

McDONALD INSTITUTE MONOGRAPHS

An Age of Experiment: Classical Archaeology Transformed (1976–2014)

Edited by Lisa Nevett and James Whitley



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with contributions from
Susan E. Alcock, Giovanna Ceserani, Alexandra Coucouzeli, Thomas W. Gallant, Michael Given, Jonathan M. Hall, Paul Halstead, Sturt W. Manning, Ian Morris, Lisa Nevett, Robin Osborne, Sara Owen, Gillian Shepherd, David B. Small, Jeremy Tanner and James Whitley

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Chapter 9

Events, Episodes and History: Chronology and the Resolution of Historical Processes

Sturt W. Manning (Cornell University)

> But does the absolute chronology really matter? Is it not the relative dating of the episodes within the Aegean area which really counts, and does this not stand independently of actual dates in years? The answer is that absolute chronology matters very much, as soon as connections are drawn (Snodgrass 1985, 36)

In his landmark vigorous critique of traditional Classical Archaeology published three decades ago in the *American Journal of Archaeology*, Snodgrass (1985, 36–7) discussed the issue of whether (then divergent) radiocarbon dates from the second millennium BC Aegean should be taken seriously. He ended with the challenging conclusion that:

> Either radiocarbon dates, which elsewhere were often exclusively relied on for the dating of archaeological sequences, should not be relied on after all; or the conventional chronology was significantly wrong.

It has taken the subsequent three decades – such are the forces of conservatism in the field despite the calls of Snodgrass – but today an answer appears (almost) clear: the conventional chronology was significantly wrong as it relates to the mid-second millennium BC (see Manning *et al*. 2014; Manning 2006; Manning 2014a; Höflmayer 2012a; Höflmayer, Kamlah *et al*. 2016; Höflmayer, Yasur-Landau *et al*. 2016). I will come back to justifying this (for some still) startling claim below, but, in the first part of this chapter in honour of Anthony Snodgrass (and thanking him for his support as I started my academic career), I wish to address the further question of what this conclusion actually means (apart from adjusting numbers in textbooks and on museum labels) when, as Snodgrass highlighted, 'connections are drawn'. Even at the macro scale of analysis (e.g. Broodbank 2013), I suggest this is non-trivial and historically structuring for the second millennium BC. In wider terms, it represents an opportunity (to try again in the model of Snodgrass) to break with the rigid nineteenth-century structuring of most classical and east Mediterranean archaeology and history, and indeed the wider white colonial historical framework of the modern 'western world'. Instead, we have the chance now to situate correctly in time (and thence in place, literally and historically) the various entangled civilizations of the east Mediterranean region of the second millennium BC that form the origin soup from which the first millennium BC 'classical world' emerged.

Meanwhile, Snodgrass (1983; 1987, 36–66) also drew attention to the event-historical problem in archaeology and ancient history. He observed how various differing processes of long to short timescale combine and conflate to form the archaeological record, but when archaeologists excavate – and usually but a small portion of the possible archaeological record – scholarship then arbitrarily and inappropriately often tries to force this evidence into a smooth, unitary, historical timescale of individuals and specific years. But any critical consideration should instead simply admit that '...archaeological material and historical events are hard to bring together, because they represent different facets of human experience' (Snodgrass 1983, 150). Examples of such problems abound and the sphere for their observation has only increased since Snodgrass wrote the studies cited. In particular, we may observe an important new area where different facets, timescales and resolutions meet and yet, apparently, an all too simple history (or rather, hypothesis) often emerges. I refer to a number of studies on climate and archaeology, and wish to express a degree of caution, concern and skepticism at current approaches in

several instances. In the second part of this chapter I look briefly at the relationships of chronology and climate with the close of the Late Bronze Age and the Iron Age resurgence (eighth century BC). There is a tempting hypothesis – but so far we lack the necessary resolution, associations and control.

This chapter could therefore be said to be about details and the application of critical skepticism. The model is Snodgrass': forensic attention to the details as the basis of a robust archaeology and history, and so the means to move beyond the naive and a fixation with supposed 'events', to address instead structuring processes, individuals and history. From small things…. This highlights the continuing relevance of classical archaeology both as a field and as a form of training. On the one hand, the critical thinking through the details of material culture and context against the constraints and possibilities provided by texts and centuries of scholarly endeavour provide an unrivalled intellectual foundation (challenge), and data-rich opportunity to investigate current concerns (like climate change and its human impacts, or how material entanglements shape human societies) back over a long, richly documented, time-depth. While on the other hand, a better understanding of the story of, and the reverberations of, 'the irradiation of European barbarism by Oriental civilisation' (Childe 1958, 70) remains today still central to analysis and critical appraisal of 'Western' civilization and its interlocutors.

Correcting the clock: the Hyksos world and the history of the Aegean and East Mediterranean

I wish to focus on just one fundamental issue: placing the Hyksos and the rise of the east Mediterranean super-site of Avaris (the Hyksos capital) into its rightful and seemingly central place in a narrative of the east Mediterranean region, together with discussions concerning the development of Cyprus, Crete and Mycenaean Greece (which also goes some way to explaining later memories of this period in classical texts).

Balter (2006, 508) reports from and then quotes Jeremy Rutter to highlight the problem in most scholarly work up until now:

> Egyptologists have tended to discount the importance of the Hyksos, whom Ahmose eventually chased out of Egypt: 'The Hyksos have gotten lousy press.'

Bernal (1987) also rightly identified this bias and missing topic area regarding the Hyksos in the historiography of the second millennium BC. The Hyksos were until then somewhat consigned to the category of a problem by scholarship: for example the first line of the preface and of chapter 1 of Van Seters (1966) begin respectively: 'The problem of the Hyksos…' and 'The Hyksos problem…'. Bernal sought – not entirely successfully – to suggest a much greater relevance for the Hyksos in the history of the wider region and especially the Aegean (and see further in chapters 8 and 9 of Bernal 1991); against Bernal's over-dramatic claims for a Hyksos role in the Aegean, see, for example, Coleman (1996, 281–5; also Vermeule 1996).

Much of the cause for this relative obscurity for the Hyksos in modern scholarship was because it was only with the long-running major excavations at Tell el-Dab'a under Manfred Bietak starting in 1966 (e.g. Bietak 1979; 1996), and the discovery there of the enormous major (capital) site of the Hyksos world (see also http://www.auaris.at/index.html – accessed September 2014), that the debated historical text information – almost all from the Egyptian side and so negative or cursory, and/or much later and difficult to interpret with any historical rigour – started to fall into some sort of proper perspective. Whereas the Kamose stela texts (Smith & Smith 1976; Redford 1997, 13–15 nos.68–9) refer to the 'great man/chief … in Avaris' – the Hyksos king and his capital Avaris – and describe the harbour and many ships of 'Avaris on the Two Rivers', earlier scholarship struggled to identify a suitable site with which to associate Avaris (see e.g. Van Seters 1966, 127–51 and compare with von Beckerath 1970, 312 on this issue). Similarly, until the extraordinary scale of Avaris became clear – the super-site of the Mediterranean for over a century – it was difficult to appreciate the likely reality (in some form) behind historical memories of this great centre of trade, contact and cultural influence as reflected in later echoes through first millennium BC textual sources. Bernal was prescient also in highlighting the issue of possibly changing dates in the mid-second millennium BC – encapsulated by the question of the timing of the enormous Thera (Santorini) volcanic eruption (the problematic topic addressed towards the end of Snodgrass 1985) – which indeed turns out to be part of the solution towards a proper timeframe and history of the Hyksos period and its impact on the east Mediterranean and the origins of the Late Bronze Age in Cyprus and the Aegean.

Bietak unearthed the great site, and, as summarized in a useful volume of papers (Oren 1997), the Hyksos world started to take on important material and historical reality. Broodbank (2013, 383–6) in his monumental history of the Mediterranean, continues to correct the long imbalance. He spends several precious pages in his tightly rationed account of the Mediterranean over the past 1.8 million years highlighting

how the capital of the Hyksos, the site of Tell el-Dab'a/Avaris in the Nile Delta, becomes *the* super-site and *the* major centre of the entire east Mediterranean for over a century (until its demise at the hands of the rising 18th Dynasty of Upper Egypt under Ahmose). With typical panache, he writes:

> …Avaris went supernova after 1650 BC. From merely ranking as one of the biggest Mediterranean urban settlements of its day it expanded to a size estimated at 250 ha (620 acres), the largest city yet seen in the basin [the Mediterranean] by a wide margin… (Broodbank 2013, 383.)

Broodbank somewhat dwells on the historical contingency and bifurcation of the end of the site, seeing this as a major historical turning point. In a subsequent comment, he adds:

> One good place to start (once, and in certain respects even before, its sequence relative to the entire eastern Mediterranean is finally agreed upon) will be the demise of Hyksos Avaris, which seems to denote a real division in potential historical pathways. How different would the subsequent history of eastern Mediterranean commerce have been if this vast trading *entrepôt* had seen off the up-river threat from Thebes? (Broodbank 2014, 119.)

Broodbank's comment highlights two key, but until now problematic, issues:

(i) It is time now to address the issue of dating and to place the Tell el-Dab'a archaeological sequence relative to the rest of the east Mediterranean. The chronology imposed on Tell el-Dab'a by Bietak over five decades of prolific publication has until now shaped the historical narrative. But this chronology is incorrect (and so also is the existing historical narrative). First, as discussed below, the supposed key archaeological-historical linkages that Bietak employed to construct a site chronology are all likely incorrect or nonexistent, and second, detailed analysis of radiocarbon dates from the site indicate a very different chronology (but one consistent with radiocarbon-based dating elsewhere in the eastern Mediterranean). Thus the place of Avaris in the history of the region needs significant reassessment.

(ii) While, as Broodbank notes, the demise of Avaris was a substantive historical episode, I wish to argue that it is in fact the long century-scale period *before* – when Avaris was by far the greatest city of the Mediterranean – that is particularly significant and which had the real historical structuring force. Moreover, with a new chronology for the site, this beginning is earlier than previously thought, and so the floruit is longer. Avaris and the associated Hyksos world became *the* driving centre of trade and much else in the eastern Mediterranean over a period of towards 150 years. Rather than – as often before – looking to downplay the wide distribution of Hyksos items (from Crete to Bagdad) (as, for example, Yurco 1996, 89), it becomes more apparent that the world-system centred at Avaris was a driving force in the (long) seventeenth century BC (end eighteenth to early sixteenth centuries) that formed the context for the creation of the subsequent Late Bronze Age world of the east Mediterranean (and much of the legendary background to the classical Greek world).

I suggest that the Hyksos and their historical importance are very much a case where 'absolute chronology matters very much, as soon as connections are drawn' (Snodgrass 1985, 36). Such an application of critical re-thinking, and a move to a mature field beyond the credulity of youth, may also start to overcome the unease memorably expressed about Aegean (and wider east Mediterranean) Bronze Age studies by Snodgrass (1985, 35–7).

Principia falsa: the lack of an archaeological-historical chronology for Tell el-Dab'a

For several decades it has been argued that there is a tightly constrained archaeological-historical chronology for the stratigraphy excavated at Tell el-Dab'a. Although there have been some questions regarding some of the assumptions involved over the years (see e.g. Bietak 1979; 1984; 1991 versus Dever 1985; 1991; 1992; and Weinstein 1991; 1995; and see also Warburton 2009), Bietak proposed his chronology and subsequently strongly and prolifically enforced it, attempting to crush any questions or dissent against all critics (e.g. Bietak 2000; 2003; 2004; Bietak and Höflmayer 2007). Bietak has been largely successful, remarkably making only minor adjustments over several decades despite excavating a huge site notable for its lack of secure stratified contexts. The end result is that for the past couple of decades the Bietak chronology for the site has not only become regarded widely as fact, but has also been held to form a key chronological reference from which other chronological associations may then supposedly be

made (e.g. Bietak & Hein 2001; Bietak *et al*. 2008; Bietak & Kopetzky 2009). In reverse, if any other regional chronological sequence was proposed which was at odds with Bietak's Tell el-Dab'a chronology, then it was held *a priori* to be wrong (and so impossible) (e.g. Bietak 1998; 2000; 2003; 2004; 2013; 2014; Bietak & Höflmayer 2007). If radiocarbon dating was at odds with Bietak's Tell el-Dab'a chronology, then it, too, had to be wrong (Bietak 2013; 2014). Thus the Bietak chronology defined the history of Avaris (for just one typical example, although Bourriau 2000 at p.177 notes 'fierce debates', on her p.181 she then simply shows the Bietak chronological scheme, and employs these dates in her text when providing an account of the Second Intermediate Period = SIP), and was also held as the bulwark which rendered impossible any suggestions (from e.g. radiocarbon) of any substantial revision of any associated east Mediterranean or Aegean mid-second millennium BC chronologies.

The trouble is that on critical examination it turns out there is no sound archaeological-historical basis to the Bietak chronology: the whole edifice is built on some false assumptions (Manning *et al*. 2014). This is a serious charge. Let us therefore review the essential points again here.

> The stratigraphic series [Tell el-Dab'a (ancient Avaris and Peru-nefer)] is fixed near its lower end (Phase K) by a stela of the fifth year of Senwosret III (*c.* 1889 BC … [a typographical error – Bietak means *c*. 1868 BC], subsequently by seal impressions of the Hyksos Khayan (*c.* 1600 BC) and the conquest of Avaris under king Ahmose (*c*. 1530 BC) and at its upper end by scarabs of Thutmose III (*c.* 1479–1425 BC) and Amenhotep II (*c.* 1425–1400 BC). (Bietak 2014, 279.)

As quoted, Bietak (also 2013, 80; Bietak & Höflmayer 2007, 14 and 15 fig.2) has argued that the Second Intermediate Period timespan at Tell el-Dab'a is delimited between start and end 'datum lines': a stela mentioning the 5th year of Sesostris III for the start of Stratum K, and the conquest (and abandonment) of Tell el-Dab'a by Ahmose at the end of Stratum D/2. Two other key linkages are also stated: scarabs and especially a palace of late Stratum E/1 and early Stratum D/3 at Tell el-Dab'a linked to the Hyksos king Khayan, and, from Stratum C/2, 'numerous scarabs from the 18th Dynasty with the latest from Tuthmosis III and Amenhotep II' (Bietak 2013, 80) which have been used to date the palatial-scale platforms to the Tuthmosid era. However, all of these datum lines and linkages are simply non-existent.

Sesostris III: There is a stela mentioning the 5th year of Sesostris III (*c.* 1868 BC: Bietak & Höflmayer 2007, 14–15) which Bietak treats as a key *terminus ante quem*. However, there is no evidence demonstrating how it links to the Tell el-Dab'a stratigraphy – it is only relevant if one makes the *assumption* that the stele is contemporary with the construction of the temple of 'Ezbet Rushdi. But this is unclear: let us just read the recent assessment of Czerny (2012, 61) after he notes the Bietak assumption:

> However, this is far from being certain. It remains unclear, whether the stele relates to the original establishment of the temple [Stratum K] or to an enlargement only. Even that it is unrelated to this particular temple, but was brought here from a proximate structure cannot be excluded. A few particulars of the stele, including the fact that the king's name is not enclosed into a cartouche, could possibly imply that it is not a contemporary document, but rather stems from the SIP.

Thus this is a very questionable 'datum line'.

Ahmose: The name of Ahmose is not attested in connection with the supposed Ahmose destruction/conquest of Tell el-Dab'a. There is just a long-standing assumption – since we have approximately contemporary and later texts saying that Ahmose conquered Avaris (e.g. Redford 1997, 15 no. 70) – that the major change in the character of occupation at the site, from a citadel to a large storage facility at the end of Stratum D/2, must be the period when Ahmose conquered the site. This has become a circular argument. Bietak *et al.* (2007, 39 and n.71) present the Ahmose assumption and refer to 'the abandonment of the Hyksos citadel and the signs of its destruction and looting', citing as support Fuscaldo (2000, 8, 19). But Fuscaldo (2000: 8) merely says '[a]ccording to our opinion the smashing of those pots happened in connection with the conquest of Avaris by Ahmose' – there is no actual evidence – and on page 19 Fuscaldo makes it clear this position is 'according to Bietak's opinion'. Again there is no evidence. In fact, Fuscaldo (2000, 19) opens up a very different scenario if we read the entire relevant paragraph:

Most of the vessels found in Locus 66 [the subject of the book] are complete or nearly complete, which implies that they were not thrown away on the street because they were broken. This fact and the kind of pots found in this assemblage – mainly cups, bowls and jars, and some amphorae and dipper juglets – show

that this pottery could have been used for a celebration and then thrown away on the street…

That is until Fuscaldo then ends this sentence with the uncalled-for specific event-historical destruction association (a dangerous approach and often a trap for scholarship since, as noted above, 'archaeological material and historical events are hard to bring together, because they represent different facets of human existence': Snodgrass 1983, 150):

> …on the street after the conquest of Avaris by Ahmose, according to Bietak's opinion. …

Fuscaldo does at least show an awareness of some alternative possibilities in the final sentence of the paragraph:

> … Another possibility is that the street could have been used as a dumping place at the beginning of the 18th Dynasty, before the destruction of the Hyksos complex for the construction of the new one.

If we ignore for a moment 'Bietak's opinion', this is a dump of feasting or similar material (relevant storage vessels and drinking/eating vessels). It is near a palatial platform, so it is likely a dump related to elite activities. With respect to the date, there is no evidence other than the ceramic material which is almost exclusively Hyksos (see rest of Fuscaldo 2000). Nothing actually points to Ahmose, or to the start of the 18th Dynasty, or to the effects of a conquest, except assumption – rather we have pottery used in a celebratory context (e.g. a feast) and then thrown away – and no obvious association with the conquest of Ahmose! If the platform and surrounding area were dated Hyksos (see below), then in fact the dump could simply be a Hyksos period dump and (even more clearly) nothing to do with Ahmose.

Overall, there is no positive evidence for the supposed Ahmose link with the end of Stratum D/2. Such a change could of course be associated instead with other changes and other persons. It is at present *un*-dated.

Khayan: Bietak associates Khayan with a late Stratum E/1 and early Stratum D/3 palace. This connection itself is less than certain since the key sealings naming Khayan come from a secondary context in a large offering pit (Bietak 2011, 41; Bietak *et al*. 2012–2013, 25). But the real issue here is the dating of the Hyksos king, Khayan.

Khayan is dated *c.* 1600–1580 BC by Bietak and has usually been placed towards the end of the 15th (Hyksos) Dynasty (Bietak 2013, 84–6). However, recently reported finds of a number of sealings of Khayan in a well-stratified context at Tell Edfu in Egypt, in near association with those of the Thirteenth Dynasty king Sobekhotep IV (who dates in the later/late eighteenth century BC: e.g. 1709–1701 BC in Hornung *et al*. 2006, 492, or 1738–1731 BC in Kitchen 2000), seem to indicate that Khayan instead dates substantially earlier, by around some 80 years or so (Moeller & Marouard 2011). A new report from Tell el-Dab'a also indicates that Khayan belongs early in the Fifteenth Dynasty (Forstner-Müller & Rose 2012–2013). If the dates for Khayan are moved back, then the dates for the Tell el-Dab'a stratigraphy would need substantial revision – assuming of course that the association of Khayan to the palace of late Stratum E/1 and early Stratum D/3 is valid. As discussed below, the radiocarbon evidence from Tell el-Dab'a also indicates the need for just such a revision.

It is worth noting that the historical evidence for the Hyksos period is either very late and problematic, or very fragmentary (as highlighted in the discussions of Ryholt 1997; Schneider 2006, 192–6; Krauss & Warburton 2006, 480–1), and does not preclude some significant revision. The relevant section of the Turin Papyrus lacks names for all but the last king, Khamudy, and even the overall length of the 15th Dynasty, usually read as 108 years, could turn out to be longer as the reading is actually '[Total:] 6 [Hyk]sos. They ruled 1[0]8 [years]' (Ryholt 2006, 29; 2004, 142). The possibility of a new reading of more than 140 years by Ryholt is mentioned preliminarily (Schneider 2006, 194 n.126 in an editors' note). With the conquest of Ahmose and the end of the 15th Dynasty somewhere in the mid-sixteenth century BC, the available evidence could let the 15th Dynasty start as early as around 1700 BC, versus the usual guess of *c.* 1650 BC. Such a change, as well as a re-ordering of the kings, with Khayan moved now to the early part of the 15th Dynasty, offers the basis for a substantial reappraisal of the history and connections of the Second Intermediate Period (Moeller & Marouard 2011).

18th Dynasty Scarabs: Some 30 scarabs 'inscribed with royal names, ranging from Ahmose to Amenhotep II' were found in a 'small building compound (I) of the 18th Dynasty' (Bietak *et al*. 2007, 27). This everyone can agree. The issue is the relationship of this small building compound with the palatial platform forming Palace F (associated with the Minoan-style wall paintings) which Bietak dates as Tuthmosid (for small building compound I, and its location in relation to Palace F, see Bietak *et al*. 2007, 21, 22 fig. 15). The walls of the small building compound (I) 'abutted on the eroded eastern ramp attached to Palace F' (Bietak *et al*.

2007, 27). There can be no doubt that this compound *post-dates* the platform – thus the find-contexts of the New Kingdom scarabs are from *after* the supposed Tuthmosid palace structures, and hence the association and dating of Palace F and associated structures as Tuthmosid is in fact questionable – since the (agreed) subsequent building has scarabs 'inscribed with royal names, ranging from Ahmose to Amenhotep II' and Ahmose and his successor Amenhotep I are both *pre*-Tuthmosid. The so-called Tuthmosid Palaces are not associated with any king's names. Simple stratigraphic logic would suggest they are at least older than the small building compound (I), and, if the scarabs there start with Ahmose, then the palace structures could well be pre-Ahmose – i.e. Hyksos. Bietak *et al*. (2007, 27) recognize this logic, noting 'This was one of the reasons why, at the inception of the excavation, Palace (F), which was built before the supposed workshop, was dated pre-Ahmose' (as in Bietak 1992). Only subsequently Bietak decided this was not the interpretation he wanted, and, despite the clear stratigraphic relationships stated, the whole area, platforms and small compound, all became earlier 18th Dynasty (as Bietak *et al*. 2007). Thus on archaeological grounds the stated Tuthmosid dating of the palatial structures has no actual basis – the kings' names relate only to a separate and later building compound. Further, as discussed below, the radiocarbon evidence from Tell el-Dab'a indicates that the original assessment based on stratigraphy was likely correct: the palatial structures are not Tuthmosid but Hyksos in date – undermining the entire Bietak chronology.

In the light of these complications and problems with each and all of the assumed/stated key Bietak dates and associations, it is clear that there is no sound archaeological-historical chronology for Tell el-Dab'a. There is, therefore, simply no basis to any consequent argument by Bietak based on the false assumption of his claimed archaeological chronology.

Is there an alternative chronology for the site which makes sense? Yes: the radiocarbon evidence offers a coherent alternative picture, consistent also with the apparent likely archaeological dating of both the pre-18th Dynasty palatial platforms and the indications (now) of an earlier date for Khayan: see below. With Bietak's supposed Tell el-Dab'a chronology removed as an obstacle to using a radiocarbon-based chronology, not only for Tell el-Dab'a itself, but for the rest of the east Mediterranean (i.e. exactly contrary to the arguments of, for example, Bietak 2013; 2014; Bietak & Höflmayer 2007) – Tell el-Dab'a has long been held to be the key or only real obstacle – we may also regard the Aegean 'high' radiocarbon-based chronology for the beginning of the Late Bronze Age as confirmed as the likely and plausible time-frame of reference (and see further in Manning 2014a; Manning *et al*. 2014). Indeed, a combination of other recent work now indicates just such a compatible 'earlier' date for the end of the Middle Bronze Age right across the Levant, consistent both with the Tell el-Dab'a *radiocarbon* (not Bietak) chronology and the 'high' Aegean chronology (e.g. Höflmayer, Kamlah *et al*. 2016; Höflmayer, Yasur-Landau *et al*. 2016; Manning *et al*. 2016).

Radiocarbon, Egyptian chronology, and a new timescale for Tell el-Dab'a

For many years there was a perception that radiocarbon could, at most, be tested or corrected by Egyptian chronology. Egyptian historical chronology was not regarded as perfect, but it was considered as set within small (one to a few decades at most) limits back through the second millennium BC (e.g. Kitchen 1991). It was thought that radiocarbon had nothing much to offer in the other direction. More particularly, there was a perception based on early work that there were in fact some conflicts between Egyptian chronology and some radiocarbon dates, as exemplified by the chronological fault line of Renfrew (1973, 116 fig. 21), or papers such as Hood (1978) entitled 'Discrepancies in 14C dating as illustrated from the Egyptian New and Middle Kingdoms and from the Aegean Bronze Age and Neolithic'. Some of these earlier issues related to the relatively low dating resolution possible with most radiocarbon dates at this time, in terms of accuracy and precision. Other problems stemmed from the large size of necessary samples in the earlier and pre-AMS (Accelerator Mass Spectrometry) days of radiocarbon which often defeated any real chronological precision if, for example, wood/charcoal samples with many years of growth – in-built age – were dated as one sample. Another potential problem was dating ancient recycled wood. On review, radiocarbon was still relevant and many apparent problems could be explained away (e.g. Manning 2006), but in reality it was only with the introduction of more accurate and precise radiocarbon dating, and the ability to do this on small samples via AMS, that the real relationship of radiocarbon with Egyptian history and chronology could be properly assessed.

The Bronk Ramsey *et al*. (2010) study represents the landmark study (see also Quiles *et al*. 2012; Dee 2103a; 2013b; Manning 2014a, 20–3, 116–33, 181–5; Manning *et al*. 2013; for a brief review of the topic, see Höflmayer 2016). Sophisticated analysis of a large set of AMS measured samples associated with Egyptian rulers produced a chronology which was not only compatible with the Egyptian historical chronology for the second millennium BC through early first

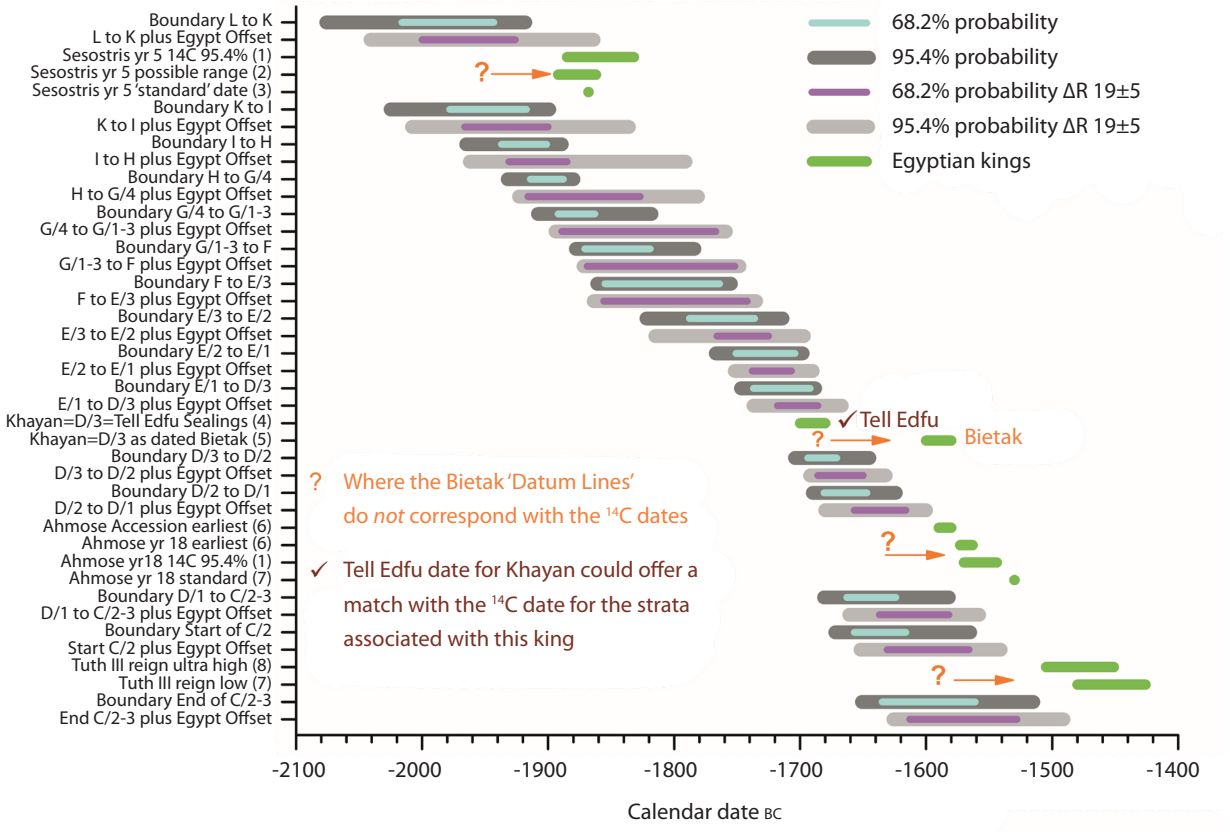


Figure 9.1. *A comparison of the modelled date ranges for the transitions between the stratigraphic phases (strata) from L to C/2-3 at Tell el-Dab'a, after Kutschera et al. (2012), but allowing also for a growing-season related Egyptian ^{14}C offset after Dee et al. (2010), versus the proposed dates for some Egyptian Kings linked to the Tell el-Dab'a sequence (Kutschera et al. 2012: Fig. 3; Bietak 2013) from historical and/or astronomical and/or ^{14}C dating. Sources: (1) Bronk Ramsey et al. (2010); (2) Schneider (2008); (3) Gautschy (2011); (4) Moeller and Marouard (2011); (5) Bietak (2013, 86); (6) Müller (2006); (7) Schneider (2010) (8) Aston (2012–2013), Huber (2011), Gautschy (2013).*

millennium BC, but was capable of refining it. This work demonstrated (once and for all) that radiocarbon works in the east Mediterranean and can yield an accurate and precise chronology. The chronological fault line in Renfrew (1973, 116 fig. 21) does not exist in reality (only in the minds of some scholars). Thus, *a priori*, radiocarbon can provide a timescale for pre and proto-historic periods in the eastern Mediterranean independent of the step-wise logic transfers inherent in the previous archaeological and art-historical modes of chronology building derived by linking, comparing, and subjectively interpreting stylistic and material traits. This includes the site of Tell el-Dab'a – especially when there is no sound basis to the assumed chronology for this site (see previous section) – and also the Aegean (e.g. Manning *et al*. 2006; 2014; Wild *et al*. 2010; Manning 2014a)

The radiocarbon evidence from Tell el-Dab'a indicates dates for the sequence from Stratum K to C as shown in Figure 9.1 (data and analysis after Kutschera *et al*. 2012). We see that most of the strata do not offer calendar ages compatible with the Egyptian kings and dates as assigned by Bietak (see also Höflmayer 2016, fig.3) – the sole exception can be Stratum later E/1 to early D/3, where the substantially revised (earlier) dating of Khayan recently proposed (Moeller & Marouard 2011) (see above), can match the radiocarbon dating of the palace context at Tell el-Dab'a linked with this king. The radiocarbon evidence would place the palace complex of late Stratum E/1 and early Stratum D/3 at Tell el-Dab'a, with which Khayan is suggested to be associated (Bietak 2013, 84–6), somewhere in the later eighteenth to early seventeenth century BC (Fig. 9.1). To quantify this, the Stratum E/1 to D/3 transition (so about the middle of this period of time) is placed at 1718–1686 BC (68.2 per cent probability) and 1737–1667 BC (95.4 per cent probability) (see Fig.9.1, taking data from the Kutschera *et al*. 2012 model, revised using

IntCal13 from Reimer *et al*. 2013 and with allowance for an Egyptian growing season offset of 19±5 ^{14}C years following Dee *et al*. 2010). This dating can match nicely the new early 15th Dynasty placement and earlier dating of Khayan (see above). The new Khayan scenario finds further potential support from the discovery of a cuneiform letter fragment of later Old Babylonian type from the Khayan palace context at Tell el-Dab'a (Bietak 2013, 84; Bietak & Forstner-Müller 2009, 115–18): since, if we accept on the basis of recent work that the Middle Old Babylonian chronology (or a chronology close to this – the 8 years lower so-called Low Middle Chronology) is most likely (e.g. Barjamovic *et al*. 2012; Roaf 2012; de Jong 2013; Manning *et al*. 2016), then this too indicates a similar date range (and not a chronology a century later as the Bietak chronology would require).

This time (Strata E/1 to early D at Tell el-Dab'a) – Khayan – is generally when the site of Tell el-Dab'a/Avaris becomes a Mediterranean basin super-site and *the* major centre in the east Mediterranean and region (until its demise at the hands of the rising 18th Dynasty of Upper Egypt under Ahmose) (Broodbank 2013, 383–6). Khayan appears to have had widespread contacts based on the distribution of artefacts with his name (Mellink 1995; Moeller & Marouard 2011: 106 and n.59 with references), and apparent contacts such as the Old Babylonian type letter fragment from his palace area at Tell el-Dab'a (above). I discuss this further below.

Meanwhile, a variety of the most recent historical, astronomical, and radiocarbon-based work all tend to indicate that the date for the start of the Egyptian New Kingdom is a little earlier than the previous default range of *c.* 1550/1540 BC (Aston 2012–2013; Gautschy 2013; Huber 2011; Bronk Ramsey *et al*. 2010; Quiles *et al*. 2012; Manning 2014a, 20–3, 116–33; 181–3). Müller (2006) had previously suggested a maximum New Kingdom start date of 1590/1580 BC, and the Bronk Ramsey model, re-run employing the subsequent IntCal13 calibration dataset (Reimer *et al*. 2013) and revised with a combination of the Aston (2012–2013 'high' and Schneider (2010) New Kingdom historical chronologies, suggests a consistent but 'high' 95.4 per cent range for the accession of the first king, Ahmose, 1585–1563 BC (Manning 2014a, 184). These new dates are only a few decades earlier than the previous standard dates (*c.* 1550 BC or 1540 BC). They suggest that Ahmose's historically attested conquest of Avaris is likely to occur in the earlier to at latest mid-sixteenth century BC. The prior super-site of Hyksos Avaris and its transformational ('going supernova') impact on the wider east Mediterranean therefore lies in time from around the late eighteenth century BC (to early seventeenth century BC) through to this point – somewhere around 130–150 years or so representing a pivotal, but, until now, rather over-looked or forgotten era of time in the history of the circum Mediterranean region! (Such were the efforts and success of the subsequent Theban 18th Dynasty in all but removing the Hyksos from history – Van De Mieroop 2011, 144–9. The Hyksos period became one of three great traumas, and what were subsequently conceived of as triumphant reversals, in collective ancient Egyptian history: Assmann 2014, 73.)

Super-site Avaris and Mediterranean History

Dates matter. Broodbank (2013, 383) writes:

> …Avaris went supernova after 1650 BC. From merely ranking as one of the biggest Mediterranean urban settlements of its day it expanded to a size estimated at 250 ha (620 acres), the largest city yet seen in the basin [the Mediterranean] by a wide margin…

His date is not really explained, but it is the standard approximate date for the start of the 15th Dynasty and the start of Stratum E/2 at Tell el-Dab'a according to Bietak. Bietak (2013, 86) makes the closely related observation that in '…the reign of Khayan in whose period (Ph. E/1-early D/3) we have a boom of Middle Cypriot imports at Tell el-Dab'a…', highlighting the new wider east Mediterranean connections from this period. The radiocarbon analysis summarized in Figure 9.1 places the Stratum E/2 to E/1 transition 1738–1707 BC (68.2 per cent probability) and 1752–1690 BC (95.45 probability), and the dates for the E/1 to D/3 transition a couple of decades later have been noted above. We are talking somewhere from the later eighteenth century BC to early/earlier seventeenth century BC – and not as *formerly* thought mid- or even later seventeenth century BC.

How do the new, higher, dates for Khayan and Avaris affect Mediterranean history? The answer is in several potentially interesting ways. We can just briefly consider a few issues here.

Khayan and Crete. A cartouche of Khayan was found in the excavations of Sir Arthur Evans at Knossos. Given the pre-Tell Edfu, and pre-radiocarbon, dating of Tell el-Dab'a and Khayan, and Evan's reported find context at Knossos, this item was long held to be a key element in the case for the 'low' or conventional Aegean chronology and seemed in conflict with the 'high' radiocarbon-based Aegean chronology. In recent years MacDonald (2005, 134) and Höflmayer (2012b, 172–5) have made a reasonable case for regarding the

original find context at Knossos – despite some issues and debate – as likely Middle Minoan IIIB. Now, with the new Tell Edfu evidence and the Tell el-Dab'a radiocarbon chronology, the Knossos Khayan lid can nicely mesh with the Aegean radiocarbon-based chronology (and can become strong evidence in support of the Aegean 'high' radiocarbon-based chronology). Middle Minoan IIIB obtains a synchronism with a point around the end of the eighteenth through early seventeenth centuries BC. Even more importantly, we see the development of Second or New Palace Knossos and Crete (later Middle Minoan IIIA to start of Middle Minoan IIIB: Macdonald 2005; Knappett *et al*. 2013, 19) as very much contemporary both with Avaris going 'supernova', and with the apparently influential and widely connected reign of Khayan. The overall Middle Minoan IIIB-Late Minoan IA 'acme' of Minoan civilization, and the start of the wider Aegean Late Bronze Age, is thus contemporary with – and we may assume influenced by – the Hyksos era – contrary to long-held views that correlated the start of the Aegean Late Bronze Age more or less with the start of the New Kingdom in Egypt (e.g. Betancourt & Weinstein 1976) for no real reason other than symmetry and simplicity (and even, by the 1970s, only by dismissing radiocarbon dates).

The same temporal coincidence, and likely influence or association, applies to the transformation of southern mainland Greece in the Shaft Grave period. The Nilotic imagery and Levantine technological connections (from inlaid, niello, work, and in jewellery forms) seen in some famous elite material culture from both the Shaft Graves at Mycenae and in wall paintings at Akrotiri on Thera, can now be seen as reflecting contemporary linkages with the dominant Hyksos world of the seventeenth century BC, as can a raft of other Levantine-Hyksos imports and influences evident across a range of material culture from the start of the Aegean Late Bronze Age, some also associated with contemporary Cyprus (e.g. Manning 1999, 137–8, 141–3; Laffineur 1990–1991; 1998; Doumas 1998; Aston & Bietak 2012, 231).

With regard to Cyprus, the later Middle Cypriot import boom observed at Tell el-Dab'a (and in the Levant: Maguire 2009), and in reverse through some related exports into Cyprus, may be placed in later Middle Cypriot through initial Late Cypriot I (for material, see Bietak 2013, fig. 8.3; Karageorghis 1995; Negbi 1978) and critically includes plain ware transport vessels moving in both directions (Crewe 2015). There is again a temporal coincidence, and likely other associations, between the dramatic transformation of Cyprus from village-level to proto urban world in the Middle Cypriot III–Late Cypriot I period (or the transformation and distinction between Knapp's 2013a; 2013b Prehistoric Bronze Age or PreBA to Protohistoric Bronze Age or ProBA periods on the island) and the dramatic rise of the Hyksos-driven trading world centred at Avaris – the 'east Mediterranean's economic super-attractor' (Broodbank 2013, 385). As Broodbank (2013, 385) suggests, such visible trade can likely be viewed as 'tracers of consignments of copper' and Hyksos interest and demand may have acted as a powerful stimulus to the contemporary development of Cyprus (see also Manning 2014b) and its new trade-oriented groupings and political units (Manning & De Mita 1997).

The end of the LBA and the Iron Age resurgence – climate and chronology

There is a large literature identifying a significant collapse, decline or reorientation in the eastern Mediterranean somewhere around and following 1200 BC. One recent book is specifically entitled *1177 BC: The Year Civilization Collapsed* (Cline 2014). Of course, the one thing we know for certain is that this date is likely incorrect: assessments of Egyptian chronology, both in terms of the historical evidence and the radiocarbon evidence, indicate the cited Year 8 of Ramses III was likely just over a decade earlier (Schneider 2010; Aston 2012–2013; Bronk Ramsey *et al*. 2010; Manning 2014a, 116–27, 181–5). But issues of chronology and linkages run much wider and deeper with regard to the climate context of the end of the Late Bronze Age and the later Iron Age resurgence. A range of recent studies from several loci using varying evidence indicate a prolonged arid phase at the end of the second millennium BC in the east Mediterranean (e.g. Kaniewski *et al.* 2010; Kaniewski *et al.* 2013; Roberts *et al.* 2011; Drake 2012; Langgut *et al.* 2013), and this seems to suggest an explanatory context. However, critical review (Knapp & Manning 2016) indicates that almost as important is what we in fact do not know at present, but might perhaps suspect. Even in the much better documented and more recent Roman and Byzantine periods, the need for more data and especially data with tight chronological control, has been noted as the big need if a real integration of climate and history is to be achieved (McCormick *et al*. 2012; Manning 2013; Haldon *et al*. 2014; Izdebski *et al*. 2016) – avoiding a mere general brushstrokes approach to the pre-modern world (as one tends to find in works such as Brooke 2014), or the fear of a reductionist logic whereby events and apparent processes are elided without actually establishing real explanatory linkages (see the rather belated caveat expressed in the very final paragraph of Büntgen *et al*.

2016 versus (at least some of) the preceding text – and hence the paper forms both warning and example). When we move back to the close of the Late Bronze Age and the early Iron Age these concerns are even more relevant.

Despite a veritable plethora of recent studies announcing new evidence for climate change and trying to link it to the archaeological record, the notable problem in the east Mediterranean and especially for the BC period is the conspicuous dearth of

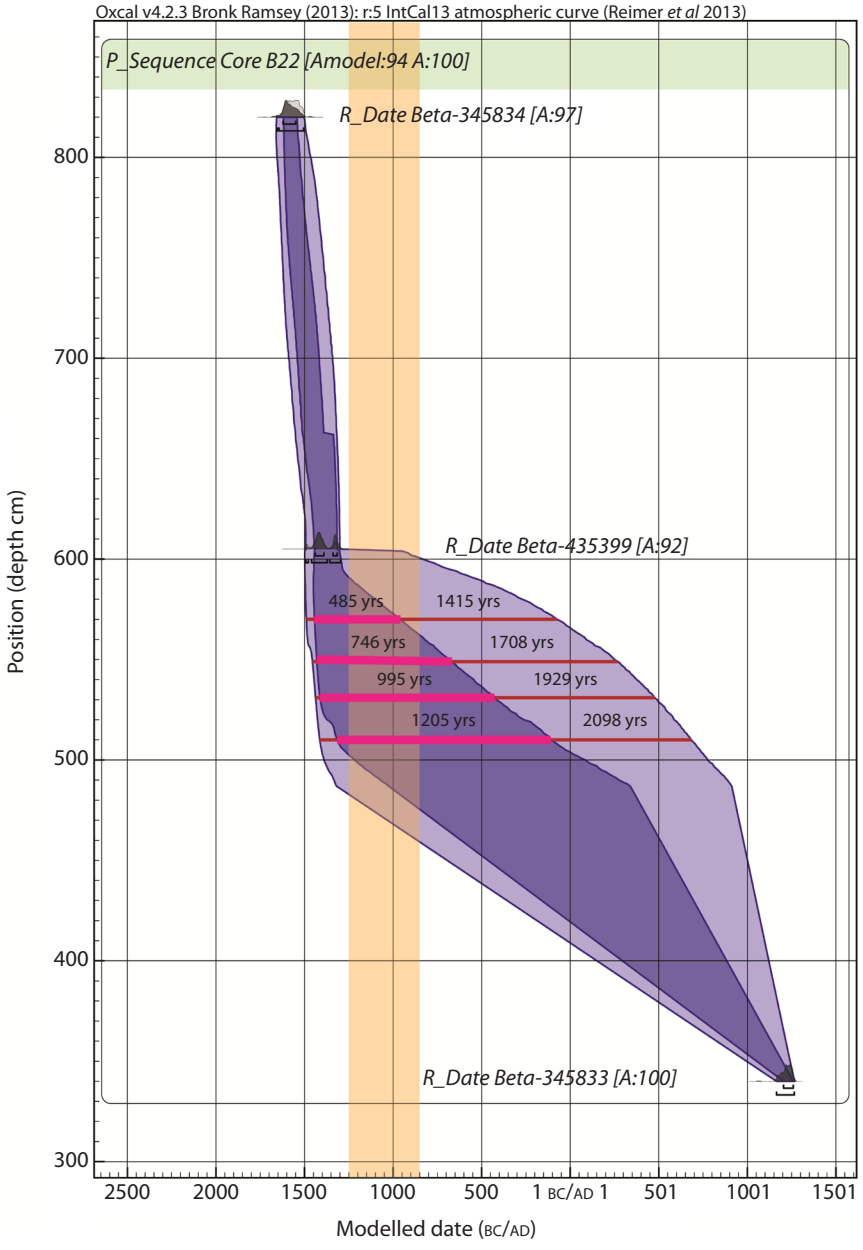


Figure 9.2. *Age-Depth model for Cyprus B22 core in Kaniewski et al. (2013), using OxCal (Bronk Ramsey 2008) with a variable k model at 1 cm resolution interpolating 1 cm depths (Bronk Ramsey and Lee 2013) and IntCal13 (Reimer et al. 2013) with curve resolution set at 5. The three radiocarbon dates modelled are shown in black and the modelled calendar age ranges – the real available precision of dating – at 68.2 per cent and 95.4 per cent probability are shown for depths 570 cm, 550 cm, 530 cm and 510 cm depth, which approximately span the mid-thirteenth to mid-ninth centuries BC. The numbers above the thick magenta bars give the spans of the 68.2 per cent probability ranges and the numbers above the thinner dark magenta bars give the spans of the 95.4 per cent probability ranges.*

high-resolution records (although very recent work such as Neugebauer *et al*. 2015 starts to address this need). The review of Finné *et al.* (2011) provides ample evidence of the problem: reviewing the last 6000 years of climate in the east Mediterranean, the authors report some 94 data sets/studies relevant to the region with a stated dating resolution, along with another 14 sets or reviews of data without chronological resolution (Finné *et al.* 2011, table 1). Of those datasets covering the end of the Late Bronze and the earliest Iron Ages, not one consistently approaches high-precision resolution. As noted by Drake (2012, 1866), the east Mediterranean region data employed in his figs 2–3 are also notable as low resolution. Several recent studies are no better: typically a small number of radiocarbon dates provide at best a vague and ill-defined chronological framework, and yet supposedly major and specific climate information is proposed. One conspicuous example is the Kaniewski *et al.* (2013) study, which places a drought episode between the mid-thirteenth to mid-ninth centuries BC, and, looking at Kaniewski *et al*. (2013) figs 3 and 4, seems quite high-resolution and specific. But, if the core that is employed, with its mere three radiocarbon dates, is modelled with a realistic Age-Depth model – see Figure 9.2 – then the dating resolution in the mid-thirteenth century BC to mid-ninth centuries BC is in fact worse than give or take several hundred years (see also Knapp & Manning 2016: fig.3). In other words: there is no real dating resolution.

Nonetheless, we may make and develop some informed hypotheses based on the solid information and indications we do have, and these do indicate some interesting historical contexts for change. The high-resolution Sofular Cave speleothem $\delta^{13}C$ record indicates – as also various of the low resolution records (see several citations above) – a shift to drier (more arid) conditions 1300–1000 BC, approximately correlating with a marked shift to cooler temperatures in the Arctic (which provide a good general guide for the whole Northern Hemisphere: e.g. Chylek & Lohmann 2005) as does – subject to some uncertainty over the correct timeframe – evidence from the SW Aegean for a shift to cooler sea surface temperatures in the late second millennium BC: Figure 9.3. Some other temperature records (from northern Spain and Romania) might on first glance appear to offer contradictory (warming) indications in this period (see Fig. 9.4), but, in the case of the Poleva Cave record from Romania, Constantin *et al*. (2007, 334–5 and fig.7) already suggest correlating the late second millennium BC temperature rise with the fourteenth century BC peak in the Greenland ice-core record and it is tempting to speculate about a similar possible correlation for the north Spain record (thus some possible minor temporal tuning among these records). In reverse, the age-depth scale for the GISP2 ice-core record as employed in Alley (2000) is likely around 60 years too old by *c*. 3300–3400 BP (Southon 2004), which may further allow all three records to synchronise with a temperature peak around or shortly after *c*. 1300 BC followed by the shift to much cooler temperatures.

The scenario could be similar to the Little Ice Age period, when drier conditions – and some prolonged drought episodes – are a feature of climate indicators for both Anatolia and parts of the Near East (White 2013; Kaniewski *et al*. 2012; Roberts *et al*. 2012). In contrast, warmer conditions, like the Medieval Climate Anomaly, are generally associated with moister conditions in the same areas. It is important to appreciate, however, that these associations are not necessarily Mediterranean-wide or even east-Mediterranean-wide. In the recent millennium there is an opposite relationship (a see-saw) between the west and east Mediterranean when considered on a longer-term (century-scale) timescale. Thus in general terms the Medieval Climate Anomaly (warmer) sees aridity in the Iberian Peninsula but moister conditions in Anatolia, whereas the Little Ice Age (cooler) see moister conditions in the Iberian Peninsula but arid conditions in Anatolia (Roberts *et al*. 2012). There appear in addition some important differences north to south in the east Mediterranean (compare Roberts *et al*. 2012, fig.1); hence we see something of an opposite pattern when comparing Anatolia and the northern Levant versus the southern Levant and parts of North Africa. Thus while central Anatolia in general enjoyed more favourable (moister) conditions during the Medieval Climate Anomaly, the southern Levant saw several major droughts (Ellenblum 2012; Raphael 2013). Episodes of prolonged drought (from different climate mechanisms) can be historically significant, both in the east Mediterranean-Near East region (e.g. White 2013; Ellenblum 2012; Raphael 2013), and elsewhere (e.g. Buckley *et al*. 2010; Cook, Woodhouse *et al*. 2004; Cook, Anchukaitis *et al*. 2010). Imposed on this pre-existing cooling trend observed from about the thirteenth century BC onwards, there is then a marked reduction in solar irradiance from the later tenth through earlier eighth centuries BC (see curves labelled as F in Fig. 9.4) which undoubtedly prolongs this already long cool (and arid) period into a multi-century episode and helps provide a context for the prolonged interruption and reorientation of civilizations in the Mediterranean observed over several hundred years from the late thirteenth to ninth centuries BC which was formerly referred to as a 'Dark Age' (and links to observed episodes of

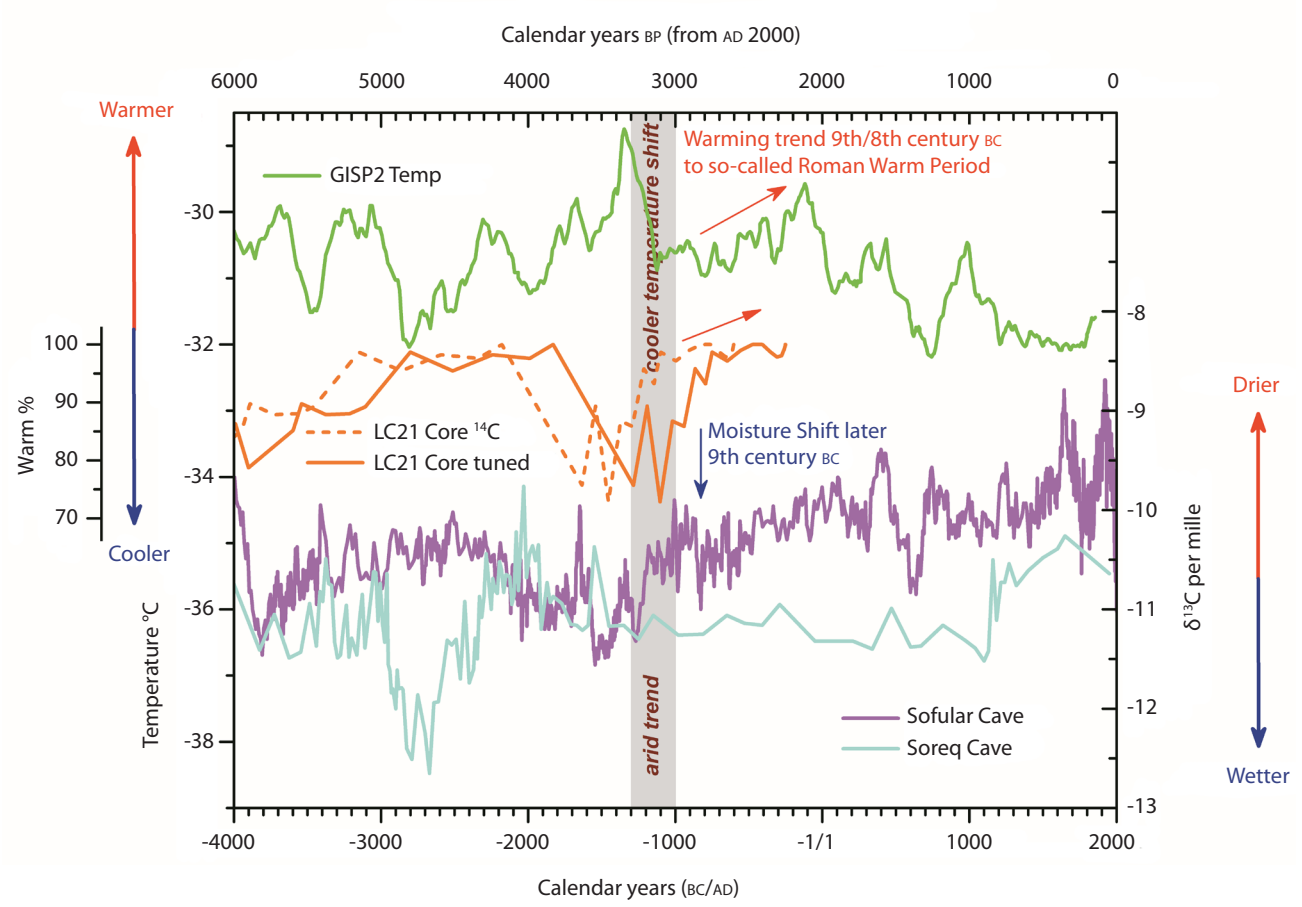


Figure 9.3. *Top: Central Greenland surface temperature proxy record from the GISP2 ice-core based on stable isotope analysis and ice accumulation data (Alley 2000; Cuffey & Clow 1997). Middle: Relative abundance (%) of Aegean core LC21 planktonic foraminiferal species with warm-water affinities from Rohling et al. (2002). Dashed line represents original ^{14}C timescale, solid line indicates the maximum correction required to match the approximate age of the Minoan eruption of Santorini/Thera. Bottom: two stable carbon-13 isotope records from speleothems from the east Mediterranean – Sofular Cave (Fleitmann et al. 2009) and Soreq Cave (Bar-Mathews et al. 2003) – offering proxy precipitation records. The grey shading indicates the cooler, more arid, period at the end of the second millennium BC.*

climate/environment change in several areas of the world around the eighth century BC as first argued by Van Geel *et al*. 1996, and since by many others: e.g. Manning 2013, 112 and references there).

In contrast, the aftermath of such a major solar minimum might, on the basis of the post-Maunder minimum period (eighteenth century AD), be expected to be a period with increased precipitation in the east Mediterranean (as evident in the Solfular Cave record in Figure 9.3; and in the reconstructions and records of e.g. Nicault *et al*. 2008 and Luterbacher *et al*. 2006). The period AD 1700–1750 forms the longest stable wet period observed in the 500-year record of Nicault *et al*. (2008, 240). It would seem likely that something similar occurred in the eighth century BC following the several decades long major solar minimum (peak) placed either in the late ninth or earlier eighth century BC (*c*. 817 BC in Steinhilber *et al*. 2009; *c*. 765 BC in Usoskin *et al*. 2007), since such a shift from arid to more moist conditions about this time is evident in several palaeoclimate records from the east Mediterranean (e.g. Kaniewski *et al*. 2011; 2013) and in the datasets summarized in Figure 9.3. This circumstance would have promoted a prolonged period of favourable agricultural conditions (rising temperatures and increased precipitation) in the east Mediterranean. In turn, this could provide an important context for the evidence of population growth and general civilization growth observed widely across the Aegean-east Mediterranean at this time, including the so-called Greek Renaissance or Structural Revolution (see also Manning 2013, 112–15).

This sketch is a tempting and attractive general hypothesis, but currently lacks control and detail. Indeed, at present, a sceptic could easily argue both that there is insufficient regionally applicable evidence to indicate any clear climate change forcing, and that what evidence there is remains typically so poorly dated and constrained that it is impossible to be certain about almost anything. The relevant historical and archaeological evidence is also, on critical examination, largely malleable and capable of plural interpretations (Knapp & Manning 2016). The recent challenge to the substantive reality – or not – of the Little Ice Age climate episode as both a quantifiable climate period and as an historically significant forcing by Kelly & Ó Gráda (2014a), however forced, selective and involving more than a little contrarianism, together with the ensuing debate in the *Journal of Interdisciplinary History* (White 2014; Büntgen & Hellmann 2014; Kelly & Ó Gráda 2014b), nicely highlights the very real challenges which face any rigorous study aiming to integrate climate

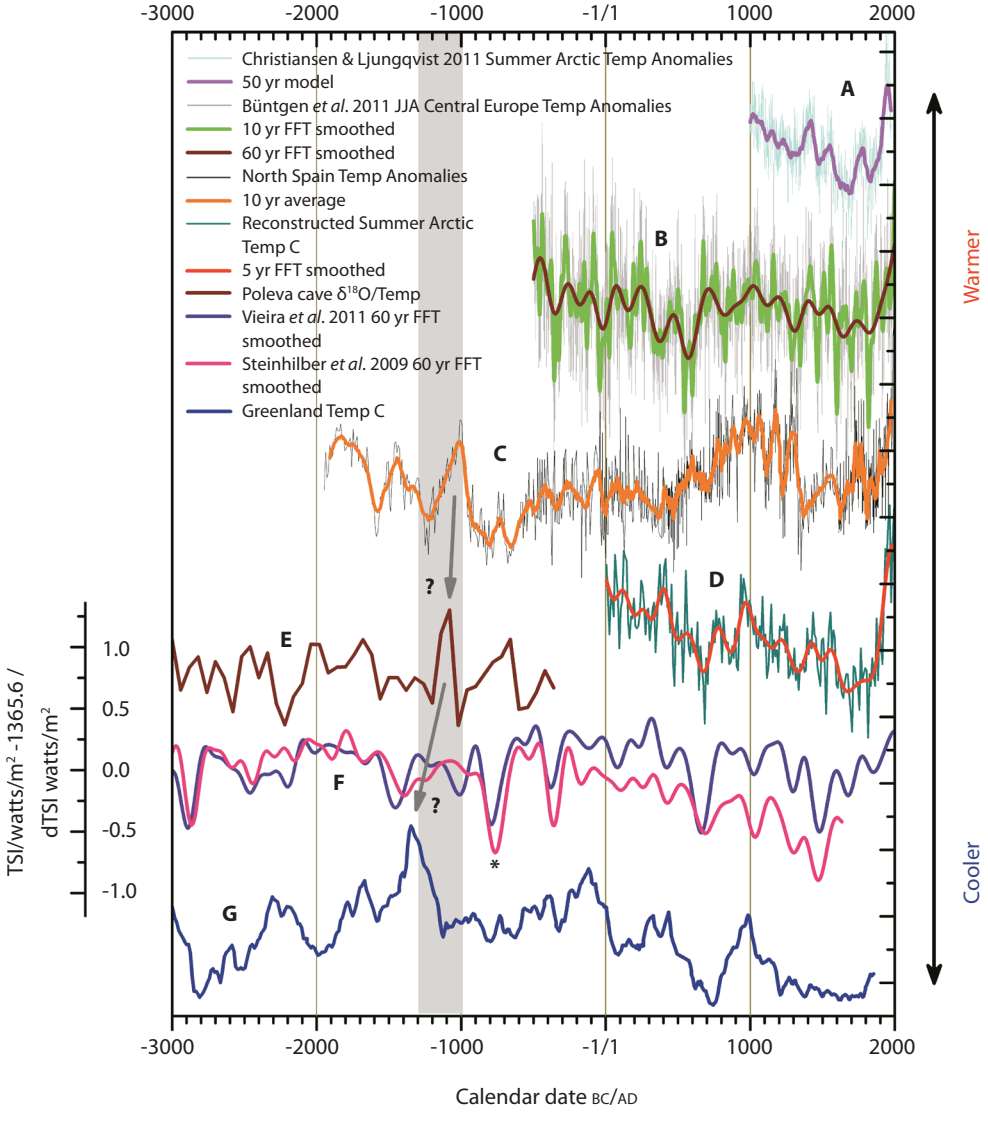


Figure 9.4. *Proxy temperature records (anomalies or proxy units) for (A) Arctic summer from Christiansen and Ljungqvist 2011, (B) Central Europe from Büntgen et al. (2011), (C) North Spain (Martín-Chivelet et al. 2011), (D) Arctic Summer from Kaufman et al. (2009), (E) Romania, Poleva Cave (Constantin et al. 2007), and (G) Greenland (as Figure 9.3). (F) shows two solar irradiance proxies (Vieira et al. 2011 from the IntCal09 radiocarbon record; Steinhilber et al. 2009 from ice-core ^{10}Be records). The grey shading indicates the cooler period at the end of the second millennium BC. The * marks the Grand Solar Minimum in the late ninth/early eighth century BC.*

into an account of the close of the Late Bronze Age to early Iron Age in the east Mediterranean, where there is much less evidence and rather less control. What are missing and needed to develop this line of enquiry are both real chronological resolution, and more climate-relevant data directly applicable to the various regions and archaeological contexts (versus remote loci and general teleconnections). Some of this chronological control and relevance needs to be acquired and applied to palaeoclimate information – for example much better dating of various marine, lake and pollen sequences, those still currently coarsely dated speleothems, and so on. But equally important is the archaeological evidence: this can offer the exciting potential to better define and characterise human-relevant climate, and provide real chronological control (in the east Mediterranean at this time through a combination of conventional methods linked to ceramic chronologies, historical evidence, and archaeological science dating), as nicely argued and articulated by Sandweiss & Kelley (2012). Such an approach can even hope to address climate scepticism of the sort advanced by Kelly & Ó Gráda (2014a; 2014b). At present in many climate-history discussions we have the situation of all-too obviously simplistic (beautiful) hypotheses over long (in human-lived terms) periods of several generations to several centuries (such and such an episode of climate change led to….), and we have to suspect that many ugly facts, contradictions, and complications will emerge, and, critically, need resolving and ordering into narrative sequence, before a final convoluted and nuanced story can eventually be written. (There are many parallels one could cite. The book by Chitty 1996 offers some nice analogies in his discussion of the process of science.)

Conclusions

We started with the question: 'But does the absolute chronology really matter?' (Snodgrass 1985, 36). The answer is yes, as the two very different cases discussed in the brief foregoing sketches show. As Snodgrass (1985, 36) argued, absolute chronology, and the correct chronology, 'matters very much, as soon as connections are drawn'. In the case of the mid-second millennium BC east Mediterranean, and the Hyksos, the correct absolute chronology offers a substantially different history – one we are only now reclaiming from the subsequent New Kingdom Egyptian attempts to all but eradicate the Hyksos era from history (allied with the assumptions of one prominent modern scholar). This extends to the appropriate context for the beginnings of the Aegean and Cypriot Late Bronze Age periods. In the case of the relevance of climate to the end of the Late Bronze Age and then the beginning of the earlier Iron Age, we have again seen that the key – and as yet largely absent – ingredient is tight chronological resolution and control. Such resolution and control are the only way very different types of data can be assessed in a relevant, contemporaneous and diachronic, framework to assess causation, relative historical importance, and trajectories.

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