The Alleged "Anchor Point" of 732 BC for the Destruction of Hazor V*

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Summary: The Alleged "Anchor Point" of 732 BC for the Destruction of Hazor V
All previous discussions of the chronology of Iron Age Hazor assume as an "anchor point" the destruction of Hazor V by Tiglath-pileser III in 732 BC. Re-examination of Yadin's case for this date shows that it was merely an assumption on his part. A review of the dating evidence – partly historical but principally the input from the independently dateable archaeological chronologies of Cyprus, Phoenicia, Mesopotamia and Egypt – suggests that Hazor V fell much later than 732 BC. Consequently both the Yadin ("high") and Finkelstein ("low") models for the chronology of Iron II Hazor are working from an incorrect baseline. A model is offered here which, while arguing a shift of the Iron IIA period from the tenth to ninth century BC, does not unduly compress Strata X-VII, closes the alleged long settlement gap at the site during the Neo-Babylonian to Early Persian period and resolves numerous dating anomalies arising from imported finds.

Keywords: Hazor – Iron Age II Chronology – Samaria –Megiddo

Resumen: El pretendido "punto de anclaje" de 732 a.C. para la destrucción de Hazor V
Todas las discusiones previas sobre la cronología de Hazor en la Edad de Hierro consideran como un "punto de anclaje" la destrucción de Hazor V por Tiglatpileser III en 732 a.C. Una reexaminación del caso de Yadin para esta datación muestra que era sólo una mera conjetura de su parte. Una revisión de la evidencia para la datación – en parte histórica pero principalmente la clave para las cronologías arqueológicas independientemente datables de Chipre, Fenicia, Mesopotamia y Egipto – sugiere que Hazor V cae mucho más tarde que 732 a.C. Consecuentemente, tanto el modelo de Yadin ("alto") como el de Finkelstein ("bajo") para la cronología del Hierro II de Hazor trabajan desde un punto de partida incorrecto. Aquí se ofrece un modelo que, mientras sostiene un cambio del periodo del Hierro IIA del siglo IX al X a.C., no comprime

* Artículo recibido: 24 de Junio, 2008; aprobado: 14 de Julio, 2008.

excesivamente los estratos X-VII, cierra el supuesto largo hiato de asentamiento en el sitio que abarca del periodo Neo-Babilónico al Persa Temprano y resuelve numerosas anomalías en la datación, producto de los hallazgos importados.

Palabras Clave: Hazor – Cronología de la Edad del Hierro II – Samaria – Megiddo

The long-standing disputes over the chronology of Iron IIA have frequently involved the site of Hazor. According to the excavators, the Iron Age IIA city, Stratum X, represented Solomonic activity of the tenth century BC.[1] Kenyon raised doubts, as the pottery of Stratum X bears great similarities to that of Samaria Pottery Periods 1 and 2, dated by her to the time of Omri and Ahab in the early ninth century BC. Accordingly, she placed the pottery of Hazor X (though not the architecture) in the ninth century BC. [2] Wightman went further, arguing that Stratum X was built in the ninth century.[3] The Kenyon/ Wightman position was criticised, and the Yadin chronology supported, by both Finkelstein and Stager.[4] More recently Finkelstein, while still eschewing the Kenyon/Wightman arguments from Samaria, has also argued that Hazor X was built in the early ninth century, a major plank of the so-called Tel Aviv "low chronology" for Iron IIA.[5] Meanwhile, the traditional Yadin chronology for Hazor has been vigorously defended, notably by the current excavator, Ben-Tor, and by A. Mazar. The present paper concentrates on Hazor's part in the Iron IIA debate, and in particular the key, and overlooked, role played by the dating of Stratum V.

Stratum	Date
X	10th century BC
IXB-A	Early 9th century
VIII	9th century
VII	9th century
VI	Early 8th century
VB	8th century
VA	8th century - to 732 BC
IV	End of 8th century
III	Early 7th century
II	4th century
I	3rd-2nd centuries BC

Table 1.
Dating of the Hazor strata, after Yadin (1972: 200).

[1] Yadin *et al*. 1958; 1960; 1961.
[2] Kenyon 1964.
[3] Wightman 1990.
[4] Finkelstein 1990; Stager 1990.
[5] Finkelstein 1999.

Hazor X – V: Stratigraphic Congestion?

A major objection to Finkelstein's version of a low chronology is that it "creates an impossibly brief duration" for Hazor Strata X-V. The point, first raised by Mazar,[6] was echoed by Zarzecki-Peleg[7] and elaborated by Ben-Tor and Ben-Ami:

> *"Lowering the date of various ceramic assemblages from the tenth to the ninth century results, for one thing, in a 'dense' stratigraphy for several sites; too many strata have to be 'squeezed' into too short a period of time. The most extreme example is Hazor, where six strata* [X-V]*, with approximately ten sub-phases, would have to be placed within a period of about 120 years* [sic]*. Whereas at Lachish and Megiddo, for example, the 'life span' of each of the Iron Age strata is approximately 80-100 years, the low chronology would allow for each of the six Iron Age strata at Hazor, with several sub-phases in each, a duration of only approximately 25 years."*[8]

The matter has also been aired by Tappy, when discussing the similarities between the pottery of Hazor X and that of Megiddo VA-IVB and Samaria Pottery Period 1 (hereafter PP 1, etc). In Tappy's opinion there are not enough known historical pegs to explain the succession of five cities at Hazor "prior to the final massive depredation at the hands of Tiglath-pileser III in 732 BCE (Stratum V)," unless one dates the end of the earliest (Stratum X) "at least in the late tenth century BCE":

> *"For this reason I disagree with Kenyon's attempt to force the pottery of Stratum X into the ninth century so as to support her desired dating of the PP 1 material at Samaria. Instead, I keep for the present both the architecture and pottery of Hazor X and Megiddo VA-IVB in the tenth century BCE and accept their Solomonic origin."*[9]

[6] Mazar 1997: 161; repeated in Coldstream and Mazar 2003: 42 and Mazar 2005: 25.

[7] Zarzecki-Peleg 1997: 283-284.

[8] Ben-Tor and Ben-Ami 1998: 32. The "120 years" given here is clearly a typographical error for 150 (= 6 x 25).

[9] Tappy 2001: 242, n. 82.

Conversely, Tappy used the comparanda from Hazor X to dispute Kenyon's ninth-century date for Samaria PP 1, preferring to attribute it to a pre-Omride settlement dating back to at least the 10th century BC.[10]

The problem thus has considerable ramifications. These extend to the dating of strata throughout ancient Israel generally, and inevitably embroil the familiar question of the relationship between text and archaeology. The arguments concerning the character (even reality) of the United Monarchy (Saul, David, Solomon) will continue to run, as they depend largely on the disputed interpretation of biblical text (with oblique support provided by the Tel Dan inscription) and will remain archaeologically unfocussed until the chronology of Iron I-II is resolved.[11] There can be, however, no dispute regarding the historicity of the Omrides: Omri, Ahab and Jehu are attested as important regional kings in contemporary extra-biblical records, such as the Mesha Stela and Assyrian royal annals.[12] Hence there can be no disagreement over whether the Omride dynasty should be identifiable archaeologically. The question remains one of which strata we should associate them with. Here some other complex issues are involved – such as the relationship of buildings to pottery at the key sites of Samaria and Jezreel (see further below).

While these difficult issues remain unresolved, and as attempts have been made to clarify them by reference to Hazor, the spacing of Strata X to V takes on further significance. A 25-year average duration for Hazor X-V indeed seems rapid, even given that the city would have been a focal point of the Israelite-Aramaean wars of the ninth to early eighth centuries BC. In reply to Ben-Tor and Ben-Ami, Finkelstein suggested that some of the strata at Hazor "may represent raising of floors and slight changes in the plan" and that there are only three major strata at Hazor for the period in question: X-IX, VIII-VII and VI-V.[13] Yet his belief that we should "eliminate this futile argument from future discussions of Iron II chronology" was sharply dismissed by Ben-Tor, who reiterated the statement that we are not dealing with merely six strata, but ten sub-phases within those.[14] An impasse has been reached between these two scholars on this point.

[10] Tappy 1992: 30, 33, 34, 47, 69, 97-101.

[11] James 2002a.

[12] See conveniently Kitchen 2003: 16-18, 34-36.

[13] Finkelstein 2000a: 242. Wightman (1990: 11) also argued that Hazor X and IX should be treated as one stratum. But, NB, there are also questions of pottery development to consider as much as the number of strata. As Yadin *et al*. 1960: 16 noted "there is a noticeable difference between the pottery of Stratum VIII and that of Stratum VII." (Cf. Mazar 1997: 165, n. 9.) For example, the cooking pots 6.2/6.3 of Hazor X-VIII are replaced in Stratum VII by a new type, 6.5 (Gal 1992a: 77-78).

[14] Ben-Tor 2001: 303.

But has the argument about the compression of strata been necessary in the first place? The calculations of both sides in the debate depend on the same base line, the assumed date of 732 BC for the destruction of Stratum V. Since it forms the bedrock for all reconstructions of Hazor's Iron Age chronology, a re-examination of this date is long overdue.

The Assyrian Conquest, 732 BC

Typically, the destruction of Stratum V in 732 BC is referred to as the "only real anchor at Hazor". Hazor is also seen as the main site, along with Megiddo, providing a "reliable chronological anchor for the late 8th century BCE" in the northern kingdom of Israel.[15] More broadly, it is accepted that Hazor V helps to provide one of the only two safe "anchor points" in the debate over Iron Age chronology. The upper point (12th century BC) is provided by finds of the Egyptian 20th Dynasty in the earliest Iron I levels; the lower (late eighth century BC) is provided by the "identification of strata which were destroyed in the course of the Assyrian campaigns to Palestine," notably Lachish III, Hazor Va and Megiddo IVA.[16] The former is usually assumed to have been destroyed by Sennacherib in 701 BC,[17] the northern sites by Tiglath-pileser III during his invasion of Israel in 732 BC. The capture – though strictly speaking not destruction – of Hazor by Tiglath-pileser is specifically referred to in 2 Kings 15:29.

Regarding the link between the end of Hazor V and the Assyrian campaign of 732 BC, Finkelstein cites Yadin's Schweich lecture.[18] Yet this is the most that Yadin stated there on the matter:

> "*Stratum V contained the remains of the last Israelite fortified city, covered here, like the other areas, with heavy layers of ashes. There can be no doubt that this destruction should be attributed to the destruction by Tiglath-pileser III.*"[19]

The site publication provides no further argument:

[15] Finkelstein 2000a: 242-243; Finkelstein, Ussishkin and Halpern 2000: 322.

[16] Finkelstein 1996: 179-180; cf. Mazar 2005: 19.

[17] The assumption has been questioned as it does not rely on any new evidence as such, but the lack of evidence from earlier strata for a presumed Assyrian destruction (James *et al*. 1991a: 176-178; James 2004: 53; James 2007: 214).

[18] Finkelstein 1996: 180.

[19] Yadin 1972: 112-113.

> "*Stratum V ended in complete destruction; the buildings were burnt down and not rebuilt. This destruction must surely be attributed to the campaign of Tiglath Pileser III in the year 732 B.C. when Hazor was among the cities he captured (II Kings XV, 29).*"[20]

And that is all.[21]

Why then did Yadin choose to date the end of Stratum V to 732 BC? The date was not arrived at by working backwards from the earliest unequivocally dated level, the Persian-period Stratum II of the fourth century BC (see below). Rather than assigning the preceding phases to the early Persian or Neo-Babylonian periods he ascribed Strata III and IV to the Assyrians (seventh and late eighth centuries respectively), in the latter case because of evidence of clear continuity from Stratum V, the end date of which was already assumed to be 732 BC. Yadin's dating thus left a considerable gap in occupation during the Babylonian and early Persian periods.

It would appear, then, that Yadin dated the end of Stratum V to 732 BC as it provided the earliest destruction of a major settlement from before the Persian period. While a reasonable guesstimate, it may be flawed in that it does not allow for destructions at the site in the centuries following the conquest of Tiglath-pileser III. There were, after all, further Assyrian campaigns in the region and there is evidence, for example, that Assurbanipal had to put down revolts in Palestine in the 650s or 640s BC.[22] According to Ezra 4:2 and 4:10, both Esarhaddon and Assurbanipal (the "great and noble Asnapper") settled deportees from Mesopotamia and Iran in the province of Samaria (*Samerina*); these transplants could have been a response to rebellions, and may have also affected the more northerly Assyrian province of *Magidu* (including Megiddo and Hazor). As Forsberg stressed, we should not ignore the possibility of site destructions during the Scythian incursion a generation later, c. 630 BC; nor, for that matter, should we overlook the resulting conflict in Palestine between the Scythians and Saite Egyptians, dated by Spalinger between 622-616 BC.[23] The struggle between the Saite and Neo-Babylonian empires for control of

[20] Yadin *et al*. 1958: 22.

[21] Hazor VA produced an inscription, *lpqḥ* ("belonging to Pekah"), incised (after firing) on the shoulder of a storage jar (Yadin *et al*. 1960: 73, Pls. CLXXI; CLXXII; Yadin 1972: 190, Pl. XXXV*d*). It is sometimes implied (e.g. Yamauchi 1974: 718) that this was Pekah, the penultimate king of Israel, which would have provided confirmation of Yadin's dating of this stratum. However, the absence of any royal title or iconography, plus the occurrence of a similar inscription *ldlyw* ("belonging to Delayo"), not a king, renders an identification with King Pekah unlikely.

[22] Malamat 1953; Rainey 1993: 157-162; Forsberg 1995: 35.

[23] Forsberg 1995: 35-36; Spalinger 1978: 52.

the Levant involved warfare and the destruction of cities from Carchemish to Philistia, and was not resolved until c. 570 BC. [24]

As a supporting argument for his dating of Stratum V, Yadin could point to the fact that the following Stratum IV was a much smaller settlement than its predecessor; it had no fortifications, while new buildings were poor. As "the pottery associated with these structures is for all practical purposes, identical with that of V," Yadin deduced that it represents a *"short-lived effort by the Israelite inhabitants to renew the settlement destroyed by Tiglath-pileser III."*[25] While the historical link is plausible, one might expect similar circumstances – a reduced, smaller settlement with poorer structures – after any destruction by invaders. Yadin did not support his identification by reference to changes in material culture, such as the introduction of new architecture or pottery. From the negligible changes in the pottery repertoire there are no grounds for suggesting that a new population of deportees was introduced, as suggested, for example, by Geva for Samaria VI and Megiddo III.[26] The few finds of Assyrian or Assyrian-style pottery from Hazor are actually problematic for Yadin's interpretation (see below).

Thus Yadin's only substantive point in associating the destruction of Hazor V with the Assyrian conquest remains the huge layer of ash and rubble, up to one metre in height, which covered it and the decline evident in the succeeding settlement. But while we may fully expect an (assumed) Assyrian destruction to have been violent, in itself this is, of course, not diagnostic enough evidence to reassure us that we have identified the correct level.

The consensus dating of Hazor V cannot be upheld simply by reference to ceramic comparisons from other Israelite typesites, such as Megiddo and Samaria. Similar assumptions to that at Hazor V have controlled their late Iron Age chronology – involving the idea that any major pre-Persian destruction automatically represents the Assyrian conquest of the eighth century BC and hence provides an 'anchor point' for dating earlier and later strata. Yet recent studies have eroded the traditional understanding that 'Assyrian destruction levels' have been confidently identified and/or dated at Samaria

[24] Kuhrt 2002: 23-24. Elsewhere (briefly James 2004: 54-55; in more detail James 2006; James in press) I have questioned the usual dating of the destruction of Ekron IB and pre-Persian Ashkelon to the 604 BC campaign of Nebuchadrezzar, suggesting instead a date ca. 570 BC during further wars with the Saites – one which would be more in line both with the dating of Ekron IC suggested by the Assyrian dating of the Ekron temple inscription (James 2005a) and with the growing case for a reduction of the dating of Archaic Greek pottery (of the kind found at Ekron and Ashkelon) by some 35 years at ca. 600 BC (James 2003; James 2005b; James 2006; James in press; see also Bowden 1991; Bowden 1996; Gill 2005; Gill in press).

[25] Yadin 1972: 191.

[26] Geva 1979.

and Megiddo.[27] The chronology of the Iron II strata at these two key sites is in now a considerable state of flux. We will return to Megiddo and Samaria last, avoiding the frequently circular arguments of cross-dating among the three sites that have hitherto dominated the literature. There is a way to cut through such circular arguments at Hazor – by using the controlling information provided by external chronologies, namely those of Phoenicia, Cyprus, Egypt and Mesopotamia, as well as historical evidence.

Assyrian Destruction, V or VII?

It therefore emerges that Yadin offered no conclusive evidence for dating the fall of Hazor V to 732 BC and external evidence shows there are serious problems with this dating. The most conspicuous is raised by Phoenician ceramic chronology, which strongly suggests that Hazor VII should be dated to the mid-eighth century BC,[28] the slot presently occupied by Hazor V on the Yadin/consensus dating.

Hazor has long been embroiled in a debate concerning the provenance and dating of the so-called 'torpedo' storage jars (a.k.a. 'sausage' or 'crisp ware' storage jars). This form first occurs significantly at Tyre in Stratum III,[29] dated to the second half of the eighth century BC.[30] Yet there are numerous examples

[27] Samaria – Forsberg 1995: 17-50; Tappy 2001: 349-441; Megiddo – Finkelstein, Ussishkin and Halpern 2000: 322, 563-65, 598.

[28] See briefly James *et al*. 1998: 30.

[29] According to Bikai (1978b: 48), "It was not common in Stratum IV (and might be intrusive there), but in Strata III-II, over 80% (c. 527 pieces) of all storage-jar rim fragments were of this type."

[30] In earlier publications Bikai considered a date of c. 740 BC for the beginning of Tyre III. Most recently Bikai (2003: 234) raised this date to c. 750 BC. This minimal adjustment does nothing to solve the problem of the occurrence of Tyre III type storage jars in Hazor VII and VI (both pre-760 BC, conventionally). But the reasons for Bikai's redating need to be noted. First, Salamis Tomb 1 and the last part of Tell Abu Hawam Str, III contain Phoenician pottery known from the Tyre IV-V horizon. Salamis 1 also has Greek imports dated conventionally to the second quarter of the eighth century BC. "Lacking a reason to redate the Greek material in the tomb, one presumes that Tyre IV-V is of equivalent date." (Bikai 2003: 234). Likewise Bikai cites Balensi and Herrera's redating of Tell Abu Hawam to c. 900-750 BC, concluding that "between the evidence from Tell Abu Hawam and Salamis Tomb 1, there seems to be an anchor for Tyre IV-V in the first half of the eighth century, raising the estimated dates from 760?-740 to 800-750 BC. Here Bikai was unfortunately unaware of the mounting case for lowering the dates of Greek Geometric (see note 48 below). Regarding Tell Abu Hawam III, Balensi (1985) has also considered an end date as late as 650 BC, with a possible compromise at c. 700 BC at the invasion of Sennacherib (pers. comm. cited in James *et al*. 1991a: 361, n. 48). Besides, as one of Balensi's stated reasons for lowering the dates of Stratum III are links with Tyre, the argument becomes somewhat circular! Second, Bikai reasons that her old scheme put "too much activity in the eighth century." Granted, but this would be better alleviated by allowing Tyre III-II to range into the seventh century (following the Egyptian evidence, see main text), rather than by raising the dates of Tyre VI-VII from 800-760? to the late ninth century as she now proposes.

from Hazor VI,[31] conventionally dated to the early eighth century, and a few from Stratum VII dated to the late ninth century.[32] Hence Geva questioned their Phoenician origin, on the grounds that they begin to appear earlier in Israel.[33]

Bikai firmly objected to this early date and defended the Phoenician origin of this type, which represents over 80% of the storage jars known from Tyre III-II. A Phoenician origin is also clearly indicated by their distribution. While there are over 60 examples (either rims or whole vessels) known from Hazor and 169 from Megiddo, far greater numbers come from Tyre (300), Sarepta (258) and the late eighth-century Phoenician shipwrecks *Tanit* (385) and *Elissa* (396). Bikai noted that finds of torpedo jars at Phoenician colonies such as Kition and Carthage also argue for a Phoenician origin,[34] an observation accepted by Ballard *et al.*, especially in light of the new shipwreck finds. They further observe that

> *"... these amphoras are purpose-built maritime containers. They are built to be easily stacked in the hold of a ship, to have consistent capacity, and to be easily tied down using special handles. This argues for a production facility familiar with the needs of maritime transport."*[35]

Geva claimed that neutron activation study had shown that the torpedo jars from Tyre were not locally made, as the composition of their clay did not match that of an unfired clay sample from *"the assumed potter's workshop nor the Cypriot pottery"*.[36] This argument was defused by Bikai, who pointed out that the composition of the single sherd analysed was actually close to that of the unfired clay, such difference as there is being possibly explainable "by the potter's addition of various materials to the clay." Further, in Bikai's opinion, Tyre Stratum III was effectively a dump for wasters and other debris from a pottery manufacturing area. A high percentage of the torpedo jar sherds were cracked or defective: "Such unusable pottery would hardly have been moved very far from the place of production." At Sarepta kilns were found

[31] Yadin *et al*. 1958: Pl. LXV, 13; Yadin *et al*. 1960: Pls. LXXII, 1-9, LXXIII, 1-17; Yadin *et al*. 1961: Pl. CLXXXVI,11-12, 15, 17-19; Ben-Tor *et al*. 1997: 264, Fig. III.40, nos. 1, 5.

[32] Yadin *et al.* 1961: Pl. CLXXX, 19-20, 23. Two examples are also reported from Stratum VIII (Yadin *et al*. 1960: 13-14, Pl. LX, 9-10; Tappy 2001: 163, n. 606), though the excavators – without specifying why – were cautious of their attribution.

[33] Geva 1982.

[34] Bikai 1985: 72.

[35] Ballard *et al.* 2002: 160 and 158-159, 161 for distribution figures.

[36] Geva 1982: 69; Bieber in Bikai 1978.

near similar dumps of storage jars.[37] Final confirmation of the Phoenician manufacture of torpedo jars comes from two recent neutron activation analyses. The petrographic profile of the shipwreck examples matches the distinctive pattern, of the central Levantine coast.[38] The three torpedo jars analysed of the ten known from Lachish III have the same provenance.[39]

The question remains whether the examples from Hazor are Phoenician or locally made. Gilboa has argued against a trade model, observing that there are tiny, but consistent, morphological differences between the torpedo storage jars from the two sites,[40] a conclusion that has received some support from profilograph copying: *"the jars from each city are far more similar to those from the neighbouring area."*[41] Computerised study of the curvature of all the published torpedo jars from Tyre and Hazor has led to this conclusion:

> *"... the lack of a significant morphological overlap raises doubts about the claims that the 'torpedo jar' assemblages indicate commercial links between Hazor and Tyre, as suggested by Geva. The higher inner similarities observed in the assemblage of jars at Tyre supports the possibility that they were produced locally, as suggested by Bikai, possibly in a single workshop. There is still a possibility that some of the jars found at Hazor were actually made in Tyre, and perhaps even one or two of the ones found at Tyre were Hazorite. ... but the 'torpedo jar' phenomenon as a whole does not indicate mass trade in any one direction."*[42]

Whether this solves the problem is far from certain. Products for home and export markets can often differ slightly, and might be made in different workshops. Further the high inner consistency of the Tyrian examples, the mass of other evidence (including distribution and petrography) that the 'torpedo jar' was primarily a Phoenician shape, and the possibility (allowed by Gilboa *et al*.) that some of the jars from Hazor *were* made at Tyre, would allow this at the very least: that such vessels were initially imported from Phoenicia and were copied locally in Israel. Finally, questions of trade aside, the chronological tension remains: on the present high chronology for Hazor, the production of highly distinctive and virtually identical vessels begins nearly a century earlier there than at nearby Tyre.

[37] Bikai 1985: 71.

[38] Ballard *et al.* 2002: 160 and n. 18.

[39] Goren in Ussishkin 2004: 2562, 2588; for discussion James 2007: 216.

[40] Gilboa 1995.

[41] Watzman 2004: 97.

[42] Gilboa *et al*. 2004: 692.

The *floruit* of the torpedo storage jars in Phoenician terms can be placed within close parameters following the Tyrian chronology established by Bikai. As stressed, the dating of Tyre III is corroborated by a complex of mutually supporting evidence which we defined as a late eighth-early seventh century *"Cypro-Phoenician horizon."*[43] The discovery of an inscribed Egyptian urn of the late 25th or 26th Dynasty from the closing stage of Stratum III allowed Bikai to set the end of this stratum no earlier than 725 BC, or possibly later.[44] In the opinion of Robert Morkot (pers. comm.), and in agreement with de Meulenaere,[45] the urn is much more likely to belong to the seventh century than the eighth, which would require a somewhat lower date for the end of Stratum III.[46] In any case, a late eighth to seventh century date for Tyre III is corroborated by its Cypriot imports – these belong to the Cypro-Archaic I period, initially dated by Gjerstad to 700-600 BC, and revised by Karageorghis to 750-600 BC, with Gjerstad accepting a compromise start date of 725 BC.[47] The chronology of Cypro-Archaic I is fixed by synchronisms with approximately secure evidence from Greece (Late Geometric-Archaic) and by Egyptian scarabs.[48]

Though there is evidence for some lowering of the dates for Tyre III-II, there is none for raising them. Indeed, it is now generally accepted that the chronological distribution of torpedo-jars *"is heavily weighted toward the middle to end of the eighth century"* or the *"three final decades of that century."*[49] As it would be difficult – rather, impossible – to raise the dates of the Tyrian torpedo jars, we suggested that the logical thing to do *"would be to lower the chronology of Hazor VII and associated levels at other sites."*[50] This would mean placing Hazor VII no earlier than the mid-eighth century BC, the very time in which Yadin placed Hazor V. Consequently we should consider that it was Hazor VII, rather than V, which fell to Tiglath-pileser III in 732 BC; Hazor VII was indeed destroyed by a widespread fire, the very kind of destruction sought by Yadin for the Assyrian conquest. Such a suggestion is radical, but

[43] James *et al*. 1998: 30.

[44] Bikai 1987: 69; cf. 1978a: 68; 1978b: 47; 1981: 33; cf. James *et al*. 1991a: 108.

[45] In Bikai 1978b: 84.

[46] This would go a step towards the suggestion of Gal (1992a: 73-74; 1992b: 184), that Tyre IV-I all postdate 700 BC.

[47] See conveniently James *et al*. 1991a: 152-3.

[48] See James *et al*. 1991a: 367, n. 37; 1998: 30. NB, if anything, Late Geometric dates are too high, by some 25 or more (James *et al*. 1991a, 111; James 2003: 241-243; James 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."

[49] Ballard *et al.* 2002: 158; Tappy 2001: 164.

[50] James *et al*. 1998: 30.

can be tested through examination of the dating evidence (both from external chronologies and historical considerations) for the preceding and succeeding strata. These are discussed below in ascending stratigraphic order.

Strata X-IX

A placement of Hazor VII in the mid-eighth, rather than the late-ninth century, would automatically resolve the "dense stratigraphy" problem created by the Finkelstein model (as perceived by Ben-Tor, Mazar, etc.). It would place four (X-VII) rather than six (X-V) strata within the ca. 150 years between the early ninth century and 732 BC, giving a reasonable average of nearly 40 years per stratum for these substantial levels (approximately the same as that on the Yadin/Ben-Tor chronology). A more relaxed, and arguably more 'natural', chronology can then be offered for strata X-VII.

There is no need to rehearse the arguments for the unhinging of Stratum X and its gateway from its traditional "Solomonic" date.[51] Likewise the similarities between the pottery of Hazor X and the more recently excavated compound at Tel Jezreel have been much discussed. These were established by Zimhoni and caused considerable stir, as ceramic types normally dated to the 10th century (Megiddo VA-IVB and Hazor X) were discovered at a site which appeared to date to the early to mid-ninth century BC, creating a discrepancy of *"eighty years or more."*[52] The historical dating of Jezreel is reasonably secure. The short-lived fortified enclosure has been convincingly linked (from biblical references) to the Omride dynasty[53], more specifically Ahab (875/874-853) [54] who had a royal residence there (1 Kgs. 21:1; 1 Kgs. 18:45-46). As Omri (886/5-875/4 BC) built Samaria during the second half of his short reign (1 Kgs 16:23-24), it was most likely Ahab who built the enclosure at Jezreel. Who or what was responsible for its destruction is harder to ascertain. Ussishkin and Woodhead dated it to the rebellion of Jehu (841 BC), when the latter slaughtered the family, officials and priests of Ahab at Jezreel (2 Kgs. 9-10). Yet despite the bloodshed, remembered in later times (Hoshea 1:4), there is no reference to Jehu destroying the city, and Na'aman has rightly asked why Jehu should have levelled such a strategically located strongpoint and administrative centre. Accordingly he links the destruction

[51] James *et al*. 1987: 64-65; Wightman 1990; James *et al*. 1991a: 189-190; Finkelstein 1996: 178-79; Finkelstein 1999: 57-59.

[52] Zimhoni 1997: 39.

[53] Na'aman 1997: 122-129.

[54] The dates summarised by Kitchen (2003: 30) are used here for convenience.

with the invasions of Hazael of Damascus, ca. 835 BC,[55] though a later date during the continued Aramaean/Israelite wars is equally plausible.

Assuming that the compound of Jezreel was built by Ahab ca. 875-850 BC and destroyed ca. 835/800 BC, the Iron IIA pottery found on its floors should have a similar date range. Ben-Tor challenged the integrity of the site, which is widely disturbed by later Byzantine construction, arguing that the assemblages from the eight key loci discussed by Zimhoni are "archaeologically unreliable" and may be considerably mixed with pottery from an earlier phase of settlement. Ussishkin fully answered this charge, showing in detail that each of the contexts involved satisfies Ben-Tor's own requirements for an "archaeologically reliable" locus (clearly defined on all sides, usually by walls; recognizable floors adjoining walls; restorable pottery). Nor is there any evidence of a preceding large-scale settlement. Ussishkin seems justified in his conclusion: *"Assuming that the Jezreel enclosure can be reliably dated to the period of the Omride dynasty it follows that the Jezreel enclosure and its pottery assemblage can be used as a 'key site' in the current debate on Iron Age chronology..."*[56]

Finkelstein offered some general resemblances between the plan and architecture of Jezreel and Hazor X,[57] but after criticism from Ben-Tor, he stressed that these were secondary observations, made only "after *the date of Hazor X in the early 9th century was established*."[58] For this he followed Zimhoni's lead from Jezreel, dating Hazor X to the *"late 10th, or better, to the early 9th century BCE"*.[59] Yet Finkelstein's version of a "low chronology" results in an extremely cramped sequence for the ninth century (because of its essential adherence to Yadin's Hazor VII dating):

Stratum X, early 9th century (Omride)
Stratum IX, first half of 9th century (Omride)
Stratum VIII, late 9th century
Stratum VII, end of 9th century

There are clearly problems with this scheme. Where Yadin had three ninth century levels, Finkelstein has four – the essence of the complaint of

[55] Ussishkin and Woodhead 1992: 53; Na'aman 1997: 122-129.

[56] Ben-Tor 2000: 12-13; Ussishkin 2000: esp. 255.

[57] *"A possible support for dating Hazor X to the days of the Omrides is the resemblance in layout and construction between this city and the Jezreel enclosure: both constitute a levelling/filling operation, a casemate wall, a moat and a four-entry gate ..."* – Finkelstein 1999: 60; see also 2000b: esp. 128-129.

[58] Finkelstein 2000a: 240.

[59] Finkelstein 1999: 59. To this Finkelstein (1996) added his observations regarding a lower date for Philistine ware, with a knock-on effect for Israelite Iron IIA pottery. These claims have not been widely accepted and I hope to discuss them in detail elsewhere.

Ben-Tor and others regarding a compressed stratigraphy. While the case for dating the construction of Stratum X to the Omride dynasty is sound (from the comparisons with the Jezreel pottery), the dates for its first king (Omri) are 886/5-875/4 BC. One wonders, then, what to make of Finkelstein's two strata assigned to the early Omride dynasty, to the "early 9th" (X) and "first half of ninth" (IX) century respectively; the two would occupy only 35 years.

By compressing both Stratum X and IX into the first half of the ninth century, Finkelstein's scheme also overlooks a potentially valuable synchronism offered by Cypriot pottery. Both Hazor X and IX produced pieces of Cypriot Black-on-Red (B-o-R) I ware.[60] This was dated by Gjerstad to ca. 850-700 BC, a chronology followed until very recently by Cypriot archaeologists, with the difference that the terminal date was raised by Karageorghis to ca. 750 BC.[61] This synchronism with Cypriot chronology naturally clashes with the 10th-century BC date for Hazor X advocated by Yadin, Ben-Tor and others. The present writer and colleagues have repeatedly drawn attention to this problem, in which ninth-century Cypriot pottery is found in "10th", or even "11th" century Palestinian strata.[62] Our suggestion was that, if we follow the

[60] The clear B-o-R I finds at Hazor are as follows: Yadin *et al*. 1961: Pl. CLXXII, 1 (Hazor Xb); CLXXIV, 15 (Stratum Xa); CLXXVII, 14 (Strata X-IX); Yadin *et al*. 1958: 11 and Pl. XLVI, 1-2 (Strata X-IX); Yadin *et al*. 1960: Pl. LII, 17 (Stratum IX); Yadin *et al*. 1961: CCVIII, 38-40, 44 (Stratum IX), see also CCCLV,11-12, 14, 16-17; Yadin *et al*. 1958: L, 14. For a detailed examination of the contexts see Schreiber (2003: 104-111, 189-195), who concludes "that BoR pottery first appears in loci assigned to Stratum X." The implication in Tappy 1992: 130, n. 128 that further finds in Strata V-IV are also B-o-R I (Yadin *et al.* 1960: Pls. LXXV, 11-12; LXXXVIII, 1-4; XCVII, 10 [Stratum V]; C, 22 [Stratum IV]; Tappy mistakenly gives C, 23) is misleading. Amiran 1969: 290, Pl. 98, 9-12 classifies them separately from B-o-R I, II and III, and in the opinion of N. Kokkinos (pers. comm.) they may be late, derivative forms. Regarding these and other possible B-o-R pieces from Hazor VIII-III, Schreiber 2003: 194 stresses that "the vessels were generally preserved in a highly fragmentary form, seldom more than a sherd. Only one almost complete vessel was found, in a Stratum VI context."

[61] See conveniently James *et al*. 1991a: 153, 366, n. 35; Schreiber 2003: 233-234, and n. 16.

[62] James *et al*. 1991a: 155-161; James *et al*. 1991b: 229-230, 234; James *et al*. 1992: 130; James *et al.* 1998: 30-32. Our statements regarding the importance of this problem have been acknowledged by, *inter alia*, Sörenson 1993 and Schreiber 2001. The only adherents of the "Tel Aviv low chronology" to have remarked at length on the B-o-R problem are Gilboa and Sharon (2001: 1348-1349; 2003: 62-67). They agree with our dates for the related question of the start of Iron IIA, yet perhaps to avoid the impression that they have borrowed our arguments, have misrepresented our position. Gilboa and Sharon 2003: 72, n. 31 and Gilboa, Sharon and Zorn 2004: 54, n. 40 state that we date the end of the LBA to the mid ninth century and lower the "beginning of the Iron IIA to the early eighth century." Our suggested end for the LBA was ca. 950 BC. Re Iron IIA, echoing (and acknowledging) the work of predecessors (mainly Kenyon), we have long argued (before the arrival of the "Tel Aviv" school) that Iron IIA, conventionally dated c. 1000-900 BC began in the ninth century – one plank of the argument being the Cypriot (Gjerstad) dating of B-o-R I. For example, at Samaria we followed Kenyon in associating early Iron IIA with the building activities of the ninth-century Omrides. This is manifestly clear from our text (James *et al.*1991a: 183-188), rather than Table 8:3 (James *et al.* 1991a: 195) which mistakenly showed Iron IIA beginning c. 800 BC. A corrected table was rapidly published in James *et al*. 1991b: 232, placing the beginning of Iron IIA at c. 850 BC. The same date

traditional Cypriot chronology, then Palestinian pottery horizons containing B-o-R must overlap the date of 850 BC.[63] If so, then the occurrence of B-o-R in Hazor X also clashes with the "Tel Aviv low chronology," which would place this stratum much earlier in the ninth century BC.

The B-o-R question returns us to Samaria. Kenyon initially established her dates for Samaria BP I and II from the biblical account, which states that the city was founded by Omri (1 Kgs. 16:23-8). Accordingly she dated Samaria PP 1 and 2, from the fills under the floors of these architectural phases, to the ninth century BC. She supported this by reference to Gjerstad's Cypriot chronology, as PP 2 included a fragment of a B-o-R I juglet.[64] Yet even Kenyon's much debated low dates for Samaria are somewhat too high.[65] As Tappy points out, since Kenyon believed that Samaria II was built by Ahab, PP 2 should precede some point within his reign (875/4-851 BC), which would conflict with Gjerstad's start-date of c. 850 BC for B-o-R I. Elsewhere we argued that Samaria II was more likely to have been built later than Ahab, probably under Jeroboam II (791/0-750/49 BC). After a thorough analysis of the earliest architecture at Samaria, Franklin has reached the same conclusion: Samaria BP I represents the palace of all the Omrides (plus Jehu), while BP II must be downdated to the eighth century.[66]

Finkelstein is of the same opinion as Stager and Tappy, that the pottery from the PP 1 and 2 fills represents 11th-10th material from a pre-enclosure settlement at the site.[67] Yet as Zimhoni predicted, the matter will need re-analysis in the light of the Jezreel excavations, where the occurrence of "10th-century" pottery in a ninth-century compound provides a close analogy to the situation at Samaria.[68] It is not consistent methodologically (as Finkelstein does) to redate the 10th-century pottery at Jezreel to the ninth century while dismissing

for the beginning of Iron IIA and the arrival of B-o-R was argued in Gilboa and Sharon 2001: 1347 on the basis of radiocarbon. Our corrected table was, however, acknowledged in Gilboa, Sharon, Zorn 2004: 54, n. 39, with the remark that "it is compatible with ^{14}C determinations for the Iron Age II horizon at Dor." While awaiting the detailed study of Chapman (in prep.) our present view is that Iron IIA likely began earlier, ca. 875 BC, though 875/850 BC may be a more realistic representation.

[63] Some have tried to resolve the issue by raising the Cypriot dates (see n. 71 below). The recent analysis by Schreiber unfortunately gives little weight to low chronologies for Palestinian contexts, and concludes that B-o-R first appeared "ca. 925 BC – at the earliest, ca. 940 BC." Nevertheless, appreciating that the dating of these contexts is not settled, she adds: *"An alternative possibility, that these levels should be dated from the early-mid ninth century, may, with future excavation, prevail."* (Schreiber 2003: 309.) Another issue raised by Schreiber, regarding the chronology of B-o-R relative to that of other Cypriot ceramic types, is beyond the scope of the present study, but see Iacovou 2004.

[64] See Tappy 1992: 127, n. 113.

[65] James *et al*. 1998: 31.

[66] James *et al*. 1991a: 185-187; Franklin 2004; 2005.

[67] Finkelstein 1990: 114-116; 1996: 179; Stager 1990; Tappy 1992.

[68] Zimhoni 1997: 25.

similar material at Samaria as belonging to a pre-Omride settlement.[69] Rather the Jezreel finds make the Stager/Tappy/Finkelstein position seem redundant. Further analysis of PP 1 and 2 contexts is awaited,[70] but it is reasonable to make some preliminary deductions. If PP 2 is what Kenyon understood it to be, then it must largely represent occupational debris that accumulated after the building of Samaria I and fills for the building of Samaria II. Using the dates suggested above for the first two building periods (and now argued by Franklin), the period of PP 2 would fall roughly ca. 875-790/775 BC, providing support for Gjerstad's dating of B-o-R I.

The evidence from Cyprus combined with the excavation of two Omride sites (Samaria and Jezreel) makes a formidable case for dating the start of Iron IIA to the ninth century. The suggested alternatives have required problematic or *ad hoc* 'solutions' in each case: for Cyprus, the introduction of unacceptable chronologies,[71] and for the royal enclosures of Samaria and Jezreel, insistence that the earliest pottery must be "residual" and unconnected with their building phases. To invoke Occam's razor, the least complicated answer is preferable: the "11th-10th" and "10th-century" pottery from Samaria and Jezreel respectively is Omride and dates to the ninth century, and the low Cypriot chronology is not in need of major upward revision. [72]

Given this, the close of Hazor X (with B-o-R I) should fall later than c. 850 BC. There would then be no room for Hazor IX to also fall within the first half of the ninth century, as in Finkelstein's model; rather it would have to belong to the late ninth century. The end of Stratum X might then be attributed to the conquests of Hazael ca. 835 BC, while the burnt layer of Hazor IX could then be attributed to the continuing wars between the Aramaeans and Israelites during the late ninth-early eighth centuries BC. [73]

[69] Finkelstein 2005: 37 rightly describes Ben-Tor's attempt (2000) to relegate the Jezreel pottery to an earlier phase at the site as "a desperate attempt to save the idea of a great United Monarchy." Relegating all the pottery of Samaria PP1 and 2 to a pre-Omride settlement might be seen as an equally desperate attempt to avoid acknowledging the British excavators of Samaria as the originators of the 10th-ninth century BC shift for Iron IIA – cf. Kletter 2004: 44.

[70] Chapman 2007; Chapman in prep.

[71] The option experimented with by van Beek created a lengthy hiatus in the Cypro-Archaic period; the alternative (discredited) scheme of Birmingham involved an extraordinary raising of the start of Cypro-*Classical* from 475 to 600 BC! (For discussion and references see James *et al*. 1991a: 153-154.) Most recently Karageorghis (2002: 6; 2005: 104) has followed Coldstream (1999: 114-115) in raising the beginning of CG III from c. 850 BC to 900 BC, in order to shorten the rather blank CG II period from one hundred to fifty years (i.e. 950-900 BC). This seems ill-advised. A general lowering, rather than raising, of Cypriot 'Dark Age' dates would be a preferable solution (James *et al*. 1991a: 156-157).

[72] Cf. Gilboa and Sharon 2003: 66-67.

[73] There is some confusion as to whether the ashes belong to Stratum IXA or IXB – see Schreiber 2003: 112.

Stratum VIII

Regarding the next phase at Hazor, Yadin wrote:

> *"The city of Stratum VIII is entirely different from that of Strata X-IX in layout, area, character, public buildings and installations. It has now become a strongly fortified city, with mighty walls, strong citadel, public store-houses and, above all, a huge underground water-system capable of sustaining the city through a long siege. The most salient contrast with the Solomonic city* [Stratum X] *is the fact that the Stratum VIII city covers the whole Tell, doubling its built up area."*[74]

This well-organised and defended city was clearly built by a powerful ruler commanding considerable resources. Yadin's suggestion that it was built by Ahab made good sense, in the context of his chronology. Finkelstein attributes its construction to Hazael of Damascus, in the late ninth century. He supported this by reference to three ostraca written in "Phoenician or Aramaic" and the tentative reconstruction of the citadel of this stratum as a bit hilani. Neither is realistic as evidence that VIII was an Aramaean stratum.[75]

On the model argued here, Stratum VIII falls between the late ninth (IX) and mid eighth centuries BC (VII), and can be reasonably associated with the renaissance of Israelite power under Jeroboam II, ca. 790-750 BC. He was said (2 Kgs. 14:25) to have restored Israel's lands in the north as far as "the entrance of Hamath," i.e. the Bekaa valley. Hazor VIII was described by Yadin as *"not just an administrative city, with palaces for governors – but a well fortified city... capable of withstanding prolonged siege."*[76] The establishment of a powerful stronghold at Hazor, strategically placed on the route to Syria, fits well with Jeroboam's expansionist policy.

Regarding the three ostraca from Stratum VIII, they could be Phoenician or Aramaean.[77] If Phoenician they might reflect traders from the Lebanon, which would match the appearance of Phoenician-style storage jars in this stratum (see note 32 above).[78] While such minimal evidence (the short inscriptions do not contain any whole words) tells us little about the population of Hazor at this time, one of the ostraca may provide an important dating clue. Ostracon 3

[74] Yadin 1972: 165.

[75] Finkelstein 1999: 61; see Kletter 2004: 30.

[76] Yadin 1972: 164.

[77] Sass 2005: 85-86.

[78] Katzenstein 1997: 195-199) argues persuasively that Jeroboam II reached a new accord with Tyre, after the rupture in relations under Jehu. Kuan 2001: 149-150 dates the "convenant" between Israel and Tyre referred to by Amos (1:9) to early in the reign of Jeroboam.

begins with an apparent *bet*, the similarity of which to the *bet* on the Byblite inscriptions of Shipitbaal and the Abdo sherd is striking, as Sass notes, "eye-catching" enough to have been agreed by all commentators. The dating of the Byblite inscriptions has long been a matter of controversy, a focal point of discussion being the probable identity of king Shipitbaal with the like-named ruler of Byblos mentioned in the records of the Assyrian Tiglath-pileser III ca. 740 BC.[79] The identification was offered long ago by B. Mazar as a potential fixed point for dating Byblite palaeography.[80] However Yadin remarked that the sherd from Hazor VIII suggested a higher, ninth-century dating for the Shipitbaal inscription.[81] Since then, Wallenfels offered palaeographic arguments for redating the relevant group of Byblite inscriptions from the 10th to the ninth-seventh centuries BC; the Abdo sherd and king Shipitbaal would be assigned to the eighth century BC, strengthening the case for identifying him with the ruler known from ca. 740 BC.[82] Sass has argued a similar though slightly earlier range for the group, but because of the Hazor sherd suggested a dating for Shipitbaal in the last third of the ninth century BC, eschewing the synchronism with Assyria.[83] Yet if Stratum VIII actually belongs to the first half of the eighth century, the identification of the two Shipitbaals, as argued by Mazar and Wallenfels, once more comes into focus. Once again, independent arguments from Phoenicia (in this case palaeographic and historical considerations), suggest that the dating at Hazor is too high.

Stratum VII

Hazor VII was a continuation (with some signs of decline) of the well-built Stratum VIII city.[84] A mid-eighth century date has already been argued (above), on the basis of the torpedo-jar finds from Strata VII and VI. As noted, the signs of widespread destruction by fire in Stratum VII would suit well the assumption that Tiglath-pileser III destroyed Hazor in his campaign of 732 BC.

[79] See conveniently James *et al*. 1991a: 248-251.
[80] Mazar 1946: 178-179; 1986: 244-245.
[81] Yadin *et al*. 1960: 71 and n. 6.
[82] Wallenfels 1983: esp. 111.
[83] Sass 2005: 32-32.
[84] Yadin 1972: 168-69, 200.

Stratum VI

Yadin's date for Hazor VI was set by the assumption that its successor Stratum V was destroyed by Tiglath-pileser III in 732 BC. As there is evidence that Stratum VI was destroyed by a violent earthquake, Yadin naturally associated this with the famous earthquake which occurred under Jeroboam II/Uzziah, ca. 760 BC (Amos 1:4; Zechariah 14:5).[85] While widely accepted as a plausible historical link[86], the association does not, of course, provide a diagnostic means of dating the end of Stratum VI; the walls could have fallen during a later, undocumented, earthquake.

There were major differences between the city plan of Hazor VII and that of its successor VI. Yadin stated:

> *"This stratum* [VII] *was completely destroyed and the pillared storehouse and other buildings were not reconstructed in the following strata... The public buildings of the* [sic] *stratum VII were not reused in stratum VI, and the entire area became a residential quarter with workshops and stores."*[87]

As stressed by Zarzecki-Peleg:

> *"... in stratum VI, Hazor underwent a significant change of status, as expressed in its urban disposition, through the utilization and repartition of its districts, especially in reference to the storage areas. It is not merely a question of their transfer to the area adjoining the city's entrance (Area G), as Yadin averred (Yadin 1972: 184). The change was more significant, and affected the settlement's entire design."*[88]

The radically different plan and character of the new city of Stratum VI, including the abandonment of old public buildings, could suggest a change in governance. In the opinion of Yadin the new layout was due to the rebuilding of the city by Jeroboam II, after he had recovered it from the Aramaeans.[89] Yet if it was Hazor VII that was destroyed by Tiglath-pileser III, then the radical change in character of Hazor VI would reflect the transition from Israelite kingdom to Assyrian province (732 BC onwards). The Hebrew inscription on a storage jar from this stratum does not, of course, demonstrate that there was

[85] Yadin 1972: 113.

[86] E.g. Fantalkin and Finkelstein 2006: 23.

[87] Yadin 1993: 601; cf. 1972: 169, 179.

[88] Zarzecki-Peleg 2005: 372.

[89] Yadin 1972: 179; cf. Finkelstein 1999: 63.

still an Israelite government.[90] We can envisage Hazor VI as a largely Israelite resettlement under the Assyrians, in the same way that Yadin conceived Hazor IV (see below).

The greatest number of torpedo jars comes from Hazor VI. A placement of this stratum after 732 BC would be in step with Phoenician chronology which places the peak of torpedo-jar production no earlier than the late eighth century BC (see above). Cross-dating with the Egyptian site of Tell el-Maskhuta would strongly suggest that VI continued into the seventh century BC. Very similar torpedo-jars were found at Tell el-Maskhuta, in the earliest Saite (26th Dynasty) deposit, the beginning of which can be reliably dated to the last decade of the seventh century BC.[91] Neutron activation analysis of the Tell el-Maskhuta examples showed the chemical composition was "very close" to the one piece analysed from Tyre.[92] Of the shapes, Holladay noted that "[type] Rim 4:5 is restricted to this earliest period," but was surprised that "a slightly different (?) version has a much longer history in the Levant, apparently being common already by ca. 735-22 BC, although this may be questioned." The examples he notes are from Hazor VI-V, but because of the apparent chronological gap Holladay wondered whether *"The positive association of these vessels with these strata probably should be reviewed."*[93] Alternatively, if the date of Stratum VI is lowered to include the early seventh century, and V to the mid seventh century BC (see below), then the gulf in time between the Hazor examples and the very similar finds from late seventh-century Tell el-Maskhuta is massively reduced. The problem can thus be solved without recourse to challenging the probity of the Hazor findspots (see also later, Methodological Considerations).

An Assyrian-empire date for Hazor VI fits the occurrence of a sherd described by Tappy as a carinated form similar to Assyrian styles.[94] Amiran saw such vessels as indicative of an Assyrian date (post-721 BC). Tappy argued that as such vessels appear to be locally made, "one must allow time for the original Assyrian style to take hold within the local cultures"; hence

[90] *LMKBRM*, read by Yadin (1972: 181-182) as "belonging to (PN) Makhbiram" and, more plausibly as "belonging to the food-servers" by Naveh (1981).

[91] The Saite foundation at Tell el-Maskhuta was clearly connected with the creation of the Wadi Tumilat canal, connecting the Nile (and hence Mediterranean) with the Red Sea, by Necho II (610-595 BC) – see Herodotus 2.158; text and translation Godley 1926. Holladay (1982, 19) allows that it is "not impossible" that preparatory work on the canal began under his predecessor Psammetichus (664-610 BC), but correctly points out that there is no reason to believe this.

[92] Paice 1986/1987: 97; Bieber in Bikai 1978a – see above.

[93] Holladay 1982: 52 and n. 77.

[94] Yadin *et al*. 1960: Pl. LXVII, 5; Tappy 2001: 313, nn. 442-443.

such imitations *"likely belong mainly to the seventh century BCE,"* [95] fitting the suggestion here that Stratum VI postdates the Assyrian conquest. A late-eighth to early-seventh century date for Hazor VI is thus supported by links with Phoenician, Egyptian and Assyrian chronologies.

Strata V-IV

Both the pottery and architecture of Stratum V (with subphases VB and VA) link it closely with Stratum VI. Likewise the pottery of Stratum IV *"is for all practical purposes identical with that of V."*[96] The conventional dating of both thus depends on the assumption that V was destroyed in 732 BC. Yadin saw the small settlement of Stratum IV as a short-lived Israelite occupation (732-700 BC), following the conquest of Tiglath-pileser III. But a much later dating for Strata V-IV is suggested by a number of Assyrian/Babylonian finds.

A bell-shaped vessel from Stratum VA has been identified by Zorn as a Mesopotamian-style burial coffin. Its occurence in a pre-Assyrian context is clearly problematic. So Zorn:

> *"This building* [3148] *went out of use, according to the excavators, in 732 B.C.E. when the Assyrian destroyed the Citadel. Thus, the piece comes from a context that predates the Assyrian presence at the site. Two alternatives can be proposed to explain its presence near the Stratum VA Citadel. One is that Mesopotamian cultural influence began to permeate Israel before the Assyrian conquest... The other possibility is based upon the Mesopotamian practice of interring the dead inside the settlement, below the floors of buildings. Perhaps the jar's original context is in the later Stratum III... when this area... was part of the even larger Assyrian/ Babylonian Citadel of Stratum III."*[97]

Zorn has identified two additional, smaller, fragments from Strata VB and VA as possible Mesopotamian burial jars, plus a fragment of what seems to be a Mesopotamian "bathtub" coffin from Stratum IV.[98] As Zorn notes, these contexts "are even more problematic than that of the complete burial jar." As "fragments they cannot be in their original contexts." The burial jar fragments

[95] Amiran 1969: 291; Tappy 2001: 315; see most recently Na'aman and Thareani-Sussely 2006, cf. Singer-Avitz 2007.

[96] Yadin 1972: 185, 191.

[97] Zorn 1997: 215; Yadin *et al*. 1961: Pl. CCXXXII, 19.

[98] Zorn 1997: 216-217; Yadin *et al*. 1961: Pl. CCXXIV, 11, Locus 3177; Pl. CCXXX, 24, Locus 3148.

would appear, then to be no later than Stratum VI and the bathtub fragment no later than Stratum V – both pre-Assyrian contexts on the traditional Hazor dating.

The problematic burial vessels need to be considered together with other Mesopotamian finds that predate Stratum IV, allegedly the first Assyrian-period settlement. Stratum VA produced an Assyrian bottle. In Amiran's opinion it is not of local or Transjordanian manufacture, but "duplicated in Assyrian pottery found in Assyria proper," hence she took it as evidence that *"commercial relations between Northern Israel and Assyria began before the conquest of Israel by the Assyrians."*[99] While the imported vessel may, of course, be from trade, other finds of Assyrian-influenced ware suggest that the problem here is really one of chronology. Stratum VA produced three further examples of the Assyrian-influenced carinated bowl known from Stratum VI and dated by Tappy "mainly to the seventh century" (see above).[100]

Two examples of so-called "Assyrian Palace Ware" (APW) are known from Hazor V and IV.[101] Known from a scatter of sites in Cisjordan, Transjordan and Syria, "Assyrian Palace Ware" (APW) was once assumed to be a diagnostic feature of the Assyrian domination, c. 732-630 BC.[102] In the 1950s, when Yadin first assigned Stratum IV to the end of the eighth century, his dating was thus in accord with prevailing opinions on the dating of locally made copies of Assyrian pottery – though his pre-732 BC date for Stratum V was somewhat out of step. But since then there has been a continuing trend towards lowering the date-range for this pottery.[103] Holladay long ago argued that APW did not actually reflect the period of Assyrian domination. Examining the findspots at Nimrud and the other Assyrian capitals, he noted that APW appears in the destruction levels of c. 612-610 BC and continues in the squatter occupations of the sixth century BC. Holladay's conclusion was that *"the floruit of this ware... should be placed in* and following *the last days of the Assyrian Empire."* An even later range is demonstrated by new evidence from Transjordan (Ammon). As Stern remarks, "In the recent excavations at Tell el-'Umeiri, this pottery was found stratigraphically together with Attic pottery and cylinder seals attributed by experts to the late 6th and early 5th centuries BCE." Routledge has shown how: *"Overall, the ceramic evidence points to a general trend which sees the introduction of Mesopotamian forms into the ceramic repertoire of Jordan in the second half of the seventh century*

[99] Yadin *et al*. 1960: Pl. XCVII:11, Pl. CLIX:13; Amiran 1969: 291, 300, Pl. 99:7.

[100] Yadin *et al*. 1960: Pl. LXXX, 25-27; Tappy 2001: 313, n. 442.

[101] Yadin *et al*. 1960: Pl. LXXIV, 3; XCVIII, 44; Holladay 1976: 272, 284; Tappy 2001: 238, n. 55, 313, n. 443.

[102] See e.g. Kenyon in Crowfoot *et al*. 1957: 97-98; Amiran 1969: 291.

[103] See James *et al*. 1991a: 181, 372, n. 62; James 2004: 49; James 2006: 94, n. 10.

BC. Interestingly enough, this means that our evidence for Mesopotamian influence comes after the decline of the Neo-Assyrian empire in the West in the last years of Ashurbanipal." With respect to Palestine generally, Stern states that Assyrian Palace Ware remained *"a constant feature"* into the Persian period, though by then an *"inferior"* product with *"cruder shapes."*[104]

Recent excavation and examination of contexts in northern Syria and Mesopotamia have tended to confirm Holladay's conclusion, in that the peak of popularity of APW in the heartlands of the Assyrian Empire fell in the late seventh to early sixth centuries BC.[105] The key question remains the initial date for this style.[106] A review of the Assyrian contexts, as provided in Hausleiter 1999, suggests that while Assyrian seventh-century contexts are well established, those from the eighth century are less certain.

Ironically, Yadin himself came to prefer dates for APW between 630 and 600 BC, close to those argued by Holladay. However, as noted, Yadin did not follow through the logical consequences.[107] Had he done so, the result would have been havoc for his eighth-century dating of Strata V and IV. *Both* would have to be lowered in date by a century or more, unless we consider the finds intrusive.[108]

Taken together the number of Mesopotamian and Mesopotamian-style finds strongly suggest that Strata VI-V, rather than predating the Assyrian conquest of 732 BC, actually postdate it. As Stratum VI was a substantial city one could envisage it as an Assyrian establishment, occupying (roughly) the last quarter of the eighth century to the beginning of the seventh century BC. In Stratum VA, the second phase of V, the fortifications were reorganised and strengthened.[109] This might conceivably reflect Assyrian reorganisation after the local revolts of the 650s/640s (see above). The destruction of Stratum VA, which was covered in a thick layer of ashes, might then reflect the Scythian invasion ca. 630 BC or a subsequent Neo-Babylonian campaign. Stratum IV would then belong mainly to the Neo-Babylonian period. At present it is usually assumed that Hazor lay completely uninhabited during the Neo-

[104] Holladay 1976: 272); Routledge 1997: 35; Stern 2001: 257, 516.

[105] Lehmann 1998: 19-21; Hausleiter 1999: 18-22, 38-40; van der Veen, in press: §2.1.4b.

[106] Stern *et. al.* 1995: 15.

[107] Yadin 1985: 62; James *et al*. 1991a: 372, n. 62.

[108] Remarking that the APW examples from Hazor "fit well earlier in the typology," Holladay (1976: 272) concluded that they "should presumably be dated to the first half of the seventh century." While his table (Holladay 1976: 270-271) allowed Stratum IV to have ended ca. 680 BC he followed the usual date for the end of Stratum V, 733 BC. As the example reported from Stratum V was found on top of a wall, Holladay considers it "out of bounds for any critical stratigraphy" (pers. comm. Nov. 2007).

[109] Yadin 1972: 187.

Babylonian period, a situation which suits Stern's model of a "Babylonian gap" prevailing not only in Judah but throughout most of Palestine. However the historical grounds for this are highly questionable.[110] Lowering Stratum IV to the Neo-Babylonian period would mean there was no major gap in occupation during this time.

Stratum III

If Stratum IV is Neo-Babylonian, Stratum III must be considerably later than the seventh century as argued by Yadin. On the model argued here it would have to fall in the early Persian period, a date which, felicitously, has already been argued by Stern (see below).

The dating of Stratum III has always been uncertain, due to the scarcity of finds. The only structure assigned to it is a large citadel fort. This appeared to have been "thoroughly cleaned out" by the builders of Stratum II, who continued to use it with minor alterations:

> *"The secondary use of Stratum II, which belongs to the Persian period, proves that the remains of the previous building were still quite visible above ground. It is therefore unlikely that there was such a long interval between the destruction of Citadel III and its reconstruction in the period of Citadel II."*[111]

These circumstances suggest that there may have been no occupational break at all between the two strata, and that III should belong to the early Persian period (late sixth-fifth centuries BC). Nevertheless, Yadin had to introduce a gap, as a fallout of his overall dating of the site. Stratum III was assigned rough parameters, before ca. 400 BC and after ca. 700 BC, the presumed end date for Stratum IV, "with the probabilities nearly even between the seventh and sixth centuries." Yadin leant towards the higher date because of alleged similarities between the plan of the Citadel and the Assyrian buildings 1052 and 1369 from Megiddo Stratum III.[112]

However comparison of the citadel plan to the above-mentioned buildings at Megiddo does not provide a compelling match.[113] As Stern has stressed, Assyrian-style fortifications were being built in Palestine as late as the 4th century: "*These were constructed following the traditional Mesopotamian*

[110] See Blenkinsopp 2002a; 2002b; James 2004: 50-52.

[111] Yadin *et al*. 1958: 54.

[112] Yadin *et al*. 1958: 52-53; Yadin 1972: 194.

[113] See conveniently Kempinski and Reich 1992: 215-216, figs. 12 and 13.

plan, which had penetrated into Palestine during the Assyrian period... The plan consisted of a large open courtyard surrounded by rooms on all sides."[114] Further, Reich has questioned the Assyrian date ascribed to Hazor III, on the basis of comparisons with *post* Assyrian structures, including the houses of the Neo-Babylonian strata at Babylon and Ur.[115] Accordingly Reich offered a dating for the Stratum III citadel to *"the end of the seventh century or beginning of the sixth,"* discussing the possibility that it was built under Babylonian rule. A slightly later, early Persian date (second half of sixth century BC), is preferred by Stern because of the continuity evident between Strata III and II:

> *"... the Stratum II fortress should clearly be dated to the late Persian period... it was not necessary for the Stratum III fortress to be reconstructed by the inhabitants of Stratum II, who only had to clear its debris. The walls of the citadel remained standing, and it is not likely that they would have remained in this state after a long period of abandonment."*[116]

In agreement with Stern, an early Persian-period date for Stratum III is followed here.

Strata II-I

The succeeding Stratum II is securely dated to the late Persian period, by the local pottery and imported finds such as a Tyrian silver stater of ca. 400-332 BC and two Attic lamps of the 4th century.Yadin dated Stratum I, with Hellenistic pottery, to the Maccabaean period (3rd-2nd centuries BC).[117]

Methodological Considerations

It might be remarked that some of the 'anomalous' pottery finds discussed above (Cypriot, Phoenician, Assyrian and Assyrian copies) often involve small quantities, even single examples. To satify the *status quo* (Yadin's dating) it can, of course, be argued that such rare finds are intrusive or badly stratified. For example, Holladay dated the APW find from Hazor V to the seventh century, but did not redate the stratum accordingly, as he questions the reliability of the sherd's stratification (see n. 108 above). Likewise, Zorn suggested that

[114] Stern 2001: 465-466.

[115] Kempinski and Reich 1992: 215-216; cf. Kletter and Zwickel 2006: 170.

[116] Stern 2001: 313.

[117] Yadin 1972: 194-197; Stern 1982: 3.

the Mesopotamian jar burials from "pre-Assyrian" Hazor had been dug down from Stratum III (see above). With respect to the 'anomalously' early finds of torpedo-jars from Hazor, both Holladay and Bikai have raised questions about their context, on the basis of Egyptian and Phoenician chronologies respectively. Bikai argued that:

> *"... until excavation reaches a level of certainty akin to mathematics, arguments based on the occurrence of pottery forms must concentrate on the wide-point of a battleship curve, not on its tails. Three jars from a single locus [Stratum VII] are not good evidence for a cultural shift."*[118]

Given the exigencies of excavation and recording, as well as taphonomy, Bikai's point is well taken. But we should also avoid a 'chest of drawers' approach to pottery development, with styles beginning and ending where our chronological charts give neat horizontal lines. As Gal put it, such an approach *"implies that types of jars appear full blown without either gradual development or growth."*[119] To concentrate *only* on the "wide-point" of the battleship curve also smacks of 'cleaning up' stratigraphy from finds unwanted because of chronological expectations. Indeed, as well as rejecting a "tail" of finds reaching through two strata (VII and VIII – see n. 7 above), Bikai also attempted to chisel away at the *wide-point* of the curve, provided by the over 40 examples of jars reported by Yadin from Stratum VI. She speculated that most of these may come from Stratum V, in order to remain in step with the late eighth century date required by her Tyrian chronology.[120] This was unwise, especially in retrospect as later excavations have recovered further examples from Hazor VI.[121] Bikai's arguments would have been more forceful had she felt able to challenge the dating of Hazor *per se*.

In some other cases, particularly that of the APW, the numbers of 'anomalous' finds may be few, yet, to put this in context, the quantity of local ware in northern Palestine which copies 'Assyrian' prototypes is very small anyway. For such small amounts of pottery, and indeed for large numbers (as per the torpedo jars), *ad hoc* explanations can always be offered, but this is to ignore the *overall pattern* of the evidence. It stretches credulity to explain away *all* the anomalous finds from different cultures (Cypriot, Phoenician and Mesopotamian), including evidence from architecture and burial practices as well as pottery, as being due to intrusion or another archaeologist's poor

[118] Bikai 1985: 72.

[119] Gal 1992a: 73.

[120] Bikai 1978b: 48-49.

[121] Ben-Tor *et al*. 1997: 262, Fig. III: 40, 1, 5.

excavation/recording; especially when *all* these finds argue in the same direction – for a systematically lowered chronology of late Iron Age Hazor.

A second strategy used to explain problem finds has been to challenge the chronology of imported vessels, such as the B-o-R ware. As noted, archaeologists have often turned to the allegedly "fixed" dates for Levantine stratigraphy to offer higher Cypriot dates than those of Gjerstad. However, the same strategy cannot work for Assyrian and Assyrian-influenced pottery, or the Tyre III-II horizon of Phoenician pottery well dated to the late eighth century BC onwards. Gal did challenge the veracity of Bikai's Tyrian assemblages, hence their dating.[122] But here we are back to the familiar argument of challenging the probity of another archaeologist's site and Gal's case was unconvincing in the lack of detail with which it was presented. As it happens, Gal (see note 46 above) has argued for lower dates at Tyre, making Strata IV-I all postdate 700 BC, which would create even greater chronological tension (involving well over a century) between Israelite and Phoenician contexts for the torpedo-storage jars.

A kaleidoscope of *ad hoc* explanations – ranging from 'intrusive' finds, the probity of another excavator's site or recording, or the value of external chronologies and back again – becomes completely unnecessary when we realise that there is a significant *pattern* to a number of interrelated problems. All the indications from external chronologies argue for a radical lowering of Israelite late Iron Age chronology as set by Yadin for Hazor.

A Revised Model for Iron Age II Hazor

When reconstructing the history of Hazor Yadin worked essentially with two 'fixed points' – the alleged "Solomonic" structures of Hazor X, and the assumed destruction-date for Hazor V in 732 BC. The problems with the first assumption are well known, but it has been shown here that the second 'fixed' point is equally unfounded. If we abandon both, then a very different interpretation of the stratigraphy of Hazor can be developed. The revision suggested here would clarify many difficulties in the archaeology of the site. To summarise these, in descending stratigraphic order:

- In agreement with Stern, Stratum III is not late Assyrian, but early Persian, resolving the problem of the continuity with Stratum II and producing the best match with Mesopotamian architectural parallels.

[122] Gal 1992a: 73-74.

- Stratum IV, with "Assyrian Palace Ware" would date to the Neo-Babylonian period.
- The occupational gap at Hazor for the Neo-Babylonian and early Persian periods (in the Yadin chronology) is closed.
- A seventh-century Assyrian empire date for Stratum V means that its imported Assyrian, Assyrian-influenced and "Assyrian Palace Ware" finds, together with the evidence of Mesopotamian-style ceramic coffin burials, no longer have to be explained as pre-Assyrian curiosities or problems.
- Likewise the Assyrian-style bowl and possible Mesopotamian jar burials from Stratum VI would now have a post-732 BC date.
- Redating Hazor VII and VI to the mid and late eighth century, respectively, produces harmony between Israelite and Phoenician chronology, with respect to the long-running debate over the torpedo storage jars at Hazor.
- A slightly lower version of the Bikai dating of Tyre III-II is supported, vital in terms of the dated Egyptian finds (Tyre and Tell el-Mashkuta) which show that the Tyre III horizon must range into the seventh century BC.
- Evidence of an Assyrian destruction at Hazor in 732 BC can be found just as well in the complete destruction of Stratum VII as it can in in Stratum V (Yadin model).
- Lowering Stratum VII to the mid-eighth century BC obviates the problem of the "dense stratigraphy" for Strata X-VII created by the Finkelstein model. Nearly a century more would be available for Strata X-V than the 150 years required by Finkelstein, giving a reasonable average of about 40 years per stratum for these substantial levels (much the same as that on the Yadin/Ben-Tor chronology).
- Redating Hazor X to the mid-ninth century (c. 875-835 BC) removes the conflict with traditional Cypriot chronology regarding the dating of Black-on-Red ware. There is no need for a major upwards revision of Cypriot chronology.
- The ceramic parallels with mid-ninth century Jezreel are sustained.
- The forced attempts to relegate all of Samaria PP 1 and 2 to a pre-Omride settlement are unnecessary.

	YADIN/BEN-TOR		FINKELSTEIN		JAMES *ET AL.*	
	DATE	**HISTORY**	**DATE**	**HISTORY**	**DATE**	**HISTORY**
X	mid-10th	Israelite: Solomonic	early 9th	Israelite: Omrides	875 BC-ca. 835 BC	Israelite: Ahab
IX	end 10th-early 9th	Israelite	first half of 9th	Israelite: Omrides	late 9th	Israelite/Aramaean?
Destr.	885 BC	Ben-Hadad I	ca. 835 BC	Hazael	ca. 800 BC?	Ben-Hadad II *vs.* Jehoahaz/Joash?
VIII	early 9th	Israelite: Ahab	late 9th	Aramaean	early 8th	Israelite: Jeroboam II
VII	later 9th	Israelite	end of 9th	Aramaean	mid 8th	last Israelite
Destr.	late 9th	Aramaean invasion	ca. 800 BC	Joash/ Jeroboam	Assyrian invasion	732 BC, Tiglath-pileser III
VI	early 8th	Israelite: Jeroboam II	early 8th	Israelite: Joash/ Jeroboam	late 8th-early 7th	Assyrian
Destr.	ca. 760 BC	earthquake	ca. 760 BC	earthquake	ca. 675 BC?	earthquake
V	mid 8th	Israelite	mid 8th	Israelite	mid 7th	Assyrian
Destr.	732 BC	Assyrian conquest	732 BC	Assyrian conquest	ca. 630 BC?	Scythian invasion?
IV	end of 8th	unfortified Israelite	end of 8th	unfortified Israelite	late 7th-mid 6th	Neo-Babylonian
III	7th	Assyrian	7th	Assyrian	mid-6th-5th	Early Persian
II	4th	Persian	4th	Persian	4th	Late Persian

Table 2.
Various chronologies proposed for Iron II Hazor.

Hazor, Megiddo and Samaria

How would the model proposed here square with the dating of related sites in Israel? Megiddo IVA is usually thought to have been destroyed by the Assyrians at the same time as Hazor V. Yet the revision suggested here would place the destruction of Hazor VII at much the same time as that of Megiddo IVA.

Holladay once remarked that "the forms typical of [Megiddo] Stratum IVA local are fully in keeping with other 733 B.C. horizon materials (e.g. Hazor V/VA)."[123] Yet judgments here are complicated by the fact that Megiddo IVA is traditionally thought to have been an exceptionally long stratum. Thus Megiddo IVA is usually treated as the temporal equivalent of Hazor Strata VII and VI as well as V. See, for example, Ben-Tor: "Megiddo IVA, the duration of which is much longer, co-existed, at least during part of its life-span, with Hazor VII."[124] Indeed, Yadin *et al*. considered that Megiddo IVA and Hazor VII both fell at the same time (ca. 815 BC to an Aramaean invasion), a suggestion which elicited surprise from Finkelstein.[125] The end of Megiddo IVA has been considerably lowered since Yadin wrote this, the consensus being that it fell to Tiglath-pileser in 732 BC. [126] If Yadin was correct in his relative dating,[127] then it would be logical to also place the fall of Hazor VII (not V) at this time. So does the pottery of terminal Megiddo IVA compare most closely with that of Hazor VII or V?

Finkelstein has stressed that comparison of the ceramics from Megiddo IVA to the Hazor sequence has hitherto remained uncertain, for purely practical reasons:

> *"The excavations of the University of Chicago team... failed to produce a significant assemblage for Stratum IVA. The first significant group of vessels for this phase in the history of Megiddo was uncovered in the course of the renewed excavations in Area H, located in the northern sector of the site to the north and northeast of Assyrian Palace 1369 of Stratum III. Almost 100 complete or almost complete vessels were found in a thick destruction layer in several domestic structures built inside and*

[123] Holladay 1976: 214.

[124] Ben-Tor 2001: 302.

[125] Yadin *et al*. 1958: 23; Finkelstein 2000a: 241; cf. Ben-Tor 2001: 302.

[126] Shiloh 1993; Finkelstein, Ussishkin and Halpern 2000: 322.

[127] Cf. Tappy 2001: 253.

> *against City Wall 325. There can be little doubt that this is the destruction of Megiddo by the Assyrians in the late 8th century B.C.E."*[128]

Analysing this new assemblage from Megiddo IVA, Finkelstein notes that seven significant ceramic types known from the preceding Stratum VA-IVB are now absent, while another six types appear for the first time. He then compares the Megiddo VA-IVB and IVA assemblages to those of Hazor VIII and VII. Two of the types (hemispherical bowls with plain rim and cooking pot with elongated, ridged rim) absent from Megiddo IVA are present in Hazor VIII-VII. *"These are clues that Hazor VIII-VII falls in the middle of the sequence, i.e. between Megiddo VA-IVB and Megiddo IVA."*[129] Finkelstein's main intention here was to show that there may have been a considerable gap in settlement between Megiddo VA-IVB and Megiddo IVA. Whether or not this is the case, his analysis reveals considerable similarities between the repertoire of Hazor VII and that of Megiddo IVA. While there are thirteen typological differences between Megiddo VA-IVB and IVA, Finkelstein noted only *two* between the latter and Hazor VII.

Some of the similarities are of particular interest. "Samaria Ware" was present in Hazor VII but first appears at Megiddo in the destruction deposit of IVA. Torpedo storage jars also first appear at Megiddo in this deposit. As noted above, their occurrence in Hazor VII – dated as early as the late ninth century – is anomalous. Their appearance in the destruction of Megiddo IVA, usually attributed to the Assyrians in the late eighth century BC, is much more in line with the Phoenician dating of such vessels. Indeed, if we consider the torpedo jar as an index type, it would appear that the chronologies of Megiddo and Hazor are presently out of step by a century.

The two differences perceived by Finkelstein between the assemblages of Hazor VII and Megiddo IVA might be accounted for by regional differences between Hazor and the Jezreel Valley,[130] but much would also depend, of course, on exactly when Megiddo fell to the Assyrians. It is usually assumed that it fell to the Assyrians during Tiglath-pileser's 732 BC campaign, but literary evidence is lacking. There is actually no historical evidence that Tiglath-pileser's 732 BC campaign reached Megiddo or that he established the Assyrian province of that name.[131] Forsberg has suggested that the "most

[128] Finkelstein 1999: 63.

[129] Finkelstein 1999: 64.

[130] See Finkelstein 1990: 116; 1996: 183; 1999: 60; Zimhoni 1997: 26.

[131] Forsberg 1995: 23. Becking 1992: 107 surveys the Neo-Assyrian documents mentioning ^{uru}*Ma-gi-du*, and notes that while they demonstrate the existence of the province of Megiddo "their dating is difficult." The earliest secure reference to ^{uru}*Ma-gi-du* might seem to be that in a letter from the reign

likely context" for the construction of Megiddo III would be the Assyrian conquest of Samaria in the late 720s. A similar date for the destruction of Megiddo IVA has been considered by the current excavators:

> *"The most significant vessel for dating the last days of the assemblage of Level H-3* [Stratum IVA] *is the locally made Assyrian bottle* [dimpled goblet] *… If Assyrian forms were imitated in the workshops of the Northern Kingdom already before Tiglath-Pileser III's conquest of the north, there would be no difficulty in dating the destruction of Stratum IVA to 732 B.C.E. Alternatively, if Assyrian forms were imitated only after the annexation of the Galilee and the northern valleys by Tiglath-Pileser III, the destruction of Megiddo IVA must be placed after that event. If the latter option is preferred, the site would have survived the Tiglath-pileser III campaign and would have been destroyed somewhat later, perhaps when the province was reorganized under Sargon II."*[132]

It may well be, then, that Megiddo IVA was not destroyed in 732 BC, but during Sargon's reorganisations c. 720-715 BC, placing the fall of Megiddo somewhat later than that assumed for Hazor (732 BC). Whether one or two decades would be sufficient to allow for the small differences between the assemblages of Megiddo IVA and Hazor VII is a matter for further study. There remains the possibility of an even later context for the fall of Megiddo IVA, for example during the seventh-century rebellions and invasions discussed earlier.

Assessing the relationship between Hazor and Samaria is more difficult, especially as there has never been one, universally accepted, system of dating (or even correlation) for the Building and Pottery periods at Samaria.[133] There has also always been chronological tension between the two sites regarding the dating of shared ceramic types. Mention has already been made of the dispute over the association of Samaria PP 1 and 2 (paralleled by Hazor X) with Samaria BP I and II, a debate exacerbated by disagreements between

of Sargon II (SAA V, No. 291), which appears to record the delivery of bricks there – suggesting a rebuilding programme in the province. However, Becking 1992: 112 offers a second interpretation, in which the lands of the workers referred to (Arpad, Samaria and Megiddo) do not refer to "the provinces in the west, but to groups of laborers deported from the territories mentioned" to work on Sargon's new palace at Dur-Sharruken. Cf. Tappy 2001: 244, n. 99: *"Contrary to popular belief, the Assyrians may not have organized this province as early as the activities of Tiglath-pileser III, but only sometime during the closing years of the eighth century BCE or even the early seventh century. Magiddu certainly existed by the time its governor, Itti-Adad-aninu, became the eponym of the year 679 BCE."*

[132] Ballard *et al.* 2002: 158; Tappy 2001: 164.

[133] For a convenient synopsis see table Appendix B in Tappy 1992: 254.

Yadin and Kenyon over parallels in later assemblages. For example, Yadin compared the pottery of Hazor VIII to that of Samaria PP 3 and that of Hazor VII to Samaria PP 4. However, Kenyon preferred a comparison between Hazor IX and Samaria PP 3 and between Hazor VIII and Samaria PP 4-6. Either set of parallels created a problem: while Yadin dated Hazor VIII and VII to the mid and late ninth century BC, both Kenyon and Wright placed Samaria PP 4 in the early eighth century BC. The solution of Aharoni and Amiran (1958) was to argue for a backdating of PP 4 to the second half of the ninth century, and PP 3 to the time of the Omrides.[134]

Further examination of the early debates re Samaria *vs*. Hazor, already treated by Tappy,[135] would be redundant: partly because new analyses need to be taken on board but, most importantly, because a rash of new studies have argued that all the traditional schemes for dating Samaria (with the exception of Kenyon, for some phases) are too high.

As discussed above (Strata X-IX), Franklin now accepts that Samaria BP II should not be attributed to the reign of Ahab (875/4-853 BC) but to the early eighth century BC, hence the time of Jeroboam II (791/0-750/49 BC). But she has yet to follow through the consequences for later strata. PP 3, which overlies the BP II structures, must represent occupational debris accumulated during the long reign of Jeroboam. This matches well with Tyrian chronology. 'Samaria Bowls B' begin in PP 3, and are known as Fine Ware plates (class 2.1) at Tyre V-IV, dating no earlier (Bikai) than ca. 800/760-750/740 BC.[136]

Given this, the BP III structures (above PP 3) can reasonably placed in the mid-eighth century BC and would represent the last Israelite city, rather than BP V-VI as thought by Kenyon, Wright *et al*.[137] The overlying PP 4 pottery deposits (which include a torpedo-storage jar fragment, no earlier than the mid- eighth century[138]) would then end ca. 720 BC with the Assyrian conquest. They are followed by the extensive rebuilding of Samaria in BP IV. This would seem to reflect the work of the Assyrian conqueror Sargon II, after 720 BC, who claimed that he *"rebuilt [Samaria] better than it was before..."*[139] BP V/VI would then belong to a later phase of Assyrian activity at the site, during the seventh century BC.

[134] Yadin *et al*. 1958: 14; 1960: 16-31; Kenyon 1964: 147-148; Aharoni and Amiran 1958.

[135] Tappy 1992: 4-8.

[136] See James *et al*. 1998: 31.

[137] See James *et al*. 1991a: 186-187.

[138] Tappy 2001: 161-164.

[139] See James *et al*. 1991a: 186.

Samaria Building Periods	*Samaria Pottery*
	1 ending ca. 880 BC[140]
I. Omrides (Omri-Ahab-Jehu; 886/5-815/4)	
	2 ending ca. 790 BC
II. Jeroboam (790-750/49)	
	3 ending ca. 750 BC
III. Last Israelite	
	4 ending ca. 722 BC
IV. First Assyrian (722-675? BC)	
	5 ending ca. 675? BC
V/VI. Second Assyrian (675? - 630 BC)	
	6 ending ca. 630? BC
VII. Post-Assyrian & Babylonian	
	7 ending ca. 539 BC
VIII. Persian	
	8 ending ca. 323 BC
IX. Hellenistic	
	9 ending ca. 63 BC

Table 3.
Simplified correlation of Samaria Building and Pottery Periods – with suggested new dates, slightly amended and corrected from James *et al.* 1991a: 183-188. NB Kenyon adhered to her system of dating Building Periods by the latest pottery found beneath them for BP I-IV, but the system began to come adrift with BP V/VI due to the lack of clearly defined deposits beneath floors (see Forsberg 1995: 19-20; Tappy 2001: 178, 223). Tappy's analysis of the few vessels which can be safely attributed to PP 5 is followed here. With PP 6 we are dealing with pottery which, contrary to Kenyon's system for earlier periods, is found above the building floors.

The later part of our Samaria reconstruction has been amply vindicated by the studies of Forsberg and Tappy. Forsberg has argued that the Samaria PP 5-6 pottery assemblages (accompanying BP V-VI) do not belong to the last Israelite periods (third quarter of eighth century BC) but to the seventh century BC. Though Kenyon herself established the close resemblance between the PP 5-6 assemblages and those of Megiddo III, she failed to synchronise them

[140] NB: Chapman (2007; in prep.) considers the possibility that the original context of most of the PP1 pottery (before it was deposited in later constructions) was, *contra* Kenyon's apparent position, from off the floors of BP I. In that case the date for PP1 pottery would have to be lowered and should largely postdate ca. 880 BC.

adequately. As Forsberg points out, it is now generally accepted that Megiddo III was an Assyrian foundation, belonging largely to the seventh century BC, leaving the Samaria dates far too high: *"Whereas the terminal date of the pottery on the Samaria side of the equation is set by Kenyon at 722 BC, the pottery of Megiddo Stratum III is later, probably not earlier than the first quarter of the 7th century."* Further, Forsberg has stressed that as we are largely dealing here with pottery from the end of Megiddo III (usually dated to c. 650 BC), the date for the Samaria PP 5-6 material could well be weighted towards the middle of the seventh century (with knock-on effects for Samaria PP 7). His conclusion was that the *"end of habitation in the Period V building should probably be dated to about the third quarter of the 7th century, at the end or close to the end of the period of Assyrian domination of Samaria"* [141], in agreement with our suggestion that BP V/VI represent the final Assyrian phase.[142]

While Tappy feels that Forsberg has *"overextended its [Megiddo III's] overall range too far into the seventh century BCE,"*[143] he is in broad agreement. His analysis has shown that *"the few ceramic fragments published from loci beneath the BP V house floors seem to postdate the Israelite-Assyrian transition around 722/721 BCE)."* Regarding the date of this small PP 5 group, Tappy cautiously offers *"no earlier than the very late eighth and, perhaps mainly, the first half of the seventh centuries BCE,"* with a consequent date for PP 6 in the seventh century.[144] Fine tuning aside, a seventh- (or even late eighth-) century date for PP 5 completely undermines the traditional understanding – shared by Kenyon, Wright and others – that it was Samaria BP VI that was captured by

[141] Forsberg 1995: 24, 50.

[142] James *et al*. 1991a: 186-187.

[143] Tappy 2001: 245, which places the *floruit* of Megiddo III largely in the last two decades of the eighth century BC. His caution seems excessive, compared to the latest conclusions of the current excavators, unavailable to Tappy when he completed his masterwork on Samaria. From archaeological reasoning, based on the apparent orientation of the town plan of Megiddo III around the latest in a series of palaces, Finkelstein and Ussishkin (in Finkelstein *et al*. 2000: 602) argue that *"the well-planned city of Stratum III can hardly be dated early in the period of Assyrian rule at Megiddo. It should be mentioned in this connection that the name of the governor of Megiddo serves as the eponym in 679 B.C.E., in the days of Esarhaddon, which, if anything, would suggest a later floruit for Stratum III, rather than an earlier."* In close agreement, Halpern (in Finkelstein *et al*. 2000: 568-570) explores the idea that the "full repopulation of Megiddo that is evident in Stratum III" might be related to the western policies of Esarhaddon after c. 685 BC. Halpern allows a much later date than Tappy for the general floruit of the stratum, considers that the 609 BC incident at Megiddo involving Josiah was "toward the end of the life of Stratum III" and recommends lowering the seventh-century date for Stratum II proposed by the Oriental Institute excavators, from the Assyrian to Babylonian or even Persian period. What remains surprising is that the excavators of Megiddo have not considered the knock-on effects of such proposals on the dating of late Iron Age Samaria and Hazor.

[144] Tappy 2001: 226, 224.

the Assyrians. Though Tappy avoided the logic, Samaria BP IV, underlying the minimal PP 5 deposits (which he agrees are *post*-Assyrian conquest in date), cannot be placed far earlier in time and can reasonably be seen as the city (re) built by Sargon II. Here Tappy was constrained partly by comparisons from Hazor which, as traditionally dated, support a high chronology for Samaria I-IV.

The revised dates for the Building and Pottery Periods, as argued in James *et al*. 1991a and the present paper, offer the (hitherto elusive) possibility of a harmonious picture between Samaria and Hazor. Recent studies show that Yadin, rather than Kenyon, was correct in his Samaria correlation for Hazor VIII. Wightman correlated the bulk of PP 3 with Hazor VIII.Many close parallels illustrate this.[145] The correlation supports the suggested dating of both these pottery periods to the renaissance of Israel under Jeroboam II. The succeeding phases, Hazor VII and Samaria PP 4 correlate well, e.g. Gal's cooking pot type 6.5 first appears in Hazor VII and at Samaria in PP 4. PP 5-7 would then broadly correlate with Hazor VI-IV. For example Gal notes that his Galilean type Jug 3.3 (a ridged decanter) first occurs in Hazor VA, becoming common in Stratum IV. It is also known from Megiddo III-I and Samaria PP 5-6. Likewise Gal's Bowl type 5.7 is known from Hazor VA-IV and Samaria PP 5-6.[146] Of Phoenician origin, this bowl is most common in Tel Keisan Stratum 4, now dated to the seventh century BC.[147] Interestingly, it is only the examples from Samaria and Hazor – as conventionally dated – that allow Gal to remark that this vessel was "widespread during the eighth-sixth centuries B.C.E." On the Yadin/Ben-Tor model Hazor Va and IV are dated to the mid and late eighth centuries respectively, out of step with the generally accepted seventh-century date for Megiddo III and Tel Keisan and the new dates offered here for Samaria PP 5-7.

A full exposition of the case for a revised dating of the Iron Age strata at Samaria is beyond the present study. While more systematic analyses are clearly needed (particularly of the extremely difficult site of Samaria), it would appear, *prima facie*, that there are no obstacles from the ceramic parallels at Megiddo and Samaria to the lowering of Hazor Strata VII-V argued here. Rather, a downward shift of the latter would seem to bring the pottery of these strata in better step with that of Megiddo and Samaria.

[145] Wightman 1990: 12; Tappy 1992: 161, 179, 189, 192, 202.

[146] Gal 1992a: 78, 75, 76-77.

[147] Humbert 1993: 866-867.

Concluding Remarks

There are many reasons to question the idea that the destruction of Hazor V in 732 BC provides a firm "anchor" in the present chronological debates. The dating of the end of Stratum V to the Assyrian conquest is merely an assertion which has become a given, used to reconstruct the dates of preceding and following layers but never properly argued out in its own right – or, for that matter, critically analysed. It has also given rise to numerous anomalies in the dating of Hazor's Iron IIA-IIB strata, concerning independently dated imports (from Cyprus, Phoenicia and Mesopotamia), which have previously been treated on an unsatisfactory *ad hoc* basis.

While the Tel Aviv school has now begun to address the problem of Iron IIA chronology originally raised by Kenyon *et al*. from their excavations at Samaria, similar uncertainties in dating extend well into the succeeding Iron IIB and IIC periods.[148] Advocates of the Tel Aviv version of a 'Low Chronology' are working within an unnecessary straitjacket, by adhering to Yadin's dating of Hazor VII-V. This has led them, while lowering Iron IIA largely into the ninth century, to conclude that this important phase should be shortened from 200 to 125 years.[149] If we abandon the "anchor" of 732 BC for the end of Hazor V, and lower Hazor VII into the mid-eighth century, then Iron IIA might be allowed a slightly longer duration.[150] The related problem, of "stratigraphic congestion" between strata X-V is also relieved, and a major obstacle is removed to lowering Iron IIA from the 10th to the 9th century BC. The wider ramifications – such as those for our understanding of the archaeology of the United Monarchy – will have to be discussed elsewhere.

[148] James *et al*. 1987: 58-64; James *et al*. 1991a: 170-182; James *et al.* 1998: 30-32; James 2004: 48; James 2005a; James 2006; James 2007; James in press.

[149] Fantalkin and Finkelstein 2006: 22-24, 32-33.

[150] Fantalkin and Finkelstein 2006: 23-24 note that Hazor VIII-VII (Tel Aviv dating: second half of ninth century BC) still feature some Iron IIA ceramic types, while VI (Tel Aviv: early eighth century BC) features Iron IIB pottery. This brings them to the conclusion that the Iron IIA/IIB transition fell ca. 800 BC, earlier than the ca. 760 BC currently suggested (Herzog and Singer-Avitz 2004). They conclude that Iron IIA should be shortened from the conventional 200 years (ca. 1000-800 BC – see e.g. Ben-Tor 1992: 2, Table 1.1), also assumed by Herzog and Singer-Avitz, to something like 125 years. While dating the Iron IIA/B transition is beyond the scope of the present study, as a fallout of the arguments presented here, and assuming the definitions of Fantalkin and Finkelstein, Iron IIA forms (in Hazor VIII and VII) continued until 732 BC, with Iron IIB in Hazor VI, post-Assyrian conquest. This might suggest that the changes in pottery from Iron IIA to IIB styles were a result of the Assyrian conquests, ca. 730-700 BC. If Iron IIA developed at the time of the rise of the Omride dynasty (ca. 875 BC), this would allow a duration for Iron IIA of ca. 145-175 years.

APPENDIX: Ayyelet ha-Shaḥar

Mention needs to be made of the problematic chronology of Ayyelet ha-Shaḥar, a late Iron Age settlement on the plain near Hazor. In 1950 Guy and Dothan investigated the remains, identifying a palace containing largely Persian-period pottery.[151] However, subsequent studies argued from parallels at Khorsabad and Arslan Tash that the prototype for the palace-plan is a Neo-Assyrian type of the eighth century BC.[152] Thus it has been argued that the palace was the residence of a Neo-Assyrian governor, built after the conquest of Tiglath-pileser III in 732 BC, which was cleaned out and reused during the Persian period. This conclusion has been accepted by Ben-Tor: *"In all likelihood the structure which Guy associated with the Persian period is an Assyrian palace contemporary with stratum III of the upper city. It is similar to the strata III-II citadel of area B in the upper city and, like the latter, continued in use in the Persian period."*[153]

As argued here (and in agreement with Stern), the Stratum III citadel most likely belongs not to the Assyrian but to the early Persian period. Hence the similarities with Ayyelet ha-Shaḥar detected by Ben-Tor would raise the question whether a Persian dating might be correct, after all. The finds have recently been published and analysed by Kletter and Zwickel who conclude: "Most of the pottery from '*Ayyelet ha-Šaḥar*, and the types that are precisely dated, belong to the Persian period." They offer a new reconstruction of the building, which they conclude *"fits better Babylonian architecture, not Neo-Assyrian,"* with the overall conclusion that it "*combines Neo-Assyrian features* terre pisée *walls, pebble-plaster floor, a reception suite) with Neo-Babylonian ones (Pits 1 – 2, the general plan following the new reconstruction)."* Regarding the date they note:

> *"It is important not to confuse architectural tradition and date. The Neo-Babylonian architectural features do not imply a late date. They appeared in Babylonian architectural tradition since the 8th century B.C.E. at the latest, that is, they are not necessarily later than the period of Assyrian rule in the west. The Neo-Assyrian features indicate that the 'Ayyelet ha-Šaḥar building was established most probably by the Assyrians in the late 8th or early 7th centuries B.C.E."*[154]

[151] See Yeivin 1960: 29; Kletter and Zwickel 2006: 151-152.

[152] Reich 1975; Lipschitz 1990.

[153] Ben-Tor 1993: 605.

[154] Kletter and Zwickel 2006: 169, 175.

Yet regarding the Neo-Assyrian comparanda, as Stern has remarked Assyrian-style fortifications continued to be constructed in Palestine as late as the 4th century[155]; one wonders whether Assyrian palace-plans were similarly copied over this period. Further, as John Bimson has pointed out to me (pers. comm. 2004), it might seem unlikely that such a structure (made of packed mud with some traces of lime-plastering) would have survived from the Assyrian to Persian periods in one of the wetter parts of Israel unless occupied and maintained. A number of conclusions remain possible, but at present it would seem most likely that the palace was built for a Neo-Babylonian regional official and that it remained occupied into Persian times. In this case it may provide further evidence for continuity in settlement at Hazor during the Neo-Babylonian period.

ACKNOWLEDGEMENTS

Completion of the research and writing of this paper would have been impossible without the support of the Mainwaring Archive Foundation, to whom I am immensely grateful. Otherwise my sincere thanks go to Nikos Kokkinos, for many years of mutual research and discussion on Levantine, Cypriot and Aegean chronology. The model in the present paper results from a series of notes I drew up in 1989, when we were working on the "biblical archaeology" chapter for *Centuries of Darkness* (1991). I am also extremely grateful to another long-standing colleague, John Bimson, for providing feedback and encouragement and, over the years, for drawing my attention to some key references; to John S. Holladay for patiently answering questions; and to Robert M. Porter, Rupert Chapman, Peter van der Veen and an anonymous *Antiguo Oriente* reader for reading drafts and providing comments and corrections.

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vistos como pioneros pueden ser también de mucha influencia. Este trabajo examina las razones para el crecimiento del "discurso del sacerdote impío" en Egiptología y su aplicación a los estudios de El Amarna. En un intento para frenar la influencia permanente de este discurso, se realizan algunas sugerencias alternativas en relación con dos áreas de aplicación: 1) las reformas religiosas de Ajenatón y el cambio de capital; 2) la persecución del nombre de Ajenatón y la reacción a su reinado.

Palabras Clave: Amarna – Ajenatón – Egiptología – religión

The Problem

The priesthood has been a main focus in the study of Ancient Egypt since at least the Classical Period. Depictions of this group have ranged from the wise, secretive initiates of the classical literature, to the deviously cunning and politically manipulative power mongers represented in many works of the modern era. This paper aims to examine the origins of the "wicked priest" theory in Egyptology and, more specifically, its application to the Amarna period. Akhenaten's battle with the Amun priesthood has now been a central tenet of most depictions of Amarna Egypt for over a century. In more recent times some have begun to question the evidentiary basis for the existence of such a quarrel.[1] I am in sympathy with this more critical approach and will argue that theories proposing this almighty conflict between Akhenaten and the Amun priesthood owe much more to the historical, religious, political and personal contexts of the scholars who contributed to the "wicked priest" discourse in Egyptology than they do to the historical sources available. Though there are certainly sharp ruptures or changes in discourses about Egyptian priests, the "wicked priest" discourse seems to develop, with earlier versions impacting upon later.[2] Classical period depictions had an enormous impact upon the portrayals of the Renaissance which in turn set the framework of understanding which was reshaped under the impact of Deism, its anti-clericalism and criticisms of institutionalised religion. To a great extent these frameworks of understanding have had a lingering impact on Egyptology even after decipherment, with similar arguments, ever shaped by context, being supported by a new body of evidence. The Romanticism of the early 19th Century and then the increasing political focus of history which coincided with the growth of nation states later that century both influenced the picture portrayed. With regards to Egyptology, and especially Amarna studies, all of

[1] Montserrat 2000: 36.

[2] Compare Foucault's notion of discontinuity between disparate discourses.