

II. HISTORICAL PERSPECTIVES

An important background for the early Greek accounts of India lies in the ancient Near Eastern awareness of South Asia. This awareness goes back to a very early period, but does not form a continuous tradition. An even more important factor was the Persian conquests in Northwestern India (now mostly Pakistan), and the various contacts of the Achaemenian period, for during this time we first find Greeks (or at least one Greek) in India and Indians in Greece, and both nations¹ meet each other in the capitals of the empire. In this period several Indian products were introduced into Greece (and in some cases there was even some vague idea about their place of origin.) In this chapter I shall discuss these factors, and thus give the outlines of the history of early contacts between India and the West and study some related problems.

1. The Ships of Meluhha

In a study devoted to the literary evidence of a much later period we can safely leave aside the question of prehistoric contacts between India and the West. Surely there were such at least with Iran, and at an early date important innovations like agriculture and pottery travelled far and wide from their original Near Eastern homelands.² The interpretation of the archaeological evidence is rather complicated – often the process seems to have been step by step without direct contact over wide distances, but there is hardly any conclusive evidence to be found. As far as our theme is concerned, the first phase of Indo-Western relations belongs to the third millennium, in the wide commercially and culturally interacting world of Sumer, Dilmun, Magan and Meluhha.

The first evidence of a western relation did not come long after the discovery of the Indus culture itself in the early 1920s. It contained some Indus seals found in Mesopotamia,³ and soon the idea of an early trade connection between Indus and Mesopotamia became established,⁴ although the important intermediaries were still mere names in

¹ The idea of an Indian *nation* at this period must of course be understood very generally. A real concept of a cultural unity (it would be misleading to speak of nations) came with the conception of Āryāvarta (see chapter VII.1.), but of course it is not the same as "India".

² See e.g. Allchin & Allchin 1982, 97ff., on possible prehistoric maritime contacts, Tosi 1986.

³ Mackay 1925.

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Sumerian documents. Little by little the picture acquired both a framework and detail. Today the evidence is considerable, though it contains curious gaps. Excavations at Bahrain (ancient Dilmun), Umm an-Nar, Tepe Yahya and other sites have brought the other partners to light.⁵ As far as the seals are concerned, Indus seals have been found in Mesopotamia⁶ and in Bahrain,⁷ and both Iranian⁸ and Gulf⁹ seals have been found on Indus sites, but, perhaps surprisingly, no Mesopotamian seals have been discovered in Indus.¹⁰

The situation is much the same with other material remains. Most of the products mentioned in Sumerian sources as brought from Meluhha¹¹ are of the kind which rarely leave any archaeological remains (e. g. timber). Yet there are many kinds of Indus products and artefacts – such as Indus stone weights¹² and etched carnelian beads¹³ – found in Iran,¹⁴ Bahrain¹⁵ and Mesopotamia.¹⁶ Common features have also been suggested in various techniques used in these countries.¹⁷ But again there has been very

⁴ The early finds were discussed especially by Gadd (1932), Frankfort (1934) and Childe (1939). A major defect with these studies was that the so-called Gulf seals found in Mesopotamia but originally coming from the then archaeologically nearly unknown Bahrain, were thought to be from Indus (see Buchanan 1967, 104ff., Bibby 1970, 189f. and Mitchell 1986)

⁵ For Bahrain see Bibby 1970 and Al Khalifa & Rice 1986, for Umm an-Nar Frifelt 1975, 359ff. For Tepe Yahya Lamberg-Karlovsky 1972, 229 contains references to his more specialized studies on the site.

⁶ See the summaries in During Caspers 1972a, 178ff., Lamberg-Karlovsky 1972, 223f., Ratnagar 1981, 190ff., Brunswig & Parpola & Potts 1983 and Mitchell 1986.

⁷ Bibby 1970, 191ff.

⁸ Seal M 353 in Joshi & Parpola 1987.

⁹ Seal L 123 in Joshi & Parpola 1987. Cf. *op. cit.* XIII (with further references). See also Ratnagar 1981, 194ff. On Gulf seals in general, see Kjærum 1986.

¹⁰ Perhaps the idea of a cylinder seal was introduced from Mesopotamia. In addition to the common stamp seal, cylinder seals were also occasionally used in Indus cities as the finds in Mohenjo-daro (M 418 and 419 in Joshi & Parpola 1987) and Kalibangan (K 65) show; see also Ratnagar 1981, 193f. and Collon 1988. Parpola (Joshi & Parpola 1987, XV) derives these Indus cylinders from similar NE Iranian seals, but Collon seems to connect these too with the overland route from Mesopotamia to India (but then this does not necessarily mean *direct* contact between Mesopotamia and India). For possible further seals from Harappa (H 368 and 369) see Joshi & Parpola 1987, XXIX. There are two further late examples from Daimabad (Dmd 4) and Maski (Msk 1).

¹¹ See the summary in Pettinato 1972, 162 – 166. The various items of trade are discussed very fully in Ratnagar 1981, 78ff.

¹² These are found in Mesopotamia and Susa (During Caspers 1979, 125f.) and especially in Bahrain, where the Indus standard seems to have been in use (see Bibby 1970, 372, 375ff. and plate 18B). See also Ratnagar 1981, 184ff.

¹³ During Caspers 1972a, 188 and Ratnagar 1981, 128ff.

¹⁴ Especially at Tepe Yahya, excavated and studied by Lamberg-Karlovsky, see e.g. Lamberg-Karlovsky 1972, 226ff. The main contribution of Tepe-Yahya to the trade was the local so-called steatite vessels found both in Mesopotamia and the Gulf region, and in Indus culture sites (see Baudot 1987). For Susa and other prehistoric sites of Iran, see During Caspers 1972a, 187f. and Lamberg-Karlovsky 1972, 225f.

¹⁵ See During Caspers 1972a, 172 and 176 with further references.

¹⁶ For instance, the three figurines from Nippur, and Indus style cubical dice from several sites, see Lamberg-Karlovsky 1972, 225f. and During Caspers 1972a, 189f.

¹⁷ For an early study see Frankfort 1934, 6f.

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little – according to one scholar, “no incontestable finds”¹⁸ – of western origin found in India.¹⁹

Another kind of evidence comes from the common motifs of Near Eastern and Indus art and iconography.²⁰ From Mesopotamia there is also the literary evidence for the trade between Sumer and the three countries Dilmun, Magan and Meluḥḥa.²¹ After a long struggle between competing theories²² – even Africa was suggested on the grounds of much later sources – these countries are nowadays commonly identified with Bahrain,²³ Makran/Oman²⁴ and the Indus civilization. Although there seem to be some chronological difficulties with the period of this trade, the identifications themselves no longer seem to be contested.²⁵

It has also been considered possible that the names of countries are derived from their respective locations. Thus *Magan/Makan* corresponds to Achaemenian (OP) *Maka* in Gedrosia, and probably also includes the opposite Oman peninsula, too. Later through Μακαρηνη and *Makuristān* came medieval and modern Makrān.²⁶ *Meluḥḥa* may correspond to the Sanskrit *mleccha* (first suggested by Gadd) and Pāli *milakkha*, both meaning ‘barbarian’, and is perhaps originally of Dravidian derivation.²⁷

Two important trade routes seem to have been in use during the third millennium B.C. There was the land route over the Iranian Plateau,²⁸ and the probably much more

¹⁸ Lamberg-Karlovsky 1972, 224.

¹⁹ Metal pins and especially pottery have been suggested, see Childe 1939, 13, Mallowan 1970 and especially the critical discussion by Lamberg-Karlovsky (1972, 225). During Caspers (1972b, 217f. and plate xxxviiiB) mentions two gatepost amulets of Mesopotamian style from Mohenjo-daro. Fabri's (1937) attempt to read Sumerian cuneiform from some meagre remains in a piece of pottery from Mohenjo Daro is hardly convincing.

²⁰ See Frankfort 1934, 9f. and many studies by During Caspers (e.g. 1971 and 1979, 126ff.) and Parpola (e.g. 1984b, 1985a and 1985b, 26ff.).

²¹ This textual evidence is collected in Pettinato 1972.

²² Summarized in Pettinato 1972, 99ff.

²³ In fact, Dilmun seems to have included also the Arabian coast from the island of Failaka in Kuwait to the Qatar peninsula (Tosi 1986, 103).

²⁴ This identification on both sides of the Gulf of Oman provides a literary source for the important sites found both in Makran (Old Persian *Maka*) – like Tepe Yahya – and Oman. Oman here includes also the western side of the peninsula with such sites as Umm an-Nar in Abu Dhabi (see Bibby 1970, 289ff.). Archaeological finds from Oman and Abu Dhabi – their sites formed a uniform Umm an-Nar culture c. 3000 – 1000 B.C. (Frifelt 1975, 389) – attest active connections with other countries. The painted red Umm an-Nar ceramics have been found in South East Iran (and *vice versa* – Frifelt 1975, 369ff.) and Bahrain (but no Dilmun ware has been found on Oman sites – Bibby 1970, 377ff. and Frifelt 1975, 369), and a few Mesopotamian ceramics at Umm al-Nar (Frifelt 1975, 371). Indus finds from Oman sites include an egg shell of *Gallus domesticus* found at Wadi Suq near Sohar on the Oman coast (Frifelt 1975, 375, though with reservations), lapis lazuli from Umm an-Nar (Tosi 1986, 105), some Indus pottery and seals (Weisgerber 1986, 140) and a sherd with a Harappan inscription from Ra's al-Junayz (Tosi 1986, 105f.).

²⁵ See e.g. Klengel 1975, 328, Tosi 1986, 103ff. and especially Parpola & Parpola 1975, *passim*, for *Maka* also Bailey 1982, on chronology During Caspers 1984.

²⁶ Herzfeld 1968, 63 and Parpola & Parpola 1975, 206.

²⁷ Parpola & Parpola 1975, *passim*.

²⁸ See Klengel 1975, 325f. and Ratnagar 1981, 172ff., both with further references.

important sea route from the harbours of the Indus civilization (Lothal harbour has been excavated, but certainly there were more of them, also on the Indus) to those of Makran and Oman,²⁹ further to Bahrain and other sites of the Gulf culture, and ultimately to Sumerian harbours. A great part of the trade seems to have been transit trade with Bahrain, the paradisiac Dilmun³⁰ being the major entrepôt.³¹ But in spite of the scepticism of Lamberg-Karlovsky,³² it seems that part of the trade, at least in the early period,³³ was "direct contact trade". The scantiness of Mesopotamian evidence in Indus culture sites points to the conclusion that goods were mostly carried on Indus ships,³⁴ and this is exactly what the Sumerian literary sources say, viz. that the Meluḥḥan ships brought their goods directly to Mesopotamian harbours.

Among the goods brought from Meluḥḥa various kinds of wood are mentioned, as well as copper (said to be of a different quality from that from Magan), gold, silver, precious stones, ivory and ivory objects, pearls and multicoloured birds.³⁵ Further evidence for direct trade and at least some presence of Indus people in Sumerian towns is given by the seals and other finds in Mesopotamia. There is even a good possibility of a local Indus settlement actually mentioned in cuneiform sources.³⁶

After the Akkadian and Neo-Sumerian periods the trade seems to have slowly withered. The direct contact with Meluḥḥa ceased first, but Dilmun still functioned as a major entrepôt, and Meluḥḥan imports are still mentioned in cuneiform sources. In the early second millennium they disappear, and even Magan vanishes from literary sources.³⁷ Soon it was forgotten where the places mentioned in the old texts actually were, and both Magan and Meluḥḥa were relocated somewhere on the Arabian coast or even in Egypt and Nubia/Ethiopia.³⁸ Of the vanished culture of Northwestern India no

²⁹ Archaeological evidence of harbours in these regions is scanty, but the important commercial role of Oman (copper) and Tepe Yahya (steatite) clearly indicates their existence. From the Sumerian sources we know that there were ships bound for Magan (Bibby 1970, 235f.). For a criticism of the identification of Lothal as a harbour see Ratnagar 1981, 66ff. On Harappan ports on the Makran coast (like Sutkagen-dor), see Ratnagar 1981, 48ff.

³⁰ For Dilmun's religious role in Sumerian literature see e.g. Kramer 1963 (but without accepting his hypothesis), Wissmann 1975, 24 and During Caspers 1984.

³¹ For Dilmun's commercial role, see the many studies by During Caspers (e.g. 1972a, 1979 and 1984) and Weisgerber 1986.

³² Lamberg-Karlovsky 1972, *passim*.

³³ It is outside my competence to take any stand on the question of absolute chronology.

³⁴ This much is granted by Lamberg-Karlovsky (1972, 224).

³⁵ During Caspers 1972a, 191. From the Indian point of view, the history of the early trade is discussed by Asthana (1976) and Ratnagar (1981). For further references see Baudot 1987, 3, note 6. The "multicoloured bird", called ^DH.A.J.ĀMUŠEN, is sometimes identified as the peacock (Falkenstein 1964, 75 and Pettinato 1972, 93). But there are other multicoloured birds in India. There is, for example, the Impeyan pheasant (*Lophophorus impeyanus*), held sacred by the Dards (Jettmar 1975, 217 and 378 and 1984, 75), and often confused with the peacock, both being referred to as *mayūra*, *mayūraka* in Sanskrit sources (Dave 1985, 271 and 277).

³⁶ See Parpola & Parpola & Brunswick 1977.

³⁷ This withering of trade is summarized by During Caspers (1972a, 191).

³⁸ Kühne 1976, 102.

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memories remained. More than a millennium followed with apparently little or no contact between South Asia and the West (with the exception of Iran).³⁹ When it began again, it was a completely new story, as we shall see in a later chapter (II.4.). Prior to this, though, we must turn to the southern route⁴⁰ and the chances of an early contact there.

2. King Solomon and the Gold of Ophir

The southern route from India to the West starts from the harbours of Western and Southern India, comes over the Arabian Sea – either by way of the coast or directly with the monsoon – to South Arabia, and parts there into two branches: the caravan route from South Arabia to the Near East, and the sea route to the northern end of the Red Sea. In Hellenistic and Roman times this route was used in a lively trade⁴¹ and some authorities give it a long prehistory, though the evidence is somewhat meagre. In the following I shall discuss the Red Sea route first.

During its long history Pharaonic Egypt embraced an exceptionally wide geographical perspective, but India seems to have been definitely beyond it. The Punt expeditions to what is now Ethiopia and Somalia began during the Old Kingdom (5th dynasty) and were continued with few interruptions for some two millennia. South Arabia was probably also visited, but there is no evidence at all that Egyptians ever went beyond this.⁴² The few products that might have come from India were probably acquired either through Near Eastern agency (hump-backed cattle)⁴³ or were of independent African origin (cotton).⁴⁴

But there were more sails in the Red Sea. Some of them belonged to the Phoenicians, who some time in the early 10th century B.C. had a commission from King Solomon. Two well-known Biblical passages⁴⁵ tell of their venture, though the phrasing is vague

³⁹ For Iran see e.g. Witzel 1980.

⁴⁰ For the sake of convenience I use the terms northern route (including both the land route and the sea route to the Gulf) and southern route (the sea route to South Arabia and the Red Sea) to indicate the major channels where contact was made. It is easy to discern between these two, but we cannot always say if some particular connection between India and, say, Mesopotamia took place by way of land or sea.

⁴¹ The classic study is Warmington 1928, now also e.g. Raschke 1978, Sidebotham 1986 and Eggermont 1988.

⁴² For the Punt expeditions see Hennig 1944, 5ff., Thomson 1948, 6ff. and Delbrück 1956, 9f.

⁴³ Berzina 1982, 17f.

⁴⁴ Berzina 1982, 18f. In an apparently still unpublished article known to me from a summary (Dixon 1988), Dixon confirms what I have said on Egypt: that in Pharaonic times no direct relations existed between Egypt and the area beyond the Strait of Bab el Mandeb. In Egypt there were no religious ideas or artistic motifs that can with confidence be ascribed to influences from South Asia. This situation changed only after the incorporation of Egypt by Cambyses.

⁴⁵ I Kings 9:26 – 10:14, especially 10:11: *And the navy also of Hiram, that brought gold from Ophir,*

and this has led to a long dispute. The crucial point is where does their destination lie – the place called in Hebrew *Ōphīr* (אֹפִיר), in Greek (of *Septuaginta* and Josephus) Σώφειρ or Σώφαρα or something like?⁴⁶ Is it in India or somewhere else? What I have to say has been said long ago, but I think it must be said again. Solomon's Ophir was not in India. The words Clark wrote more than sixty years ago, still contain the essence of the matter: "All the facts here stated have long been known, but many recent books and articles have disregarded the essential points of the problem and have been misled by popular but antiquated discussions in the pursuit of will-o'-the-wisps of etymologies. Anything can be proved by the judicious use of etymologies and the fortuitous resemblances of words from different languages."⁴⁷

In the 19th century, however, it all seemed clear and fine.⁴⁸ Ophir was indeed in India. Occasional critical voices were for the most part ignored and so the Indian trade of king Solomon became part of Indological folklore, mentioned carelessly in introductions and footnotes with no thought about reliability.⁴⁹ During this study we shall encounter (but I hope not share) other similar beliefs.

The original culprits are Josephus and before him the translators who made the Greek version of II Chronicles in *Septuaginta*. Further Biblical passages are involved,⁵⁰ speaking either of the ships of Taršiš or of sailing to Taršiš and bringing back, among other things, something called (with Hebrew plurals) *šenhabbīm*, *qophīm* and *thukki-yīm*. These were rendered in *Septuaginta* first as λίθων τορευτῶν καὶ πελεκητῶν (I Kings), then as ὀδόντων ἐλεφαντίνων καὶ πιθήκων (II Chron.) and only in one, according to Clark a none too reliable old manuscript, as ὀδόντων ἐλεφαντίνων καὶ πιθήκων καὶ τᾶνων (I Kings). Even the last mentioned manuscript leaves the peacocks out in II Chron.⁵¹ Similarly, the tree brought from Ophir, for which even the Hebrew text had the variants *almuggīm* and *alummīm*, was rendered in different ways as ξύλα πελεκητά (I Kings) and ξύλα πεύκινα (II Chron.).⁵² According to Clark,

brought in from Ophir great plenty of almuq trees, and precious stones. II Chron. 8:17 – 9:13, especially 9:10: *And the servants also of Hiram, and the servants of Solomon, which brought gold from Ophir, brought almuq trees, and precious stones.* The Phoenician king who gave his navy to help Solomon and is called Hiram in the Authorized Version, was Ḥīram I of Tyrus (Wissmann 1975, 54). From I Kings 9:26 and II Chron. 8:17 we learn that the harbour used was Ezion-geber (ʿEšyōn Geber) by the Gulf of Aqaba (cf. Wissmann 1975, 54).

⁴⁶ There are several textual variants.

⁴⁷ Clark 1920, 103.

⁴⁸ See e.g. Lassen 1847, 537ff. and 1852, 552ff. and Pullé 1901, 5f.

⁴⁹ After Clark's final refutation the error of Ophir in India or at least Indian products brought from Ophir has still been perpetrated e.g. by Rawlinson (1926, 10f.), Basham (1954, 230), Miller (1969, 261ff.) and, though with some reserve, by Sedlar (1980, 5), as well as by many Indian scholars (e.g. Saletore 1975, see index s.v. *Ophir*). See also the references given in Thomson 1948, 30, note 1.

⁵⁰ I Kings 10:22: *For the king had at sea a navy of Tharshish with the navy of Hiram: once in three years came the navy of Tharshish, bringing gold, and silver, ivory, and apes, and peacocks.* II Chron. 9:21: *For the king's ships went to Tarshish with the servants of Hiram: every three years once came the ships of Tarshish bringing gold, and silver, ivory, and apes, and peacocks.* As will soon be seen, the apes and peacocks of the A. V. are far from reliable translations.

⁵¹ All quoted from Clark 1920, 107.

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this discrepancy means that there was no ancient tradition about the real meaning of the Hebrew words.⁵³

It was Josephus in the first century A.D. who wrote that Σώφειρα lies in India,⁵⁴ and even in *India extra Gangem*, because he identified it with the famous χρυσή, commonly thought to correspond to the Suvarṇadvīpa of the Indian sources and located also in Southeast Asia.⁵⁵ In the first century A.D., India had for a long time been famed as a rich gold country *par excellence*, and in Josephus' work we find yet another Indian identification, which subsequently became famous in Christian cosmographies. It was he who tells that the river Πισδὼν of Paradise was none other than the Ganges.⁵⁶ But in a note to the Ταρσιθ passage he says that the ships sailed εἰς τὰ ἐνδοτέρω τῶν ἐθνῶν and brought ἐλέφας Αἰθίοπες τε καὶ πίθηκοι without even mentioning India or peacocks.⁵⁷

According to the old survey of Gesenius, early scholars were greatly interested in the Ophir question.⁵⁸ During the 16th to 18th centuries Ophir has been sought variously in Armenia, Phrygia, Iberia and even Peru (by several 16th century scholars). Learned opinion also supported Columbus when he recounted that he had found Solomon's mines in Hispaniola. The name of the Solomon Islands reminds us even today of another early far-fetched theory.⁵⁹ Yet the three major theories located Ophir either in India, Arabia or East Africa. For us only the Indian theory is relevant at present.

The foundation for the theory was of course Josephus, backed up by later material from Coptic and Arabic apparently "confirming" that Ophir really was in India.⁶⁰ The Ταρσιθ passages were without hesitation said to refer to Ophir and the *Septuaginta* interpretations of the products seemed to gain support from Indian etymologies. Perhaps the first to mention them was the anonymous missionary account published at Halle 1735. There it was stated that Hebrew *Koph* and *Tūcki* come from "Grendisch" (Grantha, i.e. Sanskrit) *Kapi* and "alt-Malabarisch" (Old Tamil) *Tōgei* (i. e. *tōkai*).⁶¹ Both etymolo-

⁵² Clark 1920, 104.

⁵³ Clark 1920, 108.

⁵⁴ *Ant.* 8, 164: ἄνδρας γὰρ αὐτῷ (scil. Hiram το Solomon) κυβερνήτας καὶ τῶν θαλασσίων ἐπιστήμονας ἔπεμψεν ἱκανούς, οἷς ἐκέλευσε πλεύσαντας μετὰ καὶ τῶν ἰδίων οἰκονόμων εἰς τὴν πάλαι μὲν Σώφειραν νῦν δὲ χρυσὴν γῆν καλουμένην (τῆς Ἰνδικῆς ἐστὶν αὕτη) χρυσὸν αὐτῷ κομίσαι.

⁵⁵ See e.g. Cœdès 1910. xvii.

⁵⁶ *Ant.* 1, 38: καὶ φεισῶν μὲν, σημαίνει δὲ πληθὺν τοῦνομα, ἐπὶ τὴν Ἰνδικὴν φερόμενος ἐκδίδωσιν εἰς τὸ πέλαγος ὑφ' Ἑλλήνων Γάγγης λεγόμενος.

⁵⁷ *Ant.* 8, 181: πολλὰ γὰρ ἦσαν νῆες, ἃς ὁ βασιλεὺς ἐν τῇ Ταρσιθικῇ λεγομένη θαλάττῃ καταστήσας παραγαεῖν εἰς τὰ ἐνδοτέρω τῶν ἐθνῶν παντοίαν ἐμπορίαν προσέταξεν, ὧν ἐξεμπολουμένων ἄργυρός τε καὶ χρυσὸς ἐκομίζετο τῷ βασιλεῖ καὶ πολὺς ἐλέφας Αἰθίοπες τε καὶ πίθηκοι. τὸν δὲ πλοῦν ἀπιούσαι τε καὶ ἐπανερχόμεναι τρισὶν ἔτεσιν ἤνυσον.

⁵⁸ Gesenius 1833, 201f. This interest even reached Finland and we can note as examples two dissertations published in the old University of Turku (Wanochius 1688 and Hahn 1707).

⁵⁹ Hennig 1944, 32 remarks that the origin of the name probably did not refer to King Solomon.

⁶⁰ Gesenius 1833, 202. In Coptic *Sophir* means India.

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gies were accepted by many scholars,⁶² and especially the peacock has had a long life despite the serious difficulties involved.⁶³ There are serious grounds both for emending the word to *sukkiyīm*, a word mentioned in II Chron. 12:3 and rendered in *Septuaginta* as Τρογλοδύται, a people living on the west coast of the Red Sea,⁶⁴ and for deleting it as an interpolation of the period when peacocks were known about.⁶⁵

More Indian etymologies have been suggested. The etymology of *šenhabbīm* as "tooth of *ha-aba*", explained as Sanskrit *ibha* preceded by the Hebrew article *ha*,⁶⁶ was hardly convincing even in the 19th century, yet it was accepted by many.⁶⁷ As to the *almug* tree, it was identified by Celsius (1748) as sandalwood,⁶⁸ and Lassen knew to give a Sanskrit etymology for it, although his *valguka*⁶⁹ is attested for sandalwood only by late lexicographers and involves the worse variant for the Hebrew.⁷⁰ But there are also several Biblical passages discussed by Clark mentioning the *almug* as a name for more familiar trees, and if we dismiss Lassen's rather unlikely etymology there is no real evidence for its identification as sandalwood.⁷¹ It has been known for a long time that both *qophīm* and *šenhabbīm* can equally well be derived from the Old Egyptian spoken so much nearer,⁷² rendering the Indian etymologies far-fetched and unlikely.

This is not a place to decide where the real Ophir was situated, there are probably good reasons to search for it somewhere in Arabia.⁷³ Anyway, the old theory of its being in India can be definitely dismissed. Several ancient Indian place-names like Sopara, Abhīra, and Sauvīra have been compared with Ophir,⁷⁴ but there are also Sofala in East Africa,⁷⁵ Afar in Ethiopian coast,⁷⁶ and several place-names in various parts of

⁶¹ Francken 1735, 2, 428. In an earlier volume (1730) it was told that these accounts are written by N. Dal, M. Bosse, C. F. Pressier and C. T. Walther, all Protestant missionaries from Halle in Tranquebar.

⁶² E.g. Gesenius 1833, 202 and Rawlinson 1926, 11. Laufer (1913, 539) called the second one "durchaus gesichert und annehmbar", while Benfey (1840, 26) and Lassen (1847, 538) derived both *thukkiyīm* and *tōkai* from Sanskrit *sikhin*. At least some criticism from the Indological side was expressed by Weber (1857, 73ff.). Later he wholly rejected the Sanskrit derivation (*sikhin*) and held *togēi* to be pure "dekhanisch" and the origin of *thukkiyīm* (Weber 1871, 622, note 3).

⁶³ Unfortunately, I too mention it approvingly in Karttunen 1986b, 190.

⁶⁴ Clark 1920, 107. Josephus (*Ant.* 8, 181) renders *thukkiyīm* as Ethiopians.

⁶⁵ Clark 1920, 118. See also the criticism in Lévi 1914 (1937, 288f.).

⁶⁶ Benary 1831, 762f.

⁶⁷ The words for the elephant and their etymologies will be discussed in more detail in chapter IV.1.

⁶⁸ Clark 1920, 106 (with misprint Celsius).

⁶⁹ Lassen 1847, 538. Before him there were other, still less convincing attempts at a Sanskrit etymology (cf. Benfey 1840, 25f.).

⁷⁰ See the criticism in Clark 1920, 106.

⁷¹ Clark 1920, 107.

⁷² Even knowing these Old Egyptian derivations, Rawlinson (1926, 13) tried to save the Indian theory claiming that the Egyptians had borrowed names for these familiar African animals from far-off India.

⁷³ See e.g. Wissmann 1975, 54ff.

⁷⁴ Sopara in Gesenius 1833, 202 and Benfey 1840, 27f., Abhīra in Lassen 1847, 539 and Sauvīra in Cunningham 1891, 4.

⁷⁵ Yet, in spite of *King Solomon's Mines*, East Africa seems to be a bad guess. See Hennig 1944, 32ff. and Thomson 1948, 30.

⁷⁶ Thomson 1948, 30 with further references.

Arabia.⁷⁷ Probably similar names could be found in Somalia and in Nubia, but in any case these are mere "Spielereien, die uns nicht weiter bringen!"⁷⁸

The name Ophir as such is therefore wholly unconvincing. Josephus' old identification with India (or Southeast Asia – Chryse) can be dismissed as a mere anachronism. The sandalwood identification is unfounded. As to the ivory, apes and peacocks, they are mentioned in a passage in which Ophir is not mentioned and perhaps not meant at all.⁷⁹ And even if it was, ivory and apes were easily available in nearby Africa. Thus, we are left with the peacock, which could have been written in error when Ethiopian slaves are really intended, or the name of some African bird,⁸⁰ or even an interpolation of a time when India and peacocks were already known about.⁸¹ Anyway, a solitary and dubious Tamil etymology can hardly be considered conclusive. Thus, there is little reason to search for Ophir in India, and even the idea of Ophir as an entrepôt for Indian wares somewhere in South Arabia or Oman⁸² is unnecessary.⁸³ In this respect we must keep in mind the great antiquity of the Ophir accounts; during a later age there was such an entrepôt or several entrepôts. But this discussion requires a new chapter.

3. Incense and Aromatics

The beginnings of the trade between India and South Arabia lie in total darkness. Our first piece of unambiguous evidence only comes in the late second century B.C., when Ptolemaic officials fished up a half-dead shipwrecked Indian in the Red Sea and learnt from him how to sail from Egypt to India.⁸⁴ From Strabo's account we learn that up to this date the way of sailing there was apparently unknown in Egypt. The Indian products to be found in Egypt before this⁸⁵ were probably obtained through trade, using either some form of the northern route or the caravan routes from South Arabian harbours.

The story of the shipwrecked Indian shows that in India the sea route was known, and

⁷⁷ Wissmann 1975, 57 mentions several.

⁷⁸ Hennig 1944, 29.

⁷⁹ See Wissmann 1975, 86ff.

⁸⁰ According to Hennig (1944, 38) this idea was first put forward by Quatremère in 1845. See also Wissmann 1975, 87.

⁸¹ The history of the peacock in the West will be taken up again in a later chapter.

⁸² Rawlinson 1926, 12 (but see also Tosi 1986).

⁸³ For the discussion on Ophir's possible location somewhere in Arabia or Northeastern Africa see Hennig 1944, 28ff., Thomson 1948, 29f. and Wissmann 1975, 54ff., all with many further references.

⁸⁴ Posidonius apud Strabo 2, 3, 4, p. 98, cf. Hennig 1944, 271.

⁸⁵ Indian ivory and spices, Indian dogs and cattle, Indian girls etc., cf. Sedlar 1980, 88 and Berzina 1982, 27.

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apparently in South Arabia, too. In classical literature we find many accounts of the commercial activity of South Arabians, though their Indian trade is attested only in a later period. This trade clearly preceded that of Egypt,⁸⁶ but by how much? In the second century B.C. trade relations were apparently established between South Arabia and India, but when did they begin?

It is often thought that a passage of Herodotus is a kind of indirect proof of the great antiquity of this trade⁸⁷ (at least we can now leave aside the Ophir trade also often mentioned in this connection). The aromatics concerned, cassia and cinnamon, were certainly known in the West rather early. *Κασία* is mentioned for the first time by Sappho (c. 600 B.C.),⁸⁸ *κιννάμωμον* by Herodotus.⁸⁹ They are Indian products and they came to the West from South Arabia. It seems clear that the Indian trade was already established by then. Unfortunately, it is not as simple as that.

In his account, Herodotus said that these aromatics actually grew in Arabia. Perhaps the Arabians kept the origin of the lucrative trade a secret. We face here an old question. Is it possible that Indians and South Arabians could have had an old, established trade relation, and kept it secret so well that nobody guessed the real (Indian) origin of the products sold as Arabian?⁹⁰ To this, there can be no definite answer, I am afraid, without new evidence. Those who do not believe in secrets kept successfully for centuries, have to deal with the problem that cinnamon grows only in South Asia, but then they have suggested that it could also grow or at least have sometimes grown in South Arabia or Somalia. The climate is apparently suitable, related Laureaceans are known to grow there, and both countries are very inadequately surveyed botanically.

Thus it was stated by Delbrück,⁹¹ and it sounds quite plausible. But when one finds the idea similarly proposed by Bunbury,⁹² one is bound to ask if the countries are still white specks on botanical maps. Of course they are not. An article by Schoff, apparently unknown to Delbrück, gives the answers. The statement that the countries are mostly unexplored goes back to the 1840s,⁹³ when it was true enough. After this, however, there was the colonial period, and in 1920 Schoff could quote several botanical studies made by British and Italian botanists in their respective Somalian colonies. No cinnamon was found.⁹⁴ Probably the same is true for South Yemen which for a long time belonged to the British Empire, and even Yemen is not unexplored (actually the first European botanist in Yemen was a Finn named Forsskåhl, who was there already in 1763).

The problem is even more complicated. Not only Herodotus but, as it seems, every

⁸⁶ This is expressly stated in *Periplus* 26.

⁸⁷ Hdt 3, 107 and 110f.

⁸⁸ F 44 (Lobel & Page) *μύρρα καὶ κασία λίβανός τ' ὄνεμείχλυτο*. Delbrück (1956, 17f.) remarks that Sappho could perhaps have it from Naucratis (Egypt), where her brother was living.

⁸⁹ Frisk ss.vv.

⁹⁰ The idea has been approved by many scholars, e.g. Hennig 1944, 280.

⁹¹ Delbrück 1956, 18.

⁹² Bunbury 1879, 608f.

⁹³ Cooley in *JRAS* 19, 1849, 166ff., quoted in Schoff 1920, 267f.

⁹⁴ An Italian botanical expedition to Somalia found no Laurel varieties at all and several British scholars he consulted also gave negative answers (Schoff 1920, 262f.).

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later classical author was ignorant of the Asian origin of the aromatics.⁹⁵ Even the well-informed author of the *Periplus* traded in cassia (cinnamon is not mentioned at all) only at the southern end of the Red Sea.⁹⁶ Repeatedly, classical authors stated that both plants grew either in South Arabia or in Northeastern Africa. However, I still find it difficult to believe in an Arabian or African cinnamon which for so long was an important commodity in a lucrative trade and then was either forgotten or disappeared altogether. I cannot accept Huntingford's hypothesis that the original cinnamon plantations of Somalia were wiped out by some plant disease.⁹⁷ Such things can happen, it is true, as the Ceylon coffee disease and the vine disease of Europe can testify. But it is too hazardous a method to presume an extinct population of a plant or animal in order to make the facts fit easier with the texts – especially when the texts are as vague as they are here, and there is no other evidence than them for the supposed extinct population.⁹⁸

On the other hand, it is as hard to believe in secrets kept successfully for centuries. Is it then necessary to assume that this secret really was kept so strictly? This could hardly be the case in the times of Darius, when at least two expeditions sailed around Arabia.⁹⁹ There may well have been more,¹⁰⁰ and probably there were also reports of these sailings, for instance in Achaemenian archives. It is even possible that the sailings around Arabia were arranged partly in order to bring the cinnamon trade under control. However, after Darius the Persian government was no longer interested in sea enterprises,¹⁰¹ and even the memory of such enterprises seems to have withered. When Herodotus was collecting information he asked questions, he did not rummage in dusty old archives, perhaps written in a language which he could not understand and which had fallen into oblivion a long time ago. In this particular case his obvious ignorance of the facts, and the wholly legendary character of his account of Southern Arabia seems to be sufficient proof of this.

There is among scholars a tendency to think too much in Greek terms. It was not necessarily a completely hidden secret if the Greeks – who were not yet greatly interested – did not know the secret. Later, in Hellenistic and Roman periods, the question becomes

⁹⁵ See the references in Olck 1899, 1641.

⁹⁶ Cf. McCrindle 1879, 18f. on *Periplus* 8–9 and 12. It should be noted that in Arabian literature cinnamon is mentioned as being Chinese (Olck 1899, 1642f.), but see Laufer 1919, 541ff.

⁹⁷ Huntingford 1980, 134.

⁹⁸ We can also add that according to botanists quoted by Schoff (1920, 262), the soil in Somalia is wholly unsuitable for the cinnamon tree and its relative, cassia.

⁹⁹ The expedition in which Scylax participated, and another mentioned in the Suez inscriptions (Schiwek 1962, 15f.). The sea connection between Egypt and India is perhaps also suggested by the fact that the Egyptian stele found in Susa lists among the southern subjects of Darius Egyptians, Libyans, Nubians, Makas and Indians (Hindu). See Yoyotte 1972, 258f.

¹⁰⁰ Our evidence is very scanty, and only a misguided wish that we should somehow have a complete picture motivates such ideas that Scylax and his companions sailed back to Persia on the expedition mentioned in the Suez inscription (as suggested by Schiwek 1962, 15f.). That there was already some traffic between Mesopotamia and the Red Sea before the Achaemenian period is seen by the fact that the old toponyms Magan and Meluhha was reinterpreted as situated on the Red Sea coast (Herzfeld 1968, 81).

¹⁰¹ Schiwek 1962, 19f.

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more complicated, but even here we can note the vagueness of most accounts. Only a few later authors had any idea of the plants themselves. It seems also that several other products were called cassia and sold as cassia, and it was not always clearly known which was the real thing. Therefore we cannot be too sure, either, whether Sappho and Herodotus associated cinnamon and cassia with the same spices as we do.

In the light of the above it does not seem possible to give a definite answer. We do not know when the trade between South Arabia and India began or if it had already begun in the times of Herodotus (or Sappho). There is not much evidence either for it or against it. The possibility of early trade relations is by no means ruled out. It seems that our knowledge of South Arabia in general, before the period corresponding to Hellenism in the West, is meagre enough.¹⁰² Some time before the late second century B.C. the trade began, but we do not know for certain when it occurred. Another question which cannot easily be solved (and I shall not attempt to solve it here) is whether or when these merchants used the monsoon wind, or were they just coasting.

When the trade really did begin, whenever that was, the ancient incense road leading from South Arabia to the Near Eastern markets was already in existence, established at least in the late second millennium B.C.¹⁰³ An intensive trade in incense and other wares had already started and Arab camel caravans travelled to and fro. When the Indian exports began to arrive at South Arabian harbours, they could thus find their way to Western markets with no difficulty. However, we only find such products (at least in any considerable quantity) in the Achaemenian period,¹⁰⁴ when there were certainly other and better attested trade routes. If the South Arabian trade to India really was of great antiquity, its volume was probably meagre, or many of the imports from India were only for local consumption in South Arabia. In any case, the southern route did not bring any knowledge of India to the West before the Hellenistic period, and therefore does not interest us further.

4. Mesopotamia and the Re-establishment of the Northern Route

In the histories of the western relations of ancient India it has become a commonplace to mention the so-called Mitanni Aryans, i. e. the Indo-Aryan names and words found in the Nuzi documents, in the Hittite text on horse training by Kikkuli, and especially in the treaty between Hittite Šuppiluliuma and Mattiṣadza of Mitanni.¹⁰⁵ These documents from

¹⁰² See Wissmann 1975, *passim*.

¹⁰³ Wissmann 1975, 53f.

¹⁰⁴ In a later chapter it will be seen that several Indian products came to Greece for the first time in the fifth century B.C.

¹⁰⁵ See e.g. Mayrhofer 1966. The debate started by Mayrhofer's book does not concern us here.

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the second millennium B.C. unquestionably contain some Indo-Aryan elements such as several Vedic gods and numerals in very archaic form.¹⁰⁶ But if this is taken as evidence of trade relations between the ancient Near East and India,¹⁰⁷ it is clearly mere speculation. The context of the scanty material points clearly to a very small Aryan (or Aryanized) class of people among the Hurrian Mitannians, soldiers of fortune and perhaps also horsekeepers who had become princes. Their appearance in the West at so early a date, and the archaic features of their linguistic remains, clearly points to the most generally accepted hypothesis, that they had separated from the main body of Indo-Aryans before their entering into India.¹⁰⁸ A solitary peacock motif¹⁰⁹ is inadequate as a counter-argument, even if its identification is correct.¹¹⁰

In the case of Mesopotamia, we must once again draw a line between empty speculation, possible theories and real evidence. The contact with India is a possibility. There are two routes – the overland one through Bactria and the sea route – and the distances were by no means too great to be covered by merchants and other people, even by primitive means. In the early period, as we have seen, there were contacts using both of these routes, and at least in the middle of the first millennium B.C. the contact was opened again. But it also seems that during the second millennium there was no direct contact, at least we do not have much evidence for it.¹¹¹ The oblivion into which the real geographical significance of the old toponym *Meluhha* fell is a likely proof against any such contact. A solitary journey of a single group like the ancestors of the Mitanni Aryans over wide distances¹¹² did not bring much knowledge about distant countries in as much as there was no regular traffic. The question is, when was the contact re-established between Mesopotamia and India, and which was the route that was used.

It is to be regretted that Kennedy's learned but now outdated study¹¹³ has become an authority on this question. Much of his so-called evidence comes from early excavations

¹⁰⁶ Thus, for instance, *aikaṣartana* in Kikkuli as opposed to OIA *ekavartana*, seems to indicate an earlier date than the RV, or perhaps it is just a more archaic dialect.

¹⁰⁷ Thus e.g. Rawlinson 1926, 2 and Saletore 1975, 38f. and 45f. The Vedic (i. e. Indo-Aryan) gods have led Nilakanta Sastri (1959, 43) to the suggestion that the Mitanni Aryans probably even had the RV with them.

¹⁰⁸ As the idea of Mitanni-Indian contacts is sometimes cherished among Indian scholars I give as a reference the apt summary of Dandekar (1969, 61ff.).

¹⁰⁹ Pointed out by Brentjes (1981b, further 1988), but see also Schmidt 1980, 45.

¹¹⁰ There are also other ways for a peacock motif to appear in Mitanni, as will be seen later.

¹¹¹ Contacts were frequent between the Near East-Iran and Bactria-Central Asia, for instance in the Proto-Elamite period (summary with references in Parpola 1988, 203f.), and again the case of the Mitanni Aryans (Parpola 1988, 232ff. and Brentjes 1988).

¹¹² I have left Kassites out on purpose. The evidence of their possible Indo-European (not to speak of Indo-Aryan) origin is slight, and can also be explained in other ways. What is left after a critical examination (like Herzfeld 1968, 164ff.) is the word *Šuriasš* 'sun', a *hapax legomenon*. This is easy to compare with OIA *Sūrya*, but a solitary etymology proves nothing. Yet the possibility is still there, however.

¹¹³ Kennedy 1898. Happily, for scholars following him as an authority, he did not make any wild speculations about the antiquity of the contact he tried to prove, and proposed that it began c. 700 B.C. (ibid. 242). Thus, even with unreliable arguments (like the Chinese evidence mentioned in 265f.) he offered a rather reasonable conclusion. Cf. also Pullé 1901, 2f.

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made without sound documentation – not to speak of a stratigraphy – and often it cannot be confirmed. In addition, he proposed what were reasonable hypotheses ninety years ago, but they are now not much better than wild speculation. Often our more recent evidence can be used to reverse his ideas.

From the Achaemenian period there is a large amount of evidence of many kinds, but for the period before it we are in a much worse situation, although not totally bereft of evidence. There are good grounds for dividing our discussion into the three well-established periods of political history: Assyrian, Neo-Babylonian and Achaemenian. Not being an Assyriologist, I cannot discuss fully all the points that are sometimes given as evidence of contact,¹¹⁴ but even the points where there seems to be some consensus among the scholars seem to give a good picture of the development of the contact.

Our first piece of (possible) evidence seems to be the so-called obelisk of Shalmaneser III (858 – 824 B.C.).¹¹⁵ Among the tributes brought from Musri (not incontrovertibly identified)¹¹⁶ a horned animal, a long-tailed monkey, a Bactrian camel and an Indian elephant are depicted.¹¹⁷ The elephant has led several scholars to suppose an Indian origin, but, unfortunately, there were still elephants in Syria, too. Tiglathpileser I (1115 – 1077 B.C.) hunted elephants there and Ashurnasirpal II (883 – 859 B.C.) had them in his capital.¹¹⁸ In the tomb of Rehmire at Thebes (in Egypt) a Syrian elephant is depicted and it is clearly of Indian type.¹¹⁹

It has been suggested that the horned is an Indian rhinoceros,¹²⁰ but it might equally well be a bull. Monkeys are not rare enough to make an explicit link with India. Thus we are left with only the camel, which belongs clearly to the two-humped Bactrian species.¹²¹ But though the Bactrian camel clearly indicates the east, it does not necessarily indicate India, and cannot easily be thought of as having a common origin with the Indian elephant, that is if the latter was brought from India. The elephant could also be of the Syrian variety. In this case, the picture on the obelisk seems to be the last evidence of the Syrian elephant we have, but this does not need to bother us much as the father of Shalmaneser had “herds” of them. If the elephant was from India, there are several possibilities. If it came by the land route, middlemen may have been involved. In this case the camel is easily explained, but elephants and monkeys are not likely to be traded by way of

¹¹⁴ Such points are listed e.g. in Nilakanta Sastri 1939, 25f. and Saletore 1975, 42ff.

¹¹⁵ Names and dates of the Mesopotamian rulers are given according to the chronological tables in Roux 1964.

¹¹⁶ There are several countries called something like *Musri* (and of course, with emphatic *s*, Egypt) in cuneiform sources (oral information by Ms. Mattila). Walser (1966, 13, note 9) plainly states: “Das Land Musri ist nicht sicher zu lokalisieren, kann aber keinesfalls Indien sein.”

¹¹⁷ Kennedy 1898, 260. A drawing of the side which includes the elephant is given by Walser (1966, 17).

¹¹⁸ Brentjes 1961, 16 and Scullard 1974, 29. Barnett (1948, 1, note 4) mentions that “in 1938 Mr. Dollman of the Natural History Museum, South Kensington, when shown some specimens of ivory from the collections of Nimrud considered they were of Indian ivory”.

¹¹⁹ Brentjes 1961, 16 and Scullard 1974, 27f.

¹²⁰ E.g. in Kennedy 1898, 260 and Keller 1909, 373.

¹²¹ Bactrian camels were also included in the tributes sent by King Sua of Gilzan in the same relief (Walser 1966, 12).

land. If Musri lies somewhere near the Gulf, the sea route and direct contact¹²² could also have been involved. But there is no further evidence for direct contact in the ninth century.

Our next case is uncertain, too. Only from the Achaemenian period is there unambiguous evidence of peacocks brought from India to the West,¹²³ although the bird may have been known already in the third millennium.¹²⁴ In the period between, when it is more or less wholly absent, there is only the peacock motif in Mitanni, which has already been mentioned. Now, in the annals of Tiglathpileser III (745 – 727 B.C.) for the year 738 “birds of heaven with blue wings” are mentioned among the tributes, which according to Meissner could mean peacocks.¹²⁵ It could, indeed, but without further evidence we can only posit it as a hypothesis.

It is only with Sennacherib (704 – 681 B.C.) that we reach somewhat firmer ground. In a list of building materials brought and used for his palace at Niniveh, this monarch mentions two kinds of wood apparently brought from India or nearby. One kind, the G¹⁵si-in-da/du used for pillars and doors of the palace, seems to contain a reference to India, implied by its name, “wood from Sindhu (Sind)”. It has been identified as the holm oak (*Quercus ilex*), a tree which grows today in Pakistan, and the name is later attested in Syriac (*sedjānā*) and Arabic (*sindijān*).¹²⁶ The other wood is *mušukkannu*, Sumerian G¹⁵MEŠ.MÁ.GAN.NA, or “mēsu-wood of Magan”, imported as early as the third millennium.¹²⁷ Its origin may have been either in India or Makan (Makran and Oman Peninsula), in both cases it bears witness to sea-trade. Gershevitch¹²⁸ identifies it with OP *yakā* wood and *Dalbergia Sissoo* Roxb. This trade in wood continued in the Achaemenian period, though thinking of the barrenness of these countries today it is hard to imagine.

The Assyrians were not interested in the sea, and if the sea-trade had already begun, Assyria was probably not directly involved. It was the Babylonians rather than the Assyrians who took an active interest in sea-trade.¹²⁹ The land route would be more likely for Assyrians, but we are in the dark as to when the route began. We know that Assyrians traded with Media (the Hamadan region), and Sennacherib led a campaign in Luristan,¹³⁰ but what we do not know is how far east Assyrian interests extended.¹³¹ Some think that the land route to India was opened only in the Achaemenian period,¹³²

¹²² Between India and Musri, not between India and Assyria.

¹²³ Lévi 1914 (1937, 286f.) and Steier 1938, 1415f.

¹²⁴ See above in II.1.

¹²⁵ Meissner 1913, 293. This suggestion has been taken by Przyluski (1927, 178) as proof of an already existing trade.

¹²⁶ Parpola 1975b, 17f. Cf. also Herzfeld 1968, 70.

¹²⁷ Parpola 1975b, 18f.

¹²⁸ Gershevitch 1957, 320 and passim.

¹²⁹ Schiwiek 1962, 6.

¹³⁰ Herzfeld 1968, 238ff. (trade) and 29ff. (Sennacherib's campaign).

¹³¹ There was a later tradition (Ctesias in Diodorus 2, 3–19) of a legendary Assyrian campaign (by Ninus and Semiramis) in Bactria, but according to Kuz'mina (1976, 130) there is no archaeological confirmation.

but on this point there is little evidence one way or another. For the Assyrian period it is perhaps unlikely, but there is some evidence for Bactria already being a part of the Median empire,¹³³ and as there were close ties between Bactria and northwestern India which had already begun in the prehistoric period,¹³⁴ it is indeed possible that at least indirect land contact already existed before the Achaemenian period. However, the trees mentioned above were apparently growing in the coastal region and were hardly likely to be brought by caravan.

As the timber used by Sennacherib indicates an already existing sea-trade, it was perhaps in operation throughout the entire Neo-Babylonian period. However, it cannot have been very lively, as there is so little direct evidence of it. In fact, objections are possible against every argument proposed,¹³⁵ and yet we cannot deny a strong possibility of such a trade. I shall take only a few points which are often mentioned as evidence. It remains to be seen if archaeological exploration in the still mostly virgin soil of the southern Gulf coasts and Oman can bring some light to bear on this period.

Ninety years ago, Kennedy pointed out that a beam of apparently Indian cedar was found in the ruins of the palace of Nebuchadrezzar II (605 – 562 B.C.) at Birs Nimrud, and two logs resembling teak were discovered in the moon temple at Ur.¹³⁶ The identifications, however, were founded on mere appearance, and as far as I know the wood has never been properly examined. More critical authors often leave them unmentioned.¹³⁷

The old idea that Greek *σινδών*, Hebrew *sādin* and Akkadian *sindhu* 'cotton' were derived from Sanskrit *sindhu* 'Indus'¹³⁸ is no longer tenable. Sayce's *sindhu* is in fact *šimdu/šindu* (with emphatic š!), and derived from the well attested and old root *šamādu* 'to bind'.¹³⁹ None of the three words seems to have originally meant 'cotton', instead they refer to 'fine cloth' (especially linen) or a 'bandage' and even their relationship to each other is far from clear.¹⁴⁰ Although cotton and probably even its cultivated variety have very ancient roots in northwestern India,¹⁴¹ in the West it is not attested before the fifth century.¹⁴² The same must be said of several other Indian products like pepper, rice and peacocks.¹⁴³ On the other hand, both hump-backed cattle and fowl already seem to have been imported into Mesopotamia from the Indus culture, and have been locally bred ever since.¹⁴⁴

¹³² Delbrück 1956, 18f.

¹³³ Herzfeld 1968, 323 and 344f.

¹³⁴ See below, chapter II.6.

¹³⁵ Kennedy (1898, 260ff.) had already disproved several early ideas.

¹³⁶ Kennedy 1898, 266f., both are also quoted by Rawlinson (1926, 3).

¹³⁷ E.g. Dandamajev 1982.

¹³⁸ Proposed by Lassen and Sayce, see Parpola 1975b, 15.

¹³⁹ Parpola 1975b, 17.

¹⁴⁰ Parpola 1975b, 14ff., Frisk s.v. *σινδών*.

¹⁴¹ Actual fragments of cotton textile are found in Mohenjodaro, see Parpola 1975b, 14.

¹⁴² By Herodotus. But the Egyptian cotton of that period may well not have been brought from India (cf. Berzina 1982, 18f.).

¹⁴³ These Indian exports will be discussed later.

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The most important piece of evidence from India probably belongs to the Achaemenian period, although it has often been ascribed a greater antiquity. But this was only a hopeful conjecture. As the Buddha was living in the 6th century B.C., and as the Jātakas go back further, then certainly even the *Bāverujātaka* must belong to the pre-Achaemenian period – so it is often stated.¹⁴⁵ It will be seen later (chapter VI.5.) that there are good grounds for adopting the short chronology (about hundred years later) for the Buddha and the Jātaka collection was written down much later. Undoubtedly, it contains some very ancient traditions, but without further evidence we cannot ascribe any single Jātaka even to the Achaemenian period. In the case of the *Bāverujātaka*, such evidence is given by the spreading of the peacock in the West, but it still does not bring it beyond the Achaemenian period.

It is not necessary to quote this well-known text in extenso.¹⁴⁶ It recounts how some Indian merchants made two voyages to the country of Bāveru (*Bāveruraṭṭha*) and sold at a good profit first their pilot crow (*disākāka*), then a trained dancing peacock. It was first pointed out by Minaev that Bāveru must be Babylon, and Weber added that the name was most probably derived through Old Persian *Bābiru*.¹⁴⁷ The Iranian *r* instead of the Semitic *l* seems to prove that the borrowing happened only in the Achaemenian period, although a possibility of direct borrowing from Semitic through a MIA *r*-dialect cannot be excluded.¹⁴⁸

The story itself has been analyzed by Lévi¹⁴⁹ and there is not much to add. Unambiguous early Mesopotamian evidence for the peacock is missing, but the bird appeared in Greece as a great rarity in the middle of the fifth century. The first Greek peacocks were Hera's birds at Samos. There were good communications in the Achaemenian empire, and many novelties soon spread even outside its frontiers, as we know from Greece. Therefore the idea of Kennedy¹⁵⁰ that in order to reach Greece c. 460/470 B.C. peacocks must have already arrived in Babylon in the sixth century, seems unnecessary. Early fifth is surely enough, although the sixth century is by no means impossible.

We are not told from which harbour the merchants embarked on their voyage to Bāveru.¹⁵¹ Perhaps it was Bharukaccha or Sopara, both are often mentioned in Pāli sources.¹⁵² It is very unfortunate that, with the exception of Lothal, which belongs to a much earlier period, no harbour town of the Indian western coast has been properly excavated. There were in fact many such harbours, some very prominent and well attested in later sources,¹⁵³ but we do not even know when they were established. Their remains

¹⁴⁴ Asthana 1976, 47 (hump-backed cattle) and Brentjes 1962 644f. (fowl).

¹⁴⁵ E.g. in Kennedy 1898, 268 and Rawlinson 1926, 4.

¹⁴⁶ It is *Jātaka* 339. The text and a Russian translation were published by Minaev (1870, 232ff.).

¹⁴⁷ Minaev 1870, 231f. and Weber 1871, 622, note 3. See. also Szuszkiewicz 1980, 116f.

¹⁴⁸ The text itself as we have it still cannot be pre-Achaemenian, as the price of the crow is given in *kaḥāpaṇas* (OIA *kāṣṣāpaṇa* with *kaṣṣa* borrowed from OP).

¹⁴⁹ Lévi 1914. Szuszkiewicz 1980 refers approvingly to Lévi but adds little.

¹⁵⁰ Kennedy 1898, 269.

¹⁵¹ The conventional beginning *atīte Bārāṇasīyam Brahmadatte rajjam kārente* can hardly be taken as an indication of a trade between Varanasi and Babylon.

¹⁵² Maialasekera ss.vv. *Bharukaccha* and *Suppāra(ka)*.

may still contain the evidence of the early trade relations, although the total absence of such evidence in other excavated sites may indicate the small amount of trade.

There is, or seems to be, more evidence from India, but rarely is it so reliably dated that it could be placed earlier than the Achaemenian period with any certainty. In many cases it is also open to controversy. Chronologically old enough is a set of rare words from *Atharvaveda* for which Tilak suggested Mesopotamian origin: *taimāta* from the Tiamat dragon,¹⁵⁴ *urugūlā* from *urugala* 'underworld', *āligi* and *viligi* from god Bilgi,¹⁵⁵ but it can all be explained otherwise.¹⁵⁶ A Mesopotamian derivation is by no means impossible in a later period, but there seem to be no convincing examples. On the other hand, there are several well-attested cases of words borrowed from Old Persian.¹⁵⁷

Thus, there are no clear cases of Indian words which have been derived from Mesopotamia. The supposed cases of Mesopotamian influence in the sphere of religion and mythology can also be interpreted in other ways.¹⁵⁸ In some cases a common element may even go back to the early relations between the Indus culture and Sumer discussed above.¹⁵⁹ Such fantastic equations as *asura* – Assyrian (here Iranian *ahura* rules out any possibility of a direct borrowing) and *paṇi* – Phoenician¹⁶⁰ do not need further discussion. Wild speculation like this is often promulgated by less critical studies.

More important is the question of a Mesopotamian element contained in early Indian astronomy (*Jyotiṣavedāṅga*), but apparently it cannot be dated earlier than the Achaemenian period.¹⁶¹ The common points in astronomy and cosmography discussed by Kirfel¹⁶² include Vedic evidence, but can be explained as reminiscences from the Indus culture. At least, they do not prove any direct contact between Vedic India and Mesopotamia.

Of course there was little reason to suppose such a contact, but at the same time this is no argument against an Indo-Mesopotamian contact in general. We can easily dismiss any contact between Mesopotamia and Vedic India, but it is much more natural that it was the western coast of the subcontinent or the lower Indus country which were involved in maritime contact. Both were definitely outside the Vedic sphere of culture, and we do not have much evidence for or against their early foreign contacts. But there are at least two important innovations brought from the West which we should discuss here: the arts of

¹⁵³ E.g. in the *Periplus*.

¹⁵⁴ Lahiri 1974 contains an attempt to connect Tiamat with Vedic *Vṛtra*.

¹⁵⁵ These four are listed by Dandekar (1969, 61). Others are suggested, but they are hardly convincing (see Przyluski 1927, 168ff. and 178f., Deb 1948 and Lahiri 1974, 255f., with further references).

¹⁵⁶ See Mayrhofer ss.vv. Mayrhofer (New) does not even mention the Akkadian hypothesis concerning *āligi* and *urugūlā*.

¹⁵⁷ Early attested examples in India are *lipi* 'writing', *mudrā* 'seal' and *kārṣa/kārṣāpaṇa* 'a particular weight', see Burrow 1973, 388f. and Mayrhofer ss.vv., on *kārṣāpaṇa* also Mayrhofer (New) s.v.

¹⁵⁸ See e.g. Lahiri 1974 and Asthana 1976, 131ff., but also Witzel 1980, 102, note 3.

¹⁵⁹ See e.g. Parpola 1984b, 1985a and 1985b.

¹⁶⁰ Aptly criticized by Dandekar (1969, 66f.). On *Paṇis* see Parpola 1988, 222ff.

¹⁶¹ Pingree (1973) places it in c. 400 B.C.

¹⁶² Kirfel 1920, 28*ff.

writing and minting.

There is often no means of estimating with any accuracy how much time a certain development must have taken. Nevertheless, this has often been attempted, and when such an estimation is taken as a kind of established fact, problems arise. Take, for example such well-known cases in Indology as Max Müller's estimation of 200 years for each of the Vedic periods, or an interval of at least a century between the classical grammarians. Both will be discussed in chapter VI. But at this point I would like to discuss Bühler's estimation of the time needed for the development of the Brāhmī script from its Semitic predecessor.

In his brilliant study Georg Bühler showed that, like Kharoṣṭhī, Brāhmī too must have a Semitic original that it was developed from.¹⁶³ But in addition to this, he made a series of what I think partly unnecessary assumptions. He suggested that both scripts must have a long history before Aśoka, that Brāhmī must precede Kharoṣṭhī, whose invention is with slight grounds placed in 450 B.C.¹⁶⁴ and that Brāhmī must have been adopted in a period c. 890 – 750 B.C., probably from the Aramaean script used at that time in Mesopotamia.¹⁶⁵ Bühler's suggestions lose some of their probability if we accept the late dates for the Buddha¹⁶⁶ and Pāṇini.

It is hard to say how much time a palaeographical development must have had when there are no specimens of its stages preserved. As an opposite extreme, it has been suggested that both Brāhmī and Kharoṣṭhī were invented from their Semitic models only during Aśoka's early rule, and first propagated in his inscriptions.¹⁶⁷ This is perhaps too extreme, but in spite of contrary arguments I think Megasthenes' testimony about Indians' ignorance of writing cannot be passed over. Megasthenes' point was that Indians had no written laws,¹⁶⁸ and this is in accordance both with the utopian ideal he certainly had in mind¹⁶⁹ and with the reality of dharmaśāstra. At that time there surely were no written laws in India. But he also seems to suggest that writing was not commonly known, although it most probably already existed.¹⁷⁰

There is more to say in criticism Bühler's chronology. He has, for example, overlooked the fact that the common OIA word for writing, *lipi*, is clearly derived from Old Persian *dipi*. It was apparently used for the first time by Pāṇini.¹⁷¹ In Aśoka's northwestern MIA the word has retained its original initial as *dipi*.¹⁷² The Persian word

¹⁶³ Bühler 1895, 51ff.

¹⁶⁴ Bühler 1895, 50f.

¹⁶⁵ Bühler 1895, 79f.

¹⁶⁶ Bühler 1895, 7ff. quotes several Buddhist (Pāli) sources mentioning writing in order to show that writing was known in India when these sources were written. But we must also keep in mind that these sources are later than the Buddha himself.

¹⁶⁷ Goyal 1985, 82ff.

¹⁶⁸ Megasthenes F 32: ...ἀγράφοις καὶ ταῦτα νόμοις χρωμένοις. οὐδὲ γὰρ γράμματα εἶδέναι αὐτούς, ἀλλ' ἀπὸ μνήμης ἕκαστα διοικεῖσθαι.

¹⁶⁹ Zambrini 1985, 837f. See also chapter III.8. below.

¹⁷⁰ From a fragment of Nearchus (F 23) we know that writing was known in the Indus country. But in the same fragment Nearchus also confirms the absence of written laws.

¹⁷¹ *lipikara* in P. 3, 2, 21.

was brought into India from the northwest in the Achaemenian period,¹⁷³ and it would therefore be easy to suggest that writing arrived in the same way. Yet in the northwest, where there was even direct Achaemenian dominion, a different script (Kharoṣṭhī) was introduced. Thus it remains a good possibility that Brāhmī's model was brought from Mesopotamia to Western Indian harbours, but there seems to be no need to put its introduction further back than the Achaemenian period.

Another question which is difficult to answer conclusively is the origin of coinage in India. It is an old idea that the earliest Indian coinage (the so-called punch-marked coins) goes back to Mesopotamia as it seems to use a Babylonian standard.¹⁷⁴ The idea has had many supporters ever since, but it still remains a hypothesis. It might be linked with the supposed high antiquity of the punch-marked coins, whose earliest issues are generally dated c. 600 B.C.¹⁷⁵ But there are also several objections to this. Not being a numismatist myself, I shall not go into details, but as far as the standard is concerned, I fail to see any reason why it cannot have been introduced during the Achaemenian period. The Old Persian loanwords like the already mentioned *karṣa* certainly support this, and at least in the Gangetic basin the economy in the sixth century as reflected in literature and archaeological finds had hardly attained the monetary stage.¹⁷⁶ With the short chronology of the Buddha, Buddhist literature — in any case written down only (and sometimes much) after the Buddha's death, thus reflecting a later period — loses its right to be used as evidence for the sixth century. Thus we are left with Vedic literature. The economic information contained in the early Dharmasūtras was studied by Smith who — although himself believing in the introduction of coinage in late seventh or early sixth century¹⁷⁷ — concluded that it is only “between Gautama and Vasistha (that a) money economy becomes fully organized in India”.¹⁷⁸ According to his dates, this means between 500 and 300 B.C.¹⁷⁹ The slow development of urbanization between 600 and 300 B.C., with full

¹⁷² Scialpi 1984, 64.

¹⁷³ This is also noted by Bühler (1895, 20f.).

¹⁷⁴ Mitchiner 1973, 9ff. points out that the two metrological systems are clearly related, though not identical, but maintains that the idea of using the weights for currency might be an independent invention.

¹⁷⁵ In a recent article Cribb has shown that the supposed numismatic evidence for such antiquity is not valid (Cribb 1985, 542ff.). Mitchiner (1973, 27f. and 37), too, placed the beginning of Kosalan and Māgadhan coinage as early as 575 B.C. This he bases on internal chronology and on the interrelation of these early issues, and he tries to connect it with Māgadhan expansion. This, however, he dates with the death of the Buddha, using the traditional date c. 486 B.C., and this is no longer tenable. Some Indian scholars (e.g. Saletore 1975, 604ff.) try to give a much greater (early Vedic or even Indus culture) antiquity to Indian coinage. Sircar 1977, 3f. cautiously states that the earliest coins are discovered at levels assigned to dates between 600 and 200 B.C., and that both monetary terms (though they may indicate mere metal weights too, see *ibid.* 7) *kāṛṣāpaṇa* and *paṇa* are first mentioned by Pāṇini (5, 1, 29 *vibhāṣā kāṛṣāpaṇasahasrābhyām* and 5, 1, 34 *paṇapādamaśaśatād yat*; on Pāṇini's date see VI.1.). Later (p. 6), however, he, too, sees Indian coinage starting from about 600 B.C.

¹⁷⁶ We can note that there are no punch-marked coins indisputably found in stratigraphically confirmed levels corresponding to the pre-Achaemenian period in the West.

¹⁷⁷ Smith 1957, 209.

¹⁷⁸ Smith 1957, 214.

¹⁷⁹ Smith 1957, 190. For dates, see also chapter VI.6.

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urbanization beginning only in the Mauryan period,¹⁸⁰ seems to point to the same conclusion.

Thus the early introduction of coinage from Mesopotamia is both unnecessary and unlikely. Consequently, I fully agree with the view succinctly stated by Allchins: "The earliest silver and copper coinage, marked with punched signs and hence referred to as punch-marked, appears around the fourth or fifth century B.C., perhaps as a result of Achaemenian provincial silver currency circulating in the northwest."¹⁸¹ With an Achaemenian origin there is no need to think of any direct contact with Mesopotamia at all.

Several other points of alleged evidence concerning Indo-Mesopotamian contacts have been suggested,¹⁸² but they have largely remained solitary hypotheses approved by only a few. To put it briefly, there seems to be nothing on the Indian side that would prove a definite contact between India and Mesopotamia before the Achaemenian period, and while India in the Achaemenian period acquired new ideas from the West, the channel of contact was not necessarily between Mesopotamia and India.

I am not, however, propagating the old idea of an isolated India. When the evidence is so scarce an *argumentum ex silentio* has little force. It is likely that real contacts often preceded the evidence and may have existed even though no evidence has been passed down to us. It is quite possible that there was some kind of contact between India and Mesopotamia, but probably the part of India directly involved in such contacts was situated outside the Aryan sphere of culture which most of the written sources of the period belong to. But in spite of this, we gain nothing by wild speculation and much, I think, by carefully examining what we really know or at least can reasonably presume in the light of the evidence we have. However scarce evidence might be, useful hypotheses and conclusions can only be formed from it. For Indo-Western relations there is much indisputable evidence, but only from the Achaemenian period. Therefore, we must next discuss the Achaemenids.

¹⁸⁰ Erdösy 1985, 94ff.

¹⁸¹ Allchin & Allchin 1982, 360. A similar result is reached by Cribb (1985), who concludes i.a. that "the earliest local Punch Marked Coins in the Ganges plain were issued in the area of Varanasi. The Varanasi issues derived their technology and designs from local Punch Marked Coins issued in the Gandhāra area. The Gandharan Punch Marked Coins were themselves derived from the Greek-style coins used and issued in the Kabul area. Hoard evidence shows that the Gandharan Punch Marked Coins were in circulation at a date in the mid-4th century BC. The issue of the Gandharan Punch Marked Coins probably took place in the early 4th century BC. India's earliest coins should therefore be dated to the early 4th century BC."

¹⁸² E.g. those put forth by Przyluski (1927).

5. *The Great Kings*

“Saith Darius the King: These are the countries which came unto me; by the favor of Ahuramazda I was king of them: Persia, Elam, Babylonia, Assyria ... Drangiana, Aria, Chorasmia, Bactria, Sogdiana, Gandara, Scythia, Sattagydia, Arachosia, Maka: in all, XXIII provinces.”¹⁸³ “Saith Darius the King: By the favor of Ahuramazda these are the countries which I got into my possession along with this Persian folk, which felt fear of me (and) bore me tribute: Elam, Media, Babylonia ... Drangiana, Aria, Bactria, Sogdiana, Chorasmia, Sattagydia, Arachosia, Sind, Gandara, Scythians, Maka.”¹⁸⁴ “Saith Darius the King: This is the kingdom which I hold, from the Scythians who are beyond Sogdiana, thence unto Ethiopia; from Sind, thence unto Sardis – which Ahuramazda the greatest of the gods bestowed upon me.”¹⁸⁵ Said Herodotus the Historian: “A great part of Asia was explored under the direction of Darius. He being desirous to know in what part the Indus, which is the second river that produces crocodiles, discharges itself into the sea, sent in ships both others on whom he could rely to make a true report and also Scylax of Caryanda. They accordingly setting out from the city of Caspatyrus and the country of Pactyice sailed down the river towards the east and sunrise to the sea; then sailing on the sea westward, they arrived in the thirtieth month at that place where the King of Egypt despatched the Phoenicians, whom I before mentioned, to sail round Libya. After these persons had sailed round, Darius subdued the Indians and frequented this sea.”¹⁸⁶

¹⁸³ DB I, 12–17: *ōātiy Dārayavauš xšāyaθiya: imā dahyāva tyā manā patiyāiša vašnā Auramazdāha adamšām xšāyaθiya āham: Pārsa, Ūvja, Bābiruš Aōurā ... Zraka Haraiva Uvārazmīy Bāxtriš Suguda Gadāra Saka θataguš Harauvatiš Maka fraharavam dahyāva XX III.* Text and translation from Kent 1953, 117 and 119.

¹⁸⁴ DPe 5–18: *ōātiy Dārayavauš xšāyaθiya: vašnā Auramazdāhā imā dahyāva tyā adam adaršiy hadā anā Pārsā kārā tyā hacāma atarsa manā bājim abara: Ūvja, Māda, Bābiruš ... Zraka Haraiva Bāxtriš Sug^{da} Uvārazmīy θataguš Harauvatiš Hiduš Gadāra Sakā Maka.* Text and translation from Kent 1953, 136. There are also similar lists in DNa, DSe and DSm (the latter two with the OP part partly missing), and XPh.

¹⁸⁵ DPh 3–9: *ōātiy Dārayavauš XŠ: ima xšačam tya adam dārayāmiy hacā Sakaibis tyaiy para Sugdam amata yātā ā Kūsā hacā Hidauv amata yātā ā Spardā tyamaiy Auramazdā frābara hya maθišta bagānām.* Text and translation from Kent 1953, 236f. DH is identical. A related formula is found in the Old Testament, Esther 1:1: *Ahasuerus ... which reigned, from India even unto Ethiopia, over an hundred and seven and twenty provinces.* The countries which here define Artaxerxes' dominions are called in Biblical Aramaic *Hoddu* and *Kuš*, cf. Neiman 1980, 35.

¹⁸⁶ Cary's translation as quoted in McCrindle 1901, 4f. of Hdt 4, 44 τῆς δὲ Ἀσίας τὰ πολλὰ ὑπὸ Δαρείου ἐξευρέθη, ὃς κροκοδείλους δεύτερος οὗτος ποταμῶν πάντων παρέχεται, τοῦτον τὸν ποταμὸν εἰδέναι τῇ ἐς θάλασσαν ἐκδίδοι, πέμπει πλοίοισι ἄλλους τε τοῖσι ἐπίστευε τὴν ἀληθεῖν ἐρέειν καὶ δὴ καὶ Σκύλακα ἄνδρα Καρυανδέα. οἱ δὲ ὀρμηθέντες ἐκ Κασπατύρου τε πόλιος καὶ τῆς Πακτικῆς γῆς ἔπλεον κατὰ ποταμὸν πρὸς ἠῶ τε καὶ ἡλίου ἀνατολᾶς θάλασσαν, διὰ θαλάσσης δὲ πρὸς ἐσπέρην πλέοντες, τριηκοστῶ μηνὶ ἀπικνεύονται ἐς τοῦτον τὸν χῶρον ὅθεν ὁ Αἰγυπτίων βασιλεὺς τοὺς φοίνικας τοὺς πρότερον εἶπα ἀπέστειλε περιπλέειν Λιβύην. μετὰ δὲ τοῦτους περιπλώ-

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These are the main sources¹⁸⁷ from which the history of the eastern dominions of the Achaemenids is reconstructed. Most of it is rather clear. The oldest inscription (DB) lists countries which already belonged to the empire when Darius began his rule. Among them most likely Gandāra¹⁸⁸ and probably also Sattagydia belonged to Northwest India. In later lists of Darius and Xerxes there is the important addition of *Hiⁿdu*, probably Sind (as translated by Kent), and Herodotus confirms that Sind (India) was annexed by Darius. The empire at that time had reached its greatest extension in the East – although we do not know for certain its exact boundaries – and after Xerxes there is no further unambiguous evidence of Indian possessions.

Herodotus and others leave no doubt that Darius' immediate predecessor, Cambyses, spent most of his reign in Egypt, and had no time for any eastern conquests.¹⁸⁹ On the other hand, his father, Cyrus, not only conquered Media, Lydia and Babylonia, but also "the upper regions of Asia, conquering every nation without passing one by."¹⁹⁰ In another passage Herodotus mentions him planning expeditions against Babylon, Bactria, Sakas and Egyptians.¹⁹¹ The last one he could not carry out, as he was killed during his expedition against Massagetae, a Saka people.

Therefore, it seems likely that Herodotus means the Bactrian conquest had actually taken place¹⁹² and this can well have included even regions in the southeastern side of Hindukush. Several other classical authors expressly mention Cyrus' campaign in Afghanistan and India.¹⁹³ This would be the way Gandāra and Sattagydia first came to be parts of the empire. The question whether Cyrus did have any Indian conquests is in a way anachronistic, because in the sixth and early fifth centuries the name India (or *Hiⁿdu* and its derivations) probably meant only the lower Indus country (Sind), the wider meaning developing only gradually in Greek ethnography.¹⁹⁴ For the exact chronology of this conquest we have no clear evidence, and consequently different theories have been put forward. Some think that the countries up to the Indus already belonged to the Median empire and Cyrus only reconquered them.¹⁹⁵ Wecker and Breloer

σαντας Ἰνδούς τε κατεστρέψατο Δαρείος καὶ τῇ θαλάσῃ ταύτη ἐχράτο.

¹⁸⁷ There are also the Elamite and Akkadian versions of the Achaemenian inscriptions (and some Egyptian versions and an Aramaic version of DB). Not being an Assyriologist, I am here wholly dependent on the studies of others. But although these versions are often not identical, and their differences can tell one many important things (see Cameron 1973), these differences do not appear to have relevance to our present study.

¹⁸⁸ My somewhat inconsistent use of the names Gandāra and Gandhāra depend on whether the viewpoints and main sources used are Persian and Greek or Indian. The difference is meaningful as the Western and Indian sources do not have same idea about the extension of the region, as will be seen soon.

¹⁸⁹ See e.g. Jackson 1922, 333.

¹⁹⁰ Hdt 1, 177 τὰ μὲν νῦν κάτω τῆς Ἀσίας Ἄρπαγος ἀνάστατα ἐποίηε, τὰ δὲ ἄνω αὐτῆς αὐτὸς Κῦρος, πᾶν ἔθνος καταστρεφόμενος καὶ οὐδὲν παριείς.

¹⁹¹ Hdt 1, 153 ἢ τε γὰρ Βαβυλῶν οἱ ἦν ἐμπόδιος καὶ τὸ Βάκτριον ἔθνος καὶ Σάκαι τε καὶ Αἰγύπτιοι, ἐπ' οὓς ἐπέιχε τε στρατηλατέειν αὐτός...

¹⁹² Perhaps Horace's *regnata Cyro Bactra* (*Od.* 3, 29, 27f.) is not a mere poetic expression.

¹⁹³ Xenophon (but only in *Cyropaedia*), Ctesias, Arrianus (both *Anabasis* and *Indica*) and Pliny, see Jackson 1922, 330ff. Megasthenes denied it, but he had his own reasons.

¹⁹⁴ See Breloer 1941a, 7ff.

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even thought that this dominion was inherited from the Assyrians, but a reference in a late author¹⁹⁶ to a period already legendary in Herodotus' time cannot prove anything. Many make Cyrus the conqueror of the upper Indus and some adjacent regions.¹⁹⁷ Here some follow the order implied by Herodotus 1, 177 and place the conquest of the east before that of Babylon (539 B.C.).¹⁹⁸ The Herodotean account, however, is not wholly unambiguous and did not necessarily mean that all these conquests were before Babylon. The other passage of Herodotus seems to place the Bactrian conquest only after the fall of Babylon.¹⁹⁹

An interesting theory has been put forward by Francfort, who connects the Herodotean Massagetæ with the Assaceni (and their capital Massaga) of the Alexander historians and the Derbices²⁰⁰ of Ctesias (as well as the Dadicæ of Herodotus) with the present day Dards, and thus locates the last campaign and death of Cyrus within the confines of India.²⁰¹ An attempt wholly to deny a southeastern campaign by Cyrus was made by Majumdar,²⁰² but though the classical sources as such are either unreliable as evidence²⁰³ or inconveniently late and thus could well be connected with a *legend* of the conquests of Cyrus, the evidence of the Old Persian inscriptions cannot be passed over. The chronological significance of DB does not seem to have been fully understood by Majumdar.²⁰⁴

Old Persian *Gandāra* corresponds to Old Indian *Gandhāra*, but this does not mean that it has always had same extension. In the Akkadian version of DB, the corresponding name is *Paruparāšanna*, Paropamisadae, which means the southern slopes of Hindu-kush,²⁰⁵ the country of the Astaceni and other tribes met there by Alexander. A third

¹⁹⁵ E.g. Herzfeld 1968, 344f., but not very convincingly. O.M. Cook (in Gershevitch 1985, 212f. and 220) emphasizes that the Median dominion in Bactria and Gandāra is a possibility, but our evidence is too slight to prove or disprove it.

¹⁹⁶ Arrianus, *Ind.* 1, 2 says that the Astaceni and Assaceni were subjects to the Assyrians. See Wecker 1916, 1291, and Breloer 1941a, 22 and 27.

¹⁹⁷ E.g. Marquart 1907, 139f., Jackson 1922, 329ff., Olmstead 1948, 144 and Lamotte 1958, 111f.

¹⁹⁸ E.g. Dandamaev 1982, 114f.

¹⁹⁹ This is followed by Altheim & Stiehl (1970, 123ff.).

²⁰⁰ With many variant readings (see Marquart 1907, 139, note 1). They have often been connected with the Rigvedic (2, 14, 3) *dṛbhīka*. Against this Charpentier (1923, 141f.) has pointed out that *Dṛbhīka* is a name of a demon, but the Rigvedic "demons" often seem to have a more or less ethnic origin (see e.g. Parpola 1988, 210f. on the *Dāsas*) and *Dṛbhīka*, too, could well be "an enemy chief" (thus Parpola 1988, 257).

²⁰¹ Francfort 1985. A similar theory, but with weaker grounds, had already been expounded by Prakash (1969, 138f.), and originally it seems to come from Marquart (1907, 139ff.). Dadicæ with Dards (of *Uḍḍiyāna/Swāt*) and Massagetæ with Massaga (*Maśakāvāt* of Patañjali) are also connected by Tucci (1977, 11 and 41ff.), Dadicæ with Dards already by Ritter (1833, 654; see also Eggermont 1984a, 220ff. and P'jankov 1987, 265ff.).

²⁰² Majumdar 1949, 154ff.

²⁰³ Thus Jackson (1922, 330f.) and especially Prakash (1969, 133ff.) make the bad mistake of taking the educational novel of Xenophon as a source of real history.

²⁰⁴ Majumdar 1949, 157f. (cf. the criticism in Chattopadhyaya 1974, 10f.).

²⁰⁵ Herzfeld 1968, 336f. Vogelsang (1985, 90) suggests that this apparent identification of Gandāra and Paropamisadae must be due to some kind of confusion.

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name – either for the whole of Gandāra or a part of it – was probably Πακτυϊκή.²⁰⁶ What the eastern extension of this Achaemenian province was, we cannot say, perhaps it was not much (if at all) beyond the Indus.

In a way the limit to the eastern extension depends on the identification of θαταγυ, Greek Sattagydia.²⁰⁷ This is an old matter of controversy. A location by the upper course of the Kabul and its tributaries has been suggested, but then it would correspond to Paropamisadae, and this is against DB as mentioned above.²⁰⁸ In addition, the Sattagyidian in the Persepolis relief wears the same kind of loincloth as Gandāran and Indian, and therefore he hardly comes from mountains, where much warmer clothes were needed (like those of the Bactrian in the relief).²⁰⁹ Herzfeld suggested the Pañjab on purely linguistic grounds, deriving OP *θαταγυ* through Median **sattagus* from MIA²¹⁰ *satta* ‘seven’ and an unattested word for river corresponding to Sanskrit *guda* ‘bowels, gut’ and Iranian *guḍa* ‘a riverine formation’. In this way “seven rivers” indicates well enough the Pañjab, the *Sapta Sindhavaḥ* of the *Rigveda*,²¹¹ but the derivation of Sattagydia from it is hardly convincing, and with no other evidence it cannot be accepted. Thus, we are left with the Middle Indus region, perhaps Bannu north of Dera Ismail Khan as suggested by Fleming.²¹²

If we take the Sattagyidian loincloth into account, then the similar dress of the Gandāran in the same relief hardly allows a mountainous home even for him. But then it is likely that Gandāra reached at least as far as to the Indus, where it is already warm enough.²¹³ In the tribute bearer relief the Sattagydians seem to be missing. The group commonly identified as Gandārans bear not only loincloths but also gowns,²¹⁴ while the Indians are mostly (three of the four) naked to the waist.²¹⁵ According to Foucher,²¹⁶ Gandāra included all the Pañjab, but his main argument, the identification of Casparyrus with Multan, is hardly acceptable (see next chapter). Foucher's thesis that Alexander only conquered the empire of Darius up to its eastern frontier thus becomes untenable, as there

²⁰⁶ Hkt F 295 Κασπάπυρος· πόλις Γανδαρικῆ, but Hdt 4, 44 ἐκ Κασπατύρου τε πόλιος καὶ τῆς Πακτυϊκῆς γῆς (also in 3, 102).

²⁰⁷ Perhaps the name corresponds to OIA *śatagu*.

²⁰⁸ Fleming 1982, 103ff.

²⁰⁹ Herzfeld 1968, 341 and Fleming 1982, 105 (but see also Frye 1984, 113), illustration in Walser 1966, Faltafel I.

²¹⁰ I cannot see how “Pāli was the dialect of Gandāra when the Iranians borrowed the name” even if we read “an MIA dialect” instead of the wholly inappropriate Pāli in so early a period (Herzfeld 1968, 342).

²¹¹ But see also Macdonell & Keith 1912, s.v.

²¹² Fleming 1982, 105. Frye (1983, 113) locates Sattagydia in the hills NE of Arachosia and Gandāra more or less in the same region, but extending to the plains and including even Taxila (ibid. 104). See also Vogelsang 1985, 80f.

²¹³ Yet Hlopin 1983 seems to suggest many more western locations for all eastern provinces except India (Hindu).

²¹⁴ Walser 1966, plate 21.

²¹⁵ Walser 1966, plate 25. The fifth figure – the leader of the group – is Persian, as always. Cf. the Herodotean account (7, 61ff.) of Xerxes' invasion army where, contingents formed on ethnic principles were always commanded by a Persian.

²¹⁶ Foucher 1938, 339ff. and 1947, 195ff.

is no evidence at all of an Achaemenian dominion in the Pañjab. And if the Pañjab is excluded, we cannot even be sure of Taxila. Many have taken for granted the position of Taxila in Gandāra as a part of the Achaemenian dominion, but lately there have been some critical voices.²¹⁷

Therefore, it seems that we can with some certainty include only the Upper Indus region as belonging to the Achaemenian Gandāra. Some scholars have indeed expressed the opinion that the Indus was the eastern boundary.²¹⁸ I would not go as far as this, although the idea of the Indus as a political boundary between India and Iran as stated by Strabo²¹⁹ can perhaps go back to the Achaemenian period. This could also explain the meagre amount of Achaemenian antiquities in Taxila, but then the area is not so thoroughly excavated that an archaeological *argumentum ex silentio* can be much relied upon. There are indeed remarkably few Achaemenian antiquities found even in Afghanistan, where the dominion cannot be denied.

Both the Old Persian inscriptions and Herodotus ascribe to Darius the conquest of "India", and there is little disagreement among scholars about its location in the lower Indus, the present Sind.²²⁰ The conquest itself is somehow connected with the exploration in which Scylax participated, and this will be discussed in the next chapter. From Herodotus we learn that this conquest opened the sea-route between the eastern satrapies and Egypt, and it was only natural to sail also between Mesopotamia and the Indus. The river and the country, OIA *Sindhu*, gave through the OP *Hiⁿdu*²²¹ and Ionian Greek (demanding the loss of the initial *h*) the Greek words 'Ινδός and 'Ινδία.²²² Originally, India signified only Sind, but already in Herodotus it contained areas beyond the Persian dominion.²²³ Ctesias seems to use it for all the Indus country as well as countries beyond as far as he knew of them. Unlike in the north (the Pañjab) the eastern boundary of the Achaemenian dominion is here clearly marked by the Thar desert.²²⁴ Thus, it is impossible to search for the Eastern Ethiopians of Herodotus in South India.²²⁵ They

²¹⁷ "If Taxila ever was part of the Achaemenian empire, it was a loose subjection that did not take the form of the standard satrapy", says Fleming (1982, 110, note 16) and Eggermont (1970, 118 and passim) thinks that Taxila was in any case situated well outside the geographical limits of Gandhāra as defined in Indian sources. On the other hand, Frye (1983, 104) includes Taxila in Gandāra, and Badian (in Gershevitch 1985, 461f.) sees the exceptional favour shown by Taxiles towards Alexander as a mark of Achaemenian suzerainty, which was still recognized by Taxiles.

²¹⁸ E.g. Majumdar 1949, 158ff.

²¹⁹ Strabo 15, 1, 11, p. 689 (probably from Eratosthenes).

²²⁰ E.g. Herzfeld 1968, 346f. and Dandamaev 1982, 115. Only Hlopin 1983 locates it in Upper and Middle Indus regions.

²²¹ Elamite (*h*)*hi-in-du-uš/šis*, Akkadian (of Achaemenian inscriptions) *KURⁿin-ú*, Egyptian *hndwj*, cf. Dandamaev 1982, 116 and 124, note 28 (references).

²²² See e.g. Parpola 1975b, 10ff. Probably Lassen (1827, 5ff.) was the first to show that 'Ινδία came from OIA *sindhu* through Old Iranian (then, before the full decipherment of the OP inscriptions, Lassen knew only Anquetil-Duperron's Avesta) and the Ionian dialect.

²²³ Hdt 3, 101 οὔτοι μὲν τῶν 'Ινδῶν ἐκαστέρῳ τῶν Περσέων οἰκέουσι καὶ πρὸς νότου ἀνέμου καὶ Δαρείου βασιλέως οὐδαμᾶ ὑπήκουσαν.

²²⁴ Hdt 3, 98 ἔστι τῆς 'Ινδικῆς χώρας τὸ πρὸς ἥλιον ἀνίσχοντα ψάμμος (also 4, 40). But see Edelmann 1970.

were in Xerxes' army fighting alongside of the Indians,²²⁶ and probably lived somewhere along the Gedrosian coast.²²⁷ Herodotus makes it wholly clear that they were different from the African Ethiopians.

There is one further point complicating the relation of the eastern satrapies, and this is the tribute paid in gold. If we believe Herodotus, the Indians, who were also more numerous than any other people,²²⁸ brought the greatest tribute of all Achaemenian subjects, 360 talents of gold-dust.²²⁹ This gold was obtained from gold-digging ants.²³⁰ The story of ant-gold will be discussed in detail in chapter VII.6., here we are interested only in the geographical difficulty. As will be seen, both Indian and classical evidence seems to locate this ant-gold somewhere in the north, but the Achaemenian satrapy of India was probably situated in the south. It seems that the gold should have come through the Gandāran province. Old Persian sources do not help us, as they mention only the gold brought from Bactria,²³¹ ultimately perhaps from Siberia.²³²

Herodotus himself seems to locate those particular Indians who obtained the ant-gold in the northern part of India,²³³ but the Gandārans still seem to be in the way. It does not help to say that Herodotus' account of gold dust tribute from India "cannot be true".²³⁴ It would also be nice to suppose that the gold came in fact from Gandāra, but according to Herodotus the whole seventh province (including Gandāra) paid only the very modest tribute of 170 talents.²³⁵ If we are to rely on Herodotus, we must somehow extend his India to the north around Gandāra. This can be done, but it is purely conjectural. Thus,

²²⁵ As suggested e.g. by Neiman (1980, 37f.).

²²⁶ Hdt 7, 70 τῶν μὲν δὴ ὑπὲρ Αἰγύπτου Αἰθίοπες καὶ Ἀραβίων ἦρχε Ἀρσάμης, οἱ δὲ ἀπὸ ἡλίου ἀνατολέων Αἰθίοπες (διξοὶ γὰρ δὴ ἐστρατεύοντο) προσετέταχτο τοῖσι Ἰνδοῖσι, διαλλάσσοντες εἶδος μὲν οὐδὲν τοῖσι ἑτέροισι, φωνὴν δὲ καὶ τρίχωμα μῦνον· οἱ μὲν γὰρ ἀπὸ ἡλίου Αἰθίοπες ἰθὺτριχῆς εἰσι, οἱ δ' ἐκ τῆς Λιβύης οὐλότατον τρίχωμα ἔχουσι πάντων ἀνθρώπων.

²²⁷ See e.g. Herzfeld 1968, 334ff. and chapter V. 3.

²²⁸ Hdt 3, 94 (quoted below) and 5, 3 Θρηίκων δὲ ἔθνος μέγιστόν ἐστι μετὰ γε Ἰνδοῦς πάντων ἀνθρώπων. An account like this could hardly fit with Sind, but perhaps he was thinking also of those Indians who were not subject to Darius. And perhaps he was not sure how many they really were.

²²⁹ Hdt 3, 94 Ἰνδῶν δὲ πλῆθος τε πολλῶ πλεῖστόν ἐστι πάντων τῶν ἡμεῖς ἴδμεν ἀνθρώπων καὶ φόρον ἀπαγίνεον πρὸς πάντας τοὺς ἄλλους ἐξήκοντα καὶ τριηκόσια τάλαντα ψήγματος.

²³⁰ Hdt 3, 102-105.

²³¹ DSf 35-37 *duraniyam hacā Spardā utā hacā Bāxtriya abariya tya idā akariya*, "the gold was brought from Sardis and from Bactria, which here was wrought" (Kent 1953, 143f.). See also P'jankov 1965, 41.

²³² Tarn 1951, 105f.

²³³ Hdt 3, 102 ἄλλοι δὲ τῶν Ἰνδῶν Κασπατύρῳ τε πόλι καὶ τῇ Πακτυϊκῇ χώρῳ εἰσι πρόσουροι, πρὸς ἄρκτου τε καὶ βορέω ἀνέμου κατοικημένοι τῶν ἄλλων Ἰνδῶν. Megasthenes (F 23) calls them Dards living near the Bactrians (Δέρδαι οἱ Βακτρίοισι παραπλησίην ἔχουσι δίαιταν).

²³⁴ Tarn 1951, 108.

²³⁵ Hdt 3, 91 Σατταγύδαι δὲ καὶ Γανδάριοι καὶ Δαδίκαι τε· καὶ Ἀπαρύται ἐς τῷτὸ τεταγμένοι ἑβδομήκοντα καὶ ἐκάτὸν τάλαντα προσέφερον.

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India could have included the Pañjab (if the old dominion was confined to the west of the Indus, everything on its eastern side could have belonged in the Indian province). This would fit well with what we know of Pactyice.

When we are dealing with the Achaemenian dominions near and in India we must also note that there are some grounds for locating the Amyrgian Sakas between Bactria and the Upper Indus north of Gandāra.²³⁶

Our information about the later history of the eastern dominions of the Achaemenian empire is very scanty. As to the Old Persian sources, even Indians are still mentioned as among the subjects of Xerxes,²³⁷ and their participation in his Greek campaign confirms this.²³⁸ Sattagydiān, Gandāran and Indian are still mentioned in the inscriptions accompanying the throne bearers in the Persepolis relief of Artaxerxes (II or III),²³⁹ but as the subject peoples depicted in this and related reliefs were already canonized in the tomb of Darius,²⁴⁰ their presence does not mean that they were necessarily still subjects. On the other hand, the many presents which, according to Ctesias, the Great King received from India seem to be tributes.²⁴¹

The last of the Achaemenids, Darius III, still had some Indian elephants, which he used against Alexander at Gaugamela. But while Alexander himself crossed Hindukush there is no indication at all in the histories written about him of a previous Achaemenian power there. It therefore seems likely that the Achaemenids lost their easternmost provinces – his few elephants Darius could have obtained by other means than a tribute. But perhaps this did not take place as early as in the early fifth century as has been suggested.²⁴² This is also borne out by the existence of an Indian soldier colony in Mesopotamia (Nippur) late in the fifth century.²⁴³

²³⁶ Cf. Marquart 1907, 139ff., Junge 1939, 83ff., Francfort 1985, 397, Tucci 1977, 16f. and especially Grantovski 1963, 24ff., but also the criticism in Daffinà 1980, *passim*.

²³⁷ XPh 25.

²³⁸ Hdt 7, 65f. and 7, 86.

²³⁹ A?P 11–13. Identification of the monarch was left open by Kent. According to Herzfeld he is Artaxerxes II (Walser 1966, 52).

²⁴⁰ Walser 1966, 51f. Another illustration of Darius' time is seen in the Egyptian stele found at Susa (see Yoyotte 1972, 258f.).

²⁴¹ They include elephants (Ctesias F 45, 7 and 45b), iron swords (45, 9), a curious poison (45, 34 and 45m) and fragrant oil (45, 47), which are all expressly mentioned as belonging to the Great King. Ctesias may have also included less curious items, but these have not interested Photius. He has also seen some Indians himself (45, 19). Cf. Reese 1914, 84f. and Karttunen *forthcoming a*.

²⁴² So e.g. Altheim & Stiehl 1970, 190 (ignoring XPh) and several Indian historians. Among the recent standard histories of Iran, Frye (1983, 113) accepts that the easternmost provinces broke or drifted away from Achaemenian rule, although we do not know when, while Badian (in Gershevitch 1985, 461f.) makes an attempt to show that Achaemenian dominion still reached the Indus when Alexander took over.

²⁴³ See Dandamaev 1982, 118f. and below, chapter II.7.

6. *The Opening up of the East: Bactrians and Carians*

In this chapter it is my intention to take up some additional questions and further details related to the easternmost dominions²⁴⁴ of the Achaemenian empire. It was shown in the preceding chapter that it was perhaps Cyrus who had already extended his realm to the confines of India, at least he seems to have conquered Bactria. And if he made an expedition to the southeastern side of Hindukush, he probably started from Bactria. It is therefore the role of Bactria in Perso-Indian relations that we must consider first.

Bactria (OP *Bāxtris*) is an old centre of East Iranian culture. From a very early period it had close ties both with Middle Asia and with India.²⁴⁵ As the remotest province of the Achaemenian empire in the northeast, it was used as a place of exile, yet it was no "Siberia". This is clearly seen in the eulogies (though later) of the fertile land where rice was cultivated,²⁴⁶ grains of corn were said to be as large as olive-stones²⁴⁷ and animals, too, were famous,²⁴⁸ not to speak of its many towns, its gold trade and jewels.²⁴⁹

That the "country of a thousand cities"²⁵⁰ was no vain boast (though perhaps an exaggeration) of a much later period, has been shown by the excavations made by Russians and others in the soil of ancient Bactria (now Northern Afghanistan and part of Soviet Middle Asia).²⁵¹ Urbanization seems to have developed here in a way that is similar to India, from fortified tribal and administrative centres and refuges, into proper urban centres with flourishing trade and crafts.²⁵²

Bactria's location was exceptionally favourable for trade. Bactria was a natural cross-roads of ancient routes between Iran, Middle Asia and the Eurasian Steppes, Siberia, Central Asia, and India. The rich gold hoards of the Achaemenian period testify to an

²⁴⁴ I intentionally choose such a vague term as "dominion" instead of satrapy or province because it is not important for my present task to decide if Herodotus copied from Hecataeus a list of satrapies of Darius (so e.g. Herzfeld 1968, 288, against e.g. Kiessling 1900, 39ff., cf. Hdt 3, 89 [Δαρεῖος] ... ἐν Πέρσῃσι ἀρχὰς κατεστήσατο εἴκοσι, τὰς αὐτοῖ καλέουσι σατραπῆας) or if his twenty νομοί are fiscal units of Artaxerxes I, not strictly grouped on geographical principles (so e.g. Daffinà 1967, 25). For similar problems with the existing Achaemenian lists, see Cameron 1973, 47ff. and Vogelsang 1985, 88f. See also Cook in Gershevitch 1985, 244ff.

²⁴⁵ See e.g. Litvinskij 1964, Jettmar 1967 (also 1983, 1984 and others), Kuz'mina 1976, Witzel 1980 and Parpola 1988, 202ff. Herodotus had already stressed that Bactria and India were immediate neighbours (3, 102) and had much in common (7, 66f.).

²⁴⁶ Strabo 15, 1, 18, c. 692 φύεσθαι δὲ [scil. ἡ ὀρύζα] καὶ ἐν τῇ Βακτριανῇ.

²⁴⁷ Theophrastus, *H. pl.* 8, 4, 5 ἐπεὶ καὶ περὶ τὴν Ἀσίαν οὐ πόρρω Βάκτρων ἐν μὲν τινὶ τόπῳ οὕτως ἀδρὸν εἶναί φασι τὸν σίτον ὥστε πυρῆνος ἐλαίας μέγεθος λαμβάνειν.

²⁴⁸ Aelianus, *N. An.* 4, 55 on Bactrian camels.

²⁴⁹ E.g. Ctesias F 1b), 7, 1. See also Pliny, *N. h.* 37, 65 on Bactrian emeralds.

²⁵⁰ Apollodorus in Strabo 15, 1, 3, c. 686 Εὐκρατίδαν γοῦν πόλεις χιλίας ὑφ' αὐτοῦ ἔχειν, Justinus 41, 1 *mille urbium Bactrianum imperium* – both referring to the Hellenistic period.

²⁵¹ See e.g. Kuz'mina 1976, 126ff. (especially 130) with further references.

²⁵² Pjankov 1973, 127ff. (summary 231f.), for India see Erdosy 1985, 89ff.

exceptional prosperity.²⁵³ It was here where the long Siberian gold route ended up, and both Old Persian inscriptions and classical sources mention Bactria as a centre of gold and other trade relations.²⁵⁴ Similarly, there seems to have been from time immemorial trade relations – as well as less friendly relations like invasions – with India. These close ties are attested both by archaeological evidence going back to the prehistoric period and by classical sources.

The normal route for Persians going to India seems to have been through Bactria, though a brief boom in sea traffic between Sind and Mesopotamia may have reduced this traffic for a while. Bactrian merchants were probably often seen both in Persia and in northwestern India. In addition to the presents or tributes brought to the Persian court, an important source of information about India to Ctesias seems to have been the Bactrian merchants. The Elamite tablets of sixth and fifth centuries B.C. from Persepolis and Susa contain a great deal of evidence concerning regular land travel between Persian metropolises and India, for commercial as well as for official purposes.²⁵⁵ The land route was also used by Alexander, and when Nearchus was sent to reconnoitre the sea route, there was apparently no memory left of the Achaemenian sea contacts.²⁵⁶ From later Indian sources we learn that India imported horses from Bactria.²⁵⁷

We must also discuss in greater detail the conquest of India (Sind) by Darius and the naval expedition connected with it. The main source is the Herodotean account (4, 44) quoted in the preceding chapter. The problems connected with an account apparently written about this expedition by Scylax of Caryanda will be dealt with in chapter III.1., but there are several other questions we can discuss here.

First, it must be emphasized that there is no evidence that Scylax himself was the leader of the expedition, although this has very often been assumed. It was suggested by Issberner that Darius probably sent Persians – known to have been no seamen – and then a Carian Scylax among them would have been the natural leader.²⁵⁸ But even assuming that the best seaman was also the leader, the assumption that Scylax was that leader is still

²⁵³ Stawiskij 1982, 26ff.

²⁵⁴ DSf 35–37 (quoted above) on gold from Bactria, 37–39 on lapis lazuli and carnelian from Sogdiana, probably through Bactria; Ctesias F 45, 26 and 45h on gold coming from Bactria and F 45, 6 on a Bactrian jewel merchant. See also Tarn 1951, 103ff.

²⁵⁵ Dandamaev 1982, 120ff. He notes that India (Sind) and Indians are mentioned in these tablets 21 times. The personal names given as Indian probably belong to Iranians resident there (e.g. Abbatema and Šakšaka, see *ibid.* 122). Tablets mentioning Arachosia (called *prkn/parikāna*, apparently the same as the Paricanians of Herodotus) are discussed in Vogelsang 1985, 82ff. *Prkn* is mentioned also in the Aramaic inscriptions of the so-called “Haoma-utensils” of Persepolis (*ibid.* 85f.). For a possible Indian mentioned in the fortification tablets see below.

²⁵⁶ I shall return to this question later (in II.8.)

²⁵⁷ In epics and later sources, see references in Weber 1892, 989f. Weber (1900f.) thinks it unlikely that the Vāhlika or Bāhlika of these passages would refer to Bactria, but it is not at all unlikely that horses came from (or through) Bactria. The Pañjāb (where Weber located Vāhlika) was not known for its horses. See also Witzel 1980, 88f. and *passim*.

²⁵⁸ Issberner 1888, 24, followed e.g. by Kiessling (1900, 56), Jackson (1922, 336), Gisinger (1929, 620), Roos (1939, 228), Hennig (1944, 117), Schwarz (1966, 64), Momigliano (1975, 125) and Dandamaev (1982, 115). Daffinā (1967, 11) goes still further, making him also the conqueror of Sind.

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not clear. Darius had many servants among the different nations subject to him, and in naval ventures such men who were accustomed to the sea were used.²⁵⁹ There is some evidence of a Carian naval station situated in the Shaḥḥ al-^cArab,²⁶⁰ and it is quite possible that this famous nation of seafarers was well represented in the expedition.²⁶¹

It was Richard Delbrück who first noted that Scylax was perhaps not the leader.²⁶² In fact, there is really no ground to assume that he was. Herodotus mentioned him by name, because he was the source for the expedition, but had Scylax been the leader, why did Herodotus not mention it? Instead he said that Darius "sent in ships both others on whom he could rely and also Scylax".

The next question is where was the starting point, the town Caspatyrus in the country of Pactyice? The so-called better reading "Caspapyrus" (supposing the name contains OIA *pura* 'town') is given by Hecataeus, who calls it a town of Gandāra and ΣΚΥΘΩΝ Ἀκτῆ.²⁶³ First, it must be noted that this is by no means confirmed. Hecataeus is the more ancient author, true, but we do not have the text of Hecataeus. A reading transmitted through the complications involved in a quotation by Stephanus preserved only in an abridgement of his geographical lexicon is much more liable to be corrupted than a reading found in the majority of manuscripts of preserved original work.²⁶⁴ A derivation from OIA *pura* does not confirm the Hecataean reading if the suggested etymologies with *pura* remain as conjectural as they are. In fact the Herodotean -τυρος is not so bad.²⁶⁵ There is at least one place in Gandhāra which contains a similar ending: Śālātura, the birthplace of Pāṇini.²⁶⁶ It is also possible that the name would have an Iranian derivation. But at present I must leave the question open.

In the nineteenth century scholars tried to locate the town either at Kabul²⁶⁷ (it is not always clear, but apparently the town was meant) or in Kashmir.²⁶⁸ These, of course, will not do, as one must go a long way from Kabul (the town) before the Kabul (the river) becomes navigable,²⁶⁹ and it is very unlikely that the Achaemenian power could

²⁵⁹ Cf. Hdt 7, 89–99 on Xerxes' navy.

²⁶⁰ Herzfeld 1968, 8f. and 42ff. Of course, Herzfeld often reads too much into his sources.

²⁶¹ Cf. Herzfeld 1968, 277. Nevertheless, Herzfeld thought that Scylax was the leader (281), even the admiral of Darius' navy (286).

²⁶² Delbrück 1958, 20 followed by Schiwiek (1962, 10, note 41) and Altheim & Stiehl 1970, 154.

²⁶³ Hecataeus F 295 Κασπάπυρος· πόλις Γανδαρική, Σκυθῶν δὲ ἀκτῆ Ἐκαταῖος Ἀσία. I have preserved the manuscript reading, although the editors accept Sieglin's ἀντίη.

²⁶⁴ I have discussed Stephanus and the fragments of Hecataeus preserved by him more fully in Karttunen, *forthcoming* b.

²⁶⁵ It is true, as Caroe (1958, 441, note 11) observed, that two (in fact three) manuscripts of Herodotus read Κασταπύρου at 4, 44. These three often deviate from the others without forming an exceptionally good source of text tradition, and as at 3, 102 they all read Κασπατύρω, I doubt if their anomalous reading at 4, 44 is worth much.

²⁶⁶ Scharfe 1987. See also Marquart 1907, 246f.

²⁶⁷ For references, see Gisinger 1929, 622.

²⁶⁸ Wilson 1841, 136f., Lassen 1852, 630f., McCrindle 1885, 108f. and others, see Gisinger 1929, 622.

²⁶⁹ Wilson 1841, 136f. Later the navigability of the Kabul is discussed with (apparently) good local knowledge by Caroe (1958, 30ff. with a map). The slight difference of opinion in this respect between

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ever have reached Kashmir. As the name suggested for Kashmir by Wilson,²⁷⁰ Lassen and others, *Kāśyapapura*, is also attested as an ancient name of Multan, Cunningham suggested that the ancient *Κασπάπυρος* was actually here.²⁷¹ Although a mere similarity of two names is not a strong argument (if acceptable at all), Foucher accepted all the consequences: that the place Scylax and others departed from was here, on the Ravi in the Pañjāb, that the eastern frontier of the Achaemenian empire was also here, and that Alexander therefore only reconquered the old Achaemenian territory.²⁷² This is clearly too much to build on a similarity, which may well be coincidental.

Most scholars have therefore agreed that the town must have been in ancient Gandāra, more or less corresponding to the Gandhāra of Indian sources, and most likely situated around the confluence of the Indus and the Kabul. Much force is sometimes placed on Herodotus' assertion that they "sailed down the river towards the east and sunrise to the sea". As such this is clearly irreconcilable with geography. An eastern arm of the Indus Delta suggested by Breloer²⁷³ only starts a long way from the expedition's departure point, wherever its exact location was. If we instead ignore the words *ἐς θάλασσαν* it is easy to think of a beginning of the voyage on the Kabul, where the direction indeed would be to the east. And the Kabul somewhere near its confluence (in any case after Khaibar) has been suggested by the majority of scholars.²⁷⁴ But as was noted already by Wilson and Lassen, the words are not necessarily based on what Scylax said about the actual direction. As India was the easternmost country bounded by the Eastern Ocean, it was only natural to suppose that the Indus flowed eastwards, supposing one has never been there as Hecataeus and Herodotus had not.²⁷⁵ Therefore, the starting place can equally as well have been on the Indus,²⁷⁶ as Herodotus himself seems to think.

In addition to the *Kāśyapapura* already known to Wilson and Lassen, at least five different etymologies have been proposed for *Κασπάπυρος/πυρος*. Lindegger's *pur(t)* 'town' of the *Κάσπιοι* (Hdt 3, 93) is just a guess.²⁷⁷ According to Marquart, *Κασπάπυρος* should be derived from the supposed Old Persian **Kuspapura*, corresponding to MIA **Kus(u)wapura* for OIA *Kusumapura*, synonymous with *Puṣpapura*, which was

Caroe and Tucci (who had also been to the spot; Tucci 1977, 16) is insignificant as the river had a more or less different course in ancient times. A difference of some ten miles does not count.

²⁷⁰ Wilson 1841, 137, but see Stein 1900, 353f.

²⁷¹ Cunningham 1871, 197f. followed e.g. by Breloer (1941, 15), Lamotte (1958, 112) and Eggermont (1970, 70).

²⁷² Foucher 1938, 340 and passim, again 1947, 190ff. Actually the Indian name is attested only from some 1500 years later, in *Rājatarāṅginī* and al-Bīrūnī, see Dey s.v. and Daffinà 1980, 3.

²⁷³ Breloer 1941a, 16, then Schiwiek 1962, 13, note 60. According to Eggermont (1970, 80) the middle branch of the seven was the only navigable one in the time of Alexander.

²⁷⁴ E.g. Mannert 1829, 4, Pullé 1901, XIII, Jackson 1922, 336, Gisinger 1929, 622, Hennig 1944, 119, Caroe 1958, 30ff., Schiwiek 1962, 12f., Treidler 1965, 475f., Miller 1969, 248f., Tucci 1977, 16f., Daffinà 1980, 3, Lindegger 1982, 19f. and P'jankov 1987, 265f.

²⁷⁵ Benfey 1840, 40 (referring to Larcher), Wilson 1841, 136 and Lassen 1847, 433, note 3, see also Marquart 1907, 242f. and Hennig 1944, 118 and the reconstructed maps for Hecataeus and Herodotus in Pullé 1901, 54 and 56 and in Thomson 1948, 99.

²⁷⁶ As suggested e.g. by Herzfeld (1968, 339) and Altheim & Stiehl (1970, 153).

²⁷⁷ Lindegger 1982, 26f.

supposed to be identical with *Puruṣapura*, the ancient name of Peshawar.²⁷⁸ In a similar way he derived ΠΑΚΤΥΪΚΉ from MIA *Pukhalāvatī*, modern Charsadda.²⁷⁹ These cannot be correct for several reasons. Marquart's *Kus(u)wapura* is against the rules of MIA (pointed out by Kern, as he himself confesses). Both *Kusumapura* and *Puṣpapura* seem to be known only as names for Pāṭaliputra,²⁸⁰ and even if accepted in the northwest, both derivations seem rather artificial. And as a last blow, *Puruṣapura*/Peshawar was founded only by Kaniṣka more than a half millennium later.²⁸¹

As Peshawar was a later foundation we cannot accept Herzfeld's other etymology, either. The starting-point was a hypothesis suggested by Sprengling and Henning.²⁸² They refer to an inscription of Shapur, where a place called ΠΑΣΚΙΒΟΥΡΩΝ (gen.)/ *PŠKBVR* is mentioned as a part of the Kushan kingdom. Of course, this can well be Peshawar, but difficulties arise with ΚΑΣΠΑΤΥΡΟΣ/ πυρος. It is emended to *Πασκάπυρος, which may well be compared with PŠKBR, *Puruṣapura* and its forms in Chinese and Arabian sources.²⁸³ But the derivation is not good enough to warrant an emendation extending to two different text traditions (Herodotus and Hecataeus/Stephanus). The rather sound etymology suggested by Henning for PŠKBR and Peshawar²⁸⁴ was extended by Herzfeld, not to *Πασκάπυρος, but to ΠΑΚΤΥΪΚΉ, which is much less sound. According to him the Greek name would go back to an Old Persian word **Puṣṣapura*, this to a similarly unattested MIA **Puruṣapura* corresponding to OIA *Puruṣapura*. But it is impossible to think that in the early MIA of the sixth century B.C. there would have been a form like **purṣa*. Normal MIA for *puruṣa* is *purisa*,²⁸⁵ but centuries later, in northwestern Niya Prākṛt *carapurūṣa* is still found.²⁸⁶

ΚΑΣΠΑΤΥΡΟΣ/πυρος Herzfeld connects with the kind of spikenard called in the *Periplus* 48 ΚΑΤΤΥΒΟΥΡΙΝΗ. This word is obviously corrupt, and Herzfeld seemed to be unaware that as early as in 1855 C. Müller had suggested an emendation to ΚΑΣΠΑΠΥΡΗΝΗ,²⁸⁷ which was approved e.g. by Schoff.²⁸⁸ Herzfeld's emendation reads ΚΑΠ(Π)ΑΒΟΥΡΙΝΗ, and as original form used by Scylax he suggests ΚΑΠ(Π)ΑΠΥΡΟΣ, related to **kappapūra*, which should be an MIA form for Pāli *kappūra*. The spikenard of the *Periplus* should mean camphor.²⁸⁹ But although ΚΑΤΤΥΒΟΥΡΙΝΗ can well be

²⁷⁸ Marquart 1907, 246f., note 3.

²⁷⁹ Marquart 1907, 179 (preceded by Benfey 1840, 40, then e.g. Lindegger 1982, 24f. and P'jankov 1988, 263). He also suggested that Scylax had perhaps written ΠΑΚΛΥΪΚΗ, which was then assimilated by the ethnic name ΠΑΚΤΥΕΣ.

²⁸⁰ Dey ss.vv.

²⁸¹ Foucher 1942, 43 and Eggemont 1970, 70.

²⁸² Sprengling 1940, 354f. and Hennig 1947, 53f.

²⁸³ See also Caroe 1958, 32.

²⁸⁴ Peshawar derived through **poṣṣa*/**poṣsa*/*pōsa* < *purṣa*/*pursa* < MIA *purisa* or OIA *puruṣa*. This **Poṣṣakapura* was then Iranized into **Poškapur* and further *Poskaḥur* (Henning 1947, 53).

²⁸⁵ Pischel 1981, § 124. A reduction of middle *u* is found (§ 148), but never here.

²⁸⁶ Burrow 1937, 112, number 01 (765), Under-tablet, line 4.

²⁸⁷ Noticed in the apparatus to Frisk's edition.

²⁸⁸ Schoff 1912, 189.

²⁸⁹ Herzfeld 1968, 339f.

related to Κασπάτυρος/πυρος, Herzfeld's suggestion means the equation of arbitrary emendation with an equally arbitrary reconstruction, and all this on the condition that spikenard is in fact camphor. But we are also told that no species of plants yielding a kind of camphor is indigenous in India.²⁹⁰

A further explanation is offered by Eggermont, who equates Κασπάτυρος (sic) with Spatura of *Tabula Peutingeriana*, and also with OP Kāpiśakāniš and OIA Kāpiśt.²⁹¹ But as Kāpiśt/Begram is situated to the west of Khaibar, a naval expedition could not begin there.

We must also consider Πακτυϊκή. In his provincial list Herodotus groups it together with the Armenians in the 13th νομός.²⁹² Herodotus himself asserts, it is true, that this fiscal division is not always strictly geographical,²⁹³ and sometimes this has been taken as an explanation.²⁹⁴ But in other νομοί the deviation of geography is never as large as here. Some have judged it to be just an error and moved Πακτυϊκή into some more proper location, as in the seventh νομός containing Σατταγύδαι, Γανδάριοι, Δαδίκαι and Ἀπαρύται. But we should be careful in using supposed errors in our sources as a way of making meagre evidence provide neat explanations.

There are several other Herodotean passages which speak of Πάκτυες, and these can often also be explained as living somewhere near Armenia.²⁹⁵ It is quite possible that there has indeed been two wholly different names similar enough to arouse confusion when adopted in Greek, perhaps through Old Persian. There would then be a people called Πάκτυες near Armenia and the country Πακτυϊκή within the confines of India.²⁹⁶ It was this Πακτυϊκή where the town Κασπάτυρος/πυρος was situated and where the naval expedition set sail.

As to the Indian Πακτυϊκή (we do not know for certain whether there were a people called Πάκτυες in the east), the name has often been explained by modern *paštō/paxtō*, the ethnic name of the Afghans,²⁹⁷ but severe difficulties are involved. There is the huge chronological gap: *paxtō* is only a late dialectal form for older *paštō* and the people themselves are mentioned, under the name Afghan, only in the 10th century A.D. It has also been repeatedly noted that the Middle and Old Iranian forms behind *paštō* cannot be

²⁹⁰Watt s.v. Camphor.

²⁹¹Eggermont 1982, 65. On Spatura see also Daffinà 1980, 8.

²⁹²Hdt 3, 93 ἀπὸ Πακτυϊκῆς δὲ καὶ Ἀρμενίων καὶ τῶν προσεχέων μέχρι τοῦ πόντου τοῦ Εὐξείνου τετρακόσια τάλαντα νόμος τρίτος καὶ δέκατος οὗτος.

²⁹³Hdt 3, 89 καταστήσας δὲ τὰς ἀρχὰς καὶ ἄρχοντας ἐπιστήσας (Δαρείος) ἐτάξατο φόρους οἱ προσιέναι κατὰ ἔθνεά τε καὶ πρὸς τοῖσι ἔθνεσι τοὺς πλησιόχωρους προστάσων, καὶ ὑπερβαίνων τοὺς προσεχέας τὰ ἑκαστέρω ἄλλοισι ἄλλα ἔθνεα νέμων.

²⁹⁴Thus e.g. Daffinà 1967, 25.

²⁹⁵Hlopin 1975, 47ff.

²⁹⁶Marquart 1907, 178f. and Hlopin 1975, *passim* (especially 48). On p. 53f. Hlopin identifies the eastern Πακτυϊκή with Παρικάνιοι and Παραιτακηνή.

²⁹⁷E.g. Lassen 1847, 432, Stein 1900, 353, Pullé 1901, XIII, Grierson 1921, 5, Foucher 1947, 410, Caroe 1958, 33ff., Treidler 1965, 475 and Herzfeld 1968, 337f. With this is sometimes connected Vedic *paktha* (e.g. Daffinà 1980, 2), but see Mayrhofer s.v.

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connected with Πάκτυες/Πακτυϊκή.²⁹⁸ An OP **paχstu* and Greek *Παχτυες as suggested by Herzfeld²⁹⁹ do not really help us, and Caroe seems to be simply trying to find as great an antiquity as possible for his beloved Pathans.³⁰⁰

In the fragment of Hecataeus, Κασπάπυρος is called Σκυθῶν ἀκτῆ, and this has been a cause of much discussion. The question is, what does the ἀκτῆ of the Scythians mean, as ἀκτῆ is commonly translated 'headland, promontory, peninsula'. I have found four attempts at an explanation, one of them leaning on an emendation.

If we allow Scythians only in the far north, Sieglin's Σκυθῶν ἀντίη 'situated opposite to the Scythians' seems to be the only possibility, and it has consequently been adopted by many scholars including both editors of Hecataeus' fragments.³⁰¹ The emendation gains some support from the use of ἀντίος in Herodotus.³⁰² But an emendation which is otherwise unwarranted is something one should avoid.

Among the other solutions I do not think we need concern ourselves much with Herzfeld's "a kind of parallel running along the coast line", indicating that the town is situated on the same parallel with the Scythians. It contains an interesting idea about the geographical system of Hecataeus, but unfortunately the uses of ἀκτῆ are inconsistent with it.³⁰³ The same can be said of Foucher's "limitrophe des Scythes".³⁰⁴

More interesting was Marquart's idea of Κασπάπυρος being the "Stapelplatz" of the Amyrgian Sakas living in Hindukush or Pamir.³⁰⁵ It should be the place where the boat traffic on the Indus began and would therefore be an important trade centre for northern

²⁹⁸ Morgenstierne 1940, 141ff., Bailey 1952, 430f. and Grantovski 1963, 10f. On Afghans see also Foucher 1947, 252.

²⁹⁹ Herzfeld 1968, 337f. As Hdt 7, 67 describes Πάκτυες as fur-clad (but probably this refers to the Armenian Πάκτυες), Herzfeld (337) locates them in the Kabul-Ghazni area well up in the mountains. In spite of this he then places his Κασπάπυρος on the Indus, between Ohind and Attock (339). Of course, a minor inconsistency like this must be excused in a book published posthumously from manuscript notes.

³⁰⁰ Caroe 1958, 36ff. argues for an originality or high antiquity of the form *paχtō* with arguments which can hardly be called linguistic, and then proceeds to identify Herodotus' Ἀπαρύται (37f.), Σατταγύδαι (38ff.) and Σαγαρτίοι (40f.) with modern Afghan tribal names. The old idea of Ἀπαρύται being present-day Afridis is also accepted by Lamotte (1958, 113) and Tucci (1977, 14f.).

³⁰¹ E.g. Herrmann 1919, 2270, Hennig 1944, 119 as well as Jacoby's and Nenci's editions.

³⁰² Hdt 1, 201 ἀντίον δὲ Ἰσσηδόνων ἀνδρῶν, and 2, 34 ἡ δὲ Αἴγυπτος τῆς ὀρεινῆς Κιλικίης μάλιστα κῆ ἀντίη κεῖται.

³⁰³ Herzfeld 1968, 338, note 5. Actually this would fit much better with ἀντίος, but Herzfeld definitely dismissed this emendation. Of course, in older works it is often stated that ἀκτῆ in Hecataeus means a kind of parallel in a map (see Jacoby 1912, 2718f.). But though such parallels may have been important in his geographical system, there is little evidence for the word ἀκτῆ used in this connection. Among the remains of Hecataeus it is found only once, in the fragment we are discussing, and Herodotus does not support such use (though see 4, 41 ἡ δὲ Λιβύη ἐν τῇ ἀκτῇ τῇ ἐτέρῃ ἐστί). Therefore, I fail to see why Daffinà (1980, 4ff.) even after considerable discussion of the uses of the word, can accept Herzfeld's explanation as definite.

³⁰⁴ Foucher 1938, 348. Here it is not important that with his Multan idea Foucher still supported the antiquated idea of Thomas (1906, passim) of Sakas already living in Seistan/Sakastene at that time. See Daffinà 1967, 83ff.

³⁰⁵ Marquart 1907, 242. The same idea is also found in Lindegger's (1982, 24) "Landvorsprung oder Uferplatz der Skythen". See also Vogelsang 1985, 80, note 46.

and northwestern areas. But again, we must object that "Stapelplatz" is a rather arbitrary translation of ἀκτή.

Grantovski and Tucci, who speak of a shore or bay (of the Amyrgian Sakai),³⁰⁶ have given more consideration to the Greek, but such an explanation seems somehow too insignificant. Why call Κασπάτινος/πυρος just "the shore of the Scythians"? It is best to have a look at the different meanings of ἀκτή and their use in early Greek.³⁰⁷ An early and rather common meaning is 'headland' and more generally 'sea coast' (especially a rough one), but these are mostly used only with reference to the sea.³⁰⁸ When the word is used with rivers, with dictionary meaning of 'bank', all examples are from poetry. Nevertheless, it may be significant that out of the four examples supplied, three could be given a secondary meaning of a landing place.³⁰⁹

The normal meaning of ἀκτή in early prose is 'peninsula, land surrounded by sea', used for instance by Herodotus for describing Asia Minor and Arabia as the two arms of Asia.³¹⁰ It is also used for minor peninsulas and promontories.³¹¹ I am afraid that this is not a definite enough solution. If we accept a poetic use for Hecataeus – it is not impossible in so early an author – a landing place of the Amyrgian Scythians is possible. And if we could apply the peninsula – otherwise used exclusively with the sea – to a land bounded by two rivers, a mesopotamia, why not the land bordering on the Indus and the Kabul and perhaps at least partly inhabited by the Amyrgian Sakai?

There is still another possibility. What if the words Σκυθῶν ἀκτὴ do not really come from Hecataeus? The fragment comes from Stephanus, or more exactly from the drastically and often carelessly abridged epitome of his lost geographical dictionary. It is quite possible that the words in question belonged originally to Stephanus himself or some lost quotation. From the early centuries A.D. onwards, at least, there was no difficulty in finding Scythians in this part of India, which then came to be known as Indoscythia in classical literature.³¹²

³⁰⁶ Grantovski 1963, 25 "берега скифов"; Tucci 1977, 16: "Aktè is not a port, it is a shore with easy access, a bay." But when Tucci (ibid. 17) proceeds to explain it as a place where timber felled in the mountains and floated down the rivers was (and still is) taken ashore and heaped up, I must agree with Daffinà (1980, 8) and find it quite far-fetched. Instead of the Saka Haumavarga, Junge (1939, 32 and 82f.) located here the Saka Tigrakhauda. According to Tucci (1977, 16f.), they (Saka Haumavarga) are not in fact real Sakas at all but Dards (Nuristanis?) erroneously identified as Sakas.

³⁰⁷ Liddell & Scott & Jones s.v., Herodotean examples in Daffinà 1980, 4.

³⁰⁸ In Herodotus there are two instances (7, 45 and 8, 95) both being landing places for ships.

³⁰⁹ Aesch. Ag. 697: κελσάντων Σιμόεντος ἀκτὰς; Soph. Ant. 813ff.: ἀλλά μ' ὁ παγκοίτας Ἴδιδας ζῶσαν ἄγει τὰν Ἀχέροντος ἀκτάν; Pind. I. 2, 42: πλέων Νείλου πρὸς ἀκτάν; Pind. N. 9, 40: ἀμφ' ἀκταῖ Ἐλώρου is uncertain. In all these examples the genitive of the river name is given.

³¹⁰ Hdt 4, 38f. Probably Libya in 4, 41 is similarly meant to be a great peninsula (so interpreted e.g. by Daffinà 1980, 4).

³¹¹ E.g. Hdt 7, 33f., 183, 188, 191 and 9, 120 for minor promontories in Greece, further Thuc. 4, 109 for Athos and Arist. Ath. Pol. 42, 3 for Piræus, cf. Daffinà 1980, 4 and Liddell & Scott & Jones s.v.

³¹² This idea was suggested orally to Daffinà (1980, 8) by A.D.H. Bivar, and much earlier it was stated (but mostly left unnoticed) by Kiessling (1920, 1107). See also my discussion on Hecataeus and Stephanus in Karttunen *forthcoming* b.

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The last question to be discussed in this chapter is the relation between the naval expedition and the actual conquest of the lower Indus country by Darius. The actual words of Herodotus seem to suggest that the expedition preceded the conquest. Darius was desirous to learn where the Indus actually reached the sea. For this purpose he sent off the expedition, and after it had sailed round – which took thirty months³¹³ – Darius subdued the Indians and opened up sea traffic.³¹⁴ Therefore many scholars have simply supposed that the expedition was first and conquest only came subsequently.³¹⁵

Serious objections have, however, been raised against this view. The general difficulty of a voyage of what apparently was a considerable naval contingent through unsubdued country inhabited by warlike tribes³¹⁶ on both sides of the river is often mentioned.³¹⁷ There is also the Hecataean fragment on Ὠπίαι and τεῖχος βασιλῆιον,³¹⁸ apparently an Achaemenian royal fort in their country. If it was mentioned by Scylax, it must have already been there, so runs the argument.³¹⁹ But is this really the case? Some scholars are over-confident that everything Hecataeus wrote about the eastern regions and especially India must necessarily come from Scylax.³²⁰ But even if it does come from Scylax, which is no doubt likely, was "Scylax" really an unrevised diary of the naval expedition? Even if the garrison was founded after the expedition, Scylax might have known of it and mentioned it in his text, and so could Hecataeus too, even if his general account was derived from Scylax. As this is the only fortified place in the lower Indus country³²¹ (the *India* of the Achaemenian inscriptions) that is mentioned in our sources for the pre-Alexander period, it may well have contained even the administrative headquarters of the whole province.

Such arguments have been offered to show that the actual conquest must have been first, the naval expedition following only subsequently. But as this is directly contrary to the source (Herodotus) and the arguments are open to criticism, the theory can be dismissed. More interesting is Breloer's idea that the expedition and the conquest took place simultaneously.³²² In addition to what had been said on behalf of the conquest

³¹³ A duration of 30 months seems rather long even for a slow coasting voyage from the Indus to the Suez (but see Reese 1914, 39, note 1). Perhaps this number comes, as suggested by Herzfeld (1968, 282), from the fact that it seems to have been the duration of the Phoenician circumnavigation of Africa discussed by Herodotus a little earlier (4, 42, cf. Hennig 1944, 63ff.) and mentioned even here (4, 44).

³¹⁴ The Suez inscription of Darius (OP DZc in Kent 1953, 147, for the Old Egyptian inscription see Herzfeld 1968, 293f.) mentioning ships sailing from Egypt to Persia is clear proof of the existence of such traffic. Schiwiek's (1962, 15f.) suggestion that this must refer to a return voyage of "Scylax's navy" from Egypt to the Gulf is rather arbitrary.

³¹⁵ So e.g. Reese 1914, 40, note 2, Gisinger 1929, 621ff. and Hennig 1944, 117f.

³¹⁶ Of course, we cannot be certain that they were warlike then, as they surely were in the times of Alexander.

³¹⁷ Kiessling 1900, 56f., Jackson 1922, 336, Schiwiek 1962, 9ff., Herzfeld 1968, 282, Chattopadhyaya 1974, 16 and Frye 1984, 104.

³¹⁸ Hecataeus F 299. In Doriscus in Thrace Herodotus (7, 59) mentions another τεῖχος βασιλῆιον.

³¹⁹ Jackson 1922, 336, Breloer 1941a, 17, Schiwiek 1962, 11 and Herzfeld 1968, 282.

³²⁰ This will be discussed in chapter III.

³²¹ On its location see Stein 1939 (not entirely convincing).

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being first, Breloer defends his idea with a new interpretation of the Herodotean passage. But his translation, "nachdem diese herumgesehelt waren, hat Dareios sowohl die Inder bezwungen, als auch dieses Meer beherrscht", is hard to accept for the Greek: μετὰ δὲ τούτους περιπλώσαντας Ἰνδοὺς τε κατεστρέψατο Δαρεῖος καὶ τῇ θαλάσῃ ταύτῃ ἐχρᾶτο. For the imperfect ἐχρᾶτο it seems impossible. Therefore, I cannot see any possibility for denying that Herodotus places the conquest after the expedition. The two can only be combined if we suppose that the expedition was the first phase of the conquest, and that it already involved military operations. But even if the river route was cleared by force, the country was not necessarily subjugated when the expedition continued its voyage to the sea, and perhaps the military phase in India was concluded only after the naval expedition had arrived in Suez. Herodotus is never at his best when he is giving the motives of people,³²³ and there is thus no need to stress his remark about the scientific nature of the naval expedition.³²⁴ Even if the expedition clearly preceded the conquest, it seems likely that there were military and commercial considerations behind it.³²⁵ This is not even contrary to the words of Herodotus, and it is probably anachronistic to ascribe scientific interests well attested in the case of Alexander to the Achaemenid monarch living two centuries earlier.

7. The Achaemenian Empire: Peoples and Wares

The great Achaemenian metropolises like Persepolis and Susa were swarming beehives containing many races and languages. There were, of course, the three languages of the Achaemenian royal inscriptions (Old Persian, Elamite and Akkadian), and Aramaic, too, was in official use everywhere in the empire.³²⁶ In addition, remains of Greek and Phrygian are found in Persepolis, and surely many other languages of the empire were heard and, at least by some people, understood there.³²⁷

Classical authors like Herodotus and Xenophon inform us of the many Greeks as well as the representatives of other nations employed by the state, and others were present for private purposes. There were Greek physicians in the royal court³²⁸ and Greek historians travelling in search of information.³²⁹ Although the men mentioned as Indians in Elamite

³²² Breloer 1941a, 16f. followed by Schiewek 1962, 9ff. and Daffinà 1980, 1f.

³²³ See e.g. Frye 1984, 105.

³²⁴ This was done e.g. by Kiessling (1900, 56).

³²⁵ Cf. Breloer 1941a, 6.

³²⁶ According to Cameron (1973, 52) there are some 500 Aramaic tablets found at Persepolis. See also Greenfield in Gershevitch 1985, 698ff.

³²⁷ Cameron 1973, 52f. For Greek, see also Momigliano 1975, 125f.

³²⁸ Democedes under Darius (Hdt 3, 129ff., cf. Filliozat 1964, 244f.), and Ctesias under Artaxerxes II (cf. Brown 1978a).

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tablets seem to have Iranian names,³³⁰ real Indians were not missing. There were those Callatians confronted with the Greeks by Darius³³¹ and especially the many who served in the army.

The army was a real melting pot, where people from every corner of the empire could easily meet, as we can see from the Herodotean catalogue of Xerxes' army.³³² But all these peoples were not only called from their distant home provinces just for great invasions like that attempted by Xerxes. There were permanent military colonies formed of separate nations, and fortunately we have clear evidence of an Indian colony in Mesopotamia. The "papers" (i.e. tablets) of a firm acting as royal contractor, Murašū and Sons, Nippur, mention such soldier colonies³³³ there several times and among them also Indians.³³⁴ Another fifth century Nippur document confirms their presence.³³⁵ This seems to be enough, though the other points sometimes mentioned as evidence of an Indian presence in Mesopotamia – a "Hindu woman keeping an inn at Kish"³³⁶ and a supposed Brāhmī inscription³³⁷ – are probably undependable.

In the great army of Xerxes there were Indians and Eastern Ethiopians, as well as Gandārans and Dadicae.³³⁸ There was also Indian cavalry with horses and wild asses,³³⁹ probably similar to the depiction of wild ass in the Persepolis tribute bearer relief, commonly identified as Indian.³⁴⁰ When the disappointed monarch made off after Salamis and Mardonius was allowed to choose which troops he kept with him in Greece, he included both Indian infantry and cavalry.³⁴¹ At Plataea they were placed against the Greeks from Hermione, Eretria, Styra and Chalcis,³⁴² though the battle itself did not take place with this formation. Later we hear no more of these Indians. With one dubious

³²⁹ Both Hecataeus and Herodotus travelled in the empire, though we know few details of their travels. See Jacoby 1912, 2689f. and 1913, 247ff.

³³⁰ Certainly these names do not look Indo-Aryan, but only two tentative Iranian etymologies are proposed (mentioned in Dandamaev 1982, 122). Other solutions may appear. Recently Schmitt (1988) has found at least one name (in Fort. 11246,3 = Q-2584) which seems to be Indo-Aryan viz. *hh.na-an-da* (Nanda), who is mentioned as coming from Sind to Susa.

³³¹ Hdt 3, 38.

³³² Hdt 7, 61ff.

³³³ The so-called *ḫaṭru*-associations of cuneiform documents. They formed a system of standing military reserve and at the same time served as fiscal units and agricultural producers. See Stolper 1985, 70f.

³³⁴ Unger 1931, 39f., note 6 and Dandamaev 1982, 118f. There is a recent monograph (Stolper 1985) on the Murašū firm, but Indians are mentioned only in passing (p. 78f.).

³³⁵ Dandamaev 1982, 119.

³³⁶ Olmstead 1948, 119, but according to Dandamaev 1982, 119 this *LÚhi-in-du* can be also explained otherwise, and the name of the woman, Busasa, is not Indian.

³³⁷ Bobrinskoy 1936, still mentioned by Dandamaev (1982, 119), but after Torrey 1936 Bobrinskoy's identification of the inscription as Brāhmī is hardly acceptable anymore.

³³⁸ Hdt 7, 65 Indians, 66 Gandārans and Dadicae, 70 Eastern Ethiopians.

³³⁹ Hdt 7, 86 Ἴνδοι δὲ σκευὴ μὲν ἐσεσάχατο τῇ αὐτῇ καὶ ἐν τῷ πεζῷ, ἤλαυνον δὲ κέλητας καὶ ἄρματα· ὑπὸ δὲ τοῖσι ἄρμασι ὑπήσαν ἵπποι καὶ ὄνοι ἄγριοι.

³⁴⁰ Walser 1966, 94f. and plates 25 and 86.

³⁴¹ Hdt 8, 113.

³⁴² Hdt 9, 31.

exception,³⁴³ they were probably the only Indians visiting Greece we know of in our period. Outside Greece there was another military confrontation in 331 B.C. at Gaugamela, where among his troops Darius sent against the Macedonians some Indian soldiers and elephants.³⁴⁴

There were probably also Greeks – in addition to the half-Greek Scylax – serving as soldiers (mercenaries) or civil servants in the east, but this will be discussed in the next chapter. Surely the two peoples – Indians (not only those of Sind) and Greeks – did meet in the great centres of the empire. This can be the way the supposed exchange of ideas of religion and philosophy as well as other information might have taken place.³⁴⁵ Surely there might have been at least some contact, although every single case of such exchange proposed so far can also be explained otherwise. With the exception of the Callatian episode described by Herodotus,³⁴⁶ there is no direct evidence of any exchange of ideas. We must therefore be rather careful not to build too much on it.

The origins of the trade between India and the West were discussed in a preceding chapter. It was seen that although Indo-Babylonian trade during the pre-Achaemenian period is likely, there is no unambiguous evidence for it. In the Achaemenian period there are no more doubts of the existence of a flourishing trade between Persia and Mesopotamia at one end, and the Achaemenian Indian provinces, and at least to some extent also the countries beyond,³⁴⁷ at the other end.

Much of our evidence comes from Mesopotamia and a greater part of this trade was probably maritime. Although the Achaemenids get the credit for creating the first good and extensive network of roads in the ancient world,³⁴⁸ the water route was the only really practical way for greater transportations.³⁴⁹ It is hardly imaginable that the Gandāran (not to speak of Carmania) timber used by Darius in his Susa Palace³⁵⁰ came with the caravans, although this was probably the way gold and precious stones were brought from Central Asia.³⁵¹ Both ways existed and were used, but apparently India and even the Indian provinces of the empire were little involved in Western economy when the sea route fell into disuse after Darius.

Several reasons for this decline in trade have been suggested. Kennedy thought that the destruction of Babylon by Xerxes in 482 B.C.³⁵² crushed the economy of the country

³⁴³ The Indian sage who according to a tradition ascribed to Aristoxenus (late fourth century B.C.) came to Athens and met Socrates (see chapter IV.2).

³⁴⁴ Arrianus, *Anabasis* 3, 8.

³⁴⁵ See chapter IV.2. Filliozat 1964, 238ff. suggests that some exchange of medical knowledge happened this way.

³⁴⁶ Hdt 3, 38.

³⁴⁷ The testimony of the *Bāverujātaka*.

³⁴⁸ Casson 1974, 53.

³⁴⁹ Casson 1974, 65. It remained so up to the era of the railways.

³⁵⁰ DSf 34f. *yakā hacā Gadārā abariya utā hacā Karmānā* – “the *yakā*-timber was brought from Gandāra and from Carmania” (Kent 1953, 143f.).

³⁵¹ DSf 36ff. (Kent 1953, 143f.).

³⁵² For this destruction see e.g. Roux 1964, 372 (and 440, note 5 for references).

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and immediately caused the trade to cease.³⁵³ Delbrück pointed out that while the wars in Greece bound the Persian navy in the Mediterranean, the eastern sea routes were no longer safe for merchantmen.³⁵⁴ Schiwiek points to the fact that Persians were no seamen. If Darius was interested in the sea, his successors were not, and so the sea traffic stagnated after Darius.³⁵⁵ None of these explanations can be accepted as such. Babylon was destroyed and the economy declined, but it was not crushed at once, and the flourishing metropolis of Susa also participated in the Indian trade.³⁵⁶ The absence of the navy may have contributed – in a way it also indicates the lack of interest in naval questions – and the route was not easy.³⁵⁷ A slow decline seems more likely than a drastic termination. In the fourth century it had certainly more or less ended.³⁵⁸

The silence of Herodotus – who knew only Scylax – and Ctesias may suggest that it had already ended in the fifth century, but Herodotus was not necessarily interested in foreign merchantmen, and we do not even know the whole text of Ctesias. Probably neither of them visited Mesopotamian ports. That there were still Indian products coming even to Greece suggests that some contact was still maintained.³⁵⁹ During the same period the Achaemenian grip loosened in Northwest India, but we do not know the exact date. Apparently both political and economical ties were to some extent³⁶⁰ cut off for a while, but we cannot say, how it actually happened. Perhaps the sea trade ceased first, political contact and even some trade could be maintained through Bactria, where Achaemenian presence certainly continued. Bactrians and even Bactrian merchants are mentioned several times by Ctesias,³⁶¹ and most of the Indian products he says he has seen himself are small enough to be easily transported in a caravan.³⁶²

We know of many items of this trade. Often they were sold in more distant markets and several Indian products appear even in Greece during the fifth century. While Herodotus had not heard of Indian elephants,³⁶³ Ctesias had seen elephants himself in Mesopotamia, where they felled date palms at their mahout's command. He knew also of their use in Indian warfare.³⁶⁴ As we have seen, the great king still had elephants at

³⁵³ Kennedy 1898, 269ff.

³⁵⁴ Delbrück 1956, 21 (though he was speaking of the route around the Arabian peninsula).

³⁵⁵ Schiwiek 1962, 7f. and 19f.

³⁵⁶ As testified by DSf and the Elamite tablets discussed by Dandamaev (1982, 120ff.).

³⁵⁷ As testified by Nearchus.

³⁵⁸ Nearchus certainly did not find a lively trade route and he could not even obtain pilots for the whole route (as pointed out by Sedlar 1980, 87). He did, however, find pilots at least for shorter distances (this is emphasized by Delbrück 1956, 19).

³⁵⁹ Thus for instance pepper from South India is mentioned in the Hippocratic corpus.

³⁶⁰ Indian troops and elephants at Gaugamela show that this was not complete. See Bosworth 1988, 119.

³⁶¹ Ctesias F 45, 6 and 45h. See also P'jankov 1965, 41f.

³⁶² Elephants are certainly not light, but they are not very well suited for sea transport either, and at least they can walk.

³⁶³ He mentions ἐλέφας twice (3, 114 and 4, 191), but he did not say anything about the animal and referred only to the African species.

³⁶⁴ Ctesias F 45b, cf. Karttunen 1981, 106. It seems to have taken almost a further century before the first elephant appeared in Greece.

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Gaugamela. The battle was the first instance we know in the West in which elephants were actually used in a war. Alexander soon adopted the idea and begun to collect war elephants for his army. Later they were favourites of the Hellenistic monarchs.

Other animals were also brought from India. Indian dogs were kept by Achaemenids in great kennels in Mesopotamia, and Xenophon was able to give advice about their use in a chase. I shall come back to this in greater detail in chapter VII.3. We have already seen that peacocks were brought and soon also bred, so that the bird reached even Greece by the middle of the fifth century. I have also noted that hens were probably known in Mesopotamia before the Achaemenian period, and probably in Persia too. Subsequently they were introduced into Greece, where the fifth century authors call it a "Persian" or "Median" bird.³⁶⁵ Its Indian origin was not known.

The first unambiguous account of Indian cotton is given by Herodotus.³⁶⁶ Cotton was certainly brought from India, but the details are difficult to ascertain. Egyptian cotton was probably the African species,³⁶⁷ and we do not know if the important cotton plantations of Bahrain were indigenous or introduced from India.³⁶⁸ In Greece at least cotton ceased to be a rarity only after Alexander's campaign, but this says nothing about Achaemenian Mesopotamia and Persia.

Another natural product, as has already been mentioned, was the *yakā* timber used in the building of Darius' palace at Susa. It was brought from Gandāra and Carmania. The Akkadian version of DSf confirms that a type of wood is meant and Gershevitch has shown that *Dalbergia sissoo* Roxb., a tree still growing in both countries, is probably intended.³⁶⁹

It seems that even South India to some extent participated (though not necessarily directly) in the trade, as we find rice and pepper among the products introduced into the West in the fifth century. There are so many different cereals and pulses in India,³⁷⁰ that I cannot agree that Herodotus³⁷¹ was necessarily or even probably describing rice;³⁷² nevertheless, it was introduced into Mesopotamia at this time.³⁷³ Soon it reached even Greece, as Sophocles seems to mention it.³⁷⁴ Pepper is mentioned several time in the

³⁶⁵ Richter 1975, 1239f.

³⁶⁶ Hdt 3, 106 τὰ δὲ δένδρεα τὰ ἄγρια αὐτόθι φέρει καρπὸν εἴρια καλλονῆ τε προφέροντα καὶ ἀρετῆ τῶν ἀπὸ τῶν οἴων· καὶ ἐσθῆτι Ἴνδοι ἀπὸ τούτων τῶν δένδρέων χρέωνται.

³⁶⁷ Wagler 1899, 170f. and Berzina 1982, 18f.

³⁶⁸ They are described by Theophrastus, *Hist. pl.* 4, 7, 7, cf. Wagler *l. cit.* and Bretzl 1903, 136ff.

³⁶⁹ Gershevitch 1957, 317ff. Even its OP name seems to be preserved in Brahui and Iranian as *jag* (*ibid.* 319).

³⁷⁰ See the list in Johnson 1941.

³⁷¹ Hdt 3, 100 καὶ αὐτοῖσι ἔστι ὄσον κέγχρος τὸ μέγαθος ἐν κάλυκι, αὐτόματον ἐκ τῆς γῆς γινόμενον, τὸ συλλέγοντες αὐτῇ τῇ κάλυκι ἔψουσί τε καὶ σιτέονται.

³⁷² As supposed e.g. by Stadler (1920, 517).

³⁷³ Dandamaev 1982, 120. In India, rice is (and was) mostly cultivated in the south, but not exclusively. The archaeological finds testify to rice cultivation even in Swat in the far northwest as early as in the second millennium B.C. (L. Costantini in *Stacul* 1987, 157 and 159). See also Laufer 1919, 372f.

³⁷⁴ Sophocles F 607 Nauck (from Athenaeus) ὀρίνδην ἄρτον. There have been attempts to explain

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Hippocratic corpus,³⁷⁵ and according to Athenaeus³⁷⁶ it was sold in Athens in the fourth century.³⁷⁷ Soon it was also described by Theophrastus,³⁷⁸ but his information may already come from those following Alexander. It seems probable that both rice and pepper were already known in Greece, but before the Hellenistic period they were great rarities.³⁷⁹

It has already been said that many ancient authors mentioned gold as one of India's most important exports. Some problems connected with this gold and its origin will be discussed later, in chapter VII.6. That precious and semi-precious stones were imported from India is more natural, as India has been an important producer for at least five thousand years.³⁸⁰ The Darius inscription at Susa also mentions that ivory was brought from Sind and Arachosia.³⁸¹ Among the fragments of Ctesias we find further Indian products brought at least occasionally to the West: curiosities, live animals, medicines, and even wine and cheese.

The idea that South India traded directly with Achaemenian Mesopotamia has often been put forward, but it seems that here again greater care is needed. Although such a trade is by no means impossible,³⁸² there is very little evidence for it. The people identified by Herodotus as Eastern Ethiopians may well (or may not, it is still a hypothesis) represent the ancestors of the Dravidian Brahuis of Baluchistan, but I cannot see how they could be the Dravidians of South India.³⁸³

it otherwise. The first unambiguous reference to rice comes only from the historians of Alexander (Stadler 1920, 517f.).

³⁷⁵ See chapter III.5.

³⁷⁶ Athenaeus 2, 66d quoting a 4th century comedian:

Ἄντιφάνης·
ὃ μὲν ἄρα πέπερι πριάμενός τις εισφέρει
στρεβλοῦν γράφουσι τοῦτον ὡς κατάσκοπον.
πάλλιν·
νῦν δεῖ περιόντα πέπερι καὶ καρπὸν βλίτου
ζῆτειν.

³⁷⁷ In spite of this (and without mentioning Hippocrates) Tarn did not believe that any real pepper came to Greece before the Indo-Greek period in the east (Tarn 1951, 370f.). Jong (1973, 137) follows Tarn so faithfully that he accepts this view, even after references to Hippocrates and Filliozat 1964 (Jong referred to the original French edition). But Tarn's "so-called African pepper" cannot explain how the oldest sources in the West were already using the word πέπερι, so clearly derived from the corresponding Indian word (see below), and the Antiphanes fragment cited above seems to indicate that pepper was imported from the Persian empire.

³⁷⁸ *Hist. pl.* 20, 1.

³⁷⁹ According to Tarn (1951, 371) pepper became common only in the first century B.C.

³⁸⁰ In addition to archaeological evidence see e.g. Ctesias F 45, 11.

³⁸¹ DSf 43–45 *piruš hya idā karta hacā Kūsā utā hacā Hidauw utā hacā Haraுவatiyā abariya* – "The ivory which was wrought here, was brought from Ethiopia and from Sind and from Arachosia" (Kent 1953, 143f.). Arachosia is of course unlikely as a place of origin, but its important role as a meeting place between India and Iran makes its appearance here understandable (see Vogelsang 1985, 81 and *passim*).

³⁸² Again we have reason to regret our deficient knowledge of the archaeology of the west Indian coast.

³⁸³ So Delbrück 1956, 25 ("Herodotus weiss von dunkelhäutigen Primitiven [!] in Südindien ... den Tamilen") and Neiman 1980, 37. Perhaps they have somehow misunderstood the old idea that these

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That there are some Indian products known in the West by their Tamil names was first suggested by Kennedy and again by Rawlinson. While Kennedy mentioned only rice (ὀρύζα), the peacock (ταῶς) and sandalwood (Hebrew *almug/algum*),³⁸⁴ Rawlinson could add cinnamon (κάρπιον), pepper (πέπερι) and several products attested by Western sources only in a later period (when South India certainly traded with the West).³⁸⁵ Now, from what has been said above it is clear that peacocks and sandalwood can be omitted from the list. Κάρπιον is a ἅπαξ λεγόμενον from Ctesias,³⁸⁶ and its identification with cinnamon is not at all certain.

Thus, we are left with only ὀρύζα and πέπερι. But πέπερι is clearly MIA *pipparr*.³⁸⁷ Even ὀρύζα (and ὀρίνδης) is not too near to Tamil *arici*. Actually, another derivation is suggested, from some East Iranian word corresponding to *Pastō vrizē* and more distantly from Sanskrit *vrīhi*.³⁸⁸ According to Mayrhofer a direct borrowing from OIA into Iranian is difficult to explain phonetically, instead he suggests some unknown common origin including ὀρύζα as well.³⁸⁹ This is quite possible, but I would like (against Mayrhofer) to include *arici* as well.

We see that there is no good Dravidian etymology for any Indian product with names attested in the West before the Hellenistic period. As to the products themselves, only pepper is obtained solely from South India, and its small and valuable berries are exceptionally suitable for a transit trade. Probably it was in the ports of Gujarat and Maharashtra, where the merchants spoke Middle Indian and had connections with both South India and the West, that even some southern products could be taken aboard and carried to the West. These were the same merchants we have already met in the *Bāverujātaka*, and later they were to have their share together with their southern colleagues in the new flourishing of the Western trade.³⁹⁰

Eastern Ethiopians are northwestern Dravidians.

³⁸⁴ Kennedy 1898, 268f. On Greek ταῶς/ταῶς see Lévi 1914 (1937, 287).

³⁸⁵ Rawlinson 1926, 13f.

³⁸⁶ Ctesias F 45, 47.

³⁸⁷ Frisk s.v., originally from OIA *pippali*. Too often the Greek word is compared directly with the OIA word (and its *l*). This comes through old references from the 19th century, when Western words were compared only with Sanskrit (or if that failed, with Tamil). It was first pointed out by Franke (1893, 596ff.) that the most important comparison is with MIA.

³⁸⁸ Frisk s.v. ὀρύζα.

³⁸⁹ Mayrhofer s.v. *vrīhi*. MIA (Pāli *vīhi*) does not seem to be involved here.

³⁹⁰ Cf. the somewhat antiquated discussion in Franke 1893, 606ff.

8. *The coming of the Greeks*

A link may have also developed by a Greek presence in the eastern provinces of the Achaemenian empire. There were Greek mercenaries in the Achaemenian army³⁹¹ and probably Greek civil servants too, and we may assume that some of them also served in the east, though the few sources are silent about them. If there were Greek mercenaries serving for a long period in the distant satrapies, there were probably soon colonies of retired mercenaries who did not care to travel the long way back home as well.

Unfortunately, we have no evidence of this. Yet the Greek presence even in the eastern parts of the empire is proved in another way. It seems to have been a fixed part of Achaemenian politics to transfer large numbers of people. Whole towns were moved in this way from one end of the empire to another, and often the move was inflicted as a punishment by the king.³⁹² In several cases this happened to Greeks.

After the Ionian revolt, Darius deported the majority of the Milesian population and settled them in Southern Mesopotamia.³⁹³ In the same way, the Eretrians were moved to Southwestern Iran, where they still lived and spoke Greek in the time of Herodotus.³⁹⁴ In connection with the Libyan campaign the population of Barca was deported to remote Bactria, and they, too, still lived there in Herodotus' time.³⁹⁵

Famous and somewhat problematic is the case of the Branchidae, a priest family serving Didymaeon Apollo near Miletus and a hereditary operators of a famous oracle, until they moved with the help of Darius (hardly Xerxes), apparently on their own initiative, to the northeast of Bactria. Their descendants were still there and had still preserved their Greek language and culture after nearly two centuries, when Alexander found and massacred them as a revenge for the alleged treason of their forefathers. Although this episode is omitted by some historians (for instance by Arrianus) and doomed as unhistoric by some modern scholars,³⁹⁶ its historicity seems quite clear in spite of the stain it gives to Alexander's reputation.³⁹⁷ Yet they were merely one of the Greek settlements in the east,³⁹⁸ and probably there were more than we know of.

On the Indian side of Hindukush our fragmentary evidence does not mention any

³⁹¹ Like Xenophon and his companions.

³⁹² Cf. Hdt 6, 9.

³⁹³ Hdt 6, 20. Herzfeld (1968, 7, note 3), as always with his good local knowledge, is very sure of exact locations, but gives little evidence to substantiate them.

³⁹⁴ Hdt 6, 119 ἐνταῦθα τοὺς Ἐρετρίεας κατοίκισε βασιλεὺς Δαρείος, οἱ καὶ μέχρι ἐμῆο εἶχον τὴν χώραν ταύτην, φυλάσσοντες τὴν ἀρχαίην γλῶσσαν. Philostratus (*V. Ap.* 1, 24) made a similar claim, but perhaps it should not be taken too seriously.

³⁹⁵ Hdt 4, 204.

³⁹⁶ E.g. Tarn 1948, 67 and 1950, 272ff.

³⁹⁷ The problems of the Branchidae and their massacre are discussed by Brown (1978b) and Parke (1985, with further references), briefly also Bosworth 1988, 108f.

³⁹⁸ Beal 1880, 69f. made them the source of all (often supposed) early Greek influence in India.

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Greek settlement in the Achaemenian period. Yet the existence of such a settlement – either a deportation or a mercenary colony – is by no means impossible. It has sometimes been suggested that Nysa, that fabulous town of Dionysus worshippers in Nuristan, was in fact such a settlement.³⁹⁹ If we rely on the historians of Alexander⁴⁰⁰ the town was founded by Dionysus himself, the inhabitants being descendants of his soldiers, part of whom were perhaps Greeks. Certainly they were not Indians.⁴⁰¹ Yet this was only a possibility mentioned but not necessarily subscribed to by Arrianus. In spite of the “democratic” government of the Nysaeans – which is easily explained as a tribal feature⁴⁰² – and their alleged worship of Dionysus – when so many local gods were identified with the Greek ones, why should this be an exception – there is nothing particularly Greek in them,⁴⁰³ and the silence of our authorities in this respect points strongly to the opposite view. A mention of Dionysus is here wholly in place from the view of Alexander's politics, and thus even the reliability of the accounts or at least their details is questionable.⁴⁰⁴ At least Curtius' account of a local cemetery connects the Nysaeans with the present day Nuristanis.⁴⁰⁵

Thus the presence of a Greek population is confirmed to the north of Hindukush and is wholly possible to the south, if not in Nysa. There is also some evidence from India, which perhaps could be connected with such a settlement, although the case is not free of chronological difficulties as the settlements founded by Alexander can also be given to explain the Greek presence. Such is the *yavanānī* in Pāṇini, later explained as *yavanānī lipi*, ‘Greek script’.⁴⁰⁶ A knowledge of the Greek script in these parts, at least in the third century, is confirmed by the discovery of the Greek edicts of the Mauryan emperor Aśoka in Kandahar.⁴⁰⁷ The fact that they were inscribed in Greek points strongly, though

³⁹⁹ So e.g. Narain 1957, 2 and Woodcock 1966, 21f., cf. Stein 1936a, 1652. Lamotte 1958, 109 made Nysa a Greek settlement in the sixth century.

⁴⁰⁰ Arrianus *Anab.* 5, 1–3, *Indica* 1, 5 and 5, 9, Plutarchus *Alexander* 58, Justinus 12, 8, Curtius 8, 10, 35f., Cleitarchus F 17.

⁴⁰¹ Arrianus *Indica* 1, 4f. Νυσαῖοι δὲ οὐκ Ἰνδικὸν γένος ἐστίν, ἀλλὰ τῶν ἅμα Διονύσῳ ἐλθόντων ἐς τὴν γῆν τὴν Ἰνδῶν, τυχὸν μὲν [καὶ] Ἑλλήνων, ὅσοι ἀπόμαχοι αὐτῶν ἐγένοντο ἐν τοῖς πολέμοις οὕστινας πρὸς Ἰνδοὺς Διονύσος ἐπολέμησε, τυχὸν δὲ καὶ τῶν ἐπιχωρίων τοὺς ἐθέλοντας τοῖς Ἑλλήσιν συνῴκισε.

⁴⁰² Cf. the chain of tribal societies often somewhat erroneously called “democracies” surrounding the Aryans in India.

⁴⁰³ See e.g. Bosworth 1988, 121f. It is hardly likely that they really presented themselves as descendants of the soldiers of the Greek god Dionysus – as is stated by Arrianus, *Anab.* 5, 1, 5 and Curtius 8, 10 – and for their leader Ἴακουφίς an Iranian name has been suggested (Breloer 1940, 281, note 2).

⁴⁰⁴ The Nysa problem is discussed from various viewpoints and with further references by McCrindle 1896, 338ff., Breloer 1935, 61ff., Stein 1936a, Tarn 1950, 45f., Dahlquist 1962, 271ff., Schachermayr 1973, 419f., Goukowsky 1981, 25f. and Hinüber 1985, 1082f. (also Wirth in the same, 918f.). Wise scepticism has been recently expressed by Dihle (1987, 48, note 7). For Dionysus see chapter VIII.5.

⁴⁰⁵ Goukowsky 1981, 25f. and 154 (note 29, with further references).

⁴⁰⁶ P IV, 1, 49 and Kātyāyana's *Vārttika* on the same, discussed e.g. in La Vallée Poussin 1930, 38ff. For their chronology, see chapter VI.1. It might even be that Kātyāyana was referring to the Greek inscriptions of Aśoka.

⁴⁰⁷ The first one was found in 1957 and published in 1958, see Pugliese Carratelli et al. 1964, the second

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not necessarily, to a sedentary Greek speaking population there. On the other hand, the men who translated the edicts from Middle Indian into Greek did not come from some isolated colony. The translations show good knowledge of Greek philosophical terminology.⁴⁰⁸

Here we may also pay attention to the Indian name for Greeks. Although Sanskrit *yavana* (first in Pāṇini) / MIA *yona(ka)* (first in Aśoka)⁴⁰⁹ is not indisputably attested before Alexander, it was most likely borrowed from Old Persian *yauna* in the Achaemenian period. Ultimately, it is derived from Greek Ἴάονες/Ἴωνες. There has been some discussion on the derivation of different forms,⁴¹⁰ but Tóttösy seems to have settled the case. According to him, first the MIA *yona* was borrowed from OP *yauna*, and *yavana* then came through a re-sanskritization of *yona*, while *yonaka* simply acquired the common suffix *-ka*.⁴¹¹ A purely Indian derivation⁴¹² coming by accident so near to a real Western word is, to say the least, unlikely.

Thus we know that there were Greeks living in Bactria and perhaps also in India. It may be partly due to them that Indians gained some knowledge of Greeks, but this remains an unproved assumption. Soon Alexander's campaign and colonies and especially the Bactrian Greeks were to establish the place of the Yavanas among the northwestern peoples in Indian geography.⁴¹³ But the early (before Alexander) settlements seem to have been quite isolated from Greece, and they hardly had any importance in the development of Western knowledge.

A more immediate Greek presence came with Alexander's eastern campaigns and the colonies he founded there, even if its extent is contested.⁴¹⁴ This was also the first time that Greek literature acquired extensive first hand accounts of Northwestern India. The campaign itself and the problems connected with its details do not concern us much here⁴¹⁵ as we are studying especially the contacts with and accounts of India before Alexander. But when Alexander invaded India, the very idea of the country he and his companions had was gained from the earlier accounts supplemented perhaps by some intelligence obtained in Persia and especially in Bactria.

What was Alexander's purpose with the easternmost parts of his conquests? Was he completing his empire by adding to it the eastern end of the οἰκουμένη,⁴¹⁶ or was he pursuing a dream, an idea of what the empire of Darius and Xerxes had been long

found in 1964, published in Schlumberger 1964 and again, in Benveniste 1964. A bibliography of the rather numerous literature on these inscriptions is given e.g. in Davary 1977 and Holt 1984 and 1987. Kandahar is located in the territory ceded by Seleucus to Candragupta, cf. Eggermont 1966b, 56ff.

⁴⁰⁸ See the notes by L. Robert in Schlumberger 1964, 134ff. and Harmatta 1966, 78ff.

⁴⁰⁹ Cf. Tóttösy 1955, 309, note 43.

⁴¹⁰ See e.g. Tarn 1951, 416ff.

⁴¹¹ Tóttösy 1955, passim, again 1977, briefly also Allan 1951, 863 and (with the unfounded preference for the priority of *yavana* to *yona*) Narain 1957, 155ff.

⁴¹² Suggested e.g. by Chattopadhyaya (1974, 38f.).

⁴¹³ See e.g. Law 1973, 153ff.

⁴¹⁴ For a negative view see e.g. Narain 1987, 125f.

⁴¹⁵ See e.g. Tarn 1948 and 1950, Wheeler 1968, Schachermayr 1973 and especially Bosworth 1988.

⁴¹⁶ So e.g. Schachermayr 1973, 396ff. and 438f.

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ago?⁴¹⁷ It is hard to say. The idea of conquering the whole of the Achaemenian empire, not only what remained of it under Codomannus,⁴¹⁸ but what it used to be in its heyday, is reasonable enough. In a way it was Darius and Xerxes Alexander was fighting against. This was expressly said in his propaganda, partly in order to confirm the Greek collaboration, but perhaps it also reflected the mood of the ruler who in spite of all his genius was also impulsive and idealistic. But our sources on the Indian campaign – and there are several quite detailed accounts left to us – never mention this motive. On the contrary, at least to these authors, India is always the exotic eastern country which was never conquered before except by Dionysus and Heracles,⁴¹⁹ whose example Alexander, himself a god, followed and even surpassed.⁴²⁰ In all their descriptions of Northwest India there are no traces of a previous Achaemenian domination.

But in a way both motives could have been the case. Foucher argued that Alexander had to turn back from Hyphasis because the ancient boundary lay there.⁴²¹ But in my opinion it is quite possible that he just did not know where it lay. As far as we can judge from Herodotus and the fragments of Ctesias, Greek literature offered little help. Perhaps the Persians or their archives were of more use?

But in spite of a lively discussion and speculation of more than 150 years, we still know very little about the Persian archives. There were archives – so much is clear – and if they were not destroyed in Persepolis, they probably came into the hands of Alexander. But what and how much did they contain? And how much were they used? If the Indian dominion had ceased to be, how much was still preserved from the times of Darius and Xerxes? There may have been a fire or some other accident. An old and little used part of the archives could even have been deliberately destroyed. The oldest parts – if they were preserved – were perhaps written in Elamite,⁴²² and therefore perhaps already unintelligible, especially to the Greeks and Macedonians. Be this as it may, there seems to be no evidence that the archives were used by them in order to cull out some information about India. This is not merely an *argumentum ex silentio*. There seems to have been a constant need to spy and gather information about India. There is no indication at all about a previous Achaemenian dominion in India. If the archives were intact, and used, they probably contained a report of the Indus expedition in which Scylax had participated. But when Nearchus received more or less the same commission, he as well as Alexander, his crew (including Onesicritus) and everybody seemed to think of the naval expedition as

⁴¹⁷ This was the idea of Foucher (1938 and 1942-1947) and Tarn (1948, 86f.).

⁴¹⁸ Unfortunately, we have no clear idea about how far it extended in the east.

⁴¹⁹ Some legendary Near Eastern monarchs are sometimes added.

⁴²⁰ Cf. Noiville 1929, 248f. and Schachermayr 1973, 407ff.

⁴²¹ Foucher 1938, 350 and 1947, 191, for a different view see e.g. Narain 1965, 155f. and Schachermayr 1973, 434ff. A third view is represented by those Indian historians (e.g. Chattopadhyaya 1974, 21f.) who claim that Alexander was actually defeated by Porus, and had to retreat without reaching Hyphasis at all. According to these historians reaching Hyphasis, like Alexander's victory over Porus, was just a lie put about by Greek historians ever ready to magnify Alexander. But unfortunately for this view, there were also historians who were eager enough to do the opposite. On the battle at the Hydaspes see e.g. Bosworth 1988, 126ff., on Porus *ibid.* 239f.

⁴²² See Fleming 1982, 109.

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something that was unheard of before.⁴²³

Thus, it seems likely that the Macedonians had only a vague idea of India, perhaps just what was told by Herodotus and Ctesias.⁴²⁴ They probably knew it had once belonged to the Achaemenids, but they knew no details. If they really were reconquering old boundaries, they probably had to accept hypothetical boundary lines.

If we rely on our sources, they do not speak of an attempt to reach the Achaemenian eastern boundary. And according to the sources Alexander himself, at least, had no intention to stop at Hyphasis. He was not merely reconquering the Achaemenian empire he wanted to emulate and surpass the Achaemenids.⁴²⁵ Probably his ultimate goal was indeed the Eastern Ocean or at least something comparable, an easily defensible natural boundary. This goes well with the increasing attempts to deify Alexander, which occurred during the Indian campaign.⁴²⁶ It was no longer Cyrus, Darius or some legendary Near Eastern monarch⁴²⁷ that Alexander was emulating, it was the gods themselves. It was at this point that the Indian campaigns of Heracles and Dionysus⁴²⁸ were invented. Propaganda unearthed supposed evidence of their campaigns (like Nysa, Aornus and the cave of Prometheus) and showed how Alexander surpassed them. Although the so-called evidence for a cult of Alexander in an eastern colony (Kandahar/Alexandria in Arachosia) is hardly convincing,⁴²⁹ such a cult may very well have been there. Its foundation had already been laid during the Indian campaign.

But this goes beyond the scope of the present study. The same can be said of Seleucus' eastern campaign, which did not give him any Indian dominions, but did provide him with 500 elephants instead,⁴³⁰ of diplomatic contacts between Mauryan India and Hellenistic monarchies,⁴³¹ of the third campaign executed by Antiochus III⁴³² and of course of the Indian conquests of the Hellenistic Bactria.⁴³³

⁴²³ I cannot agree here with Schachermayr (1973, 443ff.), who suggests that the earlier expedition was known but deliberately (and unanimously!) hushed up in order to magnify Alexander.

⁴²⁴ Herodotus, however, knew of the expedition sent by Darius. The accounts of some fabulous peoples, and especially of the legendary expedition of Semiramis in histories on Alexander, suggest that Ctesias was known. See Schwartz 1896, 90ff. and Brown 1955, 27.

⁴²⁵ See e.g. Bosworth 1988, 143, 146 and 153.

⁴²⁶ On the deification of Alexander see Goukowsky 1978 and 1981.

⁴²⁷ Like Sesostris and Semiramis, see Borzsák 1976.

⁴²⁸ Dionysus had already reached Bactria earlier (Euripides, *Bacchae* 13 – 15).

⁴²⁹ Oikonomides (1985, 69) reads 'Α[λεξάνδρου] στήσα τόδε εἰς τέμε[ν]ος, but a solitary Alpha is rather weak ground for deciphering the name of Alexander in the inscription. It is, however, accepted by Holt (1984, 8).

⁴³⁰ See e.g. Eggermont 1966b, 56ff., Skurzak 1964, 225ff. and Schwarz 1970, 281ff. On the problem of ἐπιγμῖα see also Tarn 1951, 173f. and Jong 1973, 123.

⁴³¹ See e.g. Schwarz 1968 and 1970 and Eggermont 1942 and 1984b.

⁴³² See e.g. Tarn 1951, 101, Eggermont 1966b, 58ff. and Schwarz 1970, 314ff.

⁴³³ See e.g. Tarn 1951 and Narain 1957. Holt (1984 and 1987) offers an up-to-date bibliography.

9. *The Impact of Achaemenian Rule in India*

The question of a possible Achaemenian influence in India is chronologically rather complicated. In the Northwest – where direct dominion is an incontestable fact – it must have been considerable, beginning already in the late 6th century, but most of our evidence is much later. And yet for instance the use of Aramaic – the letters as well as the language itself – attested in the Aramaic versions of Aśoka Edicts found in Taxila and Afghanistan⁴³⁴ probably goes back to the Achaemenian period.⁴³⁵ The same can be said of the Kharoṣṭhī script, clearly based on Aramaic script and also first attested in Aśokan Edicts. At least there was no reason for Alexander and his men (not to speak of the Mauryas) to introduce Aramaic as an official language.

In chapter II.4. it was shown that both Brāhmī script and punch-marked coinage were probably introduced in this period, the original inspiration coming from the Achaemenians.⁴³⁶ It has also been pointed out that there are some Old Persian loans in Old (and Middle) Indo-Aryan. And yet, within the Achaemenian dominion itself, there are only a few rather insignificant archaeological finds left from the eastern dominions of the Achaemenids.⁴³⁷

If the harvest is meagre when one is searching for Greeks in Indian literature,⁴³⁸ no

⁴³⁴ Five of them come from Afghanistan (Davary 1977, 11). In addition, even the Aramaic inscription found in 1915 at Taxila (cf. Barnett 1915) seems to come not from the young Aśoka ruling as a viceroy at Taxila (this old view is summarized in Bongard-Levin 1956), but is another version of Aśoka's Rock Edict IV known also from the Kandahar bilingual (Graeco-Aramaic) inscription. This was shown in 1969 by Humbach (see revised version in Humbach 1978 and Dar 1984, 203ff.). See also Altheim & Stiel 1970, 343, where Humbach's theory was briefly dismissed, though they do show how the title translated earlier as 'viceroy' is used for king Aśoka in the Kandahar edict. There is, further, an Aramaic ostrakon found at Ai Khanum (Rapin 1983, 347, number 28).

⁴³⁵ There is, it is true, the inscription I.110 at the Museum of Lahore read by Rapp (1972, 25ff.) in West Semitic (Canaanite) and dated to the 8th or 7th century B.C. As his reading contains the place-name Khotan, it should consequently have been engraved somewhere in the east. Being completely ignorant in Semitic epigraphy I cannot say much, but an inscription which presents no problems in deciphering does sound somewhat unlikely. Rapp himself (ibid. 29) puts two questions: "Where was the stone found?" and "How did it come to the Lahore Central Museum". I suspect that these should be answered first, and the stone itself examined (Rapp had only a photograph), before any conclusions can be drawn.

⁴³⁶ Wheeler's idea that iron technology came to India from the Achaemenians only in c. 500 B.C. is no longer valid, see Allchin & Allchin 1982, 309ff.

⁴³⁷ According to Brentjes (1981a, 140) the finds in Afghanistan include many coins (for them see Schlumberger 1953) and some other small finds, but no architectural remains. I cannot verify what the Achaemenian remains of Old Kandahar referred to by Holt (1984, 6) actually contain. In Pakistan there are architectural remains of the period, for instance at Taxila, but as far as I know there is nothing to connect them specifically with the Achaemenian rule. According to Tucci (1977, 12f.) there are no indisputably Achaemenian remains in Swat. Among the rock-carvings found along the Karakorum Highway there are some pre-Achaemenian and Achaemenian motifs (Jettmar 1983, 1984, 80, 1985, 757 and Jettmar & Thewalt 1987, 13ff.).

⁴³⁸ Cf. Lévi 1890a.

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mention is made at all of the Achaemenids and their empire. Przyluski's idea that the *vairājya* mentioned in the *Aitareyabrāhmaṇa* in connection with the Uttarakurus and the Uttaramadras should perhaps refer to imperial Persia,⁴³⁹ is a mere guess. Although *virāt* really means 'great king' and not 'kingless',⁴⁴⁰ the early Indian idea of a universal kingdom was modest enough and did not require anything like the Achaemenian or Mauryan (the first of its kind in India) empire. Even without its serious chronological problems Przyluski's hypothesis was a mere guess with no evidence. The direction too, seems to be wrong, although Przyluski had a contrary opinion. While the Uttarakurus did not yet live in the legendary extreme north in the Brāhmaṇa period – and the Uttaramadras were never located there – their location north of Kuru probably means the western Himalayas or beyond (to the north),⁴⁴¹ where there are no traces at all of a Persian dominion.

Also fascinating, but hardly convincing, is a theory put forward in the early forties by H.C. Seth⁴⁴² connecting the Mahābhāratan war with the supposed Indian campaign of Cyrus (identified with Sanskrit Kuru).⁴⁴³

There is in Buddhist texts certain information which provides problems for many scholars. King Pukkusāti of Taxila, an independent ruler having diplomatic relations with Magadha is unlikely to have been an Achaemenian vassal. He is usually placed within the period before the Achaemenian conquest, in the middle of the 6th century B.C.⁴⁴⁴ With the chronology accepted in these studies this is just possible for a contemporary of Bimbisāra of Magadha and the Buddha. But now the reduced chronology for Buddhism and the Magadhan expansion puts the whole question in a new light.⁴⁴⁵ As there are strong grounds for dating the Buddha's death in the fourth century, Pukkusāti as his contemporary cannot have lived before the Achaemenian conquest of Northwest India.

There are two possible explanations. Pukkusāti may belong to a period when the Achaemenids had already lost their hold over Indian provinces. But as was mentioned above, it is not certain that the Achaemenian dominion really extended east of the Indus.

⁴³⁹ *AB* 8, 14 discussed in Przyluski 1927, 172f.

⁴⁴⁰ This old translation of Haug is still sometimes mentioned, but *RV* 1, 188, 5 compares *virāt* with *saṁrāt* and the commentary ("Sāyana") explains the *vairājya* of *AB* as *viśeṣena rājatvam* (Przyluski 1927, 173). See also Ray 1922, 257 and Spellman 1964, 66f. Spellman translates *virāt* as sovereign ruler and refers to *SB* 8, 5, 1, 5 *yo vāva sarvāsu dikṣu virājati sa eva virājati*. In *AV* *virāt* is used of Indra and Agni. *Vairājya* is mentioned also in *KA* 8, 2, 5. Kangle translates it as 'being without the king' and explains it as a state where "some enemy after conquering a state, has driven out its ruler and started ruling over it from his own state". The explanation is made in order to have the translation tally with the text's own explanation (*ibid.* 8: *vairājyam tu jīvataḥ parasyācchidya naitan mama iti manyamānaḥ karsayati, apavāhayati, paṇyam vā karoti, viraktam vā parityajyāpagacchati*), but another translation would have tallied much more easily.

⁴⁴¹ Thus e.g. *AB* 8, 14, but the same text in 8, 23 already seems to contain the mythical idea of the Uttarakurus in the extreme north. See Macdonell & Keith 1912 s.v. Yet with such slight evidence one cannot be very sure.

⁴⁴² Cited in Prakash 1969, 140.

⁴⁴³ The names themselves can have a related origin, though this does not indicate any historical connection. See Mayrhofer (also Nachträge with further references) and Mayrhofer (New) s.v. *kuru*.

⁴⁴⁴ Thus e.g. Lamotte 1958, 110f., Prakash 1969, 135f. and Dani 1986, 41.

⁴⁴⁵ The chronology of the Buddha is discussed in chapter VI.5.

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Taxila may have been independent. Pukkusāti could have been an ancestor of that Āmbhi (or whatever his name was),⁴⁴⁶ whom Alexander met ruling as a sovereign king in Taxila. He (or at least his kingdom of Taxila) might even have had the role of an intermediary, introducing an Iranian influence into the Gangetic basin. There is no evidence at all of any direct contact between the Gangetic basin and the Achaemenian empire, either in Indian or in Western sources.⁴⁴⁷ But Pukkusāti, ruling a kingdom either formerly belonging directly to the Achaemenids or situated just beyond their eastern boundary, was very much involved in eastern politics, as his diplomatic and military enterprises bear out.⁴⁴⁸

The role of Taxila must have been important.⁴⁴⁹ In Indian sources the town is famous as a centre of learning, both orthodox and unorthodox. Vedas⁴⁵⁰ were taught there and eighteen śilpas too.⁴⁵¹ From the schools of Taxila came masters of archery and spells;⁴⁵² skilful surgeons and physicians like Jīvaka, who cured both king Bimbisāra and the Buddha;⁴⁵³ and probably even grammarians (Pāṇini himself was born in the region). Later, it became a famous seat of Buddhist learning. We must also remember the Gymnosophists met by Alexander's men outside Taxila. It was probably no coincidence that they were living in the outskirts of Taxila and no other town.

The cultural importance of Taxila may well go back to remotest antiquity, but here we should again be rather cautious. From recent archaeological evidence we know that Taxila (the Hathial site) was already an urban centre in the pre-Achaemenian period (contrary to the opinion of many older scholars, like Wheeler), even in the late second millennium.⁴⁵⁴ But an urban centre was not necessarily a centre of learning, and there are no written sources on Taxila which could safely be dated to the pre-Achaemenian period. In the archaeological material there is a clear difference in the finds of the early Hathial and the Achaemenian Bhir mound.⁴⁵⁵ But even in the Achaemenian period the western element in Taxila is remarkably small.

⁴⁴⁶ He is called *Omphis* (Curtius 8, 12, 4 and 14) and Μῶφις (Diodorus 17, 86, 4), variously identified with OIA *āmbhi* (Lévi 1890b, 234f.), *amātya* (Breloer 1941b) and the toponym *U(n)da-bhāṇḍa* (Pliny's *Amanda*, modern Ohind/Uṇḍ) in Gandhāra (Eggermont 1970, 104).

⁴⁴⁷ That the Ganges was mentioned by Ctesias (as suggested e.g. by Kiessling 1916) is possible but not at all certain. See e.g. Lindegger 1982, 83.

⁴⁴⁸ In various sources he is mentioned as having sent an embassy to Bimbisāra and declared war on king Pradyota of Avantī. (Prakash 1969, 135 with references). Still it must be noted that an active interest in politics of the independent states of India does not necessarily mean that Pukkusāti cannot have been an Achaemenian vassal if we only think of the active and often independent role the western satraps had in Greek politics.

⁴⁴⁹ There is a vast literature on Taxila. See especially Marshall 1951, Il'in 1958, Dar 1984 and Dani 1986.

⁴⁵⁰ But no known school of Veda is as northwestern as Taxila, see Witzel 1987a.

⁴⁵¹ See Law 1916, 17f. with many references to Jātakas.

⁴⁵² Law 1916. 18ff.

⁴⁵³ For him see Zysk 1982.

⁴⁵⁴ Allchin & Allchin 1982, 314f.

⁴⁵⁵ In the words of Allchin & Allchin *l. c.*: "The culture at this time [the Hathial period] has a markedly local Gandharan flavour; while that of the subsequent Bhir mound period appears to indicate the arrival of a much more urbane and widely diffused Gangetic character."

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Sooner or later, the Achaemenian impact was felt even in the Gangetic basin, though it is absent from our literary sources. Again, the chronology is rather problematic. There is little evidence which unambiguously goes back to the pre-Mauryan period. In its most lucid form this impact is perhaps seen in monumental art and architecture. Here the Achaemenian influence is clear enough,⁴⁵⁶ although the normal pattern is an Iranian formal element given a new, Indian interpretation.⁴⁵⁷ But it is still rather late. A rigid interpretation of the archaeological evidence has even led some scholars to claim that a monumental art using durable materials like stone was introduced only by Aśoka in the middle of the third century.⁴⁵⁸ This is probably an overstatement. As the early excavations at Pāṭaliputra were carried out too early to apply any stratigraphical methods, we do not know for certain to which period the audience hall and the wooden structures really belong. Auboyer speaks confidently of Aśoka's palace⁴⁵⁹ – and in fact these remains *might* be even of a still later date – yet a comparison with Megasthenes' description of this imperial city in the times of Aśoka's grandfather is remarkably compatible with the actual finds.⁴⁶⁰

Our knowledge of Mauryan art is in many ways imperfect. In addition to the remains of Pāṭaliputra and the Barābar caves, there are only the so-called Aśokan pillars and some terracottas, which do not interest us here. The pillars have often been thought to be erected by Aśoka himself,⁴⁶¹ but it seems that the Pillar Edicts are just an additional feature of the pillars which already stand there. In addition to having an Iranian influence in their form, these pillars have a deep Indian meaning as the *axis mundi*, the cosmic pillar (like Meru) rising from primordial waters.⁴⁶² In a recent study Irwin has pointed out that at least some of the pillars are undoubtedly pre-Aśokan.⁴⁶³

But this does not lead us much further. Although the idea that monumental art began only with Aśoka can be dismissed, we still have no clear evidence of its going beyond Candragupta. Thus Wheeler's idea of the Iranian impulse coming to the flourishing and rapidly growing capital of the new Indian empire, with Persian artisans fleeing from a Persepolis that had been destroyed by Alexander, still seems acceptable.⁴⁶⁴ Another question concerns the purely Indian conception imbued in this art, something which seems to imply Indian artisans; thus Wheeler's idea of Persians working in India must be modified. And anyway, it must not be considered to be any more than a hypothesis.

⁴⁵⁶ See the exhaustive study by Combaz (1937ab).

⁴⁵⁷ See e.g. Wheeler 1974, 254 and 256f. and Mariottini Spagnoli 1970.

⁴⁵⁸ Auboyer 1974, 264, but see Nylander 1988, 1031f.

⁴⁵⁹ Auboyer 1974, 264.

⁴⁶⁰ The comparison was made by Wheeler (1968, 131ff. and 1974, 252f.). According to Wheeler, the wooden fortifications were probably of Indian origin, and this tallies well with the wall-breaking elephants of Ctesias (F 45, 7 and 45b, cf. Karttunen 1981). In *Mbh* 2, 54, 10 elephants are called *purabettārah*. On Pāṭaliputra see also Nylander 1988, 1032ff.

⁴⁶¹ See e.g. Wheeler 1968, 138ff. and Auboyer 1974, 265, although she also mentions the possibility of an older origin (especially in note 4).

⁴⁶² This pillar symbolism is discussed by Mariottini Spagnoli (1970) and Irwin (1987).

⁴⁶³ Irwin 1987 with references to his other studies on these pillars.

⁴⁶⁴ Wheeler 1968, 127ff. and 1974, 249ff. But see the important criticism by Nylander (1988).

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Other ideas mentioned as part of the Achaemenian impact⁴⁶⁵ such as the Royal Road, the idea of inscribing edicts in stone and the very idea of an empire⁴⁶⁶ belong only to the Mauryan period. The question if there was any direct connection between Persia and the Gangetic basin remains open. In the light of our deficient knowledge of pre-Mauryan archaeology,⁴⁶⁷ we can neither accept it nor deny it. At least in the early Mauryan period the impact was felt⁴⁶⁸ and became a stimulus so strong and fruitful that its impact lasted many centuries.

⁴⁶⁵ See the summary in Ojha 1968, 59ff.

⁴⁶⁶ Another possibility here is an inspiration from Alexander (see Schwarz 1968, 225 and 1970, 273). But a story told centuries later of a contact between two great rulers (such as the meeting of Candragupta and Alexander by Plutarchus, *Alexander* 62) is likely to be apocryphal. Does an ambitious and genial ruler really need outside inspiration in order to build an empire? *Virabhojyā vasundharā*, the words ascribed in a late source (Hemacandra, quoted in Schwarz 1978, 1122) to the boy Candragupta are undoubtedly apocryphal, but illustrative. Tarn (1950, 281 note 5) calls the Plutarchus passage "the worst chapter he ever wrote". Scharfe's (1971, 215ff.) hypothesis is also probably too far-fetched. On the grounds of an equation of Aśoka's *devanāmpriya* and Hellenistic φίλος τῶν βασιλέων he makes Mauryas Macedonian (Seleucid) vassals. A similar idea is also found in Daffinà 1977, 21f. and 27f.

⁴⁶⁷ See the summary in Erdosy 1985, 84ff.

⁴⁶⁸ See e.g. Wheeler 1974, 259ff., Auboyer 1974, 266ff. and especially Combaz 1937ab, passim. Scialpi (1984, 55ff.) lists several other points of possible Achaemenian influence which are often very open to criticism (not mentioned by Scialpi). His own attempt to find some link between the ideas of the ethical content of kingship among the Achaemenids (Darius and Xerxes) and Aśoka is interesting, but not very convincing.