

The «Low Chronology» and How Not to Deal With It

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Whereas a majority of Near Eastern archaeologists and biblical scholars may not yet have realized that there has been a controversy concerning the cultural chronology of the 12th through 10th centuries BCE in Palestine, inaugurated by I. Finkelstein in 1996¹, a minority among them does already know² (by the end of 1999) that the debate is settled³: the «Low Chronology» is correct, i.e. the Iron I period commenced ca. 1150 BCE ± 25, and ended ca. 925 ± 25⁴.

¹ Cf. I. Finkelstein, *The Stratigraphy and Chronology of Megiddo and Beth-Shan in the 12th-11th centuries B.C.E.*: TA 23 (1996) 170-184; id., *The Archaeology of the United Monarchy: an Alternative View*: Levant 28 (1996) 177-187; id., *Bible Archaeology or Archaeology of Palestine in the Iron Age? A Rejoinder*: Levant 30 (1998) 167-174; id., *Notes on the Stratigraphy and Chronology of Iron Age Ta'anach*: TA 25 (1998) 208-218; id., *Hazor and the North in the Iron Age: A Low Chronology Perspective*: BASOR 314 (1999) 55-70. – The following remarks refer partially to an oral presentation by Amnon Ben-Tor at Fribourg, 12.11.1999. The arguments proffered on this occasion will doubtlessly also be included in Ben-Tor's forthcoming response to Finkelstein, BASOR 314. It should be stated clearly that Ben-Tor argues as a serious archaeologist whose contribution was duly – and profitably – considered. The same holds true for A. Mazar, whose objections were, however, already dealt with by Finkelstein, Levant 30.

² By unpublished C-14-evidence from three major sites. The present author can neither present the evidence – this will be done by the excavators in due course – nor operate without the knowledge of its existence. Nobody is obliged to believe in evidence which cannot be presented; on the other hand, knowledge spreads in time, and there is always a period when some people know something that other people do not know, and draw some advantage from that knowledge (constructing today, e.g., the theories for after tomorrow). Considering publicly evidence which is not yet available to the public does serve, then, the transparency of the scholarly construct. In addition, due to the time-lag between discovery and publication, and sometimes also due to the quality of archaeological publications some of which conceal rather than reveal the evidence (usually invisibly to readers who never had to excavate and then report on at least one square by themselves), any synthesis by «arm-chair archaeologists» will, as far as Israel/Palestine is concerned, be outdated by its very conception. Even before the final proof in favour of the Low Chronology became known, the new theory led to historically superior results in the interpretation of Megiddo, Hazor, Dan, Kinneret, Jerusalem, the rise of the «United Kingdom» under David and its demise at the end of Solomon's reign; cf., in addition to the references in fn. 1, N. Na'aman, *Historical and Literary Notes on the Excavation of Tel Jezreel*: TA 24 (1997) 122-128; E.A. Knauf, *Kinneret and Naftali*: A. Lemaire ed., *Congress Volume Oslo 1998, II: Historiography* (VT.S; in print); id., *Jerusalem in the Late Bronze and Iron I Periods*: TA, in print.

³ Whereas one C-14-date, as a rule, is useless for questions of chronology within the magnitude of one century or less, a series of C-14-probes from the same stratum may lead to precision in the range of decades. The evidence referred to in fn. 2 consists of series.

⁴ Cultural chronology is always «blurred», being marked by the «contemporaneity of the non-contemporarian». The term «baroque» has another chronological significance in music than in architecture, and, when the last baroque churches were erected in Mexico, classicism was in full swing in northern and eastern Europe. Concerning Iron I, the imprecision allows for its beginning in Transjordan 50 years before the new culture reached the coastal plain, and for a period of 50 years of transition between Iron I and Iron II.

The present note, then, is a *post mortem* on a recent debate, a note which is justified by the methodological lessons to be drawn from the discussion settled. Which arguments, or types of arguments, or strategies of argumentation have turned out to be invalid, once again?

1. «I don't see what I don't want to see»

Whereas the fallacy of this strategy, as far as daily life is concerned, is discovered by most people between their early and late childhood, it seems to linger ineradicably in politics and scholarship. The strategy in question contributed considerably to the fall of imperial China⁵ as well as to the breakdown of communism. In a recent treatment of the Iron I period, erudite and stimulating, but far from comprehensive⁶, the authors make no reference to the controversy, nor to any publication of I. Finkelstein after 1995. Although they accept 1150 for the beginning of Iron I, they still let it end around 1000 BCE. Do they hope that the basic problems and unfounded presuppositions of the traditional chronology, as elucidated in the course of the debate and its predecessors⁷, would simply go away or evaporate if only ignored consistently enough? Or did they by-pass the post-1995 Finkelstein on purpose as a «revisionist»⁸? In this case the present author is surprised to learn that Finkelstein, even he, «dismisses archaeological evidence as an historical source»⁹.

2. The «negligible minority» argument

Those who see themselves unable to ignore entirely a new theory may prefer to marginalize it by counting its adherents vis-à-vis everybody else¹⁰. By necessity, the scholar who formulates a new hypothesis is, at the time of her/his construction or discovery, always in the minority of one. So was Copernicus, so was Darwin. As the case of Darwin demonstrates, massive opposition to a new theory may not at all be based on its inherent weaknesses. Those who deny to this very day that there is evolution both in the macro- and the microcosmos do not do so for intellectual reasons.

⁵ Cf. R. Hoffmann, *Der Untergang des konfuzianischen China. Vom Mandschureich zur Volksrepublik* (Wiesbaden 1980).

⁶ E. Bloch-Smith – B.A. Nakhai, *A Landscape Comes to Life: The Iron I Age*: NEA 62 (1999), 62-92; 101-127. Published within the series of essays «Archaeological Sources for the History of Palestine», non-archaeologists may be tempted to take the article as authoritative – which it is not.

⁷ Notably G.J. Wightman, *The Myth of Solomon*: BASOR 277-278 (1990) 5-22; D. Ussishkin, *Notes on Megiddo, Gezer, Ashdod, and Tel Batash in the Tenth to Ninth Centuries B.C.*: BASOR 277/278 (1990) 71-91.

⁸ NEA 62, 119 n. 2. Labelling and name-calling is already a case of argumentation no. 3, «making a mess» (see *infra*).

⁹ A sub-case of «Not Wanting to See» is partial intentional ignorance of the state of the discussion. At Fribourg, A. Ben-Tor used the destruction levels at Taanach vis-à-vis Shoshenq's campaign as an argument against the «Low Chronology» without referring to Finkelstein, TA 25. A consensus is easily construed as long as one suppresses the dissenting votes.

¹⁰ Cf., e.g., L.G. Herr, *The Iron Age II Period: Emerging Nations*: BA 60 (1997) 114-183, 117; this strategy suggests to the uninitiated (like the average biblical scholar), that «mainstream» is right – it isn't, it's just the sum of the most widely accepted errors of the present. A curious essay in «neutrality» is attempted by B.U. Schipper, *Israel und Ägypten in der Königszeit. Die kulturellen Kontakte von Salomo bis zum Fall Jerusalems* (OBO 170; 1999) 13 with fn. 11 (in an otherwise valuable piece of research), who does not realize that his own assumption (p. 34), that Gezer, Ekron and Gaza were destroyed only once in the 10th century, notably by Shoshenq, is perfectly valid – on the basis of the Low Chronology.

The specious attractiveness of the «negligible minority» argument lies in the fact that it is hard, not just for the uninitiated¹¹ but for the average scholar as well, always to distinguish between the absurd and the genial, nor can anybody deny that the genius who Goethe undoubtedly was did not also produce a heavy amount of absurd «scientific» prose. Now the «Low Chronology» sets itself up for unfounded objection, as did Darwin¹²: the problem is highly complex, and simple solutions do not apply like «Here we have a biblical reference according to which Solomon built Hazor, Megiddo and Gezer, here we have Hazor and Megiddo and Gezer, so let's find out which stratum at each of the three sites might have been built by Solomon, and then, we have a firm chronological anchor for the 10th century»¹³. Further, the new theory does not affect a marginal problem within the field which may be left to a small number of specialists, but a central problem, and forces every participant to rethink her/his previous contributions (some scholars, though not denying that they are human, nevertheless try hard to convince themselves – at least themselves – that they never err). And finally, it affects central biblical figures like David and Solomon and mobilizes in archaeologists and biblical scholars alike non-scholarly and extra-scientific attitudes and convictions. The massive opposition against the «Low Chronology», therefore, does not so much indicate the new theory's improbability rather than the centrality and the virulence of the questions raised.

3. *Making a mess*

It is always easier to defy a new hypothesis or theory by alluding to what it does not imply than by referring to its precise statements or implications (which have usually been tested against the available evidence by the scholar who first proposed the alternative view). One of the most brilliant scholars of our time (in one of the less brilliant moments all humans sometimes encounter) argued against the Low Chronology by implicating that in this case, David, Solomon and Shoshenq would have to be transferred to the 9th century. Some students in this writer's «Intro to Biblical Archaeology» courses usually ask the same question. Traditional dating, based on nothing but conventional assumptions¹⁴, has become to such an extend matter-of-fact that even the most intelligent people might forget that cultural chronology and political chronology are not the same thing. Political chronology, based on (hopefully) precise

¹¹ For the common sense it is simply absurd that, according to Einstein, an object moving 10 km/h within another object that moves with the speed of light does not move faster than the speed of light by 10 km/h. Fortunately, common sense has no say in this matter. Unfortunately, most readers of the Bible think they have a say in everything biblical, including «biblical» history.

¹² It goes without saying that there was also, and still is, well-founded objection to parts of Darwin's theory. Therefore, the present concept of evolution is much more refined than its first proponent could have conceived.

¹³ The historical question – as opposed to the question a biblical scholar is likely to ask – is not, of course, whether the text is absolutely «right» or «wrong», but rather, to which degree it is both «right» and «wrong», and in which specific setting the text refers to a maximum of reality that can be observed within the universe of the total evidence. Thus, for the historical background of 1 Kings 9:15, one arrives at the 8th century (Jerobeam II) or the 7th century (all three places were Assyrian administrative and/or commercial centers). – As for the «argument» that the city gates of Hazor X, Megiddo IVA and Gezer 6, the writer distributed an anonymized copy of Z. Herzog, *Das Stadttor in Israel und in den Nachbarländern* (Mainz 1986) 127 Tab. 8 to several classes of «Intro to Biblical Archaeology» with the task: «3 out of these 5 city gates were supposedly designed by the same architect – which three?». The most frequent grouping returned was Hazor – Lachish – Ashdod; the combination Hazor – Gezer – Megiddo was never selected.

¹⁴ This has already been made clear by the discussion in *BASOR* 277-278 (1990).

dates in the universe of texts, remains what it is. Cultural chronology measures evolutionary processes and therefore, is subject to approximation and statistical probability.

The most effective strategy for making an intellectual mess in which, then, everything goes is name-calling¹⁵. According to oral tradition, the proponents of the Low Chronology were recently labelled «enemies of Western Civilization» by one of their colleagues at a major American convention. It is hard to understand how one can become an enemy of this civilization by exercising one of its noblest rights, the freedom of research and communication, and one of its most meaningful accomplishments: rational, critical scholarship. Name-calling is most effective (though even less respectable) if it creates the bad guys – good guys dichotomy, like «us against the minimalists» or «us against the maximalists» (one finds this strategy on both sides), thus simplifying the «non-us» world by denying to the «enemies» their specific individual profiles. That some of those called «minimalists» by somebody do not understand enough archaeology to use it properly does not imply that all of them do not. That some scholars do not agree with what W. Dever thinks constitutes the archaeological evidence for the 10th century does not imply that they «disregard archaeology» – some may have archaeological reasons for their disagreement¹⁶.

In order to overcome the unpleasant (and rather sterile) maximalist-minimalist dichotomy, the following trichotomy is proposed: there are some, let us call them «maximalists», who insist that the Bible always contains reliable historical information unless proven otherwise; by «always», this group introduces a dogmatic, ideological element into its argumentation, a statement beyond empirical prove or disprove. The second group, the «minimalists», is as dogmatic as are their opponents: the Bible never contains reliable historical information unless proven otherwise by extra-biblical evidence. The third group are the rationalists, empiricists – or simply, the scholars: the Bible may or may not contain reliable historical information, with extra-biblical corroboration or without, and there is no *a priori* to decide the matter, so one has to argue in any case whether one accepts a biblical statement as historical or not. To disbelieve, again and again, all theories and all evidence and above all, her/his own thoughts, and only to work with those items that had stood, again and again, the purgatory of logical and empirical testing, is the basic criterium for being a scholar. Believe is a virtue in the church, but a deadly sin in scholarship. Theologians, being scholars and people of the church in personal union, may have difficulties from time to time to distinguish in which character they are speaking.

¹⁵ A. Ben-Tor, on November 12th, 1999, came dangerously close to an *ad-personam*-argument when he debated Orna Zimhoni's and Nadav Na'aman's conclusions concerning the contemporaneity of Megiddo VA/IVB with Omride Jezreel on the grounds that there were no «well-defined» loci in the Jezreel excavations. This author admires the skill and the care both in the excavation and in the interpretation of this difficult site; cf. D. Ussishkin – J. Woodhead, Excavations at Tel Jezreel 1994-1996: Third Preliminary Report: TA 24 (1997) 6-72; O. Zimhoni, Clues from the Enclosure-Fills: Pre-Omride Settlement at Tel Jezreel: TA 24 (1997) 83-109. Hobby-archaeologists may need walls and floors to define loci; professionals are well able to recognize the surviving fragment of a floor even if the walls are gone, and distinguish the pottery from above the floor from the ceramics from beneath.

¹⁶ The attempt of W.G. Dever, Archaeology and the "Age of Solomon": A Case-Study in Archaeology and Historiography.; L.K. Handy ed., The Age of Solomon (SHCANE 11; 1997) 217-251 is not only futile from the point of view of the «Low Chronology» (from his Ranked Settlement List, p. 219, Hazor X, Megiddo VA/IVB, Taanach IIA-B, Far'ah VIIb, for instance, move to the 9th century), but is also based on insufficient information drawn from preliminary reports; Tel Kinrot V, e.g., does not cover 1.25 hectares, but 9-10 ha; it is Iron I/II transitional, and certainly not an Israelite settlement. For Tell el-Kheifeh I, there is not a single piece of evidence to support a 10th century occupation.

Conclusion: if you want your cat to appear as grey as any other cat, make sure (or pretend) it is night. If you are interested in the variety of cat forms, types, sizes and colours, look for enlightenment.

4. The «dense stratigraphy» argument

The most pertinent argument against the Low Chronology has been the «dense stratigraphy argument». There is consensus that Hazor V was destroyed by Tiglatpileser III. in 733 BCE. If Hazor X was founded by Solomon ca. 950 BCE, as maintained by traditional chronology, this leaves 217 years for 6 strata, i.e. an average of 36 years per stratum. If Hazor X was erected by Omri ca. 875 BCE, only 142 years are left, which makes 23 years per stratum. Although the average duration of a stratum has never been discussed, for some time the number of 50 years has formed a conventional basis of calculations. On the grounds of this assumption, traditional chronology appears, in the case of Hazor, to be the more realistic one.

So it is time to calculate the average duration of a stratum in the Iron Age Galilee. The use of the term «stratum» in the archaeology of Palestine presupposes the notion of global destruction of a town or city; the entire site has to be rebuilt. There are only two possible reasons for such global destructions: earthquakes and conquests. The probability of earthquakes in Galilee can be calculated on the basis of Ken Russell's catalogue¹⁷: a major earthquake is to be expected every 56 years (standard deviation $\sigma = 21$). Within the limits of 1σ , one has to expect between 3 (2.8) and 6 (6.2) earthquakes at Hazor between 975 and 733, and between 2 (1.8) and 4 within the period 875-733 BCE.

To the strata produced by earthquakes, the strata resulting from military destruction have to be added. According to the written sources, Hazor changed hands between 900 and 733 on the following occasions (an asterisk* marks conquest that were, according to the source, accompanied by destruction):

ca. 890*	Damaskus conquers the Galilee	(1 Kings 15:20)
ca. 875	Omri occupies the Galilee	(Tel Dan Stela)
ca. 840*	Hazaël conquers the Galilee	(Tel Dan Stela)
ca. 790*	Joash or Jeroboam II. reconquers Galilee	(2 Kings 13:24f; 14:28)
733*	Tiglatpileser III. devastates the Galilee	(Assyrian records; 2 Kings 15:29)

Between Solomon and Tiglatpileser, Hazor was conquered 4 times and destroyed by earthquakes 3-6 times. One should expect, then, 7 to 10 strata. Between Omri and Tiglatpileser, Hazor fell victim to conquerors 3 times, and to earthquakes 2 to 4 times, resulting in 5-7 strata. The 6 strata of Hazor attested in the Iron IIA/B periods agree completely with the historical expectations based on the Low Chronology.

¹⁷ Cf. K.W. Russell, *The Earthquake Chronology of Palestine and Northwest Arabia from the 2nd through the Mid-8th Century A.D.*, BASOR 260 (1985) 37-59, 39 Table 1. Only the quakes from 306 to 757 have been used, because there obviously is a gap in the documentation for the 2nd and 3rd centuries CE. Also, the quake of 672 has been disregarded for the reasons given by Russell. One should be aware that this catalogue contains less earthquakes than the previous one by D. Amiran. It is highly unlikely that earthquake frequency has changed between the 10th through 8th centuries BCE vis-à-vis the 4th through 8th centuries CE. Earthquakes are a consequence of plate tectonics. The Arabian plate, with Transjordan, moves 0.02 m to the N p/a, as compared to the Mediterranean plate with Cisjordan. This makes 36 m in 18 centuries - not enough to create completely new geological constellations.

Given that an earthquake might well strike while a site is still under construction¹⁸, or that a fortress may easily have been dismantled briefly after its foundation (cf. 1 Kings 15:17. 21-22), it is rather the opposite of the «dense stratigraphy» assumption that should, from now on, be met with suspicion. Earthquake chronology does, in any case, finally refute all attempts to attribute any Megiddo other than Megiddo VIA to the 10th century. Because Megiddo IVB was destroyed in 733, the attribution of Megiddo VA/IB to Solomon¹⁹ would assign 192 years to stratum IVB. This assumption is well beyond the 3 σ limit of earthquake frequency, i.e. it has a probability of less than 1%.

5. *The Negev sites argument*

Another serious argument against the Low Chronology has been advanced by B. Halpern²⁰: Shoshenq campaigned in the Negev, as is evident from his topographical list, in which he claims to have conquered up to 90 places²¹. The 47 Central Negev Highland sites, variously dated Iron I, Iron I/II transitional, or Iron IIA (traditionally = 10th century) may or may not have been affected by Shoshenq's campaign. It is widely assumed that they were destroyed by Shoshenq²². Burnished red slip pottery is «frequent» in these peripheral sites. Therefore, the occurrence of burnished red slip in non-peripheral sites like Gezer must antedate Shoshenq's campaign and attribute these strata actually to the time of Solomon²³.

Halpern follows J.S. Holladay, who had tried in a series of stimulating, though not entirely conclusive articles to characterize Saul, David and Solomon archaeologically as «pre-red slip, unburnished red slip and burnished red slip kings»²⁴. Holladay may overestimate the length of the reigns of David and Solomon, and underestimate the complexity of the spatial and temporal distribution of unburnished and burnished red slip pottery. Any statements on the «frequency» of burnished red slip at this, that or the other site are personal statements and impressions which are interesting in relation to the pottery expertise of the person who makes them and of the amount of the pottery in question that she/he has actually seen. As long as not enough pottery from this, that or the other site is published, not to speak of pottery statistics, the «frequency» of a pottery type in specific regions during the 10th century is nothing that can be discussed for the time being. Burnished red slip is, for example, also attested at Megiddo VIA²⁵ and Ekron IVB (two late Iron I strata).

¹⁸ This may have happened to Kinneret VI.

¹⁹ Cf., e.g., W. Dietrich, *Die frühe Königszeit in Israel* (BE 3; 1997) 123 Abb. 6; *ibid.* 125 Abb. 7, he even assigns Megiddo IVB to Solomon – thus producing a stratum of ca. 220 years.

²⁰ B. Halpern, *Research Design in Archaeology: The Interdisciplinary Perspective*, NEA 61 (1998) 53-65, 62f.

²¹ Cf. for the most recent historical treatment of the Shoshenq list N. Na'aman, *המצעדות המוקדמות והמבצעים הארכיאולוגיים*, *מסע שישק לארץ ישראל בראי התרבות* (Zion 63 (1998) 247-276, 250-268. The 90 entries for the Negev have to be reduced to 78 by the «two entry entries» like 71 *ḥql 72 'byrm*, or even to 77 if one counts 110-112 as one place: *'rd n bt-yrhm* (*n* is the Late Egyptian relative particle).

²² This, however, would furnish another argument in favour of the Low Chronology, as I. Finkelstein maintains, because, in his opinion, the Negev Highland Sites are contemporary with Tel Beersheva VII and thus, antedate Beersheva V and Arad IX, supposedly destroyed by Shoshenq according to the traditional chronology; cf. already Y. Aharoni, *Erlsr* 15 (1981) 191-192.

²³ For the «Solomonic Gezer» debate, the «red slip» argument has been overruled by stratigraphy, cf. I. Finkelstein, *Penelope's Shroud Unravelling: Iron II Date of Gezer's Outer Wall Established*: TA 21 (1994) 276-282.

²⁴ J.S. Holladay, *Red Slip, Burnish, and the Solomonic Gateway at Gezer*: BASOR 277/278 (1990) 23-70; *id.*, *The Kingdoms of Israel and Judah: Political and Economic Centralization in the Iron IIA-B* (ca. 1000-750 BCE); T.E. Levy, *The Archaeology of Society in the Holy Land* (New York 1995) 368-398.

²⁵ Well in addition to the curious pot discussed at length in O. Keel, *Studien zu den Stempelsiegeln aus Palästina/Israel IV* (OBO 135; 1994) 5-7; 52.

The wide range of different datings and interpretations of the Negev sites illustrates well the degree of precision – or imprecision – to be reached by the means of traditional archaeology. Mshash II may be contemporary with the Negev Highland sites²⁶. It is not clear whether Mshash II was destroyed by an earthquake, by enemies, or by both²⁷. Or, Mshash I might be contemporary with the Highland sites²⁸; the end of Mshash I is even less clear than the end of Mshash II. It is not clear whether the Shoshenq list refers to one campaign from the end of his reign²⁹, or represents a summary of campaigns which may have started years before and might even have continued after this Pharaoh's death³⁰. For the relationship between Shoshenq and the Negev sites, several possibilities exist:

(a) Mshash II (also a burnished red slip, even early Cypro-Phoenician and post-bichrome site!) and the Negev Highland sites were abandoned well before Shoshenq's visit to the area. This assumption is feasible within both the traditional and the Low Chronology (the latter suggests a date for the abandonment between 950 and 925). In any case, the flourishing and the demise of Khirbet el-Mshash II and of the Negev Highland sites formed part and parcel of one and the same historical process. This possibility receives support from the Shoshenq list, because the Pharaoh does not refer to Beersheva (Bir es-Seba'), nor to Ziklag, nor to Horma in his list³¹, although his army must have followed the Nahal Beer-Sheva (Wādi s-Seba') on its way to Arad³².

²⁶ Z. Herzog, *The Beer-Sheba Valley; From Nomadism to Monarchy*: I. Finkelstein – N. Na'aman, *From Nomadism to Monarchy. Archaeological Aspects of Early Israel* (Jerusalem 1994) 122-149, 138f.

²⁷ For the destruction of Mshash II, A. Kempinski: V. Fritz – A. Kempinski, *Ergebnisse der Ausgrabungen auf der H̄irbet el-Mšāš (Tel Mašōš)* (ADPV; 1983) I, 75 thinks of an earthquake, V. Fritz and A. Kempinski (*ibid.*, 230) opt for conquest (and know even, by whom), and Herzog, *op. cit.*, 134 finds «more reasonable ... a combination of both events». Because no meaningful section drawings are presented, the reader is at a loss.

²⁸ Cf. D. Jericke, *Die Landnahme im Negev. Protoisraelitische Gruppen im Süden Palästinas. Eine archäologische und exegetische Studie* (ADPV 20; 1997) 71. His dating is not sound, being based on comparisons of isolated vessels (*ibid.*, 130; 146) without regard for the sites' assemblages and the types' life-spans. In any case, one has to compare Mshash III with Tel Beer-Sheba IX and VIII (Philistine pottery!), and Mshash II with Tel Beer-Sheba VII and VI (post-Philistine).

²⁹ The common assumption, most recently repeated by Na'aman, *Zion* 63 (1998), 265-267.

³⁰ Thus H.M. Niemann, *The Socio-political Shadow of the Biblical Solomon*: L.K. Handy ed., *The Age of Solomon. Scholarship at the Turn of the Millennium* (SHCANE 11; 1997), 252-299, 297. That Shoshenq's control of Palestine was not just an episode soon to be forgotten, but must have extended for some time, is also attested by his cartouche becoming a «sign of power» to be repeated on Judean and Israelite seals well into the 8th century, cf. Ch. Uehlinger in O. Keel – Ch. Uehlinger, *Göttinnen, Götter und Gottessymbole* (QD 134; 1997) 536f. Even if the argument that Sheshonq (946-925) was already dead in the 5th year of Rohobeam (921) is not valid, because its proponents overlooked that any precise dating of Shoshenq depends on 1 Kings 14:25, and the difference is easily explained by the use of different reconstructions of Judean absolute chronology by different authors (Schippers, *OBO* 170, 120f; Na'aman, *Zion* 63, 270f), it stands to reason that (a) most of Shoshenq's reign overlapped with Solomon's, and (b) the rise of Jeroboam I. and the establishment of a Northern kingdom independent from Jerusalem must have commenced well during Solomon's rule (between 950 and 925). On the basis of Uehlinger's observation of the lasting impact of Shoshenq's cartouche, one may assume that an Egyptian army haunted Jerusalem indeed in the 5th year of Rohobeam, but no longer under Shoshenq's supreme command. The absence of Jerusalem from the Karnak list remains decisive, which cannot be explained away by the lacunæ: the part where one would expect Jerusalem to appear is perfectly preserved (between #23 and #22).

³¹ David's Ziklag has not yet been identified with a sufficient degree of probability (Tel Beer-Sheba V, the choice of V. Fritz, *ZDPV* 106 (1990) 78-87, is post-Davidic); nevertheless, one does expect the name to appear in Shoshenq's list. For Horma, most probably the ancient name of Khirbet el-Mshash, cf. Y. Aharoni, *The Land of the Bible* (ed. A.F. Rainey; Philadelphia 1979), 215f; the objections raised by V. Fritz, *Horma*: *NBL* II (1995) 195f against Aharoni do not hold water even within the chronology as proposed by the excavators of Khirbet el-Mshash, and surely not within the Low Chronology: if the site was abandoned at the beginning of the 10th century, it still existed during the reign of Saul, and if it was resettled in the 7th century, it existed during the time of the Judean monarchy. In the Shoshenq Negev list, 16 out of its 90 (77) names are completely lost. The probability that one of the three missing names is among the entries now missing is 20.8%, the probability that all

(b) Mshash II, Beersheva VII, Arad XII and the Negev Highland sites were destroyed by Shoshenq. This option is only possible within the framework of the Low Chronology. Mshash I, Tel Beer-Sheva VI and V, and Arad XI would reflect, then, a post-Shoshenq Judaean occupation of the northern Negev.

(c) Mshash I³³, still antedating Tel Beer-Sheba V and Arad XI, was destroyed by Shoshenq: still an assumption within the limits of the Low Chronology.

The already complex situation is exacerbated further by the possibility, that Shoshenq did not destroy a single site, but was satisfied by receiving as much tribute as possible from any group of natives his army encountered, and, having them registered as newly won subjects of Egypt, let them live in peace. In this case, the widespread destructions ca. 925 BCE may attest to turmoil in the area that had set in well before Shoshenq's campaign(s) (and may have inspired the Pharaoh to try his luck, too), or to violence arousing after his campaign(s) because of the destabilisation of the previous social and political system.

6. What needs to be done

The question raised by Holladay and Halpern – the chronological and/or regional significance of the burnished red slip ware – cannot be addressed, for the time being, because we do not have the data. Today, there is not a single published stratum which gives us reliable information about the percentage of burnished red slip pottery vis-à-vis unburnished/non-red slip pottery of the same functional category. In the case of Mshash II and I and the Negev Highland sites, there is no published evidence which allows the reader to decide whether they ended in conquest, earthquake or abandonment³⁴. A statement of the excavator's (or excavators') opinions simply is not enough. Excavators are not judges of their sites, nor can they dictate their readers' opinions on what they have found; they are attorneys: it is their responsibility to present the evidence for what they think has happened at and to their sites to the jury which is constituted by their peers.

The penetration of burnished red slip pottery in Palestine in the course of the 10th century well may serve as an indicator for the growing impact of Phoenicia on the economy of its *hinterland*. But this, for the time being, is nothing but a working hypothesis that needs to be tested. In order to be tested, statistical information from the side of the pottery experts of the Iron I/II transitional strata presently under excavation or publication is urgently needed. That much can be stated: excavations that don't count their pottery produce excavation records that won't count in future discussions. What we need are data that can be scientifically processed. What we do not need are further attempts to burden history, an empirical cultural science, with beliefs or dogmatics.

three were in the destroyed sectors is $(0.208^3 =) 0.89\%$. In addition, as far as a geographical order in that part of the list is discernable, from #125 (Sharuh) onwards, the recognizable toponyms are far to the West of Beer-Sheva. So the only remaining lacuna for the three names – in sequence – is #113-115 (probability: 0.00014 % $(1/90^3)$). The settlements – and, mostly, encampments – «conquered» by Shoshenq have probably to be looked for north of the Highland Sites.

³² Na'aman, Zion 63, 262 and the map p. 263. Or were the Egyptian scribes misled by their local informants, who gave them their grandmother's maiden name instead of the toponym of their dwelling place?

³³ But cf. already Holladay, *Kingdoms*, 383 for a «Solomonic» date of Mshash II. The grouping above follows Herzog; the excavators (Fritz – Kempinski, *Ergebnisse* I, 231) put the end of Mshash I before Arad XI. – It is most ironic that the site central for V. Fritz' theory of the «Israelite settlement» (most appreciated by W.G. Dever and his disciples) now finds its place within the rise of the early Israelite and Judaean states in the 10th century.

³⁴ In the case of Mshash I, the excavators opt for abandonment: Fritz – Kempinski, *Ergebnisse*, I, 230.