

Lewis *op. cit.* 57 n. 51). It has already been observed (*Persepolis Fortification Tablets* 42) that such journeys are either under royal authorisation (PF 1285, 1320, 1321, 1329, 1335) or going to the king (PF 1315, 1319, 2052); even in the apparent exception (PF 1334) the messengers may be going to the king.

Though Datiya is going to the king, it is the king's authorisation he is carrying. Journeys are generally authorised from their point of origin (for apparent exceptions, see Lewis, *op. cit.* 5 n. 14, 8 n. 31, and add the unpublished Q-901, 23rd year, 10th month, where Bakabana, based in Susa, authorises travellers from Sardis to Parnaka, probably only for the last section of their journey), and it may be that in normal circumstances only the king authorises return journeys (*cf.* perhaps PF 1318, 1474, PFa 31.13-15). It is at any rate clear on this occasion that Datiya has been on a round trip to Sardis and is now at Hidali, only three stages from the king in Persepolis, on his return journey. Hallock estimates this as at least four days' journey on foot, but he will have travelled faster.

The Persepolis tablets have rarely shed direct light on the highest politics, but this seems to be an exception. The date lies between January 17th and February 15th, 494, in the winter before the closing campaign of the Ionian revolt. It was always likely that Datis had had some experience in the Ionian revolt before his command against Eretria and Athens in 490, but clear evidence has been lacking. It now appears that he may have been sent by the king in person on a tour of inspection and co-ordination before the final campaign. Tithraustes' corresponding mission to Asia Minor in 396/5, with letters giving him the right to give orders to all satraps and the task of disposing of Tissaphernes, perhaps offers the closest parallel. No light is thrown on the question of how a Mede rose so high.

There is the unsolved question about the Lindian Temple Chronicle, which describes an undated attack by Datis on Rhodes (*FGrH* 532 D). This is hard to fit into Herodotus' description of his movements in 490, and there is some temptation to use it as evidence that he was fleet-commander in 494 (see e.g. Burn, *Persia and the Greeks* [London 1962] 210, 218). All that can be said on the harder evidence of our tablet is that, if he reported to the king very quickly, he could have been back in the summer to command the fleet in the Aegean. Datis rapidly passed into Greek popular myth (see Raubitschek, in K. Schauenburg, ed., *Charites* [Bonn 1957] 234-42), and it is the more gratifying to have him for once pinned down on a real occasion.

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The Trireme Controversy

The order in which the following contributions appear does not reflect any editorial view on the respective merits of the cases argued.

M. Basch on triremes: some observations

Herodotus informs us at ii 159 that Pharaoh Necho II (610-595 B.C.) built and employed triremes on the Mediterranean and Red Seas. In a recent study I put the case for the traditional view that these triremes were Greek rather

than Phoenician in origin.¹ That case may be summarized as follows:

(1) Thucydides believed (i 13) that the trireme had been invented in Corinth during the Cypselid tyranny (*c.* 657-583 B.C.) and that it was almost immediately taken over by the Samians.²

(2) The standard refutations of Thucydides' statement are invalid.³

(3) The Corinthians and Samians were excellently placed in Egypt to hand on the invention.⁴

(4) An Egyptian stele, probably dating to the Saïte-Early Persian Period, bears a Carian inscription in association with two crude graffiti representing ramming warships which are certainly of a Greek type. This fits perfectly with the fact that Carian and Ionian mercenaries are known to have occupied naval bases in Egypt from *c.* 664 to *c.* 570 B.C.⁵

(5) The early history of the trireme in Phoenicia is shrouded in obscurity. The textual evidence for its invention at Sidon before 676 is worthless and in general our data on the history of the type are compatible with the view that it arrived in Phoenicia from the Aegean as part of a general movement which also brought it to Egypt.⁶

(6) The suspicion that there is a tradition of Egyptian reliance on the Levant in ship-design and ship-construction is unfounded. Available evidence proves no more than a dependence on the area for high-quality timber.⁷

In a critique published in a previous volume of this journal M. Lucien Basch attempts to counter most of these arguments and raises a number of additional points.⁸ In the present study attention will be concentrated on those issues which particularly require comment.

(i) The question of reconciling the internal chronological contradictions of Thuc. i 13.1-4 is taken no further by M. Basch. Therefore, my reasons for arguing that Thucydides believed in a Cypselid date for the invention of the trireme at Corinth remain unimpaired by his study. It is a fundamental principle of all historical research that, if there is no good reason to doubt the validity of a piece of evidence, we must proceed as though it were correct. It may not be but the historian's profession becomes impossible if such a principle does not apply. The issue, therefore, becomes one of determining whether there is good reason for doubting what Thucydides clearly believed the truth to be—and that brings us to the next point.

(ii) In attacking the hoary argument that sophisticated weaponry will immediately achieve general acceptance, I pointed to two instances where this clearly did not happen: the breech-loading rifle, first used in the British Army in 1776 but not generally issued until 1865, and the quadrireme, invented at the very beginning of the fourth century B.C. but absent from the Athenian fleet until *c.* 330. The slow adoption of both inventions I attributed to 'conservatism and sheer economics'.⁹ The first example

¹ 'Were Necho's Triremes Phoenician?' *JHS* xciv (1975) 45 ff., hereafter Lloyd (1975), developing an earlier study, 'Triremes and the Saïte Navy', *JEA* lviii (1972) 268 ff., hereafter Lloyd (1972). The conclusions of both are summarized in my *Herodotus Book II. Introduction* (Leiden 1975) 32 ff.

² Lloyd (1972) 276 ff.; (1975) 52 ff.

³ *Id.* (1975) 53 ff.

⁴ (1975) 55 ff.

⁵ (1975) 59 ff.

⁶ (1975) 49 ff.

⁷ (1975) 51 ff.

⁸ 'Trières grecques, phéniciennes et égyptiennes', *JHS* xcvi (1977) 1 ff.

⁹ Lloyd (1975) 54.

was rejected by M. Basch¹⁰ on the grounds that the eighteenth-century weapon was of dubious efficiency. This judgement is quite unfounded.¹¹ On the second issue my explanation has again proved unacceptable to M. Basch but, even if his alternative were correct, it would not alter the fact that there was a time-lag—and that is the point which I am particularly concerned to establish.

Thucydides cannot be refuted by pointing to the apparent slowness with which the trireme was taken up. It is easy to imagine old salts and hardened commanders the length and breadth of Greece putting up a firm and thoroughly prejudiced opposition to the trireme, claiming that it was too heavy and, therefore, deficient in manoeuvrability, structurally too complex, too vulnerable to battle-damage because of its *parexeiresia*, too expensive in terms of men and raw materials, and that it was possible to have more penteconters for your money. The advantage of extra power may have seemed to many to have been cancelled out by such considerations—until experience showed that these reservations were unjustified. It was probably the conflict with Persia which brought the crucial breakthrough.¹²

M. Basch, following Davison,¹³ also uses the Herodotean tradition on Polycrates' naval activities (iii 39 ff.) as an indication that the Greek trireme was at best in an experimental stage when Polycrates became tyrant (c. 533).¹⁴ The assumption here is that triremes were not available to Polycrates at that time but only at a later stage in his career. The text of Herodotus does not justify any such reconstruction. At iii 39, where Herodotus speaks of Polycrates' building a hundred penteconters, he is explicitly talking of piratical activities for which light, fast, highly manoeuvrable and easily concealable vessels were *de rigueur*. French or American privateers operating against British shipping in the eighteenth and early nineteenth centuries were not in the habit of using line-of-battle ships or even frigates! Herodotus' comments do not exclude the parallel or prior existence of a battle-fleet of triremes.¹⁵ At iii 44, on the other hand, he is talking of a formal military context where such vessels clearly have their place. Therefore, nothing in the Herodotean narrative is inconsistent with the view that the trireme was introduced into Samos in the time of Ameinocles and that it was subsequently a standard, though not necessarily the only, element in the battle-fleet.¹⁶

¹⁰ *Op. cit.* (n. 8) 6 ff.

¹¹ On Ferguson's career and achievements see the excellent article in the *Dictionary of National Biography* xiii (London 1889) 348 ff.

¹² Thuc. i 14.

¹³ 'The First Greek Triremes', *CQ* xli (1947) 18 ff. Most recently the argument has been resuscitated by M. M. Austin and P. Vidal-Naquet, *Economic and Social History of Ancient Greece* (London 1977) 224.

¹⁴ *Op. cit.* (n. 8) 6 ff.

¹⁵ Polycrates may have inherited a fleet of triremes from Demoteles or Amphicrates, both of whom seem to have been tyrants before him and both of whom were embroiled in serious naval activity: see J. P. Barron, 'The Sixth-Century Tyranny at Samos', *CQ* xiv (1964) 210 ff. and A. Andrewes, *The Greek Tyrants* (London 1974) 40, 44.

¹⁶ In addition to penteconters and triremes we hear of a dual-purpose *οράματα ναύς* which is alleged to have been invented by Polycrates (Plut. *Per.* 26. 4) and which is stated by Photius and the Souda (s.v. *Σαμίαν ο δῆμος*) to have been a bireme (*δίκροτος*). I have already pointed out the feasibility of combining vessels of different ratings in an early Greek battle-fleet, (1975) 54. M. Basch comments 'l'emploi tactique des pentécontores et des trières était différent' (8 n. 75). Presumably he has in mind the fifth-century situation where penteconters functioned as frigates and triremes as line-of-battle ships: cf. J. S. Morrison and R. T. Williams, *Greek Oared Ships* (Cambridge 1968) 131. For the sixth century and earlier the argument is unsound (cf. Thuc. i 14. 1). It should be remembered that a vessel which is regarded as fit to lie in the line in one century might well be

(iii) M. Basch attempts to place the Phoenicians in a similarly favoured position to the Greeks by arguing that they were available to Necho in the Memphite dockyard of *Pruw-nfr*.¹⁷ This view is unjustified. *Pruw-nfr* is mentioned in Egyptian documents only during the late XVIIIth and early XIXth Dynasties, i.e. the heyday of Egypt's Asiatic Empire. Stadelmann's assertion that it survived into the fifth century B.C.¹⁸ is based entirely on the claim that *Pruw-nfr* and Herodotus' *Τυρίων στρατόπεδον* (ii 112) were identical and is unsound. Stadelmann begins by pointing out that we have evidence from the New Kingdom that Ba'al Sapan (i.e. the Phoenician Ba'al Zephnon) and Astarte were worshipped in *Pruw-nfr* and then relates this to Herodotus' statement that within the *στρατόπεδον* there lay a *temenos* of Proteus inside which stood a temple of the 'Foreign' Aphrodite. He infers that *Pruw-nfr* and the *στρατόπεδον* should be identified for three reasons:

(a) Herodotus often used *Τύριοι* for *Σύριοι*. (This would convert the camp into a pan-Phoenician affair and would fit better what we know—or are claimed to know—of *Pruw-nfr*.)

(b) Proteus is a sea-god. So is Ba'al Sapan.

(c) The association of Proteus with the 'Foreign' Aphrodite is paralleled by the association of Ba'al Sapan with Astarte.

All three arguments are unsound:

(a) The ethnic *Τύριοι* occurs five times elsewhere in Herodotus and in all cases it refers specifically to Tyre.¹⁹

(b) Herodotus does not regard Proteus as a god. At ii 112. 1 he describes him as *ἄνδρα Μεμφίτην* and treats him consistently thereafter as an Egyptian king on the same terms as Cheops, Rhampsinitus and the rest. The genitive in the sentence *τοῦ νῦν τέμενος* . . . might seem at first sight to militate against this view since it would normally refer to the divine owner of a *temenos*, but even in Herodotus there is an exception to this. At ii 178. 3 he speaks of those who 'own' the *Hellenium*, i.e. founded, maintained and used it, in the words *τούτων μὲν ἐστὶ τοῦτο τὸ τέμενος*. We must, therefore, accept the opinion of the overwhelming majority of commentators and regard Proteus as the king who founded the *temenos*, not the god who was worshipped there. It was Astarte who had the *ἱρόν* within it. As for the origins of the Herodotean Proteus, he is simply the Proteus of *Od.* iv 332 ff. converted into a human king. He has then supplanted Homer's Thôn (*Od.* iv 228), the latter being relegated to the status of an official. The connection of this king Proteus with the *temenos* in question will be secondary and its origins are a matter of speculation. It might, for example, reflect the presence on a wall of reliefs representing the royal founder as a Nile-god, i.e. in a form which a Greek might easily relate to the *γέρον ἄλιος*.²⁰

(c) Since Ba'al has clearly nothing to do with Proteus, Stadelmann's third point becomes worthless. We need

regarded as unfit in the next, e.g. in the seventeenth century a 50 was acceptable whereas after c. 1756 it was not: W. Laird Clowes, *The Royal Navy* iii (London 1898) 6 ff., 328. During the sixth century B.C. penteconters would have figured on the same terms as triremes in general fleet-actions.

¹⁷ *Op. cit.* (n. 8) 3.

¹⁸ R. Stadelmann, *Syrisch-Palästinensische Gottheiten in Ägypten* (Leiden 1967) 37; *id.*, *Lexikon der Ägyptologie* i (Wiesbaden 1975) 590 ff.; cf. also Helck, *RE* ix A (1967) 1409.

¹⁹ ii 49.3, 161.2; iv 45.4; vii 98; viii 67.2.

²⁰ W. Spiegelberg, 'Der Aegypterkönig Proteus', *BIFAO* xxx (1930) 103 ff. This problem will be discussed at greater length in the third volume of my *Herodotus Book II*.

only add that, in view of the popularity of Astarte amongst the Egyptians themselves, there is no reason to regard Herodotus' Astarte as a lineal descendant of the Astarte of *Prw-nfr*.

(iv) M. Basch concedes that the galleys represented on the Lausanne stele are of a Greek type but denies that they are triremes.²¹ As it happens, I did not say that they were but how does he know that they are not? Since the graffiti provide little more than ship-profiles, it is perfectly possible that the originals were triremes. At all events the most important point is that the stele provides us with archaeological evidence which is likely to be of Saïte date and which associates a Carian with Greek, not Phoenician, warships. The same cannot be said of the other three pieces of archaeological evidence which have figured in this debate: the Copenhagen 'trireme',²² the Louvre necklace-ends and the Saqqâra ostrakon; for all three are probably Hellenistic at the earliest.²³ What is more, it is doubtful whether such late material can have any relevance to Pharaonic shipbuilding practice.

(v) On the evolution of naval warfare during the first millennium B.C. I confine my comments to areas of conspicuous importance.

The bireme. In his treatment of the Greek Geometric representations and the Sennacherib reliefs M. Basch comments '... des dières aussi perfectionnées que celles du palais de Quoyndjiq ne s'expliquent pas sans une bien plus longue évolution que celle dont les frustes dières grecques de l'époque géométrique sont le fruit'.²⁴ First, sources. The Geometric artistic tradition shows a marked predilection for figures which are unnaturally slender and elongated. This trait would tend to make ships seem more fragile than they really were. On the other hand, it is equally clear that the Sennacherib reliefs show a cavalier disregard for proportion, particularly in a vertical sense, and would create the impression that the vessels were bulkier than they were. Therefore, the differences in proportion between early Greek and Phoenician biremes may not have been as great as M. Basch suggests. There is a further point. The Assyrian artistic tradition of the early seventh century was much more sophisticated than that of eighth-century Greece. In particular it shows greater interest in detail. That, in itself, would encourage the belief that the Phoenician bireme was more highly developed than the Greek. With these caveats in mind we may now proceed to matters structural.

M. Basch argues that the greater antiquity and sophistication of the Phoenician bireme is demonstrated by the presence of a deck on Phoenician biremes and its absence

or insignificance on their Greek counterparts.²⁵ This is not a necessary interpretation. The difference in attitudes to the deck may reflect a long-term difference of emphasis in tactics. From the very introduction of the single-banked ramming warship, which seems to have been an Aegean invention,²⁶ the Phoenicians may have preferred to keep their tactical options open to a much greater extent than the Greeks. The latter clearly grasped and exploited to the full the revolutionary concept of the warship as a ship-destroyer whereas the Phoenicians may have chosen to leave open the possibility of using their *naves longae* as platforms for a land-battle at sea in the old Bronze-Age tradition.²⁷ With the advent of the bireme they would then simply have added an extra bank of oars. Such an interpretation would provide the opposite situation to that described by M. Basch; for, in terms of the evolution of naval warfare, the Greek concept of the *navis longa* and bireme would be more, not less, advanced than that of the Phoenicians.

The trireme. The linchpin of M. Basch's view that the trireme was invented at Sidon before 676 is a passage of Clement (*Stromateis* i 16. 76). This I have already shown to be historically worthless²⁸ and M. Basch has not succeeded in rehabilitating it by informing us of the existence of a naval conflict between Tyre and Sidon in the early seventh century B.C.²⁹

²⁵ *Op. cit.* (n. 8) 3 ff.

²⁶ Lloyd (1975) 55.

²⁷ The clearest evidence of Late Bronze-Age naval warfare in the E. Mediterranean is Egyptian. The reliefs at Medinet Habu representing Ramesses III's battle with the Sea-Peoples c. 1170 depict just such an action. The Egyptian ships are closely paralleled by non-military types (e.g. J. Vandier, *Manuel d'Archéologie Égyptienne* v [Paris 1969] 952 fig. 360, left) and that impression is confirmed by linguistic data. The Egyptian words 'h' (*Wörterbuch* i 222.4-8) and *ms* (ii 89.8-10) are used both in military and civilian contexts, the former even occurring at Medinet Habu in the description of the sea-fight in the significant phrase 'h' n 'h3t, 'h' for fighting'. The Levantine naval activities discussed by M. Basch (n. 8) 2 ff. will doubtless have taken the same form as those of Ramesses III. (It is probably as well to scotch Linder's recent attempt to render the Ugaritic *mi-ši* in the phrase *ameluti mi-ši* as 'warship' ('Naval Warfare in the El-Amarna Age' in D. J. Blackman, ed., *Marine Archaeology* [London 1973] 319 ff.). The notion is based on the assumption that the word descends from an Old-Egyptian word *ms* alleged in one instance to mean 'warship' (*Wörterbuch* ii 156.2). This rendering has long since been abandoned in favour of the normal translation of *ms* as 'expedition': C. Boreux, *Études de Nautique Égyptienne* [Cairo 1924] 128 n. 1. In any case, even if the Ugaritic had meant 'warship', it would probably have referred to nothing more elaborate than the Egyptian 'h' n 'h3t.)

²⁸ Lloyd (1975) 49 ff.

²⁹ *Op. cit.* (n. 8) 8 ff. On the *parexeiresia*-question the case for and against has been, in general, adequately stated. Two points only need be added. First, the issue of the presence or absence of the *parexeiresia* on Phoenician triremes is irrelevant to the problem of the date of the trireme's introduction; for, if the *parexeiresia* were used in Phoenicia, Greek influence would be an obvious possibility. If it were not, Greek influence still remains a possibility since the motivation for adding the third bank could have been the prior existence of Greek triremes. Second, M. Basch misrepresents the meaning of Apollonius *Arg.* i 394 ff. The poet is not describing 'une pentécontorière' but a standard Homeric penteconter with twenty-five men a side disposed on one level.

³⁰ The British Museum Dockeyard Papyrus, probably dating to the reign of Tuthmose III (c. 1490-1436), refers to shipbuilding or, at least ship-repairing, apparently in *Prw-nfr*, but it is poor evidence of the importance of Phoenicians as shipwrights. Not one of the shipwrights has a Phoenician name. In fact the vast majority of the names are Egyptian. The only clear exception is 'In(verso 8, 11) and that means not 'Aradian', as the older literature has it, but 'Arza wan' (i.e. Pamphylian, A. H. Gardiner, *Ancient Egyptian Onomastica* [Oxford 1947] 129* ff.). On the other hand, one of the officials assisting the crown-prince Amenhotpe in dispensing materials from the magazines is called Teshub-ba'al. He has been claimed as a Phoenician but, if the name is any guide at all, he could just as well have come from anywhere in the Semitic Near East. At all events his rôle does not involve shipbuilding proper. Essentially he functions as a high-ranking store-room clerk and need not have known any more about the technical-

²¹ *Op. cit.* (n. 8) 9.

²² It is questionable whether the model represents a trireme at all. The prototype may have been a Hellenistic polyreme: G. Bass, *A History of Seafaring* (London 1972) 58, 14.

²³ The dating of the first is a problem. Since the Egyptian dealer from whom it was purchased in 1902 claimed that it derived from Armant (N. Breitenstein, *Catalogue of Terracottas Cypriote, Greek, Etrusco-Italian and Roman* [Copenhagen 1941] pl. 63, 520), there is a temptation to regard the date of the foundation of the Buheum (c. 350 B.C.) as a *terminus post quem*. (Cf. Basch, 'Phoenician Oared Ships', *Mariner's Mirror* lv [1969] 232). Unfortunately, the claims of Egyptian dealers are notoriously untrustworthy and the provenance should be treated as uncertain. On the other hand, since it is improbable that such a terracotta would be earlier than the Hellenistic Period, we are entitled to the strong suspicion that it post-dates the Macedonian conquest. On the date of the second see Lloyd, 'The so-called Galleys of Necho', *JEA* lviii (1972) 307 ff. On that of the third see *id.*, 'Two figured Ostraca from North Saqqâra', *JEA* lxiv (1978) 110 ff.

²⁴ *Op. cit.* (n. 8) 4.

(vi) In my study it was argued that, on available evidence, Phoenician influence on Egyptian shipbuilding was confined to supplying high-quality timber. M. Basch counters by invoking the activities of Syrians in the *Pruw-nfr* during the late XVIIIth Dynasty and the case of a Syrian who built divine barks for the whole of Egypt. Neither point convinces.³⁰ However, since he has recently argued with more success that the Egyptian *mnš*-ship of the New Kingdom shows Levantine influence,³¹ I should no longer wish to maintain the argument by precedent.

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ties of shipbuilding than his royal superior. The other text always invoked on this subject is *P. Hermitage 1116B (verso)*. It is generally assumed, though it is far from certain, that it refers to work at *Pruw-nfr*. At 15 ff. It mentions dispensing fifty pieces of wood to six individuals who are described as Kharu, i.e. Syrians. With one exception they bear Egyptian, not Asiatic, names. If we accept the connection of the papyrus with *Pruw-nfr*, this situation might encourage the guess that a large number of the Egyptian names in the British Museum papyrus conceal foreigners, but any such assumption would involve a *petitio principii*. How many were Egyptian and how many were foreigners is absolutely undemonstrable. One query, however, irresistibly suggests itself. If most of the workmen mentioned in the British Museum papyrus were Syrians, why do we not have at least some Syrian names? As for the Syrian who made divine barks for all the gods of Egypt, he need not detain us long. In view of the sacred nature of the divine barks in question, it is in the highest degree unlikely that under this individual there was any departure from time-hallowed and god-sanctioned formulae. Therefore, his position cannot be regarded as evidence of Syrian influence on Egyptian shipbuilding.

³¹ 'Le navire Mnš et autres notes de voyage en Egypt', *Mariner's Mirror* lxxiv (1978) 99 ff. Note, however, that influence on one type does not prove Phoenician domination of Egyptian naval architecture. Food for thought is provided by the Aramaic papyrus which refers to a Memphite naval arsenal at the very end of the fifth century B.C.: N. Aimé-Giron, *Textes Araméens d'Égypte*, (Cairo 1931) 12 ff.; R. A. Bowman, 'An Aramaic Journal Page', *AJSL* lviii (1941) 302 ff. The text mentions individuals with Egyptian, Persian, Babylonian, Khivan, Caspian, Phoenician, Moabite and probably Jewish names. It presumably refers to a situation which came into existence after the Persian occupation and is hardly relevant to Saïte conditions. We should, however, observe that the polyglot nature of the names indicates that, even in the Persian period, Phoenicians were simply one of many nationalities active in Memphite nautical circles.

M. le Professeur Lloyd et les trières: quelques remarques

Il ne saurait ici être question de rencontrer, point par point, toutes les observations de M. Lloyd: ceci exigerait un nouveau tour d'horizon allant du fusil de Ferguson au panthéon adoré à *Pruw-Nfr* et nous éloignerait sensiblement de ce qui me paraît être l'essentiel de la controverse; l'essentiel, à mon sens, est ceci.

Dans le *JHS* de 1975, M. Lloyd soutenait les quatre propositions suivantes:

(1) La trière phénicienne ne différerait guère de la trière grecque: c'était 'essentially (souligné par moi) a Greek hull', avec adjonction d'un pont continu et de superstructures perfectionnées.¹

(2) La trière grecque a été inventée à Corinthe au 7^e s., alors que les Phéniciens ne connaissaient pas encore ce type de navire.

(3) La construction des premières trières phéniciennes est due à une influence grecque.

(4) Parallèlement à l'influence exercée, dans le domaine de

l'architecture des trières, de la Grèce vers la Phénicie, une même influence s'est exercée sur l'Égypte qui, sous le règne de Nechao, a construit des trières de type grec.

A ces propositions, j'ai répondu, dans le *JHS* de 1977, en distinguant les points certains des points hypothétiques.

(1) Il est certain que la trière phénicienne différerait fondamentalement de la trière grecque.

(2) Il est probable que la trière phénicienne a été inventée avant la trière grecque.

(3) Il est certain que la structure de la trière phénicienne ne doit rien à une influence grecque.

(4) Il est probable que les trières de Nechao étaient de type phénicien.

Je réexaminerai brièvement ces quatre points.

(1) et (3) Tout le monde admet à présent que la *parexeiresia* (apostis, ou outrigger) est l'élément essentiel de la trière grecque: sans elle, la trière grecque est tout simplement inconcevable. Or le témoignage des monnaies phéniciennes est absolument formel: aucune des trières, pourtant représentées avec infiniment de précision sur ces documents, ne montre, avant la conquête de la Phénicie par Alexandre, une *parexeiresia*.² Celle-ci figure, immédiatement après la conquête, sur les monnaies d'Arados.³ Il ne s'agit nullement d'un détail: si une coque pouvait contenir trois rangs de rameurs sans *parexeiresia*, c'est qu'elle différerait essentiellement d'une coque grecque.

M. Lloyd admettait, en 1975, que le modèle d'Erment, au Musée National de Copenhague, faisait difficulté, mais il l'éliminait en le condamnant sévèrement, non pas sur la base de l'examen de l'objet lui-même, mais en s'appuyant sur des arguments d'autorités. D'abord sur celle de l'éminent historien de l'architecture navale que fut le regretté R. C. Anderson: on a vu que celui-ci avait rétracté une opinion qu'il avait émise assez légèrement. Ensuite celle de R. A. Higgins, pour qui les terres cuites gréco-égyptiennes sont 'technically . . . clumsy works'. C'est là une opinion qui concerne les terres cuites en général et il est téméraire de l'appliquer à une œuvre à tous égards exceptionnelle: les lecteurs qui auront examiné, faute de l'objet lui-même, la planche I du *JHS* xcvi (1977) jugeront s'il s'agit là d'un 'clumsy work'.

Or ce modèle ne montre pas, lui non plus—et c'est là le point capital—de *parexeiresia* et il correspond, en outre, complètement aux témoignages des monnaies de Sidon et d'Arados de la première moitié du 4^e s. Dans ses *Observations*, M. Lloyd ne semble plus attaquer le modèle d'Erment, qu'il appelait en 1975 ma 'strongest weapon', mais il estime qu'il remonte, au plus tôt, à l'époque hellénistique, toutefois (j'ai beau relire la note 23 des *Observations*) sans raison vraiment sérieuse. Certes, les dires du marchand égyptien qui a affirmé que le modèle avait été trouvé à Erment sont-ils sujets à caution, mais en quoi cela nous permet-il de lui assigner une date plutôt qu'une autre, en dehors de toute référence? Admettons donc, tout simplement, que ce modèle vient d'Égypte, sans autres précisions. En bonne méthode, le seul moyen de dater le modèle est d'opérer par comparaison. Or il ne peut être comparé qu'à certaines monnaies phéniciennes de la première moitié du 4^e s., et à elles seules. D'ailleurs, s'il fallait admettre que le modèle est hellénistique, voilà qui ne

² L. Basch, 'Phoenician Oared Ships', *Mariner's Mirror* lv (1969) pl. 7 et 8.

³ Pour la première fois sur une monnaie d'Arados sous Straton, fils de Gérostrate: Basch (n. 2) 233, fig. 20.

¹ A. B. Lloyd, 'Were Necho's Triremes Phoenician?', *JHS* xcvi (1975) 60.