

2 The Debate over the Chronology of the Iron Age in the Southern Levant

Its history, the current situation, and a suggested resolution

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Abstract

The subject of the Oxford conference—the chronology of the Iron Age of the southern Levant in the 12th–9th centuries BCE in light of current debates and ¹⁴C dating—is of great interest among a wide circle of scholars from various disciplines, since it has a variety of implications for related fields of research. The subject is important for the archaeology of the Levant, Cyprus, and Greece; it has far-reaching implications for the utilization of archaeology in the study of the emergence of various ethnic and geo-political units of the period, such as ancient Israel, the Philistines, the Phoenician city-states, the Aramean states and the Transjordanian states of Ammon, Moab, and Edom. The subject is essential for proper evaluation of correlations and contradictions between archaeology and the biblical text.

The focus of this volume should be on the dating of the transition from the Iron Age I to the Iron Age II and the duration of the sub-period widely known today as the Iron Age IIA. To estimate the latter, we need solid relative chronology and as precise as possible absolute dates for certain occupation strata, regional pottery assemblages, and architectural complexes. More than thirty excavated sites in Israel and Jordan are available for comparative study. They differ in the quantity of data recovered and published, the quality of the excavation, and the state of publication, but together they represent a huge puzzle, the pieces of which have to be correlated and integrated into a comprehensive picture. Accepted dates for the various strata and contexts are essential for interpretation, but alas, no such consensus exists today. Here I will attempt to diagnose the reasons for the disagreements and to examine whether current advances in scientific dating methods may help us to reach some agreement or at least to minimize the gap between the various views.

Short History of Iron Age subdivisions

Typical of biblical archaeology, the debate over Iron Age chronology was always based on a mixture of pure archaeological analysis on the one hand and attempted correlations with biblical and extra-biblical sources on the other. It started with the University of Chicago Oriental Institute excavations at Megiddo during the 1930s, when the excavators identified the ‘chariot city’ of Stratum IV as Solomonic. In 1940, John Crowfoot, in light of his excavations at Samaria, called for

a lowering of the date of Megiddo Stratum IV to the Omride period, due to the architectural similarity between Samaria and Megiddo (Crowfoot 1940; see Franklin [Chapter 18, this volume] for a similar view).

Table 2.1. Divisions of the Iron Age in Israel.

Wright 1961	
Iron IA:	1200–1150 BCE
Iron IB:	1150–1000 BCE
Iron IC:	1000–918 BCE
Iron IIA:	900–800 BCE
Iron IIB:	800–587 BCE
Aharoni and Amiran 1958	
Israelite I:	1200–1000 BCE
Israelite II:	1000–840 BCE
Israelite III:	840–587 BCE
Aharoni 1982; <i>EAEHL</i> 1978; Herr 1997; Herzog 1997; Mazar 1990; <i>NEAEHL</i> 1993	
Iron IA:	1200–1150 BCE
Iron IB:	1150–1000 BCE
Iron IIA:	1000–925 BCE
Iron IIB:	925–720 BCE
Iron IIC:	720–586 BCE
Barkay 1992 and Ben-Tor [ed.] 1992	
Iron I:	1200/1150–1000 BCE
Iron IIa:	1000–800 BCE
Iron IIb:	800–700 BCE
Iron IIIa:	700–586 BCE
Iron IIIc:	586–520 BCE
Current Suggestion (A. Mazar)	
Iron IA:	1200–1140/1130
Iron IB:	1150/40–ca. 980 BCE
Iron IIa:	ca. 980–ca. 840/830 BCE
Iron IIb:	ca. 840/830–732/701 BCE
Iron IIIa:	732/701 BCE–605/586 BCE
Iron IIIc:	605/586–520 BCE

In the early 1950s, Benjamin Maisler (Mazar) (1951) and Gus van Beek (1951, 1955) debated over the dates of Tell Abu Hawam Stratum III and of Cypro-Phoenician Black-on-Red pottery. While Van Beek claimed that both should be dated to the 10th century BCE, Maisler (Mazar) advocated that Tell Abu Hawam III dated to the 9th century BCE, claiming that it was destroyed by Hazael, King of Damascus. This debate recalls our current one.

William F. Albright, Ernest Wright, and their followers, including most Israeli scholars until the present day, defined the 10th century until Shoshenq I (hereafter: Shishak) invasion (traditionally dated to 925 or 918 BCE; 917 BCE according to A. Shortland [Chapter 4, this volume]) as an independent archaeological sub-period, designated as ‘Iron IC’ by Wright (1961: 97 Chart 8) and ‘Iron IIA’ by the *EAEHL* (IV, 1226; *NEAEHL*: IV, 1529). This approach isolated the time of the United Monarchy as a defined archaeological sub-period. Some considered the 9th century BCE as a separate sub-period, while others combined it with the 8th century as a single sub-period (thus the *EAEHL*). An alternative approach was suggested by Aharoni and Amiran following the excavations at Hazor (Aharoni and Amiran 1958). In their view, Hazor Strata Xb, Xa, IXb, IXa, and VIII indicate a single archaeological sub-period of long duration that included both the 10th and the 9th centuries BCE. They denoted this period ‘Iron II’ and dated it to 1000–800 BCE. Their

approach was subsequently rejected by most scholars (including Aharoni himself in later publications), but was retained by Gabriel Barkay and Amnon Ben-Tor, in a general account of the archaeology of Israel (Barkay 1992: 302-307; Ben-Tor [ed.] 1992: 2). They used the term Iron Age IIA for the time period of 1000–800 BCE. From a biblical history point of view, this sub-period included both the time of the United Monarchy and that of the Omride Dynasty and its aftermath. In my general book (Mazar 1990: 30) I followed the Albright/Wright/*EAEHL* approach; however, I am now convinced that the division suggested by Aharoni and Amiran and maintained by Barkay is preferable, although it needs some modification (see further below).

Yadin's Paradigm and its Aftermath

Since 1958 Yigael Yadin developed the earlier ideas of Albright and Wright into a well-defined paradigm for the 'archaeology of the United Monarchy', based on the excavations at Hazor, Megiddo, and Gezer (for his final view and earlier references see Yadin 1972: 135-64). He was followed by most archaeologists working in Israel, and his main points were accepted even by those who rejected some of his conclusions and suggested revisions to his scheme. The following is a short summary account.

Megiddo is the key site for this subject. Stratum VIA was the last Canaanite city at that site. It was destroyed by an intense conflagration ca. 1000 BCE or somewhat later, perhaps when conquered by David. It was replaced by the poorly built town of Stratum VB, which was attributed to the time of David. The later Stratum VA–IVB was considered to be the Solomonic city *par excellence*; it included, according to Yadin, two monumental palaces, a six-chambered gate, and a casemate wall. This city was conquered by Shishak and was replaced during the Omride period in the 9th century by the city of Stratum IVA, with its stables, massive city wall, and reuse of the Solomonic six-chambered gate. This city survived until the Assyrian conquest of 732 BCE. Aharoni (1972), followed by Zeev Herzog (1997: 211-14),¹ and David Ussishkin (1980, 1990; Ussishkin in Finkelstein and Ussishkin 2000: 600), pointed out the stratigraphic difficulties in Yadin's scheme, in particular regarding the six-chambered gate and the suggested casemate wall, but they all accepted Yadin's idea of a Solomonic city with monumental buildings at Megiddo. At Hazor, the dense stratigraphy enabled Yadin to date Stratum X with its six-chambered gate to the time of Solomon; Stratum IXa–b to the time between Solomon and Ahab; Stratum VIII to the Omride Dynasty; and Stratum VII to the post-Omride period. This scheme has been retained and substantiated by the current expedition headed by Amnon Ben-Tor (Ben-Tor and Ben-Ami 1998). The six-chambered gate and a short segment of a casemate wall at Gezer were dated to the 10th century by Yadin, and this date was later accepted by the Hebrew Union College excavators, who uncovered also a large public building next to the gate (Dever 1986).

Kathleen Kenyon, who accepted Yadin's Solomonic paradigm regarding Solomon's buildings, nevertheless suggested a later date in the 9th century for the pottery found in these buildings, based on parallels from Samaria, thereby suggesting a separation between 'Building Period' and 'Pottery Period' at Samaria and Megiddo (Kenyon 1964). Her opinion was most influential on scholars like Nicolas Coldstream, who, in the 1960s, used it to establish the chronology of the Greek Proto-Geometric and Geometric periods (Coldstream 1968: 302-305). As we shall see, it seems to me today that Kenyon was close to the correct solution for our problem.

1. Herzog 1992: 250-53 accepted Aharoni's view and dated Megiddo IVB–VA (which he denotes VA) to the time of David; yet a few years later he dated the same 'palaces city' to the time of Solomon (Herzog 1997: 212-14). He accepts Aharoni's view that the six-chambered gate belongs to Stratum IV, which in his later view should be dated to the Omride period.

During the 1970s and 1980s, there was almost a consensus concerning the dating of two main pottery assemblages that are of interest to us here:

- a. The late Iron Age I assemblage, represented by the pottery from the destruction layers of Megiddo VIA, Tell Qasile X, Tel Masos II, and parallel strata at other sites, was dated to the late 11th or early 10th century BCE.
- b. The Iron IIA assemblage, with northern and southern variants, represented by the destruction layers of Megiddo VA–IVB, Taanach Period IIB, Yokneam XIV, Beth Shean S-1 (in the Hebrew University excavations), Lachish V, Arad XII, and parallel strata at other sites, was dated by most scholars to the 10th century, until Shishak's raid.

The deconstruction of the Albright/Yadin 'Solomonic paradigm' began during the 1970s with Benno Rothenberg's and later by Gary Pratico's demolition of Nelson Glueck's concept of 'Solomon's copper mines' in the Timnah Valley in the Arabah and of his identification of Ezion Geber with Tell el-Kheleifeh. Twenty years later, in the late 1990s, questions were raised concerning the core of the paradigm by J. Wightman (1990) and especially by David Jamieson-Drake (1991) whose influential book included a frontal attack from an archaeological standpoint on the very concept of the United Monarchy of ancient Israel. This work fell like a ripe apple into the hands of historians and biblical scholars of the European 'revisionist school' who were inclined to minimize or reject altogether the historicity of the entire or parts of the biblical narrative. This book also inspired the work of Israel Finkelstein.

Finkelstein's Low Chronology

Since 1996, Finkelstein (1995, 1996) went one step further by suggesting the wholesale lowering by 50–80 years of archaeological assemblages traditionally attributed to the 12th–10th centuries BCE. His first point was the date of the appearance of the local Mycenaean IIIC or 'Philistine Monochrome' pottery. Following Ussishkin (1985), he suggested lowering the appearance of this pottery by 50 years until after the end of the Egyptian presence in Canaan. This subject is beyond the scope of the present discussion, but it should be mentioned that several recent studies and discoveries, such as those at Ashkelon, negate this approach; in fact, none of the excavators of Philistia find this suggestion acceptable. It also creates unsolvable problems in correlating the archaeology of Philistia with that of Cyprus (Dothan and Zukerman 2003; Mazar 1985 and forthcoming; Sherratt and Master [Chapters 9 and 20, this volume;]). ¹⁴C dates for this period are not of much help, due to the many wiggles and complicated shape of the calibration curve for the 11th and 12th centuries BCE. Consequently, Finkelstein suggested lowering the dates of late Iron Age I assemblages from the late 11th century to the 10th century BCE and the lowering of traditional 10th century BCE assemblages to the 9th century BCE. His view became known as the 'Low Chronology' for the Iron Age of Israel. This suggestion empties the 10th century BCE of its traditional contents. Sites and strata that were traditionally dated to the late 11th century BCE, such as Megiddo VIA, are dated to the 10th century BCE, until Shishak's campaign (Finkelstein 1998a, 1998b, 1999b, 2002a, 2002b, 2004, and Chapters 3 and 17, this volume).

In a separate study based on ¹⁴C dates from Tel Dor, Ayelet Gilboa and Ilan Sharon suggest an even lower chronology from that suggested by Finkelstein (see below).

Review of Finkelstein's Low Chronology (LC)

Since it was initially suggested in 1996, the LC and the historical perspectives that followed it have become the subject of continued controversy. I started the debate with a comprehensive review of this theory from an archaeological perspective (Mazar 1997) and others followed (Ben-Shlomo,

Shai, and Maeir 2004: 2; Ben-Tor 2000; Ben-Tor and Ben-Ami 1998; Bunimovitz and Faust 2001; Byrne 2002; Dever 2001: 131-38; Harrison 2003; Herzog and Singer-Avitz 2004; Kletter 2004 [the most comprehensive review of many aspects of the LC and its historical implications]; Singer-Avitz 2002;² Zarzecki-Peleg 1997). Today, most archaeologists in Israel still hold with the conventional chronology, while a few support the LC (Gilboa and Sharon 2001, 2003; Gilboa, Sharon, and Zorn 2004; Herzog 2002 but no more in Franklin [Chapter 18, this volume]; Herzog and Singer-Avitz 2004 (see footnote 2); Knauf 2002).

The period under debate is framed by upper and lower anchors. The upper anchor is in the 12th century BCE, represented by the well-established correlations between the Egyptian Twentieth Dynasty and several sites in the Levant, in particular Beth-Shean (Level VI of the University of Pennsylvania excavations and Strata S-4 and S-3 of the Hebrew University excavations), Megiddo (Stratum VIIA), Lachish (Level VI), Tel Sera' (Stratum IX), and the Timnah Valley copper mines (Mazar 1990: 295-300; 1993; 2002: 264-72). The end of these strata, in several cases as a result of violent destruction, occurred towards the end of the Egyptian presence in Canaan, during the reigns of Ramesses IV to VI, until ca. 1140/1130 BCE. The lower anchor is related to the Assyrian conquests between 732 and 701 BCE. Destruction layers related to these conquests were identified at many sites such as at Dan, Hazor, Tel Kinneret, En-Gev, Beth-Shean, Tel Rehov, Megiddo, Yoqne'am, Samaria, Tell el-Far'ah, Khirbet Marjameh, Timnah (Tel Batash Stratum III), Lachish (Stratum III), Beth Shemesh and Tell Beit Mirsim. Related destructions occurred also at Tel Beer-Sheba (Stratum II) and Arad (Stratum VIII). There is a consensus concerning the dates of these destructions and thus they can be taken as a datum line for further discussion. Between these two secure anchors is a period of about 400 years, which leaves us with enough room for a continuous debate.

A major point in this debate is the question of whether we are able to establish secondary chronological anchors between the two main ones mentioned above.

There are two such minor anchors on which all the sides in this debate agree. The first is represented by the site of Jezreel excavated by Ussishkin and John Woodhead (1997). The history of Jezreel is known only from the Hebrew Bible, yet all scholars agree that Jezreel was indeed the second residence of the Omride Dynasty and that it was destroyed soon after the end of that dynasty, ca. 840–830 BCE. Pottery assemblages from the destruction of Jezreel can thus safely be dated to this time. Orna Zimhoni, who published the pottery from Jezreel, pointed out its similarity to the pottery from the 'Solomonic' Stratum VA–IVB at Megiddo, and this was one of Finkelstein's main arguments for lowering Megiddo Stratum VA–IVB to the time of the Omride Dynasty in the 9th century BCE. However, Zimhoni has also shown that similar pottery was found in the construction fills below the royal enclosure of Jezreel, probably originating from a dismantled earlier village that could date to the 10th century BCE (Zimhoni 1997: 29-56). This suggests that the same pottery assemblage continued throughout much of the 10th and 9th centuries. Such a long duration of the same assemblage was also observed at Hazor, Tel Rehov, and other sites, and this, in my view, is the key to the resolution of our debate.

Another secondary chronological anchor is related to Arad and the Negev Highlands sites. All agree that at Arad, either Stratum XII or Stratum XI must be identified as the 10th-century settlement mentioned in Shishak's inscription (Aharoni 1981: 182-91). Many of us agree that it should be the Stratum XII village rather than the Stratum XI citadel (thus Finkelstein 2002b;

2. I included Singe-Avitz in this category since her conclusions concerning Arad and Lachish in the cited paper fit the conventional chronology. In Herzog and Singer-Avitz (2004) both authors accept the long duration for the Iron Age IIA (most of both the 10th and 9th centuries BCE) as suggested by me since 1997.

Herzog and Singer-Avitz 2004; Mazar 1990: 373; Singer-Avitz 2002).³ Arad thus provides an agreed reference point for the pottery of the Northern Negev in the second half of the 10th century. Yet this agreement works against Finkelstein's LC, since the pottery assemblage from Arad XII is identical to that found at other sites that have been dated to the 10th century BCE according to the conventional chronology, like Lachish Stratum V and various parallel levels (Mazar and Cohen-Panitz 2001: 277-79; Singer-Avitz 2002: 114). Concerning the Negev Highland settlements, Finkelstein (1984) dated them to the 11th century BCE, forcing an historical interpretation which would fit this period; more recently he lowered this date to the 10th century BCE, in accord with his LC (Finkelstein 2002b) and thus he now agrees to the mainstream conventional dating of these sites (Cohen 1980; Cohen and Cohen-Amin 2004; Haiman 2003; Herzog and Singer-Avitz 2004: 225-26; Mazar 1990: 390-96). He also accepts the view, long ago suggested by Cohen, Meshel, and others, that these sites should be identified with the dozens of Negev sites mentioned by Shishak. The interpretation of these sites as related to the United Monarchy as suggested by Cohen and others remains in my view the most feasible one. The pottery from the Negev Highland sites is typical Iron IIA pottery. If Arad XII and the Negev Highland sites can be dated to the 10th century BCE according to both the conventional and the LCs, so can be Lachish V, Beer Sheba VI-V, Tell Beit Mirsim B3, Tel Batash IV, Tell Qasile IX-VII, Gezer VIII, Beth-Shemesh IIA, and so on, all with the same pottery. This conclusion (based, as mentioned above, on agreements between all sides in this debate) makes the LC impossible, at least in Judah, Northern Negev, and the southern coastal plain. Consequently, the picture emerging concerning the status of Judah in the 10th century BCE must differ from that described by Finkelstein (1999a).

Shishak's raid has been considered by many as a benchmark for the late 10th century BCE, yet there are diverse views concerning the question of whether Shishak indeed destroyed cities, and if so, which archaeological levels can be identified as having been destroyed by him. Our suggestion that a vast destruction layer at Tel Rehov should be attributed to Shishak since the place is mentioned in his inscription and since this destruction can be dated to the second half of the 10th century (Bruins, van der Plicht, and Mazar 2003; Mazar 1998) was severely attacked by Finkelstein and Piasezky (2003). Yet at the same time Finkelstein (2002b) suggested that a series of other destructions should be attributed to Shishak, even at sites not mentioned in his list, such as Tel Miqne-Ekron Stratum IV. This dual approach remains mysterious to me. We should either believe that Shishak simply moved through the country without causing destructions (thus Na'aman 1998) or leave open the possibility that indeed he destroyed cities and settlements (perhaps only partly), and in such a case the search for such destruction layers remains a legitimate one, particularly in a place like Rehov which is mentioned in his list. This latter approach is more feasible in my view. Lawrence Stager brought up the case of Taanach as an example of a city mentioned in Shishak's list where only one destruction level—that of Period IIB—can be identified as the city destroyed by him. The pottery from this level is identical to that of Megiddo VA-IVB, and thus another benchmark for the 10th century BCE may be suggested. Finkelstein tried to resolve the Taanach case by lowering Period II to the 9th century BCE and Periods IA and IB to the 10th century BCE, yet this claim must be rejected on the basis of pottery analysis: as shown by Rast (1978) the pottery from Period IA-B is close to that of the Late Bronze Age and fits the 12th century BCE (Mazar 2002: 278-79).⁴

3. A variation is Herzog's view (2002: 92-93) that Arad XII was constructed during the 10th century BCE, but continued to survive well into the 9th century. This suggestion is not repeated in Herzog and Singer-Avitz (2004), where Arad XII is dated to the 10th century alone, before Shishak's raid.

4. Finkelstein's discussion of Taanach (1998b) is based on dismissing the pottery from Period I as irrelevant due to the fact that most of it included only sherds. Yet such a dismissal stands against the principles of archaeological investigation. Rast's conclusions were carefully crafted and should not be rejected.

Thus, Jezreel, Arad, the Negev Highlands, and Taanach may be taken as ‘mini-anchors’ in the problematic 400-year time-span described above. Evaluation of these points of reference negates Finkelstein’s LC.

The Modified Conventional Chronology and Iron IIA Material Culture

The results of the archaeological work of the 1990s and renewed analysis of various sites led me to change my previous view and accept Aharoni and Amiran’s scheme from 1958 with some modifications. Recognizing the long duration of the Iron IIA pottery period, I suggested that the boundary between Iron I and Iron II be placed somewhere in the first quarter of the 10th century BCE (an estimated date is ca. 980 BCE) and that the end of the Iron IIA period should be some 150 years later, after the end of the Omride Dynasty and the destruction of Jezreel, ca. 840/830 BCE (Mazar in Coldstream and Mazar 2003: 40-44; Mazar 1997: 164; Mazar in Mazar and Carmi 2001: 1340). This scheme enables the definition of three major pottery periods in the 450 years between ca. 1150 and 700 BCE: Iron IB, Iron IIA, and Iron IIB, each with regional variations and each lasting about 150 years (Table 2.1). This scheme is supported by ¹⁴C dates from Tel Rehov (Mazar *et al.* [Chapter 13, this volume]). Such a scheme may be defined as ‘Modified Conventional Chronology’ (MCC) for the Iron Age in Israel. It was recently accepted by several scholars as the best resolution for Iron Age chronology (Ben-Shlomo, Shai, and Maier 2004: 2; Herzog and Singer-Avitz 2004).

In contrast to Finkelstein’s view, I suggest that during the first half of the 10th century BCE a major change took place in the material culture throughout the country; this change brought to an end the Canaanite Second Millennium culture as is best demonstrated by Stratum VIA at Megiddo and related strata in the northern valleys, such as Yoqne’am, Dor and Tell Keisan. The new material culture is characterized by various aspects—from new modes of pottery production (dominance of red slip and hand burnish, disappearance of the Canaanite painted pottery tradition) to settlement patterns, architecture and religious art.

It should though be acknowledged that a definition of the material culture of the United Monarchy is strewn with difficulties. Since archaeology supplies the only first-hand evidence for this period, apart from Shishak’s inscription at Karnak, it is essential to define properly which archaeological remains can be dated to the time of this kingdom. The MCC with its long duration of the Iron Age IIA suggested above makes this goal hard to achieve, since we cannot say categorically whether a certain Iron IIA archaeological context belonged to either the 10th or the 9th centuries BCE. In my view both options are open in many cases, while Finkelstein’s view does not leave such an option and according to him all Iron Age IIA contexts should be dated to the 9th century BCE alone (2004: 185). I claim that the archaeological picture is far from being ‘crystal clear’, and that the traditional paradigm of ‘the archaeology of the United Monarchy’ remains a legitimate possibility, though not mandatory (for summaries see Dever 1990; Mazar 1990: 368-402). Thus, I see no difficulty in retaining the ‘Solomonic’ date of the monumental palaces 6000 and 1723 at Megiddo. Their dating to the 9th century BCE (a main point in Finkelstein’s theory; see also Franklin [Chapter 18, this volume]) would leave for the entire 100 years between ca. 980 and 880–860 BCE, a poor ephemeral occupation level at Megiddo (Stratum VB). This is not impossible, but not very feasible, especially when taking into consideration the tight stratigraphy and pottery developments at sites like Hazor and Tel Rehov, and the clear 10th-century BCE date of two Iron IIA levels at Tel Rehov. The strongest point in favor of a 9th-century BCE date of the Megiddo palaces is their building technique and masons marks which resemble those at Samaria (Finkelstein 2004: 185; Franklin [Chapter 18, this volume]). Yet this resemblance can be explained if we assume that both kings—Solomon and Ahab—used Phoenician masons. Builders families or

builders guilds might have retained similar building techniques and masons marks during a time period of less than one hundred years. Thus, this argument should not be taken as decisive.

Herzog and Singer-Avitz (2004: 227-36) proposed to divide the Iron IIA in Judah into two sub-phases—Early Iron IIA and Late Iron IIA—with the border between them ca. 900 BCE. In their view, the occupation strata of the earlier phase represent unfortified villages which cannot reflect the existence of a state in the 10th century BCE, while urbanism and monumental architecture started only with the second phase and reflect the emergence of Judah as an independent state from ca. 900 BCE. Thus, in their view even according to the MCC there is no place to a strong United Monarchy in the 10th century BCE. This may be correct to some extent, though I am not convinced that we can be so precise in dating certain Iron IIA strata to either before or after the critical line of ca. 900 BCE.

A Few Words on ^{14}C Dating

Since the mid-1990s, the potential of radiometric dating in helping to resolve the Iron Age chronological debate has been realized. The first studies on this subject were carried out in the laboratory of the Weizmann Institute of Science at Rehovot under the supervision of Israel Carmi. The samples measured in those years came from Dor (Gilboa and Sharon 2001, 2003; Sharon 2001), Tel Beth-Shean and Tel Rehov (Mazar and Carmi 2001; some measured at Tucson), Tel Hadar, and Megiddo. Since 2002 about 60 dates from Tel Rehov were measured at the Groningen University laboratories in the framework of a joint research directed by Hendrik Bruins, Hans van der Plicht, and myself (Mazar *et al.* [Chapter 13, this volume]). A large scale project on this subject is currently being conducted by I. Sharon, E. Boaretto, A. Gilboa, and T. Jull (Chapter 6, this volume). Since this is the main subject of the present volume, I will limit myself here to few comments as a consumer of ^{14}C dating over the past seven years.

In the Beth Shean Valley Archaeological Project (Tel Beth-Shean and Tel Rehov excavations) we obtained about 100 ^{14}C dates from the Early Bronze I through the Iron IIA periods, measured in four different laboratories. The results enable appreciation of both the capabilities of the method as well as its limitations and possible flaws. The many stages of selecting the samples, the pre-treatment, the method and process of dating, and the wide standard deviation of Accelerator Mass Spectrometry dates may create a consistent bias, outliers, or an incoherent series of dates. The calibration process adds further problems, related to the nature of the calibration curve in each period. In our case, there are two difficulties: one is the many wiggles and the shape of the curve for the 12th–11th centuries BCE. This leads to a wide variety of possible calibrated dates within the 12–11th centuries BCE. The other problem is the plateau between 880 and 830 BCE, and the curve relating to the last third of the 10th century BCE, which in certain parts is at the same height as the 9th century BCE plateau. In many cases the calibrated dates of a single radiocarbon date is in both the 10th and the 9th centuries BCE, and in the 9th century BCE more precise dates between 880 and 830 BCE are impossible. These limitations are frustrating, and make close dating during this time frame a difficult task. It seems that in a debate like ours, over a time-span of about 80 years, we push the radiometric method to the edges of its capability, and perhaps even beyond that limit. In spite of these difficulties, our series of dates from Tel Rehov measured at Groningen provided a coherent sequence that fits the MCC as suggested above (see papers on Tel Rehov in this volume).

Gilboa and Sharon (2002, 2003; Sharon *et al.* [Chapter 6, this volume]) suggested, albeit hesitantly, an ultra-LC based on 22 dates prepared by Israel Carmi at Rehovot during the 1990s. Similarly, Finkelstein and Eli Piasezky (2003) base their arguments for a LC on dates measured at Rehovot during the same years, of samples from Tel Hadar, Tel Rehov, and Dor. However, Bruins, van der Plicht, and myself have shown that dates of samples from Tel Rehov measured at Rehovot

during the mid- to late 1990s were consistently lower by about 100 years than the dates of samples from the same loci or very close loci from identical stratigraphic horizon measured at Groningen using two different methods (for details see Mazar *et al.* [Chapter 13, this volume]). Yet even in the series of coherent dates from Groningen, we have some outliers. We await results of the project directed by Boaretto, Gilboa, Jull, and Sharon, in which samples from many sites have been dated, but for the time being, we have to rely on the published evidence, and this comes from a few sites that have yielded contradicting results. Some of the Iron IIA samples from Strata VI and V at Tel Rehov were definitely dated to the 10th century BCE alone, and Bayesian statistics made on samples from Tel Rehov indicate a transition date between the Iron Age I and Iron Age II somewhere around 980–970 BCE (Bruins *et al.* [Chapter 19, this volume]). Even Finkelstein and Piasezky (2003: 288–92) admitted, based on these dates, that the Iron IIA probably started around 920 BCE, higher than the general 9th-century BCE date given to this period previously by Finkelstein. The difference between us now is merely 60 years for the beginning of the Iron IIA and zero for its end. This difference corresponds to the time-frame traditionally assigned to David and Solomon. Is it a coincidence? I doubt it.

The Low Chronology and Biblical History: Some Problems in Interpretation

A major difficulty in dealing with our subject is the biases relating to biblical and extra-biblical texts. Traditional ‘biblical archaeology’ always dealt with the question to what extent certain archaeological assemblages could be related to certain historical events, phenomena, and personalities described in the Bible or mentioned in written sources outside the Bible. The historicity of the biblical narrative concerning the period of the Judges, the United Monarchy, and the emergence of Israel and Judah as states have come under scrutinizing, critical, and skeptical review in the past two decades. Many authors have pointed out the biased positions of traditional biblical archaeology in these matters. Scholars belonging to the ‘Albright school’ tradition and the much-related ‘Israeli school’ represented by Benjamin Mazar, Yohanan Aharoni, Avraham Malamat, Yigael Yadin, among others, and their followers, were accused of a naive reading of the Bible and a simplistic, straightforward interpretation of the archaeological data, perceived as no more than an illumination of the biblical account (see Dever 2001: 23–52 for a survey of the various views and references). On reading the vast literature of the past two decades, one cannot avoid asking whether scholars who are trying to deconstruct the traditional ‘conservative bias’ are not biased themselves by their own historical concepts. In other words, it seems to me that the same charges used against conservative traditional biblical archaeologists can be made against a broad spectrum of minimalists, revisionists, post-modernists, or whatever term we use for a variety of current writers. All too often, archaeological issues are mistreated by scholars of all various schools of thought when it is used for historical interpretation. Lack of first-hand knowledge of the data, false methodologies, and pre-conceptions often result in biased, sometimes unacceptable, interpretations.

Some authors have claimed that archaeology should be used in a more sophisticated manner, and indeed that it, and not the text, can and should serve as the major source for objective interpretation of what actually occurred during those hundreds of silent years, for which we have almost no textual sources other than the biblical narrative itself. This approach gave archaeology additional weight as a supposedly objective source for evaluating questions like those regarding the origins of Israel and the rise of the Israelite state. But can archaeology indeed respond to such a challenge?

Table 2.2. Synchronic table of main Iron Age sites in Israel.

(Note that in this table no attempt was made to illustrate the possible subdivision of the Iron IIA period to 'pre-Shoshenq' and 'post-Shoshenq' sub-phases—for such an attempt see Herzog and Singer-Avitz 2004: 227-36)

Site	Iron IA 1200–1140/30 BCE	Iron IB 1140/30– 1000/980 BCE	Iron IIA 1000/980–840/830 BCE	Iron IIB 840/830–732/701 BCE	Iron IIC 732/701– 605/586 BCE
Dan	VI Vb Va		IV	III	II
Hazor	XII/XI		Xb Xa IXb IXa VIII	VII VI Vb Va	IV III
Tel Kinneret		V	IV III	II	I
Beth Shean (Pennsylvania)	Level Lower VI	Part of level Upper VI and part of Lower V	Part of Lower V	Upper V IV	
Beth Shean (Hebrew University)	S-4 S-3 N-4 Q-1	S-2	S-1 P-10 P-9	P-8 P-7	(P-6)
Tel Rehov	D-7 D-6	D-5 D-4 D-3 VII	VI V IV	III	II
Megiddo	VIIA	VIB VIA	VB IVB–VA	IVA	III
Taanach	IA	IB	IIA IIB	III IV	V
Yoque'am	XVIII	XVII	XVI XV XIV XIII	XII	XI
Tel Qiri		IX VIII	VIIA	VIIb–VIIc	VI V
Tell Keisan	13	12 11 10 9c-9a	8c-8a	7 6	5 4b 4a
Tell Abu Hawam		IV-1–IV4	IV5 III	0	
Dor*	B/4? G/11a?	B/13–B/9 G/10–G6b D2/12–D2/8c	B/8 D2/8c G/6a	Various remains	Various remains
Tel Mevorakh		VIII	VII		
Tell el Far'ah		VIIa	VIIb VIIc	VII d	VII e
Samaria (pottery periods)		I	I–II	III IV V VI	VII
Jerusalem (Y. Shiloh)	15		14 13	12	11 10
Aphek	X10	X9	X8	X7 X6	
Tell Qasile		XII XI X	IX VIII		'VII'
Gezer	XIV	XIII XII XI X	IX VIII	VII VI	V
Beth Shemesh**		III 6 5 4	IIa–IIb 3	IIc 2	1
Timnah (Tel Batash) Mirsim		V	IV	III	II
Ekron (Tel Miqne)	VII	VI V IV	III	II	1b 1a
Ashdod	XIIIb	XIIIa XII XI	X IX	VIII	VII VI
Gath (Tel Safit) (temporary)			5 4	3	
Lachish	VI	Gap	V IV	III	II
Tel Sera'	IX	VIII	VII	Gap?	VI
Tel Beer Sheba		IX VIII VII	VI V	IV III II	
Arad			XII XI	X IX VIII	VII VI
Tel Masos and Negev Highland	III II		I Negev Highland sites		

* after Gilboa and Sharon 2003. ** Upper line after E. Grant and G.E. Wright; Lower line Tel Aviv University excavations directed by S. Bunimovitz and Z. Lederman, information accepted May 2005.

Finkelstein's works provide ample examples of the methodological problems related to this type of research. The historical implications of his LC relate to both the period of settlement and that of the United Monarchy of David and Solomon. Regarding the former, the LC should have caused him to reshape his previous views concerning the Israelite settlement in the hill country, including the interpretation of his excavations at Shiloh and 'Izbet Sartah as well as his views concerning the settlements in the Northern Negev (Tel Masos) and the Negev Highlands (Finkelstein 1984). For example, the LC creates a possible time span of some 300 years for the hill country 'settlement' material culture assemblage, represented in sites like Giloh, Ai, Shiloh, Mount Ebal, and so on, and characterized by Collared Rim jars.⁵ This is an unfeasible conclusion in light of the thin accumulation and short lifetime of almost all the 'settlement' sites in the highlands. Alternatively, Finkelstein's numerous suggestions concerning many sites attempt to fit the results to his LC despite much evidence to the contrary. Numerous examples were surveyed by me in previous studies and will not be repeated here (Mazar 1997, 2002, 2004). It will suffice to restate a few points such as the abrupt emergence of a sophisticated Israelite state in the 9th century BCE; the condensing of too many archaeological strata (at Hazor, Tel Rehov, and elsewhere) into a short period of less than 70 years in the 9th century BCE; the distorted and incorrect conclusions relating to certain sites like Taanach (see above), Beth Shean, and others; the description of Megiddo as an unfortified city in the 9th century BCE, when most other Iron Age cities were well fortified; the unfeasible lowering of the date of the Stepped Structure in Jerusalem to the 9th or 8th centuries BCE. The inner contradiction of the LC relating to Arad was mentioned above (for these and additional points see Kletter 2004).

Finkelstein's LC fits the historical perspective suggested by several authors over the past decade, namely, that the kingdom of David and Solomon did not exist in reality, and that the first Israelite state was the Northern Kingdom of Israel in the 9th century BCE (see Finkelstein 1999a, 2004; Finkelstein and Silberman 2001: 123-45; several papers in Handy [ed.] 1997). Yet one is obliged to ask the chicken-and-egg question: Was the LC born as result of an independent archaeological endeavor or was it made to fit a certain historical paradigm? In each of Finkelstein's articles since 1996, the archaeological discussion and the historical evaluation of the United Monarchy are intermingled. He and Ussishkin have strongly attacked the approach of the traditional 'Albrightian' biblical archaeology, which, in their view, attempted to fit the archaeological evidence into a conservative interpretation of the biblical narrative (for example Ussishkin 2003: 532). Do we now see the same method in reverse? If the answer to this question is positive, the new paradigm is no better than the traditional way of thinking in biblical archaeology.

Conclusions

The suggested MCC appears to be the most reasonable and acceptable chronology for the 10th–9th centuries BCE in the Southern Levant. It indicates a long duration of the same material culture throughout most of these centuries, and also results in an even and logical subdivision of the period from ca. 1130 to ca. 732/700 BCE into three more or less even time units, each with its own material culture traits. This view appears to have become the dominant among many archaeologists currently working in Israel. This MCC fits the archaeological data from many sites, and the radiometric dates from Tel Rehov. One problem with this chronology is the difficulty in separating

5. Collared Rim jars are known to appear during the 13th century BCE in well dated contexts such as Aphek and Beth Shean. According to the LC they were still abundant in what supposed to be 10th century BCE contexts such as Megiddo VIA (for their appearance in this level, see Harrison 2003). The result, according to the LC, is a 300 years duration for the Collared Rim jars; the hill country settlement sites are 'floating' in this wide chronological span.

the 10th century from the 9th century BCE. Current and future research in well stratified sites might enable more refined separation between early and late Iron IIA assemblages, as indeed was already suggested in several cases (the Northern Negev, Dor, Tel Rehov and other sites; see papers in this volume by Mazar *et al.* [Chapter 13], Sharon *et al.* [Chapter 6], Zarzecki-Peleg [Chapter 22], as well as Gilboa and Sharon 2003; Herzog and Singer-Avitz 2004).⁶

In historical terms, the MCC utilized here enables to retain most of the traditional archaeological picture relating to the 10th century BCE and the United Monarchy, though skepticism may arise even according to this system. Yet even if we accept the traditional 10th century BCE dates of strata like Megiddo VA–IVB, Hazor X, and so on, and buildings like the Stepped Stone structure in Jerusalem, the emerging archaeological picture would not necessarily point to a United Monarchy of an excessive size or magnitude. It rather indicates a shift in the material culture during the 10th century BCE, revival of urban life in a slow process (including the erection of fortifications in certain selected sites), limited monumental architecture which indicates central administration (at places like Megiddo, Gezer, and Jerusalem), evidence for revival of trade with Phoenicia and Cyprus, and limited (probably indirect) connections with Greece (Coldstream and Mazar 2003). The network of settlements in the Northern Negev and the Negev Highlands may be related to copper production at Feinan which flourished during this time and perhaps was related to the emergence of Edom (Levy *et al.* [Chapter 10, this volume]).

The kingdom of David and Solomon has to be evaluated as a modest yet vivid beginning of the Israelite monarchic period; elsewhere I summarized the data suggesting that the United Monarchy has to be evaluated as an exceptional and temporary achievement resulting from the abilities, charisma, and achievements of the two successive rulers—David and Solomon—in a time of a political vacuum and in a limited geographic scope (Mazar 2004: 19–22). The name of the kingdom of Judah as *bytdwd* ('The House of David') as documented on the 9th century BCE Aramean stele from Tel Dan, is an exceptional extra-biblical evidence for the impact of David as a historical figure. Archaeology is very limited in its ability to evaluate the historicity of the biblical narrative relating to the half-century or so of David and Solomon. It can provide us only a general framework into which we may fit some of the finds related to this period. It is our primary duty, before any further interpretation, to date correctly the finds, and this is the goal of the present Symposium.

Postscript

In a paper dedicated to the intercomparison of ¹⁴C dating at Weizmann Institute, Tucson, and Groningen laboratories (with positive results relating to the correlation between currently measured dates in all three laboratories), Boaretto *et al.* (2005: 46) bring results of 32 dates from 10 sites, including from Tel Aviv University excavations at Megiddo. No calibrated dates are presented in the paper, since the interest of the authors was in comparing the BP dates. Yet a calibration of these dates with OxCal 3.9 software fits with my MCC and contradicts the LC.

Most revealing are the results from Megiddo. Two dates from Stratum K4 which correlate with Stratum VIA are presented in Table 2.3.

6. Gilboa and Sharon (2003), based on a study of the pottery sequence at Tel Dor, suggested to subdivide the Iron I and IIA (as appear in Table 2.1, *EAEHL* and my usage) into seven phases, four of them corresponding to the Iron Age IB (denoted by them Ir1a(e); Ir1a(l), Ir1a/b and Ir1b). This division of the Iron Age IB (as used by me here), is unjustified in my view in light of the continuity in pottery traditions throughout this period. The complex terminology used by these authors is not of great help. Their phase Ir1/Ir2 appears to correspond to the end of the Iron IB (parallel to Megiddo VIA). Herzog and Singer-Avitz (2004) suggested subdividing the Iron IIA into two subphases: early and late. This approach may be justified in light of their study of southern sites and our results at Tel Rehov, as presented in Chapter 13, this volume.

Table 2.3. ^{14}C of seeds from Megiddo Stratum K-4 (=VIA).

Sample No.	BP	1 sigma	2 sigma
RT3944	2957±31	1260–1230 (14.7%) 1220–1120 (53.5%)	1300–1040 (95.4%)
T18163a	2864±40	1130–970 (61.2%) 960–940 (7%)	1190–1170 (2.2%) 1160–1140 (1.9%) 1130–910 (91.3%)

The first is too high by at least one hundreds year even for the conventional chronology which dates the destruction of Stratum VIA ca. 1000 BCE. The second fits the conventional chronology. Both dates contradict Finkelstein's attribution of the destruction of Stratum VIA by Shishak.

Two dates from Megiddo Stratum H-5 (=VB-IVA) (Table 2.4) are of special interest since this is the 'Solomonic' city according to Yadin, while Finkelstein suggested lowering this stratum to the 9th century BCE. The two dates are clearly in the 10th century BCE; the second one is even too high according to the conventional chronology.

Table 2.4. ^{14}C of seeds from Megiddo Stratum H-5 (IVB-VA).

Sample No.	BP	1 sigma	2 sigma
T18167	2796±28	1000–985 (7%) 975–900 (61.2%)	1010–890 (86.2%) 880–830 (9.2%)
RTT3949	2859±34	1110–1100 (3.3%) 1080–970 (55.1%) 960–930 (95.4%)	1190–1180 (1%) 1130–910 (94.4%)

The main results of the additional dates published in this paper mostly fit the MCC. The following is a superficial survey.

Iron I

The dates from Iron I pits at Hazor are mostly too early for the Iron I date of this stratum; the latest are in the mid-11th century BCE; dates from Tel Rehov D-4, Tel Miqne VB and Tel Keisan 9a fit the conventional dating in the 11th century BCE; the 7 dates from Yoqne'am XVII fit the conventional late 11th-century BCE date for the destruction, except one that is too high (yet made on charcoal) and one that is somewhat too low. Of the four dates from Tell Qasile X, three fit the conventional date in the late 11th century BCE, and only one is an outlier (9th century BCE). One date from Tel Rehov D-3 fits, or at least don't contradict, our results as presented in Chapter 13 of this volume.

Iron IIA

Dates from Tel Rehov, Rosh Zayit, Hazor IX, and Bethsaida are all in the 10th or late 10th–early 9th centuries BCE and are in line with the conventional dates as suggested by the excavators (two of the Tel Rehov dates are somewhat too low). Yoqne'am XIV which was dated by the excavators to the second half of the 10th century BCE is dated by three samples to the 9th century; Hazor X provided one date in the 10th century BCE and three at the second half of the 9th century—two of these being too low even according to the LC, and appearing to be outliers. Two dates from Hazor IX fit the excavators date in the early 9th century BCE.

Thus, these rich new data from Megiddo support in my view the MCC as presented in this paper.

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