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REVIEW ARTICLE: *TREE-RINGS, KINGS AND OLD WORLD ARCHAEOLOGY AND ENVIRONMENT*

PETER JAMES

This is a festschrift for Peter Kuniholm, based on the proceedings of a conference held in his honour on 3-6 November 2006, on his retirement as Director of the Aegean Dendrochronology Project (ADP) at Cornell University. The volume is handsomely produced, the paper of high quality (enabling both black and white and colour illustrations), and the proofreading excellent. In terms of editing, the only complaint is the lack of an index. The publication of so many insightful papers is more than welcome. Not all concern dendrochronology or dendroclimatology *per se*, such as the valuable summary by Luke and Roosevelt of the 'Central Lydia Archaeological Survey: Documenting the Prehistoric through Iron Age Periods'. The present review will concentrate mainly on those contributions which touch on chronology – as it is for his work in this area that Kuniholm's research is best known to archaeologists and historians of the Eastern Mediterranean and Near East.

As necessary background, the main yardstick of the ADP's work is formed by the timbers from the 'tomb of King Midas' at Gordion. Bryant Bannister from the Arizona tree-ring laboratory was the first to examine the timbers from the tomb; some timbers were especially long-lived, allowing Bannister to build a sequence of 806 rings. (For a concise history of the subject see the contribution by Touchan and Hughes: 'Dendroclimatology in the Near East and Eastern Mediterranean Region'.) In his Ph.D thesis (1977) Kuniholm confirmed Bannister's measurements; and the 806 rings, together with other samples from Gordion, were used to form what is known as the Gordion Master Sequence (GMS). It was subsequently augmented and extended by the addition of data from many other Anatolian sites. If correlations could be made successfully with other Anatolian and Aegean sites, the GMS offered infinite promise as a dating tool for the entire East Mediterranean (including the Levant), but there have always been problems. Correlation is done by measuring the thickness of the rings in the the tree-samples – which varies depending on how well the trees were doing in terms of rainfall and other local conditions. The patterns can then be matched statistically with those from other sites, but difficulties obviously arise when dealing with trees that grew in different environments.

A further problem has always been that the GMS is 'floating', in that it cannot be connected with a tree-ring sequence worked backwards from the present day (as was done with the bristlecone pine of California). As noted by Colin Renfrew in his Foreword (p. vii):

> The long term-prize, of course, is to find timbers constituting the 'missing links' in the continuous tree-ring chronology that one would like to see for the Aegean, stretching back without a break from the present to the early neolithic and perhaps beyond.

Such were the challenges faced by Kuniholm as Director of the ADP. How far he managed to resolve them is a matter of debate. Kuniholm went on to form a massive collection of samples from the Aegean, Anatolia and Italy, resulting in sequences covering up to 7,500 years, based on over ten million tree-ring measurements (see Muhly's contribution, p. 6). However, in many cases his comparisons concerned not only trees from different climatic regimes, but also different species, including oak and cedar (as opposed to the juniper and pine of Gordion). As to pegging the floating sequence of the GMS, Kuniholm's date for the last ring of the 'Midas Mound' has changed many times. Originally he followed the excavators' estimate of c. 725 BC. But as early radiocarbon dates on the timbers seemed to conflict, Kuniholm (1977, 48-50) considered lowering the construction date to c. 547 BC. In 1990 further ^{14}C tests raised this date massively to 757± 37 BC, only to be lowered again to 718 BC in 1996, then raised again to 740+4/-7 in 2001 (see James 2002). Despite all the juggling, a late 8th-century date seems assured by comparison of the grave-goods with Greek and Assyrian material, and the tomb may well be that of a predecessor of the historical king Midas, who we know from Assyrian records flourished c. 700 BC (Muscarella 2008, 179-180).

As for the results made by comparing the GMS to samples from other sites, some key cases are reviewed in the first main paper in the volume, by James Muhly 'Perspective: Archaeology, History, [p. 145] and Chronology from Penn to the Present and Beyond': the Middle Bronze Age sites of Acemhöyük ('Sarıkaya palace') and Kültepe, Early Iron Assiros in Macedonia and Gordion itself. As Muhly notes, Kuniholm's results have given rise to considerable controversies which will doubtless continue to run. Muhly's balanced paper contrasts with an extraordinary statement made by Geoffrey Summers in his contribution, 'The End of Chronology: New Directions in the Archaeology of the Central Anatolia Iron Age' (p. 239):

> It has been Peter Kuniholm's unique achievement that dendrochronology has brought about an 'End to Chronology' for the highlands of Central Anatolia, an end which will very soon be extended to the entire Aegean and Ancient Near East... It has been a magnificent achievement! Archaeologists and their students can focus their attention on the real business of archaeology... without getting stuck in the quagmire of chronology.

Yet in this marvellously optimistic assessment, Summers does not mention *any* dendro-dates, including those from his own site of Tille Höyük. (Here Kuniholm's results have subsequently proven to be completely wrong – see below). Indeed, Muhly's paper aside, references to the actual *dates* offered by Kuniholm for archaeological sites are hard to find in the book.

Claims to have made major advances in chronology were indeed made by the ADP under Kuniholm's directorship. The most famous (or notorious) concerns the dating of the explosion of Thera early during the Late Bronze Age (a major theme of this volume). The date of the eruption – through crosslinks with the Minoan civilization – is of great importance throughout the ancient Eastern Mediterranean, and has been the subject of controversy since the late 19th century. In a paper in *Nature* (1996), Kuniholm and his team claimed to have pinpointed the destruction of Thera to 1628 BC (as opposed to the once traditional archaeological dating around 1500 BC), as tree-rings from Porsuk in central Anatolia showed a growth abnormality for that year. This date was held to match similar tree-ring evidence from North America and Ireland, and a peak of sulphuric acid in one of the Greenland ice-cores, independently dated to c. 1625 BC. The same paper gave a preliminary result from the Late Bronze Age shipwreck of Uluburun, of 1316 BC for the last preserved ring of

some wooden cargo. As the shipwreck contained Late Helladic IIIB pottery and a gold scarab of the 18th-dynasty queen Nefertiti, this date was held to 'confirm conventional 14th-12th century BC chronology against recent radical critiques' (with reference to James *et al.* 1991). The two dates were widely, and often uncritically, accepted as confirmation of the high date for Thera and the conventional chronology of Egypt.

For Uluburun see further below, but the 'fun' for the high Thera date began when ice-core specialists recovered volcanic glass samples from the same level as the sulphuric acid 'spike' and showed that their chemical composition did not match that from Thera. (Some of the debate over dating Thera by ice-cores and tree-rings is summarised in Wiener's main contribution, pp. 279-280). Squabbles with the ice-core experts followed; but the matter was side-stepped when the ADP recalibrated the GMS, backdating the Porsuk tree-ring 'event' close to 1645 BC (Manning *et al*. 2001) – conveniently matching the date for a peak in sulphuric acid from another ice-core which Hammer *et al.* (1987) had long argued was linked to Thera. There were obvious problems with this: for example a 1645 BC explosion, in which sulphuric acid from the Aegean allegedly reached Greenland, appeared to have had no effect on European trees (or American bristlecone pines). The problem was exacerbated when Hammer *et al*. (2003) claimed to have found actual tephra from Thera in their Greenland 1645 BC level. It did not take long for other scientists, using statistical analysis of its constituents, to show that the 'tephra is not from the Minoan eruption of Thera' (Keenan 2003, 7), but is in fact identical in composition to material from the volcano of Aniakchak in Alaska (Pearce *et al.* 2007).

The whole tale actually amounts to a debacle for Kuniholm and the high Thera school, and much of the volume under review is devoted to dealing with the fallout. With respect to the use of tree-rings, three papers in the volume explore the relatively new science of dendrochemistry (C. L. Pearson and S. W. Manning: 'Could Absolutely-Dated Tree-Ring Chemistry Provide a Means for Dating the Major Volcanic Eruptions of the Holocene?'; D. K. Hauck and K. Ünlü: 'Dendrochemistry of Pinus Sylvestris Trees from a Turkish Forest'; K. Ünlü, P. I. Kuniholm, D. K. Hauck, N. Ö. Cetiner and J. J. Chiment: 'Neutron Activation of Dendrochronologically Dated Trees'.) While not its sole concern, one hope for dendrochemistry is that it may be able to identify the chemical signatures of volcanic eruptions, but it has not yet produced any conclusive results relevant to the Thera case.

[p. 146] Otherwise a special section (57 pages) at the end of the book debates the current issues concerning the dating of the eruption. In 2006 Walter Friedrich and colleagues published ^{14}C results from an olive branch buried in tephra, while an accompanying article by Manning *et al*. (2006) published a series of determinations including short-lived samples from the destruction layer. Together these results are held to date the eruption to between 1627 and 1600 BC, and they now form the flagship for the high Thera school (rather than ice-core and tree-ring dates). Yet they have done nothing to silence the forceful resistance of more traditional daters such as Aegean archaeologist Peter Warren, Egyptologist Manfred Bietak and independent scholar Malcolm Wiener. Wiener's main paper, 'Cold Fusion: The Uneasy Alliance of History and Science' elegantly summarises the archaeological objections to the high dating (in terms of ceramic synchronisms with Cyprus and Egypt) and makes short shrift of alleged support from the 'proxy dating' of ice cores and tree-ring: 'There is in fact no such evidence.' (p. 279).

In the main, Wiener concentrates on a penetrating critique of the available ^{14}C evidence from Thera itself and the calibration problems involved. He highlights one obvious and extremely important issue – the effect that the upwelling of gases depleted of ^{14}C from the volcano could have had on radiocarbon tests. As Wiener notes, tests done by Bruns *et al*. on living plants from Thera in 1980 gave pseudo-ages of 1390 years at 5m from a volcanic vent and 1030 years at 10m. The possibility that similar outgassing of 'old carbon' may have skewed the results on samples from the Minoan-period settlement is disputed in the two following papers, 'Santorini Eruption Radiocarbon Dated to 1627-1600 BC: Further Discussion' by Walter Friedrich and five other authors and 'Dating the Santorini/Thera Eruption by Radiocarbon: Further Discussion (AD 2006-2007), by Manning and seven others. The cut and thrust on this point and others (enjoyable reading) continues through a further four exchanges between these authors and Wiener.

Manning *et al*. insist that Bruns' work showed the aging effect only in plants very close to vents, and that there would have had to be steady outgassing for decades to explain 'the remarkably consistent set of data from the final destruction level at Santorini' (pp. 301, 328). On the alleged consistency, Wiener (p. 329) points to the results from two pea samples from the same room where the central values were no less than 215 ^{14}C years apart: 'The claim that this evidence 'is very self consistent'... will puzzle prehistorians unfamiliar with the special statistical vocabulary here employed.' As for the claim that the aging effect only happens near vents, Wiener cites a mass of evidence from other sites to the contrary. And, a point which Manning *et al*. avoid, is that conditions would have been very different in the years (possibly decades) preceding the eruption than they are in the volcano's relatively inactive state. Here Wiener cites expert opinion: 'Floyd McCoy, the volcanologist engaged in a long-term study of the Theran eruption, notes that ^{14}C-deficient CO_2 gas in the soil commonly leaks upward from a magma chamber prior to an eruption, to the point that such leakage is one of the major signals of an impending eruption used today.' Unless Manning, Friedrich *et al*. can prove the contrary, this means that the short-lived samples (at least) from Bronze Age Santorini are effectively useless for dating purposes.

Wiener's contributions to the volume are vital in restoring some sense of balance. He has been the main financial sponsor of the Cornell tree-ring laboratory, aptly known as the 'The Malcolm and Carolyn Wiener Laboratory for Aegean and Near Eastern Dendrochronology'. To Wiener's credit, though more than a friend to the ADP, he has never uncritically swallowed its results. In the exchanges over the ^{14}C results from Thera he was allowed the last word. In other circumstances, for reasons which should be clear, one fears that a 'watchdog' like Wiener, despite his wide-ranging and meticulous scholarship, may not have been given a voice at all.

As for Kuniholm's 1316 BC date for the Uluburun shipwreck, this was shown by the present writer (James 1999; 2006; cf. Keenan 2006, 4-6) to not be a dendro-date at all, in that not enough rings were measurable for normal statistical analysis. The samples were on cedarwood, presumably from the Levant, and it is unlikely they could be cross-dated with a juniper-pine sequence from Anatolia; especially when it transpired that the matching – using two extremely gnarled specimens (not shipframe but dunnage) – was done simply by eye (Wiener 2003, 244)! After this and other criticism (e.g. Bietak 2004, 221-222) the ADP expressed increasingly cold feet over the result.

The end of the road for this non-dendro-date comes in a paper by Manning and others: 'Absolute Age of the Uluburun Shipwreck: A Key Late Bronze Age Time Capsule for the East Mediterranean' (pp. 163-187). On p. 164 it is stated flatly that Kuniholm's earlier results 'have proved... without good dendrochronological support: these are hereby withdrawn'. The paper replaces the [p.147] alleged dendro-date with some radiocarbon results. As with much of Manning's writing there is flagrant use of a radiometric buzzword generator, the paper being littered with repetitions of 'high precision', 'high resolution',[1] 'robust' and 'integrated', as if to reassure the reader (and the author?) that something scientific is being done. Here Manning's style of lexical chloroform is used to convince us of two arguments. The first is that the dendrochronological work on the shipwreck can somehow be 'integrated' with 'high resolution' ^{14}C dates to give a meaningful result. This is hard to understand given Manning's own remarks on the samples sent for radiocarbon testing (p. 168):

> The cross-matches are not decisive or strong in either statistical or visual terms between any of these timbers (and their often erratic growth), nor between any of these timbers and other conifer chronologies in the region. It should be noted that the samples are far from perfect for dendrochronology given extensive damage by shipworm (*Teredo navalis*), which makes reading the tree-ring record challenging (fig. 7). Hence, to be conservative, we have chosen to treat each timber as independent in this study.

Hence, quite plainly, there is *no* dendrochronology to speak of from the extremely poor samples available. How then, can there be any 'integration' with results from radiocarbon? Yet phrases such as 'the high-resolution integrated dendro-radiocarbon methods' continue to litter almost every following page in the article.

Second, with regard to the ^{14}C results themselves, Manning's stated aim in the abstract (p. 163) is to show that '*Our precise absolute dating provides an important chronological marker for the Amarna period in Egypt and across the Near East, resolving a number of areas of debate or contention in the scholarly literature.*' (With reference to the Nefertiti scarab in the cargo.) Yet from their analysis of the ^{14}C results, Manning *et al*. allow that the *terminus post quem* for the ship's last voyage may have been as late as 1274 BC (p. 181), while a note added in proof (p. 187), because of another revision in the calibration curve, allows a drop in the dates by c. 20 years, bringing us to 1254 BC for the *terminus post quem*. Assuming the ^{14}C dates (or rather the calibration curve) are valid, how can this be used to argue that the conventional date for Nefertiti, a century earlier, has somehow been confirmed?

We hardly see here the 'end of chronology' dreamt of by Summers. The ADP's success record for Anatolia itself is patchy, to say the least. Rather than having resolved the problems of Anatolian chronology the work of Kuniholm has merely thrown it into a quandary. Field archaeologists must have been utterly confused by the constant changes of mind regarding the dating of the GMS and the numerous preliminary pronouncements, most of which have never been backed up, to the point where many must have wondered whether to trust this kind of 'scientific' work at all (e.g. Bietak 2004, 222).

The results from Middle Bronze Acemhöyük and Kültepe (as discussed by Muhly, pp. 7-9) have never been fully published. Nor have those from Maşat Höyük, an important site to the northeast of Boghazköy where an archive of Hittite texts has been found. In preliminary reports Kuniholm offered a date of 1392 ± 37 for the last preserved ring on charcoal from a 'Hittite palace' of Suppiluliuma I, in a context said to contain Mycenaean LHIIIB pottery. His descriptions of the context muddled information from three different levels (Str. III-I) at the site (see James *et al*. 1998, 38,

41, nn. 8, 9). The present reviewer was told (pers. comm.Tufts University conference 1995) that the original records for the context of the samples from Maşat are probably irretrievable. Nevertheless, the 'date' was repeated in a later study (Kuniholm *et al.* 2005, 46).

The only results from a 'Hittite' site to reach final publication are those from Tille Höyük in southern Anatolia. Here the construction of the LBA gateway was given a *terminus post quem* of 1101 ± 1 BC by the ADP (Kuniholm, Tarter & Griggs, 1993), and Kuniholm claimed that the match of the timbers (although oak) with those of the Gordion Master sequence was 'excellent' (Kuniholm 1991, 1). Yet as Robert Porter (pers. comm.) and Keenan (2002, 232-233) pointed out, a better statistical match was actually found at 942 ± 1 BC! The site is one of the first case studies to be re-examined in detail by the ADP since Kuniholm's retirement as Director. Using numerous radiocarbon determinations the new study was able to place in relative order two groups of timber from the gateway (Griggs and Manning 2009). It transpired that Kuniholm's matching had placed the two groups in reverse order! In short, his results from Tille Höyük – outside of Gordion itself the most detailed in terms of publication for any Anatolian site – have been completely overturned. Incidentally, the ^{14}C results suggest that the '*terminus post quem* felling dates' for the gateway are from the mid-to late 11th century BC. In other words, the gateway, hitherto considered to be an LBA construction, [p. 148] could have been built close to 1000 BC, rather than 1200 BC. The researchers propose extending the use of the associated pottery by some 125 years, locally, but that is another story.

Maşat Höyük and Tille Höyük, like many other embarrassments, are politely overlooked in the volume under review. A problem site that is discussed, and in detail, is Gordion. Mary Voigt's paper ('The Chronology of Phrygian Gordion') provides a good history of the debate. Until fairly recently the date of the first excavator, Rodney Young, for the destruction of the Early Phrygian city to c. 700 BC was accepted. Kuniholm's dating of the 'Midas Mound' to the late 8th century supported the date for the destruction level, as it was clear that the material culture was the same in both. Then in 2003 the Gordion team (De Vries, Kuniholm, Sams and Voigt) announced a quite shocking conclusion. Using ^{14}C and dendro-dates, they claimed to have backdated the destruction by at least a century, to c. 825-800 BC. This would make the Midas Mound considerably younger than the city with which it was once associated. Oscar Muscarella (2003; 2008), an ex-member of the Gordion team, has documented how this would make a complete hash of the standard archaeological synchronisms in terms of fibulae, horse-brasses, other ornamental metalwork and small finds including those firmly dated by comparanda from Assyrian or Assyrian-dated sites.

As for the initial ^{14}C results, Keenan (2004) showed they were inadequate to prove a late 9th century date. Voigt (p. 233) explains that in response to Keenan further short-lived samples were tested giving results that 'confirmed the ca. 800 BC date for the fire'. While Voigt lists the dates, they await formal publication, so it is hard to say with respect to Gordion whether we are dealing with a case like Thera where ^{14}C results are in conflict with standard archaeological dating. Otherwise the Gordion Team's use of some dendro-dates to support the backdating is deceptive – Gordion is replete with examples of re-used or otherwise 'too old' timbers (James *et al*. 1991, 323-324; amplified in Muscarella 2008, 176-179).

It is easy for archaeologists to fall prey to the desire of discovering the 'earliest' such and such, and exaggerate the antiquity of their finds (for the case of Petrie see James 2003, 239-240). A worrying pattern appears in the claims of the ADP and its co-workers. Once the date for Thera was allegedly raised to the 17th century BC, Manning (1999, 367-413) experimented with raising Egyptian chronology (in order to slightly ameliorate the problems of archaeological synchronisms with Egypt and Cyprus which Warren and Wiener have repeatedly documented). In step with this, Kuniholm announced some very early dates for Hittite sites (none properly published). So perhaps it was only a matter of time before the chronological syphon would eventually start sucking backwards the end of the Bronze Age and even Iron Age sites. Using timbers from Assiros in Macedonia, Newton, Wardle and Kuniholm (2005, 190) proposed raising the dates for Protogeometric and Mycenaean IIIC, the latter by 70 years (!); with the knock-on effect that the Trojan War (if it happened) was 50 years earlier than is usually thought (Wardle *et al*. n.d.)! By now we are frankly entering 'silly' territory, as the philosophy behind these efforts is manifestly faulty – mainly based on tree-rings and ^{14}C which can *only* give a *terminus post quem* for the wood samples (often burnt) used for the structures involved. (Further, Assiros and Gordion have very different meteorological regimes and the statistical matches are dubious – see Keenan 2006, 13-15.)

Of course claims that Thera has finally been dated, that the Trojan War was earlier and that the first examples of the Phrygian alphabet precede the Greek, are great headline grabbers and will have been very useful for advertising the ADP and attracting funding. Here Kuniholm has been very successful. In his contribution 'One Hundred Years of Dendroarchaeology: Dating, Human Behaviour, and Past Climate', Jeffrey Dean (who assisted Bannister in the original measurements of the Midas Mound timbers) rightly praises Kuniholm for his 'infectious enthusiasm', 'boundless energy' and 'unceasing fundraising efforts' (p. 28). These enabled him to establish the ADP and its laboratory as a fully working institution, a formal mechanism for future research.

It is indeed a great achievement, but at what cost? Regrettably, it is also important to remember the *damage* that has been done to the cause of chronology by swarms of hasty preliminary announcements (e.g. Uluburun and Maşat Höyük), bad statistics (Tille Höyük) and half-baked speculations regarding proxy-dating (Thera). The dates thrown out by Kuniholm have been widely cited in the literature and have misled many scholars, who would have assumed that they were 'scientifically' sound. Much of the problem here has been the tendency of the ADP, under Kuniholm's management, to make preliminary announcements of 'results', without sufficient caveat. Under the new management of Sturt Manning (2006, 1, n.*, 3, 6), a statement was issued which

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.... revises past practice. ... The process of study and analysis in Dendrochronology is not an exact science... As of now (AD 2007) the Cornell Laboratory will only state as a firm dendro-date or correlation a result that passes a rigorous process of examination and verification and our best judgment (and we will explain why we think this in the publication or statement – there will be no "ex cathedra" pronouncements).

As the volume was written as a tribute by Peter Kuniholm's friends and colleagues, it is understandable that this was not the place to start the job of broadcasting the fact that almost all of Kuniholm's 'dates' (for the ancient world) should be taken *cum grano salis*. To the contrary some of the contributions amount to

face-saving exercises or mere encomia. That's a brutal assessment: but science and scholarship are not, after all, supposed to be about 'old-boy' networks glossing over huge mistakes of a colleague's judgment or pretending they never happened at all. One of the reasons that Manning, the new Director of the ADP, issued the pronouncement above was the pressure of criticism by various gadflies, none of whom works professionally in dendrochronology – a worrying sign about the lack of mutual criticism amongst its practitioners. A notable exception is Nili Liphschitz, a senior Israeli archaeobotanist who, after reviewing the abysmal Uluburun and Tille Höyük cases, concluded bluntly: 'The Anatolian master chronology has proved to be untrustworthy' (Liphschitz 2007, 165).

Peter Kuniholm will surely be warmly remembered as a pioneer in the field and the teacher of a new generation of field and laboratory workers. But now that the fulsome and well-earned praise has been made for all the time and energy he spent putting Anatolian and Aegean dendrochronology on the map, the ADP would recover some scientific credibility were it to list publicly (on its website), exactly *which* pre-2007 ADP 'results' satisfy its new exacting standards. And those which *do not* – a long list, I fear.

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NOTES

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[1] For those unfamiliar with the concept see the excellent "John Oxley and Alan Morton's Archaeological Buzz-Word Generator" at http://www.sweeting.org/mark/buzzGen.php

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