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INDUS SCRIPT AND DRAVIDIAN

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In the last few years renewed efforts have been made to decipher the script used in the seals found in the archaeological remains of the Indus civilization. Several of these attempts, among them that of the Finnish team, have resulted in the proposal that Dravidian is related to the "Indus language".

The aim of our studies was from the very beginning to investigate the possibilities of computer techniques in deciphering an unknown script. Previous attempts at decipherment were therefore purposely overlooked. However, after we found out that the results acieved seem to fit in with facts known or deducible from Dravidian, several reviewers of our reports referred to similar results by previous analysts.

It therefore seems appropriate to examine at least some of the earlier studies in order to see whether and in which way our methods and results conform to or differ from those of other researchers. It is in our opinion neither possible nor necessary to review a l l of the previous attempts. We therefore focus our attention on certain more important studies, especially on ones that have been based on the hypothesis that the language of the Indus script is related to Dravidian.

The publishers of the Mohenjo-daro excavation report already contributed in several ways to the understanding of the nature of the script. In his official account (I p. 39ff.) Marshall himself briefly discusses the possible relationships of the script. He points out that there is certain *prima facie* evidence in favour of the theory that the Brahmi alphabet originated in a pictographic script, as already postulated by Cunningham, but warns against attaching too much weight to outer resemblances of the scripts and especially against assuming that the sound values of both these scripts were the same. Marshall thus seems to polemize the hypothesis advocated by Langdon in the second volume of the report (p. 423ff.). Even the latter admits that the Indus script cannot be transliterated from the Brahmi.

As to the language of the inscriptions Marshall (*l.c.* p. 42) regards Dravidian as the most likely conjecture. The Dravidians were, according to him, the precursors of the Aryans over most of Northern India, and they are also the only people likely to have been in possession of a culture as advanced as the Indus civilization. There are to this day still the Dravidian Brahuis in the close vicinity of the Indus valley. Marshall admits that all this is so far only a conjecture.

In chapter XXII (vol. II p. 406ff.) Sidney Smith and C. J. Gadd examine the Indus script as an introduction to the sign manual published in vol. III. In addition to a description of the seals, inscriptions and signs, some highly hypothetical suggestions are submitted. Smith comes to the cautious conclusion (p. 422) that "It is disappointing, but wise, to admit that these inscriptions may in fact mean, on the present evidence, almost anything". In the following chapter (p. 423ff.) Langdon presents the above theory concerning the relationship between the Indus script and the Brahmi. In the Postscript (p. 453ff.) dated 13th July, 1928, Langdon, on the basis of the archaic Sumerian documents excavated at Jemdet Nasr, comes to the conclusion that there seems to be a more definite connection between the most archaic Sumerian script and the Indus script than he had previously been disposed to admit. If both his hypotheses are to be assumed as true the Brahmi could be derived from the Indus script only through "Aryan Sanskritists who knew the ideographic meaning, translated them into Sanskrit and derived the syllabic values from the Sanskrit words". The complexity of this hypothesis in our opinion speaks hardly in favour of a relationship between these scripts.

In the opinion of F. W. Thomas, who in the *JRAS* 1932 reviewed the Mohenjo-daro publication, Smith, Gadd and Langdon had been able to demonstrate that the direction of the script was from right to left, to show that some of the signs must be independent parts of phrases, and to furnish authoritative lists of the signs grouped according to their occurrences. They also indicated resemblances with signs found in other scripts. Since these scholars were specialists of the contemporary Mesopotamian culture they were also thoroughly familiar with the routine in the light of which the Indus texts were to be interpreted. "But the experts are rather pessimistic in regard to the possibility of decipherment, unless the Sumerians, with their highly philological tendencies, have left somewhere a vocabulary or a bilingual" (p. 461). Thomas points further out that so far "no proven case of merely syllabic value" has been detected among the Indus signs. The publishers had in the opinion of Thomas truly

recognized the clear pictographs as well as the signs for "cross-road" and for "great", which seemed to be similar to Sumerian signs with these meanings. He suggests further that the signs CCCVII-IX  are "too similar to a Sumerian sign for city-wall (*dūru*), and at the same time too complex, to be otherwise than identical with it".

Thomas then proposes the comparison of certain Indus signs with Chinese signs revealing, in his opinion, "undoubted similarities". He quotes as such cases the oldest signs for 'darkness', 'mountain', 'cross-roads'. The Indus sign CLVII  would thus be identical with the old Chinese sign for 'fire' or 'flames', while CCLXIII  resembles the Chinese sign for 'light'. The 'angle' ^ used in the Indus script seems to be the same as that in the Chinese used to denote 'heaven', 'god'. Against the writers' opinion that these common signs had been used in the Indus script with the syllabic values of that language Thomas maintained that at least in the case of Sumerian phonetical values might be tested "in connection with any hypothesis as to the language". The first choice would, according to him, be Dravidian: "The sign for 'great', which has the pronunciation *gal*, might prove decisive, if it could be shown to be used... as a mark of plurality since in Tamil *gal* has that value". A look at the Indus texts should have been enough to show Thomas that there the sign  cannot be a plural suffix: it could much better be the sign for 'great'. It is, on the other hand, hardly to be believed that a sign and its significance could be borrowed while its phonetic reading must be something quite different. The problem therefore seems to be: what is the object depicted as the Sumerian sign for 'great' = '*gal*'?

Smith, Gadd and Langdon interpreted the signs themselves to be syllabic. The short strokes ' " "' etc., however, were unknown from elsewhere, and they were therefore compared with the vowel signs of the later Indian writings. Since they seem to occur mostly after the syllabic signs of certain positions, in Thomas' opinion the most likely supposition is that they represent grammatical elements, e.g. flexional particles like those in Tibetan, the two commonest probably being those for 'of' and 'in'. Thomas' reasoning here holds good up to a point. He does not, however, seem to have studied the contemporary routine in seals nor tried to answer the fundamental question, viz. what kind of contents we may expect to find in the Indus seals, since he says (p. 464) "if such expression as «street», «wall», «house» etc., occur in the texts, we shall have to contemplate, along with names and titles, addresses also as possible items of the information which the texts conceal".

The routine contained in the Old Akkadian roll seals has been presented in the *Archiv für Orientforschung* vol. XXII pp. 12-20 by D. O. Edzard. It shows that the seals are almost exclusively either proprietor seals (198 items) or dedicatory seals (56 items). Only 3 are invocation seals and on a couple only a god or a place is mentioned while 13 are uncertain as to their contents. It seems to us that most of the cryptanalysts attacking the Indus script have not given any thought to the contents of the seals in the cultural setting of the Indus civilization.

G. R. Hunter personally investigated all the known Indus seals and submitted his studies in a doctoral dissertation to the University of Oxford in 1929. In addition to F. W. Thomas' presentation of the Mohenjo-daro publication by Marshall and other scholars Hunter reviews the contributions by Mackay, Gadd, Smith and Langdon. Since he knew the material autoptically he was able to propose important corrections to many details of the excavation reports. He points out that the objects in question are obviously seals and must have been used for the purpose of stamping e.g. offerings. Important is his observation (p. 471): "To take the seals as amulets is impossible. For if they were, how do we account for the fact that in about 99 per cent of these objects the writing is reversed as compared with the writing on all embossed, stamped, and moulded objects and with that on the copper tablets? This reversed writing can only be explained as intended for reproduction by sealing." Hunter's dissertation was printed in London in 1934: *The Script of Harappa and Mohenjodaro and its Connection with Other Scripts* (Studies in the History of Culture, No. 1). He prepared for it very laborious tables about the occurrences of the single signs, which he also submits to detailed discussion. Attention is also paid (p. 23 etc.) to the routine in this type of text: "It is probable that the seals were intended to serve much the same purpose as the Mesopotamian cylinder seals, and that their legends are, therefore, similar in meaning." His "comparative morphographic tables" are, however, based only on outer resemblances of the signs. This might perhaps sometimes help us to identify the object depicted in an Indus sign, but it cannot be used as a basis for a decipherment in other respects. The phonetic readings of the similar signs of two pictographic scripts can namely be quite independent of each other. We do not know of any pictographic script which had been borrowed. What was borrowed was obviously the very idea of writing, viz. expressing spoken words by visible signs. The clumsiness of the Minoan Linear B can hardly be explained otherwise than as depending on its being borrowed from another people to the language of which it fitted in, which it does not do with Greek.

There are also many other points on which we must disagree. Thus we cannot share Hunter's opinion (p.73, 75, 77) that the various FISH signs are only varieties of one and the same sign rendering dialectally varying pronunciations. The same objection must be made to his explanation of  as a dialectal variant of (p. 110). Hunter deduces from this "variation" as well as from the additional strokes used with (called "accents" by Langdon) "that the law of vowel harmony was rigorously observed in Proto-Indian speech". Hunter also advocates (p. 90ff.) the hypothesis that the Indus script is connected with the Brahmi, and is thus in reality alphabetic. On p. 96, while discussing the supposed numeral signs of the script, he mentions the possibility that a numeral sign may be read as a word or syllable that happens to be a homophone of that numeral, and mentions (p. 108 fn. 1 ff.) in passing that the words for 'slave' and for 'vessel' may well have been homophonous. On the other hand it is somewhat surprising that (p. 124) the picture of a beetle is interpreted as an ideogram. In what kind of texts would a *'beetle'* be mentioned?

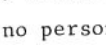
Of interest is Hunter's explanation (p. 117 and 125) that the doubled signs like were originally ideographic representations of the dual number of a word, and were later used for any word that was a homophone thereof. On the basis of the cases where two (different) FISH signs occur side by side he maintains that the language did not permit sequences of two similar syllables. The doubled signs are to be explained as duals (the trebled as plurals, respectively): "Of course it does not follow that a plural or dual meaning is necessarily implied. In many cases the word or syllable for which the doubled or trebled sign stands may be merely a homophone of the dual or plural of the signs." The deduction from the pairs of the FISH signs can hardly convince anybody, nor does the English parallel *'baby'* etc. The doubling of which Hunter explains as being the marker of the Ablative does, however, remain inexplicable. Assuming there are both ideographic and phonetic signs, the doubling in both groups must reflect quite different principles.

Hunter's conclusions seem to establish the script in fact as alphabetic and to postulate an identity of the Indus script and the Brahmi. A comparison with the Egyptian and Hittite hieroglyphics would probably have suggested that other conclusions may also be possible. Even a syllabic script with only open syllables, like the Cretan Linear B, was probably based on a pictographic script: the signs render the initial open syllables of the names of the objects depicted.

Hunter's presentation of  and (p. 116) shows that he did not identify them as case markers but considers them as nouns used as last members of compounds. On p. 27 he does, however, state: "One is tempted to regard as the suffix of the genitive case." The supposed relation with the Brahmi vowel marking makes Hunter explain  and not as ligatures but as expressing different articulations of the vowels involwed.

Hunter's work is impressive and useful because of his thorough knowledge of the material. However, his too narrow knowledge of the other pictographic scripts of the time and his preconceived ideas, especially that concerning the relationship with Brahmi, strongly reduce the value of his conclusions (cf. the review by E. Burrows in *JRAS* 1936, pp. 331-332).

Before Hunter's dissertation was published in print P. Meriggi in *ZDMG* 87, 1934, pp. 198-241 published a "decipherment" based on an ideographic interpretation of the texts. He considered a phonetical reading of them totally impossible, although Marshall's proposal concerning the possibility of a connection with Brahui is, according to Meriggi, the only reasonable possibility. He believes that the texts can be understood, at least to a certain extent, without being phonetically read, and that the variations in the "legends" might permit an insight into the "graphical flexion" of the language. If a related living language can still be identified, the phonetical decipherment may be possible. Meriggi does not make any suggestion as to the method with which this identification and the decipherment would be carried out. He states that the "postfixes" are written phonetically, but does not say anything as to how he could read them ("...nachdem die Wörter für «Getreide, Korn, Mahlen» usw. z.T. in phonetisch ausgeschriebenen verschiedenen Kasusformen bestimmt sind").

Meriggi does not in any connection refer to the routine of the contemporary Mesopotamian seals or of the later Indian seals. It is his belief that the script is pictographic-ideographic, i.e. that every picture means the object depicted, and he (p. 218) defines the texts as being administrative stamps (and thus as containing no personal names) like  "STAMP FOR HOE CORN ONE QUADRUPLE LOAD". Hunter, however, pointed out (*JRAS* 1932, p. 471) that the very find-spots of the seals suggest that every family in Mohenjo-daro possessed a seal: "Only religion can account for this universality." Furthermore, according to Meriggi's hypothesis every kind of agricultural product and every amount of every product would have had its own stamp! F. W. Thomas (*l.c.* p. 460) already remarked about the Indus seals that "despite the numerals abundant on them they were not labels denoting particular substances

and amounts as is proved by the fact that they were for the most part elaborately carved in stone". On the other hand, if the script is not ideographic but phonetic (i.e. logographic), Meriggi's attempt can hardly have brought the solution of the problem any nearer.

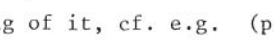
Probably the best known of all the researchers who have proposed a decipherment based on Dravidian as the language of the Indus civilization is Rev. H. Heras, S.J. Of his many writings on the topic we have had an opportunity to use only the *Studies in Proto-Indo-Mediterranean Culture* I (Studies of the Indian Historical Research Institute, St. Xavier's College, Bombay, 1, No. 19), Bombay 1953. Father Heras and his aids obviously had a good command of Dravidian, and they have done much work with the inscriptional material. The method is rather straightforward (p. 61): The remains of Mohenjo-daro and Harappa are certainly non-Aryan. They are most likely Dravidian, but the present Brahuis cannot be descendants of the then inhabitants of Mohenjo-daro. The language spoken by the latter was perhaps the parent of the present Dravidian languages (p. 64). "The grammar of this Proto-Dravidian language must have been in a state of infancy and totally undeveloped" (p. 65). While this sequence of assertions in methodically built more on possibilities than on probabilities, his statement concerning the nature of the script looks probable: "The script is a picto-phonographic script. The signs ... do not stand for syllables and much less for consonant sounds only, but express full words." The last sentence should probably be modified by the addition of a word like "mostly", since the additional strokes (the "accents" of Langdon) may very well be conventional signs rendering syllables.

Heras starts his decipherment with the sign  , which on the basis of its frequency as well as on the routine met on the later Indian seals he interprets as the marker of the genitive (p. 66f.). Without any explanation of the sign itself he decides to read it as *adu*, which according to him is the most ancient form of the Dravidian suffix of possession. The silence concerning this important sign is the more peculiar, as Heras considers the script ideographic. On p. 68 he namely describes his method, according to which the meaning of a sign, representing the object to which it refers, was at first settled, and then all the words referring to that concept in all modern Dravidian languages were investigated. The most ancient, sometimes the root only, was selected and "deprived of all suffixes and initial consonants, to obtain the probable word used by the Mohenjo-Darians". The arbitrariness of this method is obvious, to say nothing of the linguistic reconstructions. If, on the other hand, the script

were ideographic, it should be understandable in any language. In his paper, referred to above, Meriggi used only German to render the meanings of the pictograms. There is, however, no agreement whatsoever between Meriggi's and Heras' interpretations. Despite the above statement regarding the reconstruction of the proto-Dravidian sound values of the pictograms the values given by Heras (p. 68ff.) seem in general to be in Tamil form. In addition to the ideographic interpretations Heras sometimes uses the principle of homophony, too, e.g. (p. 71)  *kō* 'mountain, excellence, domination', (p. 73) "the ordinary sign for 'eight' is  *eṭ*, that has sometimes the phonetic meaning of 'reaching', also, *eṭ*", etc. Having accomplished his decipherment Heras (p. 99f.) points out that there are in the script certain signs whose values can only be explained in Dravidian languages. Among his instances the pictographs with a FISH can be regarded as really relevant. He does, however, distinguish only three variants of the sign, adscribing them various meanings: "  *mīn* 'fish, the Fish',  *min* 'shining, glittering, glorious', *mīn* 'star', and proper name or title of a king."

Also very intricate is Heras' method of interpreting the "phonetic signs" (p. 75ff.), viz. "signs that do not represent any object in a pictographic way: Of this kind are many signs to which abstract ideas correspond, for such ideas cannot be shown pictographically". He therefore compared the signs he believed to be of that kind with similar signs of the Sumerian, Egyptian, Hittite, and Proto-Chinese scripts "in the hope that similar signs of those scripts might have the same meaning". It is rather difficult to follow the author in his reasoning that since e.g. in the Sumerian script  is *du* 'to make', in Proto-Indian  is *kei* 'to make' and  is "the shortened form" of *kei* and reads *ei* (pp. 76 and 101). The Proto-Chinese and Proto-Indian signs for 'rain' and 'arrow' are again in our opinion as purely pictographic as any sign can be: the problem is how they were read (p. 77). There is no doubt a striking outer similarity between Sumerian  *gal* 'great' and Indus read by Heras *per* 'great'. While it is possible that both signs render a similar object, it is rather difficult to believe that the textual meaning represented could be the same.

Heras is, in our opinion, right in his criticism of Hrozny (p. 60): "It is again another *a priori* assumption to suppose that these seals have always a sign corresponding to the word seal, a word which though perhaps found in inscriptions of seals of other lands is not found in Indian seals at all. One seal reads *Guttasia* «of Gupta»; in the same way the inscriptions of the ancient

coins *Basileos Basileon megalou Azilizou*, which in Kharoshthi reads *Mahārāja-sa rājātirājasa mahātasa Ayiliṣasa*. They are always in the genitive. Again Harṣa Vardhaṇa signs a document with his own hand and writes *Śrī Harṣasya* "of Śrī Harṣa." Though Heras here emphasizes the routine in the seal texts, his own readings are rather lacking of it, cf. e.g. (p. 127) "  *ēḍu koḍi ēḍu mūn uḍa adu* 'the beginning of the year of the year of the Ram (is) of three garments': viz. in the beginning of the year when the Sun is in the Ram, three garments are required".

In his paper entitled "South Asia's earliest writing still undeciphered" (*Expedition*, Vol. 9 No. 4, Summer 1967, p. 34ff.) George F. Dales points out the impossibility of translations by Heras (not mentioned by name) like (Marshall No. 419) "This is the eight (formed) God one of whose sides (forms) (is) the sprinkled great Fish", and (Marshall No. 23) "The great god, who has the two sides (forms) of the high Sun of the eight (parts) of Orūr, (which is) outside the land of the rain clouds of the (constellation or month of the) Scale, which approaches with peals of thunder, of the united lands of Mīnād (the country of the Fish), (is) the rain of the year of a house of bushes" (see Heras pp. 96-99). Dales is no doubt right in emphasizing that similar signs in various scripts are by no means to be pronounced the same or have the same meanings in the different languages.

Father Heras started his decipherment from the genitive suffix identified on the basis of the routine on later Indian seals. After its completion he does, however, state (p. 99): "It may be asked what the purpose of such seals was. This is a question which I do not intend to answer in this chapter." The question looks more than justified!

Heras' observation of the homophonous character of the FISH signs is the only internal evidence speaking in favour of the language being Dravidian, since in no other language does FISH seem to have meanings that would be fitting in a seal inscription. Our own computer studies showed that there were no signs behaving like prefixes and infixes but only such which behaved like suffixes. Taking into account the hieroglyphical and logographic character of the script it may be said that infixes would hardly ever be recognizable, but on the other hand such a script would be rather troublesome in a language operating with infixes. Contrariwise a series of signs behaving like suffixes was very clearly disclosed. While it seemed unclear whether the script was in itself ideographical or phonetical, it looked probable that grammatical elements could hardly be expressed ideographically. The only possibility of rendering them in a pic-

tographic script seemed to be the homophony. If an object depicted in some of those signs, which according to the computer analysis behaved like a suffix, could thus be identified, one could look for a language in which the name of the object in question and a fitting grammatical element were homophonous. This principle seemed to work in Dravidian, and the word signs were thus also compared with Dravidian words. Even there the underlying principle turned out to be the homophony.

The homophony is here, of course, to be understood in the grammatological sense of the word (cf. e.g. Gelb, *A Study in Writing*, p. 67ff. and p. 108ff.). On the other hand we have taken it in a much narrower sense than it is used e.g. in Egyptology: the hieroglyph HOUSE is in Egyptian read as *par*, *per*, *apr*, *epr*, *epra* etc. Among the possible homophones available only such words can be chosen that have a meaning fitting in with the routine of the seal texts known from other parts of the contemporary civilized world, especially from Mesopotamia. Although the history of the Indian seals is not yet known, it seems possible that they continue the tradition originating in the Indus civilization.

A. H. Dani (*Indian Palaeography*, Oxford 1963, p. 13ff.) gives a short account of the attempts at decipherment of the Indus script. In principle he joins with the statement by Friedrich (*The Extinct Languages*, New York 1957, p. 170) who regards the task as hopeless. Dani himself investigates the signs, dividing them into categories on the basis of the objects depicted. He points out that the compound symbols suggest the same principle as underlies the conjuncts in the later Indian scripts and supposes that just this feature led Langdon and Hunter to connect the Indus script with the Brahmi alphabet. Dani remarks that "This can hardly be dogmatically asserted when the recognizable objects easily suggest pictographic or ideographic meanings". In other words, if the script is logographic we must assume that the compound symbols represent compound words and not initial consonantal clusters.

Dani's observation that the strokes added to the pictographic symbols — which he considers to be one of the chief characteristics of the Indus script — seem to speak in favour of the "picto-phonographic" nature of the script, seems to be well weighed up.

After our *First Announcement* had been released in February, 1969, we received in March a copy of the Soviet Indus team's publication *Proto-Indica: 1968* kindly sent by Prof. Yu. Knorozov. This *Brief Report on the Investigation of*

the Proto-Indian Texts was originally published for the VIII International Congress of Anthropological and Ethnographical Sciences held in Tokyo in September, 1968. It resumes the *Предварительное сообщение об исследовании протоиндийских текстов* (Moscow 1965) by G. V. Alekseev, Yu. V. Knorozov, A. M. Kondratov and B. Ya. Volchok. This publication was issued in 1968 in an English translation entitled *Soviet Studies on Harappan Script* as Occasional Paper No. 6 by Field Research Projects (Coconut Grove, Florida). The papers contained in it summarize the results of the Soviet team, which also processed the material by computer. A parallel study of the Egyptian hieroglyphics and of the Indus script has fully proved the hieroglyphic character of the latter. The same analogy shows further "with a fair measure of certainty" that in the Indus texts too the signs of great absolute frequency must be grammatical "indexes" and those of little frequency must be "root morphemes" (i.e. words). The computer processing shows that 75 per cent of all the polygrams in the Egyptian text have a linguistic reader while the rest are accidental. These results also hold good in other texts having a similar statistical structure. On the basis of the investigations the "recurring blocks" can be identified. Further, the variant and semi-variant signs of the blocks can be found out, and with the aid of these again the blocks can be divided into various classes. The variants and semi-variants together correspond to about 30 per cent of the total size of the texts. This fits in very well with the facts known from Egyptian.

The positional-statistical analysis revealed the morphological and syntactical structure of the Indus language, which the Soviet scholars then compared with those of the neighbouring languages, viz. Sumerian, Hurrite, Elamite, Indo-European, Dravidian, and Munda. As a result of this investigation Knorozov states (p. 21) that there are grounds for considering that the Proto-Indian language is close to Dravidian in grammatical structure.

In its main lines the computer research of the Soviet team was obviously carried out along similar lines to our own, and their results agree well with ours. It may be regarded as an important corroboration of the Dravidian hypothesis that two teams working quite independently arrived at conclusions that conform so well.

In the *Proto-Indica: 1968* (p. 28ff.) N. V. Gurov develops these results further towards the linguistic interpretation of the texts on the basis of the Dravidian languages. He points out that Dravidian would easily be written with a hieroglyphic script of the Indus type, since morpho-phonemic changes at junctures occur only in very restricted cases and the root morpheme is distinctly

separated from the morphemes "agglutinated" as suffixes: suffixes beginning with a vowel are joined to the roots ending in a consonant and *vice versa*. The principle of homophony seems not to be expressly mentioned but it is in any case applied by Gurov when proposing readings of certain signs, e.g. (p. 35)  is explained as 'four' = *nāl* DED 3024 = *nal* 'good, correct, kind, nice, best' DED 2986. Even  and its ligatures are joined with *kā* 'carrying pole' although the further interpretations differ from ours.

After our *First Announcement* had been released, Dr. Dieter Schrapel in Marburg published *Die Entzifferung des Yatischen*. Obviously with a view to Euclidean exactness the author omitted all discussion and argumentation stating axiomatically: 1. The language of the Indus Valley seals is a Dravidian one, 2. The "seals" are amulets, 3. They are closely connected with the Vedic *yātu*-magic, 4. Their language is that of the Yātus, called henceforth Yatic, etc. Schrapel not only reads the texts of the seals on the basis of the rebus principle but even the pictures of the animals, of the mangers etc. on the seals. The author must be appreciated for his ability to operate with Dravidian homophones. His arguments are, perhaps, now and then somewhat puzzling. Though he possesses a very thorough knowledge of Indology he explains e.g. the concept of "amulet" (p. 2) through German amulets of Christian wording, and the sign  (p. 36f.) as 'hand' through certain playing card symbols. Like the interpretations of Heras those of Schrapel also often diverge from the contemporary routine of seals and amulets, e.g. (Marshall No. 350)  with a tiger and "a dish" as the emblem is read *veḷḷiyārai mīn iṭukkunaccuvāḷ uṟuvaḷoṭu* "all honest people love passionately to take a bath" (p. 47), and (Marshall No. 351)  *maruḷaṉṟu vāḷ* "through the Diable to wealth" (p. 57). However, the argument presented by Hunter against the interpretation of the seals as amulets referred to above looks so heavy that it would have needed some refutation before Schrapel put forward his own interpretation.

The writer of these lines seems to be the only fellow cryptanalyst mentioned by Schrapel. One cannot avoid getting throughout the impression that the author published his study in order to parodize the homophony method of our Finnish team. We admit with pleasure that he did it with intelligence and humour.

It has been pointed out by some critics of the Dravidian hypothesis that there is a large chronological gap between the Indus civilization and the oldest monuments of the Dravidian languages. The use of the latter in interpreting the Indus script is therefore not possible. An objection of this kind is no

doubt in principle valid. At the beginning of the sixties Z. Mayani published an interpretation of Etruscan on the basis of Albanian. Since, however, our oldest Albanian text dates only from A.D. 1462, and this language has obviously greatly suffered from phonetical attrition, it seems methodologically impossible to interpret an Etruscan word (some 2500 years old) with the aid of its outer similarity with some present-day Albanian word.

Because of the special circumstances in India there seems to be no real gap between the Indus civilization and the beginning of the Dravidian tradition, which could be compared to that between Etruscan and Albanian. Our knowledge of the Indian chronology in this respect can be summarized in the form of a table like the following:

2500 - 1800 B.C.	The Indus Civilization
1800 - 1500 B.C.	Various sub-Indus cultures
1700 - 1500 B.C.	The invasion of the Aryans
1500 - 1000 B.C.	Dravidian loan words in the Veda
1000 - 400 B.C.	Dravidian loan words in Classical Sanskrit
500 - 1 B.C.	Dravidian loan words in Pāli
400 B.C. - 400 A.D.	Dravidian loan words in Epic Sanskrit
300 B.C.	The Tamil Brahmi inscriptions
100 A.D.	Classical Tamil literature

As to phonological changes undergone by Dravidian, it seems that certain conclusions can be drawn from a comparison of the various stages of the tradition available to us. It is striking how closely the Dravidian loan words even in the oldest Aryan source, the Rig Veda, remind us of the form of the same words in the present Dravidian languages, in many cases expressly in Tamil.

The DED shows that there occur in Dravidian many types of consonantal interchanges which would facilitate a script based on homophony, cf. e.g. *makara* ~ *nakra*, *Meru* ~ *Neru* (like Sanskrit *Sumeru* ~ Pāli *Sineru*). The specially Dravidian phoneme series *l* ~ *ḷ* ~ *l̲* also seems to permit variations reflected in old loan words in Aryan: RV *khala* 'threshing-floor' ~ Ta. Ma. *kaḷam* id., RV *bala* 'strength' ~ Ta. Ma. *valam* id., *val* 'strong', etc. On the other hand Vedic (and Pāli) shows an *-ḷ-* corresponding to *-ḍ-* in classical sources. According to Bloch *ḷ* is still met with in Marathi, Gujarati, Panjabi etc., nearly in the same geographical setting from which Pāli (*pāli* ~ *pāḷi* 'row, line, canon': Drav. *pāḷi* ~ *pāli* id.) probably originated and which once was the domicile of Vedic. We meet in certain other cases very old documentation, too,

e.g. Sanskr. *tāla* 'Palmyra palm', Prakr. *tāḍa* ~ *tāla* : Drav. Ka. *tāṟ*, Tel. *tāḍu* : Akkadian *tālu* 'a palm' (H. Zimmern, *Akkadische Fremdwörter*, Leipzig 1915, p. 54); Sanskr. *tāla*, Buddh. S. *tāḍa* 'lock, bolt', Pāli *tāḷa* : Ta. *tāṟ*, cf. Akkadian (Zimmern p. 30) *edēlu* 'to bolt', *daltu* ~ *dalat* 'door'; Akk. *pīlu* ~ *pīru* 'elephant' (Zimmern p. 50) might be derived from the original of Ta. Ma. *vēṟam* id. The name of the Chola Dynasty (*cōḷa* ~ *cōḻa*) is in Sanskrit *coḍa*, in Greek (Ptol. 7,1,13) *Sōrai*.

Interchanges are met with in Dravidian vocalism too, cf. e.g. (DED 3930) Ta. *māṭam* 'storied house' ~ Ta. *mēṭai* id. The alternations *e* - *i* and *o* - *u* have been treated in detail by Burrow (*BSOAS* X pp. 289-297, repr. in *Collected Papers on Dravidian Linguistics*, Annamalainagar 1968): among these we meet interesting cases like Ta. *uṭaṉ* 'with' : Ta. *oṭu* 'with'.

Irrespective of the identity of the underlying language the Indus script can hardly be built on any other principle than homophony, like e.g. the Egyptian and Hittite hieroglyphs. The homophony hypothesis presumes that the phonetical development of "homophonic" words has taken place uniformly so that their correlation would still reflect that in the times of the Indus script.

N o t e

A MS. of the main part of this paper was in 1975 sent for publication to the *Journal of the Epigraphic Society of India*. However, I never received any proofs or offprints. When the above text had already been processed, I learned that the MS. had been printed in the vol. II of the *JESI*, Mysore 1976.

P. A.