

DATING LATE IRON AGE EKRON (TEL MIQNE)¹

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A fresh approach is offered to the tangled arguments that surround the dating of late Iron Age Ekron (Tel Miqne). The only firm historical peg for dating the late Iron strata is provided by the temple inscription of Ikausu, an Ekronite ruler mentioned in Assyrian records in the second quarter of the seventh century BCE. From this evidence, somewhat lower dates than those of the excavators are argued for Strata IC to IA. The lower chronology suggested here should resolve the differences between various scholars regarding the character and historical associations of Ekron's final Iron Age strata. Ramifications for the related debate over the dating of Greek Archaic pottery are considered.

The dating of Stratum IC at Ekron, type-site for the southern coast of Palestine during the late Iron Age, has become the subject of increasing controversy. In 2003, Na'aman published an article arguing that the founding of Ekron (Tel Miqne) Stratum IC should be raised from the excavators' estimate of *c.* 700 BCE to a point within the second half of the eighth century. At much the same time Gitin (2003, 56*) responded to some criticisms made by Stager, including the suggestion that the temple complex of Ekron IC — and by implication the start of the Stratum — should be dated 'close to the mid-seventh century'. Shortly before the appearance of these articles, the present writer had submitted for publication a note arguing that the date for the founding of IC should be lowered to the second quarter of the seventh century BCE (James 2005a). In the same journal, a paper by Ussishkin (2005, 61) accepted Na'aman's case as showing that 'the Stratum IC city was founded as early as the middle of the eighth century'. Thus, opinions on the founding date of Ekron IC now differ by nearly a century.

The main threads of the controversy will be analysed here, taking on board the new arguments offered by Na'aman, Gitin, Stager and Ussishkin.

NA'AMAN'S PROPOSED REDATING

First Na'aman's case for raising the date of Ekron IC needs to be assessed. His main point is that 'the reference to Ekron in Sennacherib's inscriptions of his 701 BCE campaign indicates that the city was an important centre at that time.' (Na'aman 2003, 84) He also argues that Neo-Assyrian administrative documents indicate that Ekron paid 'relatively heavy tribute . . . to Assyria in the reign of Sargon II.'² In Na'aman's view Ekron's importance during the late eighth century BCE is better reflected in the greatly expanded settlement of Stratum IC rather than that of the preceding Stratum IIA, when the settlement area was largely confined to the upper city (Gitin 1998a, 167 & n. 7).

As further evidence of Ekron's stature towards the end of the eighth century BCE, Na'aman cites the 'Azekah Inscription' in which a Neo-Assyrian king describes the siege of 'a royal [city] of the Philistines which H[eze]kiah had captured and strengthened for himself' (trans. Na'aman 1974, 27). The episode, according to Na'aman, belongs to Sennacherib's Judahite campaign of 701 BCE. In his original publication of the Inscription, Na'aman considered that this 'royal city' was Gath but, following a suggestion of Mittmann (1990, 98–99), he now prefers Ekron (Na'aman 1994, 245–46). Thus, in his opinion, it would have been Ekron that was described in the Inscription as 'surrounded with great [to]wers and exceedingly difficult [its ascent]', with a 'palace like a mountain', a 'moat was dug around it' and possibly a water tunnel. Using this, Na'aman (2003, 85) broadened his case:

Ekron's centrality in the annalistic account of Sennacherib's campaign, and its depiction in the Azekah Inscription, even taking into account the latter's literary character, indicate that it was then a large fortified city. Evidently, Ekron had begun expanding already in the second half of the eighth century BCE and was soon fortified, apparently by one of Padi's predecessors. Whether the fortification system of the late eighth century included only the upper tell, or the upper and lower city, is not my concern and should be established in light of the excavations of the lower city.

The city's growth in the second half of the eighth century fits its explicit mention — alongside Gaza, Ashkelon, and Ashdod — as a royal Philistine city both in Amos 1:6–8 and in Sennacherib's inscription. We may conclude that Stratum IC at Ekron was founded in the second half of the eighth century BCE and that the documentary evidence contradicts the suggestion that the city began expanding only in the early seventh century BCE.

Are any of the strands in this argument compelling?

1. The importance of a city as perceived from historical sources can often be a poor guide to what we should expect to find in the archaeology. Thucydides (1.10.2) was the first to remark on this, asking how would future generations judge the Sparta of his own time if all they had were the remains of its buildings, 'with no temples or monuments of great magnificence, but simply a collection of villages'? Though the geographical and cultural context here is different, Thucydides' acute observation should not be overlooked.
2. The name of the 'royal city of the Philistines' in the Azekah Inscription is missing, and while the identification with Ekron is possible, basing an argument on a textual restoration can easily be wrongfooted. There are also considerable problems with the identification.³
3. Even if we assume that that the city in the Azekah Inscription was Ekron, this tells us little about the nature of the city in the late eighth century. Na'aman himself cautions regarding the 'literary character' of the Azekah Inscription. For example, it describes the fortress of Hezekiah at Azekah itself as follows:

... [like the nest of the eagle?] located on a mountain ridge, like pointed iron (?) daggers without number reaching high to heaven [... its walls] were strong and rivaled the highest mountains, to the (mere) sight, as if from the sky [appears its head?]. . . (Trans. Na'aman 1974, 27.)

That these are hyperbolic terms, used to magnify Sennacherib's achievement in taking Azekah, is self-evident. But even without recourse to the argument that we need not treat all the details of the Inscription literally — which Na'aman uses to explain some major differences with Ekron (see n. 3) — is there any reason why its description of a city could apply only to Ekron IC, and not to the preceding Ekron II?

The remains of the latter are not as poor as Na'aman's argument might imply. To cite the excavators (Gitin 1998a, 167):

In Stratum III, the last three quarters of the 10th through the 9th century, part of the acropolis was rebuilt, founded on a series of newly constructed monumental stone terraces and platforms. On top of the acropolis, the central architectural feature of Stratum IV — a street with flanking walls — was repaved in Stratum III and continued in use with modifications through Stratum II until the end of the 8th century.

Additionally, in Stratum IIA 'a citadel tower built of boulder-sized rocks and a stone-lined drain were constructed' (Dothan and Gitin 1993, 1056). Surely a city with such constructions, including monumental stone terracing, could have boasted a structure to inspire the 'palace like a mountain' of the Azekah Inscription. The more cryptic words 'its waters were in darkness and [its?] overflow . . . ' are taken by Na'aman as 'probably' indicating a water tunnel. At the very least the stone-lined drain of IIA (cf. 'overflow') shows considerable attention to water-management, but there is no need to introduce such a comparison as Na'aman has offered none for Stratum IC.

As to the fortifications, the excavators state (Dothan and Gitin 1993, 1056) that ‘... perhaps as early as the tenth century BCE, a new mud-brick city wall was built at the bottom of the slope of the acropolis. Attached to the wall was a 7-m-wide mud-brick tower faced with large ashlar blocks in a header-and-stretcher construction.’ Thus, the excavators have been fortunate enough to identify the remains of two substantial towers built in the centuries immediately preceding Ekron IC. If it is Ekron that is described in the Azekah Inscription, why should it not be these structures that inspired the description of a city ‘surrounded with great towers’? Further, the acropolis fortifications (mud-brick city wall and ashlar-faced tower) built in Stratum III ‘continued in use through the end of Stratum IB’ (Gitin 1998a, 167). Thus the acropolis fortifications of the IIB/IIA and IC/IB periods were essentially the same. There remains the question of the fortifications around the lower city known from Stratum IB. There are signs that those at the southeast corner of the lower city had already begun during Stratum IC (Gitin 1995, 59, Pl. 4). But even if it were to prove that the outer wall was built in the time of Stratum IC (as now argued by Ussishkin — see 3a below), there is no reason from the Azekah Inscription to backdate it to the eighth century BCE. The Inscription describes a city with fortifications but does not tell us whether these extended to a lower city. Na’aman himself dismisses the question of the extent of the IC fortifications as ‘not my concern’.

3a. Ussishkin (2005, 61–62, Fig. 18) offers a further argument in support of Na’aman’s redating, from the Assyrian relief depicting Sargon II’s attack on Ekron (*Amqarruna*), in 720 BCE. According to Ussishkin, ‘The relief depicts Ekron in a conventional schematic manner, located on raised ground and protected by two city walls. The relief indicates that Ekron was a heavily fortified, important city at that time.’ As he (somewhat radically) reattributes the circuit wall of Str.VII (early 12th century BCE) to Ekron IC, Ussishkin sees the presence of two walls on the relief as confirmation of Na’aman’s position ‘that Ekron of Sargon II’s period covered the entire mound’, and argues that the expanded city of IC should accordingly be backdated.

However, the nature and location of the ‘walls’ on the Sargon relief is far from clear. Jacoby (1991) has cautioned against the tendency to read such reliefs as quasi-photographic representations.⁴ Nevertheless, when Assyrian artists intended to depict a large city (like Lachish), with extensive defence works such as a circuit wall encompassing the whole city (tell) as well as a fortified acropolis, they were clearly capable of doing so (for examples see Yadin 1963, 420–421, 423; Jacoby 1991, 129, ill. 11). While one could imagine propagandistic/other reasons for such differences, comparison of Sennacherib’s famous depiction of the large, complex, fortifications of Lachish with the smaller, much simpler fortifications on Sargon’s Ekron relief hardly leads to the suggestion that the latter encompassed a larger settlement, to the full extent of the tell. (Tel Lachish covers 31 acres, Tel Miqne 40 acres.) Further, *if* one were to take the details literally, Sargon’s relief depicts both ‘walls’ as resting on a (conventionally depicted) hill. Yet Tel Miqne is conspicuously flat, the only substantial high point being the 10 acre ‘upper tell’ of the northeast acropolis (Dothan and Gitin, 1993, 1051). In that case, the lower wall on the relief, represented by a simple line of crenellations, would be the mudbrick retaining wall which surrounded the acropolis (already in Ekron III/II). What Ussishkin sees as an upper ‘wall’ could then represent fortifications on the summit of the acropolis. Read this way, the Ekron of Sargon’s relief, *contra* Ussishkin, would have been a small settlement restricted to the upper tell.

4. Regarding Amos 1:6–8, cited by Na’aman, the majority of modern commentators regard this section of the oracles against the nations as authentic, while the prophet is usually dated before 750 BCE. Presumably Na’aman has a slightly later date in mind in order for it

to be relevant to the Ekron of the second half of the eighth century BCE.⁵ Yet Amos does not describe Ekron, as Na'aman states, as a 'royal Philistine city', though it clearly counted as one of the Pentapolis. Palaces and a sceptre are mentioned in the allusions to Gaza and Ashkelon respectively; all that is said of Ekron is 'I will turn my hand against Ekron'. How this can be construed as evidence regarding the size or status of Ekron, or be used to back date Stratum IC, is hard to see.

THE TEMPLE INSCRIPTION

There is therefore nothing of substance in Na'aman's case for raising the beginning of Stratum IC from the excavators' estimate of *c.* 700 BCE. Above all, while he makes many observations on the temple inscription from Ekron,⁶ Na'aman tends to gloss over its chronological importance.

Though found in the debris from the destruction of Stratum IB, the inscription evidently relates to the building of the sanctuary in the time of Stratum IC. The builder is named as Ikausu, son of Padi. From Assyrian records we know the latter was restored to the throne in 701 BCE and was still reigning in 699 BCE; his son Ikausu is mentioned twice, in 673 and 667 BCE.⁷ Gitin *et al.* (1997, 16; Gitin and Cogan 1999, 199) have suggested, reasonably, that the reign of Ikausu probably began *c.* 675 BCE. Yet they deduced from this that the construction of the temple was 'no later than the first quarter of the seventh century' (Gitin *et al.* 1997, 16; repeated in Gitin *et al.* 1998, 31). As pointed out by the present writer (James 2005a), this is a *non sequitur*, as the Assyrian references to Ikausu are from the *second* quarter of the seventh century. This period, when its builder is historically attested, provides the most cautious range for the temple's construction.⁸ One assumes a plausible 25-year reign for Ikausu, but here Stager has noted (personal communication, 2003) that we cannot rule out a longer reign (compare the 55-year reign of his contemporary Manasseh of Judah). If the temple was Ikausu's swansong, we could be looking at a construction date as late as the *third* quarter of the seventh century BCE (cf. Master 2001, 213–214). At any rate, it would be rash to date the temple much earlier than the first Assyrian mention of Ikausu, in 673 BCE.

Since the inscription is the only object from Tel Mique that can be indisputably linked to firmly dated Assyrian history, it is surely the best guide to the dating of Stratum IC. The large Temple Complex 650 is one of the most important defining structures for this Stratum and as it is taken to have been built early in IC it should help us to date the beginning of this Stratum (James 2005a). This understanding is strongly reinforced by the excavator's most recent remarks (Gitin 2003, 56*):

The Ekron royal dedicatory inscription from the cella of the pillared sanctuary of Temple Complex 650 securely dates the founding of Iron Age II Ekron's most important structure excavated thus far This provides the historical context and *the basis for dating the construction, certainly of the sanctuary, and most probably the entire temple complex as well.* As this structure is an integral part of Ekron's well-defined city plan, it would be unreasonable to conclude, as Stager has, that the sanctuary/Temple Complex 650 could be dated to late in the reign of Ikausu, that is during the rule of Ashurbanipal, close to the mid-seventh century (Stager, oral communication). *Such a date would, by implication, necessitate reassigning the city plan of Stratum IC or, at the very least, the elite zone of which Temple Complex 650 is the focal point, to a similar late date near the end of the Assyrian period.* [emphases added]⁹

Quite so. As argued (James 2005a), the building of the temple complex and hence Stratum IC as a whole should indeed be lowered in date, perhaps close to the mid-seventh century. Gitin's objection that this would mean 'creating a significant gap in the documented history of the city' is immaterial. If the start of Stratum IC were lowered to *c.* 675–650 BCE, then a gap is created only if we cling to the dates for Stratum II guesstimated *before* the inscription was found. The transition between Strata II and I was set by Gitin at *c.* 700 BCE,

based on the idea that Padi was the ‘first king of Stratum I’ (Gitin 1989, 49) but, of course, this is merely an assumption (James 2005a, 92). As there is no independent dating for Stratum II, its date could be lowered in step with that for IC. Stratum IIA is presently dated *c.* 750–701 BCE. On the lowered chronology argued here, it could date to *c.* 701–675/650 BCE. The new fortification of Stratum IIA (citadel tower and stone-lined drain) might then reflect the work of Hezekiah or Padi, either before or after his restoration in 701 BCE.

In any case, Na’aman’s suggestion that Stratum IC began in the late eighth century BCE would be ruled out. By comparison with the firm dates offered by the inscription, such suggestions are merely historical guesswork.

THE OIL INDUSTRY AT EKRON

The debate between Gitin and Stager, a healthy rivalry between the excavators of two major Philistine sites (Ekron and Ashkelon), has embroiled broader historical questions — notably the pattern of olive oil production and trade in seventh-century Philistia. An extraordinary number of olive presses were constructed during IC and Gitin accordingly places the ‘boom’ in oil production at Ekron to *c.* 701 to 630/623 BCE, following his dates for the Stratum. Moreover, he sees ‘Assyrian imperial commercial policy’ as the ‘motivating force behind the establishment of the largest olive oil industrial centre in antiquity found to date’ (Gitin 2003, 55*). Stager (1996, 70*–71*) sees a very different scenario, as he would date the boom at Ekron to the late seventh century BCE:

What propelled the olive oil industry at Ekron into the international sphere was not a dying Assyria but a rising Egypt, ever the great consumer of Levantine olive oil . . . the expansion of Ekron and the development of its oil industry occurred after Assyrian interest and power in the West had begun to wane in the 640s.

Indeed, for a city which Gitin feels was deliberately developed by the Assyrians, there is a striking paucity of Neo-Assyrian influence on the material culture. The jewellery hoards from IC-IB are conspicuously Phoenician and Egyptianising in style, while ‘Neo-Assyrian traits and motifs are evident in only two of the silver jewelry items found in the hoards’ (Golani and Sass 1998, 74). There are no cuneiform inscriptions and only a few allegedly ‘Assyrian’ vessels.¹⁰ In lieu of such diagnostic finds the excavators have sought other signs of Assyrian influence. One case, concerning the alleged ‘Assyrian’ style of the architecture of the pillared sanctuary, is not compelling, while Phoenician influence is more conspicuous (Master 2001, 211–212; James 2004, 48–49). Another concerns a short dedicatory inscription from a side-room of the sanctuary that is highly ambiguous (see n. 9).

Perhaps significantly, ovoid storage jars similar to those used at Ekron to transport olive oil have been found in Buto in the western Delta of Egypt (Gitin 2003, 57*). These considerations tend to support Stager’s dating of the oil boom of Ekron IC to the period when it was controlled by the Saite pharaohs (last third of the seventh century BCE). However, as Na’aman (2003, 86) notes, Stager stated his case shortly before the discovery of the Ikausu temple inscription. For Gitin (2003, 56*) the inscription delivers a crushing blow to Stager’s argument, as he assumes that it supports a dating for the start of Stratum IC around 700 BCE. As we have seen, it does not. Nevertheless, the fact that Ikausu, builder of the Stratum IC temple, was an Assyrian vassal (mentioned in 673 and 667 BCE) means that we must allow that some, at least, of the oil installations at Ekron may have been flourishing early in IC and hence in the second quarter of the seventh century BCE. This would help to explain the burgeoning wealth of IC, evidenced by the construction of the temple and the elite zone and the expansion of the city generally during this phase in its history.

From the inclusion of Ekron in the list of Levantine city-states which provided troops, ships and supplies for Assurbanipal’s Egyptian campaign (667–666 BCE) it is tempting to

consider — in agreement with Gitin — that the Assyrians may have patronised the Ekronite oil industry at this stage, in order to supply the vast army needed to control Egypt. Nevertheless, if we place the start of Ekron IC *c.* 675/650 BCE, then Stager may *also* be right that the boom in Ekron's oil industry coincided with the rise of the Egyptian Empire under the Saïtes (cf. Fantalkin 2004, 257). The transition from Assyrian to Egyptian power in the Levant is a grey area with few fixed points. There was an alliance between Assyria and Saïes as early as 666 BCE, when Psammetichus' father Necho I was reinstalled as king. The relationship was soured during the 650s, when Assyrian records tell us that Psammetichus withheld his tribute. 'However, he may have mollified the Assyrians by remaining their ally (not an opponent) while they struggled with a rising tide of troubles in the east and south-east.' (Kitchen 1986, 406) When this friendship turned into a formal alliance, with the Assyrians allowing Egypt control of the southern Levant, is hard to tell. By 616 BCE, Egypt was actively supporting Assyria (against the Babylonian–Median coalition) by military engagement deep into the Euphrates valley and there are indications that 'an alliance had been formalized prior to 616 BCE' (Spalinger 1978, 50). Na'aman (1991, 39) envisages a peaceful handover of authority in Palestine, from Assyria to Egypt, in the late 620s. Some have considered an earlier date, arguing that a likely date for Psammetichus' siege of Ashdod, reported by Herodotus, is 635 BCE (see Spalinger 1978, 50; cf. however Na'aman 1991, 39).

Given this, it is difficult to draw a distinct line between the Neo-Assyrian and Saïte Empire 'periods' in Palestine. The boom in the oil industry at Ekron could have *begun* under the Assyrian vassal Ikausu (second quarter of the seventh century) but *continued* into the period of Egyptian domination (last quarter of the seventh century). There is no reason, from the political history, to see any interruption to the oil industry at Ekron during the changeover in hegemony from Assyria to Egypt.¹¹

Nevertheless, there is a general problem with Stager's position that the oil boom of Stratum IC postdates *c.* 640 BCE. It would mean putting a considerable 'squeeze' on the stratigraphy, either reducing the floruit of Stratum IB (dated by Gitin *et al.* to 630/623–604 BCE) to a decade or so, or blurring the distinction between the two strata. Gitin (2003, 57*–58*) has firmly responded that there are many architectural changes between Strata IC and IB, as well as discernable differences in the pottery, which must be allowed time. On the same note, Na'aman (2003, 88) suggests that as 'typological differences usually take some decades ... the end of Stratum IC is probably earlier than suggested by Gitin.'

Na'aman is justified in questioning the date (*c.* 630/623 BCE) offered by the excavators for the IC/IB transition, as it is not based on any certain grounds. From the number of oil installations abandoned during IB Gitin detected a decline in production during that phase. The date for the IC/IB transition was then guesstimated (Gitin 1989, 45) on the basis that this diminution in oil production would have coincided with the recession of Assyrian power, but as we have seen there is no good reason to see why this would have had major economic repercussions at Ekron. On Gitin's dating, Stratum IB (630/623–604 BCE) would have been contemporary with the rise of the Saïte Empire in Palestine. Yet even with the disappearance of the Assyrian market, it is hard to understand why Ekron's oil industry should have gone into decline under Egyptian control. As Stager (1996, 64*) stressed: 'Throughout antiquity the biggest consumer of Levantine olive oil was Egypt (where the olive tree does not grow).' One might add that with the growth of the Saïte Empire the Egyptian demand for oil would have surely grown, if only to supply the needs of the huge number of Aegean mercenaries on whom the pharaohs increasingly relied.¹²

While Na'aman's observation about the date of the IC/IB transition was clearly intended to suit his overall backdating of Stratum IC, it could lead us in a quite different direction, one which could resolve the differences between Stager and Gitin. As we have seen, there is absolutely no evidence for raising the dates of IC, and much in favour of lowering them. We should therefore consider whether the lowering of Ekron IC argued here

would have a knock-on effect on the dating of IB. In other words, should the transition from IC to IB be lowered from 630/623 nearer to 600 BCE, at the time of the Neo-Babylonian conquest rather than the Assyrian collapse? If so, Stager's suggestion that the oil boom of Stratum IC was contemporary with the Saite domination of southern Palestine during the late seventh century could be accommodated without contradicting Gitin's understanding of the stratigraphy. Further, the otherwise puzzling reduction in oil production at Ekron during IB could then be due to the loss of the Egyptian market through a Babylonian embargo during the first decades of the sixth century BCE. (The Babylonian presence in southern Palestine was particularly strong between *c.* 600 and 582 BCE.) Such a placement would obviously clash with the excavators' preferred date for the end of IB in 604 BCE (first year of Nebuchadrezzar). But one may question whether even this date, the base-line for all previous discussion, is well-supported.

THE DESTRUCTION OF EKRON IB

The excavators' preferred date for the fall of Ekron was originally 603 B.C, in which the Babylonian Chronicle records Nebuchadrezzar's siege of an unnamed city (Gitin 1989, 46). That this could have been Ekron was ruled out by Na'aman after careful analysis of the Chronicle entry. His conclusion (Na'aman 1992, 44) was that:

The destruction of Ekron by the Babylonians ... may tentatively be dated after Nebuchadrezzar's failure on the Egyptian border in his fourth year (601/600 BCE), possibly (but not necessarily) after year 595 BCE, that is, in the years when the Babylonian chronicle series, our main source for the history of Syria and Palestine in the late seventh and early sixth centuries BCE, is no longer available to us.

Nevertheless, following a suggestion by Tadmor (and allowed by Na'aman), Gitin (1998b, 276, n. 2) now prefers 604 BCE for the destruction. Ekron is not mentioned in the Chronicle entry for this year either (indeed in any year), but the belief, based entirely on a debated restoration, is widely held that Nebuchadrezzar campaigned in Philistia in 604 BCE. The Chronicle describes how Nebuchadrezzar laid siege to a city in 604 BCE, the name of which has been read by some as 'Ashkelon'. Yet there is no guarantee that this is the correct reading,¹³ and thus no clear evidence that the Babylonians campaigned in Philistia in 604 BCE. To hang the entire chronology of late Iron Age Philistia on a debated restoration seems perilous, to say the least. Further, even if Ashkelon was mentioned in this year it might be wondered why the destruction of such a major city as Ekron did not also receive mention in the Chronicle.

The post-595 BCE date suggested by Na'aman in 1992 still remains plausible, even preferable, despite an apparent change of mind on his part. He has recently introduced this argument (Na'aman 2003, 85, n. 8): 'Only Gaza and Ashdod are mentioned in Nebuchadrezzar II's list of kings and their kingdoms written in his seventh year (598 BCE) ... It is evident that Ashkelon and Ekron had been destroyed before 598 BCE.'¹⁴ Such a deduction cannot be made. The text in question reads: 'the king of Tyre, the king of Gaza, the king of Sidon, the king of Arvad, the king of Ashdod, the king of Mir[...], the king of [...] (Oppenheim 1969, 308), and is broken off at this point with an unknown number of names missing.¹⁵ There are thus no grounds here for ruling out a post-595 BCE date for the fall of Ekron.

Gitin (1989, 46) has used two passages from biblical prophecy to bolster the case for the late seventh-century date for the destruction of Ekron:

1. 'to make them a desolation and a waste, a hissing and a curse, ... in all the foreign folk among them; all the kings of the land of Uz and all the kings of the land of the Philistines — Ashkelon, Gaza, Ekron, and the remnant of Ashdod.' (Jer. 25:18, 20)

2. 'for Gaza shall be deserted and Ashkelon shall become a desolation; Ashdod's people shall be driven out at noon, and Ekron shall be uprooted.' (Zeph. 2:4)

All a prophecy can demonstrate is the existence of a given city at the time the prophecy was uttered. Further, these prophecies are part of very generalised claims and cannot be used to pinpoint the date of particular destructions (even predicted). For example Jeremiah, in the same passages, warns of the destruction of Jerusalem, Judah and Egypt (just before Uz) and (after Ashdod), Edom, Moab, Ammon, Tyre, Sidon, the 'isles beyond the sea', and so on until we reach the 'kings of the Medes' and Babylon ('Sheshach') itself. Needless to say not all these kingdoms fell in 604 BCE or even close to that date. (NB, Jer. 25 is often thought to be Deuteronomistic — for discussion see Soggin 1989, 342–44; Lundbom 2004, 256.)

More germane is the argument introduced by Gitin from Jeremiah 27, which describes how king Zedekiah of Judah received embassies from an anti-Babylonian coalition *c.* 594 BCE. Gitin assumes that the absence of the Philistines from this coalition means that they 'may no longer have posed a threat to Babylonia', hence 'Ekron's destruction did not fall after 595 BCE' (Gitin 1998b, 276, n. 2; cf. Niemeier 2001, 24). Yet others (Miller and Hayes 1986, 409) read this quite differently, i.e. that Philistia was simply 'under strong Babylonian control at this time'. Similarly, Haak (1998, 45), in an accompanying paper to Gitin's, wrote:

The absence of Philistine representatives among the delegates plotting against Babylon (Jeremiah 27) may indicate that the Philistines remained loyal to the Babylonians in this period. This may also be indicated by Ezekiel's condemnation of the faithless Jerusalem, which is being given up 'to the will of your enemies, the daughters of the Philistines' in 16.27.

Haak associates with this a further passage from Ezekiel (25:15–17), which blames the Philistines for taking 'revenge' on Judah at the time the Babylonians destroyed the kingdom.¹⁶ In that case the Philistines would not have been a spent force by 587/6 BCE, as is so often assumed.

In short there is no historical evidence (either cuneiform or biblical) that conclusively dates the destruction of Ekron IB. That it was sacked by the Babylonians is a reasonable assumption, but during which campaign? Unfortunately we have no Chronicle entries for the last 33 years of Nebuchadrezzar's reign. Nevertheless, a point appreciated long ago by Burn (1935, 145), is that there are likely to have been further wars in Philistia during these undocumented years. From a combination of cuneiform fragments with Egyptian, biblical and classical texts it is known that Nebuchadrezzar campaigned against Egypt between 571 and 567 BCE (see Spalinger 1977, 236–244; Edel 1978; Leahy 1988, 190–191; Wiseman 1991, 39–41). This would have naturally have involved manoeuvres in southern Palestine. I hope to discuss in more detail elsewhere a possible placement for the destruction of Ekron IB during the course of this (often neglected) period of warfare.

It should also be noted that Na'aman's original suggestion of a post-595 BCE date finds support in a continuing trend towards a lower dating for Archaic Greek (seventh–sixth centuries BCE) pottery. The destruction layer of Ekron IB incorporated fragments of East Greek ware of the Early Corinthian horizon (Waldbaum and Magness 1997, 34–35), traditionally dated *c.* 625–600 BCE. This is not the place to rehearse the controversy regarding the suggestions of Francis and Vickers, but generally speaking it has been realised that their proposed revision for the Archaic at this point (up to 60–80 years) was too extreme. (For references and discussion see conveniently Biers 1992, 82–85, 99–101; James 2003, 241 & nn. 31–32, 260–262). Nevertheless, this does not mean that all is well with Archaic Greek pottery chronology. It has been pointed out (Bowden 1991 and 1996; Gill 2006), that the present dating conflicts (by some four decades) with the testimony of Herodotus regarding at least three sites (Naukratis, Old Smyrna and Tokra in Libya). From the evidence at Naukratis

(historical, Egyptian, Cypriot and Phoenician) it has been argued (James 2003) that the Archaic dates should be lowered by some 35 years at *c.* 600 BCE (from the Payne/Cook/Coldstream dates).¹⁷ The same result emerges from a comparison of the ceramic and historical chronologies for the earliest episodes of Greek colonisation in Libya (Cyrenaica), while a review of ‘fixed points’ for Archaic dating shows that the balance of evidence now strongly favours a reduction of late seventh to early sixth century Greek ceramic chronology by three to four decades (James 2005b). A chronology based closely on Greek (mainly Herodotean) historical evidence would result in notional dates for Early Corinthian *c.* 590–565 BCE.

The proposed revision in Archaic Greek pottery dating would tip Ekron IB, and other coastal sites where pottery of the Early Corinthian horizon occurs, into the early sixth century BCE (James 2004, 54–55), in step with the suggestion here that this Stratum was destroyed in the Egypto-Babylonian wars of *c.* 570 BCE.¹⁸ Lowering Ekron IB into the early sixth century BCE would naturally also be in step with the redating for Ekron IC argued from the temple inscription. While accommodating the historical and archaeological arguments (offered by Stager, Master and the present writer) for a lowering of Stratum IC, it would also preserve the distance between the assemblages of these two phases, as required by both Gitin and Na’aman.

CONCLUDING REMARKS

The best guide to the dating of the stratigraphy of late Iron Age Ekron must surely be the inscription commemorating the dedication of the temple of Ekron IC. It provides the only firm link between history and the archaeology of the site — indeed for *any* Philistine site of this period. Most scholars (with the exception of Stager and his student Master) have tended to sacrifice the chronological importance of the inscription to the expected dates for the pottery. Yet it should be the inscription that dates the stratigraphy and the pottery, not the other way around.

The dating of late Iron Ekron has recently spawned widely differing opinions among scholars — Gitin (with his co-excavators), Stager (with Master) and Na’aman (with Ussishkin). Each has placed good arguments on the table. Their better arguments are not mutually exclusive, especially once we give the temple inscription its due chronological weight. But two straitjackets need to be removed from the discussion: first that the Ikausu inscription dates the construction of the IC temple ‘no later than the first quarter’ of the seventh century BCE, when the evidence points to the second quarter; and second that the destruction of Ekron IB should be dated to 604 BCE, an idea which is based solely on a debated restoration of the name ‘Ashkelon’ (not even Ekron) in the Babylonian Chronicle.

TABLE 1. VARIOUS CHRONOLOGIES PROPOSED FOR LATE IRON AGE EKRON

TEL MIQNE	IIB	IIA	IC	IB	IA
Gitin	800–750 BCE	750–701 BCE	701–630/623 BCE	630/623–604 BCE	Early 6th century
Stager	??	??	Second half of 7th century	Second half of 7th century	??
Na’aman	??	??	Late 8th to mid/late (?) 7th century	Mid/late (?) 7th century	??
James	Late 8th century	701–675/650 BCE	675/650– <i>c.</i> 600 BCE	<i>c.</i> 600– <i>c.</i> 570 BCE	<i>c.</i> 570–??? BCE

NOTES

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² Na'aman does not explicitly tie this point into his case for backdating Ekron IC, and his statement is not well supported by the six documents he cites (2003, 84). Three (nos. 1, 5, 6) merely relate the arrival of envoys from Ekron. No. 4 (undated) records the tribute of an unknown quantity of silver, no. 3 relates Padi's tribute of one talent of silver to Sennacherib in 688 BCE, and it is only document no. 2, written 'possibly in the time of Sargon' that records 'a heavy tribute' of 9 talents and 34 minas of silver.

³ Elsewhere Na'aman (1994, 245 & n. 8) lists conspicuous differences between the unnamed city and Ekron. First, 'Ekron's location on flat land does not correspond with the topographical features of the city ... in the inscription.' Second, Sennacherib's annals give the impression that Ekron surrendered, with no mention of a violent siege. Third, there is no archaeological evidence of a destruction at Ekron which might be dated to the end of the eighth century BCE. 'The difference between the descriptions in the two sources may be the result of the literary character of the "Azekah Inscription".' (Na'aman 1994, 245).

⁴ Jacoby's appeal (1991, 131) for such Assyrian reliefs to be subjected to a more 'detailed and comprehensive study ... similar to that ... in the field of text research', remains a desideratum, to be followed by a rigorous comparison with the archaeological evidence of Iron II fortifications in Palestine.

⁵ Elsewhere Na'aman (1974, 37) relates the fate of Calneh in Amos 6.2 to an Assyrian campaign of 738 BCE. See Haak (1998, 39–40, n. 5) for references and discussion of the view that this is a later insertion, a matter which he feels is undecided.

⁶ Na'aman claims that there is something unusual about the fact that Ikausu names four of his ancestors. Given that Na'aman allows that there are instances of Mesopotamian and Anatolian royal inscriptions enumerating three, four, five and even six ancestors, it is hard to see why he thinks this is so distinctive — especially as we have no other Philistine royal inscriptions with which to compare it. Na'aman concludes that the 'only viable explanation' for the number of ancestors is that Ikausu (Akhayush) 'did it for the sake of legitimation. In other words, Padi, Akhayush's father, had founded a new dynasty in Ekron, and his three predecessors (*Isd*, Adā and Ya'ir) were not rulers of Ekron ...' Such a conclusion is completely unwarranted. For comparison, the three ancestors (Tukulti-Ninurta II, Adad-nirari II and Assur-Dan II) listed by Assurnasirpal II (Luckenbill 1926, 140) were most definitely kings of Assyria.

⁷ Unnecessary doubt regarding the second reference to Ikausu (list of Assurbanipal) was raised by Frahm

(2000), who questioned the reliability of Assurbanipal's list as it 'is completely identical with the older list of Esarhaddon and may have been mechanically reproduced from it.' As shown elsewhere (James 2005a, 91, n. 2), this is incorrect, since there are differences between the two lists. The mistake has been noted by Heather Baker (editor of the series *Prosopography of the Neo-Assyrian Empire*) for inclusion in future corrigenda (H. Baker, personal communication, 2003).

⁸ Na'aman's comment here (2003, 86) is that 'The building inscription of Akhayush ... indicates that construction of the elite zone at Ekron, including the large temple, took place under the Assyrians in the first half of the seventh century BCE'. The broad range given avoids Gitin's inaccuracy, yet there is no real reason to include the first quarter of the seventh century BCE.

⁹ Gitin adds a second argument, based on the dedicatory inscription on a fragment of storage jar from a side-room of the pillared sanctuary. Gitin and Cogan (1999, 197–198) read it as meaning 'for (the life of) Ba'al and Padi' and see this formula as a calque on the requirement 'to revere god and king' incumbent on Neo-Assyrian citizens. In the opinion of Gitin (2003, 57*), 'this reference to Padi King of Ekron at the beginning of Stratum IC, c. 700 BCE, provides further evidence of the linkage of the pillared sanctuary with the early part of the Neo-Assyrian period.' However, Na'aman (2003, 85, n. 9) casts serious doubt on Gitin's interpretation: '... blessing the life of a god is odd. Moreover, there is no parallel among the Assyrian royal inscriptions for a dedicatory inscription that links god and king ...' Regarding the dating to c. 700 BCE, this is circular reasoning. The inscription was found in the destruction debris of Ekron IB (Gitin and Cogan 1999, 193) and thus, on Gitin's model, would be a century-old heirloom. Otherwise, the inscription is inconclusive as dating evidence for a number of reasons. The reference to Padi might be to a revered ancestor, and not a reigning monarch. Further, as we have no Ekronite kinglist after Ikausu, we cannot rule out that there was a second king Padi. (Cf. the discovery of the inscribed bottle from Tell Siran showed that the Amminadab king of Ammon known from Assyrian records in 667 BCE had a papponymous grandson, and threw into question the automatic assumption that seals with the name Amminadab all belonged to the former — Thompson and Zayadine 1973, 9.)

¹⁰ The published examples (Gitin 1997, 92) are an 'Assyrian palace ware' bowl, and local imitations of 'Assyrian-type' bowls and goblets (five) and bottles (two). It has become increasingly appreciated, from studies of Mesopotamian, Syrian and Transjordanian contexts, that such 'Assyrian' vessels and imitations were manufactured mainly during and following the last days of the Assyrian Empire (seventh to early sixth centuries BCE), continuing well into the Persian period (Holladay 1976, 272, 282, nn. 59–60; Routledge 1997, 35; Lehmann 1998, 19–21; Hausleiter 1999, 18–22, 38–40; James 2004, 49; Veen, van der, unpublished, §2.1.4b). Hence their presence at Ekron would fit a date within either the late Neo-Assyrian or Neo-Babylonian Empire periods.

¹¹ As Na'aman (2003, 87; cf. Fantalkin 2004, 256) has argued, we should not see the prosperity of Ekron in

terms of 'a deliberate imperial policy of economic development of these states', though of course it would have taken advantage of the *pax Assyriaca*. The same would apply to the *pax Egyptiaca*. While storage jars similar to those from Ekron have been found in Egypt, we should also remember that there were other markets: '... a larger number of similar Ekron-type storage jars have also been identified at Carthage and at several sites in Spain.' (Gitin 2003, 57*).

¹² The same point has now been independently raised by Fantalkin (2004, 257). The reliance of the 26th-dynasty kings on Aegean mercenaries is well known and, according to Herodotus (2.163.1), by the reign of Apries (589–570 BCE) the pharaoh maintained a body-guard of 30,000 Carians and Ionians. It is fair to assume that these Aegean mercenaries required regular supplies of olive oil. In this context we should remember the law passed in the early sixth century by the canny Solon, forbidding Athenian export of any produce except olive oil (Plutarch *Solon* 24.1), which Stanton (1990, 65, n. 1) sees as a measure to stimulate the olive industry. Solon himself, in his retirement, worked as a merchant-shipper (Plutarch, *Solon* 25.5); assuming he followed his own laws, when he visited Egypt in the reign of Amasis (Hdt. 1.30.1; Plutarch, *Solon* 26.1) he must have taken a cargo of olive oil. (For Solon's dates and his Egyptian visit see Miller 1969, 62–86.) Thus, when he paid for his Egyptian voyage by selling oil (Plutarch, *Solon* 2.4), Plato would have been following in Solon's footsteps.

¹³ In favour of the reading 'Ashkelon' Stager (1996, 72*, n. 1) cites a tentative approval (pers. comm.) from Irving Finkel of the British Museum. It should be noted, however, that the two scholars (Wiseman and Grayson) who have published this text have repeatedly recommended caution. Wiseman (1956: 85) tentatively read 'i^s?-qi?-il-lu-nu', adding: 'The first two signs are doubtful, being written over an erasure.' Grayson (1975, 100): 'Nothing can be read with certainty.' Grayson (1980, 161): '... as D. J. Wiseman and I emphasized ... the reading of the name of the city captured is very uncertain.' Wiseman (1991, 23, n. 153): 'The reading remains however uncertain.'

¹⁴ For the redating of the Istanbul prism from 570 to 598/7 BCE, see Na'aman 2000, 40 and Vanderhoof 2003, 238. Note, however, that Wiseman (1991, 73) and Katzenstein (1994, 46) place the text c. 570 BCE.

¹⁵ Wiseman (1991, 75; cf. Vanderhoof 2003, 238–239) states that 'The length of columns on the prism would allow for only another six kings to be cited ...', a very different matter from the *one* extra name allowed by Na'aman's treatment (2000, 40). Further, there is a hint of circular reasoning in Na'aman's writing here: 'The last missing name in line 29 is probably Byblos, as the list enumerates kingdoms located along the eastern Mediterranean coast. Ashkelon and Ekron — the latter destroyed by Nebuchadrezzar a few years later — are missing from among the Philistine kingdoms.' Finally, the very nature of the document is uncertain. Rather than seeing the list as providing a complete record of Nebuchadrezzar's court officials and vassals, Wiseman (1991, 75) sees it as the record of 'a procession to commemorate a special occasion.'

¹⁶ Regarding Ezekiel 16.27 and 25.15–17, Katzenstein (1994, 43) wrote: 'We are almost convinced that these words of Ezekiel give a picture of the affairs in 597 (and not in 587).' He gave no arguments, however, for this supposition.

¹⁷ Note that the lowering of Early Corinthian by some 5–10 years proposed by Amyx (1988, 428) has achieved wide currency. As I have stressed, there has always been a strong undercurrent of thought in the literature pulling for lower dates for the Protocorinthian–Corinthian series. Langlotz, for example, suggested lowering Early Corinthian by 20 to 30 years, while Gjerstad argued that Middle Corinthian should be dated 25 years later. (For references to these and other low daters see Amyx 1988, 403–413; James 2003, 260–262; James 2005b, 13.)

¹⁸ Fantalkin (2001, 128) remarked with regard to the local pottery of this period: '... 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 ...'

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