

# Walls of the Prince: Egyptian Interactions with Southwest Asia in Antiquity

*Essays in Honour of John S. Holladay, Jr.*

*Edited by*

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# Mezad Hashavyahu Reconsidered: Saite Strategy and Archaic Greek Chronology

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## Introduction

One of the most intriguing sites providing evidence for Egyptian military activity in the Levant is Mezad Hashavyahu, a small Late Iron Age fortress on the coast of Southern Palestine. First excavated by Joseph Naveh in 1960, it drew special attention for its Hebrew ostraca and its rich assemblage of East Greek pottery.<sup>1</sup> Further work was carried out in 1986, but full publication only came in a thesis by Alexander Fantalkin (2001). Fantalkin's work has placed all discussion of Mezad Hashavyahu on a firmer footing. It has clarified its nature as a one-period, short-lived settlement, and by quantifying the Greek pottery finds we now have a clearer idea of their importance. Mezad Hashavyahu should now be able to play a more certain role in the discussions concerning Egyptian strategy in the Levant during the Saite period (26th Dynasty), the Egyptian use of Aegean mercenaries at this time, and also the relative and absolute chronologies of Late Iron Age pottery in both Palestine and Greece. The dating of the site has been controversial, though Na'aman, Fantalkin and others have now confidently placed the site in the late 7th century BC, with a date for its closure at 604 BC, but this needs further examination, particularly in the light of continuing uncertainties regarding the dating of Archaic Greek pottery. In this paper I offer evidence for a later dating of the site, to the early 6th century BC. As it happens, Jack Holladay suggested the possibility of such a date as long ago as 1976 (see below). It is an honour to be able to dedicate this paper to Jack and—if the arguments offered here stand up—to his foresight.

## Nature of the Settlement

Though the idea has sometimes been entertained that Mezad Hashavyahu was a trading post, it is now generally agreed that it was a military station.<sup>2</sup> Indeed

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1 See Naveh (1977 and 1993) for a brief history of the excavations and early bibliography.

2 For references and discussion see Fantalkin (2001).

the only architecture at the site is a small fortress. The provenance, quantity, quality and repertoire of the Aegean (largely East Greek) pottery—including cooking pots, lamps, kraters, drinking cups and amphorae, and representing slightly less than half the total assemblage from the site<sup>3</sup>—leave little room for doubt that this was the base of a small but élite mercenary army, most likely “Ionians” from East Greece who had brought with them (or were supplied by sea with) considerable home comforts. After a short period of occupation the fortress was abandoned, rather than destroyed.

The site is strategically positioned, on a ridge near the coast about 1.7 km south of the port of Yavneh-Yam. At Yavneh-Yam remains dated by the excavators to the late 7th century have been found and designated as Stratum IX. These include a monumental structure, with a destruction layer on its floor. The pottery parallels that of pre-Persian Ashkelon, Ekron 1B and Tel Batash (Timnah) 11 as well as Mezad Hashavyahu (Fantalkin 2001: 132–33). There were also a few East Greek pieces, including a few fragments of Ionian cups and rims from a couple of East Greek cooking-pots. Despite the absence of other types, such as the Wild Goat style conspicuous at Mezad Hashavyahu, Fantalkin argues convincingly that Yavneh-Yam IX was contemporary and that both settlements met their fate at much the same time (Fantalkin 2001: 133, 136). Given their close connections, both geographical and chronological, there must have been some strategic relationship between the two settlements. The fortress at Mezad Hashavyahu must have been intended to provide logistical support for the port of Yavneh. The gateway of the fortress faces due west towards the shore, a quick route for a forced march, if necessary, to the port. Nevertheless, in Fantalkin’s opinion, the garrison’s main function was to supervise and collect supplies at the port for Egyptian troops travelling by sea further north and back (Fantalkin 2006: 203). It seems reasonable that whoever controlled Mezad Hashavyahu also controlled Yavneh-Yam, as envisaged by Na’aman: “The latter site was the major port in this area, and the Egyptians built the fortress for their Greek or Cypriot mercenaries in the vicinity . . . , in a place that enabled them to supervise the neighbouring port city and kept the mercenaries in isolation from the local inhabitants” (Na’aman 2001: 272).

The theory that the Egyptians were the owners of the fortress is far more likely than any other option. Naveh thought that the garrison was in the control of Josiah of Judah (640–609 BC) (Naveh 1977: 863), while others have suggested

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3 Niemeier (2002: 330) feels that Fantalkin’s estimate of 46 percent is slightly too high, as the mortaria and basket-handled amphorae are more likely to be of Cypriot rather than East Greek origin, as assumed in Fantalkin (2001), an assessment accepted by Fantalkin (personal communication, March 2006).

his successor Jehoiachim (609–598 BC) (Wenning 1989: 182–92; Niemeier 2001: 23; 2002: 329; Wenning 2004: 31–32, n. 13) or Zedekiah (597–586 BC) (Wenning 1989: 192–93). Yet, although not impossible, there is no good evidence that Judahite monarchs employed Aegean mercenaries at this period (Fantalkin (2001: 141; 2006, 203).<sup>4</sup> Indeed, Naveh originally felt that the garrison was created by Pharaoh Psammetichus I, and that it had only been in the control of Judah for a few years before 609 BC (Naveh 1962: 99). It is really only the presence of the Hebrew ostraca (with Yahwist names) that spawned the suggestion of Judahite control of this area during the period of the late monarchy. Another ostrakon bears a Phoenician theophoric name (Na’aman 1991: 46).

The Babylonians have also been considered as owners of the fort,<sup>5</sup> but the probability is not high. While Greeks and Anatolians are known from Babylonian records as craftsmen, there is no explicit cuneiform evidence for their use as mercenaries.<sup>6</sup> “Nor is there any hint in the Babylonian material of an awareness of Ionia in particular as a source for soldiers . . .” (Kuhrt 2002: 22). Against this Niemeier claims that “even without the existence of authentic Babylonian sources, we know from Alcaeus’ poem that Greek mercenaries did indeed serve in the Babylonian army” (2002: 330).<sup>7</sup> This is true, but this single

4 The only evidence offered by Niemeier (2001: 18; 2002: 329) concerns the “Kittim” (*ktym*) for whom Elyashib, commander of the Judahite fortress of Arad, was ordered to supply provisions, according to the ostraca of Tel Arad VII/VI. Aharoni (1981: 12–13) assumed these were Greek or Cypriot mercenaries in the employ of Judah, but their identity is highly uncertain. Herzog et al. (1984: 29) suggested that the Kittim of Arad were Phoenician-speaking merchants or caravaners from Kition. Even if they were soldiers it remains unclear who their employer may have been; Fantalkin (2006: 207, n. 83) sees them as Egyptian mercenaries. The whole question is complicated by the extremely problematic dating of the Arad strata; see briefly Kokkinos (1998: 40–41, and n. 27).

5 Including by the present writer and colleagues; see James et al. (1991: 372, n. 65).

6 For the sources Kuhrt (2002); Zadok (2005).

7 As evidence that the Assyrians employed Carians, Niemeier cites a story from Polyaeus (*Strategica* 7.3.4) in which a group of Carians, led by one Pigres, were instrumental in helping Psammetichus (I) defeat Tementhes in a battle at Memphis. Though the source is late (2nd century AD) the tradition may well be reliable—Tementhes must be Tanwetamani, last king of the (Ethiopian) 25th Dynasty, while Pekrj (Pigres) is a Carian name; a Memphite grave-stone with this name (Ray 1982: 190; Younis 2003: 584), dating to the late 7th century, may belong to the very individual mentioned by Polyaeus. Niemeier (2001: 17; 2002: 328) assumes the defeat of Tanwetamani referred to took place under the Assyrians; if Greek historiographers had replaced Assurbanipal with Psammetichus as Tanwetamani’s main opponent, then “these Carians served in the Assyrian army”. Yet, as Morkot (2000: 297–98) points out, Assurbanipal’s record of his 663 BC campaign does not refer to a battle with Tanwetamani, only his flight. In Morkot’s opinion it is more likely that the tradition in Polyaeus relates to

attested instance—concerning a brother of Alcaeus in a poorly preserved poetry fragment—needs to be weighed against the vast body of evidence that the Egyptians were the employers *par excellence* of East Greek mercenaries at this period. Indeed, Niemeier accepts that the amount of Greek pottery from Mezad Hashavyahu “may, therefore, be interpreted as evidence that Greek mercenaries were in the service of Egypt at the site, since the Egyptian army was the only army in which large units of Greeks served” (2002: 330). While the casemate construction of the fortress at Mezad Hashavyahu is of Levantine tradition—showing that local builders were employed—the L-shaped plan of the fortress is alien to Palestine (Na’aman 1991: 46), but is similar to that of the Egyptian fortress at Semna in Nubia (Fantalkin 2001: 49–50). The Semna fortress is much earlier (Middle Kingdom, refurbished in the New Kingdom), but was reoccupied for a period in the late eighth-seventh centuries BC (Morkot 2000: 152). Rather than the L-shape *per se*, it is the fact that it was dictated by the topography that suggests that Mezad Hashavyahu was built within the same, flexible, tradition of Egyptian military building.<sup>8</sup>

Egyptian control of the fortress is not incompatible with the Hebrew ostraca. The famous letter containing the complaint of the reaper concerns two apparently Hebrew individuals—one the anonymous author of the letter, a peasant, the other one Hoshavyahu (Hoshaiah) son of Shobai, thought to be an official in charge of *corvée* workers. As frequently noted, the Egyptians used Judaeans mercenaries, apparently at Migdol, Tahpenes, Memphis and Elephantine (Oren 1984: 36; Miller and Hayes 1986: 435; Na’aman 1991: 46–47; Fantalkin 2001: 145). Most such references belong, of course, to the Exilic period (post-587 BC),<sup>9</sup> though we know from Jeremiah 24:8 that by the reign of Zedekiah

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an attempt by Tanwetamani to reconquer Lower Egypt *after* the withdrawal of the Assyrians. The reference to Carians would then reflect the known policy of Psammetichus I as an independent monarch to employ such mercenaries, and not Assyrian practice.

8 Personal communication, Robert Morkot; Fantalkin (2001: 50).

9 Francis and Vickers (1985: 137, n. 71) suggested an Exilic date for the ostraca, noting that the poor status of the reaper who wrote the famous judicial plea (Albright 1969: 568) recalls the fact that the Babylonians left “some of the poorest of the land to be vinedressers and ploughmen.” However, it seems unlikely that one of the “poorest of the land” would have been so literate. Alternatively, it is tempting to see the plight of an apparently literate man working as a reaper, reduced to making the pathetic complaint that his coat had been stolen, as evidence that he was in greatly reduced circumstances—again suiting the Babylonian period. Still, the difficulties of determining the character of the Hebrew used in the letter, whether its author used a scribe and whether he was even certainly Hebrew (Young 1990), caution against reading too much into the case.

the Jewish community in Egypt was already large enough to require mention as part of the audience for his prophecies.

The only serious objection raised to pharaonic control of the fort has been the alleged absence of Egyptian finds (Waldbaum and Magness 1997: 39; Stern 2001: 142). However, Fantalkin's re-examination of the pottery finds, aided by petrographic analysis, has identified a small quantity made from Nilotic clay. With reasonable safety this can be taken as Egyptian (or in some cases Nubian) pottery, a new category at the site which amounts to one percent of the overall assemblage (Fantalkin 2001: 97–98, 103). Further, as Meẓad Ḥashavyahu and Yavneh-Yam seem to have been controlled by the same military authority, the two scarabs found in Yavneh-Yam Stratum IX (see below) then must also be taken into account.

On balance, Fantalkin's conclusion that the establishment of Meẓad Ḥashavyahu "may only be attributed to the Egyptian administration" (Fantalkin 2001: 141; 2006: 203) is the most realistic in light of the available evidence. The garrison seems to have been comprised, to a large degree, of "Ionian" soldiers, accompanied by Hebrew and Phoenician speakers working in farming, administrative and possibly also military capacities.

### Archaeological Dating of the Garrison

As Fantalkin notes, Meẓad Ḥashavyahu was previously embroiled in an unfortunately circular discussion between Levantine and Aegean archaeologists regarding chronology. Naveh, the excavator of the site, used the accepted dating of the Greek pottery found there when estimating that the site belonged to the late 7th century (Naveh 1962: 97–99). The result, at the time, was somewhat surprising: "The pottery found on the floors included the local ware common in the seventh century BC together with coarse bowls and jars, which until the excavations were considered to be of Persian date" (Naveh 1977: 862). Lapp firmly objected to such a high date, basing his conclusion on the mortaria and "other post-Iron II material"; his view was that such mortaria were only introduced into Palestine in the 5th century BC (Lapp 1970: 184–85, n. 28). Holladay shared Lapp's unease, suggesting by way of compromise that a date for the site in the first quarter of the 6th century should not be "completely disregarded" (1976: 281, n. 33).

Nevertheless, the conventional dating of the Greek finds encouraged Naveh to support a 7th-century date. From the Hebrew ostraca, Naveh deduced that the fortress was built by the expansionist Judahite monarch Josiah (640–609 BC) and abandoned in 609 BC, when Josiah was killed by the Egyptians at Megiddo.



This date, unfortunately, was then used by Cook to confirm his dating of the Wild Goat style of East Greece and chronologically related Corinthian styles:

There is also Meṣad Ḥashavyahu near Ashkalon, where a small fort which was in use only a short time contained pottery of the Middle II Wild Goat style—and also several Hebrew inscriptions. Since the inscriptions are in Hebrew and this area was occupied by the Egyptians in 609 BC, it is reasonable to suppose that the pottery is earlier and consequently that the Middle II Wild Goat style and, since it seems contemporary, Early Ripe Protocorinthian did not begin later than 620 BC (Cook 1972: 264; cf. Cook and Dupont 1998: 9).

Noting the circularity in argument, Francis and Vickers offered a much later date for the site, in step with their radical revision of Archaic Greek pottery dating (Francis and Vickers 1985: 137; Vickers 1985: 17–20). Though they seem to have been unaware of Lapp's comments, they noted Stern's remark that "several of the pottery types found at the site are of great interest . . . for they are generally considered to belong to the Persian period"—namely "jars with basket handles, 'Persian bowls' [mortaria], and amphorae" (Stern 1982: 19). Accepting the Persian tag at face value, they proposed that Meṣad Ḥashavyahu was built during the Early Persian period (539 BC onwards), "and perhaps marked the furthest extent of Cyrus' empire at the time" (Vickers 1985: 20).

However, Francis and Vickers' bold redating of Meṣad Ḥashavyahu met with little welcome—for two reasons. First Aegean archaeologists generally felt that a revision of Archaic pottery dating by as much as 70 years at this point was too extreme (Cook 1989; Whitley 2001: 72–74). Second, there had already been a sea-change in opinion among Israeli archaeologists regarding the allegedly Persian-period forms from the site. Elsewhere in the volume that Francis and Vickers had quoted, Stern showed that the "Persian" tag was no longer really applicable—the same mortaria ("Persian bowls") and basket-handled vessels had been found in a number of contexts dated though to date to the 7th century BC (Stern 1982: 97). Waldbaum and Magness list further sites where the mortaria have been found in pre-Persian contexts: "The conclusion to be drawn is that the flat-based mortarium and early types of basket-handled amphoras are completely at home in a seventh-century context and cannot be used for establishing the later date proposed by Francis and Vickers" (1997: 39–40).

Yet while removing an argument in favour of Francis and Vickers' case, this evidence certainly does not rule out a later dating for the site. For example, in their list of "clear Iron Age contexts" which have produced flat-based mortaria like those from Meṣad Ḥashavyahu, Waldbaum and Magness include the fortress of T.21 ("Migdol") near Pelusium on the Egyptian border (1997: 39). Migdol

is a Late Saite fortress, most remains from which seem to belong to the 6th century BC.<sup>10</sup> It remains true that the mortaria and other erstwhile “Persian” forms now thought to have started in the seventh century BC continued to be made through the sixth century and Persian period. Fantalkin’s conclusion here is balanced: “. . . it is clear that in this case, these common shapes (following traditional manufacturing methods) cannot be used for dating purposes” (Fantalkin 2001: 129).

With regard to the local ceramic finds, Fantalkin remarked that “the present state of research does not permit the unequivocal identification of the typological differences between the end of the 7th century and the beginning of the 6th century BCE” (Fantalkin 2001: 128). Thus, in terms of the parameters presently allowed by the non-Aegean pottery there are no grounds for ruling out an early 6th-century date for the site, as allowed, for example, by Holladay in 1976 (see above).

From the Aegean perspective, while Francis and Vickers’ proposed redating of the site to the late 6th century BC, has generally been judged to be too extreme, this does not mean that the wider questions they raised about Archaic dating are redundant. Far from it. Their forays into chronology have spawned a fresh (much-needed) critical approach to Archaic and Early Classical dating, and with respect to sculpture, epigraphy and coinage, their initiative has contributed to a number of small, but significant, downward revisions. For example, Gill’s redating of the sculptures from the Aphaia temple on Aegina is exemplary—in that even using the conventional dates for the pottery underlying the temple he could demonstrate that the traditional dating for its construction was too high (Gill 1988; 1993). Yet sculpture, coinage and pottery are different matters, and it may be where Francis and Vickers over-extended the lines between these that their suggestions overshot the mark.

A matter overlooked by Francis and Vickers is that there has *always* been a strong undercurrent in the literature pulling for lower dates for the Corinthian and contemporary styles. In 1931 Payne established his (now standard) Corinthian chronology on the basis of western colonies such as Selinus but was taking a stand against other scholars whose dates were some 45 years lower. Gjerstad, the doyen of Cypriot archaeology, suggested lowering Middle

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10 According to Oren, the local shapes “are most characteristic of the 6th century BC” He notes, for example, that some types are identical to those buried in a foundation deposit at Nebesheh with seals of Pharaoh Amasis (1984: 13, 14). This supports the excavator’s assumption, extremely probable in historical terms, that this strategic fortress defending the eastern margins of the Egyptian delta was destroyed during Cambyses’ invasion of 525 BC (1984: 38).

Corinthian by 25 years, Langlotz by some 20 to 30 years.<sup>11</sup> The excavators of Samos used a low Corinthian chronology, placing the end of Early Corinthian (hereafter EC) 25 years later than the generally accepted dating (Walter 1968; Walter-Karydi 1973). More recently Morris, in a review of the evidence from the Greek colonies in Sicily, and in particular Selinus, suggested reducing the beginning of Late Protocorinthian by 10 or 20 years, with a consequent lowering in the start for Early Corinthian (Morris 1996: 57).<sup>12</sup> The one 'low' suggestion that has so far found wide currency is that of Amyx, who argued that the Early to Middle Corinthian transition should be lowered by five to ten years to 595/590 BC (Amyx 1988: 428).

The present writer's view is that a revision approximately halfway between the Francis and Vickers and standard models is in order—in other words, at c. 600 BC (conventional) the dates for Corinthian and East Greek should be lowered by three to four decades, rather than six to eight (James et al. 1991: 359, n. 11; 372, n. 65; James 2003; James 2004: 53–55; James 2005b; James 2006). Independently, Bowden offered a similar solution, after showing that the ceramic dating at three key sites (Naukratis, Old Smyrna and Tocra in Cyrenaica) was out of step with the Herodotean dating by some four decades (Bowden 1991; 1996).

At Naukratis, Middle Corinthian (conventional 600–575 BC) and contemporary East Greek styles are well attested and associated with the earliest structures (temples). There are also a few unstratified Early Corinthian pieces, while an example of Wild Goat II Middle was found near the bottom of the temenos pit of the temple of Apollo (James 2003: 240, 250). Accordingly, the beginnings of the Greek settlement are dated conventionally to 615/610 BC, within EC (Cook 1937). This has led to a rejection of Herodotus' testimony that Naukratis was given to the Greeks, as a trading settlement where they could build temples, by Pharaoh Amasis (570–526 BC). Rather, slightly modifying Bowden's case, I have argued that the evidence from Naukratis (historical, Egyptian, Cypriot and Phoenician) favours a c. 35-year lowering in Archaic pottery chronology at 600 BC (James 2003).

11 For references to these and other low daters, see James (2003: 260–62).

12 It has been argued that the entire Protocorinthian and Geometric phases need considerable lowering. For the alleged anchor points in the Levant for dating these styles see James et al. (1991: 99–110; 1998: 39). Late Geometric dates are too high, by at least 25 (James et al. 1991: 111; James 2003: 241–43; 2005b). Such a reduction has been accepted as plausible by I. Morris (1993: 30–31), while S. P. Morris (1998: 362) has argued that the "Geometric period lasted well into the seventh century." For the case of Tyre, see James (2008: 144–47).

Bowden identified a similar problem at Tocra. The earliest pottery (Early Corinthian) has given rise to a conventional date of 620–600 BC for its foundation, whereas the Herodotean account indicates that Tocra and other sites outside Cyrene were not founded until the expansion of settlement c. 580 BC. Tocra forms part of a complex of interrelated problems. Two, possibly three, other colonies (Euesperides, Ptolemais and perhaps Apollonia), which one would have expected to be daughter-colonies of Cyrene, begun during the same expansion of settlement, also have remains from the Early Corinthian horizon (Schaus 1985: 100; Gill 2005). The historical and archaeological chronologies on this point are thus offset from each other by some three to four decades and the same applies to the very earliest settlements in Cyrenaica. Traditionally, Cyrene was founded in c. 631 BC, and its short-lived predecessor Aziris c. 637 BC. Yet at Aziris there is a horizon of pottery conventionally dated c. 675/670 BC, and at Cyrene a series of terracotta sculptures of the same date (James 2005b).

At Old Smyrna, Early Corinthian (not the latest) was found in a destruction layer associated with the conquest of the city by Alyattes the Lydian. Yet as Langlotz and Bowden have noted, the text of Herodotus suggests that the sack of Old Smyrna took place after the Median war of 585 BC (James 2003; James 2005b). Langlotz argued that the destruction of Old Smyrna (believed to be the work of Alyattes of Lydia) took place in 580 BC rather than 600 BC, a date to which Cook objected as it “would be difficult to maintain, if the interpretation of Mesad Hashavyahu is correct” (Cook 1989: 165, n. 18).

As I noted in a general review:

Either Herodotus is wrong or the conventional Archaic pottery chronology is wrong. The stakes are high. Herodotus' narrative involves a fairly tight pattern of synchronisms between Greece and neighbouring countries such as Lydia, Egypt and Babylonia. If he is wrong about major factors such as the origin of the Greek settlements at Naukratis and Cyrene we are in danger of jettisoning a large part of Archaic Greek history (James 2004: 54).

Outside of the Levant, all the “fixed points” listed by Cook for 7th-century Greek pottery chronology are either seriously flawed or fit better with a lowering of Early Corinthian and contemporary East Greek styles by a notional 35 years (James 2005b: 12–14). As already noted, Meḏad Ḥashavyahu produced a number of sherds of the Milesian Middle II Wild Goat style, contemporary with Early Corinthian, together with other styles conventionally dated to the late 7th century BC (Fantalkin 2001: 74–97). As Meḏad Ḥashavyahu is presently

one of the main lynchpins for the standard pottery chronology of the Archaic (and particularly the Middle II Wild Goat style), it provides an ideal test for the proposed revision. Is the evidence conclusive for dating its abandonment to the late 7th century BC, or can a better case be made for redating this event to a point near c. 565 BC?

### Historical Dating of the Garrison and the Ashkelon Problem

Regarding the problems of Archaic pottery chronology, Boardman has stressed the need for “written sources, but preferably not a hundred years and more after the event, and preferably not Greek at all” (Boardman 1994: 147). In an ideal world the Near Eastern contexts of Early Corinthian and related East Greek wares such as Meḏad Ḥashavyahu would contain well-dated epigraphic material excavated from sound contexts. Unfortunately such contexts are presently absent. The nearest case concerns Ekron, where the famous inscription was discovered commemorating the building of the temple of Stratum 1C, beneath that (Str. 1B) containing the Greek imports. The inscription names individuals known from Assyrian records, yet it would seem that the most obvious dates thus suggested have been sacrificed to the expectations from “pottery dating” (see below).

Without such ideal contexts, we have recourse to the historical records of Egypt and Babylonia, which can offer links between military events and the destruction levels/settlement breaks at sites revealed by excavation. Unfortunately records of the Asiatic campaigns of the Saite pharaohs are virtually non-existent (Spalinger 1977: 228–29). Likewise, royal records of the military activities of the Neo-Babylonian kings are singularly rare (Eph’al 2003: 178). The best evidence we have for Neo-Babylonian campaigns comes from the patchy record provided by the laconic entries in the Babylonian Chronicles. The surviving entries for the Neo-Babylonian Empire amount to some twenty years, less than thirty percent of the Empire’s duration (James 2004: 51).

Further, an important point often overlooked by archaeologists, is that our copies of the Chronicle series are *not* understood to be contemporary with the events they record. Wiseman noted that the script of the Neo-Babylonian Chronicle tablets “does not of itself allow any precise dating but which can mean that they were written from any time almost contemporary with the events themselves to the end of the Achaemenid rule” (Wiseman 1956: 4). Only two of the Neo-Babylonian Chronicle tablets have colophons. BM 92502 (Grayson’s *ABC 1A*) covers the period 747–648 BC and is dated to the 22nd year of a king whose name is usually restored as Darius (I), i.e., 500/499 BC

(Wiseman 1956: 1; Grayson 1975: 69, 87; Waerzeggers 2012: 283). It was “copied from an older and damaged text and claims to be the first of a number of tablets, or chapter-extracts, of the same kind” (Wiseman 1956: 4). The name of the king is badly damaged and Brinkman has not ruled out Artaxerxes I, giving a date for the colophon of 443/2 BC (Brinkman 1990: 85, n. 68). The second is BM 96273 (*ABC* 15), the so-called “Shamash-shuma-ukin Chronicle”, where the latest event mentioned is his rebellion in 652 BC. Here the colophon names the copyist as one Nabû-kāšir descendant of Ea-ilūta-bani; prosopographic evidence enables him to be reasonably identified as an inhabitant of Babylon’s sister city, Borsippa, who lived during the reign of Nabonidus (556–539 BC) (Waerzeggers 2012: 290, 295), but who could well have survived into Early Achaemenid times. In both cases we are dealing with texts *a hundred to a hundred and fifty years later* than the events they record. In lieu of more precise means of dating, one has to assume that the Chronicles that record the subsequent Neo-Babylonian Empire may have also been committed to clay in the Achaemenid period.<sup>13</sup> Though they are generally considered reliable for the periods they cover (Grayson 1975: 14), copyists’ errors could well have crept in. And, strictly speaking they fall short of Boardman’s *desideratum* for non-Greek sources “preferably not a hundred years and more after the event.” When he wrote these words, Boardman was surely implying a contrast with the classicists’ traditional reliance on Herodotus (c. 440 BC)—who, ironically, wrote in the time of Artaxerxes I.

It is from such sparse ‘primary’ sources—necessarily fleshed out with the evidence from the odd inscription, the Old Testament, Herodotus and other classical writers—that we can draw a picture of the imperial struggles in the Levant during the late seventh-early sixth centuries. Though inadequate, they are all we have to tackle the question: what is the best historical ‘window’ in which we might envisage the Egyptians establishing a garrison of Greek mercenaries in order to provide logistical support for the port of Yavneh-Yam?

Fantalkin (2001: 136) sets parameters of no earlier than the 620s and no later than 604 BC. The key date of 604 BC, as used by Fantalkin, is based on an understanding of the Babylonian Chronicle: “Of vital importance in dating

13 The analysis of Waerzeggers (2012) suggests an ingenious way of distinguishing between Chronicle tablets of the Babylon and Borsippa schools—using their accession dates by the British Museum. She assigns the crucial BM 21946 (*ABC* 5), recording the early years of Nebuchadrezzar II, to Borsippa and tentatively suggests a mid-sixth century BC date for the end of the Borsippa school of Chronicle writing. This remains speculative, however, as the greater corpus from Borsippa continues into the reign of Xerxes (485–465 BC) (cf. Waerzeggers 2012: 294).

Mezad Hashavyahu is the destruction date of Ashkelon by Nebuchadnezzar in the month of Kislev 604 BCE, as reported in the Babylonian Chronicle . . .” (Fantalkin 2001: 131; cf. Na’aman 1991: 47). Fantalkin has recourse to this date twice, in both archaeological and historical arguments. Noting parallels between the local and imported pottery assemblages of Ashkelon (Stager’s pre-Persian stratum, including a dozen Early Corinthian sherds) and Mezad Hashavyahu, Fantalkin concludes that the latter “should be dated toward the end of the 7th century BCE.” The historical argument, a strategic consideration, is used by Fantalkin to rule out Wenning’s (1989) suggestion that Mezad Hashavyahu may have fallen later, c. 598 BC: “Assuming that the fortress was ruled by the Egyptians, it seems unreasonable that an Egyptian fortress would remain standing while the Babylonian army advanced towards Ashkelon” (Fantalkin 2001: 144).

Two key assumptions are involved here: that Ashkelon fell to the Babylonians in 604 BC; and that the Babylonians had finished their conquest of Palestine by the end of the 7th century, terminating the Saite empire in the Levant.

Fantalkin follows the widely held understanding that the fall of Ashkelon is reported in the Babylonian Chronicle entry for 604 BC. But it cannot be stressed enough that the whole matter hinges on the partial restoration of the name “Ashkelon” for the city captured and levelled in that year (James 2004: 54; 2005b: 20; 2006). Both editors who have published the tablet in question (B.M. 21946) have repeatedly stressed caution about the reading of this name (Wiseman 1956: 85; Grayson 1975: 100; Grayson 1980: 161; Wiseman 1991a: 23, n. 158). Wiseman has gone as far as to say that “There is no sure extra-biblical evidence of any Babylonian military activity in ‘Philistia’, the coastal plain, or in Judah before Nebuchadnezzar’s seventh year [598/597 BC]” (1991a: 28). Finkel<sup>14</sup> and Zadok<sup>15</sup> have re-examined the tablet more recently and are confident that “Ashkelon” is the most plausible reading of the name. Nevertheless, this is hardly the kind of indisputable (or even contemporary) textual evidence that Boardman called for. Nor should the dangers of building a historical case on a textual restoration, however reasonable it may seem, need pointing out.<sup>16</sup>

14 Personal communication; cited in Stager (1996: 72\*, n. 1).

15 Personal communication to P. James (Feb. 2006); see also R. Zadok, personal communication, in Fantalkin (2011: 87, n. 1).

16 For one germane example, the reading of the name of the land conquered by Cyrus of Persia, in the year 9 entry of the Nabonidus Chronicle (547/6 BC), was for decades erroneously read as “Ludu” (Lydia)—see Cargill (1976) and Rollinger (2008). For a detailed discussion of the date of the fall of Sardis, with a proposed lower dating, see Kokkinos

The reason that there is difficulty in reading the name is that the first two signs (*ish*, *qi/ki*) were written over erasures, while the third (restored as *il* or *i*) was itself erased.<sup>17</sup> This suggests either that the scribe copying this document (very possibly in the Achaemenid period) was working from a damaged tablet or that he hesitated in transcribing a city-name that was unrecognisable to him, conceivably adjusting it to something more familiar. As we know from the Greek imports, Ashkelon was flourishing again by c. 500 BC, having been rebuilt “on a quite impressive scale” under the Persians (Stern 2001: 408–11).

Most importantly, even if the reading “Ashkelon” is correct (to the original version), this needs to be related to the archaeology of the site—a point stressed by the present writer (James 2004: 54) that has been widely ignored. The current excavators have associated the event with a destruction horizon, most conspicuous in South Tell Phase 7 (Grid 50: marketplace) = Phase 14 (Grid 38) winery—remains of the last settlement before the Persian city (Stager 1996). But the modern work (as so far published) has not been extensive enough to rule out that Nebuchadrezzar’s putative conquest might actually relate to a lower stratum. The new excavations have only recovered very limited deposits from the late Phase 8 town underlying Phase 7.<sup>18</sup>

On the North slope of the tell the excavated squares uncovered pottery of the 8th century–7th centuries (Phase 8) associated with fortifications which “most likely fell during the Babylonian attack in 604 BC.”<sup>19</sup> Would the uncertainty here allow that this part of the fortifications was actually destroyed at the end of Phase 8? Stager et al. state that the pottery and small finds “indicate the latest occupation behind the defenses was probably in the eighth or seventh century BC.” (Stager et al. 2008: 236). The pottery from the find spots inside the curtain wall at this point has not been published, with the exception

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(2009); Nikos Kokkinos and I hope to discuss elsewhere the ramifications for ceramic and Athenian historical chronology.

17 Of the tentative reading *iš?-qi?-il-lu-nu*, Wiseman (1956: 85) wrote: “The first two signs are doubtful, being written over an erasure.” Finkel (personal communication, in Stager 1996: 72\*, n. 1) stated that the third syllable is “possibly” an “erased aleph”. According to Zadok’s recent re-examination of the tablet the second syllable (over erasure) should be read as *ki* rather than *qi* (personal communication, February, 2006).

18 This has raised a problem for making any reasonable population estimates for this period, as noted by Fantalkin (2011: 92): “The archaeological evidence for 8th–7th centuries BCE. Ashkelon is limited and inconclusive and we lack a number of crucial variables required for any attempt to estimate the population of the city during this period (such as the size of residential areas versus nonresidential areas within the city walls, the expected population density based on median number of households per hectare, etc.)”

19 Stager et al. (2008: 236), emphasis added.



of this remark: "Some of the finds, including Phoenician Fine Ware, parallel those from the silos and pits of Grid 38 Phase 15." Phase 15 of the South Tell is equivalent to Phase 8 of the North slope; the end of both is dated by the excavators to c. 700 BC (Stager et al. 2008: 236 and 217, table).

The possibility that there is another Late Iron Age destruction level in the tell below that of Phase 7 is raised by the findings of the early British excavations on the South Tell. In a deep section cut into the sea cliff in 1921, Phythian-Adams identified three burnt levels of Iron Age date: the lowest ("black earth full of ashes") separates the Bronze Age stratum from the first Philistine pottery; the uppermost ("ashes") must be that identified by Stager as the work of the Babylonians in 604 BC. In between these was another destruction level ("black earth with ashes") (Stager et al. 2008: 156–57 and fig. 9.9). The modern expedition did not re-excavate the sea-cliff section so the relative date of this second ash layer remains unknown. Trenches were dug in Grids 50 and 57, either side of Phythian-Adams' section (Stager et al. 2008: 156 and 187, fig. 11.1), but have thrown no light on the question. Excavation of Grid 57 ceased at the level of the Persian period (Phase 6) (Stager et al. 2008: 319), while in Grid 50 the Phase 8 ("late 8th to early 7th" century) deposits were "difficult to understand", having been destroyed or massively disturbed by the quarrying work of Phase 7. Where the quarrying had left some coherent architecture an alleyway was discovered containing household waste and ash, but whether the latter was from a burning of this part of the city or from domestic waste is unclear from the very brief report (Stager et al. 2008: 308).<sup>20</sup> Thus, like the work on the North slope, the recent excavations on the South Tell have shed almost no light on the character and fate of the settlement that existed at Ashkelon during this Phase. Until more is known about Phase 8, the assumption that the destruction of Phase 7 can automatically be identified with a campaign of Nebuchadrezzar lacks vital context and remains, strictly speaking, speculative.

20 The extent of the damage done to earlier levels by quarrying is confirmed in the latest site report on the "7th century BC" at Ashkelon, see Stager et al. (2011: 31), which provides a clear photo-map of the large quarried area and the confirmation that, "The south edge of the quarry cut through the center of the excavation area in Grid 50 (Phase 8), removing earlier Iron or Bronze Age occupational strata from the northern half of the excavation area." For other remarks regarding the uncertainty about the dating of earlier Iron Age levels, for example, see Stager et al. (2011: 35). The problem also remains that all remarks about the dating of allegedly "8th" or "7th" century levels still depend on two highly questionable assumptions: (1) that Nebuchadrezzar did actually destroy Ashkelon in 604 BC; and (2) that the evidence of burning, etc., for the South Tell Phase 7 (Grid 50: marketplace) = Phase 14 (Grid 38) winery) relate to this putative destruction.

Dating the end of the last pre-Persian settlement as early as 604 BC has also raised some curious archaeological anomalies. Earlier excavations uncovered a *favissa* containing a large number of Egyptian bronzes, including highly distinctive *situlae*, which were dated to the Persian period (fourth century BC). The renewed excavations of Stager found seven such *situlae* in the pre-Persian destruction horizon (Room 402 of the winery), together with a figure of Osiris:

A twin of our bronze Osiris statuette was uncovered more than sixty year ago in a small salvage excavation at Ashkelon. The excavator, J. H. Iliffe, dated it to the fourth century BC . . .; but it is now clear that this statuette and 25 other bronze statuettes of Egyptian deities, as well as 14 other Egyptian bronze artifacts . . ., which were found in Iliffe's excavation, are contemporaneous with our bronzes—that is, they belong to the late seventh century BC, not the fourth century (Stager et al. 2008: 281–282).

But simply backdating the Egyptian finds does not resolve the problem. Regarding the *situlae* from the new excavations, Stern noted that: “Two almost identical bronze *situlae* bear Phoenician inscriptions and are dated to the Persian period.”<sup>21</sup> Further he still maintains that the statuettes from the *favissa* are from Persian times (Stern 2001: 498).

Similar problems have been raised for palaeographic dating. Cross accepted without question that a date of 604 BC for the last pre-Persian settlement provides a fixed marker for the Phoenician cursive script of this period. For example, regarding an ostrakon from the new excavations, Cross wrote:

It is inscribed in a Phoenician cursive, almost exactly the same as the Phoenician cursive of the Saqqārah Papyrus. Since it comes from the debris of the 604 destruction, it must be dated to the late seventh century BC. *The Saqqārah (Phoenician) Papyrus, as well as a number of other cursives, must therefore be raised in date*—in the case of the Saqqārah Papyrus, to ca. 600 BC. The highly evolved state of the Phoenician cursive, beyond that of the formal or lapidary scripts, has misled scholars, as we have seen, in their attempts to date scripts on the basis of a purely palaeographic typology without fixed absolute dates. For example, the date of the Kition Tariffs must be raised from Peckham's date of 450–400 to c. 650 BC.<sup>22</sup>

21 Stern (2001: 498–500), and illustrations III.30 (Ashkelon statuettes), III:31 (*situla* with Phoenician inscription). For brief discussion see James (2004: 49–50).

22 F. M. Cross in Stager et al. (2008: 339), emphasis added.

Cross' words are well taken, but only if 604 BC *does* provide a reliable "fixed absolute date". As argued above, the available evidence allows that the putative conquest of Ashkelon by Nebuchadrezzar II may have concerned Phase 8 rather than the last pre-Persian Phase 7. At present it is assumed that Ashkelon lay waste between 604 BC and the resettlement under the Persians c. 500 BC, which is rather puzzling in itself—for such an important port-city and economic hub. Elsewhere I have argued against the bizarre idea that the rulers of the Neo-Babylonian Empire (unlike the Assyrians) simply laid waste to cities in the Levant and deliberately left (or kept) them deserted for decades, leaving us with a "Babylonian Gap" in the archaeological record. This makes no sense in economic terms. Extreme ideas, such as that expressed by Stern that the Babylonians' "entire focus was on the welfare of the city of Babylon and its immediate surroundings, while the periphery was largely neglected" (Stern 2001: 303), are flatly contradicted by literary and archaeological evidence (James 2004: 50–52). A date for the destruction of pre-Persian Ashkelon much closer to mid-6th century (and the Persian period) would shorten its alleged century-long abandonment and go some way to relieving the chronological tensions raised by the palaeography and Egyptian bronzes—as well as being in step with the lowering of Early Corinthian and contemporary Greek pottery styles outlined above.

Finally there is evidence suggesting that Ashkelon still existed as a city in the early 6th century BC. A series of cuneiform documents from Babylon, dating between 595 and 570 BC, lists the rations administered to dependents of the royal court. These include the captive king Jehoiachin of Judah and "the sons of Aga', the king of Ashkelon". As Bright noted, "It is possible that Aga' still reigned in Ashkelon at this time, the two princes being hostages" (Bright 1949: 49). Actually, there can be little doubt that they were hostages, kept to guarantee the good behaviour of their father or erstwhile subjects. This is Katzenstein's understanding: "We believe that the sons of Aga' were hostages, and that Aga' himself continued to reign for an indefinite period" (Katzenstein 1994: 41). If this reading is correct, then the abandonment of pre-Persian Ashkelon could hardly date to 604 BC, but must postdate c. 595 BC.

For all these reasons, the widely accepted date of 604 BC for the destruction of pre-Persian Ashkelon remains highly problematic, and *far from proven*. Ceramic parallels between pre-Persian Ashkelon and Mezad Hashavyahu cannot be used to date the latter to the late 7th century (see further in n. 20 above).

We should not assume that Ashkelon or the other Philistine cities needed to be cowed only once by Babylonian troops. The Neo-Babylonian dominion over the Levant was not formed in one fell swoop, nor in a single steady progression of campaigns. We know that Jerusalem was attacked twice by the Babylonians,

in both 597 and 587 BC, with a further campaign in Judah in 582 BC. Tyre, which would have nominally submitted in 605/604 BC, had to be put to siege between 584/3 and 572/1 BC (see below) and again, it has been argued, in the late 560s BC (Zawadzki 2003: 279\*; Eph'al 2003: 187). Hamath, Amanus and Amurru, which the Babylonians subdued in the aftermath of Carchemish (605 BC), were the subject of campaigns by Nabonidus in 554 and 553 BC (Oppenheim 1969: 305). And it is usually assumed that Gaza changed hands three times (from Necho II to Nebuchadrezzar, to Necho II again and back to Nebuchadrezzar) in the eventful years at the turn of the seventh to sixth centuries BC.<sup>23</sup> Katzenstein reasonably sees a fourth change as “certain”, when Pharaoh Apries (“Hophra”) moved to relieve the siege of Jerusalem in 588/7 BC;<sup>24</sup> by inference, control over Gaza must have changed a *fifth* time either when Apries was routed or, as Katzenstein suggests, in 582/1 BC. The Babylonian domination of Palestine was far from being monolithic.

### The Late Saite Empire

The corollary, of course, is that Saite imperial adventures in the Levant did not end c. 600 BC. So Kuhrt: “The situation of armed conflict fought out between the two powers on the soil of the Levant was not resolved until after 570 BC when Babylonia and Egypt reached a concordat, with the Levant passing effectively into Babylonian hands” (2002: 23–24).

The history of the Saite Empire in the Levant can be roughly divided into three phases. The first phase, under Psammetichus I (664–610 BC) and the earlier reign of Necho II (610–595 BC), is the best documented (as the Babylonian Chronicle is preserved for those years). Starting in the 630s or 620s, the Egyptians took over the old Assyrian empire in the Southern Levant, most probably by arrangement (Spalinger 1978: 50; Na'aman 1991: 39). By the 610s the Egyptians were fielding armies in Northern Syria and deep into Mesopotamia, in support of the Assyrians, until catastrophe struck with their defeat at Carchemish in 605 BC. In that year the Babylonians, according to the Chronicle, claimed sovereignty over the entire land of Hatti (Syro-Palestine). In 601 BC the Chronicle records how the two sides fought each other to a standstill in battle (Wiseman 1956: 71), evidently at Migdol (the Magdolos of Herodotus 2.159) on the Egyptian border (Lloyd 1988: 161–63; Redford 1992: 458–59). As the Chronicle notes that Nebuchadrezzar stayed in Babylon the following year, the Egyptians seem to

23 Katzenstein (1994); Rainey (2001: 61); Fantalkin (2001: 143); and see below.

24 Katzenstein (1994: 46), and see below.

have had the edge (Redford 1992: 458–59). Necho followed up by retaking Gaza (the Kadytis of Herodotus 2.159; cf. Jeremiah 47:1) though, as already noted, this was to prove a short-lived gain.

The second phase is represented by the final years of Necho II (c. 600–595 BC) and the reign of Psammetichus II (595–589 BC), when there is almost no evidence for Egyptian military intervention in Levantine affairs. To the contrary, 2 Kings 24:7 states that by the reign of Jehoiachin (598–597 BC), “the king of Egypt came not again any more out of his land; for the king of Babylon had taken from the river of Egypt unto the river Euphrates all that pertained to the king of Egypt.” Conversely, the Babylonian presence was particularly strong in Southern Palestine at this time; the account in 2 Kings of Nebuchdrezzar’s first subjugation of Judah is confirmed by the Chronicle entry for the year 598/597. Gaza must have been retaken by the Babylonians, as shown by the appearance of its king in the court list on the Istanbul prism, now usually dated to 598/7 BC (Na’aman 2000: 40; Vanderhoof 2003: 238; cf. Wiseman 1991a: 73; Katzenstein 1994: 46). The reason for the lack of Egyptian military intervention in Asia may partly be that the Saïtes were preoccupied with troubles on their border with Nubia (Redford 1992: 462–63).

Nevertheless in 591 BC Psammetichus II undertook a trip to “Kharu”, an Egyptian geographical term that included both Palestine and Phoenicia. In this instance the term has usually been taken to mean Phoenicia, with the Pharaoh’s trip by sea, and because of the lack of military reference in the brief account that we have, it is often interpreted as peaceful, religious (though obviously propagandistic) expedition (Freedy and Redford 1970: 479–80; Spalinger 1977: 233–34; Redford 1992: 464; Katzenstein 1997: 316–17). However, Kahn rightly argues that the case for this being a religious embassy (e.g., to Byblos) is weak, as the only evidence comes from a Persian-period document where the author’s concern is to stress the role of his priestly ancestor in the expedition (Kahn 2008: 148–51). On balance, we might see the expedition of Psammetichus II to Kharu as a small show of force, most likely in Philistia, but one which had little effect; there is no reference to such a ‘campaign’ in the biblical or Greek sources. Kahn argues that Psammetichus II took advantage of a temporary period of Babylonian military weakness in the region. This may be so, and it is possible that Psammetichus II may have initiated the more aggressive policy evident under his successor Apries.

The third phase of the Saïte Empire is no less important in terms of the historical developments which may have led to the founding of an Egyptian fortress at Mezad Ḥashavyahu. As it is less well documented and somewhat neglected in the literature, a brief summary is attempted here. It begins with the accession of Apries (589–570 BC) when, “a sharp change in Egyptian

foreign politics can be detected”, with a renewal of “the old claim of Egyptian suzerainty over the Eastern Mediterranean coast” (Katzenstein 1997: 317). Apries had a hand in encouraging the rebellion of Zedekiah of Judah against Nebuchadrezzar (Ezekiel 17:15). From Jeremiah (37:5) we know that Apries brought his army to Judah in an attempt to lift the Babylonian siege of Jerusalem (588/7 BC). Outnumbered or outmanoeuvred, the Egyptian army returned home (Jeremiah 37:7). The dating of Apries’ other campaigns is more difficult to determine. The only direct references come from classical sources. Herodotus (2.161.2) states that Apries “sent an army against Sidon and fought at sea with Tyre”. Perhaps less reliably, Diodorus adds Cyprus to these conquests.<sup>25</sup> While there is general agreement as to the reality of the expeditions to Sidon and Tyre, their nature and dating have given rise, in the words of Lloyd, to “disturbing divergences of opinion” (1988: 171).

The most conspicuous problem here is the relationship of these expeditions to the thirteen-year siege of Tyre by Nebuchadrezzar, known from the ‘Tyrian Annals’ (*apud* Josephus via Hellenistic sources) and Ezekiel. This is generally dated to 586/5–573/2 BC,<sup>26</sup> now refined by Kokkinos to 584/3–572/1 BC (Kokkinos 2013: 49)—in any case contemporary with the best part of the reign of Apries. As Miller and Hayes note: “That Egypt fought against the Phoenician states while Tyre was under siege by Nebuchadrezzar, and this in support of the Babylonians, seems highly unlikely” (1986: 427). Indeed, all commentators have ruled out the possibility that Apries was working in alliance with Nebuchadrezzar. This leaves three possible interpretations: (1) Apries attacked Phoenicia before the Babylonian siege began (>584/3 BC); (2) Apries attacked Phoenicia after the Babylonian siege ended (<572/1 BC); and (3) Apries’ manoeuvres were in defence of Phoenicia against Nebuchadrezzar during his siege of Tyre.

25 Diodorus (1.68.1): “Apries was king for twenty-two years. He made a campaign with strong land and sea forces against Cyprus and Phoenicia, took Sidon by storm, and so terrified the other cities of Phoenicia that he secured their submission; he also defeated the Phoenicians and Cyprians in a great sea-battle and returned to Egypt with much booty”. While Katzenstein (1997: 318) feels there “can be no doubt that Diodorus Siculus depends on Herodotus” here, Lloyd (1988: 171) thinks that “the differences between his account of Apries’ reign and that in H[erodotus] prove that the former had access to independent sources”. Differences in the account of the civil war between Apries and Amasis may also suggest that Diodorus had sources independent of Herodotus (Leahy 1988: 189). However, it would be unwise to set much store on Diodorus’ account here until the nature and quality of such possible sources has been further investigated.

26 Freedy and Redford (1970: 469, 484); Wiseman (1991b: 235); Katzenstein (1997: 326); Zawadzki (2003: 277\*); Eph’al (2003: 186).

With regard to the first possibility, such action would have taken place in the first years of the reign of Apries (i.e., 589–586 BC). Freedy and Redford (1970: 482–83) and Katzenstein (1997: 318–19) dated Apries' Phoenician campaign to 588 BC, arguing that its purpose was to force the Phoenicians into an anti-Babylonian alliance. Freedy and Redford feel that Apries' action was a success, and that it occasioned Nebuchadrezzar's attack on Tyre.<sup>27</sup> While plausible, arguably there may have been little time for Apries to have mounted an expedition against Phoenicia at such an early date. His accession year was in 589 BC, and in 588/587 BC his army was attempting to raise the Babylonian siege of Jerusalem.<sup>28</sup>

Lloyd prefers the second interpretation, dating Apries' Phoenician adventures to the period after the end of the Babylonian siege and before the usurpation of the Egyptian throne by Amasis in 570 BC (Lloyd 1983: 339; 1988: 171–72).<sup>29</sup> We are close here to the old suggestion of Maspero that Apries' Phoenician campaign fell in 571 BC (Maspero 1903–1904: 437–38). The motive of Apries' Phoenician campaign would then have been to recapture Tyre and Sidon from Babylonian control.

The third possibility, that Apries' manoeuvres were actually in support of the Phoenicians during Nebuchadrezzar's long siege of Tyre, has also found support (Miller and Hayes 1986: 427; James 1991: 725; Grimal 1992: 362–63). On their rebellion against Nebuchadrezzar, the Tyrians would almost certainly

27 Lloyd (1988: 171) has argued that there is a serious flaw in such a scenario. Jeremiah 27:1–3 refers to the messengers of the kings of Edom, Moab, Ammon, Tyre and Sidon meeting at the court of king Zedekiah, in a usually understood to refer to a six-nation conclave planning rebellion against Nebuchadrezzar. Lloyd (1988: 171) concludes that “If, as Jeremiah suggests, Tyre was involved in the revolt of Zedekiah, we would expect her relations with Egypt to have been good from 589 to mid-587”, i.e., the first three years of Apries' reign; ergo, an aggressive Egyptian war against Phoenicia at this date can be ruled out. Yet the point is not as strong as Lloyd feels. The anti-Babylonian conclave is normally dated to within the first four years of the reign of Zedekiah (Miller and Hayes 1986: 410; Katzenstein 1997: 315–16), i.e., 597–594 BC, *before* the reign of Apries. NB the absence of Philistine envoys at the conclave should not be read as indicating that their cities had already been destroyed by Nebuchadrezzar; for discussion, see James (2006: 92).

28 It is, of course, not entirely impossible that Apries simultaneously sent a flying column to Sidon and the fleet up to Tyre, in an attempt to widen the conflict by forcing the Phoenicians into revolt. However, such a strategy would have been incredibly foolhardy when his main force was facing the Babylonians in Judah, and there is no evidence to recommend such a complicated scenario.

29 Lloyd himself offers 574–570 BC, but the first date in this range is slightly too high. Much depends here also on the exact date of the accession of Amasis, which needs reinvestigation.

have made a defensive alliance with the Egyptians. At a general level this interpretation seems the most attractive. It obviates the awkward idea that Egypt, the natural ally of Levantine kingdoms threatened by Mesopotamian conquerors, would have resorted to aggressive action against such states; or that the adventurous Apries would have ‘sat on his hands’ for thirteen years while the Babylonians took over an area of such longstanding economic and cultural importance to the Egyptians. An apparent problem with this interpretation is, of course, that it suggests the account given by Herodotus, or his source, to have been mistaken in talking about land and sea forces being sent, respectively, *against* Sidon and Tyre.<sup>30</sup>

With the sparse information at our disposal, it is hard to decide between these different scenarios.<sup>31</sup> There is not a great difference between them chronologically, the range being 589–571 BC. Nevertheless, one further factor strongly tends towards a date late within this range, hence preferring the second and third interpretations. This is the wording and arrangement of the prophecies of Ezekiel, which suggests that there was an intimate connection between Nebuchadrezzar’s siege of Tyre and his impending invasion of Egypt, prophesied by Ezekiel (29:17–19) in the “twenty-seventh year” (of king Jehoiachin’s exile), i.e., 572/1 BC:

In the twenty-seventh year, in the first month, on the first day of the month, the word of the Lord came to me: Mortal, King Nebuchadrezzar of Babylon made his army labour hard against Tyre; every head was made

30 Though, as Nikos Kokkinos reminds me (pers. comm.), it may be that the Sidonians and Tyrians that Apries fought were factions or forces which were (reluctantly) under the control of the Babylonians. Both ancient and modern parallels could be sought here.

31 The only cuneiform document with possible bearing on this question is the inscription of Nebuchadrezzar from Wadi Brisa north of Tyre (trans. Oppenheim 1969: 307). This claims that he restored peace and prosperity to the Lebanon after “a foreign enemy was ruling and robbing (it of) its riches”. Unfortunately there is little agreement either on its date or significance. Langdon (1912: 33–37) thought it reflected a Phoenician campaign of Nebuchadrezzar in 586 BC. Wiseman (1991a: 26) states that the enemy is “usually understood” as a Phoenician king, though not excluding the Egyptians. Indeed, the latter would make best sense of the “foreign” character of the enemy and this, contra Wiseman, seems to be the preferred interpretation (see e.g., Redford 1992: 465). Spalinger (1977: 228) sees the text as a mainly literary one, written retrospectively to justify the overall takeover of the Lebanon from Egyptian control by the Babylonians in 605 BC. But if it was written retrospectively, it could also refer to a later event. In any case, as Spalinger puts it, “Nebuchadrezzar’s high-sounding words do not provide much meat for the historian’s grill.”



bald and every shoulder was rubbed bare; yet neither he nor his army got anything from Tyre to pay for the labour that he had expended against it. Therefore, thus says the Lord God: I will give the land of Egypt to King Nebuchadrezzar of Babylon; and he shall carry off its wealth and despoil it and plunder it; and it shall be the wages for his army.

These words have often been taken to mean that the long siege of Tyre was a failure (Bright 1981: 352; Katzenstein 1997: 331). Against this reading, Ezekiel elsewhere (26:4) prophesied the complete destruction of the city; hence the verses quoted might simply mean that the capture of Tyre was not deemed worth the massive expenditure of a thirteen-year siege (Freedy and Redford 1970: 472; Miller and Hayes 1986: 427). And whether physically captured or not, it is normally understood that Tyre must have accepted Babylonian suzerainty in order to break the siege. Katzenstein allows, at least, that the Tyrians and Babylonians had arrived at an ‘understanding’, with the Tyrian king Ethbaal being deported to Babylon. In a subsequent invasion of Egypt (567 BC), Nebuchadrezzar brought ships (see below), difficult to imagine unless Tyre was, by then, “definitely in the Chaldean camp” (Freedy and Redford 1970: 484). The exact fate of Tyre in 572/1 BC is not of immediate concern here—only the relationship between the end of the siege and the projected invasion of Egypt. The wording of Ezekiel, that Nebuchadrezzar would seek recompense for the Tyrian siege by invading Egypt, strongly suggests that the two were causally related.<sup>32</sup> This most probably indicates that the *casus belli* for the invasion of Egypt was its interference at Tyre, further suggesting that Apries’ Phoenician expeditions fell close to 571 BC.<sup>33</sup>

The passage from Ezekiel indicates that in 571 BC, after the fall of Tyre, the Babylonian army moved against Egypt. On this point we are confronted with a dire lack of contemporary evidence, but there is a wealth of traditions from Christian and Arabic writers, conveniently presented and analysed by Spalinger (Spalinger 1977: 236–241). These either link the invasion of Egypt to the siege of Tyre or to Nebuchadrezzar’s desire to capture fugitive Jews. Though they can contain anachronistic elements (from the Persian invasion

32 The situation is closely paralleled by similar events in the early 7th century BC. Even after Hezekiah’s submission to Sennacherib in 701 BC, Phoenicia and Philistia remained a problem for the Assyrians, due to the constant interference of Taharqo. It was the aim of neutralising this that led Esarhaddon to attack Egypt itself in 674 BC (Morkot 2000: 262–64).

33 It is also entirely possible, of course, that Apries’ Sidonian and Tyrian expeditions took place at different parts of his reign. However, the evidence overall suggests that at least the Tyrian campaign was late in his reign.

of Egypt), many of these sources (including a Coptic papyrus which according to Spalinger includes genuine 26th Dynasty allusions), specifically name Apries as the king of Egypt at the time. Another, Ethiopic, account, relates that Apries was defeated and killed by Nebuchadrezzar. It is hard to know what to make of such a “motley collection of texts” (Leahy 1988: 194). Nevertheless it remains that there was a persistent tradition that Apries’ reign ended with an invasion of Egypt by Nebuchadrezzar. Here the best account, based on Hellenistic sources, comes from Josephus, who states (*Antiquities* 10.182) that Nebuchadrezzar “invaded Egypt in order to subdue it, and, having killed the king who was then reigning . . . appointed another.” How far the later traditions were reliant on Josephus is a moot point, but his account can only relate to the replacement of Apries by Amasis. Spalinger believes that there is no *a priori* reason to reject the tradition of Josephus and that there was a Babylonian invasion of Egypt in the reign of Apries (1977: 240).

The evidence for a slightly later invasion of Egypt by Nebuchadrezzar is far more secure. It is described in a cuneiform fragment (B.M. 33041) dating to his year 37, i.e., 568/7 BC (Wiseman 1956: 30, 94–95; Oppenheim 1969: 308; Wiseman 1991a: 39–41; Wiseman 1991b: 236; Donbaz 2001: 167, n. 9; Eph’al 2003: 187–88). The laconic account states that Nebuchadrezzar marched against Egypt to do battle. In response “[...] *a(?)*-*su*, king of Egypt” called up his army and allies, but the account of the ensuing battle is missing. The ending of the apparent royal name, “[...] *a(?)*-*su*”, is only matched by that of Amasis from this period (Leahy 1988: 191, n. 30). The episode is mirrored in the Elephantine Stela of Amasis, where the events of his fourth year of reign (567 BC) include the invasion of a large army of “Asiatics”, by land and sea (Edel 1978; Leahy 1988: 190–191). The fragment from Nebuchadrezzar’s year 37 is also supplemented by a badly damaged (undated) prism fragment, identified by Donbaz (2001: 167), which seems to mention a king of Egypt, his palace and city walls. The reference to a palace may have some meaning. It is tempting to suggest a connection with the immense fortified mud-brick and limestone palace built by Apries at Memphis.<sup>34</sup> Leahy (1988: 196) has suggested that this palace, or rather citadel, played a significant role as a base of support for Apries in the account of the civil war with Amasis on the Elephantine stela. Though speculative, a reference to Apries’ palace in a text of Nebuchadrezzar would reflect

34 Petrie (1909); Lloyd (1983: 321–23). Herodotus (2.163) relates that Apries had “a great and marvellous palace” at Sais, in which he was imprisoned by Amasis (2.169). As no trace of this palace has been found at Sais (Lloyd 1983: 321), it is fair to ask whether Herodotus simply mislocated it from Memphis to Sais.

the Babylonian interest in Egyptian affairs that is bound to have preceded his invasion in 567 BC.

The relationship between the historically attested Babylonian campaign of 567 BC in the time of Amasis and the tradition of that against Apries is hard to determine. Here a veritable rubik's cube of possibilities comes into play regarding the role of Nebuchadrezzar in the civil war between Apries and Amasis, 570–567 BC. It may be that the Babylonians intervened in Egypt on the part of the deposed Apries (Lloyd 1988: 178–79; Leahy 1988: 190–91), though this may seem odd considering that the latter had been a thorn in Nebuchadrezzar's flesh for some two decades. Ladynin has offered another, more elegant, solution. He questions the usual assumption that the “[...] - a(?) - su” of the Babylonian fragment should be amended to “[Am]asis”, suggesting that “it could easily correspond to the end of some word other than the name Amasis” (Ladynin 2006: 34). Noting the strength of the biblical and extra-biblical traditions that Nebuchadrezzar deposed his arch-enemy Apries, Ladynin suggests that the invading “Asiatics” of the Elephantine Stela were actually allies of Amasis in the civil war. This would certainly make sense of Josephus' implication that Nebuchadrezzar deposed Apries and appointed Amasis in his stead. Still, as Ladynin notes, harmonisation of all the sources remains elusive. Matters are not helped by our dependence on the old copy of the Elephantine Stela made in 1900 by Daressy, a scholar whose transcriptions are not noted for their reliability. Much may be clarified when the new edition being prepared at Münster University is published (Ladynin 2006: 53).

In the meantime, it seems safe to say that Nebuchadrezzar's attack on Egypt in 567 BC, near the close of the civil war between Apries and Amasis, was preceded by years of enmity and most likely direct conflict. This may have begun with an attack on Apries in 571 BC, itself in response to Egyptian aggression (Leahy 1988: 194). Pending the discovery of new texts, the precise events of the struggles c. 571–567 BC will remain obscure. Nevertheless, a point appreciated long ago by Burn (1935: 145) is that there are likely to have been “plenty of border wars [in Southern Palestine] of which we know nothing” during the poorly documented years of the later reign of Nebuchadrezzar.

For these reasons, in evaluating the destruction dates of various Late Iron sites in Southern Coastal Palestine (including Ashkelon and Mezad Ḥashavyahu), we should not restrict the window of possibility to the last decade of the 7th century BC, as has recently been the case. To do so runs the risk of writing a history of the region falsely weighted by the chance survival of given years in the Babylonian Chronicles. Yet it is clear enough from the geopolitical situation, drawn from the meagre sources available, that there would have been further warfare in the region between 571 and 567 BC.

### A Sixth-century Date for Mezad Hāshavyahu

If, as suggested above, we are to consider a c. 35-year reduction in Archaic Greek pottery (in the late 7th-early 6th centuries BC), then the imported pottery found at Ekron, Ashkelon, Mezad Hāshavyahu, etc., would date to c. 565 BC—remarkably close to the historical date (567 BC) for the documented Babylonian invasion of Egypt. Arguments regarding Aegean pottery chronology aside, there are further considerations that would support a date for Mezad Hāshavyahu c. 570/565 BC, during the third phase of the Saite empire.

#### *Ekron*

The ceramic assemblage of Stratum 1B at Ekron (Tel Miqne) parallels that of Mezad Hāshavyahu and pre-Persian Ashkelon. But in historical terms the usually accepted date, 604 BC, for the destruction of Ekron 1B is unsubstantiated, and Na'aman's (1992: 44) original suggestion that the city may have fallen to the Babylonians *after* 595 BC is still viable, if not preferable (James 2006: 91–92). Otherwise, the discovery of the temple inscription of Ikausu should have forced a rethinking of the chronology of the last Iron Age strata at Ekron, established by historical guesswork before the discovery of the inscription. As I have argued, the most likely date for the building of the temple (and hence Stratum 1C as a whole) is not the first (Gitin et al. 1997: 16; Gitin et al. 1998: 31) but the second quarter of the 7th century BC (James 2005a).<sup>35</sup> If Ikausu were a long-lived king and the temple his swansong, then an even later date, in the third quarter of the 7th century might be contemplated.<sup>36</sup> I have discussed elsewhere the likelihood that this will have a knock-on effect on the dating of the succeeding Stratum 1B, arguing that some major disagreements over the chronology of the site can be resolved by dating 1B not to the late 7th century BC (630/623–604 BC), but to the first quarter of the 6th (James 2006). The inscription alone cannot, of course, demonstrate this. Yet it is noteworthy that

35 Somewhat obfuscating matters, Gitin (2012: 245) still maintains that “it is clear that the dating of Stratum 1C to the first quarter of the 7th century BCE is based on the Neo-Assyrian texts and the archaeological data.” This completely contradicts his own repeated statement that: “It is reasonable to assume that the reign of Ikausu began at or around the time that he is first mentioned in the annals of Esarhaddon” (Gitin et al. 1997: 16). Ikausu of Ekron is first mentioned in Assyrian records in 673 and again in 667 BC. As to the “archaeological data”, Gitin is referring to assumed pottery dates which lack historically fixed points, except the one he stubbornly avoids from Ikausu (only attested from the *second quarter* of the 7th century BC), who built the Ekron Temple which is the defining feature of Tel Miqne 1C. A full reply to Gitin's remarks will have to await another study.

36 Personal communication, L. Stager, 2003.

the *only* firm historical evidence we have for dating Late Iron Ekron would prefer a lower dating for Ekron 1C and, by implication, 1B as well.

### *Tel Kabri*

The fortress of Tel Kabri (inland from the Phoenician city of Achziv) seems to have housed a small Greek garrison, at much the same time as Meḏad Ḥashavyahu.<sup>37</sup> Various dates have been offered by the excavators for its destruction—604 BC, 598 BC (Kempinski and Niemeier 1993: 184) and 585 BC (Niemeier 1994: \*35; 2001: 24), near the beginning of Nebuchadrezzar's siege of Tyre. These dates are based on the assumption that the Greek garrison was in the service of Tyre (cf. Waldbaum and Magness 1997: 38). Alternatively, with Fantalkin, we can see the apparent presence of Greek mercenaries at the site as evidence that this was a shortlived Egyptian stronghold like Meḏad Ḥashavyahu, while still accepting that the Babylonians were the likely destroyers (Fantalkin 2001: 142; 2006: 203). Following the excavators' suggestion, there is no reason why Kabri may not have fallen to Nebuchadrezzar at any point during his siege of Tyre (584/3–572/1 BC), and conceivably at the conclusion or immediate aftermath as Nebuchadrezzar progressed toward Egypt (Kabri lies to the south of Tyre). If so, Kabri may have been fortified by Apries to support his manoeuvres into Phoenicia. In any case, the date of 585 BC for its end, allowed by the excavators, approaches the early 6th-century date for Meḏad Ḥashavyahu argued here.

### *Strategic Considerations*

The similar repertoire, style and close relative dating of the Greek finds from Meḏad Ḥashavyahu, Yavneh-Yam, Ekron, Ashkelon, Tel Batash (Timna) and Tel Kabri strongly suggest that they are part of a related phenomenon, within a limited period of time (Fantalkin 2001: 140–41). Noting that they are all coastal or near to coast, Fantalkin argues that they reflect one phase of Egyptian military activity (effectively the fifteen years between c. 620 and 604 BC). I would not necessarily go as far as Fantalkin in suggesting that the finds from Timna, Ekron and Ashkelon are mainly due to the presence of Greek soldiers, rather than trade.<sup>38</sup> Trade remains a viable explanation of the smaller amounts of

37 The Greek pottery includes sherds of Milesian Middle Wild Goat II (Waldbaum and Magness 1997: 30).

38 Fantalkin later (2006: 207, n. 93) moderated his position slightly, not wishing to “reject completely the possibility of certain East Greek trade with the coast of Palestine, especially with places like Ashkelon. On the other hand, we should consider the possibility that whatever East Greek trade existed, if any, would have been directed mainly toward

Aegean pottery at these sites, with Ashkelon as the most likely point of entry.<sup>39</sup> Nevertheless, there remain at least three coastal or near-to-coastal sites (Mezad Hashavyahu, Yavneh-Yam and Tel Kabri) that can be seen in terms of a phase of Egyptian military activity within a fairly narrow time range, conventionally dated to the last decade or two of the 7th century BC.<sup>40</sup>

Yet what is the most likely historical context for such a (brief) spurt of Egyptian activity along the Eastern Mediterranean coast, including control of the port of Yavneh-Yam? It has long been realised that during the later 26th Dynasty, beginning with the reign of Necho II (610–595 BC), there was a distinct change in Egypt's northern imperial strategy, to one which de Meulenaere (1951: 60–61) dubbed a *zeepolitiek*. The shift towards a sea-based policy was a natural one as the Saites had become increasingly reliant on Greek mercenaries, who would have had naval as well as military expertise (James 1991: 721; according to Herodotus 2.163.1, Apries had a bodyguard of no less than 30,000 Ionian and Carians). Apries' Phoenician expedition and the alliances formed by the hellenophile Amasis with Cyrene, the thalassocrat Polycrates of Samos and Croesus of Lydia, master of Ionia (Herodotus 2.181; 3.39–43; 1.77), as well as his subjugation of Cyprus (Herodotus 2.210), were all part of the new *zeepolitiek*. The merits of this strategy are clear enough. By controlling the Eastern Mediterranean, the Egyptians could block Babylonian access to trade while reaping the benefits themselves; the volume of traffic can be judged from the large quantities of Greek pottery found at Naukratis, given over by Amasis to the Greeks. Second, raids and harassment of Babylonian forces and supply lines could be conducted by the Egyptians and their mercenaries, with the safety of the sea at their back. Last but not least, the sea itself was used as a barrier to Asiatic invasion of Egypt. It is unlikely that the Babylonians had any serious marine capability until they had subdued Tyre (572/1 BC) and, even after that, the Egyptians maintained naval supremacy. The policy thus worked successfully, until Polycrates of Samos and the Egyptian admiral Udjahorresne

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the East Greek mercenaries who were stationed in the region. In this case, those East Greek mercenaries were able to receive some familiar goods (including pottery), otherwise inaccessible in the local environment." See now Fantalkin (2011), which argues more strongly for an Egyptian-controlled Greek garrison at Ashkelon.

39 Still, we may have a picture here of 'trade following the flag', even though the flag was a mercenary one. In practical terms, East Greek traders would have felt safer in waters and harbours patrolled by compatriots (cf. Fantalkin 2006; 2011).

40 We should also not forget the great Egyptian fortress of "Migdol" / T. 21 near the northern coast of the Sinai peninsula, though this certainly had a longer period of use (see note 10).

switched allegiance to Persia, allowing Cambyses to invade Egypt in 525 BC (James 1991: 725–26; Spalinger 1977: 235).

The *zeepolitiek* is agreed to have been initiated by Necho II who, according to Herodotus (2.159), attempted the construction of a ‘Suez canal’, introduced the trireme into Egypt, and built fleets on both the Mediterranean and Red Seas.<sup>41</sup> Exactly when in his reign Necho II built his northern fleet is a moot point. Spalinger sees the development of the Mediterranean fleet (and *zeepolitiek*) as a reflex to the collapse of Egyptian land-power in Asia c. 600 BC (1977: 236; cf. James 1991: 717, 721, 722). Lloyd argues that Necho began developing naval resources earlier in his reign (1988: 160; cf. James 1991: 722). Nevertheless the available evidence suggests that before about 600 BC the Saites very much concentrated on land forces and routes. Psammetichus I fought at Ashdod, Gaza, Beth Shean and deep into Mesopotamia; Necho II fought at Harran, Megiddo, Carchemish and Migdol (on the Egyptian border) and had a base at Riblah south of Hamath. The interest of these pharaohs in the *via maris* running through Philistia seems to have been that it led to the Jezreel Plain, the Beqa Valley and beyond. While such ventures may have benefited from logistical backup by sea, there is no evidence that during their first imperial phase (as defined above) the Saites already possessed a strong Mediterranean navy. A case in point concerns the route taken by Necho II (2 Kings 23:29–30) on his way to the Euphrates in 609 BC. It took him via Megiddo, where he encountered Josiah, prompting Na’aman to ask: “Why did Necho II not adopt the tactics of the Egyptian kings at the time of the New Kingdom, who often sailed as far as the Lebanese coast and launched campaigns from there . . . ?”<sup>42</sup>

41 The growing importance of the navy in Egypt under the Saites is reflected in the numbers of known officials bearing naval titles—one from the reign of Psammetichus I, two under Psammetichus II and four under Amasis (Spalinger 1977: 235–36; James 1991: 724). As Spalinger (1977: 236) notes, the Herodotean account of Necho II as founder of the navy is strongly supported by the increase in frequency of naval titles among officials. Further, while the “overseer of the king’s ships” known under Psammetichus I appears to “have controlled the internal trade of Egypt” (Spalinger 1977: 235), by the reign of Psammetichus II we know of an official with the specific title “overseer of the king’s ships in the Mediterranean”.

42 Na’aman (1991: 51) suggests that Necho probably did go by sea to the Lebanon but that, en route, he stopped briefly in Palestine in order to take personally from vassals the oaths of fealty due to him as a recently acceded Pharaoh. Further reflection renders Na’aman’s scenario rather over-complicated. Would Necho have had to make further stop-offs to take oaths from the kings of the Philistine pentapolis? Or did these, together with the kings of Transjordan, congregate with Josiah at Megiddo, whether or not the latter’s intention was to battle there with Necho? See Na’aman (1991: 52–55.) We should not forget that the goal

These considerations should have some bearing on our understanding of when the Saites might have controlled the port of Yavneh-Yam and established the support-base at Mezad Ḥashavyahu. They cannot rule out the scenario envisaged by Fantalkin in which Mezad Ḥashavyahu was established c. 620 BC, late in the reign of Psammetichus, when Southern Palestine (and its coastal waters) lay in the political vacuum created by the recession of Assyria. On the other hand, Fantalkin's study overlooked the developments that culminated in the third phase of Saite imperial ambitions. Though its seeds were planted by Necho II (perhaps late in his reign), an aggressive *zeopolitiek* (with Phoenicia and eventually Cyprus as focal points) was in place by the reign of Apries and continued by his successor. As part of this strategy, we might expect the Egyptians to have tried to secure harbourage along the Mediterranean coast.<sup>43</sup> Control of Yavneh-Yam would have been invaluable, as it provides the only natural harbour between Tel Ridan of Gaza and Jaffa.<sup>44</sup> The interest shown by the Egyptians in the port of Yavneh-Yam fits just as well, *if not better*, in the period of the developed Saite *zeopolitiek* under Apries than in the late 7th century BC.

#### *Egyptian Scarabs from Yavneh-Yam*

Two scarabs with pharaonic names are known from Late Iron Age Yavneh-Yam (Fantalkin 2001: 132–34). One, bearing the name *men-ka-Ra*, is not helpful chronologically.<sup>45</sup> The other, a blue frit scarab with the name *uah-ib-Ra*

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of Necho's ambitious expedition, while it naturally helped to reinforce Egyptian authority in Palestine and Syria, was to provide emergency support for the rapidly crumbling Assyrian authority in the upper Euphrates against the Chaldeans and Medes. As Necho passed Megiddo, then a "forced march" for his large army through Palestine may simply have been the only route available, the reason being that the luxury of a sea journey to Lebanon, enjoyed by New Kingdom pharaohs, was not yet available to the Saites.

43 Josephus (*The Jewish Wars* I, 408) stressed the dangers, from storms, of sailing from Phoenicia to Egypt without suitable harbours.

44 See map at <http://www.rgzm.de/Navis2/Home/HarbourFullTextOutput.cfm?HarbourNR=Israel-Harbors>.

45 While scarabs of this type, and bearing this name, are fairly common from 7th–6th century BC contexts (Gorton 1996: 80, 82–83, 85, Type XXVA, no. 14), the pharaoh that it commemorates is uncertain. Gorton suggests a prince of the 25th Dynasty, but such a figure seems unattested. Lalkin suggests an anachronistic use of the name of the 4th Dynasty Menkaure (pers. comm. cited in Fantalkin 2001: 133). The writing of the name (as Menkare) would also have to be defective—not impossible as scarabs of this type are egyptianising products likely manufactured in a Phoenician workshop for the Punic and Greek markets (see Gorton 1996: 80). There is only an outside chance that the scarab commemorates Menkare, an utterly obscure ruler of the 8th Dynasty, known from the Abydos Kinglist (Gardiner 1961: 437).



(Wahibre), is of possibly greater significance. The name is characteristic of the 26th Dynasty, and was the prenomen of Psammetichus I (664–610 BC) and nomen of Apries (589–570 BC). Either reading supports the understanding that Yavneh-Yam and nearby Mezad Hashavyahu were under Egyptian control at some point in the 26th Dynasty. Fantalkin elected for Psammetichus I: “It should be noted that the Horus-name *uah-ib-Ra* may belong to Apries as well . . . , however, based on the context in which this scarab was unearthed (Yavneh-Yam Stratum IX) and the general historical picture in the region during the reign of Apries (above), this possibility seems highly unlikely” (Fantalkin 2001: 132, n. 61).<sup>46</sup>

Yet Apries may be a better candidate for the scarab, from stylistic dating. There are no stated grounds for Fantalkin’s view that this type “most probably appeared before the establishment of the Naukratis factory”. Rather it belongs to Gorton’s type XXXB, which she tentatively attributes to the factory at Naukratis, which “very likely . . . began mass-production only at the start of the 6th century” (Gorton 1996: 111, 178). In agreement with Gorton’s dating, I have argued in some detail that the factory at Naukratis flourished during the reigns of Psammetichus II and Apries,<sup>47</sup> and that it was a Phoenician concern closed when the Greeks were given the town early in the reign of Amasis (James 2003). If Gorton is right that this scarab type belongs to the Naukratis factory, then Apries becomes a much stronger candidate than Psammetichus I for the Yavneh-Yam find.<sup>48</sup> Further scrutiny of the Wahibre scarab from Yavneh-Yam by experts would be welcome. Unfortunately, however, it appears that its attribution to Yavneh-Yam IX (allegedly 7th century BC) is far from clear (i.e., it came from a deposit outside of the buildings).<sup>49</sup> Yet, while no chronological conclusions can be drawn from it, the presence of a possible scarab of Apries at Yavneh-Yam nonetheless fits very well the historical scenario sketched out here, that Apries used this harbour town as a stepping stone for his Phoenician adventures, fortifying nearby Mezad Hashavyahu to provide logistical support.

46 Strictly speaking, the name *uah-ib-Ra*, as it is enclosed in a cartouche, is not a “Horus-name”, but a royal prenomen or nomen.

47 The only pharaohs reasonably attested on the scarabs from Naukratis; see Gorton (1996: 178); von Bissing (1951: 65–66); Boardman (1999: 121).

48 Even Petrie (1886: 5), who unrealistically preferred a date for the factory’s foundation in the reign of Psammetichus I, accepted that the Wahibre scarabs from Naukratis belong “probably to the latter [Apries]”; and again, “it is very likely that some of the scarabs with that name belong to the later king, especially those made at Naukratis” (Petrie 1917: 32).

49 Fantalkin, personal communication.

### Concluding Remarks

The reign of Apries, who was actively involved in warfare in Phoenicia, provides a plausible historical background for the establishment of a short-lived Egyptian fortress at Meḏad Ḥashavyahu (and possibly also that at Tel Kabri). This, and a measure of control over the southern coastal plain, may have been achievable while the Babylonian army was occupied during Nebuchadrezzar's long siege of Tyre (584/3–572/1 BC). After the conclusion of the siege, Nebuchadrezzar marched south to attack Egypt. There was at least one expedition (567 BC), and possibly an earlier attack (c. 571 BC). Either occasion could have led to the abandonment of Meḏad Ḥashavyahu by the Egyptian garrison of Greek mercenaries there. The troops would have marched southwards to fall back on more defensible positions, either in Philistia or in Egypt, or simply taken ship from Yavneh-Yam. It could well have been during the same war that Ekron IB and pre-Persian Ashkelon were destroyed by the Babylonians.

A range of archaeological and historical arguments have been offered for dating the fall of Meḏad Ḥashavyahu to c. 570/565 BC. Due to the paucity of historical records for this period, there is no cast-iron proof, but nor is there proof—as has been shown—for the generally accepted dates of 609 and 604 BC suggested by Naveh, Fantalkin and others. To weigh the evidence in the balance, we have on one side: (1) the growing case for a lowering of the EC horizon; (2) the fixed chronological points for Archaic pottery suggested by Herodotus (for Naukratis, Old Smyrna and Cyrenaica); (3) the consideration that the *zee-politiek* of Apries would have benefitted from the control of a Palestinian harbour (Yavneh-Yam); (4) the likelihood that the Yavneh-Yam scarab belongs to Apries; and (5) the near certainty that there were further military struggles between Egypt and Babylonia in Southern Palestine in the years 571–567 BC. Against this, we have historical and archaeological arguments (Fantalkin) linking the end of Meḏad Ḥashavyahu to a Babylonian campaign in 604 BC. These both rely on the restoration of the name “Ashkelon” in the entry for the first year of Nebuchadrezzar (604 BC) in the Babylonian Chronicle and the unproven assumption that a Babylonian destruction in this year can only be identified archaeologically in the last pre-Persian stratum (Phase 7).

In short, there is no compelling evidence that dates Meḏad Ḥashavyahu to the late 7th rather than the early 6th century BC, and much to recommend the latter. Hence it cannot be used for a “fixed point” for dating the Wild Goat Middle II and contemporary Greek styles to before c. 600 BC.

If the site dates, as argued, to the early 6th century, it will provide new evidence for Late Saite military operations in the Levant in an otherwise poorly

documented period. Some archaeological flesh would be put on the historical bones of the campaigns of Apries, hitherto known only from biblical and classical writings. While his aggressive strategies in Palestine, Phoenicia and Cyrene ultimately failed, that does not gainsay the fact that he successfully interrupted and impeded Babylonian domination of the Levant and Southeastern Mediterranean for some twenty years.

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