately, at the end of the day, Rao's reading is not satisfying, for it is troubled by a few false but also by a few real problems. In his reading, the Harappan script seems not to contain aspirates (which are rendered by the corresponding non-aspirate consonant combined with the sign for /h/) or cerebrals, just like Avestan. This could be explained, e.g. because the perception of phonetic distinctions was not yet mature, or the development of the cerebral consonants in Sanskrit was a later development (retro-actively applied to Vedic hymns). Then again, some of the words found seem to show a development in the direction of Avestan (*hapta* instead of *sapta*),

and some terminology also points to an Avestan element (*atar* alongside *agni* for "fire", *asha* rather than *satya* for "truth").

It could be argued that the script was a phonetic script in its infancy, or a script borrowed from another language: both are possible explanations for the fact that the phonetic readings, while unmistakably resembling known Sanskrit words and names, do not represent the details of Indo-Aryan phonology accurately. This brings to mind the discrepancy between Greek phonology and the Linear-B script, obviously a foreign-originated script which had difficulties in representing some of the Greek consonants and consonant clusters; or the clumsy way in which e.g. Japanese represents the sounds of English loanwords. The alternative explanation is that the Indo-Aryans were only making their first attempts at phonological representation, and that it was to take some more centuries before the famous grammarians could cast Indo-Aryan phonology into its classical mould as represented by the elaborate and systematic Brahmi and Devanagari alphabets.

The latter explanation is favoured by the apparent rootedness of the phonetic letter convention in pictographic script: e.g. an arrow-shaped sign pictographically representing the word *shara*, and acrophonically (i.e. expressing the initial sound of the depicted word) acquiring the sound value /sh/; or the picture of a field, Sanskrit *kshetra*, representing the combination /ksh/; that of a hill, *giri*, representing /g/; that of a fish, *matsya*, representing /m/; that of a hand, *hasta*, representing /h/; that of an umbrella, *chhatra*, representing /ch/. The chance is small that in Dravidian or another language, the word for "arrow" would also start with /sh/, the word for "field" also with /ksh/, etc.

Another problem with Rao's theory: if the Semitic alphabet was adapted from the Indus alphabet (a scenario which is in itself quite plausible given the existing trade contacts), then its origins cannot lie elsewhere, in the derivation from another earlier script.

Yet, some theories have been proposed suggesting a link with Egyptian hieroglyphics or other ancient scripts. The derivation of the Sinaitic alphabet from hieroglyphics has been attempted by a number of scholars (e.g. Joseph Bouuaert: *Petite Histoire des Alphabets*, Brussels 1949, ch.3). None of these attempts has proven to be wholly convincing, but still it requires a mental leap to accept that a literate society in contact with a variety of scripts could not adapt one of these, and had to borrow the alphabetic principle from such a distant culture.

In Egypt the alphabetic principle had been known earlier, though the preference for the cumbersome hieroglyphics (allegedly by a "jealous" class of scribes) had kept it out of general use. This can be compared with Chinese, where an alphabet of 37 letters, *zhuyinfuhao*, exists for phonetic (esp. didactic) purposes alongside the character script which is the one in general use, and which is not marred by the problem of homophones (two homophone words of different meaning being rendered by two different characters). The Egyptian letters do not correspond with the Semitic ones, e.g. Semitic *'aleph* ('a)

means and shows (in simplified design) a bull, and *dalet* (d) a door, while Egyptian represents the ('a) sound as an eagle, the (d) as a hand. Since anyone knowing hieroglyphs would also know this phonetic script, someone devising a Semitic alphabet on the basis of the Egyptian example would most likely base it on the alphabet rather than on the hieroglyphs; but clearly, the Semitic alphabet is not based on the Egyptian one, so chances remain intact that it was inspired on a wholly different foreign model. Such as a Harappan alphabet. The fact that not the metropolitan cultures but a peripheral traders' community on the Levantine coast was the first to "invent" the alphabet, may also indicate foreign origins.

An argument against the derivation of the Semitic alphabetic signs from the Harappan signs is the evident presence of the afore-mentioned *acrophonic* principle in the design of the Semitic alphabet: the /'a/ phoneme is depicted as, and given the name of, an entity whose name starts with that sound in Semitic, viz. a bull, '*aleph*. Similarly *bet*, "tent, house", for /b/; *gimel*, "camel", for /g/; *dalet*, "door", for /d/, etc. From the correspondence between the shape of the letter with a name starting with that letter *in Semitic*, it would seem to follow that the letter-shapes were invented by Semitic-speaking people, and not borrowed from the remarkably similar-looking Harappan script. Yet, this is not necessarily so: a language has many words starting with a given letter, and there would always be one of them which visually resembles any given letter-shape in original or slightly adapted form (witness the acrophonic letter-names in Runic, unrelated to the Phoenician originals). Many of the Semitic letters actually bear only a vague resemblance to the entity whose name they have borrowed.

For another problem: if the script is alphabetic, why does it look so very pictographic? Many of these seeming pictographs, which have led to such colourful and profound interpretations (like the famous figures of a man carrying a bow, or a man carrying a yoke with two baskets, or a man with a ritual head ornament resembling a deer's antlers), are analyzed by Rao as combinations of alphabetic signs. Thus, the man with antlers and a yoke with two vessels can be analyzed as: the antlers = *â*, one vessel = *pa*, the other vessel = *pa*, the man sign = *ra*, together *âpa-pa-ra*, "supreme (lord) of the waters".

Rao himself explains: "The Indus writing is a mixed one in the sense that pictures of birds, scorpion, dog, goat, pipal leaf, grassy plant, bee, ant, three-peaked hill, horn of animal and a few schematized pictures like 'man', 'fish', 'hand' and 'fence' appear side by side with cursive signs, some of which bear resemblance to Brâhmî and Roman characters.

Besides true pictures and cursive signs, there are some linear signs and 'pseudo-pictures'.

The latter look like pictures but are, in reality, compound signs formed by joining two or more cursive signs. Quite often, short lines (diacritics) are attached to pseudo-pictures and sometimes two identical basic cursive signs are joined to form a compound sign.

These and other pseudo-pictures are often mistaken by decipherers for pictures of 'archer', 'bowman', 'soldier holding shield', 'coolie carrying load' or 'praying man', but they are compound signs. When analyzed, the components are found to be basic signs which appear independently in other inscriptions. Two or three basic signs are intelligently joined to form picture-like *samyukta aksharas* (conjunct consonants) and syllables. When phonetic value is given, the words formed by them are meaningful." (S.R. Rao: "Writing, Language and Religion of the Harappans and Indo-Aryans", in B.U. Nayak & N.C. Ghosh: *New Trends in Indian Art and Archaeology*, pp.201-237, specifically p.201) I think this general approach remains valid, though his readings of each letter individually are not all tenable.

Moreover, these signs were not written in a fixed order on a line, but were combined in a somewhat artful way, so as to look like pictures. If this seems strange, one may recall that in Urdu calligraphy, such type of fanciful writing variations which show flowers, birds or minarets is also practised. After all, the Indus seals were not just prosaic (though often secular) messages, but seals: short but official messages and names, a typical occasion for the use of calligraphy with its peculiar conventions. That is why Prof. Banka Behari Chakravorty calls the Indus script "the artistic version of Brahmi". (B.B. Chakravorty: *Indus Script -- the Artistic Version of Brahmi*, Calcutta 1991). Note, however, that his Brahmi-based decipherment overlaps only partially with Rao's.

Further, if we accept Rao's phonetic interpretation of the Harappan signs, we find that they often behave rather strangely. Thus, the sign which Rao reads as /a/ often appears after a consonant, which it would never do in Brahmi or Devanagari, where vowels are represented as diacritical marks on the preceding consonant. Considering the possible link with Semitic, this oddity might be positive evidence in disguise, for the same type of *scriptio plena* of otherwise unwritten vowels also occurs in Semitic. Did Harappan pre-Brahmi have these so-called *mater lectionis* vowels too? The same sign is read by Benille Priyanka as *h*, in particular the *visarga-H* common in Sanskrit noun endings, which provides another orthographic parallel with Semitic: like this Sanskrit *aH/iH* ending, the *-ah* ending of Semitic feminine nouns (with *hé* as *mater lectionis* effectively pronounced vocally as *-a*) has a weak consonant which is replaced with a stronger consonant in certain phonetic contexts (in certain *sandhi* contexts, Sanskrit final *-H* becomes *r*, Semitic *-ah* becomes *-at*).

A similar reference to Semitic might take care of the unexpected appearance of reduplicated consonants in places where Prakrit or Sanskrit doesn't need them. How could a Sanskrit word start with *ppa-*? In Semitic, as in Tamil, reduplication (typographically indicated not by actual reduplication of the alphabetic sign, but by addition of a reduplication sign, in Hebrew the inscribed dot *daghesh*, in Arabic the *w*-shaped superscript *shadda*) often indicates a phonetic modification, viz. to "harden" the consonant, whether from voiced to unvoiced (in Tamil) or from fricative to plosive (*f/v/x > p/b/k* etc., in Semitic; the sign *x* is used to represent the fricative-velar sound heard in Scottish *lo-ch*, in German *a-ch-t*, in Persian/Urdu *xân/Khan* or *xûb*, "good", as in *xûbsûrat*, "fair-faced", or in Arabic *xilâfah/Caliphate*). Could it have served in Harappan

to aspirate or otherwise modify the consonant? Perhaps it is merely the result of reading the signs in the wrong order: in newer versions of his decipherment, Rao tends to rearrange the order of the consonants. (Compare Rao's earlier publications with e.g. S.R. Rao & N.S. Rajaram: "Vedic and Harappan Societies", 1995, proposed to *Scientific American* for publication but never published) We can leave these questions open, but must insist that the enthusiasts of Rao's decipherment ought to take a hard look into them, for its credibility is seriously marred by such oddities.

Also, S.R. Rao weakens his case by displaying his own unfamiliarity with IE linguistics. Thus, he opines that the Harappan language had only three cases, while PIE, Avestan and classical Sanskrit had eight (S.R. Rao: *Dawn and Devolution*, p.268). If it is true that only three cases have been attested on the Indus seals, this could easily be explained by the fact that the seals do not contain literary texts, hardly even normal sentences, but merely "this object belongs to X", or "is dedicated to god Y" or "this merchandise was sent on date Z"; most of them seem to be mere names. Prof. Chakravorty (*op.cit.*, p.18) has argued that most of the Indus seals, or what he calls *mudrâs*, served as a kind of entry passports in the walled cities; the figurative pictures accompanying the short texts are to be compared with the ornamentation in ex-libris and other markers of ownership; though they are embedded in the Indus culture and hence often refer to Vedic motifs (e.g. "The unicorn found in so many *mudrâ*-s is but *Kurînin* or 'crested goat' mentioned in the Atharva Veda"), this does not imply that they had a talismanic or otherwise religious purpose (*op.cit.*, p.28). In these short notes, the full case system would not be needed, much less the full verbal conjugation system. Moreover, in this type of short inscription, one should expect the use of abbreviations, a far more ancient device than one might think. At any rate, it makes no sense to postulate a three-case Indo-Aryan language behind the Harappan script, when all the Indo-Aryan languages either maintained a full IE case system or lost most of the cases only at a later date.

It is not clear to us why Prof. Rao introduces sounds which do not exist in Sanskrit (emphatic or laryngeal *H*, which sometimes appears at the beginning of a word, as opposed to the Sanskrit *visarga-H* which never does; and the diphthong *ao*), while leaving existing sounds unrepresented. Thus, in one of his lists of deciphered letter combinations, he gives as transcription of a word, *aoshâ*; would *ushâ* ("bright", also name of the Dawn goddess) not have been more appropriate? (Rao: "Writing, Language and Religion of the Harappans and Indo-Aryans", p.219)

To me, the decisive argument against Rao's decipherment is that it is not convincing semantically. The readings seem contrived and often bombastic, as do most of the proposed readings based on Dravidian.

It may seem strange that Rao's decipherment has never been analysed and eventually corrected and reformulated by an accomplished linguist. Science progresses because new hypotheses of one researcher are tested independently and repeatedly by his colleagues.

6. Acceptance of S.R. Rao's decipherment

According to W.W. De Grummond, a Classics professor from the USA, "Dr. Rao's decipherment of the Indus script has met with considerable acceptance and will serve now as a basis for further and continuing study of the language of the ancient Indus Valley civilization." (W.W. De Grummond: "Linguistic Affinities of Old Indo-Aryan with Classical Greek and Latin", in B.U. Nayak & N.C. Ghosh: *New Trends in Indian Art and Archaeology*, pp.133-139, esp. p.133.) Prof. Rao himself reports with some pride that his reading of the Indus script is being accepted by scholars and institutes in the West. (See the acknowledgements in S.R. Rao: *Decipherment of the Indus Script*, p.xiv-xv; or the list in Talageri: *The Aryan Invasion Theory, a Reappraisal*, Aditya Prakashan, Delhi 1993, p.61-62)

Given the ease with which all Indian contenders in the debate claim to have the world on their side, and given the widespread suspicion among Westerners that Indians (starting with all those travelling babas selling instant enlightenment) tend to be cheats, Prof. Rao must have some patience with skeptics who will only believe him when they personally hear these commentators repeat their approval. Moreover, what does it prove that some president of the Epigraphical Society of India accepts Rao's theory, when so many academics of equal rank accept the Dravidianist theory? Perhaps these scholars simply accept Rao's theory without studying it because they cannot countenance the idea that the great Prof. Rao, one of the most successful archaeological researchers of our age, could risk his reputation with a mistaken theory (as if great scholars never make mistakes, particularly when they venture outside of their strict field of competence, as Rao does)? Or perhaps they are simply being polite?

Thus, in his article "Writing, Language and Religion of the Harappans and Indo-Aryans", Prof. Rao reproduces a letter from Dr. David Diringer, founder-director of the Alphabet Museum in Tel Aviv: "Very sincerely I have to congratulate you on the decipherment of the Indus script (...)" (in B.U. Nayak & N.C. Ghosh: *New Trends in Indian Art and Archaeology*, p.227) Fine, but we would have preferred to read something more scholarly from such an authority, rather than this piece of politeness. The article is published in Prof. Rao's 70th birthday felicitation volume: over 500 pages by dozens of contributors. How is it possible that the editors have not put any of their graduate students to work on reviewing Prof. Rao's decipherment, on testing his decipherment on newly discovered seal inscriptions, on surveying its reception by the academic community?

Consider the oddity of the situation. The Indus script has been an enigma for decades; suddenly it is deciphered; the result (Indo-Aryan rather than the expected Dravidian) makes a far-reaching revision of ancient history necessary; and yet, this has not become a cover-story in any scholarly journal, this has not been made the topic of an extraordinary international symposium, and in fact many indologists we personally know have not even heard of it. Compare: not more than a handful of mathematicians understand the problems involved in proving Fermat's theorem; but when recently the

proof was finally found, it was given coverage in the TV news programmes worldwide. Apparently the Indo-Aryan Harappa proponents are very smug about Rao's findings, and insufficiently aware that the scientific community only accepts theories which have been tested by researchers independent from the original proponent. Even Rao's fans and all those Indians who applaud his decipherment have not moved a finger to strengthen his case by adding some research of their own along the same lines.

The very fact that many people working on ancient Indian history can continue their business as usual without being disturbed by the revolutionary implications of Prof. Rao's decipherment, could be (and is being) read as a sign that there is something fishy about this sensational breakthrough. For a really convincing testimony, even sympathizing scholars would first like to see the champions of a non-Sanskritic decipherment climb down from their positions and accept Rao's reading openly. As long as the authorities who linked their names to established theories are not willing to concede defeat, most non-insiders will have the healthy conservative tendency to rally around them rather than around the innovators.

On the other hand, the stubborn rejection by these authorities would look more convincing if at least they spoke out about the new theories which challenge their own. It is in itself amazing, but in the context of the AIT debate rather characteristic, that so little debate has taken place between the Sanskritist and the Dravidianist schools. The burden of proof is shifting towards the Dravidian hypothesis, which has not yet yielded any convincing results. And its advocates are not reacting very vigorously. In his last book on the decipherment problem, *The Harappan Civilization and its Writing. A Model for the Decipherment of the Indus Script*, published in 1992, years after S.R. Rao's proposed decipherment was made known, Walter A. Fairervis jr. elaborates a lot on the work of his fellow Dravidianists but ignores Rao's proposals completely. Many writers display the same total disregard for the non-Dravidianist approaches, e.g. in a research paper, Clyde Ahmad Winters starts out with the confident declaration that the Dravidian reading has been fully established as correct ("The Harappan Writing of the Copper Tablets", *Journal of Indian History*, Trivandrum, vol.LXII, 1984, p.1-6).

Asko Parpola dismisses the Indo-Aryan Harappa school curtly: "It is now common knowledge that Brahmi (first attested in Asoka's edicts c.250 BC) is derived from the Semitic consonantal alphabet, which in turn is derived from Egyptian hieroglyphics. And yet some researchers still insist on deriving the Brahmi and with it the modern Indian scripts from the Indus script." (Parpola: "The Indus Script: a Challenging Puzzle", p.407) Remark that the claimed *consensus* about Brahmi's derivation from a Semitic script and the latter's from Egyptian does not exist: the direction of borrowing in the Semitic-Brahmi relation and the link between the Egyptian and Semitic scripts are both open questions. Unfortunately, scholars tend to cite a colleague's mere *hypothesis* as an argument of authority. So many certainties in this field are merely someone's half-serious *opinion* quoted over and over again. But no further discussion about the rights and wrongs of these attempts is given. In his magnum opus, *Deciphering the Indus Script*, Parpola mentions Rao in an archaeological context, and Rao's *Dawn and*

Devolution of the Indus Civilisation (which includes the decipherment) figures in the bibliography, but not one word is spent on Rao's or anyone else's Sanskrit-oriented decipherment.

This is but a special case of a more general tendency. Two recent Indian publications specifically intended as contributions to "the current debate on whether or not the Aryans were the indigenous inhabitants of India", i.e. to counter the rising tide of anti-AIT arguments, manage to leave unmentioned each and every recent publication presenting evidence against the AIT, including Prof. Rao's proposed decipherment. Reference is to S.K. Biswas: *Autochthon of India and the Aryan Invasion* (1995, which claims to be written in the spirit of Dr. Ambedkar, though he had argued elaborately *against* the AIT); and R.S. Sharma: *Looking for the Aryans* (1996).

7. Scholarly amendments to S.R. Rao's reading

Among the all too brief scholarly reactions to Prof. Rao's decipherment, a few are interesting because they share his general approach while not going along with him into the details of his decipherment. John E. Mitchiner, after dismissing some fanciful Indian attempts at decipherment, mentions that "a more soundly-based but still greatly subjective and unconvincing attempt to discern an Indo-European basis in the script has been that of Rao". (J.E. Mitchiner: *Studies in the Indus Valley Inscriptions*, p.5, with reference to S.R. Rao: *Lothal and the Indus Civilisation* (ch.10), Bombay 1973.) But here, reference is to Rao's earliest attempt at decipherment, written 20 years before the detailed decipherment in his *Dawn and Devolution of the Indus Civilization* (in which Mitchiner's feedback has been taken into account). For the rest, Mitchiner is on the same trail as Rao: in his opinion, it would be "unwise to exclude the possibility of a form of 'Proto-Indoaryan' language as being enciphered in the Indus inscriptions", and acknowledging that earlier scholars had "advocated a continuity between the Indus and Brahmi scripts", he notes that "many of the Indus signs are very closely similar to Brahmi signs", and that both Ashokan Brahmi and the Harappan seal inscription sometimes use the *boustrophedon* writing-style (leftwardly written lines alternating with rightwardly written ones). (Mitchiner: *op.cit.*, p.11.)

Surveying Prof. B.B. Lal's study of inscriptions on pottery and megaliths, Mitchiner cites the following figure: "89% of the Megalithic signs and symbols which appear on pottery down to the 9th century BC or thereabouts may be traced to Harappan and post-Harappan signs and symbols". Since "the period dealt with spans virtually the entire millennium between the downfall of the Indus Civilization (c. 19th century BC) and the rise of the later Gangetic civilization (c.9th century BC)", a "direct continuity between the two is thereby implied; and this is suggested also by the many signs and symbols which recur between the Indus seals and the later punch-marked coinage". (Mitchiner: *op.cit.*, p.12, with reference to B.B. Lal: "From the Megalithic to the Harappan", *Ancient India* 1960, esp. p.21-24.)

This, then, seems to be a point on which serious research is increasingly converging: there is a direct continuity from the Harappan script through the sparse remains of inscriptions in the so-called "dark age", and down to Brahmi. Brahmi would thus not be a daughter (but possibly a sister) of the Phoenician alphabet, as was hitherto assumed. That Brahmi was adopted from the "ancient Sindh-Panjab script of the non-Aryans" rather than from foreign sources had already been suggested in 1960, within the AIT framework, by

S.K. Chatterji (*Indo-Aryan and Hindi*, p.52-54, quoted by Madhav M. Deshpande in: "Genesis of Rgvedic Retroflexion. A Historical and Sociolinguistic Investigation", in M.M. Deshpande and Peter Edwin Hook: *Aryan and Non-Aryan in India*, p.302.)

Several other findings confirm this continuity. As Mitchiner notes, it had been observed soon after the discovery of the Indus cities that the signs on the Indus seals "show virtually no evolution whatever throughout the centuries of their usage in the Indus civilization", while "from the inception of the punch-marked coinage around 600 BC down to its later form around AD 300 -- nearly a millennium later -- there is a remarkable lack of evolution or change" (that fabled or notorious conservative trait in the Hindu character), so that "it would seem reasonably likely that these signs and symbols which recur between the Indus and later Indian civilizations demonstrate a further continuity of culture between the two". (Mitchiner: *op.cit.*, p.12-13, with reference to J. Allan: *Catalogue of the Coins of Ancient India*, London 1936, pp.clix-clxiii.)

Moreover, Indian seals from around the turn of the Christian era, bearing inscriptions in Brahmi script, present the same types and visual make-up as those from the Harappan period: "Such later seals frequently portray an animal-figure, above which appears the inscriptional legend -- just as in the case of the Indus seals. Two main types of seal-impressions may be found: one was attached to parcels and letters, and shows stringmarks at the back; while the other was used more as a kind of token, and generally has a hole at the back by which it may be suspended. Once again, precisely the same two types are to be found among Indus seals." (Mitchiner: *op.cit.*, p.13, with reference to K.K. Thapliyal: *Studies in Ancient Indian Seals*, Lucknow 1972.)

From various angles, Mitchiner tries to decipher specific items in the Harappan seal corpus. His conclusion: "We have now reached a stage where it is possible to conclude that the language of the Indus inscriptions may very well be an early form of Indo-Aryan. In this event, it can be seen [from our analysis of sign-groups] that certain forms of this language have been preserved only in the Prakrit branch of Indo-Aryan -- notably those which predominate in the inscriptions at Mohenjo Daro; while certain other forms have been preserved only in the Sanskrit branch of Indo-Aryan -- notably those which predominate in the inscriptions at Harappa.(...) In the first place, we have concluded that the inscriptions contain the names of towns and regions, both within and beyond the Indus Valley: such names denoting the places from which and to which certain items of merchandise are being conveyed. In the second place, we have concluded that the language used in the inscriptions is an early form of Indo-Aryan."

(Mitchiner: *op.cit.*, p.77-78.) Mitchiner's reading of particular signs partly overlaps with Rao's. The details will have to be worked out between the competing schools of the Indo-Aryan interpretation, but at least the essence of Rao's position is being supported here by an outsider above suspicion.

Subhash Kak has applied the best instruments created by modern science to compare the Brahmi and Harappan scripts. He concludes: "My analysis of Indus and Brahmi based on computer-created concordances, revealed obvious connections between the two scripts that could not be explained as arising out of chance. Such an analysis is possible since letters in a script occur with different probabilities. (...) My analysis showed that the most frequent letters of Indus and Brahmi looked almost identical, and besides they were in the same order of frequency.(...) Briefly the connections between Indus and Brahmi scripts are as follows. Both scripts use conjuncts where signs are combined to represent compound vowels. The core set of most frequent Indus signs seems to have survived without much change in shape into Brahmi where it corresponds to the most frequent sounds of Sanskrit. The writing of numerals in Indus, especially the signs for 5 and 10, appears to have carried over to Brahmi." (S. Kak: "The Indus Mandala and the Indo-Aryans", Nayak & Ghosh: *New Trends*, p.141-156, specifically p.149-150.)

8. Reading the Pashupati seal

The Sri Lankan scholar Benille Priyanka takes a fresh look at the decipherment of Harappan on the basis of the similarities with Brahmi. (B. Priyanka: "Decipherment of Indus Script: A new Attempt", in Nayak & Ghosh: *New Trends*, p.123-132.) Thus, the inscription on a famous seal showing a ram with a man's face, along with a worshipper before a tree-god, above a row of seven human beings, is read as *ma-me-sha-ha-X* (X being an apparently non-Brahmi-related, man-shaped character which is left unread, Priyanka being more averse to speculation than most). It so happens that *meshaH* means "ram", a splendidly appropriate reference to the picture on the same seal. On the famous Pashupati seal, the text would read: *X-ma-ma-hi-sha-ha* (X representing the same man-shaped character). (B. Priyanka: *op.cit.*, p.127) One of the animals depicted is a buffalo, Sanskrit *mahishaH*. It seems that the picture and the inscription on a seal were often, and quite sensibly, correlated.

However, it is to be noted that this Brahmi-based reading overlaps only partially with Rao's Phoenician-based decipherment, which reads the same inscription as *ra-ma-trida-ao-sha-â*, Sanskritized as *rama-tridhâ-oshâ*, "pleasant and shining in three ways". (Rao: *Dawn and Devolution*, p.288; and "Writing, Language and Religion of the Harappans and Indo-Aryans", in Nayak & Ghosh: *New Trends*, p.234.) Of six signs, only the second and fifth are read identically; the fourth and sixth are read by Priyanka as *h* + vowel, by Rao as *scriptio plena* vowels; the first sign is not interpreted by Priyanka, and as *ra* by Rao; while the third, which resembles the second, is read by Priyanka as *ma*, like the second, but by Rao as a pictograph representing *trida*.

It is already known what N. Jha and N.S. Rajaram have made of the Pashupati seal text. They read it in the opposite sense, but with some identical readings of signs, as *î-sha-d ya-tta mâ-râ*, "Mara tamed by Isha". Note that *sha*, *ra* and *ma* are identical in their and in Rao's reading. Likewise the vowel, initial *î* in the Jha/Rajaram reading and final *â* in Rao's, is one letter: like Semitic *aleph*, the initial-vowel sign is unspecified as to which vowel sound it indicates. In the Jha/Rajaram reading the vowel sign can represent all vowel sounds, sometimes specified with diacritics, just like *aleph*, and just as the initial-/a/ sign in Devanagari can indicate *a*, *â*, *o*, *au* (and in Marathi also *e*, *ai*) depending on the diacritic added. Incidentally, I think the reading by Jha and Rajaram has the semantics in its favour: unlike Rao's, its meaning is not bombastic (a trait of most of the failed attempts, Dravidian or Sanskritic) and it fits the accompanying picture: Isha is Shiva is Pashupati, Mara is the spirit of the animal level in man's psyche (as when he tried to lead the Buddha astray from his meditation), and the wildlife is indeed depicted as obeying (rather than threatening) the Pashupati figure.

The example given is the one with the greatest discrepancy between the Brahmic-based and the Phoenician-based reading of the Indus script which we have encountered; still the two have a lot in common. We draw attention to the discrepancy because we would like to warn the Indian scholars who correctly feel that they are on the right track, against the impression that the job has been completed. The case for a continuity between Harappan and Brahmi is fairly strong, as is the case for kinship between both these and the Phoenician script. But a number of details remain to be filled in.

9. Writing in the Vedas?

If the Harappan script renders Sanskrit texts, we have to take a fresh look at the question whether writing was known in the Vedic age. Writing is at any rate older than was thought until recently. Given the rarity of textual remains in Harappa in proportion to what we may reasonably assume to have been the literary output of this vast and advanced civilization, it is not far-fetched to suppose that the oldest remains of Harappan writing were preceded by centuries of writing which have left no remains, and which take the starting-date of Harappan writing to nearly the era of the very invention of writing (ca. 3500 BC for Sumeria and Egypt).

Then again, the date is moved back ever further in the case of non-Indian cultures, esp. in the Old-European culture of the Balkans in the 6th millennium BC. About this ancientmost known (but as yet undeciphered) script, Harald Haarmann writes: "Regarding the nature of the script, the general impression is that it is linear. The Old European script shares this feature with the Cretan writing systems Linear A and B, and with the Indus script. Arguably, most typological similarities can be established between the Old European script and the latter. (...) the internal structuring of the sign inventory is very similar. There is a smaller number of ideographic symbols versus a large corpus of abstract arbitrary signs. The latter category of signs is characterized, in both scripts, by

the frequent duality of a basic shape which is modified by means of additional graphic elements

(e.g. strokes or dots) to form a separate symbol." (H. Haarmann: "Writing from Old Europe to Ancient Crete -- a Case of Cultural Continuity", *Journal of Indo-European Studies*, fall 1989, p.251-276, specifically p.262.) This is the principle which is followed till today in the Devanagari script and all the other Brahmi-derived scripts to form syllabic signs on the basis of consonant signs and vowel markers.

Haarmann continues: "This kind of linearity of the Old European script contrasts with later versions of writing systems in the ancient Orient (Sumerian cuneiform, hieroglyphic Hittite) or in Egypt (hieroglyphic)." It is a mystery which we cannot hope to solve right here, but let us at least note the fact: the scripts of Egypt and Western Asia belong to one tradition (soon becoming two: pictographic and cuneiform-syllabic), those of the Old European Balkan culture and of Harappa belong to another. Is it due to a migration of Mediterraneans to India, as the Dravido-Mediterranean hypothesis of Father Heras held?

Or was there a transmission from India through Central Asia and Ukraine (i.e. avoiding West Asia, where scripts from the 4th millennia would reasonably have had to show traces of this transmission)?

The latter would fit the Indocentric scenario of IE migrations from India through the Kurgan area, except for a prohibitively serious objection: the Kurgan and other presumably IE cultures in Central Asia and East Europe do not show a trace of such a script, and it is with their expansion into the Balkans that the Old European linear script *disappeared*. The disappearance of the Vinca script is used as a strong argument against Colin Renfrew's thesis that the first agricultural society, of whose expansion the Vinca culture was a part, was IE-speaking (just as the disappearance of the Harappan script would have been an argument against continuity with the "subsequent" Indo-Aryan culture). (Harald Haarmann: "Pre-Indo-European Writing in Old Europe as a Challenge to the Indo-European Intruders", *Indogermanische Forschungen* 1991, p.1-8.) Except in case of a profound catastrophe, it is unreasonable to expect the loss of a useful invention like the script.

The fact that IE was spread by illiterate tribes implies that the PIE culture in the Urheimat was not yet literate; or that it was in the first stages of literacy, which had not yet reached those adventurous peripheral tribes which were to realize the expansion of IE. Within the Indocentric theory, the metropolitan culture was already literate at the time of the Rg Veda, but the wayward tribes in Afghanistan were not, or had already left to form the second centre of IE expansion in South Russia.

But when did Vedic culture become literate? Before, during or after the period of composition of the Rg-Veda? The dominant version of the Indocentric theory posits a synchronism between the Harappan cities and the Brahmana-Sutra period, which throws

the Rg-Veda back to before Harappan urbanization, from which period little or no writing has been preserved. This leaves open both possibilities for Rg-Vedic writing.

Many indologists continue to assume that the Vedas were composed by an illiterate nation. The well-known practice of committing the Vedas to memory and training Brahmins' sons from early childhood onwards in reciting them by heart, has been interpreted as proof that the Vedic writers needed an alternative way of preserving the hymns in the absence of writing. Yet, the practice in other cultures suggests otherwise. In the Middle Ages, Chinese children were made to recite the Confucian Classics by heart; yet, there is no doubt that the Classics existed in writing. In fact, the practice of learning them by heart was only started many decades after their publication in writing.

Similarly, the ancient Greeks of some social standing taught their children Homer's epics by heart. Yet, we know for fact that Homer's epics were propagated all over the Greek world in written form. Homer himself has even been credited by some scholars with reforming the Phoenician-based alphabet to create the classical Greek alphabet. (In the Phoenician alphabet, as still in the Hebrew and Arabic alphabets, the long vowels /a/, /i/ and /u/ are written as separate letters, as *mater lectionis*, but this was only due to the fact that these letters also existed as consonants: *aleph* as the glottal sound before an initial vowel, *yod* as /y/, *vav* as /w/. But otherwise, vowels are either not written at all, or, in elementary schoolbooks and in sacred writings where correct pronunciation is of utmost importance, they were only written as diacritical marks on the preceding consonant; no pure vowels exist in these alphabets. Homer has unverifiably and probably wrongly been credited with the Greek innovation of giving the seven vowels *alpha*, *epsilon*, *eta*, *iota*, *omikron*, *ypsilon*, *omega*, a place in the alphabet on a par with the consonants.) Barry Powell argues, against several critics, in favour of Homer's authorship (or that of "Homer's adapter" to whom Homer personally dictated, one Palamedes) of the vowel system in the Greek alphabet (in: "Homer and the Origin of the Greek Alphabet", *Cambridge Archaeological Journal* 2(1), 1992, p.116-126).

Whatever Homer's personal involvement, experts argue against the notion that his vast epics were composed and transmitted purely by heart: "One solution proposed is that somehow the Homeric poems were preserved by oral means for several generations until later, in the maturity of Greek literate civilization, someone wrote them down. Yet modern research in oral literature suggests that verbatim transmission of monumental poetic compositions by oral means simply does not occur." (Barry Powell: *op.cit.*, p.116.) To be sure, the versification techniques used in the epics are rooted in a tradition of poetry recitation going back to preliterate times; and it is not strictly impossible for a professional story-teller to learn extremely lengthy texts by heart, as is still done by many thousands of people with the Qur'an or with Tulsidas's *Ramcharitmanas*.

The belief that elaborate knowledge was preserved orally, has entered the classical view of ancient history through Plato's lament that the art of writing is a great step backwards and a danger to the human power of memory; that it increases the risk

of knowledge falling into the hands of vulgar people who will misunderstand it or put it to ridicule. We can leave this romanticized view of illiteracy to Plato, who himself enjoyed all the comforts of a well-established tradition of literacy, without which his name would have been consigned to oblivion.

Conversely, another ancient writer, Julius Caesar, testifies of the Druids, the Brahmin class among the Celts, that they spent many years committing to memory the traditional knowledge of their people. But since all this knowledge has been lost (or has escaped falling into the hands of us vulgars, if that is how you want to put it), we can only guess whether this knowledge was in the same standardized form of metred verse as we find in the Vedas, and whether it involved the same advanced proto-scientific knowledge. The fact that not one trace of written Druidic traditions has ever been found, or even been referred to by serious sources, is a *prima facie* argument in favour of Caesar's claim of a purely oral tradition. Yet, fact is that the Celtic people did have an alphabet since at least several centuries before Caesar: with all their preference for oral transmission, they were not above using the script for worldly purposes, and certainly not ignorant of it.

The pre-Islamic Arabs used to assemble at poetry sessions where verses were improvised; but then, the best verses were written down, and the contest-winning verses were put on display at the entrance of the Kaaba. And these improvised poems were typically brief: there is a practical difference between limerick-length poems which can indeed be improvised, and lengthy epic poems which can be memorized with great effort, but which cannot be composed al by heart.

To be sure, in some respects oral learning is superior to learning from a written text.

Thus, one should learn a language from a native speaker, by listening to him and allowing him to make corrections in the learner's pronunciation. But for that, the use of textbooks is not abolished. In learning Vedic recitation, a correct pronunciation was considered of utmost importance; but that does not mean that the Brahmins spurned the memory aid provided by a written rendering of the hymns.

Part of the confusion arose from the fact that the Vedas are called *shruti*, "what has been heard". It was assumed by scholars that this referred to the process of divine revelation, the way Mohammed is supposed to have heard the archangel Gabriel dictate the Qur'an. Through some shamanistic process, the Vedic seers "heard" the Vedic verses, and passed them on in this form. This idea has, unfortunately, been given some currency among modern Hindus due to the well-meaning efforts of the 19th century reform movement Arya Samaj to cast Hinduism in the mould of Islam and mostly of Christianity in order to enhance its respectability. The net effect is that the Vedas, whether "divinely revealed" (as according to the Arya Samaj) or "improvised by illiterate nomadic shepherds" (as according to pop versions of the AIT), have been dissociated

from a normal process of composition by erudite writers applying elaborate rules of verse, a process which presupposes literacy.

The term *shruti* forms a pair with the term *smṛti*, "what has been remembered", which refers to the more down-to-earth post-Vedic literature, esp. the *dharma-sutras*, the treatises on the social order. In spite of what the term might suggest, the *smṛti* literature was not normally "remembered". i.e. learnt by heart, or at least to a smaller extent than the actually religious texts. And in the case of *shruti* too, we are faced with a misunderstanding. The related word *shravah* (Slavic *slav-*, Greek **klew-*) means not just "hearing", but "fame" and "glory", whence the glory-connoting names *Peri-kles*, *Andro-kles*, *Miro-slav*, *Broni-slav* etc., and indeed the ethnic self-designation of the *Slavs*, the "glorious ones". Similarly, *shruti* means something like "what is worth hearing", or "what is heard from afar", i.e. the ancient tradition. The Vedic poets, after all, were not modern narcissistic litterateurs who tried to express their hyper-individual emotions in as original a form as possible; they merely gave a standardized classical form to ancient traditions, to the knowledge and wisdom capital of their tribe. In doing so, they did not spurn the instrument of writing once it became available.

Both theories, of Vedic literacy and illiteracy, should be put to the test: compose a collection of hymns, as intricate and as long as the Rg Veda. One group learns it by heart, the other also learns it by heart but at the same time commits it to writing, and has the freedom to check with the written text to see if no gaps or mistakes have crept in. For a start, one year will suffice as the time period after which the accurate knowledge of the text will be tested. We have little doubt that the "illiterate" group will have great difficulties in preserving for a single year the length of text which they claim the Brahmins preserved for centuries without the help of writing. In fact, the very first part of the test will prove even harder: *composing* such a text without the aid of a rough copy in writing, following all the rules of verse, and committing it to memory at once.

The Vedic people were engaged in international trade; it is possible to conduct this without written records, but not very practical. They also conducted astronomical observations over long periods, and devised methods of geometry and calculus. One of them also collected a thousand Vedic hymns, ordered them into books and numbered them. It is not strictly impossible to do this without knowing the art of writing, but nonetheless almost superhuman. To expect this much of the Vedic people is both flattering and unrealistic.

To advocates of a literate *rishidom*, the issue is clinched by testimony of writing in the Vedas themselves. Apart from indirect evidence and oblique or implicit scriptural references to writing (vide Bhagwan Singh: *The Vedic Harappans*, ch.14) there are Vedic passages which, in a certain interpretation, seem to provide unmistakable direct testimony.

Thus, Atharva Veda 19:72:1 mentions that "after having been recited, the Veda is put back in a chest": apparently, the Veda had been laid down in some material form. In Rg

Veda 3:53:15-16, Vishvamitra speaks of knowledge which makes permanent (as the Romans said: *verba volant, scripta manent*), is visual, is serpentine or cursive, and reincarnates itself (by copying); admittedly, this is a very unusual interpretation not given by the classical commentators. The description of the Vedic *rishis* as "mantra-seers" can be given an occult meaning, along the lines of the "divine revelation" of Quranic verses by the archangel Gabriel, but a more straightforward meaning is that they are *reading* mantras. Their special status then derived from the fact that whereas ordinary people merely saw scribbled lines, the literate *rishis* could "see" the contents of the sentences rendered by those scribbled lines.

Finally, Rg Veda 10:62:7 mentions a cow as *ashta-karni*, in the sense of "having the mark 8 on the ear", obviously referring to the shape of the character, in the same way as modern signboards say: "no U-turns", in the sense of: no manoeuvres taking the form of the character U. The signs of writing sounds as well as figures were so well-known that they had entered this kind of usage (analyzed to this effect in A.A. MacDonnell and A.B. Keith: *Vedic Index*, vol.1, p.46). A slightly later indirect reference to writing is in Aitareya Aranyaka 5:3:3, which also refers to the distinction between vowels and consonants (2:3:1), while the Chandogya Upanishad (2:10:1-4) mentions syllable counts. In the Indocentric theory, these texts would be synchronous with the literate culture of urban Harappa.

Judging by this evidence, and assuming there is any truth in the Puranic relative chronologies and the Vedic astronomical indications, Vedic culture was an incipient literate culture at least from the early 4th millennium onwards. Now that remains of writing have been found in the Old European culture in the Balkans dating back to the 6th millennium, we should not be surprised to find literacy in India at a similarly early date. It is at any rate not far removed in time from the beginning of writing in nearby Sumer.

To be sure, the value of the cited indications of Vedic writing is contested by some scholars, and I am not really convinced either. For now, I cannot pretend to have the definitive answer here. However, there are enough peepholes in the established paradigm for a fresh wind to blow through.