

## DARIUS' SUEZ CANAL AND PERSIAN IMPERIALISM

Chr. Tuplin — Liverpool

Darius' Suez canal is frequently mentioned in the modern literature but not often accorded any extensive discussion. The most recent influential exception is Hinz 1975b, which succeeded in establishing a completion date in the early 490s as the current orthodoxy, though had less to say about the motives for the enterprise.<sup>1</sup> Both before and since Hinz's article the latter question has most often been tackled in terms of trade,<sup>2</sup> though others have spoken more noncommittally about improvement of communications systems, drawing an analogy with the Royal Road,<sup>3</sup> or about strategic motives.<sup>4</sup> In the paper which follows I shall first examine the sources directly concerned with the canal to see how they relate to one another and how much they actually allow us to assert with any certainty (it will transpire that historians may have been over-optimistic about this). I shall then look at the contexts within which the canal might be seen in order to pursue the question of the motives for its construction.

### SOURCES ON THE CANAL

#### *The Greco-Roman Sources*

The sources are Herodotus II 158; IV 40; 42; Aristotle *Met.* 852b; Strabo C38, C780, C804; Agatharchides 86 F19 = Diodorus I 33, 8-12; Pliny *NH* VI 165. The major division is between Herodotus and all the others. The most striking feature is that Herodotus says that Darius completed the canal, while the other sources deny it. But there are other differences, as we shall see (to the extent that Herodotus and his successors are scarcely in contact at all, *pace* Lloyd 1977), just as there are divergences among the later sources.

I am indebted for assistance and comments to Amélie Kuhrt and to my Liverpool colleagues Ken Kitchen, Alan Millard and Peter Shore

<sup>1</sup> Edakov 1980, by contrast, has had little impact.

<sup>2</sup> Bresciani 1968, 339; 1984, 361; Briant 1981, 22; Burn 1962, 117; Cawkwell 1972, 33; Dandamaev & Lukonin 1980, 217; Edakov 1980, 118; Gallotta 1980:153, 181; Gyles 1959, 93; Kraeling 1953, 10; Schmitt 1983, 422.

<sup>3</sup> Law 1978, 100; Lloyd 1988b, 152; Ray 1988, 264; Schaefer 1960, 64.

<sup>4</sup> Cook 1983, 65; 1985, 22. Dandamaev 1985, 105f is almost entirely non-committal, making only the point that, whereas the modern Suez canal was intended to improve the links between Europe and India, the Persian one was aiming chiefly to improve those between the Nile valley and the Red Sea.

1. *The Completion of Darius' Canal.* Since the canal stelae prove Herodotus correct here, and since Herodotus writes as though the canal was still operational in his day (though oddly fails to mention it in II 11, where it surely had some pertinence to what would happen if the Nile started to flow into the Red Sea), the later view, already espoused by Aristotle, shows that within a century of Herodotus Darius' waterway had silted up. A more precise date cannot be proved. Even if Damastes 5 F8 proved the canal's use for a trip from the Mediterranean to Susa in the fifth century (so Mazzarino 1959, Oertel 1964), the date would only be slightly later than Herodotus' report; and the interpretation is disputable (cf. Breebaart 1967). But it is natural to guess that the canal's abandonment was due to the liberation of Egypt at the end of the fifth century. It is worth noting the implication that the independent rulers of fourth century Egypt perceived no advantage in its maintenance. It is also interesting in this context that the town at Tell Maskhutah on the canal-line may have been damaged during the revolt of 487-485 (Holladay 1982, 25). But in view of Herodotus' evidence it does not follow that the canal was abandoned in the aftermath of those troubles. Clédat (1920, 103f.; 1923, 85, 93; 1924, 67f.) had a quite different approach, claiming that, while both Necho and Darius succeeded in cutting a Nile-Red Sea canal via the Wadi Tumilat, they failed in a parallel attempt to cut one from the region of Daphnae via Lake Ballah, and it is to this failure that the relevant sources refer. This strikes me as a wildly uneconomical hypothesis and not worth further discussion.

2. *The Ptolemaic Canal.* All later sources except Aristotle connect Darius' enterprise with that of Ptolemy II, but whereas Diodorus and Strabo claim that Ptolemy completed a canal, Pliny denies even this, and reports that the king abandoned the project 34.5 miles north of Suez. Moreover, *pace* Oertel 1964, 21, 48, he does not seem to write as though the canal was operative in his own time. The only relevant oriental source, the so-called Pithom Stele (Neville 1902; Roeder 1959, 108-128), presents many problems of interpretation — notably the fact that the passage normally taken to narrate the cutting of the Red Sea canal (1,16) describes the venture as undertaken to 'delight the gods' of the 14th nome, which lay to the *north-east* and included Sile and Pelusium — but it cannot be denied that line 24 appears to mention a "canal of the mountain of the east" running north from the Suez Gulf and presumably linking with the waterway from the Nile to Lake Scorpion described in line 16. Pliny's treatment of the subject therefore implies that some portion of the canal (*prima facie* a part between the Bitter Lakes and the Nile) had silted up by the first century A.D. Plutarch *Antony* 69 suggests this was already so in the year of Actium.

3. *The reason for Darius' alleged failure to complete the canal* is variously

stated as fear that salt water would pollute the Nile (Aristotle) and fear of Egypt being inundated (Diodorus, Strabo). Pliny offers both explanations (as alternatives) in relation to Ptolemy's failure. These are not actually inconsistent fears — the stated justification (that the Red Sea is higher than Egypt) is the same in both cases and one *could* fear that one (inundation) or at least the other (salination) would happen, and this is in fact how Pliny puts it — but they are nonetheless distinct, with the more dramatic scenario first appearing in the tradition later than the other one — not an unnatural progression.

4. *Darius' predecessors.* Necho is named in Diodorus, Sesostris in Aristotle and Pliny. Both are noted by Strabo C804, but C38 (which only mentions Sesostris) and the phrasing of C804 show that the Sesostris version is the one that Strabo favours. It appears that he was wrong, since there is no independent Egyptian evidence for a Nile-Suez canal prior to Necho (cf. Posener 1938; Shea 1977). As for Necho, preliminary archaeological investigation at Tell Maskhutah (Holladay 1982) does appear to be consistent with the idea that the eastern part of the Wadi Tumilat was (re)connected with the Nile in the time of Necho. This favours the assumption of Herodotus and others that Necho's canal was essentially along the same route as Darius' and tells against the view of Aly Shafei (apparently reflected in the map in Shea 1977 and entertained, but not ultimately endorsed, by Bietak 1975, 138) that what Necho actually attempted was a quite different route starting in the north and passing (like the modern Suez canal) through Lake Ballah.

Only Strabo provides reasons for these kings' failure, viz. fear of the higher level of the Red Sea (Sesostris; Strabo C38) and death (Necho: Strabo C804). The economical hypothesis is probably that Diodorus and Strabo reflect two slightly different hellenistic traditions (with Strabo glancing at the one followed by Diodorus in his aside that some people make Necho Darius' only precursor, but preferring the alternative one which went back to Aristotle). This involves the assumption that Diodorus' source attributed Necho's non-completion to his inopportune death. This does not strike me as unreasonable. There is no proof that any hellenistic author spoke of the oracle which dissuades Necho in Herodotus; and since in most other respects later sources appear to be independent of Herodotus there is no particular reason to postulate a shared view at this point either.

5. *The course of Darius' canal.* Herodotus makes it start a little south of Bubastis and notes that it went past Patoumos (Pithom)<sup>5</sup> and through a gorge (*diasphagai*) just before turning south (? the narrow eastern end of Wadi Tumilat near T. Maskhutah). These are circumstantial details which

<sup>5</sup> Variousy identified as Tell Maskhutah (Lloyd 1988b, 154; Redford 1982, 1056) or Tell er-Retaba (Bietak 1984, 621; Gardiner 1924, 95f; Kitchen n.d.).

have no counterpart in other texts, where, by contrast, we do have allusions to the Bitter Lakes (Strabo, Pliny), unknown to Herodotus. A starting point is given by three other sources, all of which are, properly speaking, describing Ptolemy's canal, though the two who also mention Darius appear to assume that the same route was in question. Unfortunately the starting points on offer — viz. Pelusiac mouth (Diodorus), Phakoussa (Strabo) and 'the river north of Heliopolis' (Pithom stela) — are in conflict with one another and (in some measure) with Herodotus.

(i) Diodorus' assertion, taken literally, is quite out of line with the other evidence. We must either reject it (adding the speculation that there is a confusion with the 'first canal south of Pelusium' mentioned in Strabo C804, which may itself have some relation with the eastern canal recently discovered between Pelusium and Sile: Bietak 1975, 131-139; Shea 1977) or assume that his source actually spoke vaguely of the Pelusiac *branch* of the Nile. The passage about the Red Sea canal is a parenthesis within a long section devoted to the Nile and its peculiarities (I 32-41), so we can hardly be certain that 'from the Pelusiac mouth' should be taken literally, especially since Diodorus' own description (I 33,7) of the configuration of the Nile mouths seems to leave no room for the canal. Even then it is still an open question whether the source had in mind a location which could be *anywhere* on the Pelusiac branch (including e.g. the region of Heliopolis and Bubastis) or one which was still relatively close to the mouth. The latter assumption would, of course, suit the view of Aly Shafei on Necho mentioned above (§ 4), but it certainly will not do for Darius.

(ii) The old view put Phakoussa at Saft el-Hineh, between Bubastis and the Wadi Tumilat entrance. If this were correct, there would be no real problem of consistency with Herodotus. But current orthodoxy locates Phakoussa at Faqus and a moment's examination of the contours on Bietak 1975, plan 4 reveals that any canal starting there but passing through the Wadi Tumilat would have to run over 20 km. SW to the latitude of Bubastis before it could enter the Wadi — in other words this is a wholly inappropriate starting point for the canals of Ptolemy and Darius, and must be excluded from the argument. The fact that Hecataeus 1 F303 mentioned Phakoussa has no demonstrable connection with the matter.

(iii) The Pithom stela's "north of Heliopolis" (Naville 1902, 72; Roeder 1959, 122) is not formally at odds with Herodotus. Bubastis and Heliopolis are the largest places in the relevant region, so any spot on the Nile between them might in principle be quite naturally located by reference to either. But the matter is not straightforward. On the one hand there is Herodotus' "a little south of Bubastis": would he or his informant have made this qualification if the site was actually nearer Heliopolis? Those who lay stress on this

will in practice opt for a site pretty close to Bubastis.<sup>6</sup> On the other hand the lie of the land (cf. Bietak 1975, plan 4) seems to preclude a point much north of Tell Yahudiyeh, a site which is at least twice as far from Bubastis as from Heliopolis. Perhaps the answer is that Bubastis was on the Nile whereas Heliopolis was not (being connected to it, north and south, by its own waterway). From the point of view of someone travelling up the main Pelusiac branch, Herodotus' description might not after all be so unreasonable, even if the key point was around Tell Yahudiyeh, as Bietak 1975, 88 assumes (adding as an elaboration the suggestion that Darius' canal struck off from the main Nile branch, whereas Ptolemy's started from the Heliopolis waterway). One cannot fail to notice, though it may not constitute a valid argument, that the Darius statue whose Egyptian texts bear some relation to those of the canal stelae (see below) was originally erected precisely in Heliopolis.

6. *The dimensions and length of the canal* are variously stated, thus: Herodotus — width sufficient for two triremes, length = four days' journey from Bubastis to Suez and more than 1000 stades from the Mediterranean to Red Sea. Strabo — width 100 cubits, depth sufficient for a *myriophoros naus*. Pliny — width 100 feet, depth 40 feet, intended length 62.5 miles. Once again there is evidently no contact between Herodotus and the later tradition. As for the value of his figures: four days' journey is certainly longer than necessary for Bubastis-Suez, especially if one is thinking in terms of triremes. Perhaps, despite the apparent implication of the text, this is really a figure for the whole Mediterranean-Suez transit, or perhaps we should assume that the ships moved with some circumspection in the restricted space of the canal. A width sufficient for two triremes is probably in the vicinity of 30 metres or 100 feet (the latter is Herodotus' estimate in connection with the Athos canal). The figure of 45 metres often encountered in modern literature (e.g. Hinz 1975b) apparently derives from French measurements of the canal traces (cf. Wiedemann 1887, 562; Lloyd 1988b, 153) and may have more to do with Ptolemy's version of the canal (it is not far off Strabo's 100 cubits).

7. *The motives for a Nile-Suez canal*. Aristotle makes the unhelpful observation that it would have been 'useful' to the Egyptian kings for 'all this region' (? the Nile-Suez area or the Red Sea itself — the context leaves it unclear) to have been navigable. Herodotus offers no explicit comment, but does suggest certain inferences. (i) He comments on the relative distance between the two seas by land and by river/canal. (ii) He reports that the canal could take two triremes at a time and says that, upon abandoning the

<sup>6</sup> Lloyd 1988b, 153 espouses such a view, making the Darius canal start around Mina el-Kamh, very close to Bubastis.

enterprise, Necho constructed triremes on both seas. (iii) He also links the project with the alleged *periplous* of Libya. Taken together these remarks suggest that communication between the Mediterranean (not just Egypt) and the Red Sea was the real focus and that, if the background can be defined at all, it is one of military action, at least as far as Necho is concerned and perhaps also for Darius. Herodotus also observes that Scylax sailed from the Indus to the point whence Necho despatched the Phoenicians round Africa (*i.e.* Suez). But this is *not* an implicit hint about Darius' intention to establish India-Egypt communications; Herodotus is simply underlining the fact that both Scylax and the Phoenicians were engaged in exploration and that what emerged was a certain symmetry between Asia and Libya, since the two trips allegedly took comparable amounts of time (cf. How & Wells I, 320 and the map of Herodotus' world picture in Boardman 1980, 21).

8. *Necho's supposed failure.* The sources which mention Necho agree that he failed to complete his canal. But since both the canals which we know were completed (Darius' and Ptolemy's) could come to be described as unfinished, it is natural to suspect that Necho may actually have been successful. The assertions of Herodotus and Diodorus to the contrary cannot be regarded as decisive. If so, then Herodotus' version of why Necho abandoned the project (loss of life among the workers and an oracle warning that all the effort would only benefit a barbarian) will reflect fifth century Egyptian opinion hostile to the Persian canal as something serving the foreign overlord's interests rather than those of Egypt — an attitude consistent with the apparent abandonment of the waterway after 404 BC and perhaps with the assumption that Necho's purpose had been chiefly military, since we are at liberty to imagine that later Saite monarchs had different military priorities.

9. *Date.* The greco-roman sources cast no light on this at all. In particular, Herodotus' comment about Scylax sailing to Suez neither requires nor precludes a date for the canal project close to Scylax' voyage (*i.e.* 517-5).

10. *Conclusion.* The later sources are not dependent upon Herodotus. But, especially because of possibilities of confusion with Ptolemy's project, they are also not the source of potentially reliable independent tradition about Darius. Herodotus is really all we have that is of any use.

#### *Oriental Sources*

The line of Darius' canal was marked by various inscribed monuments. We have evidence for the following:

- (a) Two stelae at Tell el-Maskhutah, (i) hieroglyphic text (= Posener 1936, no. 8; (ii) cuneiform text (Jaillon 1890, 97, 101; Scheil 1930, 93 [OP]).
- (b) Lost fragments of both hieroglyphic and cuneiform text from 'Sera-

peum', north of the bitter lakes (Posener 1936, 48 n. 3). These doubtless could represent two separate stelae.

(c) Two stelae at Chaluf (alias Kabret), (i) hieroglyphic and cuneiform texts on opposite faces (Posener 1936, no. 9; DZa-c; Scheil 1930, 93-5 [Elamite]), (ii) a second cuneiform text, with different iconography (lost: evidence = reports by Rozières, Devilliers and Lepsius: cf. Menant 1887, 132; Posener 1936, 64-5; Mayrhofer 1978, §10.2); there is no telling if there was a hieroglyphic text on the opposite face.

(d) A monument at Chaluf, found by Bronard in 1863 and mentioned by Jaillon 1890, 98, but now lost. It consisted of a volute column on a polyhedral base and with a very shallow cylindrical 'capital' (diameter: 1.4 metres; depth: 12 cm.). The side of the capital had 7-8 cm. high cuneiform letters, the top a carved relief showing three standing figures and a 'Persian eagle holding a crown in its beak' (*i.e.* evidently an Ahuramazda figure).

(e) Fragments assumed to be from one stele at Koubri, 6 or 7 km. north of Suez (Posener 1936, no.10; Scheil 1930, 95-7 [Akkadian]).

(f) A lost cuneiform fragment 'from Suez' (Mayrhofer 1978, §10.1).

In addition we could reasonably postulate a monument or monuments on at least one more site west of Tell el Maskhutah (Clédat 1919, 215 suggests Tell el Kebir; and should there not also have been one at the start of the canal?). Moreover, in view of (a) and (b) above, it is tempting to suppose that the stelae came in pairs throughout. So the actual total should have been as high as 12, the figure suggested (without argument) by Ray 1988, 263. At any rate it seems clear that the project received its full share of monumental celebration, though as things stand now, only the three hieroglyphic texts (Posener 1936, 8-10) and the reconstructed cuneiform text of Chaluf (the reverse of Posener 1936, 9) are available for study.<sup>7</sup>

Viewed as a whole the stelae express a sort of equipollence between native and foreign elements, with Achaemenid style figures plus cuneiform texts and Egyptian figures plus hieroglyphic texts appearing back-to-back (Chaluf, Suez) or next to one another (Maskhutah). I stress equipollence. It is easy, as one contemplates the length and variety of the hieroglyphic texts by contrast with DZa-c, to think of the monuments as predominantly Egyptian. But to the majority of contemporary Egyptian and non-Egyptian viewers the fact that the cuneiform side contained a shorter text three times over in three different languages will not have been apparent: what they saw was simply equal amounts of Egyptian and non-Egyptian text.

It is perhaps also worth noting that the balance of Egyptian and non-

<sup>7</sup> Readers of Edakov 1980, 110 should note that the apparent quotations from the Serapeum stele are an illusion: the author is simply inadvertently using the *siglum* ('SS') which he earlier assigned to the Serapeum stele in reference to Posener 1936, no. 10.

Egyptian is less equal on another indirectly connected monument, the inscribed statue eventually erected (along with others) at Susa but originally intended for the temple of Re at Heliopolis (Yoyotte 1972a; Luschey 1983a). This is generally assumed to be roughly contemporary (certainly Edakov's arguments for putting it as much as ten years after the canal stela are not particularly strong<sup>8</sup>) but the emphasis does appear different. It is true that hieroglyphic texts are more prominent (they dominate the base and are the only ones on the body which can be easily read) and that the style and design (though not the use of paint) of the statue are in some respects Egyptian. But the clothing of the king is entirely Achaemenid and, unless he was wearing an Egyptian crown, the preponderant impression of the monument will have been its celebration of a *foreign* ruler. In the text, of course, the hieroglyphic composition does avoid the blunt assertion of conquest found in the cuneiform versions: where the latter declare the statue's purpose to be to show that a Persian man holds Egypt, the former says it has been made 'so that there should be a durable monument of Darius and so that he be remembered before his father Atum, Heliopolitan Lord of the Two Lands, Re Harakhte, for the whole extent of eternity'; and there is similar palliation of foreign conquest in the canal stela hieroglyphs.

The cuneiform text of Chaluf is well enough preserved to permit complete restoration, and probably represents what appeared in cuneiform on all the stela, though not on monument (d). The king announces that he seized Egypt and ordered the construction of a canal from the Nile to the sea which goes to Persia, that this was duly done and that ships went from Egypt to Persia. The stress is thus on Persian control of Egypt and on communication between Egypt and Iran (not between the two seas, as in Herodotus) — communication which one might view primarily as a symbol of the outreach of Persian power. Nothing in the text guarantees that Darius personally visited Egypt at any stage in the story; and whatever force is supposed to reside in 'I seized Egypt', there is no guarantee that the event in question was close in time to the date of the stele or even to the inception of the canal.

The hieroglyphic texts are poorly preserved and were first reduced to some

<sup>8</sup> Edakov 1980, 109 offers the following grounds. (i) The statue text uses a different version of the 'late' hieroglyphic form of Darius' name (cf. p.250) from that in the canal stela. (ii) The statue text (Yoyotte 1972a, 2,4-5) inserts an extra statement about Darius' personal qualities at the point corresponding to Posener 1936, 8,4 (iii) DZc uses the 'archaic' OP words *uvaspam* and *umartiyam*, whereas DSab ends with a prayer formula only paralleled in the 'late' texts DSn and DSs. (i) and (ii) are unconvincing as chronological indicators; (iii) seems positively misconceived. The only interesting thing about the two adjectives is that they normally appear in the 'Bestowal of Kingdom' formula, not in the 'Creations of Ahuramazda' one. In other words DZc is eccentric here and therefore not relatively datable at all. As for 'Me may Ahuramazda protect and all that I have done', even if DSn and DSs are 'late' Susa texts (on what grounds?) I cannot see that this establishes anything about the relationship between DZc and DSab.

semblance of continuous sense by Posener in 1936. Since then there has only been one complete reworking as far as I know, that of Servin (1951) in an obscure publication generally ignored in the literature. Oertel 1964 made some use of Servin's article but was not particularly impressed by his reconstruction of the texts, and the only more recent reference I have chanced upon is in Bresciani 1985, 509 n. 7, where the reconstruction is dismissed as unacceptable. Certainly it is very different from Posener's. By a combination of rearrangement of the fragments and simple divergent translation (neither of them justified by any argument), Servin produced a version in which Darius receives a report of famine in Egypt, goes to Sais and prostrates himself before Neith, consults the priests who advise donations to the gods and is then compelled to have a bit of canal west of the Bitter Lakes redug so that the said donations can be conveniently transported. There appears to be no question of a canal being made right through to the Red Sea. Servin even claims that the cuneiform text only guarantees digging 'in the direction of Persia' and that the 'Bitter Stream' in Scheil's translation of the Akkadian fragment refers to the Bitter Lakes and not to the sea (Oertel 1964, 26, 40 shares the latter misapprehension. 'Bitter Stream' is, in fact, nothing more than a restoration by Scheil, based on the OP text which reads *draya*, 'sea'). Without even entering into the merits of his treatment of the hieroglyphic texts, this gives one a measure of Servin's reliability, as does also his assertion that the *itrw* has been proved to be only 2.5 km (not 10.5) — recent research has continued to affirm the latter figure, while even adding some evidence for a longer *itrw* of 12.8 km. (Schwab-Schlott 1972, 112f). Posener's reconstruction therefore remains the only starting point.

Each stela has three registers of text. The upper two are apparently identical on all three monuments. Register 1 largely consists of speeches by the Nile gods announcing their gift to Darius of rule over the world and of happiness, joy, offerings like those for Ra and so forth. The two gods' discourses differ slightly from one another. Register 2 repeats the gift of all lands and gives a list of them. (On Posener 1936, no. 10 the two columns of the list are reversed).

Register 3 is the one which interests us most, since it gives a narrative of the canal's creation. It is also the least well preserved, and its contents are not identical on all three stela. Each text consists of (a) an introductory section with royal titularies and the like and (b) the narrative proper. Posener 1936, no. 8 (a) is a largely Egyptian composition with a Persian element embedded in the middle (4-5), viz.: "Darius, may he live for ever, the great, king of kings ... Hystaspes, Achaemenid, the great", a formula which, it now appears, could be supplemented on the basis of the Susa Statue hieroglyphic text (Yoyotte 1972a, no. 2,4: "Darius, may he live for ever, the great, king of

kings, supreme lord of all the earth in its totality, son of father-of-the-god Hystaspes, Achaemenid").<sup>9</sup>

The start of no. 9 as translated by Posener is very lacunose: "The god who ... (2) ... (3) .. men ... (4) king of kings ... [son of Hystaspes] (5) Achaemenid, the great ...", but it is tempting to follow hints in his commentary and try to make more of this. There is reason to think that Posener allows one line too many at the opening, *i.e.* that line 2 should be removed and a one-line lacuna placed later between lines 10 and 11 (see Posener 1936, no. 71f.); and if this is done we can then insert fragment 35 at the end of the first two lines thus:

- (1) the god who ... who created men, who created wellbeing
- (2) for men ... Darius may he live for ever, the king, L.P.H.
- (3) the King of Kings ...
- (4) Achaemenid, the great ...

in other words, a Creations of Ahuramazda formula, followed by something like no. 8,4-5, making an equivalent to DZc 1-6. But it must be admitted that working this through thoroughly would be problematic: the lacunae in lines 1 and 2 should be of roughly the same length, but (by reference to the cuneiform text) ought to contain Egyptian translations of two phrases which are of different length and the first of which seems far too short, *viz.* 'created yonder sky, who created this earth' and 'who made Darius king, who bestowed upon Darius the king the kingdom, great, rich in horses, rich in men'. Moreover the gap in line 3 seems to require something longer than just 'supreme lord of all the earth in its totality, son of father-of-the-god Hystaspes'.

Virtually nothing is preserved of the start of no. 10, but it seems clear that it was different from no. 8 and 9, and there is no proof of any Persian element at all.

The narrative in no. 9 is the least badly preserved version and one can distinguish the following elements:

- (a) consultation (7-13)
- (b) the order for digging of a canal (14-15)
- (c) the report that Darius' order was executed and boats sailed from Egypt to Persia (15-16)
- (d) a laudatory speech (18-20)
- (e) Darius order for a commemorative stela and its execution
- (f) further acclamations and praise of the enterprise (21-22).

Since the surviving words of no. 10,14-19 fit with the latter part of no. 9,14-19 — albeit with a slightly different vertical disposition (one which might suggest

<sup>9</sup> Edakov 1980, 109 inserts simply 'master of the land (*i.e.* Egypt)'. This identification of the 'land', based on the assumption (*ibid.* n. 40) that in standard OP texts 'this great earth' refers to Persia, is of course misguided. Edakov also proposes some supplements to the purely Egyptian parts of no. 8, 1-6, notably 'to conquer the two lands completely' (8,3) and '... with it [the bow], so as to ward off rebels, so as to kill (enemies)' (8,4).

that the version in no. 9 has one or two words more than that in no. 10 but probably results only from slightly more generous spacing of signs in no. 9), we may guess that elements (b) — (d) were similar or identical in no. 10. The parallel might have stretched earlier. The relevant part of no. 9 is missing higher up on the stela, so it is impossible to be sure; but what survives in no. 10 would fit quite well contextually into the relevant spaces in no. 9, provided that one postulates the lacuna between lines 10 and 11 noted above. On the other hand, it seems clear that perhaps (e) and more certainly (f) were not reproduced in no. 10, which ends with a tantalizing numeral, hard to fit into no. 9, 22, however it be interpreted. With 8 the divergences are much greater. We do not find what appears to be the issue of an order for boats to come from Egypt (something corresponding to no. 9,14 = no. 10,14) until lines 19-20; in other words the narrative is virtually entirely given over to the preliminary consultations. Even so, and even with the help of no. 9,14f, it is difficult to discern more than that Darius initiated an inquiry into the state of the canal zone and discovered that there was sand and no water over a distance of 8 *itrw* (84 or 100 km.). Neither treatment, it should be noted, seems to have given any great space to describing the actual building of the canal.

Both narratives could quite readily be associated with the so-called *Königs-novelle* genre, in which the pharaoh conceives a plan (*e.g.* for building or military activity), presents it to a council and then carries it through (irrespective of the council's opinion). Prolonged consideration of the examples in Hermann 1938 and of other texts which have been brought into this category (*e.g.* the Nitocris Stele (Caminos 1964) and the Amasis Stele (Edel 1978)) leaves me with the uneasy feeling that this is a genre the criteria for inclusion in which are rather too generous for the exercise to be useful. (My colleague Professor Kitchen tells me that he has a precisely similar feeling.) Still one can at least say that, traces of Persian titulary apart, the general 'feel' of the contents of Register 3 in Posener 1936, no. 8-10 is traditionally Egyptian.

*The King's whereabouts.* Hinz, following Posener, assumes that the consultation process was initiated in Persepolis (reading '[the city he loved] more than anything' in no. 8,7). All that is really certain is that the narrative starts in Persia (no. 8,6; no. 10,5) and that there may be reference to a residence built by Cyrus (no. 8,7; no. 9,6). Hinz further proposes that Darius visited Egypt at the end of the project — the basis for this is presumably the assumption that the *Festansprache* of no. 9,18-20 = no. 10,18-20 and the king's order for a stele (no. 9,20) occurred in Egypt. This strikes me as actually a gratuitous assumption. The claim that the reference to 'princes and inspectors' in no. 9,17 relates to the issue of invitations to the official opening of the canal at which the king will be present is not particularly compelling: they could just as well be passengers on the boats newly arrived in Persia (*cf.* no. 9,15); and the orator of the *Festansprache* might be one of their number. I am not, of

course, claiming that this is definitely the case, only that Hinz's interpretation is not proven.

*The course of the canal.* The only relevant data are (i) a reference to Hrm (a lake/canal in the Pithom nome) in no. 9,13, (ii) the toponym *Sb* in no. 8,10, 11, 14, 17 (probably somewhere near the canal zone) and (iii) the distance of 8 *itrw* where there was no water (no. 8,18), and of these (i) and (ii) are quite unhelpful. (iii) must relate somehow to the length of the eventual canal and might be thought to bear upon its route. The greco-roman sources have already fixed the general route as Bubastis, Wadi Tumilat, Suez, and the total length of the canal would be in excess of 160 km. It follows that only a portion had to be cleared of sand, and the question is whether we can discern which 84 (or 102) km. was involved. Since (a) the continued existence of the Tell el Maskhutah site, originally founded in conjunction with Necho's canal project (Holladay 1982), seems to presuppose that a (freshwater) canal remained open between there and the Nile (which tells against locating the 84 km. in this sector, *contra* Oertel 1964, 40) and (b) the distance from Maskhutah to Suez along the modern freshwater canal is a little over 90 km., it is tempting to suppose that it was this latter stretch which needed clearing (from which it follows that the canal was cut around the western edge of the Bitter Lakes). Indeed, so far as the 8 *itrw* are concerned, no other solution seems possible. It would not help to assume that the canal traffic did use the Bitter Lakes and that it was the whole of the remainder of the Bubastis-Suez transit which needed clearing, since the Lakes would only account for some 40 km. of the 160+ km. total. By contrast, Hinz's claim that the stelae prove the canal to have contained fresh water the whole way — precluding a transit of the Lakes — seems to me to be in itself ungrounded. Yet Clédat 1924, 62f. claimed that the ancient canal traces only existed at the north and south of the Bitter Lakes, through which (he deduced) traffic must have passed, and nobody has subsequently claimed contrary archaeological evidence. Thus Posener states the find spots of no. 8 and no. 10 in relation to the canal traces, but not that of no. 9, which is near the edge of the Bitter Lakes. Moreover, to judge from the map in Clédat 1919, the actual site of 9 is so close to the edge at a point where it has a sharp north-easterly indentation that it could hardly be supposed to mark a canal making its way from the Wadi Tumilat to Suez by the most expeditious route round the Bitter Lakes. I am unsure what the correct solution is. But Strabo thought that Ptolemy's canal at least went through the Bitter Lakes (and the fact that the Pithom stela talks of a canal from 'north of Heliopolis' to 'Scorpion Lake' might be cited in support), and I wonder whether we are justified in insisting upon a particular interpretation of 8 *itrw* when its implications are arguably in conflict with the material evidence.

*Date.* Several arguments have been or might be used to establish a date in the 490s. All turn out to be more or less flawed.

(i) *A date formula?* Posener no. 10 ends with the numeral '24'. Posener doubted that this was a date, largely because, for various other reasons, he was sure the text predated year 24. There is, of course, a reference to 24 ships in no. 9,16 (unless the correct restoration is actually 32), but it is not particularly obvious why they should suddenly reappear at the end of the final sentence of 10 — and still less obvious that the 24 peoples of Register 2 are to the point. A date remains the most probable thesis. Hinz took this view and understood a reference to year 24 (498) — this being, he claimed, when the canal was actually finished, as distinct from the date of the celebrations at which Darius was present, which he placed in 497-6 (see below). This is slightly problematic, since what immediately precedes in 10 is the *Festan-sprache* which belongs to the celebrations of (allegedly) 497-6; should the date-formula not also refer to that context? But more important doubts are raised by the facts that (i) the reading '24' is uncertain (only '4' is unimpeachable) and (ii) there is, as Professor Kitchen has kindly assured me, not the slightest reason, so far as surviving traces go, why we should not understand '24' as the 24th day of a particular month whose identity is now lost. In short, there is nothing in the final words of Posener no. 10 to require a date in the 490s.

Hinz's belief that Darius visited Egypt in 497-6 is based on an argument from PFT. The point is simple, that there is no attestation of Darius' presence within the area covered by the Persepolis bureaucracy between November/December 498 (PF 1554 — 9/xxiv) and April 495 (1313—1/xxvii), these being the dates on two travel documents (Q texts) in which a royal travel authorization is mentioned. Hence the conclusion that Darius was away from Persia for most of years 25 and 26. This is seductive but perhaps deceptive.

(a) Hallock held that J texts ('dispensation on behalf of the king') presupposed the king's presence. One such text (699) is dated to year 25 (location: Uzikurras). Naturally this might belong to the very start of year 25 and hardly encroach upon the supposed absence (see below however); but the mere existence of the text spoils the perfect silence from which Hinz constructs his argument. Of course, Hallock's view of J texts might be incorrect; and another year 25 text mentioning the king (1827) probably does not prove Hinz wrong, since it is a letter of Ziššawiš transmitting a royal order which doubtless could have reached him by letter from Egypt.

(b) There is a more general statistical point. The surviving dated PF texts are not distributed evenly through the years covered by the corpus, and the distribution of Q texts is particularly uneven. Since there are only eight texts altogether from year 26 the fact that none are Q texts is not something from which to draw any inferences. Year 25 may seem slightly less clear cut, since

it is represented by 67 texts. Yet when one considers that it is hardly probable that nobody went through Fars in year 25 carrying a document authorizing travel rations (whether issued by the king or some other high dignitary) and that in e.g. year 24, when far more tablets survive (168), only 8 are Q texts and only three involved royal authorization, it remains difficult to be impressed by much except the vagaries of survival. Incidentally, since a J text has entered the argument above, it should be noted that the pattern of survival of J and Q texts is not uniform. In year 26, for example, there are many Q texts, including ones mentioning the king, but only one J text (PFa 31). So the solitary J text of year 25 does not establish that the king was absent for much of the year. Of course, there is a contrast between year 25 and year 28, which produces a similar number of texts all told (61) and nine references to royal travel authorization in six different months. But, then, there are 26 Q texts (well over a third of the total for the year), seventeen of which, let it be noted, do not mention the king. The pattern of survival is simply quite different. To put the matter simply: one should only think of deducing royal absence in a year which produces plenty of Q texts, none of which mentions the king. There is no such year.

One final observation. I suspect that when Hinz turned to PFT he was hoping to discover some sign of Darius' absence in year 24. As a matter of fact he might have done better to stick to that aim and highlight the fact that the 8 Q texts from that year only mention the king in months 3 and 9, leaving a theoretical gap from May to November. But the time for a round trip to the Nile is rather tight, and in any case the distribution of Q texts through the months of any given year is not even either, with the middle months receiving less attestations, so there would be statistical objections once again.

The upshot, then, is that PFT provides no independent reason to postulate a date in the 490s for the canal stela. Are there other phenomena which can do so more successfully?

(ii) *The King's Name.* Hieroglyphic texts use two main spellings of Darius' name (each with additional variants which do not concern us here) with initial T and INT respectively. The stela use the latter, often regarded as proper to the very last part of Darius' reign. The facts are as follows:

1. The T form is attested in year 4 at the Serapeum and years 26-7 at Wadi Hammamat; and it is standard in demotic throughout.

2. The INT form appears in years 28, 30, 36 at Wadi Hammamat, years 31 and 34 at the Serapeum and years 33 and 34 on alabaster found in Syria and Susa. It is also reflected at least once in demotic in P. Loeb 45 (year 25).

3. Among undated documents the large majority use the so-called late form. (T appears in Bresciani 1958a, 267 (lower Egypt), Posener 1936, no. 13,114 (upper Egypt), and Posener 1936, no. 101 (no provenance). INT appears in Posener 1936, nos. 102, 103, 105, 110, Bresciani 1958a, 268,

Burchardt 1911, 71, Naville 1882, pl. viiA, the Susa statue (Yoyotte 1972a) (lower Egypt), Posener 1936, nos. 35, 107, 115 and the Hibis temple inscriptions (Winlock 1941) (upper Egypt), Posener 1936, nos. 39-42 (vases from Susa) and Posener 1936, nos. 104, 106, 108-109, 111-113, 116 and Gropp 1979 (no provenance)). These facts certainly do not justify a late date for the canal stela.

(a) The orthographic point at issue is the problem of writing OP /d/ in a language lacking that sound. This was a problem whose discovery did not need to wait a quarter century.

(b) Given that we have no dated spelling of Darius' name between his accession and his 26th year save in year 4, we are in no position statistically to draw any conclusions; at the very least we should need a set of Serapeum texts from the middle of the reign before we could claim a usable spread of evidence.

(c) The fact that inscriptions in Wadi Hammamat shift from T to INT in year 28 carries no implications even for the rest of upper Egypt. In fact it hardly guarantees that Darius' name might not have been written with initial INT at Wadi Hammamat prior to year 28. The demotic of Loeb 45 confirms that at least one scribe in upper Egypt had perceived the pronunciation problem by year 25.

(d) The sharp numerical imbalance of undated texts using the two forms (even larger than may initially appear, considering the number of texts at the Hibis temple and the fact that these would have been cut stage by stage throughout an enterprise which the excavators believe to have had more than one distinct phase) suggests on the face of it that the INT form should not be confined to Darius' final 8 years (less than a quarter of his reign). Of course, monuments bearing the king's name might not be spread evenly throughout 521-486; it could even be a matter of some historical interest that they were not (*i.e.* that there was an exceptional flurry of activity at some period). But we can hardly assume it to have been so in default of independent evidence.

(iii) *Synarchy?* Calmeyer has suggested that the iconography of the Chaluf stela (cuneiform side), in which two royal figures face one another each accompanied by a cartouche (only the left-hand one remains legible), represents two actual 'kings' — *i.e.* Darius and his co-regent/heir apparent (Calmeyer 1976). If this were so, and if the co-regent were Xerxes, then the monument could hardly be much prior to the end of the sixth century. But the *Synarchie* thesis is insufficiently compelling to be exploited thus. And in the Chaluf stela I should be surprised if the name in the missing cartouche were not Darius', just as the text to the right of the right hand king, like that to the left of the left hand one, names Darius. The iteration of figures is a matter of design not *Staatsrecht*.



(iv) *The List of Peoples* on Register 2 naturally tempts the chronologer. We now know that it contained Skudra, thus disposing of the old view that the text preceded the Scythian expedition. We also know that, since the same list was used on the Susa statue, any peculiarities (and notably its shortness) cannot simply be attributed to the spatial exigencies of a single monument but go back to a master copy. And the list certainly has various peculiarities.

(a) Ionia, Caria and Gandara, present in some form in all other texts (Caria only from DSe onwards), are here absent.

(b) The Saka are treated unusually. We expect either plain Saka or (more likely) Saka haumavarga and Saka tigraxauda as two units. What we get is 'Saka of marsh and plain' (not a bad description of possible different categories of central Asian nomads) or, according to Cameron 1975, after Hughes, 'Saka beyond Sogdiana' (a Persian formula, but not one proper to full lists of peoples). Neither is the product of mere inadvertence; and neither would most obviously be explained as deliberate rewriting of a conventional original by the Egyptian translator. So this would seem to be an eccentricity originating with the Persian officials who supplied the list to the Egyptian composer.

(c) The same cannot be said of two problems of order. (i) Sardis - Cappadocia - Skudra is eccentric by the standards of other lists. (ii) The treatment of the names in column 1 is also odd in including Arachosia and Sattagydia within the Areia, Parthia, Drangiana, Bactria, Sogdiana, Choras-mia group and in separating them one from another. Both these look like carelessness in transmission between the Persian original and the hieroglyphic translation.

Given other (questionable) indications of a 490s date, it is tempting to connect the absence of Ionia and Caria with the Ionian revolt. But are we really to think that the Persian authorities issued a text admitting to rebellion in this fashion, especially a rebellion they had every intention of putting down? Would the fact that the Egyptians could not have been unaware of rebellion in Asia Minor have compelled the Persians to be honest? And was there a revolt in Gandara as well about which they felt constrained to be open? In truth, one is tempted to say that 499-494 is the least likely period for a List of Peoples to have been constructed which omitted Ionians and Carians and that it was only in periods when the area was uncontentious that the Ionians might have been regarded as mere adjuncts of Sardis. There is, of course, an alternative approach — to suppose that the text does date from the 490s and that whoever drew up the hieroglyphic version omitted Ionia and Caria as a private gesture of resistance. One can even speculate that the original Persian list contained 28 names in two columns of 14, that the deletion of e.g. 'Ionians by the sea', 'Ionians beyond the sea' and 'Carians' from one column required the deletion of Gandara and the transfer of India

to the other in order to produce two columns of 12 and maintain symmetry. Doubtless there are other possible versions of such speculation. I do not know how to decide how likely such a scenario is or how likely it is that some Persian official would have noticed that the list had got shorter and complained, even if he couldn't actually read the hieroglyphic characters.

The list remains a puzzle, but I am not convinced that it requires a date after 499.

(v) The clearance of 8 *itrw* of silted canal is generally deduced from the stela narrative. Hinz, appealing to French surveyors' 45 metre width, calculated that Darius' workmen had to move 12 million cubic metres of sand, and thought this might take the 12 years from 510 to 498 BC. He does not explain, incidentally, why he assumes the canal to have had a depth slightly in excess of 3 metres. I have no idea whether this is a 'reasonable' estimate — the only immediately pertinent parallel, the construction of 2.2 km of Athos canal to similar width specifications, is of no help given that the latter was built from scratch and that we do not know how long it took, only that it must have been done within the period 484-482 inclusive. Hinz's figure is probably an overestimate anyway, since the canal need not have been 45 metres wide (see above p. 241, §6); the sand to be shifted may only have been some 8 million cubic metres and a job capable of completion in 8 years (even accepting Hinz's other assumptions). The canal will not have been started before Darius' fifth year and could therefore have been completed as early as 510 B.C. More radically, it may be simply wrong to think in terms of a calculation which involves multiplying 86km. by some figures for width and depth. This is to pay no attention to the fact that Necho had already done some or all of the job once. For there to be 'no water for 8 *itrw*' would not require the total, or even very significant, disappearance of the actual earthworks. After all the French found very considerable traces after a much longer period since the canal's last viability than had elapsed between Necho and Darius. One might also wonder whether the scant attention paid to the actual construction of the canal hints that the operation was not as gargantuan as is sometimes thought; but it must be admitted that the lost stela may have had a Register 3 text with concentrated on this aspect, just as Posener 1936, no. 8 concentrates on the prior consultations.

(vi) Edakov 1980 has attempted to deduce a date for the canal's completion on the basis of the following propositions. (a) The canal monuments are earlier than the Heliopolis statue, though there is nonetheless much in common between the texts used in the two cases. (b) Both the canal monuments and the statue are earlier than the Hibis temple texts. (c) There is a significant similarity between parts of the statue texts and DNab, the latter being datable c.500. (d) The building-project at the Hibis temple probably occupied c.510-490. From all this Edakov proposes a date of c.500 for the

completion of the canal. (The start he locates at the same time of the supposed visit of Darius in c.518 — itself a somewhat uncertain quantity: see below p. 264f.).

Proposition (a) has already been discussed in n.8: Edakov's reasoning is not cogent, but this may not matter such, since his final conclusion (canal completion c.500) actually minimizes the distance between canal and Heliopolis monuments (given that 500 is also the alleged chronological horizon of DNab). Proposition (c) is arguable, especially in regard to the date to be assigned to DNab. Proposition (d) was no more than a speculation by the excavators, at least partly based upon what I regard as false dogma about the 'late' form of Darius' name (above p. 250). Proposition (b) is the most interesting one. It depends on two facts: the Hibis texts use the same hieroglyphic writing of the 'late' form of Darius' name as the statue texts and a different one from the canal stelae; and the Hibis texts preserve at one point a full titulary of Darius including Horus-name (lost) and throne-name (Setut-Re), whereas these do not appear on the canal stelae or the statue (or indeed anywhere else). The first of these facts cannot really be construed as a substantially useful chronological pointer. The second is only significant if we are sure that the authors of the canal stelae and statue texts would have used the Horus- and throne-names if they had existed. If we do make that assumption, two conclusions follow. First, some time did elapse between the canal/Heliopolis monuments and the completion of the Hibis temple; but since the claimed date of c. 490 for the latter lacks real basis, this does not get us very far. Second, no coronation-ceremony had taken place by the time the canal was completed, which probably means that Darius neither visited Egypt prior to, nor on the occasion of, that completion. The contents of the canal stelae do not in fact require us to believe anything different from this (see above p. 247). But once again, since we lack an independent fix on when Darius *did* visit Egypt (see below p. 264f.), the conclusion is not very helpful chronologically speaking. Certainly there is no sense in which 500 emerges from these considerations as a more probable date for the canal's completion than any other date. One could just as well argue e.g. that Darius' first visit to Egypt was c.506/5 (see p. 266) and the canal finished a little before that. Moreover, the key assumption that the *absence* of Horus- and throne-name in Posener 1936, no. 8-10 and the statue texts is significant is not beyond question. Of all the Egyptian texts mentioning Darius, those on the walls of the Hibis temple were the most likely in any case in terms of genre to use the full titulary at some point. If they turn out to be the only ones to do so (and then, it should be stressed, only once in the preserved remains), we should perhaps not leap to any chronological conclusions.

So far as date goes, therefore, the upshot is that the oriental sources certainly do not *prove* the canal to have been finished in the 490s. The

orthodoxy established by Hinz is unjustified,<sup>10</sup> and there is in fact probably no view which deserves to acquire the status of orthodoxy.

*Motive.* No explicit statement of motivation survives in the canal stelae, though the complete text of Posener 1936, no. 8 may have contained one. One would particularly like to know the import of references to Cyrus and to the mysterious *Sb*. As in the cuneiform texts the proof of the canal's completion is the despatch of ships from Egypt to Persia, suggesting the importance of communication with the imperial heartland. The hieroglyphic text adds the ships numbered 24 (or 32) and carried tribute, confirming that this communication is (at least *inter alia*) a symbol of Persian domination. One other point may be noted. Posener 1936, no. 9 ends with the remark that 'never had the like happened before'. Although in Egyptian practice the chief characteristic of proper behaviour was often quite the reverse — an action was good in proportion to its repetition of earlier actions — claims to novelty are not precluded (cf. e.g. Amasis' innovation in the matter of Apis sarcophagi, noted on his official stele), and are particularly in place in texts which are habitually assigned to the *Königsnovelle* genre (e.g. Hermann 1938, 9-11, nos. I, III, IX, XVI-XVIII). So the comment is not alien to Darius' role as Egyptian pharaoh. What is hard to know is how great an actual novelty was required for such a claim to be made. Does it prove that Necho's canal was never completed (perhaps never even came near to being completed)? Or is the reference to something less substantial? More important, is the intention to dismiss Necho's precedent entirely or rather to hint that Darius had followed the lead of a native king, but had done the job better, this being presumably a sign of his divine election? Given the state of the text, one can only pose the question and draw attention to two different states of mind on Darius' part which might be relevant to explanation of the undertaking.

#### Conclusion

The canal ran from south of Bubastis (probably around Tell Yahudiyeh) to Suez via the Wadi Tumilat, but it is unclear whether it traversed or circumvented the Bitter Lakes. It is conceivable that Darius was simply re-opening a waterway already completed by Necho. In any case, it is impossible to be sure how large a job was involved or how long it might have taken. There is no secure textual evidence for a completion date as late as the 490s, nor any proof that Darius was actually present in Egypt at the start or end of the project — though I am inclined to think that a visit at the end may be *a priori* likely. The oriental sources see the canal in terms of communication with Persia, Herodotus sees it in terms of communication between the Mediterra-

<sup>10</sup> The protest in Root 1979, 67 is spoiled by mis-statement of the conclusions of Hinz against which the protest is being made.

nean and Red Seas and leads one to think particularly in terms of military shipping. There is no proof that he knew of any triremes actually making the trip between the two seas, though his description of the length of the Red Sea as "40 days under oar" is conceivably indirect testimony to trireme movements in that area during the Persian period (cf. Oertel 1964, 33 n.38, but see also p. 271 below). On the oriental side it should be noted that in Posener 1936, 9 and 10 the boats are *kbnt*-ships, the term which Lloyd thinks could be used to designate triremes (1972, 272f; 1988b, 153). There may be reason to think that the Egyptians of Herodotus' time and of the fourth century did not regard the canal as of sufficient use to themselves to save it from the stigma of association with the foreign power.

#### CONTEXTS

Having discussed the immediate documentation on the canal, we may turn to the wider contexts within which the enterprise could be viewed, viz. (1) the history of Persian relations with Egypt from 525-486; (2) the evidence about use of the Red Sea and the value of creating a direct Nile-Red Sea link; and (3) the general impulses of Persian conquest and rule.

##### 1. *Persia and Egypt, 525-486.*

(i) *Motives of Conquest.* There are three strands in the sources. The least interesting perhaps is the role of Phanes of Halicarnassus, whose desertion allegedly provided Cambyses with strategic information needed for a successful invasion. The important issue of access across the Syro-Egyptian desert will recur, but we should note immediately that building the canal in itself hardly made a substantial and reliable contribution to the exposure of Egypt to Persian attack (and hence continued control). If direct access across Sinai were blocked, a maritime assault starting from the Persian Gulf was only feasible if all the intermediate shores were firmly within the imperial framework, something that certainly never happened. If it was *envisaged*, of course, the canal would facilitate the transfer of the necessary maritime forces from the Mediterranean, but that is a different matter.

The second strand is inheritance of a plan of Cyrus. Deutero-Isaiah pictured Cyrus' acquisition of Egypt, Nubia and Saba (XLIII 3; XLV 14), Herodotus wrote that Cyrus planned to conquer Egypt (I 154), Xenophon even asserted that he did (*Cyr.* I 1, 4; VIII 6, 20f). How fully formed a plan this was is hard to say. The only possible Achaemenid evidence on the matter is the fact that the Pasargadae 'Genius' wears an Egyptian crown. Some argue that the immediate derivation is Syro-Levantine and that any intimation of foreign conquest is in that direction (Stronach 1985, 843); but one might regard this as unduly evasive and prefer the idea that Egypt is included within the sphere of potential Achaemenid control (Root 1979, 302; Nylander

*ap.* Donadoni 1983, 43, suggesting four figures with different symbolic crowns). I am less certain that there are other 'Egyptian' features at Pasargadae pointing the same way. *Pace* Metzler, the placing of *genii* on door jambs is as likely to be Assyrian, the comparison of the *paradeisos* and the Karnak 'botanic garden' seems gratuitous, and the derivation of palace design from Karnak rather than greco-anatolian influence (Nylander) or Iranian tents (von Gall) is unconvincing. It would be hard to prove that Egyptian stylization of wings at Susa and Persepolis represents a tradition going back to Cyrus' time. The fact remains, however, that when the sources deal explicitly with Cambyses' invasion the theme of inheritance from Cyrus is only indirectly present.

The principal explanations of the invasion turn on marriages. The Persian version was that Cambyses had Nitetis, Apries' daughter, among his wives (Hdt. III 2, Dinon 690 F11). The Egyptian version (Hdt. *ibid.*, Ctes. 588 F13a) put the same lady in Cyrus' harem and (though a third variant denied this) claimed that Cambyses was her son and therefore Apries' grandson. In either case Cambyses' motive for invasion was to punish the Egyptians for providing a daughter of Apries, when what had been requested was a daughter of Amasis. The general view is that there was no truth in any of this. The origin of the tales is variously explained. Atkinson 1956 saw it as a Persian fabrication based on the principles of 'dowry-conquest'. She actually identified Herodotus' 'Egyptian' version as the Persian fabrication; but either version would do for her purpose. The alternative view makes it an Egyptian fabrication intended to render Cambyses more palatable by turning him into an Egyptian and making him the proper successor of the last legitimate king of Egypt — Amasis and Psammetichus III being regarded as usurpers. (Amasis had, of course, acceded by armed rebellion and faced more resistance than is immediately apparent from the Herodotean account: cf. Leahy 1988). The Persian version will then, presumably, be an attempt to deny the racial slur while maintaining the basic story of Egyptian duplicity.

The independent evidence that Amasis was repudiated *by Egyptians* as a usurper is not actually strong. The erasure of cartouches on some royal monuments (not just references to Amasis in private ones)<sup>11</sup> and Cambyses' alleged mistreatment of his mummy (Hdt. III 16) have no proven connection with such a view. In P. Dem. Rylands IX 16/1 Amasis is mentioned twice in date formulae and on the second occasion 'pharaoh' is omitted; in *ibid.* 27/7-9 both Amasis and Cambyses appear in date formulae without royal title. Elsewhere in the narrative we find 'pharaoh Amasis' (16/7) and anonymous references to him as 'the pharaoh'. These phenomena are alleged to reflect denigration of Amasis during Darius' reign. One could be forgiven for

<sup>11</sup> cf. Bresciani 1967, 277; Meulenaere 1968, 184. The recently noticed 'privatisation of a head of Amasis' (Josephson 1988) had also better not be over-interpreted: the object is still, after all, only 6 cm. high.

doubting the allegation; and once again usurpation is not the proven reason. There remain the Coptic Cambyses Romance and John of Nikiu 51, 18-49 which name Cambyses' opponent as Apries and are supposed to 'make it clear ... that Amasis was not regarded as a legitimate king in all quarters' (Lloyd 1988a, 62) — i.e. the *damnatio memoriae* of Amasis as usurper was so strong that the authors of the tradition ultimately represented in these late sources came to regard Apries as not only the last *legitimate* king of Egypt, but the last one *tout court*. Yet Jansen 1950 argued that John of Nikiu is not independent of the Romance (cf. Cruz-Urbe 1986) and that the Romance derives from an originally relatively uneccentric account which was later 'revised' by a Jewish author under the inspiration of Nebuchadnezzar's conflict with the Jews; and if this is anything like the truth the intrusion of Apries could be a side product of the intrusion of Nebuchadnezzar as Cambyses' uncomfortable alter ego and have nothing to do with Saite and XXVIIth Dynasty politics. Indeed, even if the thesis of a Jewish author is rejected (cf. MacCoull 1982, Cruz-Urbe 1986), the crucial point is that, since the replacement of Amasis' son Psammetichus III by Apries is not a isolated oddity but is matched by the confusion of Cambyses and Nebuchadnezzar, we can hardly claim that the former reflects partisan views c.525 unless we are prepared to say the same of the latter. But that amounts to conceding that Egyptians associated the invasion of Cambyses with that of Nebuchadnezzar, and if we concede that we might after all conclude that Apries gets into the story because he was the target of the earlier, Babylonian attack (cf. Spalinger 1977). This dilemma leaves one feeling that the Cambyses Romance is simply not a very cogent item of evidence.

In short, it is possible that Amasis was subjected to posthumous condemnation (witness cartouche erasure), though not persistently or widely (see below), but that this was on account of his being a usurper is not proven. So there is no independent confirmation that accounts of Cambyses' motives in 525 play on marriage relations with Apries because there was an attempt to exploit native hostility to Amasis as an interloper. Even if we stick with the assumption that Herodotus III 2 derives ultimately from *Egyptian* fabrications, the explanation of Apries' appearance may be simpler. Any invention intended to turn Cambyses into a member of Egyptian royal dynasty could not do so by making him a son of Amasis without denying Cyrus' paternity and devising some complicated explanation for his Persian mother having an Egyptian royal paramour. Apries was the latest pharaoh with whom it was straightforward to fabricate a family connection — straightforward because the idea of Cyrus having an Egyptian wife was not especially eccentric. Kings did take wives from foreign places (Amasis had a Greek one; and could Herodotus' Babylonian Queen Nitocris be a confusion resulting from the presence of an Egyptian princess in a Babylonian harem?), and it was easier

to intrude a fake princess into Cyrus' harem and deny Cassandane's maternity than to intrude an Iranian princess into Amasis' court and then explain how Cambyses came to be born in Iran. By the same token, of course, such fabrications would also have been easy for Persian sources to perpetrate (especially given the evidence of conquest-by-marriage to which Atkinson drew attention, even without her elaboration about Iranian views of the law of dowries). By the same token again, there might even be some truth in it (cf. Lang 1972). It is hardly impossible that Amasis could have sought an accommodation with Persia by despatching a royal princess to the harem of Cyrus or even Cambyses. We do not have to postulate birth dates for Cambyses and Nitetis which would make the latter match grotesque. In fact, if we were to admit the truth of the story, the 'Persian' version (union with Cambyses) is perhaps the better one to follow; the mechanism of distortion into the 'Egyptian' version is easy to see, whereas it is less easy to see why the Persians should turn a wife of Cyrus into one of Cambyses. An additional possible twist to the problem is the suggestion that Amasis himself might have married a daughter of Apries.<sup>12</sup>

In short: the general approach is to regard Herodotus III 2 as evidence for propaganda — Egyptian manipulation aimed at assimilating the conqueror and Persian claims that it was Egyptian duplicity which caused the conquest (claims which could be used to suggest that the Achaemenids' real enemy was Amasis and that Egyptians at large could hope for friendly treatment). The possibility should be considered, however, that the Persian line was more firmly based in reality (cf. Lloyd 1983, 286 n.1) — that there had been matrimonial 'diplomatic' contact between Memphis and Pasargadae which Cambyses eventually chose to turn against the Egyptians by maintaining that a daughter of Apries was an unsatisfactory sort of pawn. In either event one other thing is worth noting. The Egyptian response was not to deny duplicity but to Egyptianize Cambyses: it mattered more to assimilate the new order as much as possible to the previous one than to defend Amasis' credit — which indicates how important *Persian* assimilation-policies would be.

(ii) *Cambyses' rule of Egypt* in greco-roman sources, is notoriously a spectacle of violated Egyptian sensitivities, aggravated military failure and personal madness. It is important to redress the balance, though not to overreact. Cambyses followed correct precedent by having throne and Horus names

<sup>12</sup> See Vittmann 1975, 384f. Godron 1986 suggests a connection between the oculist who figures in Herodotus' story and the doctor Udjehorresnet. Striking in a different way is the parallel with the Bentresh stele: here a foreign princess has married Ramses II and her family in Bakhtan request that an Egyptian doctor be sent to treat the princess's sister. The story is non-historical and perhaps invented as late as the Persian period (Erman 1883; Posener 1934 and most recently Moerschauer 1988, whose revival of the suggestion that Bakhtan = Hamadan/Ecbatana sharpens the connection with the Persian period).

conferred upon him, presupposing coronation as pharaoh of Egypt — which is what he chiefly was for the final three years of his reign: it is not always adequately stressed that from 525 he ruled his whole empire from Memphis. The contrast with the Assyrian conquerors of Egypt is telling. At the instance of Udjahorresnet he deported himself as benefactor of the Neith temple at Sais, doing 'all the things that had been done before' — an important gesture of continuity in the homeland of the Saite dynasty and one which sorts ill with the alleged mutilation of Amasis' corpse. Of course, the gesture followed an initial period of disturbance ('misfortune' the like of which had *not* been seen before — a telling formal contrast), during which all sorts of unpleasant things might be thought to have occurred (Posener 1936, no. 1, 17f., 31f., 37f.). He was prepared to consult Egyptian oracles (Hdt. III 64 is decent evidence for this even if the response is *post eventum*). He married two of his sisters — before 525 if Herodotus is right, but one of them accompanied the Egyptian expedition and died in Egypt (murdered by Cambyses, the hostile tradition had it). Royal sibling marriage had a long Egyptian history and may have been practised in the Saite dynasty (Vittmann 1975, 384f.; Psammetichus II, Apries). He followed Amasis' practice in donating a stone sarcophagus for the deceased Apis bull.<sup>13</sup> The animal died at the time of the invasion but was not buried until much later, circumstances doubtless responsible for the false assertion that Cambyses killed him.

Such an accusation reflects the fact that not all Cambyses's actions were respectful. Principally he overthrew 'all the temples of Egypt' — so Elephantine Jews wrote a century later (AP 30, 14), and there are similar generalizations in Diodorus I 46, 4; 95, 4 and Strabo XVII 1, 27. But two of them are tagged with suspicious vagueness onto reports on specific depredations at Thebes, while the third is part of Darius' assertion of exceptional benignity towards Egyptian values — Persian propaganda, in other words, whose effects explain the Elephantine document. Other texts refer to just three sites of damage to religious buildings; Heliopolis (Strabo XVII 1, 27), Diospolis/Thebes (Strabo XVII 1, 27; 46; Hecataeus 73A13a D.-K.; John of Antioch 4,552M; Diod. I 46; 49,5) and Memphis (Hdt. III 37, Strabo X 3,21). These were major centres, only too liable to the rapacious attentions of Persian soldiery though the claim that there is archaeological confirmation in the case of Thebes/Karnak is of uncertain weight (Redford 1986, 328 n.192). We would do well not to assume casually that there was devastation everywhere. The topos about recovery of images looted by the Persians found in fourth and third century Ptolemaic texts (Lorton 1971) is, of course, a reaction to the events of 343 not of 525. What *did* occur (almost) everywhere was an assault upon the finances of temples, which must be the chief single cause of

<sup>13</sup> Moreover Amélie Kuhrt points out to me that Gunn's description of it as 'cheap and hasty work' (Gunn 1926, 96) is an unjustified slur.

Cambyses' bad reputation in Egypt, a bad reputation his successor was uninterested in seeking to dispel. If Cambyses was an Egyptian pharaoh, he was one who wished to change the relationship of king and temple. One did not *have* to be foreign to think of this: same fourth century pharaohs showed similar aspirations.

But he was also one with military aspirations appropriate to the most energetic of his native predecessors. One should perhaps query the alleged plans for conquest of Carthage. But the Siwah and the Nubian enterprises are real enough. The despatch of an army to the former via Khargeh, Dhakhla, Farafra and Baharia was intended to assert control in areas within the sphere of Saite influence. There are signs of early 6th century activity at Khargeh; and it is in the Saite period that we start to find Egyptians active at Siwah (note especially Amasis' temple at Asharmi and the evidence of Egyptian burial practices). The local ruler of Siwah, pictured in XXVIth dynasty wall decorations, evidently acknowledged Saite power (though Lloyd 1974, 196 avoids the suggestion of Egyptian suzerainty which he is prepared to make for the XVIIIth and XXIInd dynasties) and Cambyses would have been failing in his duty as Amasis' successor had he not sought to extract at least a similar recognition, and he may have aimed at a more substantial subservience — not merely maintaining the Saite realm but extending it. The case of Nubia is arguably similar.

The facts about Nubia *alias* Kush and the Persian empire are as follows.

a) *Persian sources*. Lists of Peoples name Kush in all cases save DB and DPe (the same pattern as Libya and Skudra), which means that rule of (and receipt of tribute from) Kush is not claimed until after the first appearance of India and therefore some half-decade into Darius' reign. Bearing in mind, however, that Libyan recognition of Persian rule is already alleged in Cambyses' time (Hdt. III 13, 3), one is not entirely surprised that greco-roman sources attribute conquests in Nubia to Cambyses. The possibility remains, of course, that Darius made extra gains in both areas and that this is why they begin to appear separately from Egypt in his lists. Claims to hegemony in Nubia are also reflected in the Apadana 'tribute bearer' reliefs, where the offerings include elephant tusks and a giraffe or okapi. It is regrettable that P. Loeb I, which may refer to grain shipments from Nubia (strictly *p3 dw*, literally 'the mountain' or 'the desert'), casts no light on the nature of the relationship.

b) *Herodotus*. (i) Cambyses undertook an expedition against the Makrobioi Ethiopians at the edge of the earth on the southern sea (III 17, 1; 25, 1; 114). He ran short of food and came to halt in a desert barely one fifth of the way there. The Makrobioi are often located at Meroë on the plea that the Table of the Sun = the Sun-Temple of Meroë. But the latter is probably nothing to do with sun cult (cf. Shinnie 1978, 223) or anything like early enough in date

(Burstein 1981, 3). (ii) Later (III 97) Herodotus reveals that, on his way to the Makrobioi, Cambyses subdued the Ethiopians on the borders of Egypt who live around Nysa and hold festivals of Dionysus. Some emend the slightly inconsequential text to produce two groups — those on the borders and those at Nysa. These people, who were required to despatch gifts to the king every third year, are obviously quite distinct from the Makrobioi and will be the same as the Ethiopians 'above Egypt' in Xerxes' army (VII 69). 'Nysa' clearly derives from *nhsj* (i.e. Nubia). (iii) Writing about the upper reaches of the Nile, Herodotus makes it clear that (a) there are Ethiopians at all points south of Elephantine and (b) that one can distinguish Ethiopians at Tachompsa (= Djerar), nomadic Ethiopians a little further upstream and 'the other Ethiopians' whose capital is at Meroe, a very great deal further upstream (52 days from the second cataract), where Zeus and Dionysus are worshipped and an oracle of Zeus governs political life — defensible descriptions of the independent Kushite kingdom. Moreover Herodotus knows of something *beyond* Meroe, namely the settlement of Egyptian mercenary deserters near Atbara. It is certain from the distances involved that Herodotus' Meroe is *the* Meroe, above the fifth cataract, and not a reference to Napata. (iv) In his account of pharaonic history Herodotus shows knowledge of the Ethiopian (XXVth) dynasty. These Ethiopians cannot be the unwarlike, unimperialist Makrobioi.

c) *Later greco-roman sources* variously assert that Cambyses (a) conquered Ethiopia and introduced the *persea* thence into Egypt (Diod. I 34, 7 — comparison with Strabo XVII 2,2 shows that Meroitic Ethiopia is in question); (b) founded or renamed Meroe (Diod. I 33,1; Strabo XVII 1,5; Joseph. *AJ* II 249; Ampel. 13); (c) was the eponym of Cambysis Forum *alias* Kambyssou Tamieion in the area south of the third cataract, W or NW of Napata (Pliny VI 81; Ptolem. IV 7). Describing Roman military activities in Nubia, Strabo also mentions in passing that Cambyses' army was overwhelmed by a sandstorm between Pselchis (Dakkeh) and Premnis (Halfa, by the second cataract); he does not say where Cambyses was going at the time.

d) *Kushite archaeology and texts* have nothing to say directly about Cambyses but do bear upon the location of the Kushite capital, which certainly moved from Napata to Meroe. Briefly, Kushite kings were buried at Napata until the late 4th century, but certainly lived at Meroe by the later fifth century and are attested there from the first quarter of the 6th century. This is consistent with Herodotus' declaration that Meroe was an Ethiopian capital by the mid fifth century, but leaves the situation in 525-522 unclear. It is widely believed that the move had occurred before then as a consequence (if not solely and immediately) of Psammetichus II's Nubian campaign (see below). A variant is to locate the move very close to Cambyses' time, thus supposedly explaining his association with the founding/naming of Meroe, while denying that the Persian king actually went that far upstream. Inciden-

tally my brief forays into the modern literature on Kush have not discovered any comment on the fact that a modern settlement near Meroe is called Kabushiya.

Given the general nature of the Cambyses tradition it is unexpected that any source should credit him with successes in Nubia. That both Herodotus and the later tradition should do so is even more striking. There must be some truth in it. The only question is the degree of Cambyses' success and the identification of the region involved.

One hellenistic tradition certainly took him to Meroe. What Herodotus thought is less clear, since the geography of his accounts of Cambyses is different from his geography of the upper Nile. The common assumption is that we can relate them by equating the Makrobioi with Meroe. This is plainly doubtful. What is said about the customs of Meroe (II 29) matches the traditions of the Napata/Meroe kingdom, so far as it goes, whereas the description of the Makrobioi does not, especially when the Table of the Sun identification is rejected, and has obvious utopian elements. Moreover Meroe is on the Nile and Herodotus knows of the Asmach further south, whereas the Makrobioi live on the sea at the edge of the earth. Of course, the very fact that the Cambyses-logos and the Nile-logos represent quite different sources means that the Makrobioi may have been (unknown to Herodotus) somebody's fantasy version of Meroe (that the Table of the Sun cannot be actually identified there does not disprove this). In that case the expedition presumably got no further than the cross-desert route south of the second cataract (though, as a matter of fact, that would only be one fifth of the way to Meroe for someone following the Nile bank the whole way) and Cambyses' achievement was simply to gain a hold on lower Nubia. If we deny the Meroe-Makrobioi equation, on the other hand, Cambyses may have got much further. Those who made up the Makrobioi tale were responding to an actual expedition which went deep into the Sudan by declaring that the madman Cambyses was trying to reach the ends of the earth. The estimate that he only completed one fifth of the journey could be based upon routes going beyond Meroe to the southern end of the Red Sea (e.g. Meroe - Axum - Adulis) — in other words, the land route to the fabulous realm of Punt — and would imply that Cambyses may have got as far as Napata. Lloyd 1975, 55 n. 7 observes that Nubia and Punt were always closely connected in the Egyptian mind, even though attested expeditions to Punt are always maritime. I am inclined to accept this second thesis, and to assume further that later association of Cambyses and Meroe reflects a natural third century interpretation of traditions about Cambyses and the Ethiopian capital, traditions that originally referred to Napata. In this case the desert where Cambyses turned back will be the Bayuda on the direct route from Napata to Meroe.

Operations in Nubia have obvious special resonances in terms of Saite

tradition. That dynasty was created by Psammetichus I with the overthrow of the Kushite dynasty (XXV), and Psammetichus II executed a pre-emptive strike against the Nubian king in the form of a major invasion of Kush in 591 BC, sacking Napata and pushing on to the limits of New Kingdom domination in Nubia.<sup>14</sup> This is presumably the Psammetichus who employed Ichthyophagi to explore the sources of the Nile (Clearch. F98 W). The Ethiopian associations of other Saite monarchs are slighter. A fragmentary text of Necho's reign from Elephantine (Müller 1975, 83f.) conceivably refers to shipborne military activity up the Nile. Some mercenaries tried to desert to Nubia from Elephantine in Apries' reign but were foiled (Schäfer 1904). A text of 529/8 seems to refer to (small-scale) military operations south of Elephantine (Erichsen 1941). Plutarch (*Moralia* 151Bf, 152Ef) preserves (? or invents) a wisdom contest between Amasis and the Ethiopian king for the prize of Elephantine. Certainly there was nothing on the scale of Psammetichus II's invasion, and the practical limit of the Saite realm was at best the second cataract. If the argument above is correct, then Cambyses sought to emulate or surpass Psammetichus; and although his troops cannot be proved to have reached as far as Psammetichus' and the practical border of Persian Egypt was certainly Elephantine, the extortion of regular gifts from Kush would give Cambyses the right to claim that he had not failed in this ambition. Moreover, given the development of the Ethiopians into 'the' enemy in late period folklore (even in New Kingdom times Nubians are said to be discernibly the objects of Egyptian racial prejudice), the mounting of operations against them should have spoken to the sentiments of the time.

No doubt there were also material motives for both the Siwah and the Nubian enterprises — in fact the same material motives that attracted the Saites and earlier pharaohs (particularly the gold of Kush, most copious in lower Nubia). But it would be wrong to ignore the importance of tradition, and it may be proper to make it the decisive factor.

(iii) *Darius and Egypt*. I start with conditions in 521 and Darius' supposed Egyptian trip of 518-7.

(a) Egypt was one of the lands which rebelled against Darius while he was in Babylon in winter 522/1 (DB §21); it is also the only one of which there is not the slightest trace in the later narrative (by contrast, Assyria may relate to events in Armenia, one battle of which happened in Assyria (Izala); Sattagydia can be linked with Arachosia; and Saka may correspond to the war with Skunkha). It seems to follow that the trouble in Egypt was not successfully put down within the 'one year' of DB; and if it were to be put down by Darius in person, there is no room for this until the fourth year of

<sup>14</sup> Hdt. II 161; Arkell 1961, 145f; Bakry 1967; Desanges 1978, 221; Habachi 1974; Lloyd 1983, 346; 1988b, 167; Shinnie 1967, 32f; 1978, 217.

his reign (since years 2 and 3 are preoccupied with Elam and the Saka). I cannot believe that a rebellion *in Egypt* which lasted from 522/1 to at least 518 and which had to be suppressed by the king in person would be completely unknown to the Herodotean tradition, so the former hypothesis seems preferable. If Petubastis III belongs in this context (Yoyotte 1972b), the insurrection started early in 521 (since April is in his first regnal year) and was not suppressed (presumably by Aryandes) until at least 520.

(b) Darius had no Horus- or throne-name when the epitaph for the dead Apis bull of year 4 was composed (the cartouche which should have contained the former is present but left empty), though by the time of the Hibis temple inscriptions such names had been created (only the throne name, Setut-Re, actually survives). There had therefore been no Egyptian coronation of Darius by November 518 and we may assume that he had not arrived on his first visit to Egypt by that date or that, if he had, he did so too soon before November 518 for a coronation to have occurred in the interim.

(c) In his fourth year Darius *sent* an order for the codification of Egyptian laws to his satrap (Spiegelberg 1914, 30f.). There is no hint that he was simply ordering the satrap to assemble interested parties in readiness for his imminent arrival; indeed the source does not appear to recognize *any* visit specifically connected with the project, though we might choose to infer one from Diodorus' somewhat garbled account (see below). At least one other traditionalist instruction was issued by the king outside Egypt, viz. the despatch of Udjahorresnet from Elam to Egypt to restore the House of Life at Sais. It is tempting to associate the two both substantially and chronologically.<sup>15</sup> Both may presuppose an era of trouble calling for gestures of a new approach to rule over Egypt; but both also presuppose that no major rebellion was in progress. This will fit the conclusion in (a) above, and provide a *terminus ante quem* for the end of Petubastis' revolt.

(d) Polyaeus VII 11,7 has Darius visit Egypt during the mourning for an Apis bull and while the Egyptians are protesting against Aryandes' *omotes*. He quells the unrest by offering a reward for the discovery of the new Apis. This tale could fit into the context of August-November 518 BC; in fact, thus placed, it would have Darius arrive in Egypt at the earliest moment allowed by the other evidence. And what the *Demotic Chronicle* verso says about the law code probably need not preclude a visit decided upon after the letter to Aryandes, perhaps in the light of a worsening situation in the satrapy. Nonetheless the association, though neat, is not mandatory. I do not see much value (*pace* Cook 1983) in simply denying the historicity of Polyaeus' story, but I do think one should recognize the possibility of a visit associated with the death of a different Apis bull from that of 518. It is possible that we

<sup>15</sup> cf. Reich 1933; Posener 1936, 175; Blenkinsopp 1987, 412.

have evidence for the induction of a new Apis on 13/viii/Darius 17 (July 505 B.C.) implying the death of a previous one in 506 (cf. Apis statuette text in Schott 1967, 87 ff.). The only other rational dating for this evidence would be Xerxes year 17, and Schott's preference for this date turns on identification of *P3 srs*, father of the statuette's dedicator, with the Persian Attiyawahy; the conclusion is chronologically fair (though not absolutely inevitable), but the identification is clearly not certain. What is virtually certain is that, whatever the date of Schott's statuette, there was a new Apis between year 4 and year 34 (when a bull died on 2/iv) — for this 30 year gap is substantially longer than the highest attested age for an Apis (26 years).<sup>16</sup> Since Aryandes may have survived into the 490s (Pherendates is not attested until March 492 BC), it is entirely possible that Polyaeus refers to a visit well after the 4th year, a visit which could be the context for Aryandes' execution (post-513); for Polyaeus is silent about Aryandes' fate since it is not pertinent to the success of the stratagem and does not preclude his execution (he would be implying a different, though doubtless consistent, version of why Aryandes was executed from that in Herodotus), and Herodotus neither requires nor disproves Darius' presence in Egypt when Aryandes met his end.<sup>17</sup>

(e) One further Greek text bears upon Darius' visit(s) to Egypt. Aristotle *Rhetoric* 1393B imagines an orator, intent on Persian attempts to reconquer Egypt, making the point that both Darius and Xerxes attacked Greece after they had gained possession of Egypt, Xerxes campaigned in person against Egypt in the mid 480s (Hdt. VII 1). What about Darius? Is Aristotle referring to the rebellion of DB §21? Or to the Polyaeus context? Does he necessarily imply that Darius was in Egypt in person? And what, for the purposes of argument, counted as Darius' attack on Greece — 492 and 490 BC, or the first incursions in European Greece in c.513? The uncertainties are, I fear, too numerous to make the text any help.

In sum, greco-roman sources need not be taken to provide evidence for more than one visit by Darius to Egypt (neither Diodorus I 95,4-5 nor Herodotus II 110 = Diodorus I 58 (post Scythian expedition) need be a separate occasion, if they are occasions at all) and the date of that one visit is uncertain. We have already seen that it is a moot point whether the non-Greek sources *require* a visit in connection with the canal. But, for the sake of argument, one could propose a scenario in which Darius issued an order for the canal in his fourth year and eventually visited Egypt when it was completed, this visit coinciding with the death of the 'missing' Apis and the execution of Aryandes. I do not think that such a scenario can be said to be inconsistent with any of the evidence. One might even speculate that Aryan-

<sup>16</sup> Malinine, Posener, Vercoutter 1968, 21f; Kitchen 1983, 62; 1986, 156, 489, 548f.

<sup>17</sup> On the numismatic component to this story see Tuplin 1989.

des' *omotes* had something to do with labour-requisitions in connection with the canal and that, even if Darius' motives for building the canal and his general attitude to Egypt were laudable, the project never shook off unpleasant associations, thus accounting for apparent later hostility to it (cf. above p. 238)

I turn to the general nature of Darius' rule in Egypt. This is not wholly without what might be regarded by an Egyptian as negative aspects. We have already observed that the royal statue intended for Heliopolis would have made a clear visual statement of foreign rule, one comparable with the associated cuneiform texts. The story of the Ptah priest's resistance to a statue in Memphis (Hdt. II 110, Diod I 58) reflects this no doubt; I wish one could be sure whether Darius really acquiesced in their resistance. At a more modest level one might also compare the encouragement given to pro-Persian Egyptian dignitaries to signal their attitude and status by wearing Achaemenid jewelry,<sup>18</sup> though not (it now seems) specifically Persian clothing (cf. Bresciani 1967; Leahy 1984). Again, despite the presumed repeal of Cambyses' provisions, private temple donations dried up after dynasty XXVI, suggesting some lasting interference in temple finances; and the office of Divine Worshipper of Amun at Thebes — one of some significance in the history of the Saite dynasty — also disappears for good with the coming of the Persians, the casualty perhaps of a decline in the real significance of Thebes so pronounced that even the nationalistic kings of the 4th century never saw fit to revive the post. Darius did not aim to return everything to its pre-525 *status quo*.

Nonetheless Darius generally played the role of the traditional pharaoh. Note: (i) building activities in the Khargeh oasis, El Kab and Busiris;<sup>19</sup> (ii) restoration of House of Life at Sais (Posener 1936, no. 1 *ad fin.*); (iii) benefactions at Edfu in years 15 and 18;<sup>20</sup> (iv) offer of reward for location of new Apis (Polyaeus VII 11,7) and (after the precedent of Amasis and Cambyses) of a stone sarcophagus for the dead Apis of year 4 (Posener 1936, no. 5).

The codification of Egyptian law up to Amasis' final year has already been mentioned. Diodorus puts this in a context of Darius' general respect for Egyptian tradition (see further below). One may feel that the stele of a private individual worshipping Darius as Horus (Burchardt 1911, no. 2) is valuable testimony for his being favourably viewed; and the signs which appear later of the Egyptianization of some Persian officials could be construed as indirect

<sup>18</sup> Ptah-Hotep (Cooney 1954; Bothmer 1960, no.64), Udjahorresnet (Botti & Romanelli 1951, 33), anonymous (Amandry 1958, 16).

<sup>19</sup> Khargeh: Winlock 1941 (Hibis temple), Lloyd 1983, 294 (Kasr el Ghouaida); el Kab; Posener 1936, 179 n.3; Clarke 1922, 27; Meulenaere 1975; Porter & Moss 1927/51, V 173. Busiris, Naville 1887, 27f.; Beckerath 1975; Porter & Moss 1927/51, IV 44.

<sup>20</sup> Chassinat 1932, 219, 248; Olmstead 1949; and cf. Meeks 1972, 153; 1977, 653 for general royal donations.



evidence for the attitude encouraged as early as Darius' time. If there *had* been an orchestrated damnation of Amasis, it cannot be proved to have lasted: the law of Amasis' reign went into the law-code; Udjahorresnet does not feel restrained from mentioning his high office during that reign (Posener 1936, no. 1, lff.); private individuals bearing Amasis' name sometimes had it put in a royal cartouche<sup>21</sup> and Saite names in general become popular (even that of Necho, avoided in late XXVIth dynasty: cf. Donadoni 1983, 39, Bothmer 1960, 81); the cult of Amasis' statue is attested in the mid fifth century (Berlin 14765 = Erman 1900, 119 = Bosse 1936, no. 91). Some of these phenomena have, of course, been cited as signs of the Egyptian spirit of resistance, but I do not see how we can be sure that they are not rather signs of a continuity favoured by the imperial power. Both views may, of course, have been held by different people.

Atkinson 1956 claimed that Darius departed from Cambyses' precedent in regnal year calculation, abandoning pure Egyptian practice for an Egypto-Persian hybrid. But the argument is unconvincing. It arises as an explanation of the apparently coexistent Apis bulls between 29/v/Cambyses 5 and I/ix/Cambyses 6; but the claim that the problem is caused by the two dates being based on different ways of counting regnal years fails because the arithmetic does not add up; we are still left with one bull being born on 22 August 525 and the other not dying until 31 August 525. Moreover the age of the bull who was born on 29/v/Cambyses 5 and died in Darius' fourth year (8 year, 3 months, 5 days) makes sense of the terminal dates without any change of counting method (the 8 years are Camb. 6-8, Darius Acc.-4). The departure from tradition therefore started with Cambyses, whose arrival as new pharaoh of Egypt when already in his (nominal) fifth year as king of Persia caused the problem. The solution reached in the course of 525 was to count his 5th year in Egypt from the next New Year's Day after Amasis' death, i.e. c. January 1, 525 BC. That kept the regnal years numerically the same in Egypt and Persia for as much of the year as possible, without abandoning Thoth 1 for Nisan 1 as New Year's Day. It was the maximum concession to Egyptian tradition consistent with convenience, and Darius maintained it.

Not that Darius strove to defend the memory of Cambyses. He had his own reasons for denigrating him (consider the whole dubitable Gaumata tale, told in DB in a version actually more improbable than Herodotus' one, and the overtones of criticism in 'Cambyses died his own death'). So far as Egypt is concerned, the codification of law overturned much of Cambyses' model for the financial administration of the satrapy. It is interesting to note the

<sup>21</sup> Bothmer 1960, no.57 = Brooklyn 59.77; Athens 107; Brooklyn 16.580.150; Cairo JE 43240; Posener 1936, 13 etc.

form in which the law-code turns up in Diodorus, Diodorus knows of Darius as the sixth *nomothetes* of Egypt, but he does not actually assert that he wrote any laws; the burden of his account is rather that Darius rejected Cambyses' *paranomia* towards sanctuaries with Egyptian priests, studied *theologia* and the deeds in the sacred books, and endeavoured to live a moderate and god-loving life. It is thus clear what sort of interpretation was put upon Darius' *nomothesia*, and Darius himself may have made it explicit. It is certainly apparent from the slightly sharp tone in parts of Udjahorresnet's inscription (written in Darius' reign) and from AP 30 (written a century later) that criticism of Cambyses was not disallowed. For those who see significance in the omission of 'pharaoh' in P. Rylands IX (above p. 269), the same conclusion will follow there too. I also wonder whether the cuneiform canal and statue texts do not reflect an inclination to claim that Darius was the 'real' conqueror of Egypt — Cambyses being simply forgotten. The brief native interlude of Petubastis III would help to justify this, irrespective of whether Darius acted in person to terminate it. It is certainly to be noted that, whereas Cambyses failed to conquer Siwah, somebody did so later on (to judge from Dinon 690 F23), and this might have been Darius. It was certainly under Darius that Kush and Libya began belatedly to appear in the Lists of Peoples. Even if this signifies no substantial change in their status, the change may at least cast light upon Darius' attitude to his African possessions; the successful establishment of a claim over areas that had already required action from the Saites (Psammetichus I fought the Libyans in the western delta, and Apries invaded Libya to attack Cyrene; on Kush see above) was something to be advertised not simply subsumed under 'Egypt'. In this context it is perhaps of interest that Darius apparently adopted the same Horus-name, *Mnh-jb*, as Psammetichus II, the Saite invader of Nubia (Beckert 1984, 112, 113; Godron 1986, 294). Of course, if Herodotus is to be believed, the degree of control over Libyans outside the western delta was not great. Notice incidentally that Petubastis III had taken a royal name of the XXIIInd (Libyan) dynasty. Moreover, whereas we do not know whether Amasis' marriage connection and military alliance with Cyrene<sup>22</sup> connoted Egyptian domination and do not have to assume that it did, Cambyses certainly claimed suzerainty and tribute; and Darius maintained this claim. Herodotus' failure to make this explicit merely reflects the evasiveness of his Cyrenean sources.

When Darius tried to put his statue in the Memphite temple of Ptah, Herodotus was told, the priests dissuaded him by pointing out that he had not (yet) equalled the achievements of Sesostris. True or not, this story (and perhaps much of the development of the Sesostris myth) reveals the attitude

<sup>22</sup> Hdt II 181 (*philotes te kai summakhia*); Edel 1978; Lloyd 1988b, 178; Leahy 1988, 192.

of mind an Achaemenid had to contend with and attempt to exploit. Against this whole background, it will be clear that to embark upon the attempt to repeat or improve upon Necho's plan for a Nile-Suez canal — and to do so at least in part precisely *because* Necho had done it, and done it with ideas of military conquest in mind — was absolutely in line with certain general tendencies of Achaemenid rule in Egypt. It would be even more perfectly so if we accepted Pliny's report of a place in the canal zone called Cambysu (VI 165) and deduced that the conqueror had already thought of such a thing. I am tempted to say that in those circumstances Darius would have had no choice at all but to follow suit. Perhaps this is to gild the lily, and Pliny does describe Cambysu simply as a settlement of wounded soldiers. All the same I am convinced that the attractions of being seen to follow precedent deserve at least as much stress in the explanation of the canal venture as the considerations of commercial advantage or integration of the empire's communication systems which bulk incomparably larger in most modern statements on the topic. It remains to inspect these considerations in their own right — with, perhaps, a prejudice in favour of versions which privilege Egyptian precedent.

## 2. Sea Communications

The point of a Nile-Suez canal is to facilitate contact by ship between (a) the Mediterranean or the Nile Valley (especially Lower Egypt) or both and (b) the Red Sea and points east — more specifically, one or more of the northeast African coast, (south) Arabia, the Persian gulf and India. There is no serious reason to believe in regular direct communication between Egypt and India at this date (i.e. contact in which individual ships made the whole trip). As we have seen (above p. 242), Scylax' trip need not be seen as forging a specific practical link between the canal and India. He was rather engaged in determining the relationship between two extremities of the empire and (implicitly) the relationship of each of them to the centre (i.e. the Persian gulf). The fact (if it is a fact) that he did not sail around the gulf itself does not contradict this last point. The Persians will have been aware of the existence of maritime communications between the gulf and 'India'; what Darius wanted to know was (i) how exactly this 'India' related to Gandara (which he already knew as a subject land) and to the lower reaches of the Indus (which he was thinking of conquering) and (ii) how the seaways which linked him to the lower Indus stood in relation to the other end of his empire. To put it in the terminology of the cuneiform canal texts, he wished to know how each of the Indus and the Nile (the only two rivers with crocodiles, it was said) stood in relation to 'the sea which goes to Persia'. Having discovered that the Indus flowed into it but the Nile did not, he may very well have been incited by that very lack of symmetry to emulate Necho and set

nature to rights. On the other hand he may have decided upon the canal before Scylax' voyage. In any case it does not follow that he was interested in regular contact between the Nile and the Indus, and the fact that Herodotus declares, with tantalizing vagueness, that after Scylax' trip Darius 'made use of the southern sea' does not prove that he was.<sup>23</sup> So far as the canal is concerned, therefore, our investigation need only touch upon northeast Africa, southern Arabia as far as the incense-bearing regions and the Persian gulf. The first two were, of course, in 'local' contact with India and Indian commodities such as cinnamon (Hdt. III 111, Theophr. *HP* IX 4, 2, Strabo XVI 4, 4; 4, 14; 4, 19; Diod. III 46, 3) were available there along with native products for movement on to the west. What we would like to know is how much actual or feasible movement there was between Egypt and any of these destinations of a sort which would be made more convenient or advantageous for Persia or Egypt by the construction of a canal.

Classical sources knew something of sailing in the Red Sea. Herodotus II 11 gave its dimensions in sailing times (40 days end to end, half a day across), Hecataeus 1 F271 mentioned Kamaran Island and Ephorus 70 F172 the *Columnae Insulae* in the Bab al Mandab near Aden. References to Eudaimon Arabia in Euripides *Bacchae* 16f., Aristophanes *Birds* 144 and Ctesias 688 F71 (of doubtful authenticity according to Jacoby) are, on the other hand, rather vague. All of this *could* simply reflect information from Scylax, rather than awareness of regular traffic in the Red Sea. Herodotus betrays no idea in III 107f. that incense-bearing Arabia was approachable by sea. On the contrary his apparent confusion in II 8 of the Arabian mountains east of the Nile valley with the mountain-ranges adjacent to the incense route through Saudi Arabia and Yemen shows that his informants led him to believe that it was reached by land.

All discussion of Red Sea traffic has to bear in mind the alleged difficulties of sailing in the region. Put simply, north of the Jeddah-Suak in line the prevailing wind is almost always north-northwest, apparently severely inhibiting northward journeys. On the other hand, various periods of antiquity saw a good deal of maritime communication between the Nile valley and the east via ports on the African coast *north* of this line (e.g. Koseir). There may even be specifically Achaemenid evidence in the shape of rock inscriptions in the Wadi Hammamat naming Persian officials and a cartouche of Darius (Po-

<sup>23</sup> Salles 1988, 83f.; 85f. after some hesitation concludes that Scylax actually only sailed from India to the Persian Gulf; the idea that he did the whole trip, India-S.Arabia-Suez was a Greek misapprehension caused by confusing Scylax' voyage with separate, new information about the Red Sea which reached the Greek world in the same era. But this does not deal with Darius' own statement about ships going from the canal to Persia (as Salles 86 n. 33 admits) and the argument places far too much weight upon the lack of attested references to anywhere between Oman and Aden in e.g. the fragments of Hecataeus — a silence from which it is scarcely safe to infer anything.

sener 1936, 35) on another route to the sea. I am less sure about the view expressed from time to time (cf. recently Katzenstein 1989, 80) that we can infer regular use of these routes and of the onward maritime connection with the Persian gulf from the alleged immunity of Achaemenid Upper Egypt from the effects of revolts in the Delta. So the pattern of winds is not the end of the matter. On the other hand again, the fact that Koseir and the like were of significance even in the Ptolemaic period, when there was a Nile-Suez canal, may show that it was sailing in the gulf of Suez which was the real problem.

So far as northeast Africa is concerned, one naturally thinks of Punt as a possible destination for shipping from Egypt (cf. Kitchen 1971). The bulk of the evidence for this is, of course, much earlier (early 15th — early 12th centuries). But the Daphnae stele speaks of a miraculous rain storm in the mountains of Punt causing the Nile to rise and (somehow) saving certain soldiers of Psammetichus I (Lloyd 1975, 54-5), which does not *prove* a Saite expedition there but is at least a welcome sign of continued knowledge of the place, and some Egyptologists are inclined to think of Punt as the most likely background for Necho's interest in access to the Red Sea (cf. Lloyd 1977; O'Connor 1982, 901). I have already argued that the tale of Cambyses and the Makrobioi provides additional indirect confirmation for Late Period interest in the region. Of course, it was not necessary to have a Suez canal to mount maritime expeditions to Punt; there is absolutely no reason to postulate one in the great days of such expeditions.

If any attempts *were* made in the Late Period to get at Punt, it seems likely that what the Egyptians would have found there were, among other things, Sabaeen colonies (cf. Drewes 1987), and that, in default of direct access, the produce (especially the incense) of Punt would have been travelling to the Levant and Mesopotamia via the Arabian peninsula and in the hands of the same people who exploited the trade in south Arabian incense — and presumably largely by the same routes. How might the construction of a canal into lower Egypt affect *this* trade?

Everyone agrees that in pre-hellenistic (indeed pre-Roman) times the incense of south Arabia went north, whether to Egypt,<sup>24</sup> south Palestine, Syria or Mesopotamia, predominantly by land routes. Mesopotamia could be approached either entirely by land, or by land to Gerrha and thence up the gulf. So far as Egypt/Palestine is concerned, of course, it is strictly speaking only the evidence from points south of Eilat which proves use of land-routes. Anything to the north, whether scraps of south Arabian script (Graf 1983, 558; Shiloh 1987) or references to Minaeans travelling in Edom, Syria and

<sup>24</sup> Interesting indirect signs of Achaemenid period Egyptian connections with an Arab town on the land route, viz. Tayma, are afforded by (a) the priest ŠLM ŠZB, son of Petosiris in KAI 228, and (b) the images of the Apis bull on an altar from the site (Dalley 1986, 85f). On the incense trade in general see recently Müller 1987.

Egypt (Graf 1983, 563) or evidence for the great wealth of Gaza or the Nabataeans of Petra *could* be in part due to material coming up the Red Sea and the gulf of Aqaba. And although south Arabians at Tell Kheleifeh (near Eilat) might have been on their way by land to Egypt by the later pilgrim route to Mecca (they would hardly be going to Gaza, for which Eilat would be an unnecessary diversion from the Tabuk-Ma'an-Gaza line), it is perhaps most economical to regard them as shipborne (Graf 1990, 137). The reverse phenomenon, northerners making for south Arabia by sea via the gulf of Aqaba, is certainly attested for the time of Solomon and Jehoshaphat (I Kings IX 16, 28; X 11, 22; XXII 49; II Chron. XXVI 2; Jos. *AJ* VIII 163; Delbrück 1955-6, 11) and (implicitly) later on by the mere existence of the Tell Kheleifeh site (not to mention the doubtless phoney Phoenician mariners of the Paraiba inscription (Gordon 1968, 1972; Cross 1968, 1979), who supposedly ended up in Brazil). All the same it is difficult to know how much weight to give to this evidence; one cannot help feeling that Herodotus' ignorance of maritime contact with south Arabia (above) and the overwhelming stress on land routes even in Strabo's sources preclude any significant volume of seaborne trade. The fact that the Sabaeans evidently operated across the Red Sea proves nothing, naturally. It is unlikely, therefore, that we ought to see the opening of a Suez canals as a means of facilitating south Arabian traders' access to Egypt.

The opposite possibility, that it signals a desire on the part of the Achaemenids for improved and more substantial direct access by Egyptian traders to south Arabia, should also be examined with scepticism. Such a policy has, of course, sometimes been attributed to the Ptolemies, who are thought to have wished to cut the Nabataeans out of the profits of south Arabian trade, diverting the latter to Egypt. But the Ptolemaic kings and the Achaemenids were in quite different positions, the former seeking to increase the prosperity of an independent Egyptian kingdom, the latter to maintain an empire which included both Egypt and the whole near east. It was important for Darius and his successors to maintain reasonably good relations with the Arab populations of the Palestine/Egypt borders, for the sake of secure access to Egypt. It is true that the absolute dependence on Arab goodwill stressed by Herodotus in connection with the initial conquest of Egypt (for which there are close parallels in neo-Assyrian contexts)<sup>25</sup> may have been slightly tempered by establishment of Persian administrative and military control over the highway into Egypt (Hdt. III 7 suggests as much, speaking of the maintenance of water-supply as a responsibility devolving upon authorities in Memphis; Na'aman 1979, 79f. suggests that the frontier of Egypt and Palestine was actually shifted in this context). All the same it remained a very

<sup>25</sup> cf. Hdt. II 141; ANET 282, 286, 292f; Na'aman 1978, 85; Eph'al 1982, 138.

exposed imperial artery, and the presence of Qedarite Arabs at Tell Mas-khutah in the late fifth century, whether as Persian-employed garrison troops or not, illustrates this very clearly. To interpret the Suez canal as a determined effort to alter trading patterns between Egypt and south Arabia is to attribute to Darius a politically and strategically dangerous policy. It would also be one that had knock-on effects further south. The dominant groups of south Arabia, especially Sabaeans and Minaeans, were themselves middle-men in the incense trade, since the actual incense originated much further east. Are we to suppose that Darius had in mind to cut them out as well? That would have required a major investment of effort to ensure that trading ships from Egypt could pass back and forth to the incense coast itself despite the waterless and inhospitable shores to the north east of Aden (the point at which Anaxicrates' exploratory mission in 324 BC came to a stop)<sup>26</sup> and to provide them with ports-of-call on the northeast African coast — where, of course, there were also Sabaeans. I cannot see that any of this was worth the trouble, so long as the northwestern Arabs were prepared to go on allowing the Persian king to take his cut in the form of large 'gifts' as described by Herodotus. Could he really have expected to make a great deal more in the form of additional tribute revenue from an Egyptian satrapy rendered more prosperous by increased incense trade? In practical terms, the traders on the incense route had nowhere really profitable to take their wares where the Achaemenids were not well placed to benefit indirectly anyway. Why upset a system satisfactory to all parties? It should be noted, in addition, that even the Ptolemies' decided interest in 'developing' the Red Sea does not seem to have succeeded in wrenching the incense trade away from the land route (Rostovtzeff 1941, I, 386f.; Fraser 1972, I, 177, 180) and was anyway much more concerned with getting at elephants in northeast Africa (van 't Dak & Hauben 1978, 64) — the Hellenistic equivalent, one might say, to the second millennium's obsession with the wealth of Punt. So long as we are considering the canal in relation to the Red Sea/Arabia sector, therefore, I doubt whether the invocation of *trade* alone is very helpful.

An intention to wage war would actually seem to answer the circumstances better. The Achaemenids did not, of course, conquer Arabia. The evidence of Lists of Peoples and of Herodotus does not *require* us to assume that they claimed a hold over any Arabs but those of the Palestine/Egypt marches. The deduction in Graf (1990) from *hgr* in the hieroglyphic lists that the Arabs of Persian Royal Inscriptions belong in the Persian gulf is not compelling. The attested 'governors' of Tayma and of Dedan are not signs of Achaemenid administration. And there must surely be some other explanation of the 'land

<sup>26</sup> Strabo XVI 4,4; Arr. *Ind.* 43,7; Theophr. *HP* IV 7,2; IX 4,1f; Plin, *NH* XII 62. Cf. Arr. *Ind.* 32,7, and the claim in 4,2 that, if *anybody* had done the Gulf-Egypt trip, they had done it *pelagios*.

of Persia' in Ptolemy II's Pithom stele than the one proposed by Tarn and apparently countenanced by Fraser (1972 II, 301), that the coast of the Hejaz had once been under Persian rule (cf. Lorton 1971). But it is not, I suppose, absolutely out of the question that Darius gave some consideration to the military subjugation of Arabia — a sort of maritime equivalent of Nabonidus' attempt to squeeze the incense trade dry by occupying the oases of northeast Arabia — until (perhaps) receipt of Scylax' report and a realization of the difficulties involved or the appearance of distractions in the Aegean world put him off the idea. The chronological implications of these two scenarios are, of course, different; but neither is precluded by the direct evidence. Alternatively one could think in terms of expeditions to Punt in the time-honoured tradition — military in form (both for reasons of prestige and to afford protection from pirates), but not necessarily aimed at outright conquest and occurring only occasionally (hence the canal, to allow for temporary transfer of warships?). That such quasi-military enterprises were still familiar in Achaemenid Egypt is suggested, as Lloyd has noted, by the prominence of the military man, Khnemibre, in the Wadi Hammamat quarries; and in the case of Punt it would have the merit of avoiding the danger of upsetting the Sabaeans and threatening stability in Arabia.

The Persians were hardly ignorant of the existence of the Persian Gulf or totally disinterested in its use, especially with a capital at Susa — indeed the privileged position of the old Elamite centre positively argues an Achaemenid interest in the gulf, for all that some Alexander sources claim that there was no proper river connection between Susa and the sea. There are other indications: the road from Persepolis to the gulf was in use, to judge from the early palace at Borazjan, the palace at Taoke (Arr. *Ind.* 39, 5), the gardens of Bushehir (*ibid.* 39, 2) and its canals (Salles 1990), and (perhaps) the settlement of Greeks at Ionaka (Ptolem. VI 4,2; Schiwiek 1962, 80). At the head of the gulf deported Milesians were settled at Ampe (Hdt. VI 20), variously identified with Arrian's Aginis (*Ind.* 42, 4), the trading 'village' mentioned in Nearchus 133 F25 and Polyclitus 128 F6, Pliny's Durine (*NH* VI 138) = Alexandria in Susiana and Dur Iakin. That there were Carians too, however, is a thoroughly irresponsible conjecture, based on misuse of both Greek and Akkadian texts; the only Achaemenid period evidence for Carians in Mesopotamia firmly associates them with Nippur (Zadok 1978, 291) and Sittakene (Diod. VII 110). On Failaka the pre-hellenistic sanctuary at Tell Khazneh yields 6th-4th century material comparable with that from southern Mesopotamia and even an Aramaic text containing an Iranian personal name (Salles 1985, 588; 1987, 85). Further south there is Achaemenid period occupation at City IV on Bahrain, while at the other end of the gulf the Persians maintained a claim to Maka (both sides of the straits of Hormuz: Eilers 1983; Potts 1986) in all the Lists of Peoples. There were also the Anaspastoi islands, a

place for exiles (Hdt. III 93, 2; VII 80; Ctesias 688 F14(43)), but also a source of Iranian troops (Hdt. *loc.cit.*) and in 325 governed by a hyparch who could guide Nearchus' ships to the Elamite coast. There is textual evidence for trade in the gulf in the late Achaemenid period (Maketa-'Assyria': *Ind.* 32.1; Gerrha or Arabia to the rivermouths: Nearchus 133 F25, *Arr. Ind.* 41,8, Strabo XVI 3,3), and the wood of Makan mentioned in DSaa will surely have come by sea — though it may have originated in India (cf. Schiwiek 1962, 5, 67f on Babylonian references to Magan wood). More generally there seems no good reason why the history of interest in the gulf in the Neo-Assyrian and Neo-Babylonian periods should *not* have a counterpart in the Achaemenid era. 'The prosperity of the gulf in the neo-Assyrian, neo-Babylonian and Achaemenid periods is plain' (Salles 1987, 89). Yet it must also be admitted that Nearchus' account creates the impression that in 325 the Persian shore was not highly developed south of Bushehir, though the hyparch of Oaracta was able to guide him along that route despite the complaints about lack of native guides, and, if the reason is that the opposite (Arabian) shore had always been the more frequented one, it must be further noticed that (unless Graf's thesis is correct) the Achaemenids never sought to claim control of any part of that shore and the investigations of it ordered by Alexander and carried out by Archias, Androstenes and Hieron 'furent de véritables explorations de terres vierges' (Salles 1988, 88).

So far as contact with the world outside the gulf is concerned, we have a choice between India and the incense coast/Red Sea sector discussed above. It is not easy to prove regular maritime contacts with either.

On the eastern run, Nearchus found a bad patch between the Indus and Mostarna (cf. Schiwiek 1962, 51-60). Yet the close ethnic similarity between Indians and Maka-people evident in Achaemenid iconography must argue for regular contacts. The same inference has been drawn from the unusual juxtaposition of India and Maka in the canal stelae and Susa statue Lists of people, but I am not sure that it is primarily an implicit statement about searoutes, any more than that Libya-Kush-Maka-Caria in DNa = A?P is supposed to reflect a sea-route. Admittedly, the canal stelae/Susa statue lists are also unusual in that Maka-India is immediately preceded by *Egypt*-Libya-Kush, so they may be a special case — though I would be unhappy at the implied conclusion that Egypt-India is a regular route. We have already mentioned putatively Indian wood coming via Makan, and Androstenes arguably saw Indian teak in Bahrein in 324/3 (Theophr. *HP* IV 7,7f., Schiwiek 1962, 68, Delbrück 1955/6, 24). On the other hand we cannot prove that Indian ivory in Susa (DSf, DSz) or Indian peacocks (Eggermont 1984, 74f), dogs (Hdt. I, 192), elephants, tigers and narcotics (Ctesias *ap.* Aelian IV 21; 41; XII 79) and settlers (BE 9, 75; 10, 8) in Babylonia had come by sea. Wood from Gandara and Carmania (DSf, DSz) very probably had not,

Indian tributary gold is actually associated with Caspatyrus in Gandara, and Vogelsang's argument for the existence of a regular India-Fars route via Baluchistan or the sea (1985, 82f.; 1990, 101) is not impregnable. The argument is that non-royal travel authorizations in connection with Arachosia, Barrikana and Gandara are issued by Ziššawiš, Bakabaduš and Bakabana, whereas those for India come from Parnaka, Irdubama and Karkiš. But Ziššawiš, Bakabana and Parnaka are not to be personally located east of Fars; and Karkiš, who might be, is associated with Carmania, which should lie on both of the land routes in question. The amount of material is simply not large enough to sustain any conclusion.

Looking in the other direction we have a similar series of imponderable 'proofs' of maritime contact to set alongside the undoubted voyage of Scylax and of the ships mentioned in the canal stelae, viz. Persians putatively on their way through Wadi Hammamat to the sea and Persia (Posener 1936, 179f), and a Heliopolitan statue, Aswan granite (Ghirshman 1964, 142), Kushite ivory, gold and (?) giraffes (DSf, DSz, Tribute Bearers xxiii, Hdt. III 97) all on their way to Susa or Persepolis. Mazzarino's attempt (1966, 76f) to prove that the Egyptian materials in DSf came by sea is ingenious, but not cogent. Schiwiek 1962, 76 sought to deduce regular Achaemenid contacts on the gulf-incense coast route from the temporary presence of Mithropaustes on Ogyris (= Masirah Island), off southwest Oman (*Arr. Ind.* 37), but it seems to me that Mithropaustes' movements may as well indicate that Masirah was, in terms of official Achaemenid surveillance, rather *off* the beaten track. Of course, they show that *somebody's* ships were sailing south from the straits of Hormuz round the tip of Oman; and the Persians had had more pressing things to worry about during some or all of the time Mithropaustes was in exile. In fact the situation is a good deal worse than with the India-Persian gulf route. The distance between the Maka and the lower Indus (both certainly areas claimed as subject to Persia) was not huge; the areas were ethnically part of the same world; and there is at least *some* evidence for regular trade between the two. Moreover, Herodotus' explicit statement about Darius' regular use of the southern sea is most closely attached to the conquest of India (so that we can legitimately believe the historian's sources to have been principally speaking of communication between Persia and India). To the west things are different. The 'produce of Arabia' being moved within the Persian Gulf in the 320s (above) *might* have arrived there by sea from the incense coast, though it is by no means impossible that it had actually come by land via south Arabia to Gerrha. But for regular trade from the Red Sea/Egypt, as opposed to movement of tribute or gifts (which is also what the ships of the canal stelae are said to have contained), there is certainly no evidence; and one only has to contemplate a map briefly to see why: for any reasonably portable commodity, the Red Sea-Indian Ocean-

Persian gulf trip was an absurdly large detour as a means of getting from Egypt to Mesopotamia, Elam or Fars, and one which the ordinary traders of the period would hardly be likely to make, given the existence of long-established land routes — especially since (it is generally supposed) the unified politico-military control represented by the Achaemenid empire would tend to make those existing routes safer.<sup>27</sup>

Once again, therefore, as with the Red Sea/incense coast sector, to invoke trade and a putative Achaemenid desire to encourage it when considering the Suez canal seems misguided. We thus reach the conclusion that the nearest thing the oriental sources offer, albeit implicitly, to an explanation of the canal — viz. maritime communication with Persia — can only be accepted with strict qualifications. The symbolism of Persian power over Egypt which I suggested was implicit in the cuneiform canal text turns out not to be just a literary fact. And I should be sceptical about much even of what the imperial power requisitioned from Egypt and Kush actually travelling to the coffers of Susa by the arduous route round Arabia.

### 3. *Impulses of imperialism*

There are, of course, more general issues involved in all of this about the reasons which impelled the Achaemenids to maintain and seek to extend their *Weltreich*. Is it after all reasonable *a priori* to imagine that any particular initiative of Persian imperial administration, such as (re)building a Nile-Suez canal, should be seen in terms of a desire to augment long-distance commercial exchange of commodities?

Greek spectators of the empire tended resolutely to see Persian initiatives in terms of 'political' explanations, be they specific ones of self-defence or revenge or opportunism or more general ones along the lines of an inevitable tendency of those who have power to seek more (what Thucydides formulated pithily in the Melian dialogue was not a principle previously unperceived). Moreover, their willingness to see the eventual assault upon mainland Greece as just a step towards the conquest of Europe (which is just one aspect of the 'will to power' thesis) need not be regarded as a simple example of Greek self-esteem (*après nous le déluge* or at least *l'Europe*). Since the Achaemenids, in common with their imperial predecessors, pictured themselves as rulers of the world and did so, according to Herrenschmidt's beguiling thesis (Herrenschmidt 1976, 1977, 1980) by way of a very particular identification of 'earth' and 'empire', it was not particularly unreasonable to attribute ecumenical ambitions to them. People who style themselves world rulers will always be liable to a desire to make the reality correspond as much as possible to the

<sup>27</sup> See Salles 1988 on the difficulty of believing in regular circumnavigation of Arabia at this date.

style. Neo-Assyrian kings, for example, ruled as agents of a god whose principal injunction to them, enshrined in the coronation ceremony and reflected in the formulation of historical texts, was to enlarge the frontiers of the realm, and were always keen to note their conquest of places unknown to their forefathers. New Kingdom pharaohs went so far as to claim rule over peoples explicitly described as too distant actually to know or be known to the Egyptians! The corpus of Old Persian inscriptions lacks, it is true, statements of comparable explicitness. The image of empire is perhaps more static, with a stress on what already exists; the imperial iconography can readily be seen in these terms, the figures of the tomb facade show that 'the spear of a Persian man has gone forth far ... a Persian man has delivered battle far indeed from Peria', and the king's virtues in DNB only explicitly constitute an ideology of aggressive imperialism if 'rebellion' and 'the Lie' can apply to people not already subject to Persia. In this context it is a question of some importance whether the Scythians of DB V who, unlike the Elamites, are not called 'rebellious' but, like the Elamites, are said to be faithless and not worshippers of Ahuramazda, are the Scythian rebels of DB §21 or a group being newly added to the empire. All the same, the inference which Herodotus drew from the steady growth of the empire and put in the mouth of Xerxes, that there was a *nomos* of conquest, is not to be despised, and the interest in exploring beyond the edges of the empire (Scylax, Democedes, Sataspes) is a legitimate sign of Darius' broad horizons. Moreover, even the Assyrians, with their general quasi-religious principle of expansion, normally treat any particular gain as the result of provocation rather than an exercise in aggression for its own sake.

But by the same token, of course, individual imperial conquests and the whole pattern of imperial conquest may also have more mundane and material motives, aside from provocation or a divinely authorized will to power. What sort of room for trade and commerce might there be here?

It is no secret that the Achaemenid empire was (or, at the latest, became with Darius) extremely tribute-oriented. Nor is it any secret that plenty of strands of evidence suggest the king's interest in ensuring and promoting the productivity and prosperity of the various parts of his empire. The same strands of evidence make it quite clear that tribute and prosperity are intimately linked and that the promotion of prosperity is overwhelmingly located in the areas of agriculture and animal husbandry. What is involved is the mentality of the lover of *paradeisoi*, productive as well as ornamental establishments (illuminatingly interpreted by Briant (RTP, 451) as *inter alia* images of how the empire as a whole ought to be), and the owner of landed estates to be worked by someone else's labour. One might also notice the close identification of subjects with the bases of agricultural prosperity which is inherent in the 'earth and water' symbolism associated with acknowledge-

ment of the Great King's power (cf. Kuhrt 1988, though the point is not entirely dependent upon her particular explanation of the act of submission). Whether or not the bureaucracy of Persepolis is typical, the type of economy and wealth its records reveal there is surely what the king will have regarded as basic to the health of his empire in all its parts. Accordingly, the threats to the empire as described in royal inscriptions on occasion include famine. In all this the Persian king was simply like his ancient near eastern predecessors, for all of whom the functions of a ruler included an element of the protection and promotion of the realm's fertility, though his utterances stress the matter less (just as they stress continuous expansion less) — DNb, after all, has nothing to the point. If, moreover, we choose to pay attention to the conviction of classical Greek authors that, in the days of Cyrus, the homeland of the Persians was ill-developed and provided a poor living for a population of *poimenes*, we may see here an impulse to imperialism, not just an aspect of imperial management.<sup>28</sup> Of course, the Greek authors in question did not spell out any such thesis; at best they regard the poverty of old Persia as an explanation for Persian military prowess.

In all of this there is no hint of improving prosperity (even to the end of having more prosperous subjects who could then pay more tribute) by encouraging trade. Even the famous comment about Darius being a *kapelos* seems to be making the point that his concern for an orderly system of assessing and collecting tribute betrayed an attitude of mind apparently out of keeping with traditional aristocratic values (Greek and perhaps Persian). Darius was obsessed with profit, but with profit actually obtained not by *kapeleia* but by the requisition of resources on the basis of military force, even if the military force might be represented as available for the benefit of the subjects and the protection of their capacity to produce what was being requisitioned. I should think that Darius' institution of a Persian coinage system also played a role: compare Herodotus' comment on the Lydians in I 94,1 which shows the association which could be made between coinage and *kapeloi*. It does not, of course, prove that the minting of coinage is in origin to do with promoting trade or that it was in the case of Darius. One could, of course, say that the upgrading of some long-established land routes into well-policed royal roads represented an exercise of military force in the interests of the long-distance trader comparable to its exercise in other contexts in the interests of the tillers of the land. But I should be happier to believe that this was high among the motives if some ancient source had said it first.

Are we just the victims of the taciturnity of the kings themselves and the distressing disinclination of Greek sources in all contexts to talk about trade

<sup>28</sup> See Hdt. I 71; I 89; IX 122; Plato *Laws* 695A, In Eurip. *Bacchae* 13f. the cliché for Persia is 'sun-beaten', not e.g. 'prosperous' or 'fertile' as it might well have been after Alexander. The old theme is still reflected in Arr. *Anab.* V 4,5.

in the serious terms which historians of that subject would like them to use? One does not have to read much relevant modern scholarship to discover that the relation between trade and the imperatives of neo-Assyrian imperialism is the subject of a good deal of attention. Recent claims that Israeli archaeologists have located the 'sealed *karu* of Egypt' which Sargon opened and where he encouraged mutual trade between Egyptian and Assyrian give the matter special topicality. Yet in that particular famous case (encountered again and again in the scholarship, because when it comes down to it there are so few relevant texts to play with) there is surely a large political component. The general character of the neo-Assyrian kings' relationship with the trade patterns of their time is not really the promotion of trade as such at all but the creation of a system designed to cause the influx into Assyria of goods (lovingly listed in many a royal text) which might, in an entirely different set-up, have reached there by trade — if Assyria had had enough of anything that anyone else wanted to make the exercise really profitable. Returning to the Persians, if one forgets about the unhelpfulness of explicit characterizations of imperial motivation in Greek or other sources, and just attends to the phenomena, it does not become much easier to think that trade is an important component. We have seen the difficulties with the Suez canal. One might make the attempt with the Scythian expedition. At least, one explanation of that enterprise (only partially endorsed in the most recent study, Gardiner-Garden 1987, 344) is to claim that Darius acted under the influence of Asiatic Greeks who saw trading advantage to themselves in the prospect of the Black Sea colonies and their native hinterlands being under the political suzerainty of the same empire as they were themselves. There is, of course, not even the slightest indirect hint of such a thing in Herodotus; the way in which the Greek cities of the region are to all intents and purposes ignored is particularly striking. The most that this parallel would justify claiming about the Suez canal is that certain Egyptian advisers put to Darius the material benefits for the satrapy of conquering Arabia or at least mounting Punt expeditions. I have already indicated what Darius is likely to have thought of the first suggestion — but if he did ever entertain it, his principal material motive will not have been benefit to Egypt. The second suggestion remains the most defensible version of the trade thesis (though it is a very special version); but later Egyptian hostility to the canal and the failure to maintain it after 404 argue that the actual material benefits involved were not perceived as great and that, if a return to the practices of the New Kingdom was in view, symbolic considerations were probably predominant.

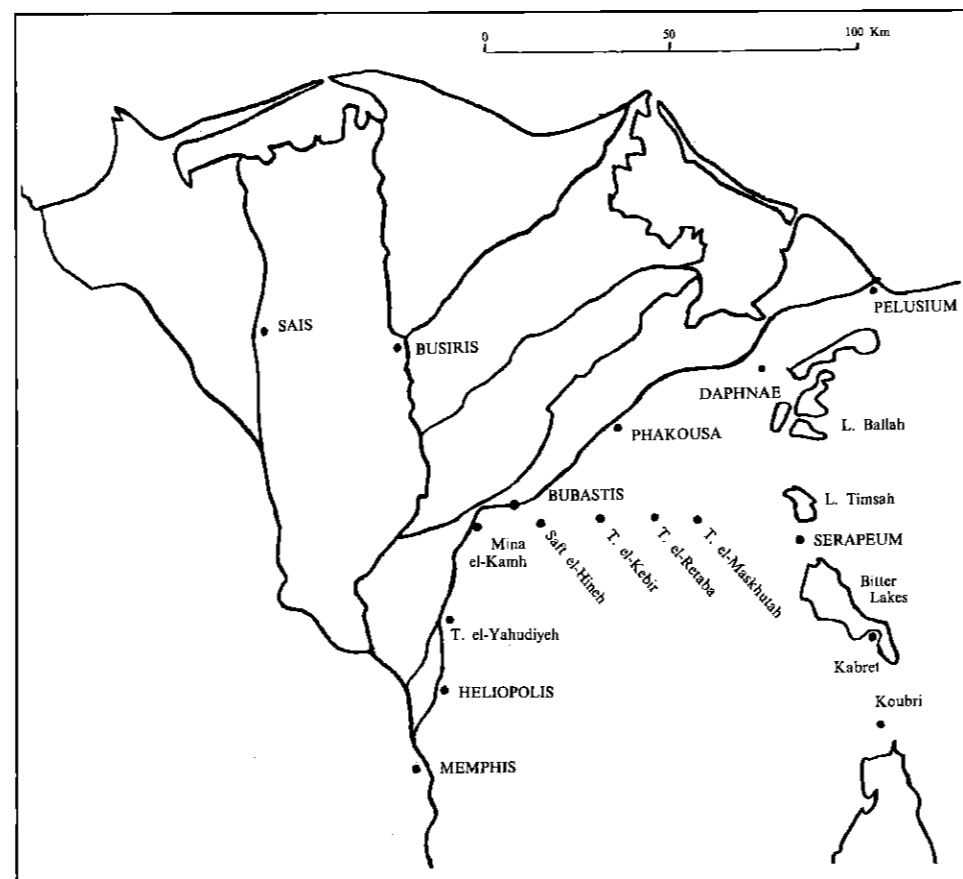


Fig. 1. (after Bietak, *Tell el-Daba II*, 171.

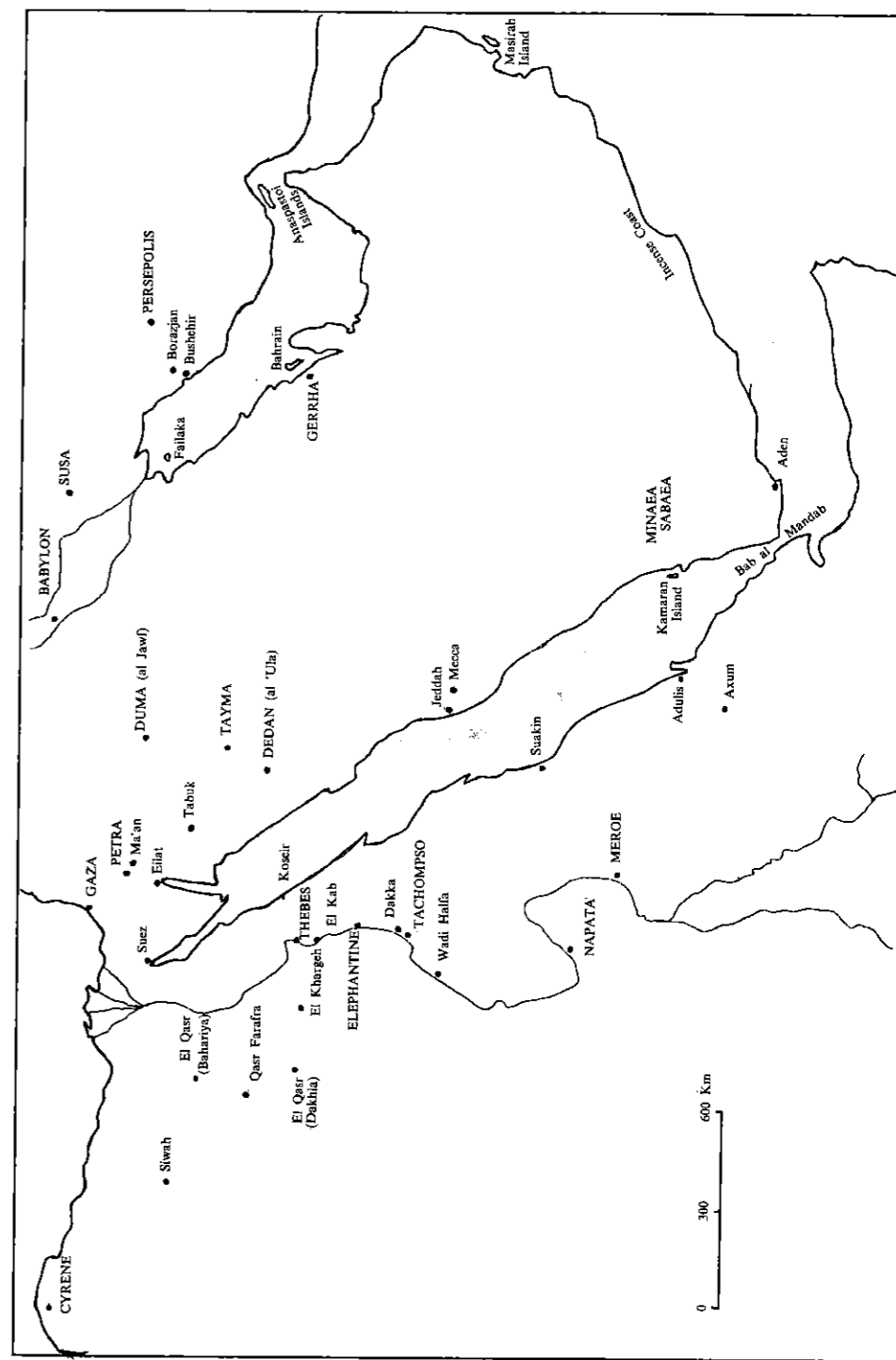


Fig. 2.