IRON AGE MEDITERRANEAN CHRONOLOGY: A REJOINDER

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ABSTRACT. This article is a rejoinder to a recent paper in this journal by van der Plicht et al. (2009) who use radiocarbon determinations from several sites in Israel, Italy, Spain, and Tunisia to advocate a High Chronology system for the entire Mediterranean Basin. We contend that they reached mistaken conclusions due to problematic selection of sites and data. We argue that a reliable way to provide absolute dates for the Iron Age in the central and western Mediterranean is by employing a combination of well-identified Greek pottery found in well-stratified sites and radiometric results from short-lived samples. For the time being, this combination exists only in the Levant, and provides an anchor for Greek chronology, which supports the Conventional Chronology for the Aegean Basin, which corresponds to the Low Chronology in the Levant.

INTRODUCTION

In a recent article published in this journal, van der Plicht et al. (2009) used radiocarbon data from 5 sites in the Levant and 5 in the western Mediterranean to argue for a High Iron Age Chronology throughout the Mediterranean Basin (regarding the Levant, this is in opposition to e.g. Sharon et al. 2007a; Finkelstein and Piasetzky 2003a, 2009; Fantalkin and Finkelstein 2006; regarding the west it stands against, e.g. Fantalkin 2001; Coldstream 2003). In what follows, we contend that the authors reached mistaken conclusions due to their: 1) problematic selection of sites; 2) problematic selection of data; and 3) problematic Bayesian analysis. For their only reliable site (Tel Rehov), we present alternative models to show that the Low Chronology prevails.

PROBLEMATIC SELECTION OF SITES

The Levant

Four of the 5 sites discussed by van der Plicht et al. (2009) are of no value in resolving the chronology debate: they are not stratified sites and 52 of their 57 samples are long-lived, which could introduce the "old wood effect." Only 1 site, Tel Rehov, is a well-stratified and well-excavated mound that provided short-lived samples and thus is instructive for the chronology debate.

Tel Dan

Tel Dan cannot be part of a significant chronology discussion because its stratigraphy is chaotic. David Ilan, who analyzed the Iron I strata from Tel Dan, described the situation as follows:

> "There was much continuity in use of architecture from stratum to stratum in these levels .... In places, elevations seem not to mesh and, in Area B-east, the area's grid orientation was changed, creating difficulties in matching up architectural remains. Also, the data gleaned in earlier seasons were often not integrated properly into that attained in subsequent seasons …. I have been forced in places to rely on ceramic assemblages to correlate contexts that are not otherwise endorsed by elevations or other stratigraphic criteria. It is also plain that many features, especially mudbrick ones, have gone unidentified. The pits that characterize Stratum VI, and are also present in Strata V and IVB, create a stratigraphic headache of another kind in the context of dense architecture—one that often has no good cure. For these reasons stratigraphic resolution is not always sharp as might be expected" (Ilan 1999:27–8).

In addition, Ilan showed that material from living surfaces was swept into silos in preparation for new construction (1999:114).

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From the 19 loci that provided the samples for ^{14}C measurement, only 1 may be sufficiently safe to be assigned stratigraphically; 7 were affiliated according to the ^{14}C results (Finkelstein and Pias-etzky 2006: Table 4). By the Megiddo standards, for instance (Boaretto 2006), not a single locus in this list—not even the "good" one—would have qualified as stratigraphically reliable for ^{14}C measurement. In a situation like this, ^{14}C dating samples should not have been collected in the first place. Indeed, van der Plicht et al. admit that "the original stratigraphic assignment was sometimes adjusted on the basis of the ^{14}C results" (2009:222), and in another place Bruins and van der Plicht (2005) state that:

> "A sample of mixed charred seeds … gave a very low date that does not fit the other results …. The strati-graphic archaeological context was re-evaluated as a result."

> "The youngest radiocarbon date in our Iron Age series from Tel Dan came from charcoal of olive wood … derived from destruction debris above a floor, associated originally with Stratum IVB. The radiocarbon date in this case also suggests that at least part of the destruction debris is much younger, perhaps from Stratum II …".

Fixing the entire stratigraphy in such a chaotic situation (to differ from a specific case in a well-strat-ified site) according to ^{14}C results (Bruins and van der Plicht 2005: Table 4 reveals that the same was done in 5 additional instances) is erroneous. By doing so, one could conceivably arrive at any result one wishes to obtain. It is also a mistake to base the original stratigraphy of Tel Dan on charcoal rather than short-lived samples (16 of the 19 items; see below). Under these conditions, the Tel Dan results are of no value for the Iron Age chronology discussion.

Horvat Haluqim

We do not think that this Negev Highlands site can add to the Iron Age chronology debate. The sam-ples dated originated not from secure floors at the site, but rather from an ancient agricultural terrace located in a small wadi *near* the site. Unlike floors in a stratified site, earth behind built-terraces can-not provide secure stratigraphic sequence. Moreover, they lack ceramic assemblages and hence can-not be correlated with nearby habitation sites. Two points are important here:

1. Work on dung in rockshelters in the same region (the Negev Highlands) provides evidence of human activity in periods not represented in the region's habitation sites (Rosen et al. 2005). Note also, that 2 of the samples from the Horvat Haluqim terrace provide dates in the Late Bronze Age, which is not represented at the site. The charred organic matter from the Horvat Haluqim terrace, therefore, is only reliable for confirming times of human activity in the vicin-ity of the terrace; it cannot be securely linked to the early Iron IIA layer at Horvat Haluqim.
2. Avni et al. (2009) have shown that terracing in the Negev Highlands, including terracing in the immediate vicinity of a site contemporary to Horvat Haluqim (early Iron IIA), had not been conducted before the 4th century CE.

In order to ^{14}C date the Negev Highlands sites, one needs to retrieve short-lived samples from safe contexts on floors. The value of such an endeavor has recently been shown in careful stratigraphic excavations carried out by Shahack-Gross and Finkelstein (2008) in 2 nearby sites: Atar Haroa and Nahal Boqer (see Cohen 1970; Cohen and Cohen-Amin 2004:28–34, respectively). The 2 sites are located in close proximity to Horvat Haluqim and all 3 are contemporaneous; they date to the ceramic phase labeled early Iron IIA. Atar Haroa provided 16 results from short-lived samples found on the floors of the peripheral broad-rooms of the site (the casemates of the "fortress" in van der Plicht et al.'s terminology). Nahal Boqer provided 3 more dates. The results fall in the late 10th century and mainly in the first half of the 9th century BCE (Boaretto et al. 2010). They fit well the

overall ^{14}C picture from Israel (e.g. Finkelstein and Piasetzky 2003a, 2009; Sharon et al. 2007a) and contradict van der Plicht et al.'s dating of Horvat Haluqim according to the charred wood found in the nearby agricultural terrace.

Tell el-Qudeirat

In the case of this site, too, we do not see the relevance to the Iron Age chronology debate. First, 3 of the 4 samples come from the late Iron II fortresses that according to all authorities date to the 8th and 7th centuries BCE (Cohen and Bernick-Greenberg 2007). Second, all samples are long-lived except one; the latter originated in the upper fortress, which dates to ~600 BCE. Third, *contra* to what van der Plicht et al. maintain, the single long-lived (charred organic material) sample did not originate in the lower fortress, but rather in remains to its west (Squares K/6-7, information in Bruins and van der Plicht 2005:357; 2007:491; for the location *vis à vis* the fortress see Cohen and Bernick-Greenberg 2007: Plan 3), and may have originated from the pre-fortress settlement at the site (Cohen and Bernick-Greenberg 2007:7; Singer Avitz 2008; Finkelstein 2010). The excavators describe this location as follows:

> "The remains include both the western part of the oval fortress and part of the settlement west of it. The stratigraphic correlation between these two units is crucial for understanding the chronological relation between them, but unfortunately this issue has not been clarified as there are no direct stratigraphic links in the excavated units" (Shor and Bernick-Greenberg 2007:67).

In short: the only relevant sample from Tell el-Qudeirat 1) is long lived; 2) does not come from the oval fortress; and 3) may belong to the pre-fortress settlement, which dates to the Iron I.

Khirbet en-Nahas

This site, important as it may be for the study of copper production in the Levant (Levy et al. 2004; 2008), cannot contribute to the Iron Age chronology debate. Khirbet en-Nahas is a copper production site, not a stratified settlement, and much of its accumulation is industrial waste (photo in Levy et al. 2008: Figure 2). The many (almost all long-lived) ^{14}C results from this site, measured at Groningen and Oxford, may tell us a great deal about the history of copper production there—beginning, peak, and end (Finkelstein and Piasetzky 2008)—but not much more. The only spot at the site that is not directly related to the copper industry is the large square fortress. Alas, no floors with secure ceramic assemblages were found there; hence, there is no way to tie the fortress to the ceramic sequence of the well-stratified mounds in northern Israel (Finkelstein and Singer Avitz 2008, 2009; *contra* Smith and Levy 2008). Moreover, 1) the dates associated by the excavators with the fort come from production waste under it, and 3 of the 7 measurements associated with the supposed 10th century fortress provided 9th century BCE dates; 2) while no ^{14}C sample at Nahas dates to the Iron IIB, most of the pottery found at the site belongs to the Iron IIB-C (Finkelstein and Singer Avitz 2009; in fact this also emerges from the report of the excavators: Smith and Levy 2008); 3) the layout of the fortress resembles 2 Iron IIB fortresses (with no Iron IIA remains) in the south: En Hazeva on the other side of the Arabah Valley and Tell el-Kheleifeh at the head of the Gulf of Aqaba.

Tel Rehov

Tel Rehov is a well-stratified and well-excavated site that provided a sequence of measurements from short- and long-lived samples. Still, some of the samples used by van der Plicht et al. come from dubious stratigraphic contexts. We refer to pits and open spaces that may have been used for dumping refuse (in a case like this old material, including a few olive stones, could have been swept into the pits—Ilan 1999); therefore, the results should be taken as the oldest possible dates.

The meaning of these dates for the Iron Age chronology debate has been discussed in a series of articles. Suffice it to say here that van der Plicht et al. (2009) interpreted the Tel Rehov determinations as supporting the High (or the Modified Conventional) Chronology (see also Bruins et al. 2003, 2005, 2007; Mazar et al. 2005). Yet, they failed to acknowledge that the same set of measurements can also be interpreted differently—in a way that substantiates the Low Chronology (Finkelstein and Piasetzky 2003b,c, 2006; Sharon et al. 2007b). For instance, the Iron I/IIA transition, set by Mazar et al. (2005) in ~980 BCE, can as conveniently be put in about 930–920 BCE (see analyses of the Tel Rehov data below).

The Central and Western Mediterranean

Carthage

van der Plicht et al. state that they chose Carthage for their research because its foundation is historically acknowledged (2009:225). They refer to the date of the foundation of Carthage in 814/813 BCE, which, according to Timaeus of Tauromenium, took place 38 yr before the first olympiad. Yet, dating the first olympiad is notoriously problematic (Christensen 2007:45–160) and there is no way to evaluate how Timaeus, who lived in the late 4th and early 3rd centuries BCE, calculated this date. According to Timaeus, for example, the foundation of Carthage and Rome took place in the same year, but this symbolic synchronization (Feeney 2007:52–99) is an artificial construct with little, if any, chronological value (Möller 2005:247). The ostensible historical reference datum for van der Plicht et al. is therefore unreliable.

The first series of ^{14}C dates from Carthage came from layers below the *decumanus maximus* in the University of Hamburg excavations. These layers produced a pottery assemblage consisting of Phoenician types of Bikai's Kition Horizon and Aegean wares of the Late Geometric period. Four ^{14}C dates obtained from animal bones yielded results with an average of 895–795 BCE (95%; 830–805 BCE 68%) (Nijboer and van der Plicht 2006: Table 2). Based on these dates, Nijboer (2005) suggested raising the beginning of the Early, Middle, and Late Greek Geometric by a number of decades, with the Late Geometric period starting in the second half of the 9th century BCE rather than around the mid-8th century BCE, which is the conventional dating (Brandherm 2006, 2008). Yet, these bones came from problematic contexts, with possible residual material (Docter et al. 2005; Núñes Calvo 2008); moreover, averaging in such a situation is not a legitimate procedure.

The Ghent University Carthage Expedition contributed 2 more series of bone samples for ^{14}C dating (13 determinations altogether). According to van der Plicht et al. (2009:227 and Table 9):

> "This set of ^{14}C dates is slightly younger than the Hamburg material. Unfortunately, from the viewpoint of precise dating, that moves them into the Hallsttatt plateau of the calibration curve. Even so, they still refer to the period around 800 BCE."

Thus far, detailed information regarding stratigraphic and ceramic contexts has been published for one of these groups, which includes 6 samples. They originated from a 9-layer pit below the bastion of Trench 4 in the Bir Massouda site, located in the center of present-day Carthage. The stratigraphic sequence was interpreted as a gradual filling (probably including garbage) over a long period of time, of a natural or manmade depression (Docter et al. 2008; the samples for ^{14}C dating came from layers [from the bottom up] BM04/4465, BM04/4463, BM04/4461, BM04/4460, BM04/4459, and BM04/4458).

Nijboer and van der Plicht (2006) state that:

"[T]he gradual lowering of the six radiocarbon results and its effect on the calibration, except for the three ^{14}C determination [sic!] around 2600 ± 20 BP, is well illustrated. However, a seventh and sixth century BC date for the sequence and its contents can be excluded on archaeological grounds. This narrows the date of these six radiocarbon determinations from 850 to 730 cal BC." (in Docter et al. 2008:314)

They add that

"[T]he three radiocarbon results around 2600 ± 20 BP and their content would imply that the Greek pottery present in these layers [emphasis ours] need to be dated to the first half of the eighth century BC, if not before…. Once more it is implied that the absolute chronology of the Late Geometric sequence needs to be raised." (in Docter et al. 2008:315)

The 3 determinations in question come from layers (from the bottom up): BM04/4465, BM04/4463, and BM04/4458. According to the excavators (and contrary to Nijboer and van der Plicht 2006), Layer BM04/4465 yielded 4 locally made pottery fragments only, with no Greek imports (Docter et al. 2008:317); Layer BM04/4463 included no pottery at all; and only Layer BM04/4458 contained abundant pottery finds, among them at least 2 Late Geometric pieces. The latter layer provided a date of 2580 ± 25 BP, which translates to a calibrated date of 799–773 (68%), 810–601 BCE (95%). According to van der Plicht et al., this datum negates the Conventional Greek Chronology for the Late Geometric.

Needless to say, a deposition (including garbage) in a pit is not a secure context for ^{14}C dating. The fact that the determination from the upper layer is earlier than those from the 3 layers below it indicates that the pit is not stratified. For argument's sake, let us assume that the layers in the pit do provide reliable stratigraphy. In this case, *contra* to van der Plicht et al., one cannot ignore the samples from layers BM04/4461, BM04/4460, and BM04/4459, found below Layer BM04/4458 (Nijboer and van der Plicht excluded these samples, ostensibly due to their problematic location on the calibration curve). These layers provided dates (from the bottom up) of 2520 ± 25, 2520 ± 40, and 2520 ± 25 BP, which would force the sample from the uppermost Layer BM04/4458 to a later date, a date that would not contradict the traditional Greek chronology, which corresponds to the Low Chronology in the Levant.

To sum up, due to the unstratified nature of the deposits, Carthage's ^{14}C dates are of minimal value in shedding light on the Iron Age chronology debate. Beyond this, in the case of Bir Massouda, van der Plicht et al. ignored some of the samples under Layer BM04/4458 with no reason; moreover, averaging results of samples from different layers in a pit whose fills accumulated over a long period of time is not a legitimate procedure (see the OxCal Web site http://c14.arch.ox.ac.uk/oxcalhelp/hlp_commands.html: "[14]C date combination: allows you to enter a series of dates for combination; the assumption is that they are all exactly the same age").

Huelva

Two contexts at Huelva, known as the Town deposits and the River deposits, produced organic samples that were ^{14}C dated. van der Plicht et al. (2009:226) claim that the dates obtained from both deposits provide support for the High Chronology in the Levant, and strengthen the view that the Phoenicians established trading links with the western Mediterranean prior to the Greeks. They take a further step, arguing that the Huelva dates corroborate the biblical material ("Scriptures" in their terminology) on the United Monarchy of ancient Israel, e.g. the story of Hiram King of Tyre and King Solomon's trade expeditions to Tarshish, which they identify with Tartessos in southwest Spain (for the same argument, see in more detail Nijboer and van der Plicht 2006, 2008; González de Canales et al. 2008, 2009).

Before looking at the ^{14}C results, we wish to comment on the identification of Tarshish. There are several competing locations for biblical Tarshish, from Tartessos in the west to Tarsos in Cilicia and beyond, and some scholars even doubt whether Tarshish refers to a specific place (for extended summaries, see Lemaire 2000; Lipiński 2004:225–66; Das 2008). Nijboer and van der Plicht (2006:35) follow Lipiński's identification of Tarshish with Tartessos in the region where Huelva is the primary settlement. In their opinion, the historicity of this tale is corroborated by additional materials in the "Scriptures" (2006:35). Yet, they ignore Lipiński's warning that the "fleet of ships of Tarshish" of 1 Kings 10:22 is an obvious anachronism "which is explainable by the endeavor of biblical redactors working about the 5th century B.C. and eager to boost the image of King Solomon" (Lipiński 2004:247). Indeed, many biblical scholars see this material as anachronistic (e.g. Van Seters 1983: 307–12; Knauf 1991; Miller 1997; Niemann 1997; Finkelstein and Silberman 2006:151–77).

The Town Deposit. According to van der Plicht et al. (2009:226):

> "The Town deposits, found in a clear archaeological stratum, contained a wealth of material culture, including the oldest Phoenician material recovered thus far. Various cattle bones ... were found, which offered the opportunity for a precise ^{14}C date by the conventional method. ... The averaged value for these 3 dates is 2755 ± 15 BP, resulting in a calibrated age range of 920–845 BCE (1 σ)."

Yet, in an article dedicated explicitly to Huelva, Nijboer and van der Plicht describe the nature of the Town deposits in a different way (2006:31):

> "The waterlogged, archaeological stratum investigated at Huelva, 5 to 6 metres under the present street level, is secondary. This means that the deposit was not stratified and that it contains mixed older and younger material, which is dated in the conventional chronology from around 900 to 770 BC. The material derives from a single stratum *in situ* in a layer of grey-dark estuary sediments…. The secondary nature of this archaeological context limits its significance ....Even so, it allows for a calculation of the mean-age of the three radiocarbon dates available. This mean-age cannot be used to date individual shards as found in the deposit. Thus it cannot date the Middle-Geometric shards in the deposit since the mean-age indicates that there can be shards older or younger than the average date range from 930 to 830 BC." [emphasis ours]

According to González de Canales et al. (2008:633), the earliest stratum referred to in the above citation yielded a large amount of pottery fragments, including 33 Greek and 8 Cypriot sherds. Most of the Phoenician ceramics from this layer can be assigned to Bikai's Salamis Horizon, corresponding to Strata IX through part of IV in Tyre, although some earlier pieces that may correspond to the end of her Kouklia Horizon (Tyre Strata XIII-X) were recognized as well (González de Canales et al. 2009:8–9). The Greek pieces included Attic Middle Geometric II types alongside Euboean SPG pieces, ranging from SPG I to SPG 3. Assuming that the identification of the earliest Phoenician pieces is correct, the earliest stratum of the Huelva Town deposits include a few items that may be broadly dated to the early Iron IIA in the southern Levant (Gilboa et al. 2008:168–73), making them the earliest Phoenician sherds discovered in the West thus far. The majority of the Phoenician assemblage, however, corresponds to the late Iron IIA-early Iron IIB in the southern Levant. Whatever absolute chronology one deploys, this means that the major part of the earliest stratum of the Huelva Town deposits should be placed toward the end of the 9th to first half of the 8th centuries BCE (cf. Núñes Calvo 2008; for the dates in the Levant, see Finkelstein and Piasetzky 2009, 2010a).

According to van der Plicht et al., the earliest stratum of the Huelva Town deposits provides an average uncalibrated date of 2755 ± 15 BP, resulting in a calibrated date of 920–845 BCE (1 σ). Yet, averaging 3 different samples from mixed, secondary contexts is not a legitimate procedure (see above). It is also misleading, since it creates an artificial "high" average date as a reference point for

the assemblage in question. When each of the 3 dates is calibrated separately, one arrives at the following dates (68%): 1) GrN-29511: 2745 ± 25 BP, 911–842 BCE; 2) GrN-29512: 2775 ± 25 BP, 975–859 BCE; 3) GrN-29513: 2740 ± 25 BP, 905-841 BCE. All that can be said with certainty is that a number of livestock was slaughtered and consumed at Huelva during the 10th and much of the 9th centuries BCE. Moreover, since both the bones and the Greek sherds may be residual, there is no way to safely correlate between them. The same holds true for the Phoenician pottery assemblage that attests to at least 150 yr of activity.

The River Deposit . Nijboer and van der Plicht (2008:110) raised another argument, which ostensibly supports the High Chronology in the Mediterranean: 6 samples taken from wood in the shafts of throwing spears found in the River deposits provided an average date of 2815 ± 30 BP (dates published in the 1970s), which translates into a calibrated date in the 10th century BCE (also van der Plicht et al. 2009:226). Nijboer and van der Plicht dismissed the possibility of the "old wood effect" since the shafts were made of young trees or branches. A number of fibulae, which were found with the spears' shafts, have a parallel in Tomb 1 in Achzib in Israel. This led them to argue that the "Phoenicians travelled the whole Mediterranean, from Tyre to Tartessos, from the 10th century BC onwards. For us this has become a fact since it is confirmed by three different types of records": the stories in the Scriptures, the distribution of goods such as the Achziv-Huelva fibula, and the ^{14}C dates (Nijboer and van der Plicht 2008:113).

This is not the case. First, Nijboer and van der Plicht (2008:110–2) rely on E Mazar's dating of the earliest burials in Tomb 1 at Achzib to the 10th century BCE (Mazar 2004:21–2; for the fibula 113, Figure 28:1). Yet, all layers in the tomb contain Iron IIB (8th century BCE) pottery, and no pottery in the tomb can safely be assigned solely to the Iron IIA. Second, the throwing spears come from a hoard that found its way into the River deposits. Items in a hoard may represent a long period of collection and therefore the dates of the wood shafts of the spears do not necessarily bear on the date of the fibulae.

To sum up, the Huelva's ^{14}C dates from both Town and River deposits cannot help resolve the Iron Age chronology debate.

Italy

We do not intend to deal here with the numerous problems related to Italian Iron Age chronology; suffice it for us to cite Leighton:

> "Over decades of research, Italian Iron Age chronologies have come to resemble entangled circuits, which should perhaps not be meddled with ..." (Leighton 2000:34)

> "… [O]ne cannot help noticing a certain 'academic' preference for high dates in the Italian protohistory." (Leighton 2000:44)

"Academic" preference aside,[4] the problem with the absolute chronology of Iron Age Italy stems from the fact that 2 distinctive chronological systems are employed in different parts of the country (see a variety of studies in Bartoloni and Delpino 2005). The absolute chronology of the major part

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[4]Leighton seems to refer to reaction to generations of prominent Classicists, who maintained that it was the Greeks who brought the "high culture," including the Orientalizing influences, to Italy (e.g. Blakeway 1932–1933, 1935). According to Boardman (1980:199): "The Etruscans were a rich but artistically immature and impoverished people, and they became ready and receptive customers for anything exotic that the Euboeans could bring them to a small degree the forms of Greek Geometric art, and to a great degree the wonders of the east, gold, jewelry, and bronzes."

of peninsular Italy is connected to the Alpine dendrochronology, while southern Italy, mainly Sicily, is linked to the Mediterranean chronology via Greek pottery.

Celano

van der Plicht et al. (2009:225–6) present a set of absolute dates from tombs at the site of Celano in the Abruzzo region; the samples were extracted from wooden sarcophagi and wiggle-match dated. They suggest that the tombs belong to the final stages of Late Bronze Italy (with consequent implications for the beginning of the Iron Age there) and date them to about 1000 ± 25 BCE (Nijboer and van der Plicht 2008:105–8). The Celano dates may have some bearing on the chronology of the eastern Mediterranean (see e.g. Pare 2008:92).

A closer look at the Celano determinations (van der Plicht et al. 2009:241) suggests that a date ~1000 BCE is not the only option, at least for Celano Tomb 5. *Contra* van der Plicht et al. (2009: Table 7), the 3 calibrated dates of Rings 21–50 may belong to an advanced phase in the 10th century BCE. This means that Celano can also be converted to the Low Chronology: Even if these rings are indeed the latest in the wood, and even if the wood was cut specifically for making the sarcophagi, based on the Celano data the final phase of the Late Bronze Age in Italy can be extended to include much of the 10th century BCE.

Indeed, the most recent studies that attempted to correlate between the Italian relative chronology of the Bronze and Early Iron Ages, Italian ^{14}C dates, the Alpine dendrochronology, and the Late Helladic III–Submycenaean/Protogeometric Chronology in Greece have shown that on the whole, the Conventional Aegean Chronology (which corresponds to the Low Chronology in the Levant), should be maintained (e.g. Jung 2006; Pare 2008; Weninger and Jung 2009). The most important in this regard is a possible correlation between the Bronzo Finale 2 in Italy and the Submycenaean in the Aegean. For this phase, Pare (2008:94) suggested a date in the mid-to-late 11th century BCE, while Weninger and Jung (2009:392) would have it terminate in about 1070–1040 BCE. The latter date would call for a slight upward shift (in relation to the conventional scheme) for the beginning of the Early Protogeometric period. Weninger and Jung's proposal is based on the assumed synchronism between a number of wheel-made Aegean style pots found in the destruction horizon of the settlement of Rocavecchia on the Adriatic coast of Apulia and the Submycenaean phase on the Greek mainland (Jung 2006:153–65). Assuming that the Aegean-style pottery from Rocavecchia may also belong to the LH IIIC late *milieu*, we would agree with Pare's date for placing the Bronzo Finale 2 in the second half of the 11th century BCE, implying that the beginning of the Early Protogeometric was around 1020/1000 BCE (Maeir et al. 2009:66–71). Such a dating system does not contradict Bietti Sestieri and De Santis' (2008) ^{14}C-based chronology for the Late Bronze–Early Iron Age phases in Latium Vetus. Whatever the case, there is no reason to raise the beginning of the Protogeometric period in the Aegean to ~1100 BCE if not earlier, as suggested by Newton et al. (2005a,b; Wardle et al. 2007) based on Wardle's excavations at Assiros (the reasons for this are explained in Maeir et al. 2009:70–1; Weninger and Jung 2009:385–8).

Latium Vetus

According to van der Plicht et al. (2009:225, 241), a ^{14}C sequence obtained from a number of sites in Latium Vetus indicates that the chronology of the Iron Age in Italy should be raised by about 25 to 50 yr, supporting the High Chronology in the Mediterranean.[5] In this article and others, their discussion focuses on the sites of Fidene and Castiglione.

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[5]Note that the Latium phases reflect the archaeological sequence in the region of Latium Vetus.

The Fidene dates come from 5 samples, 3 of charcoal and 2 of seeds. Initially, it was claimed that the Fidene assemblage is characteristic of the Latium Phase IIB and the beginning of Phase III (Nijboer et al. 1999/2000). Later, this assemblage was labeled Latium III Early (Bietti Sestieri and De Santis 2008:129; Nijboer and van der Plicht 2008:108). One may assume that we are dealing with a transitional assemblage within the Latium sequence.

According to Müller-Karpe's system (1962), the beginning of Stufe III (i.e. Latium III Early) should be placed around 800 BCE. Colonna suggested a reevaluation of the Müller-Karpe scheme based on the presence of Greek Geometric pottery in the Quattro Fontanili necropolis in Veii. At first, she claimed the Latial III Early began ~760 BCE; she later changed this to ~770 BCE (Colonna 1974, 1976, respectively). Correlation between the Latium and Veii phases implied a Latium IIIA and Veii IIB synchronism (Close-Brooks 1967; and see Close-Brooks and Ridgway 1979; Pare 2008: Figure 5.3). Yet, a few years later Meyer (1983:61–90) offered a modified synchronization between the different phases of Latium Vetus and Veii, concluding (without changing the conventional dates for the Greek chronology) that Veii IIA should be correlated with Latium IIIA. This implies that the latter began ~800 BCE, which is a revalidation of the Müller-Karpe system. This alone—before dealing with the actual radiometric results—would weaken Nijboer and van der Plicht's arguments for raising the dates of the Middle and Late Geometric Greek pottery.

[14]C dates for the Fidene hut yielded the following results (after van der Plicht et al. 2009: Table 6):

GrN-20127 (charcoal) 2820 ± 50 BP

GrN-20125 (charcoal) 2800 ± 50 BP

GrN-20126 (charcoal) 2790 ± 50 BP

GrA-5007 (seeds) 2770 ± 50 BP

GrA-5008 (seeds) 2760 ± 50 BP

Suffice it to take a look at the ^{14}C dates from the <u>preceding</u> Latium phase, namely from Castiglione tombs that were assigned to the Latium Phase II, in order to detect a problem. Bones from Tombs 25 and 40 in Castiglione yielded the following results:

GrN-23475 2670 ± 30 BP

GrN-23478 2670 ± 30 BP

These Latium Phase II dates are contemporary or younger than the dates for the Fidene seeds, assigned to the Latium Phase III Early (a later phase in the Latium sequence). Bietti Sestieri and De Santis (2008:126–9), who used the dates from both sites for establishing the absolute chronology of Latium Vetus during the Bronze and Iron Ages, concluded that based on the evidence from Castiglione, the Latium Phase II should be dated between the 10th and early- to mid-9th centuries BCE, while the Latium Phase III should be placed within the late 9th–early 8th centuries BCE. However, calibrating the 2 (similar) dates from Castiglione Tombs 25 and 40 puts one in the second half of the 9th century BCE (841–801 BCE, 68%) for Latium Phase II. Excluding charcoal, the date for the 2 ^{14}C determinations from seeds at Fidene (Latium Phase III, which should be later) may be calculated at 2765 ± 50 BP; 974–840 BCE, 68%; 941–840 BCE, 58%. This means that the Fidene dates make sense only when they are matched up with the Latium Phase II, not with the Latial Phase III Early—thereby disengaging the discussion from the Greek pottery in Veii. In any event, raising the beginning of Latium Phase III Early to around 800 BCE does not involve any significant change in the Greek chronology.[6]

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[6]It may be necessary, though, if one applies Descoeudres and Kearsley's (1983) Low Chronology for Veii (see also Descoeudres 2002). This chronology, however, has not gained wide approval (see e.g. Popham and Lemos 1992; Kourou 2005).

The ^{14}C results from central Italy, if at all relevant, do not contradict the conventional Greek Geometric chronology.

To conclude the discussion on the western Mediterranean, most of the material originated from stratigraphic contexts that are not secure, or from contexts that are problematic from the standpoint of ceramic affiliation. Nothing in the material cited by van der Plicht et al. (2009) calls for a change in the Conventional Chronology of Greece or the Low Chronology in the Levant.

PROBLEMATIC SELECTION OF DATA

Since non-systematic selection of data may lead to bias, it is essential to employ a rigorous method for the inclusion and exclusion of determinations. We have always worked with a clear set of criteria for accepting or rejecting measurements. They are as follows:

3. Due to the risk of the "old wood effect," only short-lived samples are included (for bias created by the "old wood effect" see Schiffer 1986; Sharon et al. 2007a:5–6; Finkelstein and Piasetzky 2010b).
4. All available readings from loci safely assigned stratigraphically are incorporated;
5. Results from all laboratories are included (*contra* Mazar et al. 2005, who doubted the accuracy of 1 laboratory *contra* intercomparison results—Boaretto et al. 2005; Finkelstein and Piasetzky 2006).
6. The uncalibrated dates corresponding to a given stratum are checked for consistency. Only determinations that differ by more than 5 standard deviations from the weighted average of the other measurements in their group are excluded as outliers.

van der Plicht et al.'s selection of data for their discussion is unsystematic methodologically on all these points; the first three are dealt with below.

Inclusion of Long-Lived Samples (Mainly Charcoal)

van der Plicht et al. (2009:215) rightly maintain that "short-lived samples are preferred for ^{14}C dating." Yet, as Table 1 shows, their discussion is overwhelmingly based on long-lived samples (less than a quarter are short-lived). Their comment that fine charcoal does not necessarily originate from old wood is misleading. The point is that it might, and would then bias the model; this is the reason that we recommend avoiding such samples.

Table 1 Short- versus long-lived samples in van der Plicht et al.'s (2009) model.

Site	Short-lived	Charcoal/wood	Bones[a]
Dan	3	16	—
Horvat Haluqim	—	7	—
Tell el-Qudeirat	1	3	—
Khirbet en-Nahas	—	21	—
Tel Rehov	18	3	—
Carthage	—	—	10
Huelva	—	6	3
Celano	—	10	—
Latium Vetus	2	3	2
Total	**24**	**69**	**15**

[a]For the rationale behind separating bones from the short-lived column, see text below.

Correlating ^{14}C dates that derive from charcoal (to differ from short-lived) samples with relative dates of strata (ceramic assemblages) is problematic, as timber may have had a long history of use and reuse. Bones, especially individual bones (rather than an articulation), may also introduce old dates, as bones may originate from brick material. The same holds true for individual olive pits or individual seeds (to differ from a groups of such finds).

In other words, measurements that originate from charcoal samples may introduce the well-known "old wood effect" (Schiffer 1986; Sharon et al. 2007a:5–6; the latter for the case discussed here). This is especially so in multiperiod sites, where the inhabitants may have recycled used timber for construction. In certain cases, a quick glance at a set of determinations that originate from charcoal/ wood samples is sufficient for concluding that the samples represent old wood; either the results group too far from the short-lived determinations from the same horizon, or they do not fit what we know about the given layer from other sources, e.g. well-dated Egyptian finds (see e.g. Finkelstein and Piasetzky 2010b regarding Strata XII–XI at Hazor). However, at sites characterized by dense stratigraphy (in our case, Tel Rehov, Tel Dan, and to a certain extent Tell el-Qudeirat; the relevant contexts at Carthage and Huelva are not stratified at all and Celano features tombs), the reused wood may originate from a previous layer close in date to the one under investigation. In Area H at Megiddo, for example, the period from ~950 to ~700 BCE is represented by 9 layers, that is, an average of ~30 yr per layer—below the uncertainty in a normal ^{14}C measurement. In such a situation, charcoal from a layer immediately before the one investigated (or even 2 layers older) will be almost impossible to detect, though it would still introduce the "old wood effect." In addition, in the case of long-lived tree species such as oak and juniper the sample measured may derive from the inner rings of the tree, which may be significantly older than the date when the tree was felled. Charcoal derived from olive tree trunks (rather than from young branches) is no less problematic, as the tree rings are difficult to identify. Therefore, ^{14}C dates of charcoal and wood can only provide a *terminus post quem* for the stratum where they were found. Charcoal and wood should be excluded altogether from analysis of ancient mounds with multiple strata representing a long settlement history.

In a rejoinder to an article by Mazar and Bronk Ramsey (2008), two of us have shown how inclusion of long-lived samples brings about a bias toward older dates (Finkelstein and Piasetzky 2010b, see especially Figure 1). Table 2, which presents the results for the data in Mazar and Bronk Ramsey's Table 1 (end of Late Bronze and Iron I; Mazar and Bronk Ramsey 2008:162), demonstrates this problem. The charcoal measurements that were included in their model are significantly (and, as seen in Finkelstein and Piasetzky 2010b: Figure 1, also systematically) older than the short-lived measurements.

Table 2 Mazar and Bronk Ramsey's (2008) Table 1 with and without charcoal.

	Iron I	Late Bronze
Short-lived samples only	2901 ± 21	2914 ± 15
Charcoal only	2978 ± 14[a]	2989 ± 16
Difference	77 ± 25 (3.1 σ)	75 ± 20 (3.8 σ)

[a]Without the 4 (substantially earlier) outliers as defined by Mazar and Bronk Ramsey.

In the case of the Iron I, for instance, this translates into calibrated dates (68%) of 1126–1045 BCE for the short-lived results and 1262–1134 BCE for the charcoal results—a meaningful difference, without an overlap. This example deals with a model that used 10 short-lived and 6 long-lived samples. The meaning for a study that uses 24 short-lived and 69 long-lived samples (plus 15 bone samples) is evident.

Ignoring Results of Other Laboratories for the Same Strata

The very same strata and even loci discussed by van der Plicht et al. yielded ^{14}C determinations from other laboratories (Rehovot and Arizona; for the ^{14}C intercomparison, which indicates the reliability and consistency of the Rehovot laboratory, see Boaretto et al. 2005). Such a selection of data is legitimate only if one explains why results from other laboratories, on some occasions contradictory, are rejected. Below, we show how the usage of the Rehovot and Arizona determinations changes the results of the model, both for Tel Rehov and for the entire Levant.

Ignoring Results from Other Sites (Measured at Groningen and Other Laboratories)

van der Plicht et al. (2009) argue that:

> "Putting all the dates of many sites together in one mix does not seem helpful. It increases complexity in a nontransparent manner and it prevents systematic evaluation of both individual and successive strata for each site in archaeological and ^{14}C terms."

In our view, this claim is wrong; more reliable data provides a more secure result. In the case of Tel Rehov, van der Plicht et al. combine results from different areas of the dig. This is achieved by means of ceramic typology. There is no difference between combining different areas at 1 site and combining different strata with the same ceramic assemblage from the same region. This is especially so for the Iron Age, in which the pottery assemblages change at a fast pace, 8 assemblages for the period between about 1130 and 600 BCE.

We believe that a database for a model made of 143 samples (339 determinations) from 38 strata at 18 sites, measured by 3 methods in 6 laboratories, with consistent results (Finkelstein and Piasetzky 2010; similarly Sharon et al. 2007a) is far more reliable than a model of 13 samples and 46 determinations based on a single site and a single laboratory.

To sum up this section, out of the first 5 (and most crucial) items in van der Plicht et al.'s recommendations for sample selection (2009:215–6; the other recommendations deal with technical laboratory work), their analysis fails in four. In fact, if one takes their recommendation #2 (the sample must represent the event of archaeological interest) as trivial, their work fails to adhere to 100% of their own rules.

PROBLEMATIC BAYESIAN ANALYSIS

Despite all our reservations about arriving at conclusions on the Levant, let alone the entire Mediterranean Basin, based on a single site, we decided to engage van der Plicht et al.'s Bayesian model for the Iron I/IIA transition at Tel Rehov. We did so adhering to our comments above regarding selection of data. We performed Bayesian analyses (using the OxCal v 4.1.3 program; Bronk Ramsey 1995, 2001) for a few sub-models that address the same question asked by van der Plicht et al. (the assumptions behind the different models are presented below with the results).

Sub-Model A

The first sub-model is our attempt to reproduce the one presented by van der Plicht et al. (2009). We have constructed it according to the data in their Figure 3, without omitting any item. Where the "R_combine" option is shown in that figure, we combined all the data listed in their Table 1.

The result for the transition between the Iron I and IIA (i.e. the transition between Strata D3 and VI) is shown in Figure 1. Contrary to van der Plicht et al. (but using their model and their data set), we

get ~50% probability of placing the Iron I/IIA transition in the first half of the 10th century (which fits the High Chronology system) and 50% probability that the transition took place in the second half of the 10th century (as proposed by the Low Chronology system).
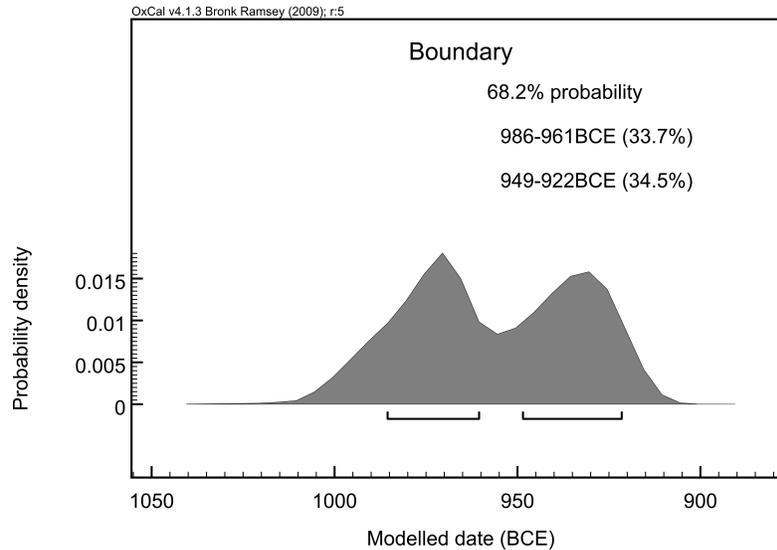

Figure 1 The Iron I/IIA transition as calculated by Sub-Model A

Sub-Model B

We now removed from the data set of Sub-Model A all charcoal/bone data (from Loci 4816, 4426, and 4218) and added the short-lived samples excluded by van der Plicht et al. without any explanation (all measurements of Locus 6449). We too excluded Measurement GrN 26119 for the sample from Locus 2862, which was found to be inconsistent with the model. The results are similar to those of Sub-Model A.

Sub-Model C

In their model, van der Plicht et al. created 2 phases for Stratum V—one titled "stratum V city" (Locus 4218) and a second titled "stratum V destruction." We see no reason for this division; the fact that the olive pits were charred seems to indicate destruction by fire. If one assigns Locus 4218 to the destruction of Stratum V, that is, unites the 2 Stratum V phases to a single phase, the Iron I/IIA transition swings to the second half of the 10th century BCE, adhering to the Low Chronology system (Figure 2).

Sub-Model D

To our Sub-Model C, we now added all the published measurements of short-lived samples from the same strata at Tel Rehov measured in other laboratories (Table 3).

We removed 2 samples (R7 and R103) that are inconsistent with the model fit, reaching a 65% overall agreement between the model and the data. The result is shown in Figure 3. This model also prefers the Low Chronology solution.
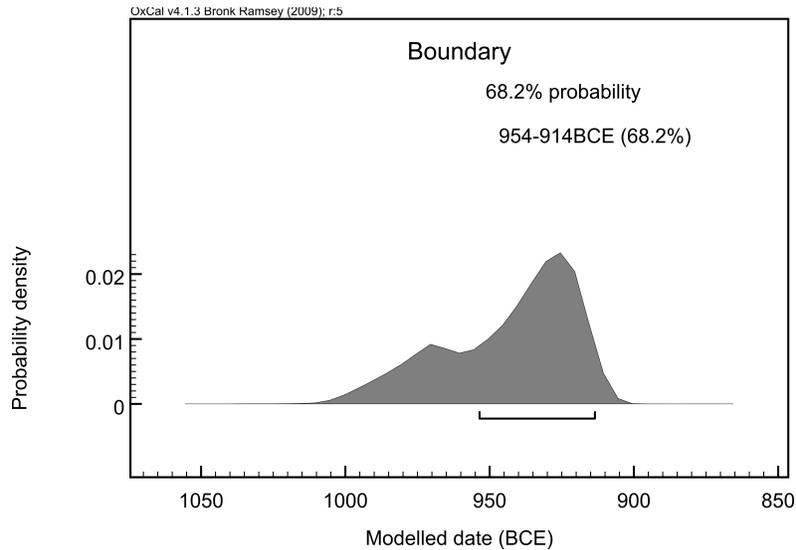
Figure 2 The Iron I/IIA transition as calculated by Sub-Model C

Table 3 Tel Rehov samples measured in other laboratories.

Lab method[a]	Lab nr	Site/stratum	Sample type	Average uncalib. date	Our sample nr[b]	Individual uncalib. dates	Reference
R AMS	3809.4,5	Rehov D-4	Olive pits	2845 ± 25	R1		
T AMS	3809a,aa			2913 ± 45	R2		
R AMS	3805.3-5	Rehov D-3	Olive pits	2800 ± 20	R3a		
R AMS	3806.3			2754 ± 24	R102	2728 ± 35	Sharon et al.
	3806.5					2779 ± 34	2007a
RW LSC	3120			2670 ± 40	R103	2670 ± 40	Mazar et al. 2005
T AMS	18159a,aa	Rehov V	Grain seeds	2685 ± 25	R6		
R AMS	3808.3-5	Destruction		2678 ± 20	R7		
T AMS	AA30431-			2749 ± 16	R110	2830 ± 55	
T AMS	U3-11,12,					2745 ± 50	
T AMS	13,21,22,23,					2730 ± 45	
T AMS	31,32,33					2815 ± 50	
T AMS						2770 ± 50	
T AMS						2710 ± 45	
T AMS						2685 ± 45	
T AMS						2760 ± 60	
T AMS						2740 ± 50	
RW LSC	3122A			2699 ± 10	R111	2700 ± 20	
RW LSC	3122A1					2655 ± 25	
RW LSC	3122A2					2655 ± 25	
RW LSC	3122B					2720 ± 20	
RW LSC	3122B1					2700 ± 25	
RW LSC	3122B2					2650 ± 30	
RW LSC	3122BB					2725 ± 15	
RW LSC	3122C					~~2860 ± 20~~[c]	
RW LSC	3122D					2710 ± 20	

[a]Tu = Tucson; Gr = Groningen; R = Sample prepared in Rehovot and measured in Tucson; RW = Rehovot; AMS = Accelerator Mass Spectrometry; LSC = Liquid Scintillation Counting; GPC = Gas Proportional Counting.
[b]See Finkelstein and Piasetzky 2010b.
[c]Outlier, not calculated.

OxCal v4.1.3 Bronk Ramsey (2009); r:5

Boundary

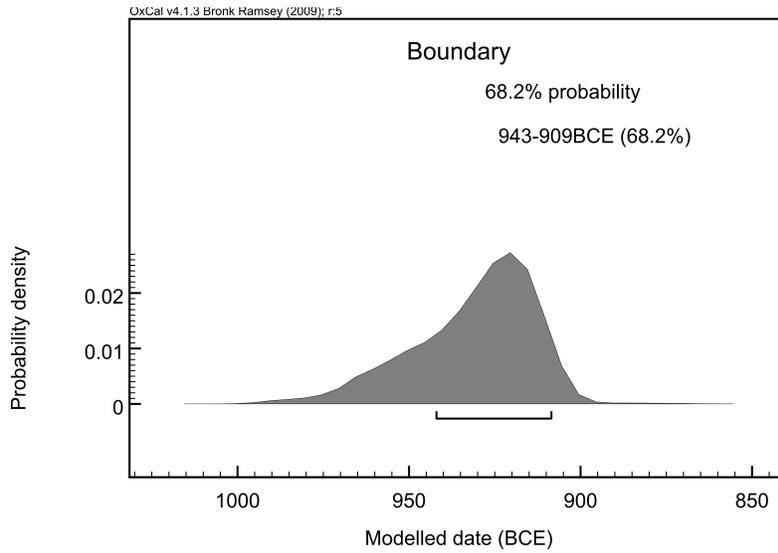68.2% probability

943-909BCE (68.2%)

Figure 3 The Iron I/IIA transition as calculated by Sub-Model D

Sub-Model E

Even though we see no reason to have 2 separate phases in Stratum V (see above), we also checked what happens if we use all the data set as in Model D, but adapt van der Plicht et al.'s idea of 2 separate Stratum V phases. The result (Figure 4) demonstrates that the second part of the 10th century solution is robust and does not change as a result of this assumption.
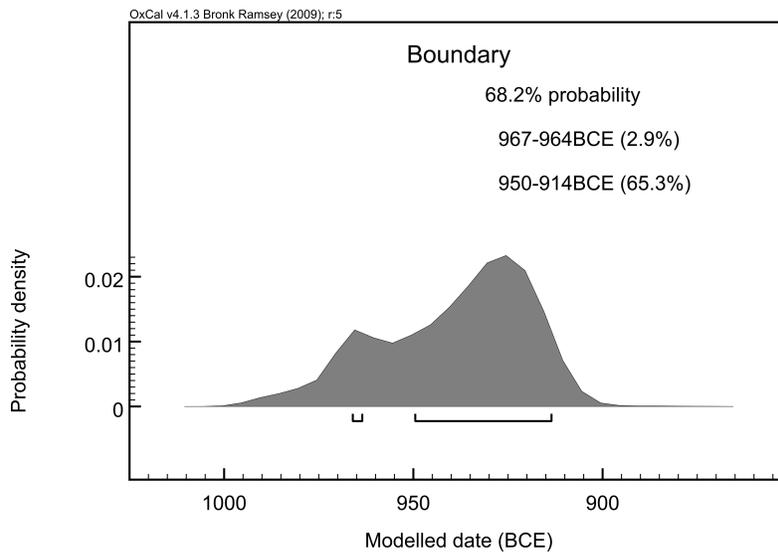
OxCal v4.1.3 Bronk Ramsey (2009); r:5

Boundary

68.2% probability

967-964BCE (2.9%)

950-914BCE (65.3%)

Figure 4 The Iron I/IIA transition as calculated by Sub-Model E

Sub-Model F

Finkelstein and Piasetzky (2010a) recently published a comprehensive Bayesian model that deploys ^{14}C results in order to date the entire sequence of the Iron Age in the Levant. It deals with 6 ceramic phases and 6 transitions that cover ~400 yr, between the late-12th and mid-8th century BCE. The model uses 143 samples (339 determinations) from 38 strata at 18 sites. From this model, we now removed all the data except for those from Tel Rehov. We also removed 3 data points (R7, R103, and R111; see Table 3) that are in poor agreement with the model. After their removal, an overall agreement of 75% between the model and the data has been reached. The result (Figure 5) clearly favors the Low Chronology solution.
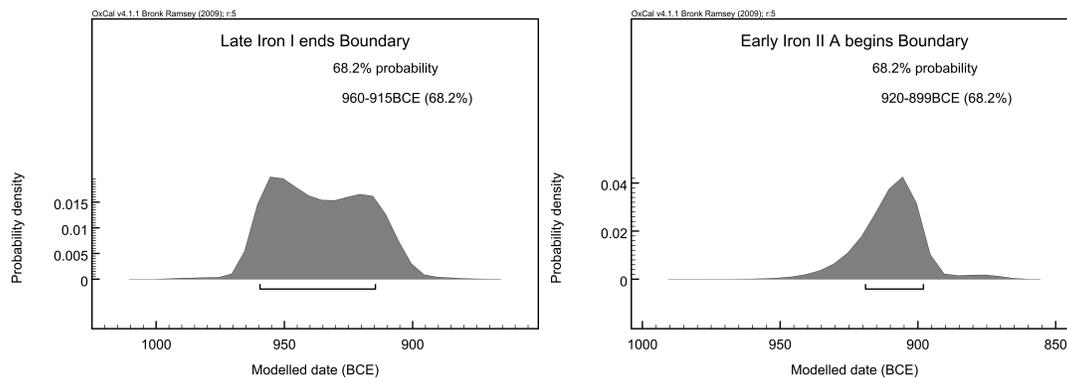

Figure 5 The Iron I/IIA transition as calculated by Sub-Model F

CONCLUSION

van der Plicht et al.'s claim (2009) that the High Chronology is the only possible solution for the Iron Age in the Mediterranean is based on a very specific and selected set of data and model structure. Their conclusions suffer from problematic interpretation of historical sources and wrong selection of archaeological assemblages, and demonstrate the danger of using a single site, with a limited (and selected) set of data (see also Sharon et al. 2007).

The most reliable way to provide absolute dates for the Iron Age in the central and western Mediterranean is by employing a combination of well-identified Greek pottery found in well-stratified sites and radiometric results from short-lived samples. For the time being, this combination exists only in the Levant. All the Bayesian models for the Iron I/IIA transition in the Levant that use short-lived determinations from well-stratified sites (Sharon et al. 2007b; Finkelstein and Piasetzky 2010b) favor the Low Chronology system. The same is true for the dating of other ceramic phases of the Iron Age (Finkelstein and Piasetzky 2009, 2010a). This, in turn, provides an anchor for Greek chronology, which supports the Conventional Chronology for the Aegean Basin (Coldstream 2003), which corresponds to the Low Chronology in the Levant.

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