

## INTRODUCTION

Since 1979 over ten thousand petroglyphs and thousands of inscriptions were systematically explored in the Northern Areas of Pakistan (consisting of the districts Gilgit, Baltistan and Diamir) and at the northern fringe of Indus-Kohistan. In this area, rock-carvings have been known for at least eighty years, of which Aurel STEIN (1944: 16–24) had studied a small but important sample.

So I was certainly not the “discoverer” of this complex, but I instigated for the first time a systematic and extensive investigation of such monuments, as first presented during the Fourth International Conference of South Asian Archaeologists, held at Naples in 1977 (JETTMAR 1979: 917–925). My first success in this venture was to convince Prof. Gérard FUSSMAN then of the University of Strassbourg that a personal examination of the inscriptions near Alam Bridge would be rewarding. That led to his successful visit in 1976, and the results were published in 1978.

In 1979 I “persuaded” Prof. Dr A.H. DANI “to have a field trip along the Karakorum Highway (opened to tourists about a year ago)” together with me (DANI 1985: 5). In an earlier report he said even more graphically that I had “literally dragged” him “into this field” (DANI 1983: III). It was so fruitful that we included the investigation of petroglyphs into the tasks of a joint Pakistani-German project for anthropological research in the Northern Areas which had formal approval by the Government of Pakistan in 1980. Prof. DANI became the coordinator of our counterparts, participating personally during the earlier campaigns, before his commitment to more general programmes of



the UNESCO became too demanding. As a distinguished archaeologist and a palaeographer, trained in Sanskrit, he worked independently in interpretation and publication, with a characteristically imaginative approach.

Since 1980, the Pak-German team has had regular expeditions every year, at first sponsored by the German Research Council (with a substantial contribution from the Volkswagen Foundation). Later on, financial and administrative responsibility was taken over by the Heidelberg Academy for the Humanities and Sciences, which in 1984 inaugurated a special committee for the implementation of our programme.

Until 1985 I joined the team with the intention to devote most of my time to further exploration in places not previously visited. But negotiations with sponsors and the involved governments and, of course, public relations, became my permanent burden. I had seen important clusters of carvings beforehand during travels in 1955, 1958, 1964, 1971, 1973 and 1975. The earlier journeys were still done on foot and on horseback, which gave a general idea of the environment, its possibilities, difficulties and even dangers. By writing a book on the pre-Islamic Religions of the Hindukush (JETTMAR 1975) I had the chance to become thoroughly familiar with the cultural heritage and the linguistic situation, thanks to information provided by my friend Prof. BUDDRUS. And I had the invaluable advantage to be aware of the nearby excavations made in Soviet Central Asia and their interpretation.

Dr. V. THEWALT had joined our team already in 1980. Then he held a scholarship provided by the German Archaeological Institute, and was immediately confronted with responsibility for the documentation of the very large site of Hunza Haldeikish. The involvement of the great institution just mentioned turned out to be ephemeral. So our next expeditions — when we both had to fend for ourselves — with changing, not always trained collaborators, became especially exacting. So in his proper field of Buddhist iconography, many results are not yet included in his preliminary reports (1983, 1984, 1985).

The assignment to our team of Director M.S. QAMAR, as representative of the Department of Archaeology and Museums was maintained through several campaigns. We found him most



cooperative, and the investigation of yet unknown areas became his special interest. He made many excellent photographs for his department.

All other participants were specialists, necessarily confined to their "own" discipline, and often interested in a particular period.

Prof. FUSSMAN had made valuable contributions to the political and cultural history of the states and statelets in the North-West during the Early Buddhist period; so his focus was on Kharoṣṭhī inscriptions. Prof. O. v. HINÜBER had splendidly dealt with the problems raised by the Gilgit Manuscripts, so he deciphered and interpreted the Brāhmī-inscriptions, well aware of their importance for Buddhist studies. His published results are intended for experts in this field. Coordination, with the sometimes diverging views of art-historians (e.g. Prof. HÄRTEL), is still a task for the future.

Relevant work was done by a specialist for Indian palaeography, Dr L. SANDER, and by scholars in the field of Iranian studies: especially in respect of more than 500 Sogdian inscriptions, by Dr LIVŠIC, Prof. Dr H. HUMBACH and Dr N. SIMS-WILLIAMS. The results are available to collaborators of the project, but only a part are published so far. In respect of the restricted number of Chinese and Tibetan inscriptions we are in the same situation. Translations were proposed by qualified and interested colleagues either by a short article — by the late MA YONG — or by personal letters (from Prof. D. SECKEL, Prof. H. FRANKE, Prof. L. LEDDEROSE, Prof. K. SAGASTER and others); but they have not been properly discussed in print so far.

I was involved in this work since its modest beginnings, later on entrusted with the commitment to write the regular report on the last campaign, and the application for the following one. So I could hardly avoid trying to sketch a broad overview on the basis of the complementary or conflicting views of these specialists, correlating them with my own observations. An initial problem was to bring the material into a chronological sequence, and to discern the presence of ethnic and social groups expressing their sentiments and intentions by signs, images and written texts. Petroglyphs and nothing else form the "archaeological record" here. Excavations were beyond the frame of the licence granted



to us by the authorities of Pakistan. And even without this restraint, much of the remains elsewhere preserved in the soil were here destroyed by terrible floods sweeping again and again the base of the Indus valley. In 1841, when a natural barrage, formed by a huge landslide in the gorges north of the Nanga Parbat finally broke, hundreds of Sikh soldiers, camping on the banks of the Indus near Attock were drowned; and a similar inundation in 1859 let the Indus rise for ninety feet during one day — at the same place (DREW 1875: 414–421).

However, for a part of my interpretations I could refer to texts written in China (dynastic histories, reports of Buddhist pilgrims), in Eastern Iran, in the Tarim Basin, or Kashmir. As almost no ancient coins were found in these distant mountains, the typical “index artefact” of the Indo-Iranian borderland is lacking.

Since 1984 we have tried to gain the interest of a broader public for this province of rock-art by arranging exhibitions of large-size photographs in several European museums. That proved to be a great help for obtaining the financial support which we badly needed for our programme. In order to write the text of a pertinent catalogue (JETTMAR—THEWALT 1985) in a comprehensible way — explaining the sequence of the photos — I had to transform my reflections into a coherent “story” of what had happened during the Prehistoric and Early Historic periods in the valleys which we will call the Karakorum Region — strictly speaking, the area between the Hindukush-Karakorum ranges and the westernmost Himalaya.

This is not the place for a synopsis of this kind; but a few statements must be made in advance of the publication of the linguistic articles which form the body of this volume:

The inscriptions which have been properly studied in respect of content and palaeography offer valuable although not unshakable points of reference for the classification of most of the associated “figural” petroglyphs. They tell about their date and meaning, forming a rough guide-line during the first millennium A.D.

From the second century B.C. onwards — when ZHANG QIAN collected concrete information — we have an almost continuous



sequence of relevant written sources in many languages: Chinese and Sanskrit, later on Persian, Arabic, Tibetan, and the Saka language of Khotan. Neither Aurel STEIN nor TUCCI (1977) exhausted these sources, which at least limit our speculative freedom (see JETTMAR 1977).

For the same period, observations and classifications proposed by art historians, in most cases specialists for Buddhist iconography, should not be neglected.

However, in the field dominated by classical disciplines, the archaeologist is not deprived of an independent approach by comparing techniques of production (by stone or metal implements), motifs, style, and degrees of repatination. A few words should be said about the problems involved. When we compare attempts to bring the rock art observed in other parts of the world into a meaningful chronological order, we are forced to realize that there is no generally accepted dominant methodology at the exclusion of others. Apparently the natural environment and the cultural context open highly individual strategic chances for the explorers. Data acquired by elaborate systems of recording (ANATI 1977) are scarcely available when a small team has to work in a large territory without suitable living conditions — among a not always compliant population.

So it is necessary that I explain the criteria I used for dating petroglyphs which could not be classified with the help of pertinent inscriptions, or by comparing them with well-known works of Buddhist art. (It turned out that my approach has much in common with that of RANOV when working in the Pamirs (1982)).

In this “problematic” material I mainly differentiated according to degrees of repatination. I discerned:

#### *1. Completely repatinated petroglyphs*

It should be said at the outset that this approach can only be used in areas where the bare rocks are found under special conditions: rainfall must be minimal and the temperature very high, at least in summer. In such places, the rocks are covered by a sort of patina well-known from other arid and hot areas, and therefore called desert varnish (“Wüstenlack”). It was intensely investigated in many continents, but the most promising article



on possible dating of desert varnish is, I suggest, based on field-work in the Indus valley during the Karakorum Project of the Royal Geographical Society (WHALLEY 1983). So I may refer to this publication and start with the statement that “bruising” is the chosen technique under such conditions for carvings and writings, durable and visible from a great distance.

The result of bruising was called “sgraffiti” by STEIN (1944: 17), and basically the same term (graffiti) may be used further on — in spite of the evident difference to what we mean when speaking of monuments from Roman or Greek antiquity (for the terminology see B. ALLCHIN 1986: 152–155). The bruising, however, are subject to repatination. Full repatination makes bruising invisible to a person standing in front, but when the stone is reflecting sunshine, the figures will appear.

Bruising is often done with a pointed stone. But hammering with metal implements — chisels and pick-axes — was used very early, as observed by RANOV (1982: 86). Suitable copper and bronze tools from Middle Asia were published by KUZ'MINA (1966: pl. I/4,7; III/1,2,7,8; XII).

On the basis of the data presented by WHALLEY (1983: 22), but mainly relying on my own experience from several expeditions, I would say that in the Indus valley complete repatination needs a very long time: three or four millennia. That is a crude margin, since exposure to sunshine (hence orientation) and the quality of the stone are important as well and must be taken into consideration, but it is confirmed by the observation that none of the completely repatinated images are connected with inscriptions. They do not belong to iconographic groups strongly reminiscent of those attributed elsewhere to the first millennium B.C. — a time when we are already near the first written sources, e.g., Chinese annals mentioning Saka-migrations through the Karakorums. So we may call these fully repatinated carvings “prehistoric”.

Before 1988, such “prehistoric” carvings were only observed in the Indus valley between Chilās and Shatial, where nature presents a multitude of barren and polished, heavily patinated rocks in bizarre formations — such as enclosures for meeting places, galleries, or “chapels” waiting for the decorating artist. Here, we have a series of enormous open-air galleries inviting gifted con-



temporaries or men of later generations to a sort of contest. Maybe the tradition to produce petroglyphs started here and was then adopted by later immigrants. The technique and the artistic ambition were transferred to other areas with poorer conditions as well.

Artistically, the fully repatinated carvings do not belong to one and the same stylistic group. There are several clearly differentiated complexes, sometimes in the same cluster. The spectrum is even broader than that what would be expected on the basis of cultural diversity expressed by the inventories known by excavations in neighbouring Swat (STACUL 1987: 115–127).

## *2. Petroglyphs with incomplete repatination*

Differing degrees of repatination are relevant for chronology in this group, but in order to obtain (relative) dates they can only be used when they occur on the same or perhaps on neighbouring rocks.

Certainly, recent engravings or bruising, made by workmen during the construction of the Karakorum Highway, are easy to identify — they are not patinated at all. The contrast with all other petroglyphs is so sharp that I had to assume a hiatus of several centuries between the last traditional petroglyphs and modern “revivals”. That is supported by the complete absence of Islamic inscriptions, so frequently occurring in the Pamirs and in the Wakhan corridor (cf. KASYMOV 1984). But already in the northern part of the former Gilgit Agency the situation is different, and P. SNOY indeed made a short film about the making of petroglyphs there.

The explanation is already offered in the first reasonable account of such engravings (GHULAM MUHAMMAD 1907: 110): “The tradition is that in ancient times the land was frequently visited by fairies, who used to make these inscriptions, as in the opinion of the Chilāsis it is beyond man’s power to produce such inscriptions.” We must only add that these fairies were considered as powerful, dangerous and incalculable like the forces of the surrounding nature. It is a great pity that our investigations are injurious to the former sentiments of awe and respect. Maybe for similar reasons, palimpsests, namely carvings overlaying and destroying those of an earlier period, are rare in most sites (ex-



cept Shatial and Oshibat). Evidently, the native, pious artist of the Karakorum respected the work of his predecessors, maybe his ancestors. This fact is helpful for documentation but obviously not for the establishing of a relative chronology.

So we have to shift to an approach which was postponed for the first group — the fully repatinated carvings — namely systematic comparison of the “motifs”.

2.1 There is a small group of semi-patinated carvings which can be identified by iconographic studies, but they are not connected with inscriptions. The explanation is that they were made or ordered to be made by foreigners from countries where literacy was virtually unknown or uncommon.

One such group arrived from western Iran (it was wrongly identified as Parthians), the other from the steppes. Nomads from the steppes, apparently arriving or passing in the last centuries of the first millennium B.C., are responsible for animal style carvings. That is in accordance with an information contained in the history of the Earlier Han Dynasty, mentioning Saka hordes, migrating to South Asia via the “Hanging Passages”, correctly identified with the gorges of the Indus (JETTAMAR 1984, 1987a).

Later waves of such Northerners can be identified by their “tamgas” or distinctive emblems, as will be mentioned when discussing the contribution of SIMS-WILLIAMS.

2.2 Many motifs of the local rock art are derived from the great civilizations of Asia. For over two centuries scholars have developed reasonable sequences for these, showing the artistic developments in iconography and style. As for the inscriptions, the specialists for palaeography have done similar work. The languages used in the inscriptions were studied following well established disciplines.

In many cases, pictorial carvings and inscriptions are situated on the same rock, and they are often interrelated. Thus we can try a sort of crossdating — if not always with absolute certainty. We may learn about the meaning of the image, and the name of the artist may be mentioned as well, or the name and title of the



patron. But even inscriptions in different languages may be inter-related. In this context, the external written sources, speaking about major powers and world-religions, must also be used as a contextual framework.

That so far published as the historical interpretation of our expeditions is mainly based on sites with such optimal conditions; which may be recapitulated here as briefly as possible. Already since the 1st century A.D., a route, used by merchants, Buddhist missionaries and pilgrims, artists and political envoys of the Kuṣāns, existed between Kashmir and the Tarim Basin. Using another access, traders from the lowlands reached Chilās and founded a Buddhist sanctuary there. Not much later, an emporium and a sanctuary were established near Shatial, 70 km farther west, just above the entrance of the Indus into the last gorges.

Between the 5th and the 8th century A.D. was a period of Buddhist dominance. The main sanctuary was opposite Chilās. Many members of the ruling elite are known by name, Buddhist Sanskrit being used as an actual language, but many of the local lords had Iranian or still enigmatic names. The Indus valley was not then a part of Palūr, the Buddhist kingdom which had its centre in Baltistan, also including the Gilgit valley. The inscriptions published in this book indicate that Chilās was rather a frontier district of the Dārada-kingdom, which had its capital in the Nilum/Kishanganga valley. Apparently, the local chiefs remained more or less independent. Trade between Central Asia and North-West India brought them substantial gains, so they accepted refugees from formerly Buddhist countries, now haunted by nomadic inroads and internal unrests. Such foreigners were used as clerks, religious advisers and artists who had to adapt themselves to the local medium of rock art. So it is quite possible that late Gandhāra art "hibernated" in this area before expanding for a last time to the lowlands, as proposed by DANI.

The political events of the 8th century A.D. — namely the conquest of Palūr by Tibetan armies and the vain attempts of the Chinese to stop this expansion (well known from Tibetan and Chinese annals) are not directly documented. Seemingly there is a hiatus, and only thereafter we find a few late inscriptions (10th or 11th century A.D.) as well as pertinent carvings.



The noble carvings of the main Buddhist period can be used in order to identify the origin of the travellers, and the provenance of the local lords. We might say that the attempt to discover the full meaning of this unique combination of inscriptions and carved images imposes tasks which are not too different from those typically encountered by students of classical archaeology. In many sites the natural rocks were decorated like the shelter walls, surrounding the sanctum of a religious building. Some inscriptions have an official character, others correspond to graffiti, expressing more private views and concerns, aspirations and animosities.

2.3 A special challenge for ethno-archaeologists in this “historical context” are figures which occur side by side with the carvings made by Buddhists — or adherents of other world religions. However, they are not easily included into classical iconographic schemes.

2.3.1 Depicted are animals, including birds, human figures and quasi-architectural structures, looking like framework buildings, or erected by using square stones, ending in a flat or pointed roof. Often a spire is indicated, topped by a three-pronged fork or equipped with lateral branches, maybe flags. In most cases the similarity with the stūpa is evident.

The statistical data available for this group are still incomplete; but I estimate that they are more frequent than regular stūpa-carving. This is somewhat obscure in our preliminary publications, since “classical” or “prehistoric” carvings are more recognizable and easier to explain. So in the catalogue of our exhibition only five plates clearly feature this group. They are often technically simple, and our local guides, influenced by our predilection called them “Pakistani work”, i.e. made by locals. Very rarely are they connected with inscriptions.

It was observed by A. STEIN (1944: 22) and v. HINÜBER that such figures might not necessarily correspond to actual buildings, being rather “quasi-substitute donations”. To make them or to have them made was apparently as meritorious as the erection of a stūpa. We could say that they had an ideal-typical function not so different from a maṇḍala, which is always a symbol of the



cosmic order. But they do not render a ground-plan: they rather stress the vertical elevation. The anthropologist will recall that the structuring of the world into cumulative zones of ritual purity was predominant in the only slightly diverging philosophies of the Dardic mountain peoples — almost up to the present day (JETTMAR 1975). Plants, animals, and also humans are classified in the same way. Sanctity is always paramount, corresponding to vertical altitude in such schemes — and not unexpectedly, when the grandeur of the mountains, the wonderful smell of trees and flowers beyond the high meadows would be formative experiences to all immigrants into the Himalayas, independently of their origin.

That is perhaps the simplest explanation for the enormous popularity of such carvings. They were a sort of “vertically transposed maṇḍala”.

The endowing of the figures with human attributes is also compatible with popular mythology, which contains the concept of a giant supporting (and representing) the habitable earth (JETTMAR 1975: 215). On the other hand, the transformation of the stūpa into a mountain model is no absolute break with the Buddhist tradition, which is known to have encompassed such ideas as well.

2.3.2 When I wrote my first report (JETTMAR 1980: 169–170), I could not clearly discern between this indigenous group and another one, which I now classify as chronologically different. It has a larger, but still restricted number of motifs: especially axes, decorated wheels or rosettes, a special kind of ibex-pictures, a human figure with moderately straddled legs, extended arms and large palms, and the same figure standing over a horse, together with composite fighting scenes.

In his book on “Chilas”, DANI conflated what I now consider two groups — not too closely related — under the one term of “Battle-Axe Culture”. According to him this culture was diffused by a “Battle-Axe-People” who introduced horses and a new system of horse riding and fighting together with the “popularization of a higher form of Hindu religion in symbolic way”, yet also integrating local beliefs and practices. And what I have here called quasi-architectural structures he explained by the



“development of a temple type” — under “Brahmanic influence” crowned by a śikhara — “on the basis of the older form of terraced stūpas”.

The diffusion of the Shina language is connected by DANI with these events, but he is not entirely clear as to whether it was imported by a ruling elite — “the kings who used Sanskrit names” — or by a “new movement of people” superseding the sedentaries, but not the “earlier hunters who continued their primitive life and experienced themselves in the primitive style of art on the rocks” (DANI 1983: 230–231).

DANI was apparently unaware that the term “Battle-Axe Culture” had already a problematic history of use among European prehistorians attempting to connect the diffusion of archaeological cultures with the dispersion of Indo-European languages. Strangely enough, he is confronted with much the same problems as his (often German) ancestors in terminology.

My own moving away from such ideas, implying a chronological distinction between Buddhist and Non-Buddhist carvings, was due to my commitment to another approach — that now called “spatial archaeology” — so I was following a trend, earlier propagated by the New Archaeologists (as overview cf. EARLE and PREUCEL 1987: 503–506).

Spatial archaeology has to be applied to the petroglyphs on two levels: firstly a “macro-perspective”, to explain why certain localities were used for compact clusters (I called them sites) or extended lines of carvings. Secondly, a “micro-perspective”, where we have to take into consideration how the different kinds of carvings are arranged in one particular site. To briefly recapitulate my observations here:

Fully repatinated carvings are found at Thalpan Ziyārāt and Thor North on barren plateaus, especially on rocks near to open and sandy places. TUCCI mentioned “communal meeting places” for yearly festivals “in the borderlands of Tibet” (TUCCI 1973: 50) and FRANCKE has also described one (1914: 20). That seems to me the most plausible basis for an explanation.

The place where so many west-Asiatic and northern motifs were observed is an imposing rock bastion near Thalpan Ziyārāt — it looks like a scene or a natural altar — outside of the former holy precinct, but not far away.



During this late pre-Buddhist period, the rocks bordering the clefts, where streams or streamlets flow out from a side valley and join the Indus, were almost as a rule decorated with impressive carvings. Apparently the sanctuaries were situated here. We may imagine that the population of each valley came down to the mouth of their river for festivals, especially in wintertime. Then the banks of the Indus, extremely hot in summer, are almost hospitable.

All humans and animals depicted in such places are explicitly males. That corresponds to the system of orientation still preserved in some remote valleys which I could visit thirty years ago: In the inner sphere (the inhabited valley with the surrounding mountains) men are rated as pure and superior, their privileged space is with their goats on the highmeadows or roaming about on the slopes and tops of the mountains as hunters for the mayaro — the pure and noble game (markhor, ibex, snow-leopard). Females are considered as unclean and weak, to be confined to the lower part of the valley, working in the fields.

In the outer sphere, the other but never distant world, the highest mountains are considered as the abode of goddesses, most powerful and of extreme purity. Fairies are their retinue, sometimes descending to compliant humans.

The male principle, represented by chthonic deities and demons — is entering from below, lusting for sexuality and dominance. But without such dark forces there could be no fertility — so sanctuaries were necessary at the bottom of the valley. Such places are still considered as ambivalent or even dangerous, to be avoided by women. (I have seen one altar of the females high up in the Haramosh valley, facing the summit of this famous peak.)

But even if such interpretations remain but bold speculations, we must concede that the clusters of carvings spread over otherwise barren parts of the Indus valley must be connected with the settlers, living in the side valleys. Other carvings were added in later centuries: together they form a sort of spiritual biography of the related valley, the earlier carvings situated in the centre, the later ones forming the periphery.

Such valley-sanctuaries were still maintained after the establishment of "Main Sacred Precincts" during the predominantly



Buddhist period, as represented by the eastern part of the site Shatial Bridge, by the central part of the site Thalpan and by the site Chilās I. Minor Buddhist sanctuaries were near to the places where the Indus had to be crossed, always a dangerous interlude of the journey.

Even the routes connecting the sacred precincts and the river-crossings were decorated. So we learn that they were almost unchanged during the last millennium up to the British conquest. A major resting place with hundreds of inscriptions was discovered at Oshibat, where water (of the Indus) became available after a long march through a desert with rocks, cracked by the glowing sun.

Most of the camp sites seem to have been near to the mouth of a side valley, and here the foreign visitors contributed to the pre-existing decoration. This is a great help for dating. In such places we see that typical Buddhist engravings and “vertical maṇḍalas” were approximately contemporary. In some cases, there are still regular Buddhist inscriptions connected with them, so we might identify them as primitive or “rustic” stūpas. Whether they are “temples”, as assumed by DANI, is quite another question, for they have neither doors nor windows.

Hindu influence should not be flatly denied, but a “pagan” motif already present in the prehistoric period was now rendered with a superior technique, typical for the best works of the Buddhists, namely the giant with extended arms.

Spatial archaeology reveals that the axes, decorated wheels, highly stylized ibex-carvings, human figures (with a horse) mentioned above must belong to another context. They are restricted to the Indus valley near Chilās. Here they were sometimes observed together with Buddhist carvings — but they appear to be quite inconsiderate, almost intrusive. Mostly they are concentrated in certain areas. Near Chilās there is a site which I was tempted to explain as a clan sanctuary, the meeting place of a population which could afford to make their own spiritual and political decisions. And just here a rocky slope, scarcely engraved earlier, was almost exclusively decorated by the battle-axe worshippers.



An even larger area west of the mouth of the Hodar stream certainly did belong to a local community, devoted to the same cult. Here we find a most expressive demonstration of the contemporary spiritual situation. It is a battle-scene, the warriors equipped with picks or swords. A strange anthropomorphic figure is leading one party — much larger than the warriors, the body and legs being simple strokes, and the face is replaced by a circle or wheel, with a dot in the centre. The other party is apparently defending a stūpa — but this building is situated in the left corner of the tray in the rock, brushed aside by the victorious giant.

There are more rocks, decorated with a full set of such symbols — and one of them shows a combat between the representative of the stūpa and the man of the wheel. I proposed here a connection with the solar cults, propagated by the Chionitic invaders in the lowlands.

Upwards curved battle-axes of the same type as those suddenly appearing in the Indus valley were also used in the valleys of the Central Hindukush by the so-called Kafirs of the nineteenth century, the last pagans of the region. Other symbols, e.g. solar rosettes, have the same extensive diffusion. So we are confronted with a powerful movement. No historical report tells about the exact circumstances, but a date before the end of the 8th century A.D. is improbable.

2.3.3 Only a small number of petroglyphs can be safely attributed to a horizon which is later than that of the axes and decorated wheels: There are carvings of stūpas enclosing the umbrellas of the spire into a sort of mandorla, like the paintings in the temples of Alchi. That points to a date later than most of the other petroglyphs — around the 11th century A.D. The Śāradā inscriptions observed on the same rock may also speak for such a date (cf. GOEPPER 1982, pl. 29).

This period is not well known from rock-carvings, but the historical background can be reconstructed by using the literary sources — if they are used more systematically than has been done so far. They indicate that Gilgit, Astor and Chilās were situated in one and the same state, which must have been identical with the Kingdom of the Dāradas. Buddhism was the official religion, but



maybe there was an “anti-Buddhist” movement as well, proclaiming solidarity with similar tendencies in Tibet. During their repeated raids against Kashmir the kings of the Dāradas could rely on a lavishly equipped cavalry — we are told “with golden trappings” — and on foreign, mostly Muslim mercenaries. Since carvings which deploy the deeds of richly equipped mounted warriors in close combat and during hunts in an otherwise unknown style were found near Chilās, we could assume that a feudatory lord of the Dārada Śāhi had his residence nearby.

Finishing this exposition of my own interpretations, the reader should be warned of the very small size of the area where the concentration of petroglyphs and inscriptions allows such far-reaching conclusions. The monuments are situated in a narrow belt lining the banks of the Indus. Only here are ideal conditions for rock art. In the side-valleys there is more rainfall and vegetation, and the schistose stone is not so inviting for depiction. The same unfavourable conditions are to be found south of most westward extensions of the Great Himalayas. (There was alternatively an area for rock paintings, but they survived only in caves. One site near Barikot is known but not yet published.) So we are basing our conclusion on a narrow probe, like a test drill leading down into the past.

Certainly, the chances and dangers for travellers north of the Indus valley were different from those on the southern side. In the north the passes are high, especially those of the main ranges, but they are snow-bound only between December and March/April. Therefore, the best time for travellers was late autumn, as in this season the rivers can be crossed without major risk. In summer they are transformed into torrents by the melting glaciers. In former times most deadly accidents are known to have happened by untimely crossings.

In contrast to winter-travelling in the north, the passes in the south — due to heavy snowfall and many avalanches — are open only in summertime. When they are open — between June and August — they are without problems. The perennial southwards connection however — through the gorges of the Indus — is extremely dangerous. The path is not only steep and narrow, but muddy and slippery and could only be used by men on foot.



Therefore, the valleys on both sides of the Indus must have been a series of seasonal "waiting rooms" for travellers. Southbound traffic started in June, northbound traffic in October. The "waiting rooms" were linked by paths which had to pass the Indus valley, and they were also needed for choosing the right exit.

So the Indus valley was in strategic position, situated at a place where the distance between the ranges in the north (Karakorum) and those in the south (Himalaya) was minimal. A shortcut between Central and South Asia was possible, partly compensating for the dangers and strains.

The data gained during the expeditions were stored in an archive, located at present at the South Asia Institute, Heidelberg. It contains notebooks, official reports, plaster casts, maps and drawings, and the main part comprises colour-slides (most of them 24 x 36 mm), supplemented by photographs in colour or/and black and white.

Sir Aurel STEIN (1944: 18) has already foreseen that colour-photographs might be the best documentation. He was perfectly right when he wrote about the basic necessities in the future "for any safe attempt at decipherment. Such is not likely to become possible until these inscriptions are studied on the spot by a fully qualified Indian epigraphist or else adequate reproductions are secured through an expert photographer provided with the requisite aids such as colour sensitive plates (or films) and special scaffolding where the position of the inscriptions makes its employment indispensable" (STEIN 1944: 17).

He also clearly recognized that the petroglyphs are "not properly speaking engraved, but like a sort of graffiti produced by 'bruising' on the smooth but exceedingly hard surface of the detached rocks and huge boulders." They are perfectly visible by their lighter colour, but "this method applied to the very dark patina like surface" of the rock "does not permit anywhere (of) useful estampages being taken."

In Baltistan the situation is different; rocks with important carvings and reliefs are covered with moss and lichens, so a careful cleaning and a tracing by more complicated techniques are



necessary. Therefore the procedures described by ANATI (1977: 10–22) must be applied in the future:

“In many cases it is necessary to wash the surface of the stone with non-ionic soap” and to apply “very delicate washable colours”; that is called the “neutral method for discerning the smallest and faintest figures”. The next step is called “tracing” by ANATI (1977: 59): The rock is covered by sheets of polyethylene. On such transparent sheets all underlying features are marked by hand, with felt tipped pens, discerning superimposed compositions by using different colours. Photographing and casting (with liquids, contact- and positive casts) are considered by ANATI as additional methods. This procedure needs for a rock of the size 30 x 15 metres 14 collaborators working two and a half months – 8 hours a day. “In order to thoroughly prepare text and illustrations 1277 more working days will have to be employed” – and that would keep our small team busy for more than two years.

I am so explicit here because by this comparison it becomes clear that there is no feasible alternative to photography under our present circumstances. The other reason to avoid time-consuming procedures, as demanded in E. ANATI’s booklet under the points “Cleaning – Tracing – Casting” was the fact that the petroglyphs are endangered by actual economic and spiritual developments.

When the Karakorum Highway was built the destructions remained rather moderate – in relation to the fact that 8,000 tons of explosives were used (HAMID 1979: 172). Only the site called Chilās I was heavily affected, where some carvings described by Sir Aurel STEIN have been blasted.

The plan to build a dam at Basha transforming the Indus valley into a lake, would mean the inundation of most of the historically important clusters of petroglyphs (as can be seen on the “Basha Reservoir Map” belonging to a feasibility study of the Montreal Engineering Company). But more dangerous are small daily activities, e.g. the blasting of boulders near to the new roads. Many of them are covered with petroglyphs, but none of the villagers is particularly concerned about that. The fragmented rock is cut into cubes and transported by jeeps or tractors to the villages for the construction of new, prestigious houses in the old



British colonial style. Other monuments were damaged by passengers engraving their names while travelling along the Karakorum Highway.

The Department of Archaeology and Museums did its best to protect the most spectacular clusters; but as they are spread along the banks of the Indus, at many places accessible from the highway, dozens of attendants would be needed for their control.

So a slow and ideally systematic documentation, step by step, was not advisable; the areas bordering the highway had and still have priority. These parts of the valleys of the Indus and its main tributaries, which have a higher frequency of rock art and inscriptions, are to be divided into segments. (The main clusters located in these segments are called sites.) The description and analysis of the segments and the understanding of the sites must be the final outcome of our efforts.

A simple archive of all recorded petroglyphs (even if they can be used by visitors with outmost liberality, and the data and images are retrievable with the help of modern computers) cannot be considered a full account for the academic community — and even less for interested visitors and authorities. So the necessity remains to present the carvings in a printed form, as a series of volumes.

Recently the Director General of Archaeology and Museums, Government of Pakistan, demanded for the final report “publication in Pakistan”. That corresponds to a general trend and precludes me from going into details here. In this case one task would be the systematic training of our designated collaborators and successors. That is what I had always in mind when I started my work.

A systematic documentation means that all petroglyphs which had been observed should be mentioned — even if not all deserve the same detailed illustration and interpretation. Many images are schematic and can be characterized in the framework of a pre-conceived typology. But they may also form the setting for others which are highly informative. The larger context must also be recorded, i.e., the coordination of the figures within the context of the site and the site itself in connection with others, which had different and maybe complementary functions. Then



conclusions may follow in respect of specific and general problems.

But discussion, by printed articles, should start as soon and as intensely as possible. The decipherments proposed by different specialists in the same field are often disarmingly divergent. And we find that even the same scholar may improve or at least change his earlier decipherment after subsequent inspection on the spot. So much of our work is still undergoing a phase of trial and error.

Such preliminary studies have already been disseminated in various journals (as indicated by the bibliography); the Sogdian inscriptions will be published in the frame of the *Corpus Inscriptionum Iranicarum* by Dr SIMS-WILLIAMS, and a "second harvest" will certainly be needed in later years.

Henceforth, we shall offer to such preliminary efforts a series under the heading "Antiquities of Northern Pakistan" with the subtitle "Reports and Studies".

In the first volume of this series — presented here — only linguistic studies have been included, with a strong bias towards Indology. When we received the manuscripts, the staff was even more modest in numbers than it is today, Dr THEWALT had to deal with an enormous number of photographs necessary for the evaluation of the decipherments. As for myself, I had to take over the tasks of an editor, albeit too late in my life to be as effective as I would have wished. So I made vain attempts to reach an overall standardization. On the other hand, I made some dangerous concessions: So in one case additional readings and corrections submitted later on were interwoven with the text into a remarkable embroidery — a practice that clearly had to be stopped if we were ever to reach our schedule of publication. So I have to insist that we are here publishing preliminary reports and studies, often speculative, rather than postpone publication to some indefinite future when systematic documentation is completed. Some historically important inscriptions were already provisionally deciphered by Professor DANI — and the scientific community will want to know what alternatives are proposed by other scholars working on the spot, having access to perhaps better photographs.



Excellent maps of the area were certainly produced when the route for the Karakorum Highway was traced out, but as usual they are labelled “top secret”. So photogrammetry had to replace them for the important clusters, excellent work being done by our site surveyor Robert KAUPER (Dipl.Ing.). In the meantime, linguists developed their own systems, correlated with our own by references to our series of photographs — also mentioning the number given by DANI, who developed himself a system.

If I am mentioned as editor, it simply means that I have taken over responsibility for some editorial decisions, most of the actual work being done by Dr THEWALT and Dr KÖNIG.

In order to make it evident that the articles are preliminary reports, I add some remarks in order to start the discussion:

The Kharoṣṭhī inscriptions observed so far in the site Chilās II were studied by FUSSMAN on the spot, several with the help of a binocular. As the access to some of the niches is difficult or even dangerous, for final publication it will be necessary to use “special scaffoldings” as already demanded by A. STEIN (1944: 18), or else the assistance of a photographer who is an experienced climber. This fact explains the not optimal quality of some photographs.

Most of the inscriptions render proper names, without titles. In many cases they mention visitors, persons who produced — or ordered to produce — the graffito. Some of the names are Buddhist, but a “slave of Śiva” is also mentioned. The religion of the visitors responsible for the accumulation of this remarkable cluster was certainly open to a broad syncretism. Therefore, we find deities like Balarāma/Saṃkarṣaṇa and Vāsudeva/Kṛṣṇa (cf. HÄRTTEL 1987), and, most probably, Hāritī. According to the palaeographic analysis, most inscriptions were made in the pre-Kuṣān or early Kuṣān period. Several names appear repeatedly, maybe indicating several visits to the site.

FUSSMAN explains the complex (the term “site” is extremely problematic; we deal only with one of the layers, which can be distinguished so far) as connected with a camp of traders who came with their caravans to the Indus valley, eventually waiting for an easier crossing of the river, when the summer floods had



faded away, or turning back to the plains after a time of rest passed in the shadow of the rock-shelters, near to the river where their animals found a watering place.

That implies that the identifications and explanations in DANI's book are refuted altogether: no kings, no conquest, not even a sanctuary.

On the basis of my own observations, I am tempted to offer a simple explanation as to why traders might have visited the Indus valley near Chilās and, after completing their transaction returned to Gandhāra, Kashmir or the Punjab.

The banks of the Indus between Chilās and Harban are still much frequented places for gold-washing. In early autumn, I saw the camps of the goldwashers, within a few hundred metres of Chilās II. Gold must have been an export-product of this area since very early times. On the other hand, no reasonable merchant would select the rock face at Chilās II as a resting place. The access to some of the niches is a hazardous exploit, and no grass is available for pack-animals nearby. In summer the area is infested by blood-sucking flies, wrongly but graphically called "tiger mosquitos".

Nevertheless, cliffs like those of Chilās II have been used as sacred places at least since the second millennium B.C., decorated with graffiti presenting deities and demons. So it seems quite possible, even likely, that an intrusive community of Buddhists (as proved by their petroglyphs) took over the habits of the indigenous peoples and placed their own sanctuary within such an impressive scenery.

FUSSMAN deciphered other Kharoṣṭhī inscriptions as well, observed outside of the remarkable concentration at Chilās II. They are somewhat later and attest a more modest Buddhist activity, certainly not only by traders since monks are also mentioned. A more substantial commentary may be given to the composite inscription translated by FUSSMAN as: "This is the pious gift" (signed by) "Vijaya-priya (the beloved of victory), the king of the Ribemdhatha-race." The first part is in Brāhmī, the second one in Kharoṣṭhī. A date not earlier than the 5th century A.D. is proposed.



According to FUSSMAN both parts (Brāhmī as well as Kharoṣṭhī) show the same solemn gravity, indicating the prestige of the lord, who had a Sanskrit name but belonged to a lineage (or tribe?) who had preserved the seemingly non-Indoeuropean name of their ancestor. That this man was the chief — or one of the chieftains of the Chilāsī — as assumed by FUSSMAN, is possible. The site where this inscription was observed was probably not associated with the dominant group (who expressed their sentiments in Chilās I), but was rather representative for the population of Chilās; maybe it was the meeting-place of a tribal section or large clan.

In this case we have to face another problem: how to explain that no other bilingual inscription was observed? Why did the chief use Kharoṣṭhī for his signature? Maybe Vijaya-priya was a foreigner — from an area where Kharoṣṭhī was still used for ritual purposes.

In the time when this inscription was made — according to FUSSMAN — the Kharoṣṭhī script survived mainly at the southern fringe of the Tarim Basin and we may take into consideration that the name Vijaya-priya would be convenient for a scion of the house ruling Khotan (cf. FRANCKE 1928). Maybe there were collateral lines ruling in dependencies, sheltered in the mountains. Thus it might be identified with the area called Tūsmat in the *Ḥudūd al-ʿĀlam* (1970: 259–262). There, the preservation of the Kharoṣṭhī script would not be surprising. (Impressions of seals, presenting the particulars of the owner in two scripts are known from Bactria as well — KRUGLIKOVA 1984.)

One of the two inscriptions from Oshibat, already deciphered by FUSSMAN, has peculiarities similar to those studied by him near Alam Bridge. Both sites, Oshibat and Alam Bridge as well as Hunza Haldeikish belong to the same category. They were situated on main routes, so the contribution by foreign travellers was even higher than was usual in the sites near Chilās.

In the Indus valley between Shatial and Gor, more Kharoṣṭhī inscriptions were observed during the last campaigns. They will be treated together with those from Hunza Haldeikish. For this site as well, the presentation of decipherments alternative to those of DANI (1985) may be useful.



O. v. HINÜBER has contributed two articles on the Brāhmī material divided by language and indices, but with a common bibliography. First he deciphered inscriptions visible on our photos, between 1983 and 1987 he participated in most campaigns of our team. His studies bear witness to an astonishing achievement.

Engaged in other projects as well, he could only stay for restricted periods, hardly more than three or four weeks; but during this time, he worked with utmost intensity, sometimes even at night, because in some cases the faint akṣaras are almost invisible in broad daylight and can only be discerned with oblique artificial lighting. Many places were visited in more than one campaign; so we have alternative readings in a number of cases.

Before entering into scientific discussion it must be stressed that the two studies render results of his researches only up to 1985. Later results mainly but not exclusively due to his participation in the campaign 1987 are not included. Quantity and quality of the new contributions deserve publication in a third article rather than complicated insertions in the present texts.

In many cases v. HINÜBER visited areas where systematic registration had not yet been done by other members of the team. Then the current number in his notebook is mentioned together with the pertinent year as e.g., 518-84.

In other cases photos made by members of the team before the documentation are used (e.g. Je 80-17-02).

v. HINÜBER had studied the Gilgit Manuscripts with the main focus on their relevance for Buddhist studies. However, an analysis of the pertinent colophones provided information for the history of the Paṭola Śāhis, the kings of Palūr ("Bolor", which I used earlier, is not the best rendering, cf. PELLLOT 1959: 92). Palūr was the paramount power in the Karakorum region between the 5th and the middle of the 8th century A.D.

Before the conclusions of v. HINÜBER appeared in print, TUCCI (1977) and myself (1977) had independently — and apparently without too many errors — reconstructed the outline of the political developments. But v. HINÜBER could add in a series of articles — one of them still unpublished — many details, names, and dates. For his investigations, he could use the unpublished royal inscription at Danyor, which I had seen as early as 1958. In



1975 I could convince the owner of the land by substantial payments, that the preservation of this monument already destined to be blasted would be rewarding to him.

Palaeographic competence is a basic requirement for such researches, and one's chances are additionally improved by a permanent preoccupation with related kinds of graffiti.

This training allowed v. HINÜBER to decipher Brāhmī inscriptions, in fact different variants (the so-called "Gilgit/Bamīyān script type I" and Proto-Śāradā) in an astonishingly short time: 150 was the maximum in one day; 400 during one campaign may represent the average, repetitions included. So it was a sizeable problem to organize this material and to select interesting and representative samples.

Looking for a systematic approach v. HINÜBER discerned different types: He started with "names either in the nominative or endingless or less often in the genitive case" (type 1), proceeded to those adding verbs meaning "has come, has arrived" (type 2). The next type (type 3a) contains indications of professions, castes and tribes, religious status, official titles. Thus he arranged the inscriptions more or less in conformity to their degree of complexity. Numbers were attributed accordingly. The following type (type 3b) encompasses persons who mention their relation to an ethnic group, a caste or place of origin. A further group (type 3c) gives names with additional terms revealing religious affiliation. The next group of inscriptions announces the names and titles of rulers or officials (type 3d).

We can say that the organizing principle is preserved throughout the first article. Activities are mentioned only in respect to rulers. All the other inscriptions treat of lesser persons and their attributes.

The second article deals with inscriptions describing activities — in most cases the presenting of devotional offerings such as the works of rock art.

The next topic are religious texts mentioning venerated Buddhas and Bodhisatvas, invoking their blessing, but not always telling the name of the adorant monk or layman. Such inscriptions were not produced in the usual technique as "bruising", but are actual carvings, made with metal implements, i.e. chisels. Very small and exact akṣaras resulted; and they turned out to be ex-



tremely interesting in content. They quote many names of Tathāgatas so far only known from texts found in Central Asia. The connection with dhāraṇīs and deśanās is evident; and the rock-carvings were probably designed for similar magic functions.

v. HINÜBER did not explain the underlying principle for the arrangement of the material in the second part of his contribution and the system of numbering was clearly constrained by the fact that at all costs it had to close with 109. (The numbers 110–147 had already been used in an article which in fact presented Iranian names which should rather form a subgroup of the type 1 and 2).

On close examination, we see that even in the second part of these twin-articles, v. HINÜBER has maintained a systematic approach and a principle of organization, compatible with that of the first part. But the focus of attention has shifted from the subject and its attributes to activities and motifs including veneration and incantation (also mentioning the pictorial accompaniments of these activities — figures of the Buddhist pantheon).

Most of the results are fascinating for both the linguist and the scholar of Buddhism. Other observations will be crucial for future attempts to reveal the political and ethnic history of the westernmost Himalayas. Some should be discussed here in a broader framework.

In the subsection type 3d dealing with the “titles of officials and rulers” (including “simple honorifics” as well), we hear about two inscriptions which have been translated into English. (A rare concession to readers not trained as indologists!)

Inscription 59 (a,b,c,d) has the following translation: “The subduer of enemies....the glorious (śrī) great king of the Dards, Vaiśravaṇasena, the subduer of enemies, the king, who is a subduer of enemies...The teacher of the glorious (śrī) Vaiśravaṇasena, great king of the Dards, Rudraśarma, is firmly established in the district Avardī.”

A comparable inscription was found on the northern bank of the Indus: Inscription 59a. a–d. “The glorious Vaiśravaṇasena, the subduer of enemies, great king in the land of the Dards”.

In respect to this second inscription, v. HINÜBER gives the following comment: “This inscription, found directly below the



modern bridge connecting Chilās and Thalpan, seems to mark the border of the territory held by Vaiśravaṇasena.”

It should be mentioned that there are other inscriptions on a man called Vaiśravaṇasena but without mentioning titles; one inscription says that a rājaputra with the same name wrote the inscription (= 59b) with his own hand.

If the inscription 59a had the function of marking the border of the territory held by the Dāradas, as assumed by v. HINÜBER, their rule hardly transgressed the left, i.e. southern bank of the Indus, where the main inscription (59) is situated. That refers to the 4th/5th century A.D. — if the palaeographic attribution should be correct as well. Taking this inscription as a starting point, we may conclude that the centre of the Dārada state was not in the Indus valley nor in the valleys north of the Indus, because we know the powers situated there by name: In the 10th century the Gilgit valley was known as the district Prūśava (BAILEY 1968: 70–72; LAUFER 1908: 1–10). Here had been the land Bru-ža incorporated under the designation “Little Palūr” in the state ruled by a dynasty known as Paṭola or Palolo Śāhis. Their headquarters were farther to the east, in Great Palūr (including present Baltistan and perhaps Ladakh). West of Bru-ža were areas known to the Chinese under the names of Chieh-shih and Shang-mi (STEIN 1921: 28–45 after CHAVANNES 1903: 158f., 129, 292).

On the other hand, the population of the Indus valley was called Dāradas and the centre of their state cannot have been very far away. In one of the commentaries to Pāṇini we find an expression meaning “The Indus (coming) from the Dārada-lands” (JETTMAR 1977: 420). So we should shift the focus of our attention towards the south. Here we find Uraśā, corresponding to the present Hazara district (cf. STEIN 1900, II: 434), Oḍi, which may have been located in the Kaghan valley (BAILEY 1980) — and the Nilum/Kishanganga valley, which fits the geographical and historical requirements most perfectly. In the 11th century A.D. this area was called Daraddeśa, land of the Dards (cf. STEIN 1900, II: 435); the town of the Dards (Daratpurī) was situated here (STEIN 1900, VII: 912), “hidden in the mountains”, maybe near Gurēz.



Since there is an easy route between Gurēz and the Indus valley via Barai pass, it is quite reasonable to assume that the state controlling the Nilum/Kishanganga valley could have expanded its influences to the Indus valley. However, this dominance would remain shaky, because in winter-time all passes east and west of Nanga Parbat are closed.

In earlier centuries, there existed an institution of royal fratricide in the principalities of North Pakistan, especially in Chitral, which could explain why Vaiśravaṇasena was educated in the Indus valley. When the king died, the heir apparent killed or tried to kill his brothers and especially half-brothers before they could start a rebellion. Therefore, princes who had no serious chance to become heirs to the throne, but were dear to the heart of the ruler, were sent to border districts. Here they were educated by a powerful and loyal personality. After the death of their father, such a prince could either take refuge beyond the border — or return victoriously to the capital if other princes had failed to get public compliance.

By assuming a similar system we might find an explanation for the modest inscriptions mentioning Vaiśravaṇasena and another person with a similar name, perhaps his brother. Living in a sort of exile, the young prince would have had plenty of time to make rock-inscriptions, and he would have certainly needed a reliable teacher and protector — even of a religion different from his own. To this man he remained grateful after an unexpected promotion.

Another royal inscription made much later — in the 8th century A.D. according to v. HINÜBER — was found on one of the rocks of the site Chilās I. DANI and v. HINÜBER have offered not too discrepant interpretations. It can be taken as certain that the names of the ruler and those of two other members of his family are Sanskrit, although the names of official nobles of his retinue are “obscure at present”. v. HINÜBER proposed tentatively that they might be explained on the basis of an early form of Burushaski. Such enigmatic names occur in the colophones of the Gilgit Manuscripts as well, leading to the conclusion that: “This might point to a political as well as cultural difference between the ‘Sanskritised’ Paṭola Śāhis and the perhaps local rulers of Chilās”.



We cannot be sure that Vajraśūra was one of the “local rulers” of Chilās. Maybe he came as a pious Buddhist — together with his family and entourage — to the famous holy places in the Chilās/Thalpan area. Then the inscriptions, topped by the carving of a stūpa, bear witness to this solemn visit.

If v. HINÜBER’S interpretation is indeed correct, and the inscription reveal Vajraśūra as overlord in the Indus valley during the first half of the 8th century A.D., then the consequences would be even more interesting. In this case we might dare to identify him with the king of the state called Chieh-shi or Chieh-shuai/shu (TUCCI—ENOKI 1977: 89) in the earlier translations of Chinese sources.

The centre of this state had apparently been somewhere not too far from Tokharistan (ENOKI believes that it “occupied the upper waters of the Kunar”, i.e., Chitral). It had been subject to the Hephthalites but remained Buddhist. Maybe one of the modern names of Chitral, Kashkar, corresponds to the term used by the Chinese as originally proposed by Aurel STEIN (1907: 19). Perhaps Kashkar was the land of the Khasa (TUCCI 1977: 82). They were certainly an important ethnic group, expanding far towards the east.

The Chinese sources tell that Chieh-shi formed an alliance with the Tibetans and attacked Tokharistan; on the other hand, it closed the way to the caravans bringing rice and salt from Kashmir to the Chinese troops which had occupied Little Palūr. That probably means that Chieh-shi controlled the Indus valley between Chilās and Bunji, the zone which had to be crossed by the main lines of supply. Several scholars including myself (JETT-MAR 1977: 416; STEIN 1907: 13–16; 1921: 32) were convinced that something must have been wrong in this story. How can one minor state be able to be aggressive in two areas far away from each other? However, if one of the rulers of Chieh-shi had conquered the Indus valley beforehand, then such a situation could have existed for a while. That could explain the strange names of the courtiers as well; the language of Chitral, Khowar, still has an enigmatic non-Indoeuropean substratum (MORGENSTIERNE 1973: 241).

There is still another open question: Who were the overlords of the Indus valley in the long interval between the inscriptions of



Vaiśravaṇasena and that of Vajraśūra? Hardly the Paṭolas, because their inscriptions, found in the Indus valley and discussed by O. v. HINÜBER, are rather informal, mentioning no titles. So they probably came as pious visitors — or refugees.

At the moment, it is more reasonable to assume that the valley between the gorges of the Indus north of the Nanga Parbat and the gorges below Shatial were still considered to be a part of the Dārada kingdom but virtually independent. Tribes and landlords could make their own politics; the Chinese pilgrims heard nothing about local rulers, but tell about a religious centre, situated in Ta-li-lo (STEIN 1928: 20–22). BĪRŪNĪ heard about the area under the name Shamil or Shamilan (JETTMAR 1984: 212); he was informed about a famous “idol” of Shamil, perhaps the wooden Maitreya-statue already known to XUANZANG (BEAL 1884/1969: 134).

v. HINÜBER closes his first article by presenting the almost complete deciphering of a lengthy inscription, found on a boulder on the barren plateau overlooking Shigar, the former capital of one of the principalities in Baltistan. This inscription was made when Baltistan was part of the Palūrian Kingdom, i.e., a well administered Buddhist state, using Sanskrit as the official language.

The author had submitted the second part of his contribution in German, also used for the third part (Inscriptions 110–147), already printed in *Studia Grammatica Iranica* (1986).

Starting in the same area, where the last inscription of the first part is located — namely in Baltistan — he now discusses “votive inscriptions”. Most of them belong to graffiti (produced by bruising) or actual engravings of distinctly Buddhist content — representing Buddhas, Bodhisatvas and most frequently stūpas.

They record the names and the pious intentions of “the persons who in lieu of erecting stūpas had to content themselves with offering such rock-pictures as modest proofs of their devotion”. This explanation proposed by A. STEIN (1944: 22) is accepted as reasonable by v. HINÜBER. He believes that many of the Buddhist donors remained only for a relatively short time in the mountains, not long enough to make all the necessary arrangements for the construction of a real stūpa. Therefore, they had



to restrict themselves to producing a graffito plus inscription. For the same reason, he says, many bruising remained incomplete.

But this is only one of the possible explanations; and in some cases it seems improbable. Some of the chief donors were responsible for petroglyphs, representing stūpas of different types, made by artists who did not use the same technique; and stylistic differences are evident as well.

We should rather take into account that in the Indus valley petroglyphs were made for religious purposes from its earliest prehistory. So apart from real stūpas, which were much more exposed to destruction, graffiti were made or ordered as well — with the same pious intention.

We do not know how the artists were rewarded by their customers — certainly not in cash, since no coins were ever found here. Maybe gold-dust in fixed quantities was used, as was the custom only a century ago. More likely are liberal allocations of food or hospitality for a prolonged period.

The Gilgit Manuscripts were produced under similar conditions. They were copied at the request of wealthy and influential persons eager to gain religious merits for themselves as well as their living or dead relatives — and willing to care for the subsistence of the scribes (v. HINÜBER 1983: 55).

The pertinence of the donors to the regional elite does not mean that they were necessarily settled in the immediate neighbourhood — it is quite possible that they ordered additional decorations of the sacred precinct including Thalpan and Chilās I during subsequent pilgrimages.

None of the great sponsors seems to have boasted of official titles. Maybe they exercised pious restraint, or they were merchants, the respective organizers of the caravans.

A recurrent donor named Siṃhoṭa clearly had a preference for an artist or a workshop with a distinctive style; another, Kuberavāhana was less selective, but here also a favorite artist is evident. Maybe such local donors made use of a wide choice offered by the temporary presence of foreign artists. That would explain sophistication as well as diversity.

Maybe some of them came due to the “loss of patronage” in the lowlands as assumed by DANI (1983: 130); maybe they came for



their own safety which was threatened by the Chionitic inroads in the time between the travels of the Chinese pilgrim SONG YUN (518–522 A.D.) and the journey of XUANZANG (629–645 A.D.). (JETTMAR 1986: 193).

If such conjectures are correct, then we indeed find “the final stage of Buddhist Art of Gandhāran origin” radiating from otherwise destroyed centres in North Pakistan, i.e., in the Indus valley. Nothing could be more interesting for the historian of art, even when the concrete places of reference are still doubtful in most cases. Systematic excavations in the Hazara District and Azad Kashmir are necessary in order to solve the relevant problems.

In the site Chilās I some Bodhisatvas visible on the rocks facing the Highway are wearing “crowns” which attracted v. HIN-ÜBER’s special interest. He concluded that the components of this ceremonial headdress were not mounted on a solid ring-shaped hoop, but were fixed to a sort of ribbon. It was closed at the back of the head by a bow-knot, the ends of the ribbon hanging down. This was certainly an ingenious construction: very practical because, due to the flexible base, it could be adapted to the size of the head, but also folded and preserved in a small bag or casket.

In Tillja-tepe, northwestern Afghanistan, a golden “crown” or rather diadem matching this description was discovered in the grave of a noble lady, almost certainly a queen or princess (SARRIANIDI 1985: 258, pl. 12–15). The basis was a band of gold-sheet, 2.5 cm broad, with a length of 45 cm. At the ends were loops for fixing the ribbons, to be knotted behind the head. Small transverse tubes are fixed to the band while five decorations in the form of plants or trees with flowers had similar tubes. Thus, the decorations could be fixed to the diadem with the help of straight pins. The extremely rich grave goods (including coins) attest a date not later than the first century A.D. So the Bodhisatvas were featured as wearing a ceremonial head-dress which had a long tradition.

A part of the material, which N. SIMS-WILLIAMS presents in his contribution “The Sogdian Inscriptions of the Upper Indus: A Preliminary Report”, had already been studied by HUMBACH



(1980) who had at his disposal a series of rather poor quality photographs which I took in 1979, mainly at the site Shatial Bridge. In the same report, a limited number of inscriptions in other Iranian scripts were discussed. I had earlier submitted selected copies to V.A. LIVŠIC who reacted by two letters, one addressed to me, the other one to HUMBACH. For a comprehensive presentation, it was agreed with the editors of the *Corpus Inscriptionum Iranicarum* that Dr SIMS-WILLIAMS should join our team, see the sites, and publish the pertinent part of our material in their series, notwithstanding our intention to include the inventory of all sites in the final edition.

Therefore, only a short summary, sparsely illustrated, is printed in this volume just sufficient to indicate the historical importance of the site of Shatial Bridge, where most of the Sogdian inscriptions are concentrated.

LIVŠIC dated the earliest inscriptions within the 3rd to 4th centuries A.D., others between the 5th to 8th centuries; but in some cases even the 9th century would be acceptable to him. HUMBACH assumed that at least a part of the Sogdian inscriptions are contemporary with Sanskrit inscriptions of the 5th and 6th centuries A.D. The rest might be earlier, nearer to the famous Sogdian "Ancient Letters", which were found in or near watch-towers of the "Tun-huang Limes" by Aurel STEIN. SIMS-WILLIAMS insists that the "Ancient Letters" were correctly attributed by HENNING to the beginning of the 4th century (312/313 A.D.). The date proposed by J. HARMATTA (1979: 164), at the end of the 2nd century A.D., is rejected by him.

What intention could have lured to many literate people speaking Sogdian to the site Shatial Bridge, a rather gloomy place in the Indus valley?

In my first attempt to deal with the question (1980: 175), I called Shatial Bridge a point of junction in a trade system maintained by the Sogdians. My explanation was based on the well-known fact that many of the trans-Asiatic routes in Central Asia were implemented by merchants of Sogdian extraction (SMIRNOVA 1970: 122–198). Religious motivation, for instance pilgrimage, seemed improbable. Their names give evidence that many Sogdians considered themselves as "servants of Iranian deities".



Therefore, I concluded that the Sogdians were not only active in the flourishing trade with the Far East: they also organized the caravan routes to India via Gandhāra or Kashmir.

A traveller looking for the shortest connection between Sogdiana and India might choose a route over the Pamirs or via Badakhshan: after crossing the Hindukush range he will reach the upper part of the Gilgit valley. From there, via easy passes, he may enter the valleys of Tangir or Darel; they would lead him down to the Indus, not far from Shatial. Here, the river had to be crossed — and that was certainly a major event marking the end of one section of the journey, a major occasion for offerings and prayers, and for the production of commemorative petroglyphs.

When I was reading the report of SIMS-WILLIAMS I felt that my opinion was not contradicted but was perhaps even confirmed by the new data. SIMS-WILLIAMS observes the “heroic tinge” of the proper names occurring in many inscriptions — quite compatible with the profession of a Sogdian merchant venturer. Almost three hundred years later, noble families supporting their life of luxury by commercial activities, decorated the assembly halls of their residences at Panjikent with wall paintings glorifying the heroic deeds of armoured knights, supposedly their ancestors (AZARPAY 1981: 79–125).

Certainly, actual behaviour did not always match such chivalrous standards. If the interpretations offered so far are correct, then we see that fellow travellers were also ridiculed without inhibition. Some of the carvings are “sexually explicit”. Allusions in uncouth wording occur as well. Elsewhere in the Indus valley, later visitors were inclined to respect the signs and names of earlier travellers; but the Sogdian merchants were rather inconsiderate in this respect.

A disturbing question is still open: Several hundred Iranian inscriptions (surely more than 90% of those known from Northern Pakistan) are concentrated in the western wing of the site Shatial Bridge, on rocks spread over a steep slope, leading down to the left bank of the Indus. They are intermingled with many Brāhmī inscriptions, together with some other scripts and languages.

The eastern wing of the site, however, now called Shatial II or Shatial East, has almost exclusively Brāhmī inscriptions (an



isolated Sogdian palaeograph was recently observed). Here, stūpa-carvings are most numerous. We are definitely in another cultural setting.

There is no reasonable chance of getting an explanation of the difference between eastern and western wing by chronology; for they are contemporary or at least overlapping. And what was the reason that Sogdian inscriptions are rare in most of the other sites in the Indus valley east of Shatial Bridge?

It is well-known that merchants were considered as potential spies in the days of the Mongol Empire. There was an age-old tradition of this kind. The movements of Muslim traders were submitted to many restrictions in the heyday of Mohammedan power and aggression. We learn from BĪRŪNĪ that all entries to the valley of Kashmir, most of them situated on high passes, were strictly controlled. No foreigners from western countries were allowed to enter, and only Jews were permitted for a while (SACHAU 1888/1962: 277; cf. JETTMAR 1987: 669).

In the town Rajawari, Muslim merchants had to hand over their goods to their partners (SACHAU 1888/1962: 279). IBN KHURDĀDHBIH (9th century A.D.) learnt that the snow-bound mountains in the north “are crossed only by a certain tribe of infidels who carried the merchandise on either side for the merchants of Multan and China” (JAFAREY 1979: 213) — evidently an equally restrictive practice.

Should the site Shatial Bridge not be explained as an emporium, situated on the fringe of the territory normally permitted to Sogdian caravans? Was it the place where the traders exchanged their goods against Indian exports brought by partners from Gandhāra, Taxila and Uraśā? Maybe one of the Chinese inscriptions refers to this situation. One word might be translated as “mountain pass” but also as “transition” or “boundary” — as I was informed by H. FRANKE and D. SECKEL.

In this interpretation one problem remains, i.e. that all inscriptions — Sogdian as well as those in Brāhmī — are situated on the southern bank of the Indus.

The river below the inscriptions is not broad — but swift, even raging. Travellers using skin rafts crossed the Indus a few miles upstream — at the mouth of the Harban valley. There it was even



possible to bring horses to the opposite embankment by using flat barges (as it was in 1955, when I myself had to cross there). The problem can be solved by assuming that the Indus could be crossed near Shatial by a bridge, broad enough and solidly constructed so that it could be used by riders and pack-animals. Such bridges must have existed in the heyday of the Trans-Karakorum Traffic System (5th–8th century A.D.). A bridge connecting the banks of the Gilgit river, over a distance of an arrow-shot and suitable for cavalry is mentioned in a Chinese report. A full year had been needed to complete it (cf. CHAVANNES 1903: 153 n.).

Such a laborious construction was certainly well guarded. Accordingly, I concluded that the power controlling the access from the northern side, protecting the Sogdian merchants, had a fortified bridgehead on the southern bank. So I named a suitable place overlooking the site Shatial I and easy to defend “Shatial Fort”. In the meantime, Dr THEWALT found the remains of stone walls closing the gaps between the natural barriers — so my prediction was justified.

Most southern trade partners wrote Brāhmī and were Buddhists. Not all of them were completely aliens, many of them had Iranian names as well (cf. v. HINÜBER 1986), but this did not preclude tensions and a sort of “joking relationship” with their Sogdian colleagues coming from the north. The strange observations which were made at Shatial, but especially in Thor North might be explained by such a behaviour. We will come back to this point when discussing SANDER’s contribution.

Some of the persons who made Brāhmī inscriptions were identified by v. HINÜBER as tribals. Two persons call themselves *jaṭṭa*, i.e. *Jāṭṣ*. One man with an unexplained name declares his intention to proceed further into the Khasa kingdom. According to the *Rājatarāṅgiṇī* (STEIN 1900/1961: 430) the Khasas were located in the hills south and west of Kashmir. But Chitral was considered as a homeland of the Khasas as well, where the name Kashkar was still known in the 19th century (TUCCI 1977: 65, 82). Maybe this Khasa-land on the soil of modern Chitral was the destination of the traveller — and this was mentioned in the inscriptions, because normal persons would not be allowed to go so far.



If Shatīal was situated near to a boundary protected by a frontier guard, then which states or tribal associations were involved?

The most perspicuous prospect of the political setting was offered by B.N. VAJNBERG (1972) using the studies of GÖBL (1967), LUKONIN (1967, 1969) and MARŠAK (1971) as a base.

The main breaks in the history of Tokharistan, formerly held by the Kuṣāns, were the campaigns of Shapur II. They caused the destruction visible in the Buddhist monastery Kara Tepe and were followed by the minting of the (so-called) Kuṣāno-Sasanian coins. The rulers portrayed were Sasanian princes invested as governors in the Kuṣān territories, preserving the titles of their famous predecessors. VAJNBERG, accepting LUKONIN's interpretation (but not in all respects), thinks that the earlier series of such coins started in 368 A.D. and lasted until the end of the 4th century A.D. In this period the main economic centres of the Sogdian homeland were under the influence of the Sasanians, but they were not incorporated into the Sasanian state. Sogdian coins of this period must be attributed to local chiefs. Therefore, there was no barrier against the infiltration of Hunnic tribes, the so-called Chionites. The Sasanians considered them as most effective allies, as we can see in the wars of Shapur II against the Roman Empire, e.g., during the siege of Amida (AMMIANUS MARCELLINUS XVIII,7/2; XIX,1/1–7).

Chionite presence in Sogdiana was longlasting. We learn that a large part of the nobility was of Chionitic origin, many of them descendants of mixed marriages (SMIRNOVA 1970: 38–69; 1981: 20–66).




According to numismatic evidence we can attribute only rather restricted periods to the Kuṣāno-Sasanians, according to VAJNBERG (1972: 150–51) between 368 and 400 and a second one between 450 and 459 A.D.

It seems, therefore, more reasonable to make a general statement and to say that the protectors of the routes from Sogdiana to the Indus were probably Chionites, who preserved their position as part of the ruling elite up to the end of the 8th century A.D.

Since the late 4th century A.D. a Chionitic federation which had occupied the eastern part of Tokharistan as well as Badakhshan



produced coins, on which HUMBACH recognized the name "Goboziko" written in Bactrian letters. Later on, the better known Hephthalites became the dominant group. In 519 A.D., the Chinese pilgrim SONG YÜN was received by the Hephthalite ruler with great honours — maybe in Badakhshan. Evidently the nobility had still preserved the ancient nomadic life-style in spite of luxurious material conditions provided by the settled population. When the Turks entered the scene, they liquidated the centralized state of the Hephthalites, who were attacked by the Sasanians as well — between 553 and 557 A.D. But in the area called Tokharistan Hephthalites remained still an important ethnic element, as reflected by the relevant chapter of the T'ang chou (translated by CHAVANNES 1903: 155–160). Hephthalite rulers were ready to collaborate with the Chinese, when the Tang had defeated the Turks; legations reached the Tang capital in 729 and 748 A.D.

That such groups were present in Shatial, at least men who felt themselves related to or protected by them, can be confirmed by "tamgas" or emblems observed on the rocks. Signs like  or  are known from the coins of the Goboziko group,  is hephthalitic.

SIMS-WILLIAMS comments that "the main importance of the material lies in the large number of personal names which it attests". It may be added that the frequent occurrence of the ethnic term XWN = Chun which is used here as a personal name means that the Chionites acted as individuals, proudly announcing their origin. This is understandable and justified, e.g., by the noble attitude of GRUMBATES and his men during the siege of Amida (AMMIANUS MARCELLINUS XVIII,6, XIX,1,2). Apparently they were well accepted by the resident population, and then slowly assimilated. This is in accordance with the observations of archaeologists in Tokharistan and Sogdiana: there is no break in the cultural development in the time between the 4th and 6th centuries A.D.

Among the rock-carvings observed in Shatial Fort we find the impressive representation of a fire-altar, rather low and broad, with two horns and a central funnel-shaped spout.

The shape is similar, but not identical, with that seen on coins of the Iranian Huns published by GÖBL as emission 174 (1967, III:



Pl. 38, 39; I: 122–125). There, such compact fire-altars are flanked by two attendants. Below, we see two wheels connected by a curved line like a pair of spectacles upside down. The name of the ruler mentioned on the front-side of such coins was Narendra or Naraṇa. GÖBL is convinced that such coins belong to a relatively late period in the history of the Hunnic tribes in India, between 570/80–600 A.D. Then, he concludes, defeated in the plains and even losing their grip on Kashmir, they returned via present-day Afghanistan to their northern starting position.

Maybe the occurrence of the typical fire-altar at Shatial Fort means that the Hephthalite rulers who had formerly controlled the bridgehead were now replaced by “White Huns” (Chionites) who had their base in the south. This would explain an inscription in Brāhmī observed near the fire-altar. We may add that the strange symbol on Narendra’s coins similar to a pair of spectacles was also seen near Chilās below a stūpa-carving!

There must have been disturbances even in earlier periods, and then the merchants were able to transgress the demarcation line. That may explain the minor clusters of Sogdian inscriptions in Oshibat and Thor North – and of exotic “tamgas”.

“Tamgas” were originally conceived as the owner’s marks for branding cattle. This function has been preserved to the present day among Mongolian pastoralists. But they were also used by the nomads of the first millennium A.D. as symbols for tribes, clans, families, even individuals. They appear on coins – also on those of the Sogdian towns. Such tamgas are to be seen on the rocks between Shatial and Chilās – and that will help us to identify the place of the travellers’ origin (SMIRNOVA 1970: 155–198; 1981). So far I can see a concentration in the upper part of the Zerafshan valley.

L. SANDER is a specialist for palaeography, trained by intense and longlasting studies devoted to the manuscripts of the Turfan-collection at Berlin. She contributed an important article with the modest title “Remarks on the Formal Brāhmī of Gilgit, Bāmiyān, and Khotan”. Apart from Khotan, Bāmiyān (and Mary = Merv, for this information I am obliged to Dr Sander) this script was spread in the mountains between Chitral (STEIN 1921: 37–41) and Ladakh (as recently observed). Manuscripts



belonging to this group were found in Gilgit. A further attempt to delimit the actual area of diffusion should be based on a study of the legends on late Kuṣāṇ, Kidaritic, and Chionitic coins, still a task for the future. Previously other terms had been proposed for this variety of the Brāhmī script; however, the name preferred by SANDER should be maintained in the future in order to avoid unnecessary confusion.

There are other problems. E.g., we read: "When the Mahāyāna movement became popular, it may have spread to Khotan, perhaps starting from Gilgit, a center of Mahāyānism and the Mūla-sarvāstivāda school". Here we should add the reservation that in this case "Gilgit" stands as a token for a much larger territory. My assumption that the Gilgit Manuscripts were rather produced in Baltistan is still not falsified. Baltistan had even closer connections with Kashmir than Gilgit proper — and a direct route via the Mustagh pass to Khotan.

It should be repeated that it is still an open question whether the so-called Gilgit Manuscripts were written in Gilgit. There are many reasons to accept v. HINÜBER's conclusion that the manuscripts formed the working library of a monastery which had close connection with the kings of the Paṭola Śāhi dynasty (v. HINÜBER 1983: 50). It must have been under the special protection of these rulers — and near to their residence. But Gilgit was situated in Little Palūr, and that was apparently an area which was conquered by Paṭolas, the old name being Bru-ža. A new residence was built near Hatun (CHAKRAVARTI 1953/54; JETTMAR 1981). So it seems that the main residence was rather situated somewhere in Great Palūr, further to the east, maybe in the basin of Skardu. The term "palolo" is still used as a heteronym, the inhabitants of Baltistan or "pale" are called so by their Dardic neighbours. We may assume that after the annexation of Great Palūr by the Tibetans, when the dynasty retreated to their western colony — the monks of the royal monastery followed their protectors, bringing their books with them. According to HUEI-CHA'AO, the Tibetans were still pagans, when that happened (FUCHS 1939: 443); even if this is not correct, it may reflect the situation among the soldiery.

A period of bloody battles between the invading armies — the Tibetans and the Chinese, who exiled the last Paṭola Śāhi from



his former domain to China and transformed his state into a military district (CHAVANNES 1903: 151–154) — was the next phase; then came a period of Tibetan overlordship, ending with the collapse of the empire in the middle of the 9th century A.D. Gilgit became the capital of the next dynasty, the Dārada Śāhis. It is reasonable to assume that one of them ordered the enshrinement of the manuscripts in an especially constructed stūpa — near to their own palace. After all, Śāradā was now the regular script; so the library would probably have been incomprehensible, merely a treasure with magical powers.

A further argument for the identification of the main capital is the claim of the Paṭola Śāhis mentioned in the Hatun-inscription to be “born in the lineage of Bhagadatta” (CHAKRAVARTI 1953/54: 228). The descendants of Bhagadatta (son of Naraka) apparently ruled over two different kingdoms far from each other — both called Prāgyotiṣa. One was in Assam — but the other one in the mountains north of Kashmir. When Lalitāditya Mukṭāpīḍa conquered and destroyed a town which had exactly this name, he came to an area with sand-dunes; Dāradas and Bhautṭas are mentioned in the same context, the land of the women was not far. Everyone of these details fits into my proposal (KALHAṆA, IV/168–174).

SANDER’s article has an appendix dealing with Brāhmī graffiti from Thor North, where after a short introduction, she makes most interesting observations. Checking our photos, she became aware that on one of the rocks, located on the barren plateau overlooking the Indus, just opposite the mouth of the Thor valley, there is a cluster of strange “pornographic” graffiti. When I had seen these carvings in 1979, I was also struck by these provocative images. I postponed immediate publication in order to avoid destruction by the pious and strictly moral inhabitants of the hamlet on the opposite bank of the river. The villagers — goldwashers by profession — had just started in these years to bring their cattle to the other side of the Indus for grazing, although I could hardly detect any blades of grass there between rocks, gravel and sand. But the landowning peasants in the upper part of the Thor valley had just decided to cut off the goldwashers from access to the high-meadows until they were ready



to pay the traditional taxes. The goldwashers had refused further tributes anticipating a future land reform. In the meantime, an agreement was reached, and the ferry constructed for the transfer of the cattle — which gave us access to the other side — is normally out of use. Therefore, the site Thor North is now almost inaccessible, so I do not think that this discussion will have fatal consequences for the “bold” graffiti now.

I refer to the description given by SANDER and I will not discuss whether a similar motif was already depicted in the Achaemenid period (cf. SURIEN 1979: 83 — the date is rather problematic), but propose an explanation for this rather primitive and vulgar graffito.

Sexually explicit, humorous, or rather playful clay-figurines are not rare in the collections brought together by Sir Aurel STEIN and others in the classical sites of the Tarim Basin. Many of them came from Khotan (D’JAKONOVA-SOROKIN 1960: 20–22, pl. 29). Apparently, the enlightened part of the settlers in the rich oasis (many of them merchants) had a special predilection for such toy-like items; often monkeys are the actors, instead of humans. This gave me the explanation, when I observed such “dirty graffiti” on the rocks of the site Shatial Bridge. They depict monkeys, but the upper parts of their bodies are transformed into phalli. After all, the Sogdians were sophisticated and urbane travellers as well. Some inscriptions may belong into the same category (HUMBACH 1980: 204–205).

But in Thor North the person sexually attacked (without female attributes) has prolonged earlobes, the aggressor with the large phallus is provided with an (misunderstood?) “uṣṇīṣa”. These marks can be explained by the intention to indicate that the persons involved are Buddhists — or belong even to the Buddhist pantheon. Maybe we here find an allusion to the bias for homosexuality which was imputed to monks in many cultures. So we could assume that this was meant as a taunt directed by a Sogdian, non-Buddhist merchant against his Buddhist partners.

There are two possibilities for the offended to react in such a situation: One normal reaction would be to destroy the graffito — the other one would be to retort with an even more provocative accusation. And in fact, we see on the same rock a man in a sodomising pose, characterized by the long girded coat as worn



by Iranians. Maybe there is one more graffito of the same kind – a compound reaction to the initial insult.

The inscription which gave the stimulus to deal with this group of graffiti is written in Brāhmī, and this should be a warning to connect this short text with the man who had conceived and executed the graffito. It is quite normal that such “accusations by drawings” are turned into a new direction by the later addition of a name.

I regret that MA YONG, the author of the last contribution did not live to see the printed text of his article, nor even the preprint, which appeared in the pages of “Pakistan Archaeology”.

As for the content of this important article, it seems to me an open question as to whether a delegation of the Great Wei-Dynasty would have made such an enormous detour via the Karakorum – crossing the main range (and necessarily re-crossing later on) in order to approach a town in Sogdiana. It seems more reasonable to consider the possibility that the Chinese characters transliterated and interpreted as Mi-Mi (= Maimurgh) rather mean another town not identified so far, situated somewhere in Gandhāra, in the mountains between Swat and Kashmir.