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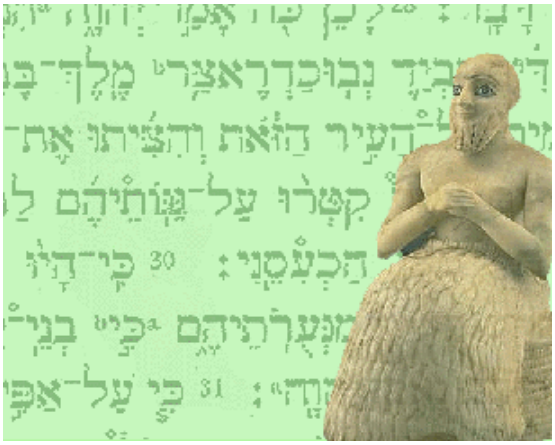

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ISRAEL FINKELSTEIN, ARCHAEOLOGY AS A HIGH COURT IN ANCIENT ISRAELITE HISTORY: A REPLY TO NADAV NA'AMAN

ARCHAEOLOGY AS A HIGH COURT IN ANCIENT ISRAELITE HISTORY: A REPLY TO NADAV NA'AMAN

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INTRODUCTION

In a recent article Nadav Na'aman (2010) expressed doubts regarding the meaning of negative archaeological evidence for reconstructing biblical history, especially regarding sites in the highlands. Na'aman's tackling of the sometimes uneasy relationship between text and archaeology is important methodologically, and hence should be put to the test of hard-core data. In what follows I wish to deal with all six points discussed in his article.

THE CASE OF CANAAN IN THE AMARNA PERIOD

Na'aman rightly spotlights the discrepancy between the testimony of the Amarna letters, which describe sites such as Jerusalem, Shechem, Gezer and Lachish as strong polities, and the meager 14th century BCE finds unearthed in the excavations at these sites. The question is, what can we learn from this tension between text and archaeology and does it reflect on the situation in the Iron Age?

Regarding the texts, in the case of the Amarna tablets we are dealing with real-time textual evidence which can hardly be contested. This is not the case in much of the biblical materials dealt with by Na'aman. Regarding archaeology, I think that the problem should be phrased differently: Would an archaeology-based, text-free study of the Late Bronze II produce a map of Canaanite city-states that would be considerably diverse from the one drawn according to the Amarna letters? I believe that such an investigation—even without the results of the mineralogical study of the tablets (Goren, Finkelstein and Na'aman 2004)—would point to Lachish, Gezer, Pehel, Megiddo, Hazor and possibly Ashdod as centers of city states, or peer polities. By inference to the situation in the Early and Middle Bronze (as well as the Iron II), such a study should point to two centers in the highlands, in Shechem and Jerusalem or their vicinities. Ashkelon and Gath (Gimtu) would not be on such a map because excavations at these sites have not yet reached sufficient exposure in Late Bronze strata. The only identifiable city-state mentioned in the letters which would not be on an archaeology-based map is Yurza (if the site is identified at Tell Jemmeh—Maisler 1952).

The lesson is that the discrepancy depends on the rules of the comparison. When judged on the background of the Late Bronze

Age only—and assuming a system of city-states or peer-polities[1]—the discrepancy between the Amarna tablets and archaeology is less dramatic than suggested by Na'aman.

JERUSALEM IN THE IRON AGE IIA

Na'aman takes the discovery of ca. 180 bullae in a late Iron IIA context in the rock-cut pool near the Gihon Spring in Jerusalem (Reich et al. 2007) as a case in point for the coincidental nature of archaeology and an indication for extensive writing in the capital of Judah at that time, "though it may have been used only by the royal palace and the elite". He then concludes that "The reconstruction of tenth-ninth century Jerusalem made on the basis of the archaeological evidence alone, while ignoring the biblical text, might be misleading". The problem is that the finds near the Gihon Spring have nothing to do with the 10th and most of the 9th centuries BCE.

The pottery that was found in the rock-cut pool with the bullae (de Groote and Fadida 2010) includes both late Iron IIA and Iron IIB forms (e.g., both hand and wheel-burnished bowls). There are two ways to treat this assemblage:

1) As a homogeneous, short-term assemblage, which should then be dated close to the Iron IIA/IIB transition; recent radiocarbon results put this transition sometime in the first half of the 8th century BCE (Finkelstein and Piasetzky 2007; 2010).

2) As an accumulation of pottery from the Iron IIA and the early phase of the Iron IIB (before the construction of the Iron IIB building over the fill in the rock-cut pool); in this case it is difficult to precisely date the bullae within the late 9th century/mid 8th century time range.

In any event, the bullae, which are indeed highly important for understanding the history of Jerusalem ca. 800 BCE, do not reflect on the situation there in the 10th and early 9th centuries BCE.

JERUSALEM'S WALL IN THE LATE 9TH CENTURY BCE

Na'aman argues that the City of David must have been fortified as early as the beginning of the 8th century BCE and "probably earlier". He backs this statement up with three arguments:

1) Major Judahite cities in the Shephelah and Beer-sheba Valley were fortified in the 9th century and hence it is only logical to assume that the capital of the kingdom was fortified too.

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[1] I am mentioning this because of a different kind of discrepancy: Based on its sheer size and opulence, archaeology alone would probably portray Hazor as a capital of a large territorial kingdom rather than a city-state.

2) 2 Kings 14:13 describes a breach made in the wall of Jerusalem by Joash king of Israel in the early 8th century.

3) Rezin king of Damascus and Pekah king of Israel besieged Jerusalem but were not able to conquer it (RSV translation of 2 Kings 16:5; see also Isaiah 7:1).

Argument 2 assumes that the late 7th century author had reliable information on an event that took place almost two centuries before his time and that he reported it with no influence of the situation in his own time; this assumption is not rock-solid. The verse regarding the attack of Rezin and Pekah is too vague to be used here; Na'aman translates "were not able to attack"; 'could not fight' is another possibility, but in any event, 'could not conquer' is an interpretation. We are left then with Argument 1—admittedly a strong one, especially if one takes into consideration the possibility that the Great Wall at nearby Tell en-Nasbeh was constructed in the late 9th century (Finkelstein forthcoming). Still, thus far this argument has not been backed by solid evidence on the ground.

Na'aman argues that an early 8th century wall on the western side of the City of David must have been removed or eroded "so that no fragment of it was discovered until now in the excavations". Fortifications do not disappear; but the western slope was excavated mainly in one area—by Crowfoot in 1927 (Crowfoot and Fitzgerald 1927)—so the information there is relatively limited. Yet, had there been an early 8th century (if not earlier) wall in the west, it should also be found on the eastern slope, which has been thoroughly explored in the course of many excavations. Indeed, two lines of Iron Age fortifications are known there and both date to the Iron IIB:

1) Pottery in the fill close to bedrock inside of the foundation of the wall unearthed on the lower part of the slope includes Iron IIB types (Reich and Shukron 2008).

2) Houses and fills inside the wall uncovered at mid-slope (Area E of the Shiloh excavations) all date to Stratum 12 of the Iron IIB (Shiloh 1984).

As I mentioned above, radiocarbon dating indicates that the Iron IIA/B transition could not have taken place earlier than the first half of the 8th century: first, some late Iron IIA destructions in the north cannot be dated much earlier than ca. 800 BCE (Finkelstein and Piasetzky 2009); second, a transitional Iron IIA/B assemblage at Beth-shemesh should probably be dated 765–745 BCE (Finkelstein and Piasetzky 2007). Hence, in order to date the fortification on the eastern slope to the early 8th century one needs to argue that the Iron IIB assemblages in Judah appeared full-fledged as early as 800 BCE. Radiocarbon results make such an assumption unlikely.

The discrepancy between the existence of Judahite fortifications in late Iron IIA sites in the Shephelah and Beer-sheba Valley and the absence of such fortifications in the City of David is yet to

be resolved, but not by arguing that the Iron IIB city-wall was erected ca. 800 BCE.

THE WALL OF NEHEMIAH IN JERUSALEM

I have already answered reservations—some similar to Na'aman's—about my assertion that in the Persian period the City of David was not protected by a city-wall (Finkelstein 2009 contra, e.g., Lipschits 2009) and there is no need to repeat my arguments here. Na'aman adds a textual argument—that a letter from Elephantine, addressed to the governor of Judah, mentions priests and nobles in Jerusalem. Yet, this text says what it says; it has no implication, neither regarding the question whether Jerusalem of that time was fortified nor about the size of its population.

Na'aman argues that "in the course of these centuries [until Roman times—I.F.] the fragile buildings of the Persian period were destroyed and obliterated and the pottery broken and dispersed". Yet, as I showed in my original article (Finkelstein 2008), large sectors of the City of David did not yield *any* pottery that dates between the late Iron II and late Hellenistic period. Moreover, had the pottery of Persian period Jerusalem been dispersed, my estimate of the size of Jerusalem at that time as covering ca. 2–2.5 hectares would be a maximal one.

GIBEAH

Contra the excavators (e.g., Lapp 1981: xvii; 1993), Na'aman argues that Tell el-Ful was inhabited in the 8th century BCE. He bases this assertion on the following arguments:

1) Fourteen *LMLK* seal impressions were found at the site;

2) Gibeah is mentioned three times in the Book of Hosea and Gibeah of Saul is mentioned in Isaiah 10:28–32.

Na'aman wonders about the "marked discrepancy between the biblical evidence and the results of archaeological excavations at Tell el-Ful". As far as I can judge, there is no such discrepancy. First, all 14 *LMLK* impressions found at Tell el-Ful belong to Lipschits et al.'s (2010) late types, which probably date to the first half of the 7th century.[2] Second and most important, there is no certainty in the identification of Gibeah at Tell el-Ful. In another place I recently sided with Miller (1975) and Arnold (1990: 54–60), who identify Gibeah and Gibeah of Saul with Geba/Geba of Benjamin and thus the village of Jaba; I have also suggested an alternative identification for Tell el-Ful (Finkelstein in press).

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[2] I am grateful to Oded Lipschits for providing me with the information regarding the *LMLK* impressions found at Tell el-Ful. It should be noted that Na'aman probably wrote his article before the publication of Lipschits et al.'s article and hence could not have known about the new division and date of the *LMLK* impressions.

Na'aman says that the pottery of the earliest stage of Stratum IIIA "must have been scattered and most of it completely disappeared". This is a surprising statement, as material (including ceramic) does not evaporate. Moreover, in a small and shallow-accumulation site such as Tell el-Ful the earliest sherds are not difficult to come by; even the poor Iron I and early Iron IIA settlement left behind a few sherds (ibid.). Incidentally, Lachish III pottery types *were* found at Tell el-Ful; but they may date to the early 7th century, before the change in the pottery repertoire in Judah from the Iron IIB to the Iron IIC (Lachish II) assemblages (Lipschits et al. in press).

BETHEL

Na'aman rejects the results of a thorough study of the Bethel finds, according to which activity at the site in the Babylonian period was very sparse, at best (Finkelstein and Singer-Avitz 2009). He argues that after Josiah's conquest the temple of Bethel was restored "and formed the nucleus of the place, being supported by the government of the province", and that the priests of Bethel "composed literary works that emphasized its great antiquity, its consecration by the Patriarch Jacob, and its religious importance". Na'aman summarizes that Bethel was an important center in the 6th century BCE.

Na'aman bases this description on his comprehension of biblical materials; on his understanding of the role of the site in the compilation of biblical texts; and on Hurowitz's (2006) dating of the composition of the Jacob dream story (Genesis 28: 10–22) to the Babylonian period based on linguistic and thematic considerations. Needless to say, the two first arguments are a matter of interpretation. The possibility raised by Hurowitz's, that "transferring traditions of Babylon to Bethel occurred earlier than the days of Neo-Babylonian domination of the world", during the reigns of Sennacherib or Esarhaddon (ibid.: 447) undermines the last argument.[3]

The area that was available for the excavation of Bethel was a significant one. In 1927 Albright estimated it to cover 1.5 hectares (Kelso 1968: 2), which makes up about half the area of the mound. This sector was explored in several relatively large fields (Kelso 1968: Pl. 120) and tested in a few additional soundings. In some of the excavated areas the dig reached bedrock. This means that the finds—including stray sherds—should represent the settlement history of the site. The lack of 6th century finds, including *mwsh* and lion impressions, does not support Na'aman's notion of an impor-

[3] Na'aman's Nippur example does not help either: Nippur was a huge site of 135. Bethel was a small site of ca. three hectares, large parts of which were excavated.

tant site with active shrine and priests composing biblical texts at that time.[4]

SUMMARY

As in every discipline, archaeological evidence can be fragmentary and may be misinterpreted. Yet, when solid data from well-excavated sites is compared to assumptions regarding the nature of biblical texts and their date of compilation, the former should prevail, at least until tested by new archaeological evidence or extra-biblical texts.

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[4] The case of the late 7th century BCE – the time of King Josiah – demonstrates this point. This period is represented in the pottery repertoire of Bethel, though less than the Iron IIB in the 8th century, which means that the site could already be in decline (Finkelstein and Singer-Avitz 2009). Still, though the temple of Bethel was not found in the excavation, enough late 7th century pottery was unearthed to support at least some activity at the site.

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