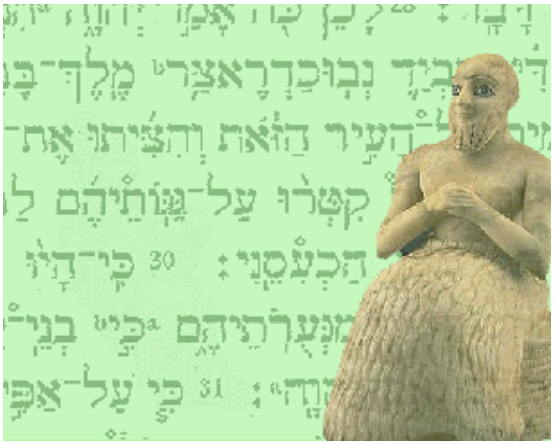
The Journal of Hebrew Scriptures

ISSN 1203–1542

http://www.jhsonline.org and

http://purl.org/jhs



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VOLUME 9, ARTICLE 24

ISRAEL FINKELSTEIN, PERSIAN PERIOD JERUSALEM AND YEHUD: A REJOINDER

PERSIAN PERIOD JERUSALEM AND YEHUD: A REJOINDER

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INTRODUCTION

I have recently published articles on Jerusalem and Nehemiah's wall (Finkelstein 2008a), and about the light that archaeology sheds on the List of Returnees in Ezra and Nehemiah (idem 2008b). My main conclusions in these two articles are:

1. Persian period Jerusalem was a small settlement that covered an area of ca. 2–2.5 hectares, with a population of no more than a few hundred people.
2. Over a century of archaeological investigation in Jerusalem has failed to reveal any trace of a city-wall that can be dated to the Persian period and identified as the wall of Nehemiah.
3. The description of the construction of the wall in Nehemiah 3 may represent the reality of the erection of the First Wall in the Hasmonean period.
4. The archaeology of the places mentioned in the List of Returnees in Ezra (2:1–67) and Nehemiah (7:6–68) seems to show that this text, too, probably represents a Late Hellenistic (2nd century BCE) rather than a Persian-period reality.

A few recent publications have taken issue with these observations (Zevit 2009; E. Mazar 2009; Barkay 2008; Lipschits 2009). This article is meant to address the main arguments advanced in these publications. My major interest is not the dispute itself, but rather the methodological questions that stand behind the debate, namely issues related to the methods of field archaeology and the interface between archaeology and the biblical texts.

LITERAL, UNCRITICAL READING OF THE BIBLICAL TEXT

Zevit (2009) has contested my treatment of the archaeology (mainly surveys) of sites mentioned in the List of Returnees and defended the dating of the list to the Persian period. The underpinnings of this debate are methodological and involve two con-

trasting attitudes to the reconstruction of the history of Ancient Israel. Zevit—in the footsteps of the Albright School—repeats the biblical testimony in modern language; adapts archaeology when it is useful and rejects it when it stands in his way; and fiercely fights-off any attempt to challenge the historicity of the descriptions in the text. I tend to give archaeology a central, independent role and treat the text as a stratified literary work whose layers are embedded with the ideological goals of their authors and the realities of their time.

Zevit's summary of the history of Jerusalem in the Persian period best demonstrates his approach:

> Some early returnees rebuilt an altar and reinstituted sacrifices to ward off misfortune. Over a year later they got around to setting the foundation for a new temple. Only some years later, during the reign of Darius I, was the temple completed.... Jerusalem, however, remained unsettled with her ruined houses and breached walls. It was only during the reign of Artaxerxes I ... that Cyrus' original project was completed. Nehemiah, an official in the royal court, turned Jerusalem into a religious and political centre in Yehud ... by completing a slapdash wall with some descendents from the first returnee settlers among the labourers... (Zevit 2009: 134).

It is my contention that the reconstruction of the history of Ancient Israel should be based on three pillars: archaeology, the biblical text and ancient Near Eastern records. The latter do not exist for the Persian period (except for a single reference in Elephantine), hence Zevit's description should be read as no more than an English translation of the ancient text.

Moreover, from an archaeological perspective, Zevit's position ends up challenging the archaeological finds. In fact, as the following observations demonstrate, his article is lacking in knowledge and understanding of archaeological method and techniques:

1. Zevit argues against the reliability of archaeological surveys: "Surveys are simply surveys. The accidental origin of what surveyors pick up somewhat randomly cannot be used to determine the actual nature of a site..." (Zevit 2009: 131).[1] In a properly conducted survey finds are not picked up "randomly" and the results are not arbitrary. This has been demonstrated by the best and brightest of American archaeology (none belonging to the biblical archaeology branch of the profession), who in fact established the basics of the art of modern archaeological surveys, namely Braidwood (1937) in the plain of Antioch,

[1] Incidentally, in the same breath Zevit (2009: 128) uses the very same surveys to support his reading of Haggai and Zechariah regarding population decline in Yehud in the 6th century BCE.

Willey (1953) in South America, Adams (e.g., 1981) in Mesopotamia, and others. Survey work, though not devoid of errors, is a highly sophisticated domain of archaeology (see, e.g., Banning 2002; Collins and Leigh Molyneaux 2003), and is especially valuable when a large number of sites are examined (and some of them excavated)—exactly the case under discussion here.[2]

2. "Theoretically an historical presence [in the Persian period—I.F.] could be invisible to archaeology" (Zevit 2009: 125). This is a surprising statement. Walls, floors, sherds, stone vessels, metal implements and other finds do not evaporate. Even faint human activity leaves traces, which can be detected in excavations. Surveys, too, if properly executed, provide a good picture of the settlement history of a site. This is especially true in the highlands, where settlements are usually located on ridges and hills and thus sherds are eroded to the slopes, where they can easily be collected in large numbers.
3. Though, to use Zevit's words again, "surveys are simply surveys," that is, in certain cases—mainly when the number of sherds collected is small, they may supply less than a full picture on the settlement history of a given site, this is certainly not true in the case of sites which produce hundreds of sherds: 242 sherds were collected at Anata, 440 at Deir el-'Azar (the location of Kirjath Jearim), 243 at Khirbet el-Kafira (the mound of biblical Chephirah), 359 at er-Ram (Ramah), 284 at Jaba (Geba) and 643 at Mukhmas (Michmash) (for details see Feldstein et al. 1993; Dinur and Feig 1993). In these cases the results are decisive, even when the evidence is negative; certainly, they cannot be ignored.[3]
4. What Zevit says about the "two partially overlapping Persian periods" (i.e., the historical and the archaeological; see Zevit 2009: 132) is trivial. Similar phenomena have been studied long ago regarding other transition periods, e.g., from the Roman to Byzantine and from the Byzantine to Early Islamic periods. Similarly, what Zevit states

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[2] Six of the 17 sites of the List of Returnees treated by me were thoroughly excavated.

[3] When dealing with the location of biblical Anathoth, for example, can one ignore the 242 sherds from the village of Anata only because of what an early scholar claimed in 1936 (Zevit 2009: 134), when archaeology was in its infancy? Incidentally, in this case, too, Zevit practices a double standard, on the one hand arguing that surveys cannot supply an accurate picture of the settlement history of a site; on the other hand criticizing my cautious description of the results in terms of intensive or weak activity (idem: 131).

> about the transition of pottery traditions between the late Iron II and the 6th century BCE (idem: 125) is known to every first-year archaeology student and taken into consideration in every serious research of the period. In any event, the 5th-4th centuries BCE pottery repertoire is well-known and easy to identify (e.g., Stern 1982; Lipschits 2005: 193–203). This repertoire is missing from five of the 17 sites that appear in the List of Returnees and that are discussed in my article, including the *well-excavated* Gibeon and Bethel.

Surprisingly, despite his reservations, Zevit seems to accept the fact that five of the sites mentioned in the List of Returnees were not inhabited in the Persian period. His logic of solving the problem is as follows: The returnees settled in all places mentioned in the list in the late 6th century BCE, but a few decades later (and I should add – during a peaceful, empire-dominated period), they abandoned five of them, including the two highly important sites of Gibeon and Bethel. This acrobatic proposal is aimed only at saving a simplistic reading of the text; it does not seem to me to be a viable historical option.

Zevit's discussion of geographical history shows how much hypothetical is his approach when it is compared to the solid, conservative method of archaeological survey. A good illustration is his logic regarding the location of Senaah (Neh 3: 35), which according to him is *possibly* a place rather than a family name—italics mine. His argument is as follow: (a) Senaah *may be* equated with Hassenuah of Neh 11:9 and 1 Chr 9:7, (b) the latter *may be* associated with Madalsenna of Eusebius, (c) which was *probably* located north of Jericho; (d) this place can *possibly* be equated with Toponym 88 of the Sheshonq I Karnak list (incidentally, this toponym is listed with the Negeb group of sites!), and (e) this in turn, *may* tell us that the location of (f) another *possible* place (if not a clan) – Elam – was north of Senaah near Wadi Farah.... I counted seven conditions in this single identification. Zevit adds that perhaps "each territory, Senaah and the other Elam, contained some small permanent villages, a scatter of seasonal hamlets, and many range-tied, migratory tent communities..." (2009: 129). Yet, there are no settlements, hamlets and migratory community in the region mentioned by Zevit; the archaeology of the Jordan Valley north of Jericho (see, e.g., Bar Adon 1972) does not have them. Here, too, highly hypothetical interpretation holds the upper hand, while the archaeological facts are ignored.

The identification of Gibbar and Magbish, supposedly named for Persian personalities (supporters of Darius I) with Gibeon and Mizpah (Zevit 2009: 129–130) is even more far-fetched. We do not know if these are indeed place names. Moreover, there is no testimony for calling places after Persian personalities. In addition, there is no indication for a change in the names of Gibeon and Mizpah. In short, in these cases the important field of geographical

history becomes a playground for unsupported proposals that ignore archaeological evidence for the sake of consistency with a biblical reading.

In sum, Zevit's article provides an excellent case-study of the flaws of the Albrightian approach to biblical history and as such it does not provide any solid base for reconstructing the history of ancient Israel in the Persian period.

NEHEMIAH'S WALL IN JERUSALEM: BUILT ON QUICKSAND

Eilat Mazar (2009a; 2009b) recently announced the discovery of a fragment of the wall built by Nehemiah in the City of David. She refers to the northern tower in Area G (and a section of a wall connected to it on the south), which were first excavated by Macalister and Duncan (1926). The tower has commonly been interpreted as part of the First Wall built in the late Hellenistic (Hasmonean) period (e.g., Kenyon 1974: 191–195; Shiloh 1984: 29–30; Wightman 1993).

Mazar bases her dating of the fortification on finds retrieved *under* the tower: two dog burials (with no pottery) were found "directly below the lower course of the tower." A 1.5 meter thick layer uncovered under these burials produced sherds dated to "the end of the 6th and the first half of the 5th centuries BCE."[4] Further down Mazar uncovered a three meter thick layer with a large quantity of pottery dating to the late 6th and beginning of the 5th century BCE (E. Mazar 2009a: 74–76; 2009b). According to Mazar's logic, these finds date the tower to the middle of the Persian period which allows her to identify it as part of Nehemiah's wall.

Needless to say, these layers provide no more than a *terminus post quem* for the construction of the tower—later than the 6th/early 5th century BCE. The absence of Persian period material here (unless the dog burials belong to this period) means nothing; the wall was constructed on the edge of the ridge, at the top of the steep slope, and it is only logical to assume that it was laid after preparatory work, which could have included a leveling operation. The most logical date for the towers and the wall is the late Hellenistic (Hasmonean) period. This wall is known from many locations in both the southeastern and southwestern hills (e.g., Wightman 1993; Geva 2003: 529–534).

Similar to Zevit's, Mazar's attitude to the biblical testimony is highly literal and uncritical: "Decades after the Babylonian destruction of Jerusalem in 586 BCE, the city appears to have remained desolate and in ruins. A change occurred with the surrender of the Babylonians to the Persians and the decree of Cyrus, king of

[4] Similar material was found laid against the northern tower by Kenyon (1974: 183) and possibly under the tower by Macalister and Duncan (1926: 51).

Persia, in 538 BCE, which allowed exiled Jews to return to Jerusalem and rebuild their temple .... In 445 BCE, Nehemiah was appointed governor and given the authority from the Persian king to rebuild the walls of Jerusalem. Nehemiah's descriptions reflect the actual appearance of the ruined, and, later, the restored walls of the city" (E. Mazar 2009a: 72; for Mazar's similar literal reading of other biblical material on Jerusalem see Finkelstein et al. 2007).

HOW FULL CAN AN EMPTY GLASS BE?

Barkay has recently addressed the question of Persian period Jerusalem (2008). Indirectly, Barkay agrees with me about the positive evidence, which testifies to a 2–2.5 hectare settlement (2.8 hectares according to Lipschits – 2009; see below). But based on his rejection of the negative evidence, Barkay adds almost 10 hectares and argues that in the Persian period Jerusalem covered an area of 12 hectares. In this, he positions himself as the ultra-maximalist of our generation. One can understand how a text scholar reaches a maximalist estimate based solely on the biblical testimony (e.g., Weinberg 1992: 43). But how does an archaeologist arrive at such an estimate?[5]

Barkay rightly acknowledges that no Persian period structure or floor has ever been found in Jerusalem (2008: 50), but he says that this is also the situation in the Late Bronze, Iron IIA, Babylonian and Early Hellenistic periods. He seems to see this as evidence for the possibility that even periods of prosperous settlements can leave no remains, because "the core of the urban area, which was the nucleus of the settlement in these periods, was entirely 'devoured' (*sic*!) by the intensive settlement of later periods..." (here and below my translation – I.F.). I have already challenged the notion that walls, vessels and other finds can disappear and in any event, I would interpret the situation described by Barkay in the opposite way: in these periods (apart from the finds from the Iron IIA which do indicate a growing settlement—Finkelstein 2001) the settlement was indeed poor and limited in size and population.

Barkay claims that the Persian period pottery is difficult to distinguish from the pottery of the Iron II and Hellenistic periods (idem: 49). This may be true for a limited number of pottery forms, especially when found in a small quantity in a survey. It is certainly not the case in a large-scale survey and in an excavation. Persian

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[5] The large number of Yehud seal impressions mentioned by Barkay is acknowledged by all scholars and cannot be an argument in the discussion of the size of the settlement. Barkay accepts Eilat Mazar's identification of the northern tower on the eastern slope of the City of David with the wall of Nehemiah: "Recent excavations by Eilat Mazar proved that that Macalister and Duncan's northern tower should be dated to the Persian period" (Barkay 2008: 52). As seen above, this statement gives new meaning to the term "prove" in archaeology.

period storage jars, mortaria, cooking pots, juglets and imported vessels are easy to identify and distinguish from their counterparts in the late Iron II and Hellenistic periods.

Barkay argues that the entire southern part of the Persian period settlement on the ridge of the City of David was eradicated by the Hasmoneans in the late Hellenistic period, in connection with the construction of the Akra fortress (2008: 48). This is a *hypothesis* based on an *assumption* (the location of the fort here), which is not supported by a single find. It also contradicts all other suggestions for the location of the Akra, which are supported by at least some evidence—archaeological and/or textual (summary in Tal, in press).

Barkay's reference to the few finds retrieved in the sifting of earth taken by the *waqf* from the Temple Mount (2008: 49) is misleading. He acknowledges that very few Persian period finds were retrieved, but withholds the full picture of strong evidence for Iron IIB-C and late Hellenistic activity there (Barkay and Zweig 2006). The same holds true for the evidence from the vicinity of Jerusalem. Barkay mentions the existence of spots with Persian period remains (2008: 50), but does not provide the reader with the full set of data: The thorough survey of the Jerusalem countryside revealed 185 Iron II and 140 Hellenistic find spots, compared to 17 Persian period find spots (Kloner 2003: 19*).

Barkay attacks my proposal to see a Hasmonean reality behind Nehemiah 3. Though I have suggested this with caution, as a possibility, none of *his* arguments stands scholarly scrutiny:

1. Similar to Zevit and E. Mazar, Barkay's point of departure is the acceptance of the geographical material in the book of Nehemiah as a testimony for the Persian period (2008: 51). This, of course, is a circular argument.
2. Barkay rejects the idea that Chapter 3 is a later addition to the book of Nehemiah (2008: 51). Yet, text scholars have noted the independent nature of the list in Nehemiah 3 as compared to the rest of the "Nehemiah Memoir" (Mowinckel 1964: 109–116; Williamson 1985: 200; Blenkinsopp 1988: 231 to name a few) and some understood it as a later addition to the book (e.g., Torrey 1896; 37–38; 1910: 249; Mowinckel 1964: 109–116).
3. Barkay opposes my notion that highlands sites were not fortified in the Persian period and brings the fortification of Stratum I at Lachish as an example of a Persian period city-wall in Judah (sic! 2008: 51). But Lachish is not located in the highlands and was not included in the territory of Yehud (see, e.g., maps in Stern 1982: 247; Lipschits 2005: 183); rather, it was an Achaemenid administrative center (Ussishkin 2004: 95; Fantalkin and Tal 2006).
4. "The list [in Nehemiah 3 – I.F.] is clearly of administrative nature, it is technical and boring, composed of names of

people and places only. ... Had the list been fictitious and anachronistic, we would have expected that it would disclose an ideological motivation of sort, and that it would be less dry and more interesting" (2008: 52). According to this logic the description of the list of Judahite towns in Joshua 15— dry, boring, composed of toponyms only and lacking any apparent ideology—should be dated to the time of Joshua....

5. If the list dates to the Hasmonean period, one would expect it to disclose Greek names (2008: 52). According to this logic almost no biblical text was written in the Hellenistic period.
6. Some of the names of individuals and families which appear in Nehemiah 3 are mentioned in other chapters of Nehemiah and even in Ezra; they do not appear in sources of other periods (Barkay 2008: 52), plus, at least 10 toponyms which appear in the list are known from earlier sources (idem: 52–53). These are not arguments, because the complier of Nehemiah 3 could have taken names of individuals and places from earlier biblical texts.
7. The list of districts of Yehud in Nehemiah 3 "well fits the distribution of the Yehud seal impressions of the Persian period and does not fit the extent of Hasmonean rule at the time when the First Wall was built" (idem: 53). This statement is wrong. The overwhelming majority of Persian period Yehud impressions (Types 1–12 in Vanderhooft and Lipschits 2007) are concentrated in Jerusalem and its immediate surroundings, including Ramat Rahel. No such seal impressions were found at Beth-zur and Qeilah and only a relatively small number was found north of Jerusalem. The list in Nehemiah 3 seems to fit the extent of Hasmonean Judea before it started expanding to the west and north in ca. 140 BCE (Finkelstein in press).
8. The list of enemies of Judah—Tobiah the Ammonite, Sanballat the Horonite and Geshem the Arabian (e.g., Nehemiah 2: 19, 6:1)—fits only the Persian period (2008: 53). This argument should be seriously considered for the following reasons: (a) a Sanballat is mentioned in the Elephantine papyri as the Governor of Samaria; and (b) Gashmu the king of Kedar appears in a 5th century Aramaic inscription on a silver vessel ostensibly found at Tell el-Maskhuta in the Delta. But the fact that these names appear in the "Nehemiah Memoir" means nothing for the date of Nehemiah 3, considered by most scholar to be an independent source (see partial list of references above). Moreover, as I will try to show in detail in another place, these names cannot be read in a simplistic way also in re-

> gard to the rest of the Book of Nehemiah. A few observations suffice: (a) the name Sanballat is mentioned in the Wadi ed-Daliyeh papyri and by Josephus Ant 11; both (and probably also Sanballat of the Elephantine papyri) are later than the conventional date given to Nehemiah; (b) the Tobiads appear in extra-biblical texts in the 3rd and 2nd centuries bce and being a symbol of Hellenistic culture, in Hasmonean times there was good reason to portray them negatively; (c) Geshem is a common Arab name; though there must have been a Qedarite king named Geshem sometime in the Persian period, a Lihyanite (northwest Arabia) king with the same name ruled in the early 2nd century BCE (Farès-Drappeau 2005: 122–123) and (d) the Ashdodites are also included among the enemies of Yehud in Nehemiah (4: 7); there is no logic in seeing Ashdod as a foe before the expansion of the Hasomoneans to the west in the 140s BCE, an expansion which brought them closer to the territory of Ashdod. In short, in the case of the enemies of Nehemiah, too, 2nd century realities could have been mixed with old traditions.

FACTS OR HYPOTHESES: WHAT COMES FIRST?

Lipschits, too, challenges my analysis of the size of Jerusalem and its population in the Persian and Early Hellenistic periods (2009). Lipschits concludes that Jerusalem of the Persian period covered an area of five hectares – twice the area that I suggested. This case is different from the ones dealt with above, because Lipschits' reading of the biblical text is critical. Here the dispute is only about the meaning of negative evidence in archaeology.

Lipschits agrees with me that the Persian period settlement covered *mainly* the central part of the City of David's ridge; we are also not too far apart regarding its size: 2.2–5 hectares in my analysis, 2.8–3 hectares in his. Yet, Lipschits and I differ regarding the negative evidence, namely the areas with *no* Persian and Early Hellenistic finds. Lipschits adds the two hectares of the Ophel (between the Temple Mount and Area G) to his calculation and gets a ca. five hectares settlement.

In this case too one is faced with a major methodological problem: What should rule: Archaeological facts, even negative evidence (which is also a fact), or hypotheses? Lipschits writes (my comments in italics in square brackets):

> The importance of the Ophel hill as the main built-up area in the Persian and Early Hellenistic periods was never discussed in the archaeological and historical research. The reason was the scarcity of finds [*in fact, no finds*] in this area, of about 20 dunams. ... This is the only flat, easy-to-settle area in the city. Its proximity to the Temple Mount on the one hand and the

> easy option to fortify it ... [*no fortification has ever been found*] made it the preferred option for settlement in the Persian period. In spite of the scarcity of finds [*in fact, no finds*] in this area, the relatively abundance of Persian period finds along its southern slope, its proximity to the temple mount, its geographical characteristics and its importance in the Iron Age and post-Persian periods – all these facts [*these are interpretations rather than facts*] indicate that this area should be considered part of the settled area of Jerusalem during the Persian and Early Hellenistic periods. The absence of Persian period finds in the Ophel hill [*here "scarcity of finds" is correctly replaced with "absence of finds"*] ... is an indication of the limitations of archaeological research" (Lipschits 2009: 19–20).

Needless to say, hypotheses and interpretation, not facts dictate Lipschits' discussion: There are no finds, but since the area must have been settled according to his logic, there must have been a settlement there. As an archaeologist I cannot accept this line of reasoning (which is also true for Cahill 2003; A. Mazar 2006 and Na'aman 2007 regarding other periods in the history of Jerusalem; see, for instance, Finkelstein 2008c). It is worth repeating that it is simply impossible that all pottery sherds, walls and other finds— even those representing a meager settlement— have disappeared.

SUMMARY

This article is about method as well as data. I have dealt with methodological issues such as inconsistencies between archaeology and text; the meaning of negative evidence in archaeology (in surveys and excavations alike); the trustworthiness of a theory built on unsupported hypotheses; the pace of change in material culture; the meaning of *terminus post quem* in archaeology, and the like. On the factual level, with the available data at hand, I see no reason to change my views on the issues: Persian period Jerusalem covered ca. 2–2.5 hectares, and both the description of the construction of the city-wall in Nehemiah 3 and the List of Returnees in Ezra and Nehemiah probably reflect late Hellenistic (Hasmonean) period realities. Only new data that would change the *archaeological* picture can call for a new interpretation of these texts.

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