

HAZOR IN THE SECOND HALF
OF THE TENTH CENTURY B.C.E.:
HISTORIOGRAPHY, ARCHAEOLOGY AND HISTORY

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For a historian of the ancient Near East, there are three main kinds of evidence: archaeological material remains, contemporaneous epigraphy and literary sources, especially ancient historiography. These three sources are not always at hand. For instance, there is almost no evidence in the ancient historiography about the history of the second millennium B.C.E., meaning that historians have to rely only on archaeology and, more specifically, on epigraphy. Such a situation is well emphasized by D. Charpin regarding the history of the Babylonian king Hammurabi (2003, 15–22).

For the history of Hazor in the second half of the tenth century B.C.E., the historian has to face an opposite situation. There is practically no contemporary inscription with regard to the history of this town and we can only rely upon archaeology and literary sources. Each of these sources presents its own problems and has to be critically interpreted. Actually, each one has to be dealt independently before trying a historical synthesis. After a few remarks about epigraphy, we shall try to deal first with the literary tradition and then with archaeology before proposing a tentative synthesis.

Epigraphy

Tenth-century B.C.E. epigraphy is generally very poor for the whole ancient Near East (Millard 1991, 25–30). It is especially the case for ancient Palestine, where we have only a few graffiti with personal names (Renz 1995, 29–30; Bunimovitz and Lederman 1997, 29; Mazar 2003a, 172–74; Tapy et al. 2006; Finkelstein et al. 2008b; Maeir et al. 2008), sometimes only fragmentary, and the Gezer calendar (Renz 1995, 30–37), which is probably to be identified as Philistine (Lemaire 2000, 247).

At Hazor itself (Yadin et al. 1960, 70–75; 1961, Pl. 357–58; Delavault and Lemaire 1979, 5–11; Naveh 1989), only two letters, G□, incised on the rim of a bowl found in Stratum VI (B 4851), could be very approximately dated ca. 1000 B.C.E. These letters have proved difficult to classify, and have been variously identified as Hebrew, Phoenician or Aramaic. Another small incised inscription of five letters (B 4440), found in stratum IX, is also very difficult to classify and date (c. 900?). A third incised inscription of six signs (A 382/1), found in stratum VIII, could be Phoenician or Aramaic, again to be very approximately dated ca. 900. Except for the fact that alphabetic script was apparently not unknown in tenth-century B.C.E. Hazor, these small inscriptions do not teach us much about the contemporaneous history of the site or about its inhabitants. It even seems impossible to be precise about their ethnic affiliation since, for instance, though inscription A 382/1 is probably Phoenician, it might be on an imported jar.

No contemporaneous foreign inscription mentions Hazor. The famous Karnak list of Palestinian towns submitted to Pharaoh Shoshenq (Wilson 2005; Moers 2005) apparently does not contain the name of towns situated north of Megiddo. Actually, the archaeological interpretation of this list as a list of destroyed towns seems very doubtful (see also Finkelstein 2003, 79) since political relations with Egypt were not the same in Jerusalem (Lemaire *in press*) and in Israel (Lemaire 2006a, 1713–14). Anyhow, as for Hazor, the situation is clear: there is no reason to attribute any destruction level of Hazor to Shoshenq.

Literary Tradition

For the second half of the tenth century B.C.E., it seems that three texts of the biblical books of Kings contain some information about Hazor: directly only 1 Kgs 9:15b and, indirectly, 1 Kgs 4:15 and 15:20.

The books of Kings generally belong to the literary genre of historiography, but there is no agreement between scholars about their date and interpretation. Since M. Noth, these books are often considered part of the so-called Deuteronomistic History. However, while Noth (1943) thought of only one redactor, writing about the middle of the sixth century. and using various kinds of sources, many scholars think today that there were several redactions, and ultimately two (one under King Josiah and one during the Exile: Cross 1973, 274–89; Römer 2007; 2008, 102), or three (an earlier one under King Hezekiah: Weippert 1973; Provan 1988; see also Eynikel 1996), without taking into account another one probably at the beginning of the Persian period. I have myself

proposed to understand this book as a kind of textbook used in the royal school of Jerusalem, revised from time to time, with various strata from the tenth century until about 500 B.C.E. (Lemaire 1986).

It is clear that the historical appreciation depends in great part upon the date of the redaction of the texts referring to Hazor in the tenth century. At first sight, if they were written about 500 B.C.E. or during the Exile, or even during the reign of Josiah, their author was far away from the events. The historical value of an originally oral tradition transmitted over three to five centuries would not seem very high! However, even though the last redaction of Kings is probably to be dated at the beginning of the Persian period, it may have used ancient sources or reused earlier strata of the scribal tradition of the book. In this case, the historical appreciation will much depend not only upon the character of the last redaction, but mainly upon the date and character of the used source or earlier stratum. Actually, for the historical interpretation, there is not much difference whether a verse is considered to be taken from a source or from an earlier stratum of the book. The problem is much more the date and characterization of this source or earlier stratum.

With these general methodological remarks in mind, let us examine the three passages.

First, 1 Kgs 9:15 explicitly mentions Hazor in a list of constructions realized by Solomon with the use of forced labor (*mas*). Verse 15a mentions various constructions in Jerusalem and v. 15b: “Hazor, Megiddo, and Gezer.” After an insertion about Gezer (vv. 16–17a), this list of towns resumes in vv. 17b–18. As already well noted by Montgomery and Gehman (1951, 206), such “reports of the building, rather rebuilding, of cities are innumerable in the Ass. Inscriptions. Closer to hand are the similar inscriptions from lands contiguous to Palestine. Mesha of Moab in his stele gives a list of some eight cities which “I built”; the Syrian stele of Zakar (ca. 800 B.C.E.) records building operations in a broken passage. Indeed the present list may well have been taken from a contemporary royal inscription.” Actually, it is characteristic enough of memorial inscriptions (Montgomery and Gehman 1951, 206; Gray 1970, 243; Miller 1974; Drinkard 1989, 140; Lemaire 1991a, 146) that emphasize the victories at war and the monumental (re)buildings, especially to glorify kings. However, that does not mean that the list of towns in 1 Kgs 9:15a, 17–18a is necessarily copied from a memorial inscription. Such memorial inscriptions used previous official documents and, as already noted by Noth (1968, 212–13; Würthwein 1985, 110; see also Mulder 1998, 472; Fritz 2006, 303), the list of 1 Kgs 9:15a, 17–18a appears to be copied from such an official document (“eine amtliche Aufzeichnung”).

The insertion regarding Gezer seems to have been added by the author of the “book of the acts of Solomon” (1 Kgs 11:41) that was probably written during the reign of Rehoboam by a previous servant of Solomon (Mowinckel 1963, 7, 12–13; Liver 1967, 101; Lemaire 1995a, 116; Cogan 2001, 92; see also Porten 1967, 113). If that is the case, it is easy to understand that this author could use an official document written during Solomon’s reign.

Of course, even though it makes sense, such a history of redaction cannot be proven and is only a working hypothesis, since there is no chance of finding an original tenth-century B.C.E. document written on papyrus or leather. Other interpretations have been presented. However, it is clear, and already emphasized by Noth (1968, 208), that this entire passage does not contain any trace of deuteronomistic redaction and is pre-deuteronomistic; the problem is to fix the approximate dating of this pre-deuteronomistic redaction. For instance, Na’aman agrees that the biblical history of Solomon is “a composite and multi-layered text” (2006, 94) and recently proposed that 9:15, 17b–18 was taken from a source that can be identified with the “book of the acts of Solomon” (2006, 89, 95). For him also, this “book” is a “school text that described Solomon’s success in consolidating his kingdom and making it flourish” (2006, 88, see also 82). However, for him, it was apparently written “in Jerusalem in the late eighth century B.C.E.” (2006, 116). Actually, this dating seems to be axiomatic, or the result of a kind of circular reasoning, since he declares: “The beginning of historical writing in Judah probably did not antedate the eighth century B.C.E.” or “it is widely accepted today that writing for administrative purpose in the tenth century court of Jerusalem was minimal and that the composition of historiographical works began at a much later time” (2006, 103; see also 82). Although he recognizes that “writing began in the court of Jerusalem in the 10th century,” for him, “only in the 8th century B.C.E. did writings spread beyond the bounds of the royal and temple court” (2006, 82). For this strict limitation, he refers to the thesis of D. W. Jamieson-Drake (1991). Unfortunately, as I have tried to show elsewhere (1992), the conclusions of Jamieson-Drake are flawed by many mistakes in dealing with the archaeological data. Furthermore, while the main argument of this thesis, a direct and systematical correlation between the use of scripture and the importance of the archaeological data, is indeed an attractive theoretical model, it is much too simplistic and not corresponding to reality. Such a correlation seems somewhat ridiculous to anyone with experience in archaeology and epigraphy. Two examples will suffice: one in Palestinian archaeology and one in Transjordanian

epigraphy. On the one hand, the Early Bronze ruins of Tell Yarmut, which are undoubtedly very impressive, have thus far failed to yield any evidence of the use of script. On the other hand, in Transjordan, while we now know of thousands of Safaitic inscriptions, they are apparently not connected with any fortified town (Macdonald 2008). When we look at the facts, the argument that writing was limited to the capitals of Israel and Judah during the tenth and ninth centuries B.C.E. seems to contradict the poor epigraphy of this period: so far, we have no inscription from Jerusalem or Samaria and the small inscriptions of this period come from other towns or villages (see above and Lemaire 2007).

Actually, there is no serious reason why historiography could not start in Judah before the eighth century. Na'aman himself recognizes that 1 Kgs 14:25–28, referring to Shoshenq's campaign, depends upon a source (2006, 80), and that "the account of Shishak's campaign in the Book of Kings indicates that there was some kind of scribal activity in the court of Jerusalem in the late 10th century BC..., " activity that was probably "not introduced by a petty king like Rehoboam, but rather by one of his ancestors, either David or Solomon" (2006, 81). However, it is difficult not to qualify the kind of scribal activity at the origin of 1 Kgs 14:25–28 as a kind of historiographical work or chronicle, even though it might have been limited to the royal temple and/or the royal palace.

In fact, it is difficult to find any trace of a special interest in Solomon's reign during the late eighth century B.C.E.: kings Ahaz (2 Kgs 16:2) and Hezekiah (2 Kgs 18:3) are compared to David but not to Solomon, and there is only some indirect connection between Hezekiah and Solomon in Prov 25:1, where "Proverbs of Solomon" is simply the reused title of 1:1. Furthermore, we have no indication that any city of the list of 1 Kgs 9:15b, 17–18 played a special role during Hezekiah's reign. The dating of the "book of the acts of Solomon" to the late eighth century B.C.E. seems to be based only on the *a priori* idea that it could not have been written in the late tenth century B.C.E., and does not seem to have any positive basis.

However, even if we accept this late dating as a working hypothesis, it would still be possible that the author of the "book of the acts of Solomon" employed the list in an earlier document, one that was about two centuries old. If that were the case, the historical appreciation would practically be the same as in our working hypothesis of a late tenth-century author of the "book of the acts of Solomon." The problem is the dating of the list itself: since it mentions Israelite (Hazor, Megiddo, Gezer, Beth-Horon) as well as Judean (Baalath and Tamar) towns, it is probably connected neither with the Israelite kingdom nor with the

Judean one, but has to be dated to the period of the so-called united monarchy.

The text of 1 Kgs 9:19a specifies somewhat the kind of building or rebuilding of these towns. They were “store-cities” (*‘ārēy hammisk^enôt*), “chariot-cities” (*‘ārēy hārekeb*) and “horse-cities” (*‘ārēy happārāšîm*). This explanation and characterization probably does not belong to the original list, but may have been a commentary added by the author of the “book of the acts of Solomon.” The phrase *‘ārēy hammisk^enôt* appears only here in the books of Kings and in the parallel text of 2 Chr 8:4, 6, as well as Exod 1:11. The meaning and etymology of *misk^enôt* has been discussed and is not completely clear; nevertheless, the interpretation “supplies, stores” seems very probable. The parallel mention of “chariot-cities” and “horse-cities” appears to refer to garrison-cities (see the possible parallel of the Aramaic inscription of Zakkur B 2) (Montgomery and Gehman 1951, 209). However, after Würthwein (1985, 112), one may emphasize the limited objective of these new towns: they are called neither “cities of fortification” (*‘ārēy mibšār*) (1 Sam 6:18; 2 Kgs 3:19; 10:2; 17:9; 18:8 etc.) nor “cities of fortification/siege (?)” (*‘ārēy māšôr*)” (Ps 31:22; 2 Chr 8:5; 11:5.10.23 etc.). This probably means that they were administrative centres, eventually garrison cities, but not fortified cities able to resist to a siege. One may note that this limitation of 1 Kgs 9:19 disappears in the parallel passage of 2 Chr 8:5 that emphasizes that these new towns were fortified towns.

The building or rebuilding of the towns during Solomon’s reign is not dated and the length of the reign of Solomon during forty years (1 Kgs 11:42) might be rounded up. The dates of his reign are therefore approximate: ca. 971/970–931. A dating in the second part of this reign (ca. 950–931) is tentative even though it may seem reasonable because it is likely that Solomon gave priority to the constructions in Jerusalem.

The second passage, 1 Kgs 4:15 does not mention Hazor, but may throw some light on its mention in 1 Kgs 9:15: “Ahimaaz in Naphtali; he also married Basmat daughter of Solomon.” The first part of the verse is clearly copied from the list of so-called governors of Solomon, even if these “governors” could have originally been local leaders (for more on this proposal, see Niemann 1993, 27–41, 246–51; 1997, 281–86; 2000, 64–66); the second part of v. 15, the marriage with a daughter of Solomon, is probably an information added by the author of the “book of the acts of Solomon.” This addition, however, reveals a way for Solomon to ensure the fidelity of the “governor” of this region (for this kind of political marriage, see Lemaire 2006b). No town is mentioned for Naphtali in 1 Kgs 4:15, but it is described in Josh 19:32–39, where Hazor

is mentioned (v. 36) as one of the nineteen “fortified cities” (*‘ārēy mibṣār*) of this “tribe” (vv. 38–39).

The list of Josh 19:32–39 may go back to an administrative document dating from the reign of Jeroboam II, more or less contemporaneous of the Samaria ostraca; by this time, Hazor appears as a “fortified city.” The list of the twelve “governors” used in 1 Kgs 4:7–20 is generally considered to go back to an administrative list from the reign of Solomon (Alt 1925, 76; Noth 1968, 58–62; Gray 1970, 129–30; Mettinger 1971, 111–27; Caquot 1972; Würthwein 1985, 43; Rösel 1986; Fritz 1995; Mulder 1998, 169–86; Kamlah 2001; *pace* Ash 1995).

According to N. Na’aman, this claim “can no longer be upheld” because “claims of extensive writing in the tenth-century court of Jerusalem and of an early development of historiography in Israel have been severely criticized” (2006, 102–3), and he proposes a double origin for this list:

The author of the original text, who might have worked in Jerusalem in the late eighth century B.C.E., found an old administrative list of twelve names... He reconstructed the twelve districts...according to the reality of his time. The district system he drew probably reflects a combination of the main outlines of the Assyrian province system of his time and the districts of the kingdom of Judah. However...it is possible that the district list was written earlier, during the eighth century B.C.E., when Israel was still an independent kingdom. The text of the district list must have been part of a more comprehensive historiographic composition, probably the work called “the book of the acts of Solomon”... (2006, 116).

While I would agree that the original document has been completed and inserted in the “book of the acts of Solomon” (see also Fritz 1995, 19 n. 2), I have tried to show above that this book was probably written in the late tenth century B.C.E. However, even in the hypothesis that it would be written during the late eighth century, there is no reason why the original document (without the commentary of the author of the “book of the acts of Solomon” and the later additions) did not contain place names connected with personal names. Both categories, for instance, appear on Hebrew ostraca from Jerusalem (Lemaire 1973, 239–44; Renz 1995, 310–11, *pace* Ahituv 2008, 32–33) and Horvat ‘Uzza (Ahituv 2008, 106), and more generally in ancient Near Eastern administrative texts (Hess 1997). The indication of place names in a list of personal names does not make it a historiographic composition and there is no difficulty in accepting that an original administrative list indicated a name and one (or several) place names(s). Actually, the eventual reconstruction of twelve districts from the Assyrian province system does not seem to fit

what we know of the Assyrian provinces: for instance, we do not know of any Assyrian province such as Naphtali, Asher, Issachar or Benjamin.

The working hypothesis of an original administrative document from the reign of Solomon seems much more simple and consistent with the later developed biblical text, as well as with what we know of administrative documents. In the probable context of Solomon's reign, the original list may throw some indirect light on Hazor in the tenth century, since the (re)building of Hazor in 1 Kgs 9:15 is presented as that of a store-city playing apparently an administrative role; considering the geographical location of this city (Niemann 1997, 276), one may propose that it was the administrative centre of Naphtali (Wright 1967, 67*). Although this is only a conjecture waiting for confirmation, it makes sense of the actual state of the documentation.

A third passage may throw some indirect light on Hazor during the tenth century, namely, 1 Kgs 15:20:

Ben-Hadad listened willingly to King Asa and he sent the officers of his soldiers against the cities of Israel, and they struck Iyyon, Dan, Abel-beth-Maacah, and all Kinnereth, the whole land of Naphtali.

This verse is part of the story of the war between the king of Israel, Baasha, and the king of Judah, Asa (vv. 17–22). As already well observed by Noth (1968, 338; see also Würthwein 1985, 188), these verses apparently go back to some annalistic document, and they fit the ancient Near Eastern context (Gray 1970, 351; Lemaire 1995b, 136–41; Parker 1996, 219, 223 n. 19; Lemaire 2007b). Furthermore, this story reveals that, by this time, the king of Jerusalem had to appeal to the king of Aram against the king of Israel, which does not seem *ad majorem regis gloriam* of the Judean king. It seems, therefore, a reliable source (Pitard 1987, 107–9; Dion 1997, 182–83; Lipiński 2000, 372).

The verb used for the action of the Aramean king, *nākāh*, may have the nuances of “attack, defeat, destroy, subdue” (DCH 5:685). When used with a town as an object, it may indicate not only that the town has been taken, but also that all its inhabitants were eventually killed (Josh 10:28, 30, 32, 35, 37, 39; 11:12; 19:47; Judg 1:8, 25; 20:37; 1 Sam 22:19; 2 Sam 15:14; 2 Kgs 3:19; 15:16 etc.). Even though Hazor is not explicitly mentioned in this verse, it probably suffered from the invasion of this part of the country. Yet there is no indication that this campaign concerned Israelite cities outside Naphtali. Actually, the historiographic tradition of the books of Kings does not specify the political result of this Aramean campaign. Was it a fierce but short military campaign or was Naphtali later on dominated by Damascus? There is no positive indication of an Aramean annexation that could have been expressed by

the verb *qāṣāh* (see, for instance, 2 Kgs 10:32–33 concerning Hazael in Transjordan), and the use of the verb *nākāh* seems rather to indicate that it was only a short campaign to stop the activity of the Israelite army in the south and oblige it to go northwards (see already Noth 1968, 341).

The date of this military campaign is difficult to specify, but it must correspond to a time when both Baasha and Asa were kings. The chronology followed by the books of Kings for this period is not certain, but the dates of 909–886 for Baasha and 912–871 for Asa are probably not far from the truth. That means that this campaign is probably to be dated ca. 909–886. The books of Chronicles present a more precise date, “the thirty-sixth year of the reign of Asa” (2 Chr 16:1), but it is very doubtful that the author of this work used independent ancient sources; and this date was probably proposed for theological reasons (Pitard 1987, 109–14).

To sum up: a critical analysis of the earliest biblical historiographic tradition reveals that Hazor was apparently built as a store-city during Solomon’s reign, perhaps more precisely ca. 950–931, and may have been destroyed by an Aramean campaign in ca. 909–886.

Archaeological Data

The identification of Hazor with the site of Tell el-Qedah, “some 14 km...north of the Sea of Galilee” (Yadin 1993, 594) is generally accepted and has been confirmed by tablets connecting Hazor and Mari (Horowitz and Wasserman 2000, 2004; Ziegler and Charpin 2004; Horowitz and Oshima 2006). There were archaeological excavations there by Garstang in 1928, by Yadin in 1955–58 and 1968–69 and by Ben-Tor from 1990 onwards, excavations that are still ongoing. Since these excavations are now well known, it is not necessary to deal with all the details; instead, I will concentrate on the main points that concern us here.

For the levels of Iron Age II, both Yadin and Ben-Tor agree in distinguishing four phases: Strata Xb–Xa, IXb–IXa (Ben-Tor and Ben-Ami 1998, 3). Especially in the area A4 (gate and casemate wall), “the latest wall of the architectural unit under discussion (Stratum IXa) is sealed by the walls of the pillared building of Stratum VIII, dated to the second quarter of the ninth century B.C.E.” (Ben-Tor and Ben-Ami 1998, 5). As for the historical interpretation, the monumental gate and casemate wall of Stratum Xb were dated to the middle of the tenth century and assigned to the reign of Solomon, taking 1 Kgs 9:15 explicitly as a basis (for instance Yadin 1972, 135), while the larger fortified town of

Stratum VIII was assigned to King Ahab after an apparently violent destruction with “a thick layer of ashes” of Stratum IXa that “may be attributed to the campaign of Ben-Hadad King of Aram in 885 B.C.” (Yadin 1972, 143; Ben-Tor and Ben-Ami 1998, 11). As for a precise historical interpretation of the four phases, Yadin found “it futile to speculate on the possible causes responsible for the changes in the various phases where there is no clear archaeological or historical datum” (Yadin 1972, 143 n. 3).

Yadin went a step further in comparing his finds at Hazor with those at Gezer and Megiddo. This led him to propose that the six-chambered gates of Hazor, Gezer and Megiddo, eventually all connected with casemate walls, “all belong to the same period, and were in fact built by Solomon’s architects from identical blue-prints, with minor changes in each case made necessary by the terrain” (Yadin 1958, 85–86; see also 1972, 147–64). The last detail of this interpretation, “identical blue-prints,” does not seem justified (Milson 1986; Hopkins 1997, 303) and the precise dating of the Megiddo gate is still discussed.

Actually, as is well known now, the general attribution of “massive fortifications” to the tenth century has been questioned during the last twenty years, especially following a paper by G. J. Wightman (1990) proposing a “low chronology.” Wightman recognized that “1 Kgs 9:15 was probably drawn directly from annalistic sources” (1990, 18) and that “Solomon attempted to establish a provincial administration at sites like Hazor and Megiddo, and possibly Gezer... The Solomonic period witnessed the emergence of a prosperous state with an efficient administrative system” (Wightman 1990, 19). However, strangely enough, Wightman rejected the existence of Stratum X at Hazor. For him, the “settlement... enclosed casemate wall with its six-chambered gateway” corresponds “approximately to the excavators’ Stratum IX” (1990, 11).

The issue of the “low chronology” was taken up later on by Finkelstein, who explored its various aspects across numerous articles (1996, 1998, 1999, 2003, [with Piasezky] 2007). This “alternative chronology” has been severely criticized and rejected, with nuances expressed by several archaeologists, especially Mazar (1997, 2003b, 2008), Ben-Tor (2000), Dever (1997) and L. E. Stager (2003) (see also Zarzeki-Peleg 1997; Kletter 2004; Ortiz 2006). I shall not enter here into this general discussion, but will instead emphasize that, during this discussion, two points clearly emerge.

First, for this period (the second half of the tenth and first half of the ninth centuries), pottery dating is generally approximate and very difficult to determine precisely. For instance, according to Mazar (2008,

98) “the Iron Age IIA, characterized by hand burnished and red-slipped pottery, lasted about 150 years, from the first quarter of the tenth century B.C.E. until close to 840/830 B.C.E., namely until the Aramaean wars following the Omride Dynasty,” and Ben-Tor and Ben-Ami emphasized “the difficulty in differentiating between the ceramic assemblages of the tenth and those of the ninth century B.C.E.” (1998, 30). Herzog and Singer-Avitz (2004) proposed an interesting distinction between two phases in Iron IIA ceramic horizon; according to Mazar, however, “this point needs further clarification in future research” (Mazar 2008, 100).

Second, one may now refer to C^{14} dating. Yet, it has to be stated that this technique is also approximate and sometimes problematic: both sides think that C^{14} justifies their interpretation and Mazar has noted that “some of the dates measured in the late 1990s from Tel Rehov at the Weizmann Institute were lower by about 50–100 years than those measured at Groningen on the very samples or samples from similar stratigraphic contexts” (2008, 100 n. 15). Actually, there is not such a large difference between the two historical interpretations, only about fifty years between the end of Solomon’s reign (ca. 931) and the beginning of Omri’s reign alone (c. 881). For non-specialists in the analysis of C^{14} , this approximation is clear when they see that an analysis of the C^{14} results in the dates “895–805/825–790” for Hazor IX (Finkelstein, Fantalkin, and Piasezky 2008, 33: Table 2), while, just one page before, Hazor IX is dated “First half of the 9th century” (2008, 32: Table 1). Actually, “the uncertainties in the C^{14} results” are also emphasized by Finkelstein and Piasezky (2007, 273).

These two considerations reveal that, in the absence of any informative epigraphic material clearly connected with a given level, the historical interpretation of the archaeological data is not certain and must remain a working hypothesis. Furthermore, one has to take seriously into account the fact that each Palestinian city may have had a different story: thus, there is apparently no indication that Hazor had any connection with Shoshenq’s expedition. Furthermore, and conversely, there is no indication that the Aramean campaign of 1 Kgs 15:20 extended beyond Naphtali. In these conditions, for that period, the dates of the Hazor strata might not exactly correspond to strata of other Palestinian towns.

Now, as for Hazor, what exactly is the alternative historical interpretation of Finkelstein? According to him, “Hazor X was built by the Omrides as an administrative center,” though he notes that “from the pottery perspective, a slightly earlier date, in the very late tenth century B.C.E., cannot be excluded” (Finkelstein 1999, 60). Furthermore, the destruction of Stratum IX should be related to the conquests of Hazael in

northern Israel and Stratum VIII was rebuilt and fortified by Hazael (1999, 61). This interpretation has already been criticized by Ben-Tor (2000) from the point of view of archaeology, and has led Ben-Tor (2000, 14) to make the following conclusion: “Finkelstein’s proposed low chronology for the Iron Age in Israel is an interesting and very stimulating working hypothesis. However, it lacks supporting evidence.” As he notes (2000, 14), one of the problems of this alternative interpretation is that it relies extensively on the biblical tradition for the ninth century, but rejects it totally for the second half of the tenth. This seems somewhat inconsistent. Actually, the problem is not that the “low chronology” is selective in its use of the biblical tradition: everybody agrees that there are legendary as well as historical materials in the biblical text and that the historian has to be critical. The real problem is that this alternative chronology rejects biblical historiography as critically evaluated to propose an interpretation that has no basis in the actual documentation. As we have seen above, on the one hand there is apparently no serious reason to reject the historical reliability of 1 Kgs 9:15 and 15:20, mentioning explicitly the (re)building of Hazor and giving a probable historical context for its destruction; on the other hand, the rebuilding and fortification of Hazor by Hazael is not mentioned in the actual documentation and does not seem very likely since Hazael’s control of Cisjordan was short (ca. 814–804) (Lemaire 1991b, 102, 108). Even if, without the light of a clear inscription, the identification of relevant archaeological level(s) is always a matter of probability, this way of arguing does not seem convincing.

In my view, besides the general but approximate dating of the levels on the basis of the ceramic finds and the results of C¹⁴ analysis, another aspect may throw some light on the problem of strata identification. As Yadin already saw clearly, “the Solomonic city occupied only the western portion of the Mound” (1972, 140; see also Ben-Tor et al. 1997, 7–10), it having only casemate walls that could be built without too much effort but which were not so strong if attacked by a well-trained army. Furthermore, this modest administrative city was not built to sustain a siege: apparently there was no way to get water from inside. According to Yadin, “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 strong 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” (Yadin 1972, 165). This double characterization seems to fit well what we know of Solomon on the one hand and the Omride dynasty on

the other. Solomon apparently did not make war and we have seen that 1 Kgs 9:15 probably indicates that the cities that he built were essentially store-cities for administrative purpose. By contrast, Omri was a well-experienced general (1 Kgs 16:16) who subdued the Transjordanian kingdom of Moab (Mesha stele, lines 4–5, 7ff.) and Ahab did not hesitate to lead his army as far as Qarqar against Shalmaneser III (Kurkh monolith). That probably means that he was well aware of the military power of Assyria, whose army was already famous for its siege warfare. In this context, it seems easy to understand that Omri/Ahab built the strongly fortified city of Stratum VIII while the Aramean army of Bar-Hadad I apparently had no difficulty in invading Naphtali (1 Kgs 15:20), Solomonic Hazor being unable to oppose any strong resistance.

Finally, even though it is not completely certain, but only a working hypothesis in the actual state of the documentation, there does not seem to be serious reason to reject Yadin's and Ben-Tor's historical interpretation concerning Hazor in the second half of the tenth century B.C.E. One can only emphasize that this interpretation reveals a relatively modest administrative city that would fit well an early state.

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