

Review Article

The Assyrian, Babylonian and Persian Periods in Palestine

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Ephraim Stern, *Archaeology of the Land of the Bible. Vol. II: The Assyrian, Babylonian and Persian Periods 732–332 B.C.E.* Doubleday: New York/London, 2001.

This is the second volume in Doubleday's series on the 'Archaeology of the Land of the Bible'. As one would expect from the Anchor Bible Reference Library, the design, illustrations and paper, are all of high quality.¹ The selection of authors has also been exemplary. Amihai Mazar coped brilliantly with the unenviable task of summarizing in Volume I the archaeological record from 10,000 to 586 BC!. Here Ephraim Stern brings to bear his long experience of the field to summarise a more comfortable 400 years (732–332 BC). A more equal division of labour in a larger series of volumes would clearly have been preferable. There is also an awkward overlap (732–586 BC) between the two published volumes. Unfortunately this constrained Stern to omit discussion of 'the structure and history of Jerusalem in the 8th to 6th centuries BC (particularly the surrounding tombs) to avoid repeating what has already been published' (p. xv). Nevertheless through his treatment of the wider picture of Judahite archaeology, Stern manages to include a brief treatment of Jerusalem – absolutely essential for understanding these periods. And any deficiencies at the editorial stage are outweighed by the pleasure of sharing Stern's synthesis, conveyed in writing of admirable clarity.

Stern, amongst many other achievements, is of course well known for his seminal study of *The Material Culture of the Land of the Bible in the Persian Period 538–332 B.C.E.* (1982). This is not replaced or superseded by the present semi-popular volume. Nevertheless, both in text and bibliography, it provides a vital and most welcome update. Shepherding a vast quantity of data into an easily digestible format (with subsections on all aspects of material culture for the three periods covered), Stern's survey is an extraordinary achievement – especially as most of the timespan is, in terms of dating the archaeology, a rather grey area in the southern Levant. Surprisingly, the relative dating of artefacts is often far easier during the

Late Bronze Age – when, whatever the absolute dates, pharaonic finds from the eighteenth–twentieth dynasties provide us with a firm internal chronology for a period of some four centuries – than it is during the Iron Age, for any period up to late Persian times. Witness for example the continuing controversy over the tenth versus ninth century BC dating of Iron IIA strata. Yet, a fact not yet appreciated by Israel Finkelstein and the ‘Tel Aviv school’, who have now begun to address the Iron IIA problem,² is that similar dating problems run into the succeeding periods of the Iron IIB, IIC and Persian – those covered by Stern’s survey. To Stern’s credit, his survey often highlights rather than glosses over these problems.

The uncertainties begin with the Assyrian period. The Assyrian Eponym canon – universally agreed to be sound back to *c.* 910 (\pm 1) BC – has provided the touchstone for all attempts to resolve the ‘mysterious numbers of the Hebrew kings’ (as Thiele called them) into a workable dating scheme for the divided monarchy of Judah and Israel. It is Assyrian chronology that allows us, for example, to date the presence of Ahab at the battle of Karkar to 853 BC. By the same token, we would expect firmly datable Assyrian artefacts and inscriptions to provide the acid test of any archaeological chronology in Palestine. Stern (pp. 14–18) provides a handy (and the most up-to-date) list of the Assyrian texts from Palestine.³ He describes and illustrates the stela fragments of Sargon II from Ashdod and Samaria, but unfortunately omits the important fact that neither was found in its original context. Stern mentions the tablets from Gezer, Hadid and Keisan but, again, does not discuss their chronological significance. The Gezer tablets were found during R. A. S. Macalister’s excavations in the first decade of the twentieth century, too early for their contexts to be judged in modern terms. In the case of Tell Keisan, the context of its Assyrian tablets led to an acrimonious dispute between the director of the site and the site-supervisor of the area in which they were found.⁴ Most potentially useful, chronologically, are those from Hadid, where two tablets precisely dated (by their eponyms) to 698/97 and 664/63 were discovered. Only preliminary reports of the contexts have been published. Both are described as ‘Iron II’, with a little more detail for the first tablet, which was found with pottery ‘dated by the excavator to the second half of the 8th century BCE’ (Na’aman and Zadok 2000: 159). The awaited full publication of the contexts may (hopefully) tell us whether the seventh-century Assyrian tablets confirm the local dating of the associated pottery to the second half of the eighth century BC, or whether they would be better accommodated by nudging the local dates down by a few decades.

Such a possibility has been raised by the discovery of the famous temple inscription at Ekron. This provides the best available fix for Iron IIC chronology (at least with respect to Philistia), since the builder of the temple, Ikausu, is known from Assyrian records. Here unfortunately Stern (p. 120) uncritically repeats the excavators in concluding that ‘Temple complex 650 was erected in the first quarter of the 7th century BC.’ The Assyrian references to Ikausu (673 and 667 BC) fall in the *second* rather than the first quarter of the 7th century, indicating a later construction date for the temple and the beginning of Stratum IC (see James, in press). Again following the excavators, Stern refers to the architecture of the

aforementioned temple as influenced by ‘Neo-Assyrian royal palaces and temples’. Yet it is hard to see what, precisely, is Neo-Assyrian about the building⁵ and the excavators have actually been more forthcoming about aspects suggesting Phoenician influence (Gitin 2003: 59*, n. 3).

The difficulties extend to other Assyrian material (seals, Mesopotamian pottery imports and local copies), where the contexts are often uncertain or the identification equivocal. With respect to pottery Holladay (1976) long ago argued that ‘Assyrian Palace Ware’, conventionally dated to the eighth–early seventh centuries, did not reflect the period of Assyrian domination. Using Mesopotamian contexts, Holladay argued that the *floruit* of the ware should be placed in and following the last days of the Assyrian empire (late seventh–early sixth centuries BC). This now finds strong support from Ammonite archaeology: ‘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.’ (Stern, p. 257). With respect to Palestine generally, Stern states that Assyrian Palace Ware remained ‘a constant feature’ during the Persian period, though by then an ‘inferior’ product with ‘cruder shapes’ (p. 516).

In the second ‘book’ of the volume, the acute difficulty of identifying the material culture of the Neo-Babylonian domination looms large. Stern’s section here on ‘Pottery Vessels’ begins with the frank statement: ‘There are almost no pottery vessels from Palestine that may be exclusively attributed to the Babylonian period.’ (p. 342) Yet the problem does not seem to be due just to its relatively short duration (c. 604–539 BC). The difficulty extends well into the Persian period, down to c. 450 BC when we begin to reach a firmer horizon of material, identifiable largely through imported Greek pottery and Aramaic *ostraca*. Stern (p. 344) makes these salutary remarks:

... distinguishing among tombs dated to the late Ammonite kingdom, the Babylonian period, and those of the early Persian period is difficult on the basis of local pottery alone, and it is almost impossible without the help of the Greek material. For example, a Persian-period tomb from Shechem, recently published, included all the typical vessels of the Babylonian period – clay alabastra, sack-shaped decanters, a rich assemblage of metal artifacts, one of those shaped like the closed Babylonian lamp – but the date was securely fixed by the presence of three Attic *lekythoi* (bottles), dated to the beginning of the 5th century BCE...

Were it not for the presence of the Greek imports these tombs may well have been dated to the Babylonian (or even Assyrian) period. Surprisingly such discoveries have not forced a rethink of the dating for Iron IIC, but merely an extension. While it was once thought that Iron IIC came to a dramatic close in 587/6 BC, in recent years its terminal date has been lowered by degrees to 530/520 BC (Barkay 1993; cf. Lipschits 2003: 349). In Transjordan it now seems the consensus that the local Iron IIC pottery continued well into the Persian period (MacDonald 1994: 71–72). As one can see from Stern’s own remarks, it is now increasingly clear that Iron IIC forms continued into the fifth century, and not just in Transjordan.⁶

It is not only the pottery that creates dating problems. At Ashkelon a *favissa* was uncovered containing a large number of Egyptian-style bronzes, originally dated to

the Persian period (fourth century BC). Yet, as Stern reports, the current excavators have found ‘at least one’ such bronze in the stratum sealed by a destruction level they date to 604 BC, and have redated the *favissa* accordingly. Nevertheless, the Persian comparanda remain: ‘At Ashkelon, some seven imported Egyptian bronze *situlae* were recovered in the same late 7th century BCE stratum... Two almost identical bronze *situlae* bear Phoenician inscriptions and are dated to the Persian period.’ (pp. 498–500) With this confused picture, Stern had little choice than to present the same finds twice – once for the Assyrian period (p. 124), and then again in the Persian chapter (pp. 498–501).

Stern provides many such illustrations and fully admits the fluidity of the pottery chronology between the seventh and fifth centuries. Nevertheless, he tends to use as a dating fulcrum his firm belief in a ‘Babylonian gap’. In a recent article he stressed the traditional view that Palestine was very poorly settled during the time of the Neo-Babylonian Empire: ‘While the Assyrians left a clear imprint of their presence in Palestine, there is a strange gap after the Babylonian destruction. Call it an archaeological gap, if you wish.’ (Stern 2000: 46). This gap, according to Stern, can be discerned not only in the kingdom of Judah, but at sites in Philistia, the Arabah, the Jordan Valley, the area of the erstwhile northern kingdom and even southern Phoenicia. In a nutshell, Stern argues that outside of northern Judah (Benjamin) and Ammon, ‘the rest of Palestine was largely barren’ (Stern 2000: 51). To many such views are extreme (see e.g. Barstad 2003: esp. 3, n. 1). Biblical scholar Joseph Blenkinsopp took issue with Stern’s position and a heated exchange ensued. Stern made a tactical error in resorting to the argument that Blenkinsopp is not an archaeologist, a point which the latter easily deflected: ‘I systematically compared his [Stern’s] results with those of authors in the two most recent encyclopedias of archaeology in the region, one of which Stern himself edited.... In most of the cases cited, their conclusions disagreed with those of Professor Stern.’ (Blenkinsopp 2002: 13).

In the present volume Stern expounds more fully his model for the ‘Babylonian gap’:

A clear distinction must be made between the influence of Babylonian occupation on the country and its material culture and that of their Assyrian predecessors and the Persians after them. Although the Babylonians created a new administrative organisation, different from that of their predecessors, this did not leave any clear traces in the country’s archaeological record. (p. 308)

Stern argues that this ‘unique situation’ can be explained by two factors. The ‘more important’ factor concerns the character of the Babylonian domination:

Unlike the previous Assyrian imperial system, which strived to create a network of semi-independent provinces, the Babylonian concept was quite different: their entire focus was on the welfare of the city of Babylon and its immediate surroundings, while the periphery was largely neglected, with negative consequences for those living in those territories. (p. 303)

This is a rather sweeping statement. As to the ‘entire focus’ being on Babylon, Stern himself naturally mentions the fact that Nabonidus spent some ten years in Teima in the Arabian desert (p. 307). His sojourn there is usually thought to have been connected to Babylonian attempts to control Arabian trade (Kuhrt 1990: 131–132). And even though we have few relevant economic texts, they show a far-flung trading network reaching to Egypt and Ionia. With the latter, relations ‘seem more intimate and direct than in the preceding Neo-Assyrian period’ (Kuhrt 2002: 21). How was such long-distance trade conducted if ‘the periphery was largely neglected’? While the depredations of the Babylonians were in some cases more severe than those of the Assyrians, one wonders whether they would have truly left their western empire a virtual economic desert, a situation which would have hardly benefited the capital.

Stern misunderstands the important difference in the amount and character of evidence that survives from the two Mesopotamian empires. The Babylonian kings did not leave the kind of detailed ‘annals’ familiar from their predecessors Sennacherib, Esarhaddon and Assurbanipal, whose military records, spattered on sculpted reliefs to clay cylinders, often survive in numerous editions. For Neo-Babylonian campaigns we are largely thrown back on the laconic entries in the Babylonian chronicle, often no more than a sentence each. Here we have chronicle entries for some 20 years, *less than thirty percent* of the empire’s duration. Viewed this way one must question Stern’s statement that: ‘The Babylonians waged far fewer military campaigns for the domination of Palestine than the Assyrians, and the number of the written sources at our disposal describing these is likewise much smaller.’ (p. 303) Here the second part of the sentence effectively (and ironically) demolishes the impact of the first. Unfortunately Stern’s *idée fixe* of Babylonian emperors neglectful of the ‘periphery’ leads him into exaggeration: ‘The Babylonian kings who came to power after Nebuchadnezzar, Amel Marduk (562–560 BCE) and Nergal Shar Usur (560–556 BCE), reigned for too short a time to influence the remote regions of the empire.’ How can this be said? We have no chronicle entries for Amel Marduk,⁷ while a fragment describes a campaign of the latter (a.k.a. Neriglissar) reaching as far as western Cilicia and the borders of ‘Ludu’ (Lydia). If such a campaign does not reflect a king with time or interest enough to ‘influence the remote regions of the empire’ it is hard to imagine what might.

Of course Stern is not the only archaeologist to make the tacit assumption that the absence of Babylonian records equals Babylonian inactivity in the west. Assyriologists, however, are acutely aware that this assumption is mistaken and assume that there were many more Neo-Babylonian campaigns, for which the records are simply lacking (see e.g. Sack 2003: 223). At the same time, there is an increasing understanding that the Neo-Babylonian empire was truly the heir to the Assyrian, both in terms of its geographical scope and administration.⁸ In short, Stern’s attempt to draw *such* a sharp distinction between the character of the Assyrian and Babylonian administrations is overstated.

Stern’s other factor explaining the putative lack of Babylonian evidence in Palestine is that ‘it is almost impossible to determine if a certain artefact with

Babylonian parallels should be dated to the late Assyrian period, to the Babylonian period, or even to the early Persian period.’ (p. 308) But there’s the rub. When Stern confidently contrasts the number of ‘Assyrian’ structures and fortresses with a Babylonian lack of them (p. 349), how sure can we be that we are not looking at a false picture? Rather than being archaeological, could the ‘gap’ perceived by Stern be the artefact of an incorrect and attenuated chronology? In which material from the sixth century has been incorrectly assigned to the seventh?

In lieu of the smoking gun which a well-stratified Assyrian or Babylonian inscription might provide, some light on the chronology might be expected from local epigraphic material. Stern devotes generous space to this and it is good to see as much weight given to Ammon, Moab and Edom as the Hebrew kingdoms. With the latter, as any reader of popular magazines on biblical archaeology will now be aware, almost every king, prince and even minor official of Judah now has his own seal, miraculously supplied by antiquities dealers. (Any gaps in the lists can be expected to be filled in the coming years!) The extent to which the field has been poisoned by such material (either illegally excavated or suspect) is surely one of the most tragic developments in modern Near Eastern archaeology. The seething remarks of Joffe (2003) on this matter are required reading. Here, unfortunately, Stern fails to warn the reader of the awful dangers of using unprovenanced material. For example he cites an intriguing bulla which bears a regnal year (arguably that of Josiah) with the tag *lmlk*. Stern presents it as an ‘ending’ to the debate on whether the *lmlk* system of taxation continued into the seventh century (p. 175) – yet a stronger caveat than ‘said to originate at Eltolad’ is needed for a piece that only surfaced through the antiquities market (Avigad 1990: 262). Again, with respect to the fifty sealings discovered by Shiloh in the ‘House of Bullae’ of the City of David excavations, Stern states: ‘It was possible to date the bullae accurately, which included the names of Gemariah son of Shaphan and Berachiah son of Neriah the scribe, contemporaries of Jeremiah...’ (p. 323). While the first was from the excavations, the second came from a separate, unprovenanced collection. While this does not mean that it is a forgery, the names on it (compared with those of a character in *Jeremiah*) must necessarily be suspended from any discussion of palaeographic dating. Of the welter of biblical names found on epigraphic material that might be diagnostic enough to help date Iron IIC, the case essentially boils down to two ‘matches’ from the House of Bullae (pp. 182–184) and one with a bulla from Lachish II (p. 180).⁹ These presently provide some of the strongest evidence for dating the end of the Lachish II horizon of pottery to 587/6 BC, but whether they are enough to sustain it is a matter for debate.

Of the non-Hebrew epigraphic material reviewed by Stern, intriguing evidence for the existence of *lmlk*-stamp iconography much later than *c.* 700 BC (and close to the original Albrightian dating) comes from the (excavated) seal of Milkomur, servant of the Ammonite king Baalis who reigned about 585 BC (p. 241); it bears the same four-winged scarab beetle as many of the Judahite royal stamps. Further discussion of *lmlk* chronology is beyond the scope of this review, but it is interesting that Stern accepts continuity in tradition of royal stamps from the time of Hezekiah

down to Persian times (p. 175). This is difficult to envisage given the large gaps in time (on the present chronology) between *lmlk* and Judahite rosette production (approaching a century), and between the latter and the rosettes of the Persian period (another half century). The problem is aggravated by Stern's 'Babylonian gap', during which the only Hebrew stamps he allows are those bearing the name Mozah (NB: no rosettes), produced in the territory of Benjamin (pp. 335–338). In the Persian period we encounter familiar difficulties, with the Hebrew Yehud (*yhwd*) stamps: '... the various chronologies proposed extend over a very long period: from the 6th to the 2nd centuries BCE.' (p. 549) This difference of opinion may have been resolved in favour of a Hellenistic date, but with respect to their Aramaic counterparts things remain less certain. Here Stern mentions the arguments for dating them to either the late sixth–early fifth centuries (as favoured by Avigad) or the late fifth–fourth centuries (as favoured by most other scholars). In the case of some of the governors mentioned on these Aramaic sealings, he cautiously notes that the question 'remains open'.

In short the fundamental problem of chronology haunts almost every page of Stern's otherwise magisterial survey. Amazingly, we are still unclear about the dating of much of the material under review, from the Assyrian period down to the Hellenistic. Blame for these problems is of course not laid at Stern's door – aside perhaps for his overeagerness for the 'Babylonian gap'. The problems come from a general reluctance by archaeologists in Israel to reappraise, and where necessary abandon certain cherished 'fixed points'. In lieu of those we might have desired from Mesopotamian inscriptions, various historical dates have been accepted, on the assumption that certain destruction levels can be attributed to particular campaigns of Tiglath-pileser III, Sennacherib, Nebuchadrezzar etc. But it should be remembered that these are often assumptions, not in any way 'proven' by diagnostic evidence. While the redating of the destruction of Lachish III from 597 BC to 701 BC (in the late 1970s) now appears to be set in concrete, it has been pointed out that the case does not depend 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).

The 'Lachish revolution' in dating, as well as being a process still in completion, is producing some surprising consequences. A conspicuous case concerns the substantial amount of Edomite pottery, *ostraca* and other material remains, now dated to the seventh century BC, found in the Negev of southern Judah. Stern sees this as evidence, surely correct, for a westwards expansion of Edomite settlement (pp. 161, 276–279; for further details, see Vaughn 1999: 50–58). Yet from numerous Old Testament sources we would expect such Edomite settlement to belong to the period after the fall of Jerusalem in 587/6 BC, rather than the seventh century when the area was still under Judahite control (Kokkinos 1998: 40–41, esp. n. 27).

Given all these chronological tensions it is for good reason that Stern repeatedly highlights the finds of imported Greek pottery. From the Persian War period (*c.* 490 BC onwards) this can provide an almost decadal ceramic guide, to cross-date with the less glamorous production in Palestine. Yet, a few generations before this period

we are entangled in another chronological mare's nest. The fragility of Archaic Greek pottery chronology has often been recognized, most recently due to the critical efforts of Michael Vickers (now Professor of Archaeology at Oxford) and the late Persianist David Francis.¹⁰ While it is fair to say that their suggested reductions (at points as much as 80 years) have been generally received as too extreme, the matters raised have certainly not been settled. One problem has always been circularity in argument between Greek and Israeli archaeologists (see Fantalkin 2001: 128, n. 57). The conundrum is highlighted by Stern:

... there has previously been little independent historical evidence to confirm the accuracy of this dating, for the chronology of early Greek pottery is based on its presence at Near Eastern sites with problematic stratigraphy. Some scholars, questioning the traditional chronology for Greek pottery, have tried to lower its dates. The new, securely dated material from Ashkelon, Ekron and Tel Batash, supported by evidence from Me'zad Hashavyahu, Kabri, Tell Keisan, Malhata, and other sites, assures the earlier ones [dates]. (p. 221)

However, Stern's belief that there is 'new, securely dated material' from the listed sites is optimistic. For all those listed there is not a *shred* of historical evidence for dating their destruction to one Babylonian campaign or another, with the single exception of Ashkelon. Indeed, it is only on the basis of a debated epigraphical restoration of the name 'Ashkelon' in the Babylonian Chronicle for the year 604 BC,¹¹ that the whole concept of a wave of destructions by Nebuchadnezzar in Philistia (and by extension, through the Greek pottery to the other sites) *c.* 605–603 BC rests. Even allowing that Ashkelon was destroyed in 604 BC, there is still no firm link to its archaeology. The current excavators have associated the event with *a* burnt level (incorporating '7th-century' Greek sherds – Stager 1996) but, as there is no published stratigraphy with details of lower levels that might bear signs of destruction, the link remains speculative. Otherwise, the dating of the local pottery at such sites during this period remains *sub judice*. As Fantalkin (2001: 128) remarks, 'the present state of research does not permit the unequivocal identification of the typological differences between the end of the 7th century and the beginning of the 6th century BCE.'

Further, there is a mounting case for reducing the dates of the Greek imports in question. At three key sites (Naukratis, Old Smyrna and Tokra near Cyrene), Herodotus gives an account consistent with the archaeology – except that the pottery associated with the events he discusses is presently dated some three to four decades too early (Bowden 1991; 1996; James 2003). Either Herodotus is wrong or the conventional Archaic pottery chronology is wrong. The stakes are high. Herodotus' narrative involves a fairly tight pattern of synchronisms between Greece and neighbouring countries such as Lydia, Egypt and Babylonia. If he is wrong about major factors such as the origin of the Greek settlements at Naukratis and Cyrene we are in danger of jettisoning a large part of Archaic Greek history. If on the other hand we do not reject Herodotus, then the horizon of Greek pottery presently dated *c.* 600 BC will need to be lowered. There will be dramatic effects on the horizon of destructions in Philistia (including Ekron IB) presently dated to the Babylonian invasions *c.* 604 BC. The dating here is very tight, as some of the Archaic pottery

found in the destructions can be (conventionally) dated close to *c.* 600 BC; thus even a relatively small adjustment on the Greek side (25–35 years) would tip the Philistian destructions into the first part of the sixth century BC. The ‘Babylonian Gap’ perceived by Stern may then start to disappear, at least with respect to Philistia, and many of the problems reviewed above will have to be considered in a fresh light. In the meantime, Israeli archaeologists should keep a weather eye on the developments in the continuing debate over Greek Archaic chronology – as much as *vice versa*.

To Stern’s credit, he at least alerts his readers to the fact that there may be a problem *viz a viz* cross-dating with Archaic Greek pottery, though he may be premature in pronouncing that ‘firmly dated’ destructions have now solved it. On this point and on many others (such as the Yehud stamps and the Ashkelon *favissa*) the candour of Stern’s writing is most welcome – particularly in a semi-popular book where readers are all too often dished up the ‘varnished truth’.

It is a platitude to apologize for nitpicking when reviewing an otherwise excellent book. But I will repeat it, as my comments (largely chronological) certainly fail to do justice to the wider aspects of a work of monumental labour. In completing it, Stern has created a vital platform for all future discussion. Without synthesizing works like this it would be impossible to control all the data and make progress, in whatever direction that might be. Stern has made a significant and lasting contribution to our understanding of these strangely elusive periods (Assyrian, Babylonian and Persian) in the archaeology of Bible lands.

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Notes

1 But not, alas, the editing. There are numerous slips, unfortunate in a textbook like this, which could have been avoided by using more expert readers. For example: p. 120, Achish of Ekron is not ‘known from Assyrian sources from 676 to 667 BCE’, but only in the years 673 and 667 BC; p. 240, the reign of Pada’el did not end in 635 but no later than 667 BC; on p. 268 the names Qosgabar and Qosga (which in any case should be ‘Qosg[...]’) are interchanged; on p. 363 we are told that the Darius whose accession year is mentioned in a cuneiform tablet from Tawilan in Edom was ‘probably the third’ Persian ruler of that name (giving 335 BC), but on p. 457 ‘Darius I (521 BC) is the most likely candidate’. Other errors should have been caught by the copy editor, e.g., p. 328, ‘Nebonidus’ instead of ‘Nabonidus’ – worse still, the phantom ‘Nebonidus’ gets his own entry in the index (p. 656).

2 The problem was highlighted in James *et al.* 1991a: 183–188, James *et al.* 1991b (esp. Table 2, p. 232 – partly corrected from that in James *et al.* 1991a: 195), and James *et al.* 1998, 30–31, where it is argued that Iron IIA, conventionally dated to *c.* 1000–900 BC, began in the mid-ninth century BC. For example at Samaria we followed Kenyon in associating it with the building activities of the Omrides, *c.* 870–850 BC. Finkelstein’s advocacy of a tenth-to-ninth-century shift has now become the focus of a long-running debate. For the initial

salvoes, see Finkelstein 1995; 1996; Mazar 1997; Finkelstein 1998; for a recent contribution and bibliography, see Gilboa and Sharon 2003. For some brief remarks on the wider implications of the tenth to ninth century controversy, see James 2002a; 2002b.

3 The brief survey in van der Toorn (2000: 99–100) has useful references but was written too early to include the Tel Hadid finds.

4 For further details and references (Gezer and Keisan) see conveniently James *et al.* 1991a: 182, 373 (nn. 66–68).

5 Personal communications from Bob Porter and Lawrence Stager, who have independently examined the alleged Assyrian architectural parallels.

6 In James *et al.* 1991a: 175, we recommended lowering the end of Iron IIC to c. 440 BC.

7 According to 2 Kgs. 25:27, Amel Marduk considerably raised the status of the captive Judahite king, Jehoiachin, at his court. If this is true, it would suggest a measure of conciliation to the Jews, both to those in Palestine and Babylonia.

8 See Sack 2003 and Dalley 2003 – in particular her discussion of the recently discovered tablets from Šēkh Hamad, where Neo-Babylonian administrative texts from the early reign of Nebuchadrezzar continue the scribal tradition of the Assyrian empire.

9 The sealings of ‘Gemaryahu son of Shaphan’ and ‘Azaryahu son of Hilkiyahu’ from the City of David excavations are believed to reflect the biblical characters Gemariah son of Shaphan (Jer. 36:10) and Azariah son of Hilkiyah the priest (1 Chr. 6:13–14) from the last decades of the Judahite monarchy. The sealing of ‘Gedaliah who is over the house’ from Lachish II is believed to come from an early stage in the career of Gedaliah son of Ahikam, appointed governor of Jerusalem after the Babylonian sack of Jerusalem (2 Kgs. 25:22–24) – an identification described by Avigad (1978, 52) as ‘not absolutely certain’. A detailed examination of these and other possible historical links is included in the forthcoming PhD thesis of Peter van der Veen: ‘The Final Phase of Iron Age IIC and the Babylonian Conquest of Judah – a Reassessment of Provenanced Seals and Bullae from Israel and Jordan’. (Trinity College/University of Bristol).

10 The bibliography here is extensive – see conveniently Biers 1992: 99–101; James 2003: 241, nn. 31–32.

11 Wiseman 1991: 23, n. 158; for further references see conveniently Waldbaum and Magness 1997: 37, n. 105.

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