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## THE CITY OF DOR AND WENAMUN

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Very recently we have seen the publication of S. A. Kingsley and K. Raveh, *The Ancient Harbour and Anchorage at Dor, Israel* (TEMPUS REPARATUM, BAR International Series 626, 1996), which reports on the underwater surveys carried out between 1976 and 1991 along that coast by a number of archaeologists, including themselves. The detailed examination of the objects found underwater and compared to similar objects found elsewhere will greatly add to the knowledge of this city which has been recorded so far. The archaeological work carried out on land at Dor for fourteen seasons has been recorded elsewhere by Ephraim Stern,<sup>1</sup> and we have also a number of important reports by Avner Raban.<sup>2</sup> In this book it is clear from their extensive bibliography that Kingsley and Raveh spent a considerable time studying the objects, which is something the earlier reports had not dwelt on, as they concentrated on the site itself.

The purpose of this book was to throw more light on the geographical characteristics and history of this shoreline as a harbour and as an anchorage by collecting the objects found on the sea floor and making them the primary focus of attention rather than the site itself. The investigators had the advantage of comparable objects from work on the mainland. Yet there were some interesting exceptions to this, marked by a gap in the material. The report is very well illustrated and includes detailed maps of the underwater areas where the objects were originally situated.

The most notable result of this work so far, I believe, is the publication of a very substantial number of anchors and their parts, both of stone and of iron, which will enrich the growing corpus of material in this field.<sup>3</sup> The authors also gave their serious attention to millstones and their chronology, and to the metal artefacts including tools, which, underwater, are always found with thick concretions of sand, shells and small pebbles, quickly reducing the objects to a poor state of preservation. They also pay considerable attention to the amphorae and the coarse pottery from the site, regretting the fact that a corpus of imported amphorae has not yet been compiled for Palestine which appears to be lagging behind the European work in this field. Raveh and Kingsley tell us that "the Dor material contains the most diverse and numerically extensive range of Late Roman and Byzantine amphorae so far published from Israel" (p. 46).

Dor harbour has already yielded one of the largest assemblages of ancient anchors in the Mediterranean, the only other assemblage to rival this being from Atlit, not very far north along the same coast.<sup>4</sup> Dor has produced 143 one-hole stone anchors, 6 two-hole stone anchors, 6 three-hole stone anchors as well as 32 iron anchors, which reflect the considerable activity along that shore during the Roman and Byzantine periods. Our own finds of heavy stone anchors at Marsa Matruh along the Mediterranean coast of Egypt do not match these numbers<sup>5</sup> and the limited physical area on the bay of Marsa Matruh where our pierced stones were found did not include iron anchors at all. All our finds were swept into land (and out again!) by the sea, but with the similar problem of having no stratigraphic evidence for dating them.

Kingsley and Raveh describe their anchors from Dor in considerable detail (pp. 29-41) and compare them to similar finds elsewhere wherever this is possible. They comment on the fact that even when a group of stone anchors appears on the sea-bed in isolation, indicating that they had all been lost from one ship, they are never identical in shape and weight. This means that their provenance was often from different workshops and procured at different times. No two stone anchors are ever exactly the same, anywhere.<sup>6</sup>

The photographic record gives us a good idea of the surfaces of these stones (see Plates 23-40). Sean Kingsley's comments on the typology and significance of these anchors are to be found in Appendix A (pp. 87-94). Not surprisingly he agrees that we need a wider geographical documentation for these pierced stones before we can classify them satisfactorily in any category.<sup>7</sup> Nevertheless some of our Mediterranean stone anchors do have decisive characteristics which set them apart from others. We cannot do less than recognize these characteristics while at the same time refrain, for just a little while longer, from attaching them to national or political states until we have more data.

No mention is made in this study about the possibility of identifying the provenance of these anchors by analysing thin sections of the stone itself as Honor Frost suggested many years ago. This would at least give us some possible sources of origin, perhaps close to the sea or to the navigational channels and would rule out other areas as sites for their manufacture.

Considerable attention is paid in this report to the thirteen shipwrecks situated at the entrance to the South Bay at Dor, which the investigators suggest date from the 13th century B.C. to the late 18th century A.D.<sup>8</sup> In at least two of the wrecks, the main cargo seems to have been ashlar masonry,

which can clearly be distinguished from ballast (p. 56). Other cargoes appear to have been perishables.

It is interesting that the study of these wrecks led the authors of this report to the conclusion that the *maximum* length of the merchant vessels operating within the shallow local waters of Dor was from 16-18 metres (p. 80). Many of the vessels would have been smaller. We may recall that many ancient and mediaeval sea-going ships could measure little more than ten metres. The Kyrenia vessel was nearly fourteen metres in length and the one from Yassi Ada was twenty-one metres. In earlier times few vessels appear to have achieved the length of 43 metres, as is the case with the Cheops vessel, which was destined to sail on the Nile.<sup>9</sup> The iconographical evidence from Egypt indicates that a number of large vessels were used on the river to transport quarried stone blocks and obelisks of great weight.<sup>10</sup>

However, the shallow waters around Bates's Island at Marsa Matruh, which cannot have been more than about five feet in pharaonic times,<sup>11</sup> demonstrate that sea-going vessels which brought the Late Bronze Cypriot and Mycenaean ware to that island can have been no heavier than about forty tons, and thus unsuitable for transporting long and heavy loads even on calm seas. In his discussion of the size of ships in the ancient world, Lionel Casson considered a vessel of sixty tons to be "absurdly" small.<sup>12</sup> Compared to the evidence he gives for the classical world and particularly for the Roman grain carriers which could be 340 tons, an eighty-ton ship was a small one. Yet we must take into account the ports into which they were expected to load and unload their cargoes. While it is true that many harbours have silted up only after the Hellenistic period, there are others which could never have received the very large vessels described by Casson.

If vessels of a maximum of 16-18 metres operated within the waters of Dor, then where are we to suppose that the vessels went which are said in the textbooks to have sailed to the Lebanon from Egypt in order to obtain their timber for building ships? Clearly tree-trunks carried on a vessel of 18 metres in length could never have been as long as the tallest trees mentioned in the ancient records for use in the construction of vessels and their masts.<sup>13</sup> It is evident that such timber could not have been carried by ship along that coast. Otherwise we should be finding on the sea-bed as many ancient tree trunks as stone anchors. I shall not repeat here what I have said in my two articles on *cedar* in recent years, in which I pointed out that many errors were combined in the textbooks to give us a totally false picture of the ancient Egyptian situation with regard to cedar and timber in general, and of Byblos as a

shipping port.<sup>14</sup> It is now clear that we must consider Byblos a port to Egypt only for religious reasons and not commercial ones.

When speaking of the refurbishment of the city of Caesarea near Dor by Herod, Josephus (l. 407) tells us that until then, the coast between Dora and Joppa "had been harbourless, so that anyone sailing along the Phoenician coast towards Egypt had to ride the open sea when threatened by the southwest wind; even when this was far from strong, such huge waves are dashed against the rocks that the back-wash makes the sea boil up a long way out." Josephus must then have been speaking of international shipping rather than of small, local merchant and fishing vessels.

Our understanding of Dor is handicapped by the fact that archaeologists keep insisting that the city goes back to the 20th century B.C. However, the evidence for this is not forthcoming.

After more than twelve years of excavation in the soil of Dor, Ephraim Stern concluded that the site represents "four different civilizations (as opposed to historical periods) ..... the Canaanite; that of the Sikil tribe of Sea Peoples; of the Phoenicians ..... and the Hellenistic-Roman culture".<sup>15</sup>

For some years now, I have been asking for a definition of *Canaan* in order to find out what essential material makes an archaeological site a Canaanite one.<sup>16</sup> Are the criteria cultural, geographical or chronological? There has been no response so far. According to Stern the Canaanites were the original settlers at Dor from the beginning of the second millennium B.C.E. and according to him they dominated the entire Near Eastern region. But he does not define *Canaan* or *Canaanite*. He says:

Canaanite material culture, well-known from scores of excavations throughout the region, is exceedingly rich and varied, especially in its later phase which overlaps the Egyptian conquest of Palestine (1500-1200 B.C.E.). It was a time of far-flung international trade which strongly influenced the local cultures. The most prosperous Canaanite communities seem to have been concentrated in the coastal towns and in their immediate hinterland — Ugarit, Byblos, Megiddo, and others. During this long period of Canaanite hegemony Dor was one of the important harbor towns along the Mediterranean coastline of the country. The well-preserved state of the site portended good possibilities of recovering invaluable evidence of all facets of the Canaanite period. Because of the great depth of the remains, our excavations have not yet reached these strata, and even had we been able to do so, we would most

probably only have provided additional details of an already well-known and familiar culture.

(Stern, 1994, pp. 19-20)

Up to the time his book was published in 1994, Ephraim Stern had found no Bronze Age levels at Dor and he admits that even from his Iron Age I period (1150-1050 B.C.E.) "we still know more from the historical sources than from the excavations" (p. 85). This is a dangerous admission.

Are we therefore speculating about the cultures involved here? And has anyone yet produced evidence for the "Egyptian conquest of Palestine"? I believe this to be an assumption repeated naively and thoughtlessly by one scholar after another.

Avner Raban has also contributed substantially to the discussion on the history of Dor. He speaks of the "marine structures of the 13th-12th centuries at the south fringes of the ancient mound".<sup>17</sup> He goes on to remark that during the earlier part of the 13th century B.C. the sea level at Dor was lower than the present one by half a metre or more. Can we be quite sure of this date, however? He also tells us of the existence of a safe anchorage "at the lee of the line of the then connected rocky islets of Tantura. A large number of Bronze Age stone anchors and pottery to be dated from 17th to early 12th centuries B.C. — including what might have been part of a cargo of conical jars around 1200 B.C. — were found at this protected anchorage."<sup>18</sup> He therefore concludes that during the 13th century B.C. new settlers came to Dor.

We are assuming that the diagnostic pottery from the Tantura lagoon dated to the 17th century B.C. consists of one, possibly two sherds, according to the excavators.<sup>19</sup> This is not very substantial evidence for the existence of the town of Dor as early as those sherds might indicate, because they were found approximately one kilometre south of Dor. As to the "Bronze Age" stone anchors, we all agree that they cannot be dated accurately unless they are found in a stratified context. These do not belong to a stratified context. Moreover Avner Raban himself showed how stone anchors continued to be used in mediaeval times in the Red Sea<sup>20</sup> and we have evidence of their presence in Byzantine contexts in Dor harbour itself.<sup>21</sup> Reference to the pottery of Dor is disturbing because there is so little of it. Moreover, any study of this combines it with pottery from other neighbouring cities to get a general result, rather than a precise and particular picture of Dor itself and its harbour.<sup>22</sup>

In Avner Raban's report in 1982 of their efforts to find better stratigraphical and architectural interrelations between the series of quays and landing stages along the waterline of Dor and the in-shore structures he concludes:

The ceramic evidence for dating each of the five stages is not sound enough. Yet it is clear that the post-transgressional stages (4-5) later than 1200 B.C.; and should be dated some time in the Early Iron Age. The few pottery sherds which are securely associated with the floors of the second and third stages are all of the second half of the Late Bronze Age.

(*IJNA* 12 (1983), 229f.)

The published studies of the pottery of Dor so far do not tell much as we would like to know. We understand that further studies of the ceramic ware of Dor are at present in hand and will be more helpful in due course.

We are told by Ayelet Gilboa that one of the main phenomena marking the end of the late Bronze Age in Palestine is the cessation of the intensive import of Cypriot pottery vessels.<sup>23</sup> Gilboa goes on to affirm that there is a gap of about a century and a half before the renewal of Cypriot pottery imports into Palestine in Iron Age I. Tel Dor is important in this period because a relatively large assemblage of White Painted and Bichrome Cypro-Geometric fragments of Early Iron Age date were found along that shore. However, the term *Tel Dor* includes the Tantura lagoon approximately one kilometre to the south, so that we are speaking here of the general area of Dor and not of a particular ancient harbour at this precise site.

Gilboa tells us that the Cypro-Geometric pieces uncovered to date in the assemblage mentioned above belong to at least eight different vessels.<sup>24</sup> Some of them are very fragmentary. Three sherds only are distinguishable having typological significance and one of them only, the white-painted bowl no. 32218/1, is singled out as having a unique schematic floral design on it, paralleled on some of the funerary urns of Hama and Carchemish, and on Late Minoan and Proto-Geometric vessels in Crete.<sup>25</sup> The piece has been scientifically analysed and established as imported.<sup>26</sup>

It is not clear whether this is the same bowl as the one from the Tantura Lagoon mentioned in Wachsmann and Raveh in 1984:

The lagoon has now surrendered a ceramic bowl which appears to date to the latter part of the Middle Bronze Age found in conjunction with a single pottery sherd identified by Dr. Robert S. Merrillees as a fragment of a Cypriote White Painted Cross Line Style jug and dated to the Middle Cypriote III-Late Cypriote IA, c. 17th century B.C. These artifacts indicate that Tantura Lagoon was indeed in use by that time and suggest that the existence of the 'proto-harbour' here may have been the *raison d'être* for the establishment of Dor.

(*IJNA* 13 (1984), p. 239)

We must bear in mind the fact that the terms "Bronze Age", "Late Bronze Age" and "Iron Age 1" used in the context of Dor (or in any other context) are never absolute dates allowing precise comparison with other areas.

As to the "Sikils" to whom Stern gives pride of place at Dor,<sup>27</sup> we have problems there too:

The Sikils, whose place in history occupies an intermediate phase between the early Canaanites and their Phoenician descendants, were the dominant element at Dor for about a century. Research on the Sikils is still in its infancy.

(Stern, 1994, cit. p. 20)

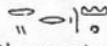
We might say that this research is still precariously in embryo and in danger of being aborted. For all the reputed dominance at Dor of the "Sikils", nothing is produced anywhere to identify them in any way at all. But investigators at Dor continue to insist on their presence, even though no archaeological evidence for them can be produced. In order to be able to speak of the "Sikils" at all, Stern's references are all to the Egyptian texts on the Sea Peoples, which in fact tell us nothing about their identity or where they came from and not even that they came by sea.<sup>28</sup> Stern also refers to the work of Trude and Moshe Dothan, which is centred on the Philistines and not the "Sikils".

We must not forget that the Dothans never, in all their extensive published work over many years, ever define the Philistines, even in a temporary working definition. They conveniently set aside without comment any opinion they do not like, such as Claude Vandersleyen's masterly paper on the Philistines, seen from the Egyptian context.<sup>29</sup> It is significant that after



to accommodate mainly the murex shell fishermen. Through what remained of the work of Claudius Iulius (Iolaus) in Stephen of Byzantium we learn that in his day Dor was a very small town with a rocky shore and an abundance of purple-fish. As time went on the Phoenicians who settled there built more lasting structures, a city wall and a harbour with good and safe anchorage. According to Iolaus, it was the Phoenicians who gave the city its name which the Greeks adapted to *Dora*.

If we accept Dor as a city founded by the Phoenicians (another "nation" which has never been defined or properly identified)<sup>34</sup> it may well have consisted of anything more than a few huts and may have been readily identifiable as a town at the time of Wenamun's journey. It is significant that Tantara, which is about one kilometre south of Dor, was considered by seafarers to be a much more convenient port over the centuries.<sup>35</sup>

In the Egyptian text of Wenamun the name of the town at which Wenamun stopped was Dir .<sup>36</sup> There is no certainty that this name must apply to Dor on the coast or to the biblical city.<sup>37</sup> It has been suggested that the name of Dor appears in one of the topographical lists of the time of Ramesses II, in the temple at Amara West (Nubia) as Tw-i.<sup>38</sup> As we see, this is not identical with the name that is found in the text of Wenamun. Furthermore, Elmar Edel found the name of (T)w-13-r on a topographical list from Soleb, also in Nubia, dating much earlier, to the time of Amenophis III.<sup>39</sup> In addition to that, Manfred Görg has drawn attention to the name of Tw-r on a topographical list on one of the statues of Amenophis III north of the 10th pylon at Karnak.<sup>40</sup> At the present time there is no reason to equate all these names with Dor. Moreover, we do not yet have any archaeological evidence for the existence of a town on the site of Dor at an early date.

Also a discussion has occurred as to whether the biblical Dor could be identified with En-Dor, situated at about 30 miles inland from Dor on the sea.<sup>41</sup> This is another possibility that has not yet been sufficiently discussed.

The belief that Wenamun sailed on the Mediterranean Sea and stopped at Dor is the result of a chain of assumptions which must be set aside as soon as possible, both by Egyptologists and by non-Egyptologists. In 1965 I discussed these assumptions and proposed alternatives which have not yet been properly considered.

The geography at present accepted for the route of Wenamun to the place where he was to get his timber for the bark of Amun was based from the very beginning on the unfounded belief that all of Egypt's timber came from the Lebanon, even if the timber was not cedar.<sup>43</sup> It was this assumption

that led Golenischeff and other scholars to identify *the great ym of Kharu* as *the sea*,<sup>44</sup> although there is no parallel for this expression to confirm that this interpretation is correct.

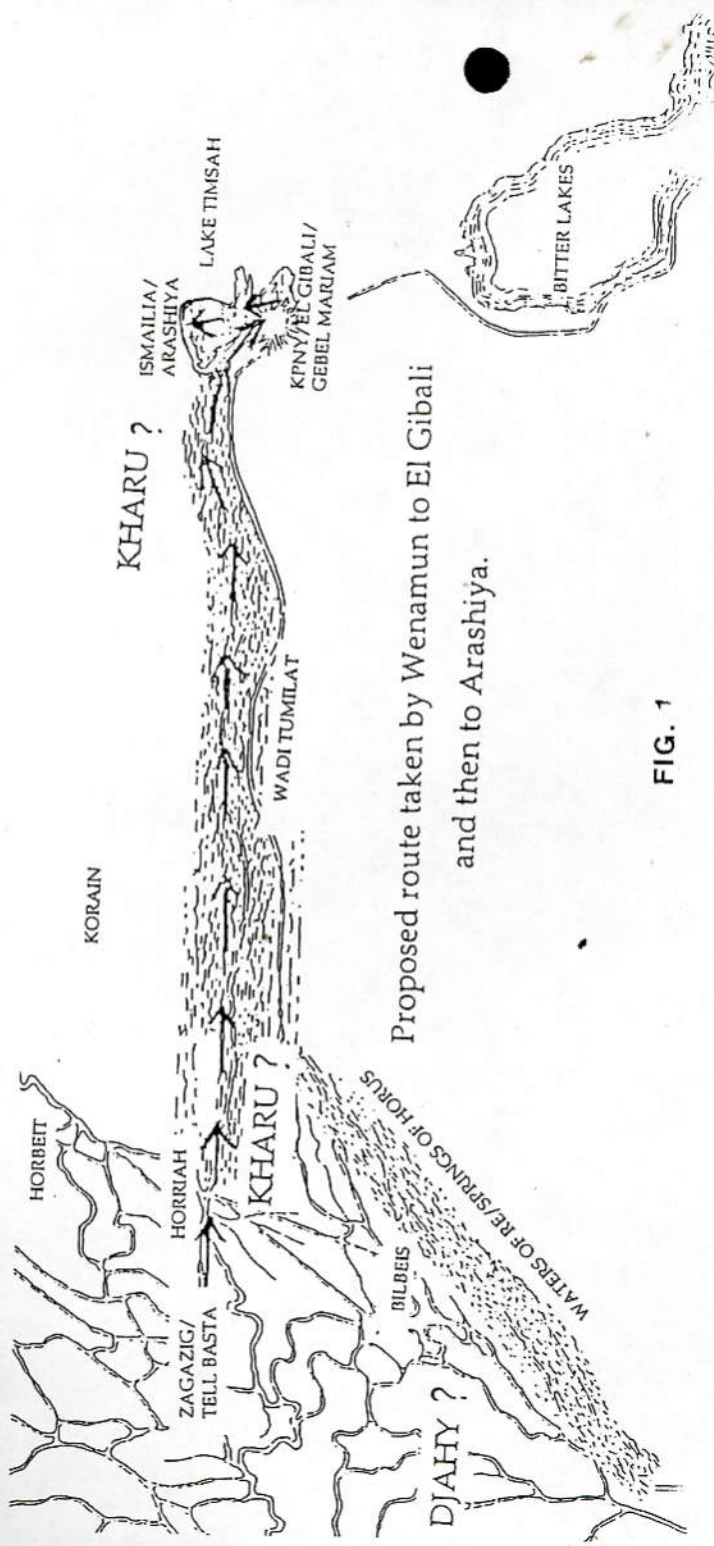
On several occasions in the past I have asserted that there was no evidence that the ancient Egyptians had any word for *sea* or even that many of them could possibly have known what the *sea* was. I have said that ancient Egypt was the equivalent of an island, cut off on three sides by the deserts and in the north by the delta itself where many foreign groups were settled and were actively hostile to the pharaoh. Not many Egyptians would have known how to live in the ancient delta without fortifications around them, and without a particular knowledge of the skills required to live in swampland. The native Egyptian living on one of the two banks of the Great River, before it divided into small streams in the delta, would not normally have had to face these problems.

At the International Congress of Egyptologists in Munich in 1985, Claude Vandersleyen showed indisputably that the expression *Great Green* did not ever mean *sea*.<sup>45</sup> It had been translated by some early scholars as *sea* to fit Maspéro's personal understanding of the "Sea Peoples" texts,<sup>46</sup> thus interpreting them wrongly and leading us astray as to its meaning and its associations. Sadly even today a few scholars still adhere to the translation of *sea* for *Great Green* without, however, producing a single textual example to support their view. The same is true of *ym*, a semitic word for *water* of any kind, adopted into the Egyptian language at the beginning of the New Kingdom.

In the case of Wenamun, *ym* is associated with *Kharu*, a geographical area which is not properly understood by Egyptologists and is usually translated vaguely as *Syria*, incorrectly I believe. I have already several times protested against the translation in the ancient Egyptian texts of the three names of *Kharu*, *Retenu* and *Djahy* as *Syria*. I believe we must insist on considering them as different areas in the north of Egypt, according to the evidence in the Egyptian contexts.<sup>47</sup>

It is not unreasonable to suggest that the name of *Kharu* should be associated with water channels, as an ancient forerunner of today's *El Khor*.<sup>48</sup>

The texts imply that a person was immediately in *Kharu* upon leaving Egypt. He could travel there by land or by water. It was clearly a fertile area because it produced food, animals and leather goods. There are records that the Pharaoh kept an army there, where the soldiers sometimes suffered hardship.<sup>49</sup> Horses were sent there, which means that the distance from Egypt cannot have been excessive and that food and water were adequately



Proposed route taken by Wenamun to El Gibali  
and then to Arashiya.

FIG. 1

available. Horses were too valuable to risk losing in difficult or arid territory. According to the evidence, they were, in any case, used only by high officials.<sup>50</sup>

My proposal for the route of Wenamun in *Kharu* was that he travelled on an inland waterway in an area which corresponded to one with many water channels and the obvious one was the Wadi Tumilat (see fig. 1).<sup>51</sup> There are some doubts as to whether this wadi was always available as a waterway because the Napoleonic survey found that it filled with water only in years when the Nile was high and this could well have been the cause of the tension with regard to the time element in Wenamun's story. If he missed the opportunity of sailing away with his cargo within the year of a high Nile, Wenamun would have had to wait for another high Nile, which might not occur the following year. In such circumstances the timber might have to be transported overland or wait many months, or even years, for sufficient water in the wadi to move it.

The Wadi Tumilat flowed into Lake Timsah, just north of the Bitter Lakes along a natural depression (see fig.1). In times of high Nile, it filled automatically despite the barriers built to stop the flooding.<sup>52</sup> Before the Suez Canal was cut through Lake Timsah, this lake was a much more extensive and imposing stretch of water than it is today. At the south-west corner of this lake there used to be a large mound called El Gibali, full of pottery, extending several kilometres across, of which only a small fraction remains today because it has been occupied by the army. Had this not been the case, the remaining part of this mound would have been totally bull-dozed away. I believe this mound, which is still called El Gibali today, was Wenamun's *Kapuna*.<sup>53</sup>

Towards the end of the text of Wenamun, we are told that his vessel was blown to a place called *Alashiya*. It is an incontrovertible fact that the north shore of Lake Timsah is still today called *Arashiya* by the local people, although it is now physically a part of Ismailia which has extended southwards as far as the shore of this lake. Pine and other pollens were found in boreholes from this area by Shukri Saad of Alexandria University,<sup>54</sup> though we do not have exact dates for them. However, as we have already said, there is considerable evidence for forests in southern Palestine in pre-biblical times<sup>55</sup> and even today we have the final remains of an ancient juniper forest in the northern Sinai.<sup>56</sup>

In my remarks on the story of Wenamun, I discuss some elements in it such as piracy and robbery, which scholars have automatically associated with the sea and not with inland waterways. But there is a great deal of

evidence throughout the ages that travellers on inland waters were subjected to such attacks.<sup>57</sup>

In conclusion, not only is there *no internal evidence* in the story of Wenamun indicating that he was ever at Dor on the Mediterranean coast.

Dor itself has produced nothing to indicate that it had any early links with Egypt, before the Phoenician period. There is not a single Egyptian inscription, no piece of Egyptian relief, re-used or otherwise, not even a stray scarab to show that the inhabitants of Dor themselves had any traffic with Egypt or enough interest in its culture to bring back some relic of it to their own city. From Megadim, north of Dor, we have some blocks with Egyptian reliefs, re-used as anchors.<sup>58</sup> From Dor, absolutely nothing. This seems securely to confirm what the archaeological data indicates, that Dor came to flourish only with the expansion of the Phoenician culture. The city may not even yet have had a name at the time that Wenamun made his journey, wherever it was.

#### NOTES

1. *Dor, Ruler of the Seas*, 1994, is a summary of the work carried out over twelve years of excavation at this site by E. Stern. A series of annual preliminary reports may be found in the journal *Israel Exploration Journal* from 1980 onwards.
2. A. Raban, "Maritime Dor" in E. Stern (ed.), *The New Encyclopedia of Archaeological Excavations in the Holy Land*. Vol. 1 (1993), pp. 369-71; "The Constructive Maritime Role of the Sea Peoples in the Levant", in M. He'itler and E. Lipinski (eds.), *Society and Economy in the Eastern Mediterranean (c. 1200-1000 B.C.)* (1988), pp. 261-94; "The Harbor of the Sea Peoples at Dor", *BA* 50 (1987) pp. 118-126; "Submerged Prehistoric Sites off the Mediterranean Coast of Israel" in P. M. Masters and N. C. Flemming (eds.), *Quaternary Coastlines and Marine Archaeology* (1983) Academic Press, London; "Recent Maritime Archaeological Research in Israel", *IJNA* (1983), pp. 229-251; "Some Archaeological Evidence for Ancient Maritime Activities at Dor", *Sefunim* 6 (1981), pp. 15-26.
3. My comments on this may be found in "Stone Anchors: The Evidence Re-Assessed", *Mariner's Mirror* 79 (1993), 5-26.
4. A. Raban and E. Linder, "Maritime Atlit", in E. Stern (ed.) *The New Encyclopedia of Archaeological Excavations in the Holy Land* Vol. 1 (1993), pp. 117-120.
5. Preliminary report in *Discussions in Egyptology* (hereafter *DE*) 29 (1994), pp. 5-22: "A Stone Anchor Workshop at Marsa Matruh".

6. Remarkably, the only stone anchors to resemble each other closely in size and weight are those from the Wadi Gawasis, along the Egyptian side of the Red Sea. See Nibbi, "Ancient Egyptian Anchors: A Focus on the Facts", *Mariner's Mirror* 70 (1984), pp. 247-66. Also Nibbi, "Some Remarks on the Two Monuments from Mersa Gawasis", *Annales du Service des Antiquites de l'Egypte* (1981), pp. 69-74.
7. See Nibbi, (1993) (note 3 above).
8. See chapter 8, in which each of the shipwrecks is identified and commented on.
9. The Cheops vessels does not have any timbers as long as its overall length. P. Lipke, *The Royal Ship of Cheops* (1984); N. Jenkins, *The Boat Beneath the Pyramid* (1980); A. M. Abubakr and A. Y. Mustafa, "The Funerary Boat of Khufu", *Beiträge zur ägyptischer Bauforschung und Altertumskunde*, Heft 12 (1971), Festschrift H. Ricke, 1-16. M. Z. Nour and others, *The Cheops Boat Part 1* (1960), Antiquities Department, Cairo, 45ff.
10. B. Landström, *Ships of the Pharaohs* (1970); D. Jones, *Model Boats from the Tomb of Tutankhamun* (1990); D. Jones, *Boats* (1995); S. Vinson, *Egyptian Boats and Ships* (1994).
11. It is possible to wade out to the island from the shore of the bay. At the time I did this the water was about four feet high. See Nibbi, "The Stone Anchors of Bates's Island", in *Proceedings of the Seventh International Congress of Egyptologists*, Cambridge, 1995, ed. C. Eyre, forthcoming.
12. L. Casson, *Ships and Seamanship in the Ancient World* (1971), 170-173. Also L. Basch, *Le Musée imaginaire de la marine antique* (1987), pp. 55-75.
13. Pliny (*NH* 16-203) reported a cedar of 130 feet from Cyprus to make a mast for a large vessel.
14. "Some Remarks on the Cedar of Lebanon", *DE* 28 (1994), pp. 35-52; "Cedar Again", *DE* 34 (1996), pp. 37-59. Also Nibbi, *Ancient Egypt and Some Eastern Neighbours* (1981), Chapter 1, on the vegetation of ancient Egypt. Also Nibbi, "The Byblos Question Again", *DE* 30 (1994), pp. 115-141.
15. Stern, 1994, cit. (see note 1 above), pp. 85-87.
16. A. Nibbi, *Canaan and Canaanite in Ancient Egypt* (1989); "The Canaan in Egypt", *Atti del II Congresso Internazionale di Studi Fenici e Punici*, Consiglio Nazionale delle Ricerche, Roma (1991), pp. 169-178.
17. See note 2 above, Raban cit. 1988, p. 273.
18. *Ibid.* p. 274.

19. We are referring here to the sherds reported in the postscript by Wachsmann to his "A Concise Nautical History of Dor/Tantura" (1984), pp. 223-41, in particular p. 239, note 1.
20. A. Raban, "Medieval Anchors from the Red Sea", *IJNA* 19 (1990), pp. 299-306.
21. S. Kingsley and K. Raveh, "Stone Anchors from Byzantine Contexts in Dor Harbour, Israel", *IJNA* 23 (1994), pp. 1-12.
22. The published studies of the pottery at Dor must necessarily refer to the coastal and other cities nearby. See A. Gilboa, "New finds at Tel Dor and the beginning of Cypro-Geometric Pottery Import to Palestine", *IEJ* 37 (1989), pp. 204-218; J. Yellin, "The Origin of Some Cypro-Geometric Pottery from Tel Dor", *IEJ* 39 (1989), pp. 219-227; F. de Créé, "The Black-on-Red or Cypro-Phoenician Ware", in *Society and Economy in the Eastern Mediterranean* (c. 1500-1000 B.C.), ed. W. Heltzer and E. Lipinski, Leuven, (1988), pp. 95-102. See also D. T. Ariel et alii, "A Group of Stamped Hellenistic Storage-Jar Handles from Dor", *IEJ* 35 (1985), pp. 135-152.
23. Cit. note above, p. 204.
24. *Ibid.* p. 205.
25. *Ibid.* pp. 205-6.
26. Yellin cit. (note 22 above), pp. 223-227.
27. Stern (1994) cit. pp. 85-101. We should say that the Hittite letter (RS 34.129) to the ruler of Ugarit, referring to people called *Sikilaju* is not really relevant in this context because in order to explain it, assumptions are drawn from many other different disciplines, including Egyptological ones which are unfounded. See M. Dietrich and O. Loretz in *U. F.* 10 (1978) and A. Lehmann, *U. F.* 11 (1979). Many questions will have to be clarified with regard to this letter before it can be of any use in enlightening us as to the inhabitants of the Near Eastern coast.
28. Claude Vandersleyen, "Le sens de OUADJ-OUR (W3D-WR)", *Proceedings of the International Congress of Egyptologists*, Munich 1985, S. Schoske (ed.) (1991), vol. 4, pp. 345-352; also in *Göttinger Miszellen* 103 (1986), pp. 75-80. See also by C. Vandersleyen, *L'Égypte et la Vallée du Nil* (Tome 2). De la fin de l'Ancien Empire à la fin du Nouvel Empire, 1995. There is an example where Great Green must mean *sea* from its context. The same is true of *ym* which entered the Egyptian language in the 18th dynasty.
29. C. Vandersleyen, "Le dossier égyptien des Philistins", *The Land of Israel: Crossroads of Civilizations* OLA 19 (1985), pp. 39-54.

30. I began this discussion in 1972 with *The Sea Peoples: A Re-Examination of the Egyptian Sources*. The arguments appeared more fully in *The Sea Peoples and Egypt* (1975). Further discussion concerning our non-scientific approach to this problem may be found in: "The Sea Peoples: Some Problems Concerning Historical Method" in *Studia in Honorem Christo M. Danov, Terra antiqua Balcanica II, Annuaire de L'Université de Sofia "Kliment Ohridski" Tome LXXVII, 2, Sofia (1985), pp. 310-319.*

31. *Ibid.* see note above. It does not help us to find that Nancy Sanders in her book on "The Sea Peoples" (1978) identifies them as the people who introduced the use of ashlar masonry in the Near East. It is incongruous that people who are essentially to be associated with the sea (sic) should also have special skills in stone-working and presumably, quarrying.

32. It is impossible here to list all the published comments on the story of Wenamun. The first complete publication of the text was by W. Golenischeff: "Papyrus hiératique de la collection W. Golenischeff", *Recueil de Travaux* (1899), pp. 74-102; this was followed in 1932 by A. H. Gardiner in his *Late-Egyptian Stories* 61-76. Translations followed by A. Erman in *ZAS* 38 (1900); E. F. Wente, "The Report of Wenamun" in W. K. Simpson (ed.) *The Literature of Ancient Egypt* (1972) pp. 142-155; H. Goedicke, *The Report of Wenamun* (1975); M. Lichtheim, "The Report of Wenamun: Papyrus Moscow 120", in *Ancient Egyptian Literature Vol. II* (1976) pp. 224-230. Some radical differences of opinion on the route of Wenamun are expressed in A. Nibbi, *Wenamun and Alashiya Reconsidered* (hereafter, *Wenamun*), (1985). The most recent commentary on the chronology of this story may be found by A. Egberts in *JEA* 67 (1981), pp. 57-67, with excellent bibliographical references. Finally one must mention a brave attempt at commenting on the proper names in this text by Anne Scheepers, "Anthroponymes et Toponymes du Récit d'Ounamun" in E. Lipinski (ed.) *Phoenicia and the Bible Studia Phoenicia XI, OLA* 44 (1991), pp. 17-83.

33. See Claudius Iulius (Iolaus) in Stephen of Byzantium. A. Westerman (ed.) *Stephani byzantii—ETHNIKON*, Leipzig, (1839), pp. 113-4.

34. It is remarkable that we have given the Phoenicians an ethnic identity purely on the evidence of some pottery and a language. As we all know, culture and language are two separate problems which do not necessarily link up with ethnic identity at all.

35. Raban (1988), cit. p. 274, note 66.

36. In line 1.8.

37. See *Dor* in K. Galling (ed.) *Biblisches Reallexikon. Handbuch zum Alten Testament I*, (1977)<sup>2</sup>, pp. 62-3.

38. Galling, cit. column 63 and other scholars as well. See K. A. Kitchen, *Ramesside Inscriptions Vol. II*, 216, no. 76.



39. E. Edel, "Die Ortsnamenlisten in den Tempeln von Aksha, Ar-Soleb im Sudan", *Biblische Notizen* 11 (1980), pp. 63-79; see also M. Görg, "Die Teukrer und die Gargasiter", *Biblische Notizen* 28 (1985) pp. 7-14.
40. Görg *ibid.* pp. 9-10.
41. See J. P. Brown, "The Mediterranean Seer and Shamanism" in *ZAW* 93 (1981), pp. 374-400, particularly p. 399. Also O. Margalith, "Dor and En-Dor" in *ZAW* 97 (1985), pp. 109-111.
42. See Nibbi, *Wenamun* cit. Part I.
43. This too is an assumption which must be corrected on several grounds. See Nibbi, note 13 above. Firstly, we must not overlook the evidence for forests in southern Palestine and the northern Sinai; and secondly, we must be realistic about the possibility of transporting large tree-trunks on ships in the Mediterranean Sea. Thirdly, there is no evidence that Gebeil/Byblos was ever a merchant port at any time in its history. See Nibbi, note 14 above and Nibbi, *Ancient Byblos Reconsidered* (1985).
44. See lines 1.7-8.
45. Vandersleyen *cit.* note 28 above.
46. A. Nibbi, *The Sea Peoples: A Re-Examination of the Egyptian Sources* (1972); *The Sea Peoples and Egypt* (1975); *Ancient Egypt and Some Eastern Neighbours* (1981).
47. See Nibbi, *Wenamun* *cit.* pp. 44-104; also Nibbi, "Some Remarks on the *Lexikon* Entry *Syrien*", *DE* 8 (1987), pp. 33-47.
48. Nibbi, *Wenamun* *cit.* 74\*.
49. *Ibid.* 70-1.
50. A. Nibbi, "Some Remarks on Ass and Horse in Ancient Egypt and the Absence of the Mule", *ZAS* 106 (1979), pp. 148-168.
51. Another clue that *Wenamun's* vessel was sailing on an inland water is the reference to its complement of seven logs. This is the number of logs that we see on all the boats in the reliefs from Khorsabad, which are believed by all maritime scholars to be sailing on still water and not on the sea. See (1996) *cit.* on cedar, p. 50.
52. G. Posener, "Le canal du Nil à la Mer Rouge avant les Ptolomées", *Chronique d'Égypte* 13 (1938); also A. Lloyd, *JEA* 63 (1977), pp. 142-155.

53. Photographs of this site may be seen in *Ancient Byblos Reconsidered* (1985), Plates III, IV, V, VI, and VII.
54. Nibbi, cit. *Ancient Egypt and Some Eastern Neighbours* (1981) pp. 5-7.
55. *Ibid.* Chapter 1.
56. See also notes 13 and 14 above.
57. Nibbi, *Wenamun* cit. pp. 29-39.
58. E. Galili and K. Raveh, "Stone Anchors with Carvings from the Sea off Megadim, Israel", *Sefunim* VII, (1988), Bulletin of the National Maritime Museum, Haifa, pp. 41-47.