

TRIREMES AND SHIPSHEDS

I offer this paper as a tribute to John Morrison. It was he who first inspired me as a schoolboy with an interest in ancient ships; he who, as my tutor at college, revived my interest; he who later gave me the opportunity to contribute to *Greek Oared Ships* and committed me to a life-long involvement with shipsheds.

I have benefited from John Coates' comments on this paper, as delivered.

Abbreviations

AHM	D. J. Blackman, "Ancient Harbours in the Mediterranean", <i>JNA</i> 11.2, 1982, 79-104; 11.3, 185-221.
AT	J. S. Morrison & J. F. Coates, <i>The Ancient Trireme</i> . (Cambridge 1986).
Evidence	D. J. Blackman, "Evidence of Sea Level Change in Ancient Harbours and Coastal Installations", in D. J. Blackman (ed.) <i>Marine Archaeology, Colston Papers</i> 23, 1973, 115-39.
FSD	V. Foley, W. Soedel & J. Doyle, "A Trireme Displacement Estimate", <i>JNA</i> 11.4, 1982, 305-18.
GOS	J. S. Morrison & R. T. Williams, <i>Greek Oared Ships, 900-322 B.C.</i> (Cambridge 1968).
Hurst 1977;1979	H. Hurst, "Excavations at Carthage: third (fourth) interim report", <i>Antiquaries Journal</i> 57.2, 1977, 232-61; 59.1, 1979, 19-49.
IG	<i>Inscriptiones Graecae</i> (Vol I covers Athenian inscriptions pre 404; Vol II post 404).
Lehmann-Hartleben	K. Lehmann-Hartleben, <i>Die antiken Hafenanlagen des Mittelmeeres</i> , <i>Klio Beiheft</i> 14 (Leipzig 1923).
SSAW	L. Casson, <i>Ships and Seamanhip in the Ancient World</i> . (Princeton 1971).

The starting point for this paper is a long article published in 1982 by V. Foley, W. Soedel and J. Doyle (hereafter FSD). It contains some interesting ideas combined with some serious errors in the underlying argument; it is because of the interesting ideas that I feel it worth some discussion also of the errors.

The main aim of the article is to argue for a low trireme displacement estimate - closer to J. G. Landels' 36 metric tons (or, one could now add, Morrison & Coates' estimate, which I take to be about 48 metric tons) rather than 70 tons (W. L. Rogers 1937) or even 110 tons (D. H. Kennedy 1976). It is unfortunate for them that the main quantitative data from an ancient source which they have used, and assume to be reliable, are not in fact reliable at all; they are contained in an Athenian inscription of about 430 BC (normally dated "early in the Peloponnesian War" and most recently to 440-425: *IG* I³ 153), which refers to various activities with warships, which we may assume to be triremes (from references to trierarchs, and *trieropoioi*): Annex I. Unfortunately the stone is extremely fragmentary and their university colleague, who translated the text for them, failed to understand the meaning of square brackets in epigraphic publications — [hypothetical restoration] — or if he did understand it, failed to warn his Greekless

colleagues, the authors of this article. They quote the key passage as follows (lines 6-10):

"It is possible for no-one to draw out (a ship) with less than 40 men, nor to launch (one) into the sea with less than 20 men, nor to pitch or undergird (one) with less than 50."

This translation is of the text as published in the second edition of *Inscriptiones Graecae* (Kirchner/Hiller, *IG I²* 73 with addenda p. 302 +), since the third edition appeared only in 1981. If the latter had been available, some but not all of the mistakes would have been avoided.

There are four objections to the translation:

- (a) The numerals "40" and "20" are legible on the stone (lines 7, 8) but could well be incomplete — for "40 and [100]" and "20 [and 100]". The word "and" is legible after "40" while the following word is lost; after "20" the next words are lost. In Ancient Greek, after a negative verb, as in this case, "40 and" is likely to be part of a composite numeral (another verb would be preceded by "nor", not by "and"). In the third edition Lewis following Jameson does accept the larger numerals [1]40 and [1]20; I am sure that this is right, and I see that Morrison also agrees (*AT* p. 134).
- (b) The numeral "50" is entirely hypothetical — the number of missing letters in line 10 would allow the restoration $\eta\epsilon\chi\sigma\acute{\epsilon}\kappa\omicron\nu\nu\alpha$ or $\omicron\gamma\delta\omicron\acute{\epsilon}\kappa\omicron\nu\nu\alpha$ just as well as $\pi\epsilon\nu\tau\acute{\epsilon}\kappa\omicron\nu\nu\alpha$: 60 or 80 rather than 50.
- (c) The text has no certain reference to "pitching": the word "to pitch" (*pittōn*) was hypothetical in line 9 of *IG* second edition. There is no certainty of a verb between "launch" and "undergird" — indeed even the verb "launch" is a restoration in line 8, though a fairly certain one in the context. The space in line 9 on the stone would be filled by the "100" or "20 [and 100]" if that hypothesis is correct; we must of course beware of circular argument.
- (d) The translation does not complete the sentence. The list of operations continues with one — "painting round (?)" (*IG I²*) or "conveying round" (*IG I³*) — which was to be carried out by a minimum of 100 men, and then continues with references to the obligations of the trierarch and helmsman, which conclude the decree. The numeral "100" in line 11 is the only certainly complete numeral surviving on the stone. As for the nature of the operation, the suggestion of Lewis (*IG I³*) seems more plausible: two alternative verbs, which would fit epigraphically, refer in Demosthenes (51.4) and Thucydides (7.9) to the operation of trierarchs bringing triremes round, after fitting out, from the smaller harbours of Piraeus which contained most of the shipsheds, Zea and Munychia, to the *choma* or hard in the larger harbour, Kantharos, for final inspection by the *apostoleis*, the commissioners responsible for overseeing the dispatch of a naval expedition. It is more plausible to have a minimum number of crew prescribed for this operation than for pitching or painting.

From this we may conclude:

- (a) The word "undergird" is clearly legible (line 9), and it is clear that this was a difficult operation for which a minimum number of men was prescribed. Morrison & Williams (*GOS* p. 294 & n. 31) had restricted their use of the inscription to this point. Morrison & Coates (*AT* p. 170), with the advantage of the third edition of the inscription, note that the minimum number is not preserved (*AT* p. 170). It was 50, 60 or 80. For the operation involved see now J. Coates (*JNA*, 16.3, 1987, 207-11).
- (b) The whole argument which FSD based on the need for 50 men for the operations of pitching and undergirding is, one might say, based on sand.

(c) Similarly unfounded are their deductions from the minimum numbers of men needed for hauling out and launching a trireme: the figures are more likely on epigraphic grounds to be 140 and 120 respectively than 40 and 20 — and this would point to a higher trireme displacement.

A thought occurs to me at this point — when the oarsmen have finished their sea trials on the *Olympias*, could one not ask them to conduct tests on this point also, rather than have to rely on human physical performance *estimates* alone?

Could a slip be found with a gradient of 1 in 10 (much steeper than the usual slope nowadays — 1 in 20)? Or if not, could one be built? Then the *Olympias* could be properly housed, and one could also test the (plausible) assertions of Morrison and Coates about men standing beneath the outrigger to manhandle the ship (*AT* p. 134; compare FSD 308, Fig. 1). Coates tells me that he has assumed a weight at slipping or launching of 25 tonnes, which would mean that the pull up the slip required was 5 tonnes, a very feasible pull for 140 men (just under 36 kg. per man).

Is it still the intention, as Morrison and Coates hoped (*AT*, p. 8), to "house the ship in a reconstruction of a Piraeus shipshed of the fourth century BC from which it will be launched for historical research and demonstration at sea"? Will the new maritime museum contain a faithful reconstruction of a shipshed? I am glad to learn that this is the firm intention.

FSD also adduce as a further argument for the "light trireme" the lack of evidence for windlasses or similar apparatus in the fifth century BC. The point is not certain and deserves further investigation. What is not acceptable is the circular argument of FSD, who use the supposed reference in the Athenian inscription — "50 for an undergirding crew" — as evidence that the windlass was *not* in use ca. 430 BC. The earliest evidence known to me is from the pair of slipways at Sounion, cut for ships smaller than triremes and with an unusually steep gradient of 1 in 3.5 (launching calculations are needed to determine what ship could have been launched from such a steep slope). Admittedly in first publishing these remains, E.J.A. Kenny argued for a mid-third century date, but I would still favour a date in the late fifth century — the emergency of 413/12 (see my comments *GOS*, pp. 184-5). There is also no certainty that the bronze ratchet wheel found on the site belonged to a windlass; it has been suggested that it belonged to a catapult, installed in the fortifications at Sounion during the Chremonidean War (265/4) or the Macedonian occupation (263-229) (J.A. Dengate, *AJA* 71, 1967, 185-6). Casson, however, agrees that at Sounion "craft were necessarily drawn up with the aid of winches and the like" (*SSAW*, p. 364).

There are also what seem to be the mountings for three capstans at Thuri, at the back of the pair of slipways which are fifth-century or later; here, however, they were less necessary than elsewhere, for the gradient is only some 1 in 20 (*AHM*, p.205, fig. 12). The evidence from Munychia is not reliable (*Evidence*, p. 129, fig. 25).

There is no doubt that sophisticated hauling equipment existed later. Archimedes clearly achieved great improvements, such as the screw windlass used to launch Hiero's great ship, the *Syracusia* (Athenaeus, 5.207b, a passage quoted in full in *SSAW*, 194-5; Casson may be right that the description by Plutarch (*Marcellus* 14.8) of another launching by Archimedes, using a multiple block and tackle, was in origin perhaps a variant on the former description). Horace refers to *machinae* hauling "dry keels" down to the water in spring (*Odes* 1.4.2); Vitruvius (10.12) describes arrangements of blocks and tackle for hauling ships ashore and we *may* have

references to hauling equipment as early as Herodotus. He uses the word *holkoi* of the installations, still visible when he visited Egypt in the mid-fifth century, built on the Red Sea for his triremes by the Pharaoh Necho (died 593) and by Greeks at a garrison post in the eastern Nile Delta at the same period (2.159.1; 2.154.5; *AHM* p.204 & n.107). Does the word mean "hauling equipment" rather than "hauling-way" (the normal interpretation)? Stone structures seem more likely to have survived nearly a century and a half for Herodotus to see, but Thucydides uses the word (3.15.1) of equipment for hauling ships across the *diolkos* at Corinth.

To complete my criticisms of FSD's article, before turning to the positive points:

- (1) they strangely argue that triremes were beached or hauled into shipsheds stem first; but surely they could *only* have been hauled up *stern first*,
 - (a) to enable rapid re-launch and operation in an emergency; and
 - (b) to avoid the problems which the stem (or the ram) would create with mounding of sand on a beach, or with running the keel into a keel slot (if that was done-see below) or on to a runway on a slip. There were admittedly exceptions, but if *okellein* does mean grounding a ship bow first (as *AT* p. 163 n. 3), then it is clear from Thucydides (4.11-12) that the manoeuvre ordered by Brasidas was unusual and unpopular with the Spartan trierarchs and helmsmen.
- (2) they do not take into account the evidence from recent shipshed finds: they used my account published in 1968 (*GOS* pp. 181-6, written in 1966) but there is now much new evidence which I tried to summarise in 1982 (*AHM* pp. 205-6), the same year as FSD's article. To take only two examples which I discuss below: they ignore the evidence from the Carthage excavations of 1974-78; and Thuri provides clear evidence of the use of a timber "cradle" in slipways. Furthermore, they ignore evidence long known and published: for example, the Oeniadae slipways, which have a full length of 47 m and, like Sounion, have a steeper than normal gradient (just under 1 in 6), and which have the upper ends of the slips apparently prepared to fit the stern configuration of a warship. Incidentally, Heuzey recorded an oral report that the piers at the head of the colonnades dividing the five slipways bore bronze rings on their front faces, which could have served in making fast the ships; but the report has not been confirmed and must be regarded as doubtful (*Le mont Olympe et l'Acarnanie* p. 449, quoted by Lehmann-Hartleben, p. 117 & n. 2).

I now turn to the positive points in FSD's article. They have made an interesting contribution by conducting a series of friction coefficient tests with oak and fir planks on a polished granite surface, both wet and dry; the friction coefficients were high with dry surfaces, and higher with wet. (A question which occurs to me is: were the results the same with oak and fir?) They then made tests using lubricants — olive oil, beeswax and lard — producing friction coefficients which were again "too high" (for their theory). They then tested their lubricants again, dragging wood over wood, with the same results, and therefore conclude that the shipshed slips were not sheathed with planking or equipped with wooden skidways.

What FSD do not specify is whether they tested these lubricants on a *wetted* wooden surface, or only over a *dry* wooden surface; nor do they say what was the species of wood underlying. The question of a *wetted* wooden surface remains in my mind when I consider the last of their tests, which was very interesting: *pitch* lubrication of a wood/polished stone interface produced significantly lower friction coefficients, particularly when the pitch was *wet*. Since, however, they do not refer to having done tests of pitch lubrication of a wood/wood interface, I do not

believe that we have to accept their denial of the possibility of wooden planking or skidways in the shipsheds.

FSD have carefully checked the different performance of wet and dry pitch, and investigated the reasons. They conclude that temperatures above 25° were needed for good results, and remark that Athens would normally have had those temperatures in the campaigning seasons. Even if this is true, I wonder whether the same is true of wax and tallow; with tallow as a lubricant, for example, would not hotter temperatures have made things worse?

FSD also conducted their tests with weighted planks of several configurations other than a plank with flat undersurface. Tests using keel-shaped boards (apparently lubricated with pitch) meeting the stone in an obtuse V, including about 170°, yielded significantly higher friction coefficients. "Hence", they argue, "here again is some slight experimental evidence for flat bottoms in trireme design, and some explanation for the frequent incorporation of keel slots into shipshed launching ramps. In such an arrangement, the weight of the ship would be taken mostly by the bottom planking and the ribs".

In reply to this I would make two points. First, on the evidence which we have keel-slots were *not* frequently incorporated into the rock-cut shipshed launching ramps, and on FSD's hypothesis we should have to reject the idea of timber runways laid on the stone ramps.

Secondly, we do have the evidence of Theophrastus, no mean authority on such things, that triremes were given keels of oak to withstand the strain and wear of hauling (*neôlkia*: HP 5.7.2). This seems to me clear support for the standard view that the keel took the weight of the trireme during hauling operations. In Coates' view "warships were most certainly slipped stern first, and it seems to me most unlikely that their weight was supported on the slip anywhere but through the keel. The problem of transmitting concentrated forces to the hull when the bow lifts would be acute if the ship did not hinge on the keel."

What is the ancient evidence for lubrication? There is one literary reference to smearing a keel with beeswax, in a fragment of the mid-sixth-century Ionian poet Hipponax (frag. 46 Diehl³ ἔπειτα μάλθη τήν τρόπιν παραχρίσας). I am not sure whether one should give full force to the verbal prefix παρα-, used where one would have expected περι-: smearing the *sides* of the keel?) The lexicon entry of Liddell-Scott-Jones explained the purpose of the operation as caulking the ship. Morrison & Williams (*GOS* p. 120) suggested that the aim was more probably to enable the ship to run smoothly on a runway, and that Hipponax may have been describing a launching. Morrison & Coates now say (*AT* pp. 188-9): "The purpose of this treatment could be to make watertight the seam between the keel and the garboard strake, often a main source of leakage." But they then continue: "Applied to the underside of the keel, particularly, it could have been a lubricant to facilitate slipping and hauling up in a shipshed." It is not clear to me which explanation they prefer — or do they suggest a double purpose? Certainly lubrication must have been intended.

Morrison & Williams had already plausibly suggested (*GOS* p. 280) that we may find here an explanation of the "white *hypaloiphe*" ("white bottom paint") referred to several times in the fourth-century Athenian Naval Lists — notably in a miscellaneous collection of ship's stores in the list for 330/29 BC (*IG* II² 1627.313). It is either wax, or "likely to be resin, probably mixed with lime" (*AT* p. 188), or some kind of clear varnish, or tallow. Federico Foerster Laures has

provided some useful information about the use of tallow. In an immediate comment on the article by FSD, he noted that even nowadays wooden boats are hauled up on Mediterranean beaches over wooden beams greased with sheep's tallow (and apparently laid across the beach slope), and adds that these beams should never be of conifers, since most keels are of pine and use of the same type of wood causes bad friction (*JNA* 12.2, 1983, 176). In a later note he describes the use of tallow on the underside of ships of 13th-century AD Aragon - and the tallow was "white" (*JNA* 15.2, 1986, 161; cf *JNA* 16.2, 1987, 171). Foerster suggests that the trireme crew could have laid their oars on a beach to provide a kind of slipway (N.B. the reference to the "60 oars of a trireme" must be an oversight).

Also included in the same Naval List are two type of "black *hypaloiphe*", which must be two varieties of pitch (sic *GOS & AT* loc.cit.). One wonders why the list does not use the word *pitta*. One reference to pitching must now be deleted — the reference in the Athenian inscription of ca.430 is entirely hypothetical (see above).

Morrison & Coates (*AT* p. 189) have usefully brought into the discussion a reference first noted by Casson (*SSAW* p. 211 n. 46): a description by Pliny the Elder (*NH* 16.56) of a mixture of pitch and wax with salt added, known to the Greeks as *zopissa*, "live pitch", which was used on ships' hulls and according to Pliny was "much more effective for all the purposes for which pitches and resins are useful". Morrison & Coates conclude that "pitch and wax were customarily applied, either successively or as a mixture, to the wetted surface of a ship's hull". On this I can only comment that Pliny clearly seems to refer to a *mixture*, and that he does not mention application to a wetted surface. (Interestingly enough, when I aired this subject in an earlier lecture, before the publication of *AT* reminded me of *zopissa*, I was asked if there was evidence of a pitch and beeswax mixture, since the wax would prevent cracking of the pitch). Casson (loc.cit.) stated that "It was usual to smear the seams or even the whole hull with pitch or with pitch and wax" and for pitch and wax quotes not only Pliny but also Vegetius (4.44): "unctasque cera et pice et resina tabulas" ("planks smeared with wax and pitch and resin").

To return to keel waxing: I had assumed that an oak keel was waxed to reduce friction over stone and timber — not, I think, within a "keel-slot" but on the ramp of the slip, whether timber-clad or not. I am now increasingly convinced that those "keel slots" which have been identified, with the possible exception of one at Apollonia, were not intended to take the ship's keel direct onto the stone, but may have contained a timber runner. FSD had suggested that the aim of the keel slot was to transfer the ship's weight from the keel to the lower strakes; if this were true, one would have to assume lubrication of the lower strakes also, but the evidence against this view seems to me compelling (see above, and note also the configuration of the slipways at Sounion and Oeniadae).

What is now becoming clear is that (pace FSD) timber was used on shipsheds ramps. The time has come to review the main evidence. The best evidence has been provided by the excavations on the Ilôt de l'Amirauté at Carthage. One ramp from the stone shipsheds, the monumental phase destroyed in the Third Punic War, was excavated over slightly less than half its width and for a length of 28m (Hurst 1979, p. 24 and fig. 1). Mixed sand, clay and loam layers were deposited as a make-up of a ramp with a regular sloping upper surface, with a slope of about 1 in 10. Its maximum preserved height towards the centre of the island (not its original maximum height) is about 2m above earlier levels. In the upper surface of the ramp

parallel timber sleepers, c.10 x 15 cm in section were set at regular intervals of about 60 cm, at right angles to the slope. At one point another timber ran at right angles to these sleepers, along the slope of the ramp, but the excavator maintains that it is not clear whether this was an intentional feature of the ramp (nor does he make clear its position within the ramp); in his conclusion, however, he says (p. 30): "there may also have been longitudinal timbers to serve as guides for the hulls of the ships or timber cradles when they were hauled up: the possible trace of one such longitudinal timber was found in the excavated ramp (pl. VIII a)". The reconstruction by Sheila Gibson (loc. cit. fig. 3) indicates a pair of longitudinal timbers which is plausible, but the evidence for two is not made clear (and her axonometric detail shows only the sleepers: fig. 4). The cross sleepers are much easier to explain as serving as a base and fixture for the longitudinal runways (Figure 1).

Absolute confirmation that the ramps were for slipways was provided by the discovery of acorn barnacles and copper nails (from ships' hulls) on the surface of the ramps and under the burnt destruction level — the superstructure of the shipsheds burnt in 146 BC.

A point to emphasize is that only the central part of the ramp was excavated, and not top or bottom, but there at least the ramp is not of stone. It *may* have been at the top, where the stone would have been robbed; and there is some evidence to show (what was in any case most likely) that the bottom of the ramp was of stone: on the south-east side of the island the Punic quay wall has been defined, with its top sloping down *outwards*, at -0.36 to -0.55 m below present sea level (which is now thought to be about 50 cm above Punic sea level: Hurst 1979, p. 27).

This could represent the continuation of the ramp; it is unfortunate that the question of the end of the shipshed ramps does not seem to have been fully borne in mind during the excavation of this area. It is worth noting that at this point (shipshed 4) the shipshed length would have been ca. 44 m — and these were the shorter ones; the longer ones such as number 16 would have been about 48 m long. This evidence must not be forgotten when considering the length of the shipsheds in the harbours of Piraeus.

I am left with the feeling that one major problem remains unsolved concerning the Carthage shipsheds. We have explicit evidence for 220 shipsheds at Carthage in Appian's description of the final phase before the destruction (*Libyca* 96, using an eye-witness account by Polybius). Hurst assumes 30 stone shipsheds on the island, which looks about right; but where were the other 190? Could the outer edge of the harbour have held so many?

So far the discussion has been confined to the stone shipsheds of the final phase before the destruction of Carthage. Belonging to an earlier phase, of the third century, are timber structures which Hurst originally interpreted as timber shipsheds (1977, p. 235). He later rejected this interpretation, saying the weight of evidence has shifted away (1979, p. 23), but I have not read anything which convinces me that his first reaction was wrong. The evidence is a series of parallel east/west slots or wall trenches (Hurst 1977, fig. 3), spaced 6 m apart like the lines of piers of the stone shipsheds. I am aware that I am thought to be inclined to interpret every structure 6 m wide as a shipshed, but here I think I am justified until stronger evidence against this interpretation is produced; the lengths would have been just right, also — 45-50 m.

Hurst points out that these wall trenches appear to be exactly parallel to each other, while the lines of stone piers radiate from the centre to fit the circular outline of the island. This

suggests to me that the wall trenches belong to a phase before the island had undergone its monumental layout; the lack of evidence for parallel timber-period trenches on the west side of the island is in my view an argument *for* rather than *against* the timber shipshed hypothesis (in any case too little has been excavated to this depth scientifically on the west side - Hurst's work was mainly in the centre and east of the island). There is even evidence for raising of the ground level at the centre of the island at this period.

The only contrary evidence known to me is the existence of other, parallel timber-period trenches within the main 6 m intervals.

The evidence from Carthage confirms me in the interpretation of a feature of some remains of shipsheds at the south end of the little harbour (Mandraki) in Rhodes, at the north end of street P31, just north of and partly overlapped by a Roman tetrapylon erected over the junction with east/west street P6 (Figure 2). Excavations during the Second World War (never completed and never published) revealed remains of seven north/south lines of piers or walls which clearly belong to shipsheds. Unrecorded removal during the excavation of most of the levels between these lines has made it very difficult to reconstruct the original sequence, but there were at least two phases:

- (i) Three rows of piers, at intervals of 7.35-7.85 m (clear width 6-6.3 m) west of a solid wall (D); and east of the solid wall three rows of piers at intervals of 5.45-5.94 m (clear width 4.20-4.40 m — no doubt for smaller ships). Depending on the height of sea level in (probably) the late third century BC, these shipsheds could have been 40-45 m long, with a slope possibly of as much as 1 to 4.6 (at least at the upper end). This phase belongs probably after the earthquake destruction of 227 BC.
- (ii) In the second/first century BC the shipsheds were rebuilt at a higher level. Ramps were built over and round the solid wall (D) and round at least one line of piers to the east (C), and probably two (C & B); the piers apparently remaining in use as column bases (figure 3). The ramps have a slope of at least 1 in 4 over their surviving length; again, this was near the upper end of the shipsheds, where the slope may have been greater (John Coates feels that this would have suited the after keel, say 10 m long from the after cut up of the keel to the after end of the ship, and would go well with a keel lying lower on the slip on a slope of 1 in 10; this idea cannot be tested without excavation).

The ramp was stepped (laterally), and in the lower of the two steps surviving slots were cut: 6 on the east side of ramp D, with a width of ca. 25 cm. and variable depth; 2 on the west side of ramp D; and 3 on the east side of ramp C, ca. 15-20 cm wide. (Figure 3). The ramp is missing on the west side of C, probably dug away during the wartime excavations, and no remains of ramps have been found on line E or line B (though I presume that one did exist on at least the east side of line E and the west side of line B); we therefore do not have slots surviving on both sides of any one shipshed, and cannot check if they line up. Assuming that they did, I had always favoured the explanation that the slots were to receive timber sleepers, probably on the surviving evidence set in the top of a solid ramp. The only other interpretation of the slots would be that they were to hold the timber shores which would have supported ships when slipped. These must have been used *pace* the hesitancy of Morrison & Coates, AT p. 221 "before any bow or stern shores are knocked away"; but Coates comments to me "bow and stern shores, several of each, would most certainly have been set up when a ship

was stored on a slip; during that period the *hypozoma* would have been relaxed or completely unrigged"); and I still believe that the word *parastatai* in the Naval Lists refers to shores (as GOS pp. 183, 293), not mast partners (as AT p. 160 n.1).

A final answer will depend on further excavation down the length of the slips, which are now, unfortunately, overlaid by houses of the period of the Knights; but the discoveries at Carthage have already provided a valuable parallel.

I have earlier suggested (GOS p. 185) that the rock-cut side ledge on either side of the Sounion slips may have held wooden runners. This cannot be proved, but it seems the sensible interpretation; Coates agrees in "believing that the steps would have been timber clad to make bilge groundways, either to take the weight of the ship (improbable) or to support stern poppets or a stern cradle about which the ship must hinge as the bow rises under its buoyancy on launching" — particularly important, I feel, with such a very steep slip.

I am tempted to see further evidence for the covering of slipways in an obscure reference in a fragment of the fifth-century Athenian Comic poet Cratinus (frag. 197 Kock; quoted by GOS p. 191 n. 24): "the triremes despite all their efforts cannot get (to) shipsheds and reed". How do we explain "reed"? Is it for roofing or fencing? — this does not seem to fit, though Pollux (X 184) preserves the quotation from Cratinus as an illustration of the use of reed in fencing. Is it for caulking? Pliny refers to this use of reeds (NH 16.158, quoted in SSAW p. 209 n. 39): "pounded and inserted in the seams of ships, (they) solidify the structure, being more tenacious than glue and, for filling cracks, more reliable than pitch". Or is it for matting, laid on the slip? Liddell-Scott-Jones suggest "reed mat" for its meaning here (cf. GOS p. 188).

Some curious remains found in Munychia and published by von Alten in 1881 may be relevant (GOS p. 181 & note; *Evidence* p. 128 and fig. 25). The parallel lines of blocks apparently slope seaward, and are now fully submerged at the top. If they *are* remains of shipsheds, then three explanations are possible: (i) if we assume a relative rise in sea level, then the upper part of the slip would be in the dry, but we should have to assume very short slips; (ii) if we assume that timber skidways were laid on the lines of blocks, this would raise the level, and one could perhaps project the theoretical length a good deal farther seawards; or (iii) a combination of (i) and (ii). I must emphasize that only one "slip" extended for any distance seawards, and doubt was expressed soon afterwards about the nature of the remains found. Angelopoulos noted the difference in the gradient reported by von Alten for the Munychia remains (2° - 3°) from the gradient of the Zea remains (7° - 8°); he assumed the slipped ships were not completely in the dry in Munichial! (Περὶ Πειραιῶς καὶ τῶν λιμένων αὐτοῦ, Athens 1898, 50-1, 124-5 & Fig. A.3-4).

The question of the bottom ends of shipsheds and their slips remains a vexed one. The only shipsheds where the bottom end has (supposedly) been firmly established are those at Apollonia, and I am now less convinced than before that their original length was "just under 40m", since because of their slight gradient (1 in 14) we are left with a dry length of only ca. 28 m; or am I over-influenced by the preconceived idea that they "ought to" have a dry length of 35-40 m because they have the "normal" clear width of 6 m? Elsewhere we are guessing: the foot of the slips has been broken away e.g. at Matala and Siteia, or covered by sand or later buildings e.g. at Sounion and Rhodes.

At Piraeus it is best to confine our discussion to a small group of a dozen excavated by

Dragatzis and planned by Dörpfeld. The plan shows the columns continuing into the water (for some 5 m); the lower end was nowhere established. The longitudinal section, oddly, shows the slip ending at sea level. Although in fact the bed of the slip was not preserved in its lower part in the one shipshed excavated down in its entire length (and part of its width), it is obvious that such a reconstruction is impossible.

The slip must have continued into the water at least another 10 m, if we assume (i) a depth of 1 m at the foot for the trireme to float in and enter the slip stern first, and (ii) no change of sea level since antiquity. On the latter point I can only repeat what I said 20 years ago (*GOS* 182 n.1): "the possibility of a change in the relative sea level since antiquity has not always been taken into account in discussions of the shipsheds, though if it were established it would affect all the length measurements of the shipsheds and any conclusions drawn therefrom on the dimensions of the *trieres*, and also the general picture one tries to form of what ancient military harbours looked like". I have therefore to agree with Basch, particularly since he uses my words as the basis of his argument, that the measurement 37 m has been assumed too readily as a fixed point by many, including Morrison & Coates (Basch, *Mariner's Mirror* 73.1, 1987, 94; see now a response by Morrison & Coates, *IJNA* 16.2, 1987, 168-70: they still maintain that "the length of the vessel is determined more critically by three factors other than the length of the sheds" — but the sheds *were* built to house *these* ships). I would emphasize on the other hand that there is no firm evidence that the dry length of the Zea slipways was *not* 37 m — just that caution is necessary; also I feel that evidence *is* now accumulating of a standard measurement in the range 40 - 45 m. If Curtius' report *were* correct (see *GOS* 182 n.1) that in Zea basin the harbour bottom falls away sharply at a depth of 6 feet all the way round, then we have a maximum original length of slip of ca 55 m and a maximum dry length of slip of ca 45 m. If Curtius' report were correct, we cannot assume that the slips ran out to the edge, but it is plausible.

This shows how important it was and is to know all we can about the submerged remains in Zea and Munychia. Graser claimed to have measured 38 shipsheds in Zea and 9 in Munychia and this has never been adequately checked. I wonder how much is left to be checked after modern dredging and construction work, and I must ask the question: what records were kept of finds and observations during those operations? It is important now, with the prospect of a reconstruction project of a shipshed for the *Olympias*, that areas of the shore of Zea and Munychia which remain unscathed are checked again, above and below water level, to see if any further information can be obtained, notably on the questions of (i) underwater length of slip and (ii) evidence for the use of timber on the slips.

There would also be value in following up the ideas of FSD. One test I have already suggested — with the *Olympias* on a slip. Their friction tests should be repeated and expanded, to include as comprehensive as possible a range both of species of wood and of lubricants: of species of wood known to have been used for building ancient hulls and keels (fir, pine, oak, beech, cedar?, cypress?); and of lubricants and coating materials for which there is ancient evidence (pitch, pitch and wax, wax, tallow, resin, bitumen?); tested on wet and dry stone (limestone as well as granite) and on wet and dry wood (of various species, laid as skids and as sleepers). Then we should be better equipped to look again at the questions of man-handling a trireme, and the need (or not) for mechanical aids, and the alternative readings of the Athen-

ian inscription: 40 or 140 men minimum for hauling out a trireme, 20 or 120 minimum for launching a trireme.

Throughout this discussion I have concentrated on the question of slipping, or more generally beaching, ancient warships. It is worth remarking, in conclusion, that though ancient merchant ships were not slipped in shipsheds, they certainly were beached (*pace* many scholars). One need only recall the passage of Theophrastus referred to above (*HP* 5.7.2): "Triremes have an oak keel to endure hauling ashore (νεωλκία), merchant ships have a pine keel (πεύκη) but they place under [it] also an oak keel when they are hauling it ashore (ἐπὶ νεωλκῶσι), and smaller ones a beech keel; and the false keel (χέλυσμα) is totally of beech." Casson translates the key phrase "but they put on an underlayer of oak" (*SSAW* pp. 212-13 n.51). I do not quite see how this is to be distinguished from the false keel referred to in the next sentence. Are we dealing here with an (obscure) reference to a cradle? or simply to a timber runner laid on the slip, possibly in a "keel slot"? This is not clear, but it is clear that merchantmen were beached. (Morrison & Coates, *AT* p. 181, translate χέλυσμα as "breastwork" [of the bows], which avoids the problem but goes against the explicit definition of the word by Pollux (1.86); *SSAW* p. 221 quotes the evidence). Even Hiero's gigantic *Syracusia*, later called the *Alexandris*, was eventually, after finding no ports to accommodate it, beached at Alexandria (Athenaeus, 5.209b, quoted in *SSAW* pp. 194, 199; Casson rightly takes the story of the ship seriously; he translates ἐνεωλκήθη as "docked", but the ship was clearly "hauled out of the water"). A fragmentary inscription of the later third century BC from Thasos quotes port regulations concerning the size of ships which could be "hauled out" within the harbour limits (*IG* XII Suppl. No. 348). We must allow for light timber structures on the shore of commercial harbours in antiquity or, at least for light boats and warships, on any beach site which was frequently used — just what one still sees in the Mediterranean today. Traces of such structures would have disappeared, or at least have not been found or recognised.

What operations could be carried out in the shipsheds?

FSD assume that a trireme could have been tipped over within the shipshed for such purposes as pitching the bottom (314, fig. 3); this seems inherently unlikely, and indeed would be necessary only with a ship as flat-bottomed as that proposed by FSD. The wine-glass mid-section of the *Olympias* provides for the bottom to be accessible when the ship is upright. Work on the outside of the hull would be cramped but feasible (compare *AT* p. 135, fig. 35b). Caulking and pitching would probably have needed to be repeated after all but the briefest periods on a slip. Most work on the inside of the hull could have been carried out in the shipshed. Certain operations may have been carried out outside the shipsheds with the ship afloat or slipped in the open air. This leaves unanswered a major question:

Where were ancient warships (and merchant ships) built?

In Piraeus no remains have been found which can be linked to shipbuilding. Lehmann-Hartleben (p. 119) suggested Eetioneia as the site of the shipbuilding yards — a theory incapable of proof or disproof since that area of the main harbour has been completely redeveloped, but it remains plausible. We do have one reference in a late Athenian inscription (*IG* II² 1053.43:

first century BC) to *psyktras tas pros tois neoriois* — "drying-places close to the dockyards" (or "shipsheds" — the word *neoria* is sometimes to be translated thus): I would imagine these to be sites where ships were careened, caulked and pitched, somewhere just west of the main port. The main impression one has of *naupegia*, however, is that they were in the *neorion*. There are several references to the Athenian *naupegia*, even perhaps the *Telegoneia [naupegia]* (IG II² 1611.130-3); in a fifth century Athenian inscription the *trieropoioi* are to deposit material in the *naupegion*. But we get no idea of what the *naupegia* were like. (For the details of the literary and epigraphic evidence, and the question of public or private shipyards, see B. Jordan. *The Athenian Navy in the Classical Period*, [University of California Publications: Classical Studies, vol. 13], 1972, 46-54).

Some indications may be given by the large open area around the pair of slips found at Thurii: here we have clear evidence of a double timber cradle, and here at least there was an unusual amount of space (6.2 m) on either side of the slips (which together measure 12.4 m in width: Plan reproduced in *AHM* p. 205 fig. 12). The Oeniadae slips seem to have an open space beside the group at one end, but this looks more like normal storage space for slipped warships; fuller investigation is needed here and we look forward to hearing more from W. M. Murray.

A side chamber by one of the slips at Dor has been described as a rock-cut basin which could have been used for pre-soaking timbers (I followed this interpretation in *AHM* p. 211 n. 114); but on inspecting it I am not convinced that it was anything other than a storage chamber — there is no evidence that it contained water. Poidebard suggested that the eastern end of the "South Harbour" at Tyre was closed off by a shipbuilding yard and repair basin (with a provision store and fresh-water tank nearby), but strong doubts have been expressed about its identification as a harbour.

Flemming reported "ship-formed depressions" in the rock at Apollonia, at the north-west corner of the "inner harbour", inside the "Grotto reef", and suggested that these may have been part of a shipyard. He speaks of "an area of rubble from which a set of parallel walls lead down to the ancient harbour (Fig. 14, C.5). The walls are cut out of the solid rock and are badly weathered, so that the upper part tapers slightly, but it is clear they separate a series of four flat-bottomed bays with floors at a depth of about 2.4 metres. The bays are 18 metres long and 4 metres wide, and though the floors are flat, the tops of the walls slope down towards the ancient harbour, being nearly 2 metres high at the western end and only 75 centimetres high at the eastern end. In both the southern bays the floors are indented with boat-shaped hollows about 10 metres long, and the southernmost bay has square slots of side 15 centimetres cut into the top inner edge of both its walls at 1-metre intervals."

"It is quite impossible to be certain about the use of this strange assembly of walls and hollows, but perhaps a little speculation will do no harm. The proportions of the bays make them suitable to accommodate boats slightly smaller than those which used the slipways and the quays, but the bays themselves are obviously neither. The niches in the walls of the southern bay were probably to take the ends of beams of wood lying across from wall to wall, and the set of beams 1 metre apart would then have made a strong surface with a slope of 3 to 4 degrees towards the harbour. This would have made an ideal slipway up which to drag light boats for easy access to the bottom of the hull. Anyone who has seen fishermen scraping and painting boats at low tide will know that a craft of 10 to 15 metres with a 4-metre beam is far too heavy

to turn over, and that it is very difficult to get at the keel and bilge keels. A light raised slipway may have been just the answer to this problem, and the boat-shaped depressions in the floor may have been intended to allow a bit of extra headroom. It is also possible that boats were actually built on these sloping rests." (*Cities in the Sea*, 1971, 108-9).

If Flemming's interpretation is correct, then one should bear this possibility in mind for other slipways, except where there is clear evidence that sleepers rested directly on a solid slip.

In the Western Mediterranean the harbour of Pandateria might have offered some evidence if it had been properly studied before being almost completely built over (compare (a dismal experience) the plans and views in L. Jacono, "Un porto duomillenario", *Atti del III Congresso nazionale di studi romani*, 1933 (Bologna, 1934) Vol. I, Tav. XLVI, fig. 2, XLVII and Schmiedt, *Livello antico* (1972), pp. 176 ff.). The "harbour" entrance at Motya may have been used for shipbuilding or repair in a late phase. It would be interesting to know more of the mysterious structures at Fos-sur-Mer — 2 complexes of 156 blocks in 6 parallel rows (6 × 26) which could have been the bases for timber posts or stocks. All that has been published is a sketch plan, with inadequate information on the dimensions (L. Monguilan & others, *Archéologia* 110, 1977, 59-65). There is certainly nothing as convincing as the remains of a late 3rd century BC shipyard found in Canton (Chao Lei, *Archéologia* 118, 1978, 70-1).

The question remains: what precisely are we looking for? Rows of bases for stocks? We have the ancient Greek word *dryochoi* — "oak-holders", for the keel was traditionally of oak; and a picture on Longidienus' tombstone (ca. AD 200: SSAW Fig. 163). Basins for pre-soaking timbers? It is hard to find other diagnostic criteria to suggest. The Thurii complex as a whole looks plausible.

Was it normal to launch ships when part-built, like Hiero's *Syracusia* (SSAW, p. 195)? This would be quite feasible, once the hull was structurally complete; Theophrastus tells us that new constructions were let stand until they had set, then launched so that the wood closes up and becomes watertight (*HP* 5.7.4, quoted in SSAW, p. 205 n. 21). Casson has pointed out that early launching could speed up completion of the job, since work on the superstructure could be done with the ship already in the water for the wood to close up. Where would the last stages have been carried out? The quays in the inner harbour at Apollonia may provide an explanation (E9 on Flemming's plan: loc. cit. pp. 100-01 Fig. 14, 105), but they are only 3.5 m apart. Again one suspects that at least part of the answer is "light timber structures on the beach".

I conclude with another question: where in a wider sense were triremes built? If a Greek city had timber sources close at hand, then obviously the ships would be built in the city's own shipyard; but what if the timber sources were distant?

The *communis opinio* is that in these cases the timber would be shipped or towed for construction "at home". Thinking in particular of the case of Athens, I do not find this answer fully satisfactory. Evidence to support my doubt is provided by an inscription in which the Athenians honoured King Archelaos of Macedon (*IG* I³ 117; R. Meiggs and D.M. Lewis, *A Selection of Greek Historical Inscriptions*, 1969, no. 91; previously *IG* I² 105); it contains a specific reference to the occasion, probably (following Meritt's studies) in 407/6 BC, when Archelaos seems to have allowed Athenian shipwrights to go to Macedon to build ships there. This reference has been explained away as an exception, justifying an honorific decree — a valid point; ac-

ording to Meiggs (*Trees & Timber in the Ancient World*, 1982, 228) the timber would normally have been shipped to Athens, but at this crucial period of the Peloponnesian War "merchantmen under sail with heavy timber cargoes would have been more vulnerable to interception by an enemy than oar-powered triremes". Precisely on these grounds I wonder whether shipbuilding near the timber source may have been the norm, not the exception, at least in wartime.

Now Honor Frost has provided a new element for the discussion: a large group of the stone anchors of the triremes are of a dark stone not found in the Athens area, but found, she argues, in Thessaly and Chalcidice: and she reports finding a similar anchor in Volos Museum ("*Les Constructeurs Puniques*" in Symposium on *Flotte e Commercio Greco, Cartaginese ed Etrusco nel Mar Tirreno*, Ravello, Centro Universitario Europeo per i Beni Culturali, 19-25 January 1987).

If it is true that the source of the stone can be so precisely defined, then we have a little more evidence for Athenian ship construction in Macedon/Chalcidice, or perhaps in the region of ancient Pagasai, using timber from the slopes of Pelion and Ossa. Anchors are unlikely to have been imported from Macedon or Thessaly to Athens for ships built at Athens, but if ships were built in Macedon or Thessaly and brought down to Athens by skeleton crews (sailing and if necessary even rowing), they would have needed anchors from the start. The Thessalians were allies of Athens for most of the fifth century BC, and rather more reliable allies than the kings of Macedon (the Athenians were always trying to pin down kings such as Perdikkas, e.g. to deliver oar timbers exclusively to the Athenians: *IG I³ 89.31*, variously dated 435, 431/0, 423/2 or 417-13; the later dates seem preferable). However, not too much should be made of this until we have firmer evidence about the source of the stone for the anchors.

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ANNEXE I

The naval decree as published in the second and third editions of *Inscriptiones Graecae*.

FIGURES

1. Carthage: Reconstruction of the Punic shipsheds. (Hurst 1979, fig. 3, with the permission of the author).
2. Rhodes shipsheds: Plan of the surviving remains.
- 3a. Rhodes shipsheds: Row D from the west. Two slots are visible in the second course of the ramp (phase ii). On the right, the solid wall of phase i. Beyond, Row C.
- 3b. Rhodes shipsheds: Row D from the east. Slots visible in the second course of the ramp.

IG¹ 153 DE TIREMIBVS. Piraei, nunc EM 6617. Stela venis micae insignis, a dextra et partim a tergo integra, a. 0,466, l. 0,268, cr. 0,11.

Lit. Att. formae variae ΕΚΚΛΠΠΣΦ, a. 0,009-0,011, O 0,008; στοιχηδόν 0,0121, 0,0103.

Edd. IG¹ 77; IG¹ 73 c. add. p. 302 + (c. suppl. ex parte Kirchneri). Ect

σ. 440-425

IG¹ 73 DE TIREMIBVS. Piraei. Stela a dextra integra. Nota formam Φ. Ed. IG¹ 77; cf. Frenkel Neue Jahrb. 1888, 475; Kolbe Afa. Mitt. XXV 1902, 417; B. Keil Anon. Arg. 217. Ect.

.....²⁰.....¹²..... στοιχ. 33.
 [.....¹⁷..... Τ. ην τρι[ερ.....¹.....]
¹⁹..... ΝΕΟΝ ΕΚ
 [.....¹¹..... ἄσι δὲ τριεροποιοὶ κ[αι] ἄσι]
 5 [ΤΡΙΑΤΑΝΕΣ ἄσι(?) ἄσι ἄσι]ΓΡΑΦΟΝΤΟΝ Τ[ὸ]ς Τρι-
 [ΕΡΑΡΧΟΣ. ὁ ἔχσέτο δέ] ΜΕΦ' ἑΝΙ ἈΝΕΑΚ[ΕΝ ἈΝ]-
 [ΑΡΑΣΙΝ ἘΛΑΤΤΟΙ Ε ΤΕΤ]ΤΑΡΑΚΟΝΤΑ ΚΑΙ [ΚΑΘ]-
 [ΕΚΕΝ ΕΣ ΤΕΝ ΘΑΛΑΤΤΑΝ] ἘΛΑΤΤΟΝ Ε ΕΙΚΟ[ΣΙ] Ἀ]-
 10 [ΝΔΡΑΣΙ ΜΕΔΕ ΠΙΤΤΩΝ Μ]ΕΔΕ ἈΥΠΟΖΟΝΥΝΑ[Ι] ἘΛ]-
 [ΑΤΤΟΝ Ε ΠΕΝΤΕΚΟΝΤΑ Ἀ]ΝΔΡΑΣΙΝ ΜΕΔΕ ΠΕΡ[Ι]Α]-
 [ΛΕΙΘΕΝ] ἘΛΑΤΤΟΙ ἈΝΔ]ΡΑΣΙ Ε ἙΚΑΤΩΝ ΜΕ[Δ] Ἀ]-
 ΦΟΙΡΕΘΑΙ ΚΕΥΟΣ ΜΕ]ΔΕ ἑΝ, ΜΕΔΕ ΤΕΝ Θ[Ρ] [ΜΕ]-
 [ΗΝ ΕΠΑΓΓΕΛΑΕΤΟ Ἄ]Ο ΤΡΙ]ΕΡΑΡΧΟΣ ΚΑΙ ἌΟ Κ[Υ]ΒΕ]-
 [ΡΝΕΤΕΣ· Ε ΔΕ ΘΥΛΕ Ε Ο]ΥΝΕΙΣ, ἌΠΟΣ ἈΝ ΤΑΥΤΑ Γ-
 15 [ΙΓΜΕΤΑΙ ἌΟΣ ΚΑΛΙΣ]ΤΑ, ἘΠΙΜΕΛΕΣΘΟ. ἘΑΝ ΔΕ
 [ΤΙΣ ΤΟΥΤΟΝ ΤΙ ΠΑΡΑΒ]ΑΙΝΕΙ Ε ΤΡΙΕΡΑΡΧΟΣ Ε
 [ΚΥΒΕΡΝΗΤΕΣ Ε ἌΛΛΟΣ] ΤΙΣ, ὄφελέτο χιλιά[ς] Δ.
 [ΡΑΧΜΑΣ Ἱεράς Τῆ] Ἰθηναιαί καὶ ζεμιόντ-
 20 [ΟΝ ΑΥΤΩΝ Ἄοι Ἐπιμε]λόμενοι τῷ νεορίο. τ[ὸ] Δ-
 [Ε ΟΥΕΙΣΜΑ ΤΟΔΕ Ἀ]Ν]ΑΓΡΑΦΑΕΤΟ ἌΟ ΓΡΑΜΜ[Α]Τ]-
 [ΕΥΣ ἌΟ ΤΕΣ ΒΟΛΕΣ Ε]στ[ί]ται λιθίνει. Ἄοι Δ[ε] κ-
 [ΟΛΑΚΡΕΤΑΙ ΔΟΝΤΟ]Ν Τῶ Ἀργύριον. Ἄοι Δε π[ο]λ-
 [ΕΤΑΙ Ἀπομισθο]σάντων].
 τ

.....²⁰.....]ο[.....¹².....] ΣΤΟΙΧ. 33
 [.....¹⁹.....]ν τριε[ρ.....¹.....]
 [.....¹⁴.....]ν νεδὸν ἐκπ[η]λο ...¹...]
 [.....¹⁹..... ἡοι τρι]εροποιοὶ κ[αι]...¹...]
 [.....¹..... τὰ ὀνόματα] γραφόντων τῶ[ν] τρι]-
 [ερόρχων· μεδ' ἔχσέστο] μεδ' ἑνι ἀνεακ[ύ]σαι]
 [ἀνδράσι ἔλαττων ἔ τε]τ[τα]ράκοντα καὶ [ἡεκ]-
 [ατὸν, μεδὲ καθελκύσαι] ἔλαττων ἔ εἰκοσι κ[ι]-
 [αὶ] ἡεκατὸν ἀνδράσι, μ[ε]δὲ ὑποζονύνα[ι] ἔλ]-
 [αττων ἔ ...¹... κοντα ἀ]νδράσιν, μεδὲ περι[ο]-
 [ρμίζεν ἔλαττοσι ἀνδ]ράσι ἔ ἡεκατὸν, με[ι]δὲ
 [.....¹⁵..... μ[ε]δὲ ἑν' μεδὲ τέν ορ[...]
 [.....¹⁹..... ἡο δὲ τρι]εραρχος καὶ ἡο κυ[βει]-
 [ρνήτες ἐκάστες τές] νεός ἡόπος ἀν ταῦτα γ-
 15 [ίγνεται ἡος κάλλι]στα ἐπιμελέσθο· ἔάν δέ
 [τις τούτων τι παραβ]αίνει ἔ τριεραρχος ἔ
 [κυβερνήτες ἔ ἄλλος] τις, ὄφελέτο χιλιά[ς] δ-
 [ραχμᾶς ἱεράς τῆ]ι Ἰθηναιαί καὶ ζεμιόντ-
 [ον αὐτὸν ἡοι ἐπιμε]λόμενοι τῷ νεορίο. τὸ δ-
 20 [ε φρέψμα τόδε ἀ]ναγραφάστο ἡο γραμμ[α]τ-
 [εὺς ἡο τές βολές ἐ]στ[ί]ται λιθίνει· ἡοι δ[ε] κ-
 [ολακρέται δόντο]ν τ[ὸ] ἀργύριον· ἡοι δὲ πολ-
 [εταὶ ἀπομισθο]σάντων
 v[er]cat
 v[er]cat 0,14

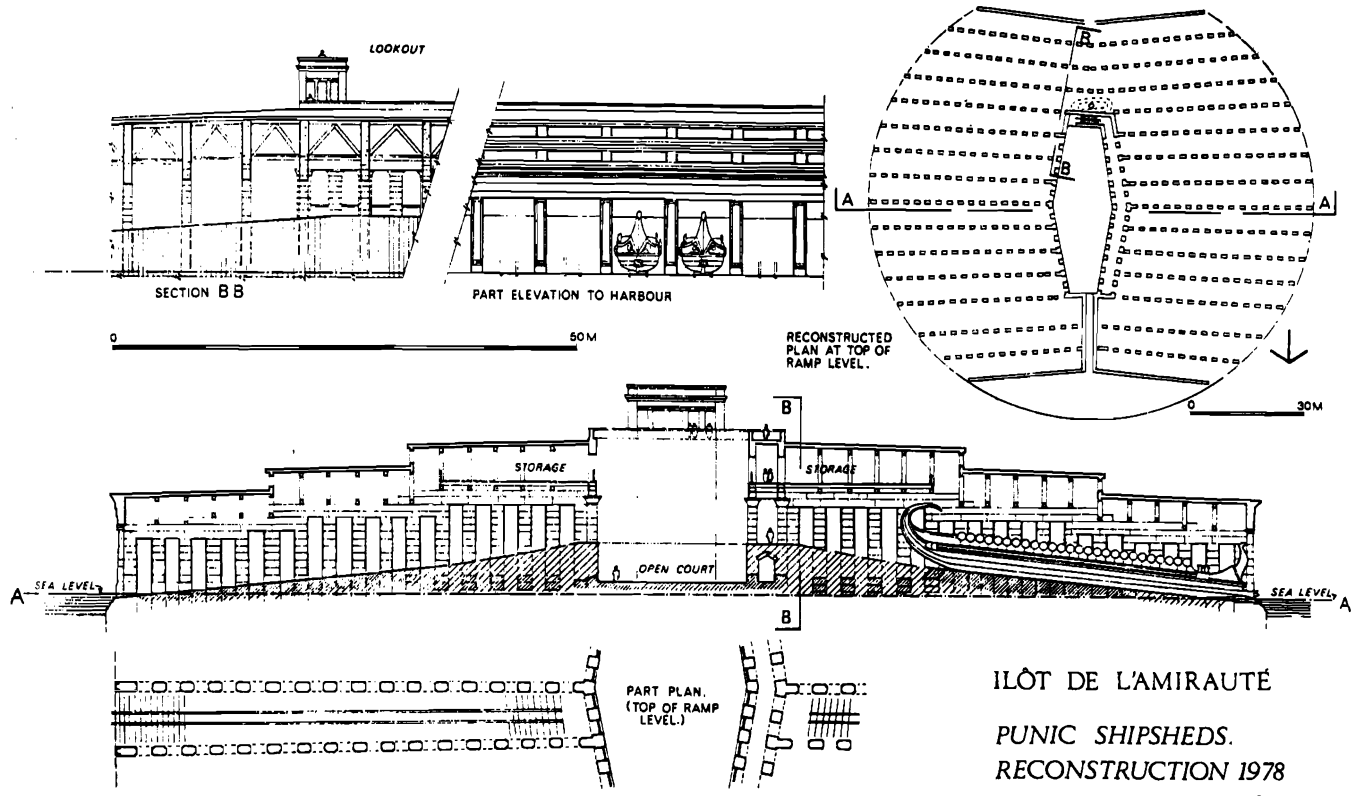


FIG.1.

ILÔT DE L'AMIRAUTÉ
 PUNIC SHIPSHEDS.
 RECONSTRUCTION 1978

SG.



3a



3b