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Low and Lower?
New Data on Early Iron Age Chronology from Beth Shean, Tel Rehov and Dor

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1. Introduction.

New and decisive ^{14}C data for LBII through Iron IIA strata have just been presented by A. Mazar and I. Carmi[1]. Before taking issue with their interpretation of the data, I wish to express my gratitude to the authors for the complete and impeccable presentation of the evidence, and to the two expeditions involved for not shunning the expenses of the tests. This is the stuff of which scientific progress is made[2].

On various occasions I have heard one of the authors refer to these dates as forthcoming proof for the Traditional Chronology, especially on the basis of various short-lived samples. A superficial reading of the paper as published might convince interested parties of this opinion. But it is wrong. *All data of short-lived samples from Rehov and Beth Shean are in favor of the Low Chronology*, in one decisive case *to the exclusion of the Traditional Chronology* (see infra, § 4).

2. On points and periods: some preliminary remarks.

In order to achieve as much clarity as possible, it is imperative to strive for as much clarity of thought and expression as attainable. Let us assume the transition from IBA to MB I happened «ca. 2000 BCE». Most archaeologists would agree, but what does «ca.» mean:

$$-2 * 10^3 = -2.5 < -2 \leq -1.5 = 2499 - 1500?$$
$$-2.0 * 10^3 = -2.05 < -2.0 \leq -1.95 = 2049 - 1950?$$
$$-2.00 * 10^3 = -2.005 < -2.00 \leq -1.995 = 2004 - 1995?$$

And, do we think, considering this transition, of an exact year defined by an interval because of insurmountable difficulties in measuring this specific moment more precisely? In this case, I will write

$$2050/1950 \text{ or } 2000 \pm 17.$$

Or do we think of a period of transition, which is the correct thing to do in cultural history, given that MB I might have started at Tell el-Maǧnūn eš-Šarqî at little bit earlier than at Ḫirbet Zibb eṭ-Ṭawîl, and I now will write

$$2050\text{-}1950 \text{ or } |2000 \pm 17|.$$

The ± will always indicate the standard deviation unless stated otherwise (e.g., as in $|2000 \pm 50|$ [$50 = 3\sigma$] for the sake of those historians who are still insufficiently familiar with the standard distribution and its appurtenances and who can safely disregard the content of the brackets).

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[1] A. Mazar & I. Carmi, Radiocarbon data from Iron Age Strata at Tel Beth Shean and Tel Rehov: Radiocarbon 43/3 (2001) 1333-1342. If numerical notations in the text differ from the same data as given in the tables, I always follow the tables.

[2] There is no point in responding to S.M. Ortiz, Methodological Comments on the Low Chronology: A Reply to Ernst Axel Knauf: BN 111 (2002) 34-39, since there seems to be more agreement than disagreement between the two of us. I might note, though, that Ortiz argues on the basis of data as known in early 2000 (cf., for the meantime, BN 103 [2000] 30f n. 2 and 3). I also fail to see what pottery sequences can do to solve the riddle (at least for the present); no typology and typological distribution of Ekron pottery can answer the question of whether Ekron IV was destroyed ca. 1000 or ca. 925.

It might indeed be advisable to calculate probability ranges for «ca.»-dates based on historical evidence, e.g. 1134 ±4 for the end of Megiddo VIIA instead of «ca. 1140». By my notation I express the opinion that the destruction of Megiddo VIIA happened with equal probability during the reign of Ramses VI or shortly thereafter[3], and attribute a probability of 95% to it happening between 1142 and 1126. The date as such, of course, is not really more precise now than it was before, but what I think of it is expressed with much more prescision.

The difference between points and periods must be kept well in mind when ^{14}C data for cultural horizons are accumulated. The variation in data based on grain samples from the same destruction level can safely be regarded as resulting from errors in measurement inherent in ^{14}C-dating, and these errors can be minimized by established procedures[4]. All the grain burned in the year of the destruction was most probably harvested in the same year, or the year before. Based on the nature of the sample, ^{14}C dates might refer to the construction of a stratum (timber), to its duration (olive pits from floor sequences), or to its destruction (charred grain in the destruction debris). The differences will be expressed by the following notations:

Megiddo VIB° refers to the construction of VIB.
Tel Rehov $D(6\text{-}3)^{d}$ refers to evidence from the life span of strata D6 through D3.
Tel Rehov V^{t} refers to the destruction of Stratum V.

^{14}C dates from several sites belonging to a cultural horizon, or from quite different samples from the same stratum, can, however, also be accumulated in order to look for the maximal systemic probability. If a number of human activities during a well-defined period are known by a number of normal distributions N_i, and these overlap in a–b (fig.1), than the accumulated systemic probability for human activity evidenced by all the distributions covering the period a - b is

$$\frac{1\Sigma^{n}\ {}_{a}\int^{b} N_i}{\bar{n}}$$

Each radiocarbon date (in the 2σ range) is regarded, in a simplistic but pragmatic manner, as probabilitydistribution for a human action having taken place during the period as defined by the date. Calibrated ^{14}C dates do not accord to the bell curve any longer. The overlap of the $2\sigma_i$ ranges can serve as a first approximation for where to look for the maximal systemic probability, especially if all (or most) $\mu_i \in a'–b'$. This simple – or even simplistic – approximation surprisingly leads to better results (from a historical point of view) than much more sophisticated approaches[5], at least in a number of cases. If the two distributions of fig. 1 would represent the ^{14}C dates of two arte- or ecofacts, fig. 2 represents the probability of human activity indicated by either one of them.

Even if the calibrated range of a ^{14}C-date does not follow the bell curve, it can still be emulated by that curve without running into an intolerable margin of error. If it should become necessary to calculate the 3σ range based on the 2σ range (as it will in the course of this paper), the error, especially concerning the media, is minimal in the case of the data for Rehov's Iron I period (§ 4.1):

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[3] Note, however, that Ramses VI may have reigned 1142-1134 or 1140-1132; J. von Beckerath, Die Chronologie des pharaonischen Ägypten (Mainz 1997) 103-106. What is certain, in any case, is the fact that his statue base cannot have shown up at Megiddo before 1142/1140 (and see further *infra*, § 3 and 4.2).

[4] Mazar & Carmi, Radiocarbon 43, 1334 and 1338. I now doubt that the same approach was justified in the case of I. Carmi, Radiocarbon dates: Megiddo III, 502f (see *infra*, § 4.2).

[5] See infra, § 4.3 with n. 12.

Sample	1σ		→3σ	2σ		→3σ
D3	895/795	«845±50»	945/745	900/700	«845±23»	923/767
D4	1000/900	«950±50»	1050/850	1050/830	«940±55»	1105/775
D6	900/800	«850±50»	950/750	920/790	«855±33»	953/757

A final remark must be made on levels of significance. Mazar & Carmi state: «Even when 1σ range dates provide close dating, there is always the option of the legitimacy of the 2σ range dates» (p. 1341). No, there is no «option». In history and archaeology, a probability of 95% is excellent (2σ range), a probability of 99% (3σ range) usually unattainable, and a probability of 80% still acceptable. A 95% probability of scientific results means that no more than 1 statement out of 20 is wrong. In medicine, a probability of 99% for a new drug against the common flu not having lethal side effects is unacceptable (1 out of 100 consumers of that drug would die of it). A probability of 68% (1σ range) is useless: one statement out of three would be wrong, and I do not think that a historical theory with this known characteristic (even if we do not know for the present which 32% of it is wrong) does not necessarily deserve consideration.

3. Beth Shean at the end of the LB period.

Beth Shean $N4^t$ dates to 1220/1120 BCE (88% probability). According to the description of the find context, it is immediately under Beth Shean Lower VI, i.e. in Beth Shean VII, a stratum which is by universally attributed to LB IIB. This is wonderful confirmation for the Low Chronology, which has LB ending 1125 ±25 (25 = 3σ). The traditional beginning of the Iron Age, ca. 1200, is not excluded by this date, but less probable than the lower date.

Beth Shean $S3a^d$ dates to 1260/1040 (95% probabilty). S3a is evidently part of Beth Shean Lower VI, which belongs to the LB IIB/Iron IA transition, not yet to Iron I^6. The range is so broad that it agrees with any chronology. The ^{14}C date for Beth Shean S3a is no proof for Iron I beginning prior to 1150.

What is worse for the traditional chronology: both dates do not only overlap, Lower VI^d encompasses VII^t. Negative systemic accumulation is called for. In positive systemic accumulation, the individual means attract each other and transfer probability from the fringes to the center (see *supra* , § 2 and fig. 2); in the case of negative accumulation, the means repel each other and shift probability from the center to the fringes (fig.3). We just happen to know that Beth Shean VII must precede Lower VI. The probability for VII decreases proportional to the increase of probability for Lower VI. A simple – and simplistic , but pragmatic – procedure takes the mean of the two means (μμ) for the separation of the two probability fields. $N4^t$ (VII, LB IIA) then dates to 1220/1160 (88% probability), and $S3a^d$ (Lower VI, LB IIB/Iron IA) to 1160-1040 (95% probability)[7]. The «mean of the means» is, of course, not a historical (absolute) date, but a statistical («fluffy») date. It is useless to calculate the standard deviation for a population of n=2, so it might historically be estimated as ±3, leading to a «separator» 1160 ±10 (10 = 3σ).Regarded together, these dates argue against rather than for the beginning of the Iron Age at Beth Shean prior to 1150.

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[6] Cf. I. Finkelstein, The Stratigraphy and Chronology of Megiddo and Beth-Shan in the 12^{th}-11^{th} Centuries B.C.E.: TA 23 (1996) 170-184, 173-176. H. Weippert, Palästina in vorhellenistischer Zeit (HdA II/1; 1988) 286-288 and 355, goes as far as claiming both Beth Shean VII (1250-1175/50) and VI (1175/50-1075 – her *traditional* chronology) for the LBIIB.

[7] It is simply impossible that human activity which led to datable deposits in Stratum VII postdates human activity depositing organic material in Stratum Lower VI, the possibility of intrusive objects disregarded. One also needs to keep in mind ` that the sum of the total probability within any distribution of it has to be 1.

4. Tel Rehov Iron I[d]: 950-900 or 920-830?

4.1. The data. Three olive pits from three floors belonging to Iron I Rehov gave the following sequence of dates: $D6^d$ = 920/790; $D4^d$ = 1050/830; $D3^d$ = 900/790 (only the 2σ ranges being considered). D6 is older than D4 ; D4 is older than D3. Because of the anomaly that D4 seems to be older according to the ^{14}C than D6, and because these data appear to the authors as far too low, Mazar & Carmi throw them out: another method of how not to deal with evidence for the Low Chronology. Please keep in mind that 1 out of 20 true dates is expected to lie outside the 2σ range, and that in 19 out of 20 cases it lies somewhere whithin the range, but by no means necessarily in the middle of it. Within the three 95% reliability ranges, it is between 920 and 830, that D6 could precede D4, and between 900 and 790, that D4 can be older than D3. Treating the calibrated distributions approximately as normal, the 3σ range of overlap (99% probability) would reach from 950/953 to 850/775[8]. Whether these dates are «too low even according to the "low chronology"», as Mazar & Carmi state (p. 1335) requires further consideration (see *infra*, § 4.3).

If the three probability distributions are separated according to § 3 (fig .3), and 3σ is calculated on the basis of the calibrated 2σ range, the separators are 893 and 888, which gives $D6^d$ = 950/893, $D4^d$ = 893/888, and $D3^d$ = 888/775 with an accumulated probability of 99% each.

4.2. Excursus: transfer of probability and historical recalibration. Imagine the following situation: A quizmaster presents a candidate with three closed doors. A stack of gold waits behind one of them, a heap of straw behind the other two. The candidate pronounces her/his choice – phase 1. Now the quizmaster opens one of the two unmarked doors; it opens to straw (the quizmaster knows where the gold is) – phase 2. Now the candidate has the chance to transfer his/her choice to the third door, not chosen in (1) and not opened in (2). In this game – as in real life – intelligent candidates change their mind because new evidence came up – phase 3. But why should they?

In phase 1, each door has a 33.3 % probability of hiding the gold, each pair of doors a 67.7 % probability. By opening one of the two unmarked doors in phase 2, the probability of gold behind it drops to 0. In phase 3, the last door now «inherits» the combined probability of the pair of doors left after phase 1 (the quizmaster cannot open the candidates first choice, which contains the gold in one out of three cases, nor can he open the door leading to the gold in the 66.7 % of cases in which the candidate did not hit pay dirt with his first choice)[9].

We can apply this logical structure whenever historical evidence allows attributing 0 probability to some part of a ^{14}C-date or another probability range. To return to the destruction of Megiddo VIIA: the 99% (3σ) range is 1142/1122 and *not* 1146/1122, because Ramses VI could not have sent his statue(s) abroad before ascending to the throne (but in the re-calibrated range, the distribution of probability is no longer normal: the medium becomes the maximum with the median on the side of the «long end» of the distribution). By the same token, the (historically re-)calibrated date for the construction of Megiddo's LB gate is 1202-1126 and *not* 1202-1075[10], because the gate cannot possibly have been constructed after the destruction of its city. If we could be sure that no Iron I pottery was produced by any Israelite or Canaanite after 900, we could re-calibrate the 2σ range of Rehov D6-D4 to 920-900, and the 3σ range to 950-900. But can we be sure?

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[8] The differences reflect whether the (fictive) standard deviation for the 3σ range is taken from the calibrated 1σ or 2σ dates (see *supra*, § 2).

[9] In order not to appear more intelligent than I unfortunately am: the problem and its solution were explained to me by Baruch Halpern during an extended nocturnal conversation at Kibbutz Mizra, autumn 1999.

[10] Carmi: Megiddo III, 502f.

4.3. Rehov and Dor. In the same issue of *Radiocarbon*, A. Gilboa and I. Sharon published ^{14}C evidence from Dor[11]. If traditionalists have a personal problem with the «Low Chronology», now there is a real problem, for Gilboa & Sharon introduce the «Very Low Chronology»: Dor Iron Ib (= Iron IC Megiddo VIA) dates to 975-870, and Iron IIA starts around 850. Because the primary data, their contexts and Sharon's formula are still unpublished[12], a discussion of the «Very Low Chronology» is not yet possible. But the measured dates for Rehov Iron I are in perfect agreement with it – another reason not to discard them for the time being.

5. Iron Age IIA° at Beth Shean and Rehov: 925/900.
For the beginning of Iron IIA, only charred timber is available. Dates based on timber tend to be problematic, and in this case, they come as ugly as they can be.

Stratum	**Probe**	**Date (2σ)**
Beth Shean S1	RT 2734 (1)	1270/1040
Beth Shean S1	RT 2733 (2)	1130/890
Tel Rehov V	RT 2995 (3)	1430/1210
Tel Rehov V	RT 2997 (4)	1130/930
Tel Rehov IV	RT 2996 (5)	1000/830

Probes (2) and (5) agree with both the traditional and the low chronology. Probe (4) matches only the traditional chronology. Probes (1) and (3) are far too high for either chronology. Mazar & Carmi propose that the wood derives «from an old olive tree or taken from the inner part of the tree trunk, where cells could die long before the tree was cut down» (p. 1336f). I have no ethno-archaeological data concerning the use of dead wood for construction at my disposal. A more probable explanation for the common phenomenon of «old wood in new houses» might be provided by recycling: instead of cutting fresh wood, builders re-used timber from houses that went out of use or were pulled down for reconstruction. This practice had the double advantage that the timbers were already cut and shaped, and that the wood was already dry. «Intrusive timber» might well be the rule rather than an exception[13]. The question is: is probe (4) also intrusive or not?

The question can be answered by «negative simplistic systemic accumulation» (see *supra*, § 3) of the Rehov Iron I dates (§ 4.1) and the dates of probes (4) and (5). The separation between Rehov Iron I and IIA should be looked for, approximately, around 924 BCE[14]. In this case, probe (4) is most likely (but not conclusively, cf. n. 14) intrusive. If the separation date is calculated on the basis of probe (5) only (admittedly, a very small sample), it sinks to 895.

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[11] A. Gilboa & I. Sharon, Early Iron Age Radiometric Dates from Tel Dor: Preliminary Implications for Phoenicia, and beyond: Radiocarbon 43/3 (2001) 1343-1352.

[12] I. Sharon, "Transition Dating" – A Heuristic Mathematical Approach to the Collation of ^{14}C Dates from Stratified Sequences: Radiocarbon, forthcoming. Sharon's formula is impeccable, but contains a number of estimated parameters. Perhaps some of his estimates are up for revision? My simplistic accumulation of his dates leads to 1000-970 for Dor Iron Ia, 970-910 for Dor Iron Ib (the Megiddo VIA horizon), and 900-890 for the Iron I/IIA transition.

[13] Of the three 14C dates for Megiddo VIA published so far (Carmi: Megiddo III, 502), two (simplistically combined to 1113/1017) probably derive from the construction of VIB, and only one (1034/921) is a candidate for a true VIA-date.

[14] The mean of the 2σ accumulated range for Iron I (920-830) is 875, the mean of probes (4) and (5) is 973. That the means appear in «inverse startigraphy» is no problem at all – the separation still works, and numbers do not develop sea-sickness. The «push» is still «down» for Iron I and «up» for Iron IIA. Nor is it a problem that the separation date lies slightly outside one of the measured ranges (900-790) – one out of 20 measurements is expected to be off.

6. Iron Age IIA[t] at Tel Rehov: 900/825.

18 short-lived samples from Rehov IV[t] produce a date for its destruction (and for the end of Iron IIA at Rehov) of 900/825 (95% probability)[15]. This date is irreconcilable with the traditional chronology (Iron IIA = 1000-925), it agrees perfectly with the low chronology and, of course, Mazar's «modified traditional chronology» with Iron IIA = 980-830. But how much of a basis for the assumption that Iron IIA starts during the 10th century is left after the presentation of the data discussed in this paper? The μμ –separator between Rehov's Iron I (N=3) and Iron IIA (n=18) is 890,

7. Conclusion.

I disagree with Mazar & Carmi stating that «^{14}C dates are our last resort in establishing a precise as possible absolute chronology for the Southern Levant in the time span between the mid 12th century BCE and the late 8th century BCE» (p. 1341). ^{14}C dates are our only means of absolute dating between Ramses VI and Tiglath-Pileser III[16]. Taken in isolation, they do not say much; interpreted in their systemic context and historical interconnections, they can lead to surprisingly exact conclusions. «Yet it seems that there is a long way to go before the final word will be said in this debate» (ibid.). Here I agree: now we have to discuss the «Very Low Chronology» of Gilboa & Sharon. The low chronology provides a politico-economical niche in which Saul, David and Solomon (somewhat diminished in stature, though) might survive as historical, not just literary figures. The «Very Low Chronology» endangers that niche.

[15] In this case, simplistic systemic accumulation leads to the same range as 1σ: 900-830.

[16] It would help a little if we could all agree than Dan III was built by Hazael (840-800).

Fig. 1: Two probabilities, overlapping

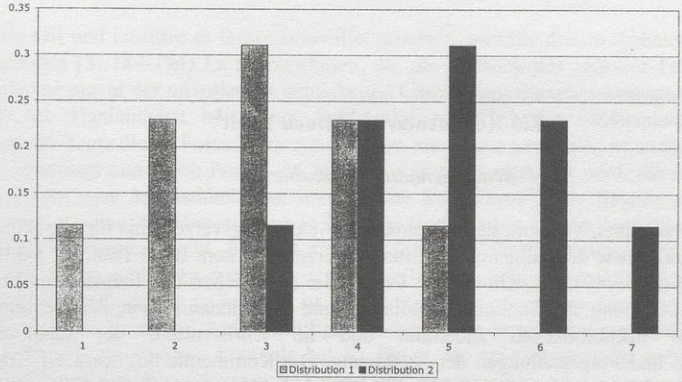

Fig. 2: The same probabilities, accumulated

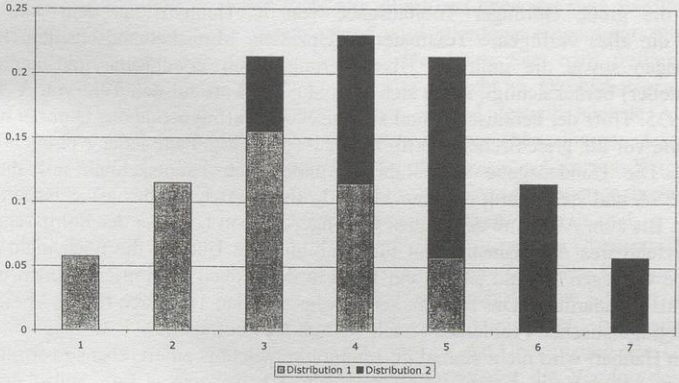

Fig. 3: The same distributions, separated