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7. Mediterranean Chronology in Crisis

Peter James, Nikos Kokkinos and I. J. Thorpe

In 1990 E. Lipiński referred to a growing 'crisis' in West Semitic epigraphy. The focus of his discussion was the Nora Fragment from Sardinia, the 'crisis' springing from the serious divergence of opinion among palaeographers regarding its date – the 9th century BC according to Lipiński, but the 11th according to Cross. As Lipiński rightly stressed, the controversy over the Nora Fragment should be seen in the broader context of numerous related epigraphic tangles from the same time range. The inscribed bowl found at Tekke in Crete has created similar difficulties. Its date, originally published as c. 900 BC, was raised by Cross (1986: 125–126, n.12) to the 11th century. As it was found with Attic Late Protogeometric vessels (generally dated to the late 10th century BC), Cross suggested that PG chronology should be raised as a whole. At Tell Fakhariyah in northern Mesopotamia we have the disturbing anomaly of an Aramaic inscription, dated by most palaeographers to the 11th century (see James *et al.* 1991a: 276–277), on the statue of an official dated by Assyriologists to the mid-9th century (Millard 1993).[1] There also remains the classic puzzle of the origins of the Greek alphabet: the forms of the 8th-century Greek letters seem to resemble most closely those of the Proto-Canaanite alphabet of three centuries earlier (James *et al.* 1987: 24–25; 1991a: 81–85).

There is a strange whiff of unreality to these problems. Our approach is that there is no need to take sides regarding whether the script of the Nora Fragment belongs to the 11th or 9th centuries: *both sides may be partly right.* Instead, the debate should be widened even further, taking into account many similar tensions in Mediterranean archaeology across the same centuries. These suggest that it is *chronology*, rather than palaeography, that is in crisis. A solution to the dilemma seems to lie in shortening the overall chronological framework for the Late Bronze and Early Iron Age in the Old World, which is still ultimately dependent on the accepted chronology for Egypt. This approach may be controversial, but we are pleased to note that the proposals we made, initially in a monograph in 1987 and then in expanded book form in 1991, are attracting increasing support from new research and discoveries.

Archaeologists working in the Central Mediterranean and Aegean may not be fully aware of the critical stage which the arguments over the dating of Palestinian stratigraphy – central to the epigraphic puzzles – have now reached. W. Dever (1990: 127) remarked: 'How can we know *anything* with certainty about the past (in this case, ancient Palestine and Israel), if we cannot even date the major phases of historical and cultural development within a margin of a century or less?' Indeed – and the problem is worse than Dever imagines. For example, with regard to Edom, southern Jordan, we now find two polarized schools of thought: one (Bienkowski 1992a; 1992b) insisting that the earliest Iron Age settlements date no earlier than the end of the 9th century, the other (Finkelstein 1992a; 1992b) placing them as early as the 12th-11th century, on the basis of parallels with Israelite pottery.

Rethinking of the conventional chronology for the Israelite Iron Age has been prompted by the recent discovery of a 9th-century Aramaean stele at Tel Dan (Biran & Naveh 1993). Fragments of the inscription were re-used in Stratum III (mid-9th to early 8th century) according to the excavator Biran, but Chapman's analysis (1993–94) has argued that it was actually re-used in a gateway belonging to Stratum IV. Given this, Chapman (1994: 4) notes:

> ... it would necessitate a reconsideration of the dating of the ceramic assemblage of that stratum from 1050–950 BC (Stratum IVB), 950–883 BC (Stratum IVA), with the initial date being lowered to at least 883 BC for whichever of these phases

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marks the construction of the gate containing the inscription. Should such a chronological revision be required at Dan, it would also be required at all other sites which have been placed in the same archaeological horizon on the basis of their ceramic assemblages.

Palaeographic and historical analysis (e.g. Lemaire 1994; cf. Halpern 1994), evidently confirmed by the discovery of new fragments (Biran & Naveh 1995), has now shown that the stele actually dates from c. 800 BC, meaning that Chapman's *terminus post quem* for the construction of the Level IV gateway should be reduced even more.

Further waves are certain to be created by the find of a late PG 'bowl krater' at Tel Hadar in northern Israel (Kopcke forthcoming; cf. Waldbaum 1994: 57). It came from a sealed context in a level which the excavators believe was destroyed 'sometime in the 11th century' (Kochavi 1993: 551), yet according to the accepted PG chronology such vessels should date to c. 900 BC or later. The find has been welcomed by those who wish to raise the dates of Aegean 'Dark Age' chronology (see Morris, this volume), placing the beginning of Protogeometric at ca. 1100 or even earlier. The fallouts of such a revision would be extraordinary. The start of Early Geometric would have to be raised to c. 1000 BC. Long ago Desborough (1957: 218) warned of the consequences when a similar attempt was made to raise Aegean dates by strict adherence to a 'high' chronology based on Palestine (via Cyprus). While, we might, with Morris, question the existence of Submycenaean as a separate phase, the massive lengthening of the Early and Middle Geometric phases entailed is as unacceptable now as it was in Desborough's time – even more so given that Morris, echoing our criticisms of the Near Eastern 'fixed points' for Greek chronology, is happy to *lower* the dates for the Late Geometric. The extension of the Geometric to over three centuries (from its present two) will only attenuate further the limited material available for the interpretation of Greek 'Dark Age' culture. It seems clear that the vast majority of Aegean and Cypriot archaeologists will reject these proposals. The alternative solution is, of course, to lower the dates for Israelite archaeology.

Light on these interminable 11th-9th century controversies can be shed by proceeding from the known to the unknown, working backwards from the more securely dated archaeological sequences of the late 8th to early 7th centuries. In particular, continuing work in Phoenicia, Cyprus and Carthage over the last twenty years has sharply focussed a 'Cypro-Phoenician' pottery horizon which is firmly dated and can be used as a control over the dating of related, earlier, periods.

The Cypro-Phoenician Horizon

Though limited, the excavation of Tyre by Bikai in 1973–74 provided an Iron Age ceramic sequence – confirmed since by other excavations, notably at Sarepta – which is of vital importance for Mediterranean chronology. The discovery of an inscribed Egyptian urn of the late 25th or 26th Dynasty in the closing stage of Stratum III, sets the end of this stratum no earlier than 725 BC, or possibly later, according to Bikai (1987: 69; cf. 1978a: 68; 1978b: 47; 1981: 33; James *et al.* 1991a: 108).[2] The absolute date for the close of Stratum III is corroborated by the discovery of associated pottery of the Cypro-Archaic I period (Bikai 1978b: 47), initially dated by Gjerstad to 700–600 BC, and revised by Karageorghis to 750–600 BC (see James *et al.* 1991a: 152–153). The chronology of Cypro-Archaic I is itself fixed by synchronisms with approximately secure evidence from Greece (Late Geometric), by influences from safely dated Assyrian material and, again, by finds of Egyptian origin. For example, many scarabs of the precisely-dated 26th Dynasty (664–525 BC), found on the floor of the Period 4 sanctuary at Ayia Irini together with Cypriot Period IV wares, unquestionably set the middle of Archaic I (i.e. CAIA to CAIB) around 650 BC (see James *et al.* 1991a: 367, n. 37). Further corroboration comes from Carthage, where the earliest local pottery is associated with material akin to Tyre III–II (Bikai 1978: 54–55). Lancel's current summary of the archaeological evidence (1995: 25–34, 43, 67–70; cf. Køllund in this volume) shows without doubt that the earliest trace of habitation (tombs, *tophet* and domestic settlements) date to the late 8th/early 7th centuries: firm limits are set by the presence of a substantial amount of Greek pottery, and large numbers of Egyptian amulets and scarabs.[3]

As a result, many characteristic shapes and forms of later Phoenician ceramics – such as the Crisp-Ware storage jars and the Fine Ware plates – can thus now be confidently placed within relatively narrow time margins and used as a chronological index for sites from the East to the West Mediterranean. For example, Tyrian storage jars (so-called 'Torpedo jars'), *should* help to date archaeological strata in Israel. However, Geva (1982) questioned their Phoenician origin solely on the grounds that they appear earlier in Israel, notably in the 9th-century Stratum VII at Hazor. Bikai (1985) firmly objected to this early date and defended the Phoenician origin of these jars, which are found in great numbers at Tyre III in a pottery manufacturing area of the second half of the 8th century BC. The logical thing to do, clearly, would be to lower the chronology of Hazor VII and associated levels at other sites.

The same tension between Cypro-Phoenician and Israelite chronologies lies behind the conflict over the dating of Black-on-Red Ware. Using the firm dates for the second phase of Black-on-Red in the Cypriot Period IV (750/700–600 BC), Gjerstad – on the basis of a statistical method involving numerous sites on the island – set the beginning of the first phase of Black-on-Red no earlier than 850 BC. Naturally, the occurrence of Black-on-Red I in Palestinian contexts dated by local chronology to the 11th-10th centuries created pandemonium. The well-known debate between Gjerstad and Albright/Van Beek, raging since the 1940s, has never been resolved (James *et al.* 1987: 53–57; 1991a: 155–161). As Sörensen (1993: 18) remarked in response to our work: 'Some of the finds from Palestine appear indeed to match the finds from Cyprus to perfection, and a discrepancy of several centuries is disturbing.' The problem is highlighted by the conclusions of the excavators of Tel Mevorakh and Tel Qiri in Israel: the 'solution' at Tel Mevorakh was to introduce a four-century hiatus into the site's chronology, and at Tel Qiri to reject the dates offered by Cyprus. Recently, at Tel Dor, Gilboa (1989: 211, n. 14) simply chose to temporarily exclude Black-on-Red finds from publication, concentrating on what she classified as earlier pottery of Cypriot Geometric origin:

> We have not included in this list the Cypriot Black-on-Red vessels: their occurrence outside Cyprus seems to represent a different phenomenon [*sic*]. Their initial appearance, moreover, both on Cyprus and the mainland, is later than that of the other decorated Cypro-Geometric groups. The non-Cypriot Black-on-Red vessels [*sic*] do not concern us here [*sic*].[4]

Even if one takes the extraordinary step of ignoring the Black-on-Red finds, the other Cypriot material from Tel Dor raises many questions. Gilboa published three 'early' Cypro-Geometric fragments from Phase 9 (dated by local pottery to 1050–980 BC): a White Painted I amphoriskos, a White Painted bowl, and a Bichrome bowl. Yet it seems certain that the bowls (both of which she describes as 'advanced') are much closer to White Painted II types rather than to I. This would date them not earlier than c. 900 BC, and almost a century later than the closing date of Phase 9. One can see why, therefore, the Philistine sherds uncovered in the same context could not 'be ascribed with confidence to Phase 9' (Gilboa 1989: 205). It was precisely the occurrence of Cypro-Geometric pottery together with 'Philistine Ware' in Palestinian sites which sparked off the chronological argument between Van Beek and Gjerstad.

The Black-on-Red Ware controversy embroils the long-standing debate over the chronology of the Israelite capital of Samaria. Kenyon assigned the construction of the site to the time of King Omri (888–877 BC) – known from the Old Testament as the founder of Samaria – and, as the earliest pottery includes early Black-on-Red, her dates were broadly consistent with Gjerstad's. Wright, however, insisted that Samaria Pottery Periods 1 and 2 should date to the 10th century (and precede the Omride palace), a view which attracted increasing support among American and Israeli archaeologists. The Kenyon-Wright controversy has been simmering since the 1950s (James *et al.* 1991a: 183–185), and was recently the centre-piece of a major debate as to whether the archaeological levels in Israel deemed to be 'Solomonic' (10th century) are really 'Ahabic' (9th century BC), or vice versa (see especially Stager 1990; Wightman 1990). Tappy, after a detailed analysis of the finds from Samaria, has now plumped for a 'high' chronology, dating Pottery Period 1 in the 11th century. In doing so, he (1992: 131–132) accused Kenyon of blindly following Cypro-Phoenician 'low' dating. Tappy's mentor Stager (1990: 103) had already pinned the problem down well:

> ... A surprise number of types (of pottery from Samaria Periods I and II) ... has its most impressive parallels from Megiddo Strata VII–VI, Taanach Strata IA, IB, IIA, and Tell Qasile Strata XI–X, *where those types overlap with Philistine painted pottery, which cannot be dated much later than ca. 1000 B.C.*

The question is: on what basis has 'Philistine' pottery been dated so accurately? The answer, of course, is the hitherto generally accepted Egyptian chronology (via Mycenaean pottery styles, serving as a prototype to Philistine). But as this can now legitimately be thrown into doubt, the alternative is clear: in assessing Samaria, the Cypro-Phoenician low dating should be preferred.

Indeed, even Kenyon's much debated low dates for Samaria seem to be too high (James *et al.* 1991a: 187, tab. 8:2; see also now Forsberg 1995: 49–50). For example, it is clear that the so-called 'Samaria Bowls B', of the kind known from Tyre V–IV as Fine Ware plates (Class 2.1), begin in Pottery Period 3 (Crowfoot *et al.* 1957: 157; Tappy 1992: 159, *contra* Bikai 1978b: 52–53). This type is dated at Tyre c. 800/760–750/740 at the earliest (Bikai 1978b: 52; cf. 1981: 33–34; 1987: 69). Yet, Pottery Period 3 was dated by Kenyon to the time of King Jehu (842–816 BC), and Tappy has allocated the earliest Fine Ware bowl, on stratigraphic grounds, to the reign of Omri (888–877 BC) – impossible in view of Bikai's dates for Tyre.

The tension between Israelite and Cypro-Phoenician chronologies has now come to a head at Rosh Zayit in Galilee. Gal (1992a: 184) decided to date the destruction of this short-lived site to the middle of the 9th century, on the basis of an entirely hypothetical connection with the campaign of the Assyrian king Shalmaneser III in 841 BC. Fudging some of his local pottery, which according to Palestinian typology would date to the 10th century, but to the late 9th according to Phoenician (Gal 1992a: 175), he went on to analyse the Cypriot imports, claiming, not uncharacteristically, that Cypriot dating is too low. Indeed, Black-on-Red I does not begin before 850 BC, and some of the examples Gal illustrates (fig. 5, nos. 9–12) may well belong to the Black-on-Red II phase, conventionally dated no earlier than 750 BC!

Gal's conclusion is that there is a need to revise a large part of Tyrian chronology *upwards*. But he is confused regarding the real consequences that Cypro-Phoenician dating has for Palestinian archaeology. While raising the dates for Tyre XI–VI, Gal (1992a: 184; 1992b: 73–74) also lowers IV–I to post-700 BC! Such lowering might be welcome, but it has to be applied throughout in a consistent fashion – the 'Fine Ware plates' which Gal (1992a: 182, fig. 9:1; 1992b: 51, no. 2) claimed not to *postdate* the mid-9th century at Rosh Zayit, are now also claimed by him not to *predate* the 7th century at Tyre!

No amount of mental gymnastics will get Palestinian chronology out of the mess in which it presently rests. There is a clear need to perceive the situation from a broader perspective, taking in the extraordinary range of problems we have documented affecting areas as widely separated as Greece, Sicily, Libya, Nubia, Edom and ultimately Egypt itself, the source of the accepted dates for the Palestinian Iron Age.

Generations	**HPA Thebes**		**21st Dynasty Tanis**	
(1)	Paiankh		SMENDES	cont. Ramesses XI (20th Dynasty)
(2)	Pinudjem I	=	Henttawy	
(3)	Menkheperre			
(4)	Pinudjem II		cont. SIAMUN	

Figure 7.1. Genealogy of 21st-dynasty High Priests of Amun (HP As) at Thebes, as known from contemporary evidence. Links with the 21st-dynasty kings ruling at Tanis are shown to the right.

The Egyptian Third Intermediate Period

The fragile nature of the chronology of the Egyptian 'Third Intermediate Period' (c. 1070–664 BC), which precedes the well-established era of the 26th Dynasty, cannot be overstressed. The evidence on which the 21st Dynasty has been reconstructed is particularly slender (James & Morkot forthcoming). It presently occupies 125 years of history (1070–945 BC), covering much of the problematic area under discussion, yet the full gamut of its materials has been summarised by Niwinski (1988: 37–38) in less than one page: burials of kings, high priests, and their families; a few official scenes with inscriptions; and a handful of letters and papyri. In all other respects this 125-year slice of Egyptian history is a mysterious grey area. Records for burials of the sacred Apis bulls, known from the 20th and 22nd dynasties, are completely missing for the 21st. Likewise, as Bierbrier (1975: 45) notes:

> With the advent of Dynasty XXI the copious sources of information which were available in the previous two dynasties vanish. Administrative papyri and ostraca prove practically non-existent. Votive statuary would seem to disappear almost totally. Graffiti and inscriptions decline to a few badly preserved examples... because of this dearth of material, it is not possible as in Dynasty XIX and Dynasty XX to present a coherent outline of the descent of various families and their interrelations.

Plentiful source material on private individuals, in the form of votive statues and administrative documents, reappears under the following 22nd Dynasty (Bierbrier 1975, 54). This includes two genealogies which stretch back to the 19th. These (the genealogies of the priests of Memphis and Ankhefenkhons) have given particular trouble to Egyptologists because they are *too short* to cover the 125 years required for the 21st Dynasty on the conventional chronology – so it is assumed (Kitchen 1986: 189–192; Bierbrier 1975: 51–53; see James *et al.* 1991a: 238–242) that six to seven and three to four generations respectively were accidentally omitted by the scribes drawing up the documents!

Such an attitude, that the primary evidence must be faulty because it does not fit the accepted model, surfaces again with respect to the royal genealogy for this Dynasty. Actually, there is *no* royal genealogy. The nearest thing we have is a sequence for the High Priests of Amun at Thebes – closely interlinked with the kings at Tanis – which provides the backbone for its internal chronology. The High Priests' genealogy, reconstructed from contemporary documents, occupies four generations (Fig. 7.1). This can be instructively compared with the reconstruction provided by Kitchen, doyen of TIP chronology (Fig. 7.2): by using a stream of entirely hypothetical links, the 21st-Dynasty kings known from the monuments are strung into a neat succession, with the result that a full five generations (i.e. approximately 125 years) are made to separate Ramesses XI, last ruler of the 20th, from Shoshenq I, first ruler of the 22nd. Yet it is known from several documents that the first generation, Paiankh and Smendes, were already mature and in office by the end of Ramesses XI's reign. At the end of the 21st, a pattern of genealogical evidence (Fig. 7.3) shows that Pharaoh Siamun (from generation 4) must actually have been of the same generation as the second ruler (Osorkon I) of the succeeding 22nd (James & Morkot forthcoming). This means that as well as an overlap of one generation between the late 20th and early 21st, there was also a substantial overlap of some two generations between the late 21st and the early 22nd. (The chronological overlapping of dynasties, ruling from different centres in Egypt, was a common phenomenon in the TIP). In short, the time when the 21st Dynasty ruled as an independent entity can be whittled down to one generation (time of Pinudjem I), say about 25 years. There is nothing to prevent its chronology being reduced by a clear century.[5]

Why, then, was the 21st Dynasty allocated 125 years in the first place? It was clearly *not* on the evidence from the monuments. The succession and regnal years of its kings actually come from the extant fragments of the hellenistic Egyptian priest Manetho. He is now scrupulously shunned as a source by Egyptologists, except, it

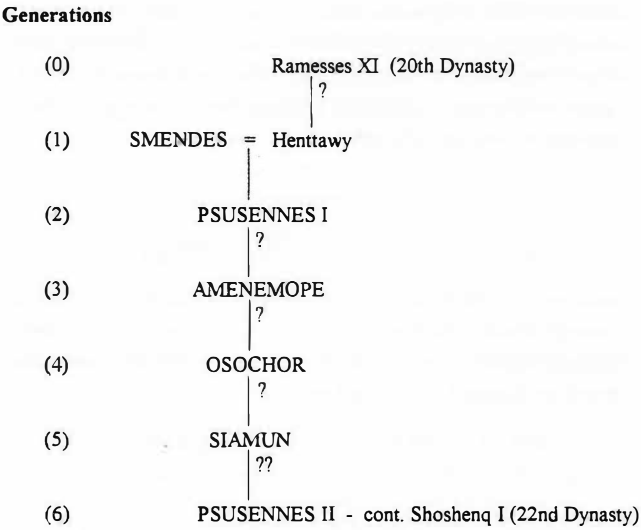


Figure 7.2. Royal 21st-dynasty genealogy from Kitchen 1986. The question marks are Kitchen's.

seems, when he helps to fill in grey areas. As for its absolute chronology, its beginning is set by the date, c. 1070 BC, for the end of the 20th Dynasty, which is calculated by dead reckoning forward from the 18th Dynasty. This, in turn, is placed c. 1525–1300 BC on the basis of a supposed fix provided by astronomical or Sothic dating. The theory of Sothic dating is rarely taught to Egyptologists, and hence rarely understood by them. Those that do understand it are generally more critical, even of the primary evidence on which it is based. For example Helck (1989: 40–41) has demonstrated that the 'Sothic date' which has been read into the Ebers Papyrus, supposedly fixing the beginning of the 18th Dynasty, is illusory. In the absence of the Ebers Papyrus reference, the chronology of New Kingdom Egypt is left dangling by a thread from the second major 'astronomical' point, ostensibly provided by the Illahun Papyrus of the 12th Dynasty (Middle Kingdom). Even if this document could provide a usable date,[6] it hardly provides a meaningful *terminus post quem* for the New Kingdom as the length of the 'Second Intermediate Period' (or 'Hyksos Period') is still unknown. In other words, there is no real astronomical dating for the 18th Dynasty and the New Kingdom as a whole.

Few Egyptologists today are prepared to defend the validity of Sothic chronology. In lieu of the supposed astronomical fixes, the claim is now being made that dead-reckoning *back* from the established dates of the Nubian (25th) and Saite (26th) dynasties of the early 7th century arrives at the same results (e.g. Kitchen 1991a; Kitchen 1991b: 236; cf. Bietak 1989: 91). This claim is a myth. All that has really been done is to fill a preconceived framework. The real linch-pin of TIP chronology is provided by the identification of Shoshenq I, founder of the 22nd Dynasty, with the 'King Shishak', who looted Solomon's

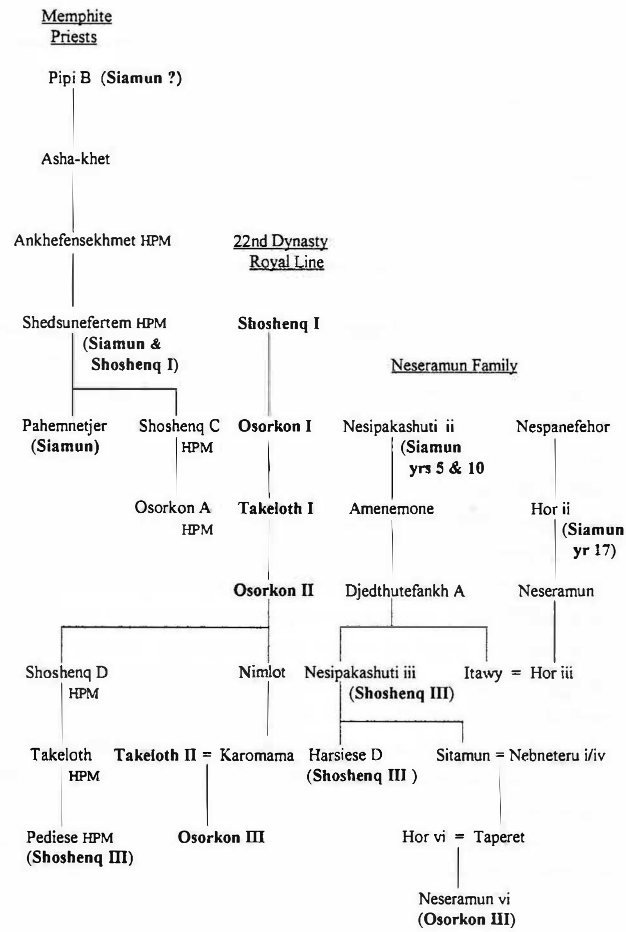


*Figure 7.3. TIP genealogies showing the placement of King Siamun of the 21st Dynasty. Royal names are shown in **bold** letters. Kings known to be contemporary with individuals in the genealogies follow their names in brackets. HPM=High Priest at Memphis.*

Temple, around 925 BC on Biblical chronology. As a text at Karnak dated to Shoshenq I's 21st year records a campaign in Palestine, the beginning of his reign (and consequently the beginning of the 22nd Dynasty and end of the 'preceding' 21st) is set at 945 BC. Hughes (1990: 192) clearly realized this: 'Egyptian chronologists, without always admitting it, have commonly based their chronology of this period on the *Biblical* synchronism for Shoshenq's invasion.' Yet there is no real resemblance between the geography of Shoshenq's Palestinian campaign (largely restricted to Israel) and the biblical account of Shishak's invasion of Judah. The biblical Shishak story also contains a conspicuously Ramesside element (the Tjukten/Sukkiim troops) which seems anomalous in a 22nd-dynasty context (James *et al.* 1992: 127). The other side of the coin is that acceptance of such an early date for Shoshenq I has forced the rejection of a perfectly good synchronism between Shipitbaal of the Byblite inscriptions (whose grandfather was a contemporary of Shoshenq I) with the Shipitbaal of

Byblos mentioned in Assyrian records, c. 740 BC (Mazar 1986: 231–247; James *et al.* 1991a: 250; 1992: 233). Criticism of our case against the identification of Shishak with Shoshenq I has, apart from sheer mis-statements of fact (James *et al.* 1992: 127; James & Morkot forthcoming), amounted to simple repetition of the commonplace that the name Shoshenq makes a good philological match with 'Shishak'.

It is a frightening prospect that Cypriot and Greek archaeologists are now being asked to raise their dates by reference to a Palestinian chronology which is based on the Egyptian. This Egyptian chronology is not based on astronomical evidence, not based on genealogical or contemporary inscriptional evidence, and not based on dead-reckoning back from the known 7th century, but on faith in a single identification with a biblical character – and with that, faith in the veracity of the biblical verse in question, with no respect to source criticism. The whole of TIP chronology – and, incidentally, the dates for early Phoenician palaeography – ultimately rests on the belief that the deuteronomist, who prepared his text no earlier than the 6th century BC – possessed the correct orthography of a Pharaoh's name from some four centuries earlier (James *et al.* 1992: 127).

Because of this act of faith nothing is allowed to shift. Yet it is already clear that various students of TIP chronology have admitted, albeit individually and in a piecemeal fashion, that it needs considerable revision. For example, we proposed (James *et al.* 1991a: 230, 235) that Kitchen's date of 716 BC (ultimately based on a mistranslation) for the accession of Shabago (25th Dynasty) can be lowered to as late as 708/707 BC. A study published by Depudyt (1993) now gives the same date (with no acknowledgment), with a consequent lowering of that for Piye's invasion of Egypt (Kitchen: 728 BC) by 19 years. (We would also argue a figure of this order.) As Piye's conquest of the Egyptian Delta is the benchmark from which all earlier reigns are calculated, lowering its date by 19 years would require an equivalent reduction in all TIP dates. Add to this Aston's demonstration (1989) that there was a 20-year overlap between Takeloth II and Shoshenq III (22nd Dynasty), and Dodson's (1987) that an independent reign of 14 years for Psusennes II (21st Dynasty) should be scrapped, and we already have more than fifty years which Egyptologists – by their own admission – are prepared to remove from TIP chronology. Indeed Dodson (1992) and Ray (1992) have allowed that the New Kingdom can be lowered by some fifty years. We should be grateful for small mercies. This reduction might be well received by archaeologists of the Eastern Mediterranean who have been struggling over the last century to fit their material into an Egyptian framework. Much more dead wood, however, should be cut. As we have seen, the real length of the 21st Dynasty as an independent chronological unit may be a full century shorter than is currently supposed. Further lowering of New Kingdom chronology can be effected up to an absolute maximum (and possible optimum) of 250 years – with a consequent lowering of Eastern Mediterranean Late Bronze Age chronology of the same scale. The problematic 'Dark Ages' of Greece, Anatolia, Nubia, the Levant and other regions would largely melt away.

Central Mediterranean Chronology

Such a revision would only have beneficial results for the Central Mediterranean. The accepted scenario was summarized frankly by Colin Renfrew, in the foreword he wrote to *Centuries of Darkness*:

> It is already widely known that the chronology for early Italy, during the Iron Age period, down to and including the foundation of Rome, is a complete shambles. Swedish scholars debate with Italian scholars over dates which may differ by as much as two centuries.

We have drawn attention to the fact that these contentious two centuries may simply not have existed – they can be seen as an artefact produced by reliance on Mycenaean chronology, which is in turn based on an incorrect Egyptian chronology.

For Sicily we argued that dependence on the 13th-century dates provided by Mycenaean links for the Thapsos culture has produced an attenuated and unnecessarily long sequence of phases for the Late Bronze to Iron Age (c. 1250–650 BC). In particular we suggested that the Pantalica III ('South') phase, which currently intervenes between Cassibile (Pantalica II) and the Early Colonial (Finocchito) phases, should be scrapped as an independent period. On a number of grounds it seemed logical to see Cassibile (currently dated c. 1000–850 BC) lowered to meet the time of the earliest Greek colonies and the start of the Finocchito, c. 735 BC. This step would, at a stroke, remove some 120 years from the 'Dark Age' of Sicily. We suggested further telescoping, by the overlapping and shortening of earlier periods in the 'Dark Age' sequence – for example there seems to have been a considerable overlap between the Thapsos culture and the Pantalica I ('North') phase.

We are pleased to see that Leighton (1993) has considered the broad consequences of our model for Sicily and noted that:

> The relative sequence and stratigraphy of settlement occupation would be unaltered, and it would become even easier to believe in uninterrupted Aegean and East Mediterranean contacts with Sicily (and the West Mediterranean) from Mycenaean times until the colonial period, by closing the gap which coincides with the Greek Dark Age.

While Leighton does not agree with the overall lowering we argued for Bronze Age chronology – preferring to wait and see what happens with the Mycenaean dates –

he accepted some of the overlaps we argued in an attempt to move away from the 'chest-of-drawers' model for the Sicilian 'Dark Age'. He agreed that it is 'increasingly likely' that the Pantalica I ('North') phase did overlap with the preceding one of Thapsos. He also accepted our argument that the Cassibile culture continued until the time of the Greek colonisation in the mid to late 8th century, and in so doing provided more arguments in support of our case for scrapping Pantalica III ('South') as an independent phase. Leighton suggested that much of the Pantalica South material could be contemporary with the Finocchito/Early Colonial period (c. 735–650 BC), as some of the pottery from the Pantalica South necropolis seems to betray knowledge of early Greek colonial wares.[7] He also considers reducing the date of the Ausonian abandonment of Lipari from 900/850 BC to the mid-8th century, which would be in step with our reduction of the related Cassibile period.

The problem with Leighton's otherwise welcome approach is that, by lowering the date of the end of Cassibile (and related Ausonian) at one end of the scale and by raising the starting point of Pantalica I at the other, he is merely lengthening the 'Dark Age'. The evidence from all other areas of the Mediterranean – and from Italy itself – strongly suggests that this period should actually be shortened, by effecting a considerable lowering of the traditional dates for the Bronze Age. It would be instructive to see what would result if archaeologists of the Central Mediterranean held the dates they receive from the Aegean and further east *sub judice* and developed independent chronologies, based on local stratigraphy and fixed by ^{14}C dating.

Unfortunately, the application of radiocarbon to the problems of Central Mediterranean archaeology has yet to begin in earnest. A trickle of dates continues to accumulate, but generally in an undirected and random fashion. The attitude taken by many archaeologists working in this area towards the use of radiocarbon dates also continues to be a cause of concern, particularly their lack of awareness of the crucial importance of good quality samples. A case in point is the chronological treatment of the evidence from the recent excavations at Contrada Scirinda in southwestern Sicily. A *single* ^{14}C determination of 460 ± 90 bc (A-5446), with a supposed calibrated result of 764–679 BC (at 1σ), is used by Castellana (1993: 49) to date Phase VI of the site to the 8th century BC. This is held to support his dating of the earlier phases, which is based on associations with eastern Sicily and the Lipari islands; for example, the preceding Phase V, linked to Ausonian II, is placed in the 9th century. The result is held to contradict the lower chronology we advocated (Maniscalco & McConnell 1993: 43). Unfortunately, Castellana's interpretation is both illogical and uncritical. First, sweeping conclusions must never be drawn from a single determination. Second, even if the ^{14}C date came from a well-contexted short-lived sample, the *terminus ante quem* for Phase V would of course be 680 BC, not 'the 8th century'. Third, if the date were to be calibrated in the now preferred way, at 2σ (using the Stuiver 1993 curve), it would fall in the range 800–200 BC, allowing for a much later dating for Phase VI.

As we noted (James *et al.* 1991a: 42–47), the dating of Sardinian archaeology for the same time range remains equally uncertain. The Phoenician connection with Sardinia, which acts to tie the Early Iron Age chronology of the island to the Eastern Mediterranean, has recently been revived by examination of the pottery from the tomb of Tekke in Crete (where the controversial bronze bowl with a Phoenician inscription was found). That there were two burials in the tomb is widely accepted, although their dating is less secure and ranges between 950 and 680 BC (Boardman 1967; Coldstream 1982; Vagnetti 1989). Among the pottery is a small jug (*askos*) decorated with concentric circles and horizontal lines. This has been identified by Vagnetti (1989) as a Late Nuragic vessel, a type with an apparently long time-span in Sardinia itself, running from the Final Bronze to Early Iron Age (in Tykot's chronology 12th-8th centuries BC). Outside Sardinia, these jugs are also found on Lipari in Ausonian II levels (conventionally 12th-9th centuries BC) and in Villanovan (9th-8th centuries) contexts in Italy; Køllund (this volume) has reported three askos fragments in 7th-century levels from Carthage, which she interprets as residual pieces. Of the known exported examples only that from Kommos (Watrous in this volume) can be used to support an early date within the time-range given in Sardinia itself.

Slight though this evidence is, it may act as a corrective to the recent trend in Sardinian studies to push back the dating of Nuragic civilization. In particular, a single calibrated date (2888–1520 BC) from Brunku Madugui was used to date the *floruit* of Nuragic developments (understood as Nuragic II/Sardinian Late Bronze Age) to the mid 2nd millennium BC (e.g. Lilliu 1988: 18). More cautious opinions are beginning to prevail as further dates with much smaller standard deviations have been produced, and current feeling is that *nuraghi* begin to be constructed around 1600 BC, reaching their height of development between 1300 and 1150 BC (Ugas 1992; Tykot 1994). This is far closer to the chronology we proposed (James *et al.* 1991a: 44), with the Sardinian Late Bronze Age ending c. 1000 BC – a date which ^{14}C results would accomodate quite happily. Although there are now many more radiocarbon dates available, there are few sites with good date sequences, and even fewer where they have been fully published with an assessment of their stratigraphic integrity. Hence the broad measure of uncertainty noted by Balmuth (1993: 39), who jokingly suggested that Nuragic dates change on an almost daily basis.

This still leaves the Early Iron Age (Nuragic IV) use of *nuraghi* curiously unclear. At Serucci three ^{14}C dates come from the final use of one of the 'Chambers' making up the village (Balmuth 1992: 679–680); all are on charcoal, and when calibrated at 2σ range from 1266 to 800 BC (Tykot 1994: 132). Judging by the brief account of the context from which the samples were taken, it seems likely that this broad date range does not truly reflect the chronological spread of activity, but rather results from the use of charcoal with its well established 'old wood' effect (see below).

Even with new findings, the early stages of the Phoenician phase of Sardinian history remain extremely difficult to detect. The claims for a Phoenician presence in Sardinia towards the beginning of the first millennium BC, based on one interpretation of the Nora inscriptions and the Tekke jug, still await confirmation by stratified archaeological evidence. The earlier dates ascribed to the Iron Age *nuraghi*, and thus the bronze figurines (of possible Phoenician inspiration) they contain have been cited by Balmuth (1992: 690–691; 1993) in support of the high date for a Phoenician presence, but the dating for this material is still far from secure, as we have seen. As recent reviews by Balmuth (1992) and Negbi (1992) make it clear, there is no new, direct, evidence to support the high dates proposed for the arrival of the Phoenicians in Sardinia, only the knock-on effect of raising the overall Nuragic dates, a trend which is now in reverse.

Radiocarbon Dating of the Aegean

In *Centuries of Darkness* we advanced the theory that the end of LHIIIC in the Aegean needs to be lowered from c. 1075 to c. 900 BC (James *et al.* 1991a: 111). In his otherwise favourable reviews, Snodgrass (1991a; 1991b) stated that the currently available radiocarbon dates from the Aegean create a problem – but without quantifying. This position was taken further by Manning and Weninger (1992: 637), who claimed that the ^{14}C record actually demonstrates that our proposal is 'impossible'. Their claim, while it has been cited approvingly (e.g. Dickinson 1994: 17), is fallacious, being based on a poor understanding of the archaeological significance of the radiocarbon results.

On a general level, Manning and Weninger's approach to logical debate is very loose. Something can be deemed 'impossible' only if there is contrary evidence of absolute certainty – not through arguments based on supposed probabilities or guesswork. In discussing ^{14}C dates, they often forget what *terminus post quem* really means, a particularly acute problem when dealing with long-lived samples, as well as underestimating other problems of interpretation. Despite acknowledging some well-known caveats, they utilize any available results (some going back to the 1950s!) with little regard for source criticism. Archaeology, however, is primarily about context and association – not inventive statistics. As Jope (1986: 1060) noted when discussing the 'stringent credentials' necessary for a sound ^{14}C determination:

> No amount of statistical manipulation will yield calendric dates of meaningful accuracy out of data from samples that do not meet these requirements.

Manning and Weninger collected 109 determinations, of extremely variable quality (and not always pertinent to the dating of the close of the Aegean LBA). Only ten came from short-lived material. Thus, from the outset one could argue that 90% of the figures embroiled in their presentation can be discarded. What, then, of the ten short-lived samples? Again, five (Bln-2658, P-2046, OxA-2096, 2097, 2098) are, strictly speaking, irrelevant as they belong to earlier periods. Of the remaining five (P-760, KI-1784, St-1267, St-1549, OxA-146) only two are results produced after 1980, one of which has a calibrated error of ± 169 years (OxA-146)! In a sense M & W's case rests on a single result, with a calibrated error of ± 102 years (KI-1784). The sample is a chestnut from Phase 10 of Kastanas, giving a date of 1134–930 BC as calibrated by Weninger at 1σ (and 1210–840 BC at 2σ, using the Stuiver 1993 curve). Phase 10 belongs to the beginning of the PG, conventionally dated to c. 1050 BC, on our chronology c. 900/875 BC. Evidently no verdict is possible.

It is not normal practice in the interpretation of ^{14}C dates to reach sweeping conclusions from such a poor set of data. Indeed, it is surprising that an antiquated approach of this kind could still be adopted in the 1990s. Numerous pleas for intelligent caution have been made; for example, Whittle (1990: 301), citing a paper co-written by one of the authors of *Centuries of Darkness*, predicts that 'the next radiocarbon revolution will be very closely to scrutinize the contexts, associations and compositions of samples (Kinnes & Thorpe 1986)...' Aside from the familiar problems at laboratory level, concerning individual samples (contamination; lack of pretreatment; undersize; ^{13}C normalization; environmental effects from volcanos, seas, lakes and rivers; etc), we must remember four other areas of uncertainty:

1. Interlaboratory differences, which have been been reported as being as great as '310 to 730 years' (Scott *et al.* 1990: 319);
2. The 'publishing filter' – as it might be described – is rarely discussed in print, but it is well known that a number of radiocarbon results not suiting preconceptions have never been published (e.g. Nelson *et al.* 1990: 201; Warren 1990; James *et al.* 1991a: 387, n. 137; cf. Iakovidis 1990);
3. Lack of a year-by-year calibration – the presently available curves lack the detail needed for short-lived samples and could mask significant differences in time (Mook *et al.* 1987: 147–148; Aitken 1988a: 21; Bruins & Mook 1989: 1026). Further, in calibrating radiocarbon dates, it is increasingly realized (e.g. Tykot 1994) that there is a need to use two standard deviations (95% certainty), rather than one (68%), especially with results obtained before high-precision counting was

introduced (Warren 1987: 209; cf. Aitken 1990: 107);

4. As the majority of Aegean results are from long-lived samples, the most crucial uncertainty – from the archaeological perspective – is the 'old-wood effect'. In the words of Aitken (1988b, 19):

> It is well known that long-lived samples carry a particular problem of interpretation, *viz.* what length of time elapsed between fixation of the carbon atoms in the cellulose of wood (for example) and the event being dated? For a large tree, this might be several centuries.

Several case studies involving sites with established historical dates illustrate the seriousness of the problem, for example: the medieval settlement of Starigard, N.W. Germany, where in many cases the calibrated results on wood and charcoal were 'several hundred years too old' (Willkomm 1983: 645); the Viking site of L'Anse aux Meadows, where the mean charcoal age is about 125 years greater than the short-lived and historical ages, pointing to a maximum age of 500 years for wood used at the site (Waterbolk 1971: 23); at Pompeii results from the wood samples gave calibrated age ranges between 60 and 205 years older than the burial of the site in AD 79 (Vogel *et al.* 1990: 536).

While paying lip service to the importance of the 'old wood effect', Manning and Weninger apply a correction *only* at Kastanas. But even here they gloss over the extent of the problem, by assuming 'that wood is normally about 50 years older than its context of cultural employment in the Mediterranean' – citing Vogel *et al.*, who actually used this figure as a hypothetical correction for undersized charcoal samples from North America. In fact, a correction figure deduced from Pompeii would be 132.5 ± 72.5. Cf. two charcoal results from Manning and Weninger's own listing: 1209–1043 BC (KI-1785) from the Kastanas level that produced the chestnut result of 1134–930 BC; and 862–580 BC (I-9054) from a 4th-century BC context at Nichoria.

Even allowing for the poor quality of most of the available dates, if one applies realistic 'old wood' corrections, with due consideration for archaeological context, a different picture emerges from that arrived at by Manning and Weninger and *even with* Weninger's 1σ calibrations.

No weight can be given to the evidence of sites with only one or two samples (Cape Gelidonya, Asine, Lefkandi, Midea), though different interpretations to those of Manning and Weninger could be offered. Likewise, little can be done with the 17 results from Nichoria (from Middle Helladic to Byzantine), admitted by Manning and Weninger to be 'worryingly inconsistent'. Yet they still conclude that a peak from the entire set falls in the early 1st millennium BC – 'as would be expected'. Expected by whom, from what? That trees were growing near Nichoria at this time has never been in doubt!

Turning to Mycenae, the one short-lived sample, of 'charred wheat' (OxA-146) is thought by Manning and Weninger to have limited value because of 'its solitary nature, and the large standard error of the measurement'. Three of the four LHIIIB long-lived samples were from the destruction level of the Citadel House, usually dated to c. 1200 BC: two of charcoal (P-1455; P-1456) and one from a burned beam (P-1457). They produced dates of 1298–1118 BC, 1399–1211 BC, 1256–1078 BC – combined range 1399–1078 BC. This merely gives a *terminus post quem*, from which one must subtract a figure that takes account of three time spans: (a) the further growth of the tree after that of the rings present in the samples; (b) the time that elapsed before its use (and possible later reuse) in the construction in question, and (c) the age of the building before its destruction. The Citadel House was built at the beginning of LHIIIB, conventionally c. 1335 BC. Using a notional 'old wood' correction of up-to-200 years derived from Pompeii, the destruction at Mycenae could have fallen as late as 1199–878 BC, in perfect agreement with our chronology, which ends LHIIIB c. 975 BC. This also agrees with both the fourth result (P-1454) from 'carbonized matter' (1149–961 BC) and the fifth (P-1459), described only as 'Mycenaean' but evidently from the same LHIIIB context (1278–1096 BC). For a visualisation of this archaeologically realistic approach, see Fig. 7.4.

From Pylos there are three 'Late LHIIIB' charcoal samples (P-332, 337, 341) which give a *terminus post quem* (combined range: 1498–1183 BC) slightly higher than Mycenae's. With an 'old wood' correction, the destruction could have occurred within the period 1498/1298 to 1183/983 BC – again, a lower trend than that of accepted chronology. The rest of the Pylos (mostly 'mid-LHIIIB') charcoal samples (P-326, 328, 329, 330, 340, Gro-998), have produced a massive scatter (combined range: 1868–1186 BC) not worth further consideration.

The Iron Age results from Assiros are consistent with our chronology if one allows for the 'old wood effect', while those from the Bronze Age (even lower than Mycenae's) clearly support us. Manning and Weninger (1992: 641) have considerable difficulty with the three results (BM-1431, 1432, 1433) from the earliest Bronze Age phases, and are forced to suggest that either they are wrong or the excavator's classification is too early. As for Kastanas, the 'wood correction' used by Manning and Weninger is, as mentioned above, far too small. Willkomm (1990: 177) has also made the important observation that the results from the LHIIIC to Geometric phases (conventionally c. 400 years) 'do not span more than 150 years'. An obvious explanation is that wood cut at an early period was extensively re-used. This by itself raises the question whether the kind of statistical massage ('archaeological wiggle matching') performed by Manning and Weninger to conclude that this site 'offers remarkably strong support for the conventional chronology', is really applicable to Kastanas – or indeed anywhere.

Despite the inadequacy of the available radiocarbon evidence from the Aegean in the LBA (James *et al.* 1991a:

xviii-xx, 321–325; 1991b: 231–233; 1992: 128–129), Manning and Weninger are prepared to use it in a 'probabilistic' (as they see it) statistical defence of the conventional chronology. But the fact is that ^{14}C cannot presently demonstrate the accuracy of the accepted dating framework. Setting the standard for future ^{14}C dating, we only need to quote Betancourt & Lawn (1984: 279): 'A more rigid use of evidence should insist upon at least 20–30 dates from each of the several levels at each site, all of good samples...' When something approaching even half this ideal becomes reality, then statistics can come into play, but Manning and Weninger will have to co-ordinate their interpretations since with respect to Thera the two authors (Manning 1990; Weninger 1990) have disagreed by as much as 150 years! It seems absurd to assume that greater precision can be obtained from sites where only a fraction of the number of radiocarbon tests have been performed. It is also incredible that Manning (1990: 37) himself could only shortly earlier have made the following statement:

> ... new series of high quality dates from sealed stratigraphic contexts from all the Aegean periods are required. The current corpus consists of dates from very different technical processes, and dates usually lacking carbon-13 normalization, or alkali pre-treatment! This is unacceptable .... The pressing need is therefore for Aegean radiocarbon dates with the contextual and measurement quality to match the precision of the current radiocarbon calibration curves.

At the end of their presentation, Manning and Weninger appeal to the radiocarbon record from Egypt, as presented by Weninger (1990), and to the developing dendrochronological sequence from Anatolia (see below) as evidence that all is well with Late Bronze Age chronology. The real situation with the ^{14}C dates from Egypt is that they are equivocal and almost as many dates could be cited in favour of lowering chronology as maintaining the *status quo.* Almost all of them have been performed on unsuitable material (wood, charcoal, reeds), and there are vast interlaboratory differences (particularly between the Universities of Uppsala and Pennsylvania). Weninger's study itself should be treated with the utmost caution, containing as it does numerous factual errors. (For more reliable studies, see Shaw 1985; Hassan & Robinson 1987.)

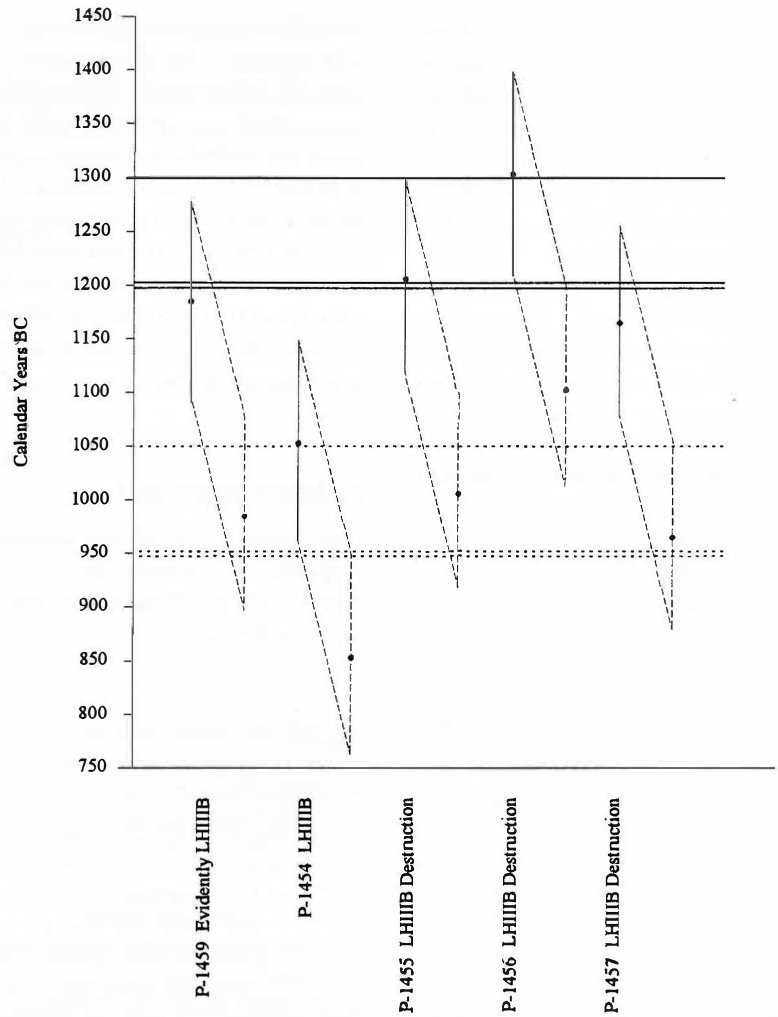
Dendrochronological Prospects

For Snodgrass (1991a; 1991b), strong evidence against our low chronology comes from the emerging dendrochronological record. Somewhat optimistically, he predicted that this would in the near future, 'perhaps within less than five years', have produced a volume of evidence sufficient to build up a new scientifically based chronology which would show that the conventional dates are sound. We are still waiting.

Snodgrass made his prediction on the basis of a preliminary report for Bronze Age Anatolia published by Kuniholm (1988: 8). This stated that the last preserved ring of charcoal from a 'Hittite palace' of Suppiluliuma I at Maşat lay 654 years before the end of the master dendrochronological sequence from Gordion, producing a date of c. 1379 BC following a notional end of c. 725 BC for the sequence. As Suppiluliuma is generally thought to have died c. 1320 BC, the result represented, in Snodgrass' view, 'a shot in the arm' for the conventional chronology. A subsequent report (Kuniholm 1990: 4) adjusted this figure to 635 years, and as new radiocarbon dates were available for the master sequence, gave a date of 1392 ± 37 for Maşat. The context was also said to contain Mycenaean LHIIIB pottery.

As we stressed in reply to Snodgrass (James *et al.* 1992: 128), such a result, far from giving a date for the building's construction, merely gives a date for the death of the last tree-ring preserved, not even the felling of the trees involved. (Note that there was no bark present on the samples.) Further, the information about the timber's context makes no sense in terms of the site as it is published (Özgüç 1978: 52–67; 1982: 76–78). There are three Hittite levels at Maşat: III, which contained a palace (time of Tudhaliya, father of Suppiluliuma); II, with new buildings (time of Suppiluliuma); and I, the final level of Hittite times (13th century BC) which contained a stirrup jar and fragments of four other Mycenaean LHIIIB vessels. Kuniholm's presentation of the timber's context as a 'palace' of 'the time of Suppiluliuma' with associated LHIIIB pottery thus seems to be a pastiche of elements from Levels I, II and III.[8] The stress laid on the Mycenaean pottery found in the context leads one to conclude that the samples came from Level I. Had they been found at a one-level site the result may have seemed significant, highlighting the serious danger inherent in uncritical use of a single dendrochronological date. Yet as the result actually predates the reign of Suppiluliuma, who built the *preceding* level (II), then it has to be accepted either that the tree was much older than the context it was employed in (there was no bark present on the sample), or that it was reused from a much earlier level.[9] In either case it cannot possibly be used to demonstrate that the conventional date for Suppiluliuma I is correct.

The misleading case of Maşat highlights three problem areas which face the application of the developing dendrochronological sequence from Anatolia. First, there is the danger of drawing premature conclusions. Second, unless full and precise details for each sample are published by the excavators, dendrochronological results are not susceptible to archaeological interpretation and are simply wasted effort. Third, one must be mindful of the extensive reuse of the precious resource of timber in the ancient world, as illustrated by two other sites. The royal tombs at Gordion (Kuniholm 1988: 8; cf. James et al. 1992: 128)



Calibrated date range
Possible date range for samples allowing for 'old wood' problem
P-1459 Charcoal (1187±91 BC)
P-1454 Carbonised matter (1055±94 BC)
P-1455 Charcoal (1208±90 BC)
P-1456 Charcoal (1305±94 BC)
P-1457 Charcoal (1167±89 BC)
NB: The latest Oxford calibration programme (OxCal 2.18) would produce date ranges lower by some thirty years than those given by Weninger.

Figure 7.4. The radiocarbon dates from the Citadel House, Mycenae, plotted at one standard deviation, following Weninger's calibration. Conventional dates for the construction (1300 BC) and destruction (1200 BC) of Citadel House, together with revised dates for the construction (1050 BC) and destruction (950 BC) events are indicated.

and the Middle Bronze Age building complex at Aksaray, Açemhöyük, Northwest Trench (Kuniholm 1994: 7) have demonstrated that timber was reused on a large scale, sometimes centuries later.

From another LBA site, Tille Höyük on the Euphrates, enough detail on the context of the dendrochronological samples has been published to make discussion more worthwhile (Summers 1993). The excavation focussed on a massive Gateway, which was burned down along with the rest of the site near the end of the LBA. Charcoal samples were taken from the wooden roof which had collapsed into the passageway of the Gate. These fell into two groups, which had outer rings of 1210 ± 37 and 1140 ± 37 years respectively,[10] with the older timbers assumed to represent an earlier phase of construction in the Gateway. Kuniholm *et al.* (1993: 188) believe 'for subjective reasons' that there are no more than six rings missing from the latest timbers, so that their felling date would fall near 1134 ± 37 BC by cross-matching with his master sequence from Gordion. Allowing for an unknown number of years for the use of the Gateway before it was burnt, this date is uncomfortably tight for the conventional chronology of the LBA, which, in this region ends c. 1175 BC.

Understandably, Summers (1993: 38) attempted to circumvent the effects of the dendrochronological results by minimising the period of the Gateway's use. From the lack of material accumulated in the Gate passage, the degree of wear on the lime plaster of the passage floor, the fact that the walls were not re-plastered and the slight traces of occupation in the rooms attached to the Gateway, he argued that this period was fairly brief, 'perhaps no more than a few years' (Summers 1993: 14). While these factors may give the impression of a short period, to ascribe such a tight limit appears unrealistic, and depends on a series of assumptions concerning the expected level of activity which may well not be correct. The pottery associated with the destruction level was Drab Ware (Summers 1993: 47) known from other sites to belong to the LBA. Although the excavators (Blaylock 1991: 5) at one point considered reallocating the Tille Höyük Drab Ware to the early Iron Age ('perhaps 12th or 11th century'), they have retracted this suggestion and now envisage the site as a Hittite fortified centre which outlasted the fall of the Empire by anything up to one hundred years, with the Drab Ware demonstrating continuity of pottery production into the early Iron Age (Summers 1993: 47).

Finally comes the significant question of the overall calibration of Anatolian dendrochronology. Unfortunately, a *continuous* tree-ring series running back to the Bronze Age has yet to be developed, and the Gordion 'floating' sequence presently relies on ^{14}C dating to be fixed in time. The first radiocarbon tests performed gave results up to two centuries lower than those expected. Kuniholm (1977: 45-49) then considered lowering the end of the Gordion master sequence from its assumed archaeological date of c. 725 BC to c. 547/6 BC. However, such a radical revision seemed unwarranted, and a larger set of radiocarbon tests (performed at Heidelberg and still unpublished) were used to peg the end of the sequence to 757 ± 37 BC (Kuniholm 1990: 3, 6). Calibrating these results by the 1993 Stuiver curve, Manning suggested (at this conference) that the Gordion master chronology should be lowered again by 39 years, a conclusion approved by Kuniholm. This now means that the *construction* of the last phase of the Tille Höyük Gateway must be reckoned at 1101 ± 1 BC, with its use lying in the 11th century BC, surely an impossible result for the conventional chronology.

Conclusion

In conclusion, we might echo the words of our predecessor Cecil Torr (1896: 1) who exactly a hundred years ago resisted the attempts of Egyptologists to raise the dates of Mycenae and interpose a lengthy 'dark age' before the floruit of Archaic Greek civilization: 'A statement is current that the Mycenaean age in Greece can definitely be fixed at 1500 BC, or thereabouts, on the strength of evidence from Egyptian sources.' A 'statement' now seems to be current that radiocarbon dating and dendrochronology demonstrate that the conventional chronology for the Mycenaean and Late Bronze Age world is correct. Far from it, we submit that a significant lowering of LBA chronology is not only compatible with the available ^{14}C evidence – when assessed critically – but is also beginning to receive support from dendrochronology. After a century of further excavation and study, the weakness of the traditional Egyptian chronology highlighted by Torr is still apparent. We believe that we have shown that a radically lower model (again with the proviso of a maximum of 250 years) for Egyptian chronology is desirable, given the multiple tensions and conflicts that exist in Iron Age Mediterranean archaeology. In particular the current impasse between Cypriot/Greek and Israeli dating systems *must* be resolved, and the only logical solution seems to be a general compression of the Early Iron Age. We can only appeal to specialists in Sardinian archaeology to take the dates they have received in the past from the Aegean and Eastern Mediterranean worlds *cum grano salis*.

Acknowledgments

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Notes

1. Cross (1993: 541, n. 23) states. that 'The Tell Fekheriyeh inscription either dates to the 11th century (and the names of the dynasts repeat in the 9th century), or is from the second half of the 9th century and is a stunning piece of archaizing.' Yet one must ask why archaic forms were used and what they were copied from. Can any parallels for such archaising be made for comparable political inscriptions from this period/ region?
2. The initial Egyptological diagnosis of this urn by de Meulenaere had actually pointed to a date *not earlier* than 700 BC (in Bikai 1978a: 84; cf. James *et al.* 1991a: 361, n. 41), and Bikai (1987: 69) later considered the suggestion by Anderson that some elements of Tyre III–II seem to recall the 7th-century. Interestingly, Gal (1992b: 73–74) argues a post-700 date for all strata from IV to I at Tyre.
3. Nothing, it should be noted, has been found to contradict the low chronology for Carthage which we advocated (James *et al.* 1991a: 53–54).
4. There are many problems with this statement. First, what does a 'different phenomenon' mean other than a serious archaeological inconsistency? Second, although the application of Neutron Activation Analysis has produced varying results, which tend to suggest that perhaps some of the Black-on-Red Ware may have been copied on Palestinian soil (James *et al.* 1991a: 155–156), there is no evidence over a number of sites for assigning any specific type of presumed *non*-Cypriot Black-on-Red Ware to any specific chronological horizon. Therefore, it is a mystery why Black-on-Red vessels do not concern Gilboa in her discussion of Phase 9 at Dor, dated between 1050 and c. 980 BC (Gilboa 1989: 205), when Black-on-Red vessels are commonly included in the publications of '11th-10th century' sites in Israel (e.g. see James *et al.* 1991a: 67, n. 45).
5. When attacked by Kitchen (1991a) on this point we challenged him to prove that the 21st and 22nd Dynasties did not overlap by reference to primary evidence alone (James & Morkot 1991), as Kitchen had referred to it as the 'single point which must suffice' to destroy our model. As we subsequently noted (James 1991) Kitchen's reply turned to different matters entirely.
6. For a damning critique, see Rose 1994, which shows that the

conventional placements of the document (19th and early 18th centuries BC) provide a very poor match with the lunar data it also contains. See also Read (1970), curiously overlooked by Rose, who showed that the Illahun lunar data finds a perfect match in the year 1549 BC. Taken literally, this would necessitate a reduction of Middle Kingdom Egyptian chronology by some 250 years! (James *et al.* 1991a: 228–229.)

7. Maniscalco & McConnell (1993: 45) admit that 'the Cassibile and Pantalica South material cultures have yet to be distinguished stratigraphically', yet objected to our suggested change in Pantalica South's chronological status: 'If Pantalica South were eliminated as a chronologically distinct phase, the Pantalica South material culture would have to be mixed with some other group – where does one put it'? Leighton (1993) seems to have answered their question.
8. Elsewhere the same dendrochronological result was vaguely reported as coming from 'Levels I & II' (Kuniholm 1989: 96). Matters are further confused by the statement that the timber came from a 'room...from the time of Suppiluliumas I with imported Late Helladic IIIA pottery' (Kuniholm 1992: 383). Only LHIIIB pottery was reported in the original excavations (Özgüç 1978: 66; 1982: 102–103), though an LHIIIA2 stirrup jar was subsequently excavated from Level II in the lower city (see conveniently Bloedlow 1988: 41, n. 150), which would be more consistent with the date for Suppiluliuma, but can have no connection with the wood samples taken from the 'palace' on the acropolis. In conversation (at this conference) Kuniholm admits that the original data for the context of the dendrochronology samples from Maşat is probably irretrievable and that the date should be discounted as historically meaningful.
9. Indeed, Kuniholm 'seems to remember that wood allegedly from Level II postdates wood allegedly from Level I' – pers. comm. 12/12/91.
10. Further, it should be remembered that the Tille results are matched to the sequence by statistical analysis. Kuniholm's data show that an alternative fit for the later group falling at 981 ± 37 BC (in Manning's revision 942 ± 1 BC) is actually the *best* in terms of the T-score normal for dendrochronological correlation. Our thanks to Bob Porter for drawing our attention to this important point.

Postscript

Two developments since this paper was written need to be noted. First, the 39-year reduction of the Anatolian Bronze-Iron Age dendrochonological sequence, discussed by Manning and Kuniholm at this conference, has now been published – see P.I. Kuniholm, B. Kromer, S.W. Manning, M. Newton, C.E. Latini & M.J. Bruce: 'Anatolian tree rings and the absolute chronology of the eastern Mediterranean, 2220–718 BC', *Nature* 381 (1996): 7807–83. Second, in 'The archaeology of the United Monarchy: an alternative view' (*Levant* 28: 1996: 177–187), Israel Finkelstein has outlined a radical revision of the Iron Age in Palestine which argues a lowering of Iron II ('10th-century') levels to the 9th-century, similar to that proposed by the authors although attempting to work within the conventional chronology.

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