MANFRED BIETAK, ERNST CZERNY (EDITORS)

THE SYNCHRONISATION OF CIVILISATIONS IN THE EASTERN MEDITERRANEAN IN THE SECOND MILLENNIUM B.C. III

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Clarifying the 'High' v. 'Low' Aegean/Cypriot Chronology for the Mid Second Millennium BC: Assessing the Evidence, Interpretive Frameworks, and Current State of the Debate

*Sturt W. Manning**

An expert is a person who has made all the mistakes that can be made in a very narrow field (Niels Bohr)

Introduction

In the proceedings of the first SCIEM 2000 Euro-Conference, Manfred Bietak (2003) presented an analysis entitled "Science versus archaeology: problems and consequences of high Aegean chronology". He outlined what he saw as the problems of science versus archaeology in terms of the 'high' Aegean chronology, and its incompatibility with the archaeological evidence as he reviewed it. In this paper for the proceedings of the 2nd SCIEM 2000 EuroConference, I wish to address a number of Manfred Bietak's concerns (and also those of Wiener 2003; this volume; and Kitchen 2002), and to consider what exactly are the real problems involved in this area. I wish to consider where and to what extent 'science' really is at odds with 'archaeology' – to isolate what really is a problem requiring attention, versus what is merely a non-meeting of interpretative frameworks and viewpoints. This is an important issue: at present facts and interpretations are being confused and conflated in the literature, and there is more than a little misunderstanding of some arguments. Mistakes and misjudgements have also been made by many – including several by the present author – and there is perhaps some comfort to be drawn from the well-known quotation of Niels Bohr (above)! Various pieces of independent information that are valid and useful by themselves are also being associated wrongly with other issues; and, as always in *both* science and archaeology, some views and hypotheses must be revised or discarded as new and better evidence, and new and better analyses, come to hand.

The key 'point' I wish to make in this paper is that in reality we do *not* have a simple science v. archaeology situation. All the 'problems' noted by critics of the Aegean high chronology (here taken as Bietak 2003; Wiener 2003) exist if one analyses only the traditional (archaeological) evidence. The scientific evidence – and I mean solely radiocarbon (see below) – merely exaggerates (or highlights) these problems – it does not in fact create them.

I have tried not to burden this short paper completely with endless references to what is now a very considerable body of literature from the last 30 years; I try to cite just various key and where possible recent publications (which then have relevant further references). Readers seeking further bibliography should consult the papers and books cited for more details on specific areas and views.

Prior assumptions and mentalité – the need to be open minded

A significant problem with much of the debate and literature on the chronology of the middle second millennium BC east Mediterranean, and on the date of the Thera eruption, is that various authors begin any study with a largely pre-determined position. They believe some set of views, or set of data, are effectively right or paramount and everything else is then analysed accordingly – thus alternative evidence receives intense critical comment and or dismissal (even is ignored), while confirmatory evidence or scholarship is simply stated and or praised with little critical consideration or self-reflection. Some of the evidence we have is only partial or less than explicit and unambiguous – the temptation (often even unconscious) is to interpret/manipulate such data to serve a pre-conceived point of view. Arguments on such matters abound across our research fields, even in the hallowed world of Egyptian chronology, where Luft (2003:202) makes exactly such criticisms of some other leading scholars – concluding that: 'each scholar had a pattern to interpret the texts, while I prefer tracing a pattern inside the texts'. The outcome of such pre-conceived positions and assumptions, the resultant selective filtering of information, and the not unimportant role of the

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academic ego, is that only small and incremental changes and revisions are made to the 'right' basic position. Radical revision is avoided where possible, and the approximate status quo is maintained almost on principle.

Nearly everyone is guilty here – the present author included. The number of leading scholars in this field who have significantly changed their minds and published positions (once made) over the last three decades is very small. Instances of new evidence prompting a leading scholar to wonder if all the evidence can be re-assessed and interpreted in a significantly different way are very rare: one especially thinks of the remarkable paper by PHILIP BETANCOURT (1987). This is an indictment of us and of the way the modern academic 'industry' works: people are often not encouraged, or given the time and opportunity, to reflect and to re-assess.

What is worse is that this topic involves inter-disciplinary research and cross-overs. It is difficult or impossible to be expert in all areas, and difficult to be even handed to all evidence and to judge and criticise it appropriately on its merits. Sometimes a person from outside a field will in fact offer better critique and analysis than those within who have lost some perspective as they struggle with minutiae and tradition. Other times specific expertise is necessary even to be qualified to offer a worthwhile opinion. And so on.

How did we get to where we are? And is this whole debate a case of science versus archaeology? No.

By 1980 the 'conventional' archaeological chronology of the mid-second millennium BC had been challenged on solely archaeological grounds – different interpretation of the archaeological evidence. The key scholar was Robert Merrillees, who proposed a synthesis of the Cypriot evidence that led to a 'high' chronology, and who, with Barry Kemp, also did the same for the Minoan evidence (key publications: MERRILLEES 1968; 1977; KEMP and MERRILLEES 1980). There was of course a vigorous critical response, especially in the Levantine/Egyptian and Minoan spheres (e.g. OREN 1969; WARREN 1985) – whereas Merrillees' Cypriot chronology in many ways went on to become the standard and mainstream one in its field. Merrillees did *not* use radiocarbon or any other scientific evidence – indeed he was highly critical at that time of the value or utility of radiocarbon evidence.

Independently in the mid 1970s, and onwards, sufficient radiocarbon evidence became available from mid second millennium BC contexts to indicate that radiocarbon dates seemed to point to a somewhat earlier chronology. But, and entirely plausibly given the poor precision and accuracy of radiocarbon dating at that time, and the limited evidence, it at first seemed that something must be wrong with these new dates and that preference should still be given to the conventional archaeological chronology (BETANCOURT and WEINSTEIN 1976). But the radiocarbon evidence continued to mount. Then, in the 1980s, it also seemed that tree-ring and ice-core evidence combined to indicate a series of 'packages' of major volcanic eruptions in the past (BAILLIE and MUNRO 1988; HUGHES 1988). It seemed plausible (possible anyway) that one of these packages in the 17th century BC might be Thera.

In 1987 Betancourt published, noting the science-dating evidence in favour of a 17th century BC date for the Thera eruption, and so also for the mature Late Minoan IA period; he then proposed that a re-assessment of the archaeological evidence was possible which could yield a compatible archaeological chronology. Manning, who was a student at this time in Australia, had likewise noted this situation, and published a similar independent case in 1988. The critical thing is that such an archaeological re-assessment was *already* possible (pre-science evidence), and the case had been argued by Merrillees, and Kemp and Merrillees. As HALLAGER (1988:12) noted, the flexibility possible for the Middle Minoan III–Late Minoan IA periods existed because there was in fact almost no archaeological evidence – its date was a best interpretation/estimate between better synchronisms and from some fairly loose stylistic associations. Thus he concluded that 'it is important to stress that the renewed investigations of the traditional synchronisms of the MMIII/LMIA material have shown the contexts – both the Egyptian/Near Eastern and Aegean – so dubious that a revised high chronology for the beginning of the LMIA is possible'.

Therefore, we did and do not have a science versus archaeology split: instead there were two existing views of the archaeological evidence and some scholars argued that the science evidence seemed to support one of these more than the other. This alternative case was then developed over the following 11 years up to MANNING (1999). However, this was undoubtedly the road less travelled by.

A great deal has changed since 1988 and indeed since 1999 (and much has not). I note below (Sections 1–3) several key things, which may, for the present (AD2004), be put aside – but which seemed important in previous years (the tree-ring and ice-core evidence) – so a key change. Much new evidence of much better quality is now available from radiocarbon measurements. This needs to be considered and is another key change to the past situation. And much new archaeological evidence and study is also available – and in particular data emanating from the important Tell el-Dab^ca project led for many decades by Manfred Bietak. This too needs to be carefully considered. And so on.

What the field needs is honest and open re-assessment of all of the relevant evidence now (AD2004) pertaining, and of its interpretation(s) – without a pre-determined answer. The following is my attempt.

1. The Aegean Bronze-Iron Dendrochronology (the Aegean Dendrochronology Project datasets from the later 3rd through earlier 1st millennium BC built up over 30 years by Peter Ian Kuniholm and collaborators)

This 1503 year tree-ring sequence has been nearly absolutely dated through the measurement of a great many high-precision radiocarbon dates on specific decadal blocks of wood within that chronology (Manning *et al.* 2003; 2001a; and further work in progress). A previous – and now replaced – dating published in 1996 was based on just 18 dates (Kuniholm *et al.* 1996). This 1996 paper placed ring 854 (for example) of the chronology at c.1641BC as the 'best' fit from the radiocarbon data (this does not mean the correct fit – merely the one most likely given the then available data). The error range on this fit was quite large. For various reasons we then proposed, within this 1996 error range from the radiocarbon data, a date some 13 years later given information and our interpretation/viewpoint in 1995–1996. In 2001 using 52 radiocarbon data, and then in 2003 using 58 data, we found that the 1996 date (and the underlying hypotheses) was no longer possible. Moreover the date of the Dendrochronology could now be defined at good confidence levels within quite narrow boundaries even allowing for the range of data and the variations at issue. This led to a date for ring 854 now somewhere between 1653–1650BC, give or take small errors (and note this is in fact just +9 to +12 years from the 1996 radiocarbon date and well within the error range specified in 1996 from the radiocarbon evidence). This new 2001 onwards dating had nothing to do with any other evidence or other hypotheses. It is independent. This dendrochronology is a fact and its dating is very near absolute; it forms a key chronological framework for the Near East and associated regions. A probable link with the previously floating Early Bronze Age chronology means that we are close to having a nearly absolute 2009 year long tree-ring chronology for the entire Bronze Age of the Aegean-east Mediterranean region (Newton and Kuniholm 2004).

As is well known, a remarkable growth anomaly occurs over a few years in this Aegean dendrochronology starting in ring 854 (in 61 constituent trees as of early 2004). It has been suggested that this anomaly *could* be consistent with the impact of a massive low-mid latitude northern hemisphere volcanic eruption, and in particular Thera (Santorini). However, there is at present absolutely no positive evidence that connects the two events. The tree-ring anomaly, while extraordinary and at present unique in the seven thousand years of Aegean Dendrochronology available, could be something else. We do not know. A plausible/possible suggestion (and no more) has been made. However, any connection with Thera is *not* a fact and has *no* worth in strict analysis concerning east Mediterranean chronology. On present stated dating errors, there is possibly (but not necessarily) a temporal overlap with the very large volcanic signal in the Dye 3/GRIP ice-core c.1645BC – however this is not certain (they could be a few years even a decade apart), and, moreover, this volcanic signal seems not to be related to Thera on current evidence (see Section 3. below). (I note that there is just the one, solitary, tree-ring growth anomaly – Bietak 2003:23 implies we abandoned one and then chose another – no – the absolute date of the same ring 854 growth anomaly, and the whole Dendrochronology, moved with the re-dating based on many more and better radiocarbon determinations as reported in Manning *et al.* 2001a; 2003).

2. Other Tree Ring Information

A widely attested significant tree-ring growth anomaly occurs in the northern hemisphere at 1628/1627BC (Amarche and Hirschboeck 1984; Baillie 1995 and references; Grudd *et al.* 2000). It has been suggested that this phenomenon could be compatible with the impact of a large volcanic eruption, and Thera has been suggested as a candidate. However, there is no positive evidence for either proposition. The putative correlation with ice-core evidence previously suggested as possible (e.g. Hughes 1988; Baillie 1996) now seems on dating grounds to be impossible (Hammer 2000; Hammer *et al.* 2003) or unknown (Southon 2002). By itself this evidence tells us nothing *firm* about Thera and the dating thereof.

The seemingly strong circumstantial case of the late 1980s through early 2000s, where tree-ring and ice-core evidence offered a set of compatible 'packages' of evidence around several likely major volcanic events of the last several thousand years (e.g. BAILLIE 1995), has since been disproved and shown to be irrelevant. First, the better dating of the ice-core data broke apart most of the suggested 'packages', and negated the circumstantial case (MANNING and SEWELL 2002). Second, with regard to the Thera volcanic eruption, recent work has shown that it is unlikely that Thera can be identified with the proposed ice-core volcanic signal – again negating the supposed package of events discussed in the later 1980s through early 2000s (see next Section).

3. Ice-Core Information

At the time of writing there is no satisfactory evidence for the identification of, and thus the date of, the great Thera volcanic eruption in any ice-core. There are various acid signals in various ice-cores across the period c.1700–1450BC; these represent various volcanic eruptions – but there is no positive link at present for any of these with Thera. The proposal that the very large volcanic signal c.1645BC in the Dye 3/GRIP ice-cores be identified with Thera, on the basis of analyses of the composition of tiny volcanic glass fragments recovered (Hammer *et al.* 2003), has been shown to be incorrect (PEARCE *et al.* 2004; n.d. this volume) – indeed Pearce *et al.* argue that the glass shards recovered from the c.1645BC GRIP core layer appear compatible with an Aniakchak provenance.[1] Thera is thus *not* dated at present from ice-core evidence. Clearly, various other candidate volcanic signals exist in the several ice-cores, and it is to be hoped that future work will provide both a robust identification and a precise date (although one may speculate in advance that, with the likely limited number of tiny glass shards available, and the available [or presently foreseeable] analytical possibilities, it may not be possible to get a truly definitive outcome). Significant problems with dating for some cores (e.g. GISP2) have also to be overcome (SOUTHON 2002). This body of evidence can be set aside at present, until future work brings it back into play.

4. Radiocarbon evidence for Aegean Late Bronze I–II chronology

With the tree-ring and ice-core evidence currently *irrelevant* to the dating of the Thera eruption, radiocarbon offers the only high-precision, direct, and independent science-dating source for the timing of this event, and for the associated archaeological phases. A large high-quality body of new radiocarbon evidence has been produced in the period 2000–2004 directed at the dating of the Thera eruption and the associated Late Minoan IA, IB and II periods, developing previous work. One recent summary was presented at the May 2003 SCIEM Euro-Conference 2, and another up-dated one at a meeting in Vienna in January 2004.

A publication of all the data from this project produced at the Oxford Radiocarbon Accelerator Unit, and a robust set of analyses of these data (and previous Oxford data – from HOUSLEY *et al.* 1999; 1990) via a set of holistic Bayesian models is in BRONK RAMSEY *et al.* (2004a). Numerous known age data run at Oxford across this time demonstrate the good accuracy and precision of the laboratory (MANNING *et al.* 2002b:735 caption to fig.1; BRONK RAMSEY *et al.* 2002:2–4; BRONK RAMSEY *et al.* 2004b). The BRONK RAMSEY *et al.* (2004a) paper uses the (then) current internationally recommended radiocarbon calibration curve (INTCAL98; STUIVER *et al.* 1998a) – a further paper (presentation in Vienna January 2004, written text in preparation) will discuss these data, and additional data run at the VERA laboratory in Vienna (final samples still in progress as this text is written), against both the new INTCAL04 calibration curve, and a variety of other radiocarbon calibration datasets in order to assess the robustness of the calibration (cf. WIENER 2003:384–386). (Such analyses include calibration against Aegean data where *no* significant regional/growing season offset can apply *even*

[1] KEENAN (2003) previously disputed the HAMMER *et al.* (2003) claim on statistical grounds. His conclusion has been shown to be correct. But the statistical method employed by Keenan is probably not appropriate for the type of data in question, as PEARCE *et al.* (this volume) make clear:
Recently, KEENAN (2003) employed t-tests on the standard errors of the analyses of HAMMER *et al.* (2003) (standard error = standard deviation / $\sqrt{\text{number of analyses}}$) to show that the Minoan Bo-1 sample and the A1340-7 glass cannot be the same. This approach, where the numbers of analyses are large (i.e. n=174 for the ASEM analyses of the ice-core glass) may however reduce the errors to unattainably small values, far less that the true analytical reproducibility attainable by multiple analyses of homogeneous materials (PEARCE *et al.* 1997). In doing so, when comparing different materials, the standard error approach may enhance the apparent differences between samples.

during the few major solar minima cooling episodes where such regional factors may create a *small* short-term offset as discussed in KROMER *et al.* 2001; MANNING *et al.* 2001a – at other times no systematic detectable/significant offsets appear to exist within mid latitudes of the same hemisphere: also see MANNING *et al.* 2002c; TALAMO *et al.* 2003.) These new papers supersede previous preliminary discussions using only a few of the initial data from this project – although the general findings remain similar.

When we consider the date of the Thera eruption, we can begin with the 23 determinations at present available from internally consistent sets of defined final Volcanic Destruction Level (VDL) contexts on Thera from short-lived samples from standard modern pre-treatment and processing regimes – comprising (i) the Copenhagen laboratory n= 4 set (FRIEDRICH *et al.* 1990 – see comments by MANNING and BRONK RAMSEY 2003:128–129), the Oxford Laboratory 1990 published series using only the standard pre-treatment samples from the final VDL n=8 set (stages 2/3 in that paper) (HOUSLEY *et al.* 1990), the new Oxford measurements n=8 set (published in BRONK RAMSEY *et al.* 2004a), and the first half of a VERA set with n=3 at present (publication forthcoming once dating programme finished), then all these data (all on *different* seed or short-lived plant matter samples – even if from the same pot – and one twig) can combine satisfactorily within 95% confidence limits to offer a weighted average – this can then serve as a best estimate for the real average or typical radiocarbon age representative of the overall range of radiocarbon values within the set of short-lived samples from the VDL horizon: see Figure 1.

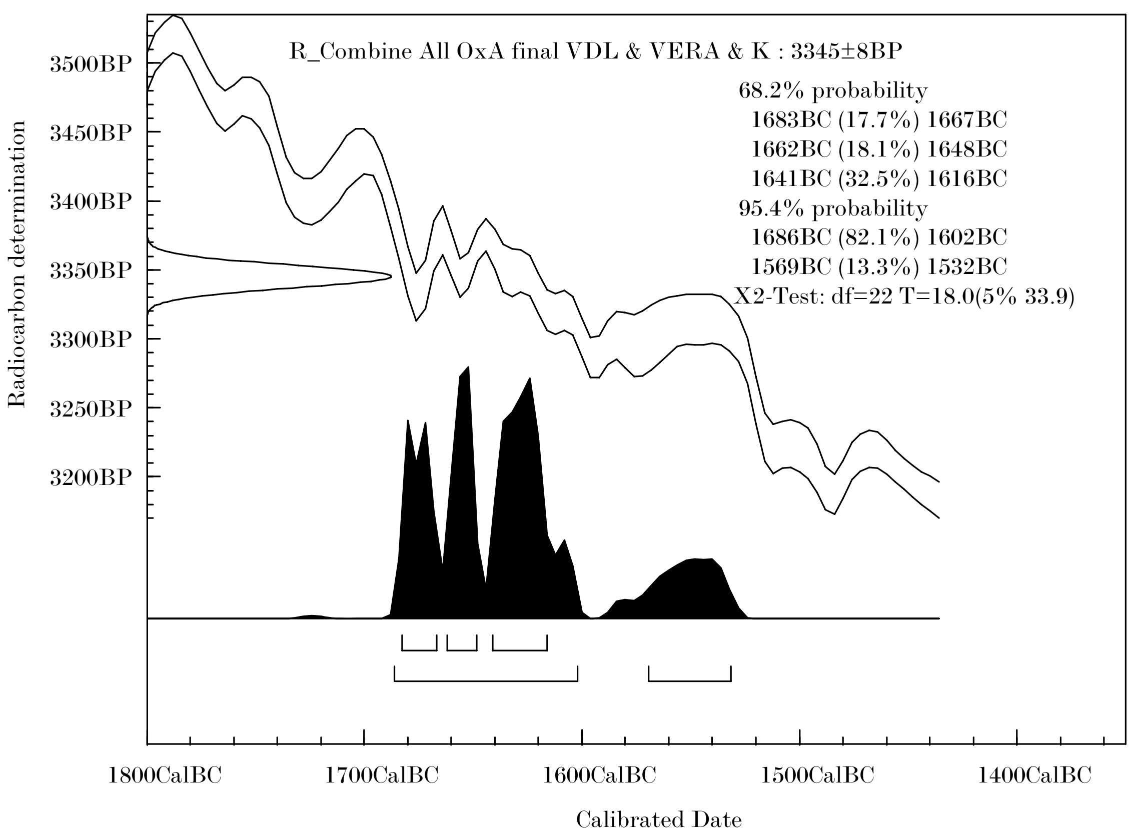

Fig. 1 Calibrated age probability distribution for the best current weighted average radiocarbon age for the Volcanic Destruction Level at Akrotiri, Thera. Data from the 23 determinations at present available from defined VDL contexts on Thera from short-lived samples from standard pre-treatment regimes – comprising (i) the Copenhagen laboratory n= 4 set (FRIEDRICH *et al.* 1990 – see comments by MANNING and BRONK RAMSEY 2003:128–129), the Oxford Laboratory 1990 published series using only the standard pre-treatment samples from the final VDL n=8 set (stages 2/3 in that paper) (HOUSLEY *et al.* 1990), the new Oxford measurements n=8 set (published in BRONK RAMSEY *et al.* 2004), and the first half of a VERA set with n=3 at present (publication forthcoming once dating programme finished – I thank Walter Kutschera and the VERA laboratory team for their collaboration). Calibrated data from OxCal 3.9 (BRONK RAMSEY 1995, 2001 and later versions) with curve resolution set at 4 and INTCAL98 (STUIVER *et al.* 1998a) – note: the calibrated age ranges will need revision in light of INTCAL04 (and also assessment in light of other relevant calibration datasets); such issues will be considered in a future publication formally reporting the final data from the New Palace chronology project and their analysis

Note: the fact that all 23 data *can* combine satisfactorily together within 95% confidence limits is quite impressive and very much indicates that the *same* narrow dating horizon is being sampled (and then combined) and that the data represent a spread around the typical age (despite some likely issues concerning humic acid contamination for some samples [Manning 1999:237–239] – but the effect is clearly small as the overall set offers a consistent analysis and so, overall, is probably insignificant). The calibration of this average tells us what is already well known: the radiocarbon evidence indicates a most likely age range in the 17th century BC (82.1%), but a mid-16th century BC (13.3%) range is also possible.[2]

To try to move beyond the limitations of the single case analysis, the New Palace project analysed data from LMIA through LMII contexts from before, around, and after the Thera eruption. On the basis of a Bayesian sequence analysis of the current Oxford data for the New Palace period (in all 108 determinations – and only these data – the paper does not discuss other data or issues still in progress), and so including the constraints available from the archaeological seriation for the phases before and after the VDL on Thera, and using INTCAL98, the date range of the eruption of Thera is further refined in the study of Bronk Ramsey *et al.* (2004a), and is placed at 95% confidence level between c.1663 and c.1599BC (i.e. the mid-16th century BC range is largely excluded by the analysis of the New Palace data sets together). A date for the close of Late Minoan IB at two sites is suggested to lie roughly within the period c.1520–1490BC (give or take), and the close of Late Minoan II destruction at Knossos (Unexplored Mansion) is placed roughly c.1420BC give or take a decade or so. It must be noted that this is a large and mostly high-quality dataset. It cannot simply be dismissed as a few odd measurements.

These data, produced in the context of frequent known age tests, and consistently on two different accelerators at Oxford, offer a significant and internally robust chronological framework. They suggest an Aegean chronology for the Late Minoan IA and IB periods that is rather 'higher' and 'longer' than the conventional chronology. This evidence is dependent on the archaeological stratigraphy and relative phasings provided by each of the site excavators with regard to each specific sample; it is *independent* of any preconceived 'high' or 'low' chronological syntheses.

In the course of wide-ranging reviews of Bronze Age chronology, Malcolm Wiener (2003:380–395, this volume) raises a number of possible or theoretical concerns with the accuracy or precision of radiocarbon dating, and also makes critical comments on the preliminary papers of Manning *et al.* (2002b), and Manning and Bronk Ramsey (2003). I thank him as always for his close interest and critical attention to our work, and for a number of stimulating conversations. All his points deserve consideration; but, at the same time, it has to be noted at the outset that the level of ultra-scepticism directed now at the radiocarbon evidence is not also so directed at the archaeological evidence and its synthesis. The playing field is not level; nonetheless, radiocarbon is starting to make a real, robust, and relatively precise and accurate contribution to the debate (Bronk Ramsey *et al.* 2004a).

In general, I refer the reader to the Bronk Ramsey *et al.* (2004a) paper where a large body of data and a robust analysis over a set of analytical models

[2] Data not employed in this Akrotiri VDL assessment and reasons are:

(i) The old Pennsylvania data employed in e.g. Manning (1988 with references) – many of these data lack either NaOH pre-treatment or ^{13}C correction; further a number of these samples are not clearly or necessarily 'short-lived', and/or short-lived directly relevant to the final VDL (versus roofing matter and so on).

(ii) The Simon Fraser data (Nelson *et al.* 1990) – since this group did not publish their data (so impossible to analyse) and there remains no evidence to support their novel sample preparation strategy (Manning 1999:237–238).

(iii) The Zurich data – although a presentation was made at the Thera and the Aegean World Conference in 1989, these data were not then published in the proceedings (mention with sample details occurs later in another paper: Wölfli 1992:40–41) and are impossible to analyse.

(iv) Heidelberg data – the two dates on short-lived samples published by Hubberten *et al.* (1990:184 and table 2) do not offer a consistent set, as the two measurements are widely varying(N.B. while p.184 identifies just two samples as short-lived, table 2 indicates that sample 7092-6795 of "peas" is also short lived. Its radiocarbon age of 3360±60 BP would agree with the short-lived average in the text very happily). However, as the only two other available data (with details) with standard pre-treatment and correction (using just the two samples identified on p. 184), it is worth noting that they could in fact be added to the 23 date set employed in the main text without making the set fail to combine satisfactorily within 95% confidence limits (and their inclusion/exclusion hardly changes the weighted mean): weighted average of all 25 data 3344±8 BP with Chi-Squared test statistic of 29.7 less than the 95% confidence limit value of 36.4 for 24 degrees of freedom. Calibrated calendar age ranges: 1686–1602BC (81.0 %), and 1569–1532BC (14.4%). (OxCal 3.9 and INTCAL98)

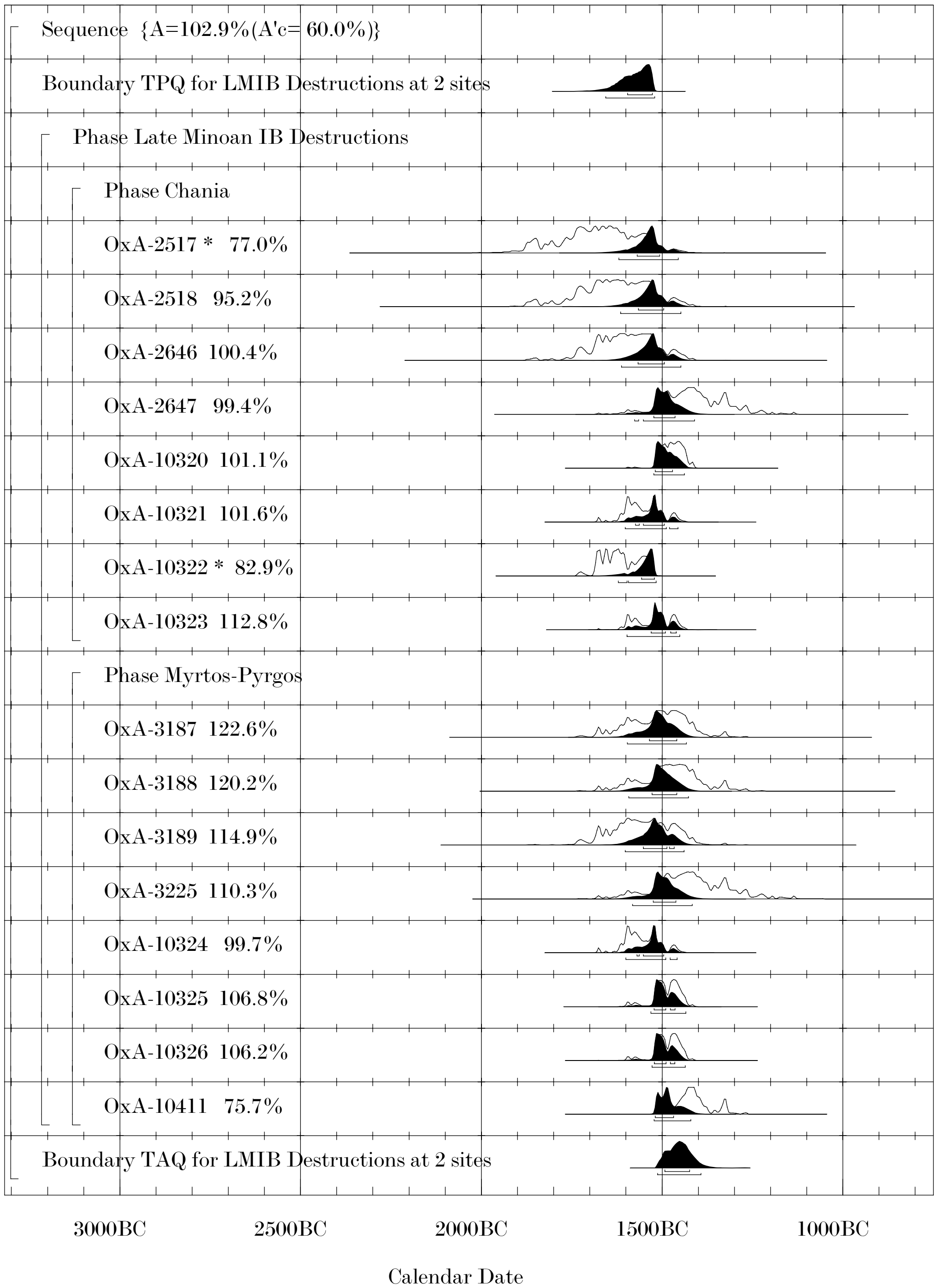

Fig. 2 Sequence-Phase analysis of the radiocarbon ages on short-lived samples from the close of Late Minoan IB destructions at Chania and Myrtos-Pyrgos, Crete (for data, see MANNING *et al.* 2002b:737). Individual calibrated ranges are shown by the hollow histograms, calculated calibration ranges given Phase and Sequence constraints are indicated by the solid histograms. The % number indicates an agreement index between the former and the latter – a value over about 60% indicates satisfactory agreement at the 95% confidence level. The data centre around c.1500BC give or take a couple of decades. Each pair of determinations on similar sample matter (sample from same pot/context) combine satisfactorily, but two Chania samples, marked with an *, do not then combine with the other Chania data within 95% confidence limits under a Chi-Squared test (12.6 v. 7.8 for df3). Data from OxCal 3.9 (BRONK RAMSEY 1995; 2001; and later versions) and INTCAL98 (STUIVER *et al.* 1998a). Curve resolution set at 4. The lines under the distributions indicate the (upper lines) 1SD (68.2%) and (lower lines) 2SD (95.4%) calibrated ranges

offers our best current evidence. A number of other suggested problem issues were discussed and dismissed as significant by MANNING and BRONK RAMSEY (2003:124–129). Overall, and since WIENER (2003) was written, a considerably greater number of data now exist, including new data from the VDL on Thera, and a number of these samples, or similar samples, have been measured twice (and yield consistent outcomes) – a number of samples have also been measured at the VERA laboratory in Vienna. Further considerations of suggested issues of intra and inter year variation, and volcanic source carbon dioxide, were offered in the January 2004 Vienna presentation, and will be included in a written text in preparation at present (again I thank Malcolm Wiener for on-going discussions). Some of Wiener's concerns require additional research (in progress). I take this opportunity, however, to admit/concede to some failings in preliminary publications, as highlighted by WIENER (2003).

(i) MANNING *et al.* (2002b) was a little over confident and sweeping in its conclusions – 'excited preliminary report syndrome'. Mea culpa. Nor did it consider variation in calibration dataset issues (something which is not considered in almost any other archaeology/radiocarbon work anywhere either – work on this topic as part of the East Mediterranean Radiocarbon Intercomparison Project [e.g. KROMER *et al.* 2001; MANNING *et al.* 2001a] in fact places the Aegean-east Mediterranean at the forefront of current radiocarbon research). Much more robust and reflective analyses are offered in BRONK RAMSEY *et al.* (2004a) and especially – dealing with calibration robustness – in the January 2004 Vienna presentation (written paper in preparation). The actual conclusions reached do not vary significantly from those in MANNING *et al.* (2002b) and MANNING and BRONK RAMSEY (2003) – but they are much more securely based both in terms of quantity of data and in terms of robustness of analysis. The only point of relevance (versus refinement) concerns the close of the LMIB period as dated at Chania and Myrtos-Pyrgos. Here use of the INTCAL98 curve favours a date around c.1520BC where there is a steep slope in the calibration curve – but this slope's existence/scale is strongly influenced by one date on Irish Oak for a bidecadal sample centred 1510BC (WIENER 2003:392). Removal of this datum removes the slope here and would seem to allow the LMIB data to move downwards, and back perhaps nearer 1490BC (compare the conclusions of HOUSELY *et al.* 1999, who in their main text employed just the Seattle dataset on German Oak – see also Figure 5 and 6 below). In fact, the effect is fairly marginal. Much more critical is that one of the Chania samples could also be considered to be problematic (the sample of peas from TR10, Room E), which in two measurements yielded rather 'high' ages – and which do not combine with the other Chania data at the 95% confidence level) – these two determinations are marked with an * in Figure 2; removal significantly helps to favour a slightly lower date centred c.1500–1490BC.[3] For a new review of the dating of the Late Minoan IB destructions at Chania and Myrtos-Pyrgos, see Figures 2–6. This revised analysis finds that the majority of probability favours a date c.1520–1490BC for the close of LMIB destructions at both sites (and so a gap between the respective destructions of only a few years to a couple of decades). A rather lower probability could allow a date for one site (most easily Myrtos-Pyrgos – and so also a longer gap between the destructions) or (marginal probability) both sites within c.1475–1460BC. Either position requires a significant revision to the conventional chronology where LMIB ends c.1425BC (WARREN and HANKEY 1989:169) or c.1430BC (WARREN 1999:902).

(ii) WIENER (2003:391 and n.148) quoting William Cavanagh criticises MANNING *et al.* (2002b) for citing some modes or peak probability regions in their text/captions, rather than complete 1SD or 2SD ranges. I accept this criticism and regret the phrasing. The figures showed the 1SD and 2SD ranges, but the text perhaps implied greater precision than reasonable (versus suggesting as a commentary the most

[3] Why this sample yields a significantly older radiocarbon age is not clear. There is no reason to believe that its context is not similar, and the peas will not have been stored for more than a few years (I thank Erik Hallager for pers. comms. on this issue). Rapidly changing atmospheric radiocarbon levels over a few years might help explain the range in the Chania set – but this approach, based on the INTCAL98 calibration curve, has now been shown to be insecure (WIENER 2003:392). Growing season, or typical inter-annual, variations are unlikely to account for such a significant difference. We are thus left with a problem and no clear explanation. Since the two data from this sample are significantly different from the six data from the other Chania samples, the best course on review seems to be to exclude them. Since the excluded data are 'high' data, this means the provisional conclusions reached now are conservative and doing everything to help the 'low' chronology. There is no high chronology bias at all.

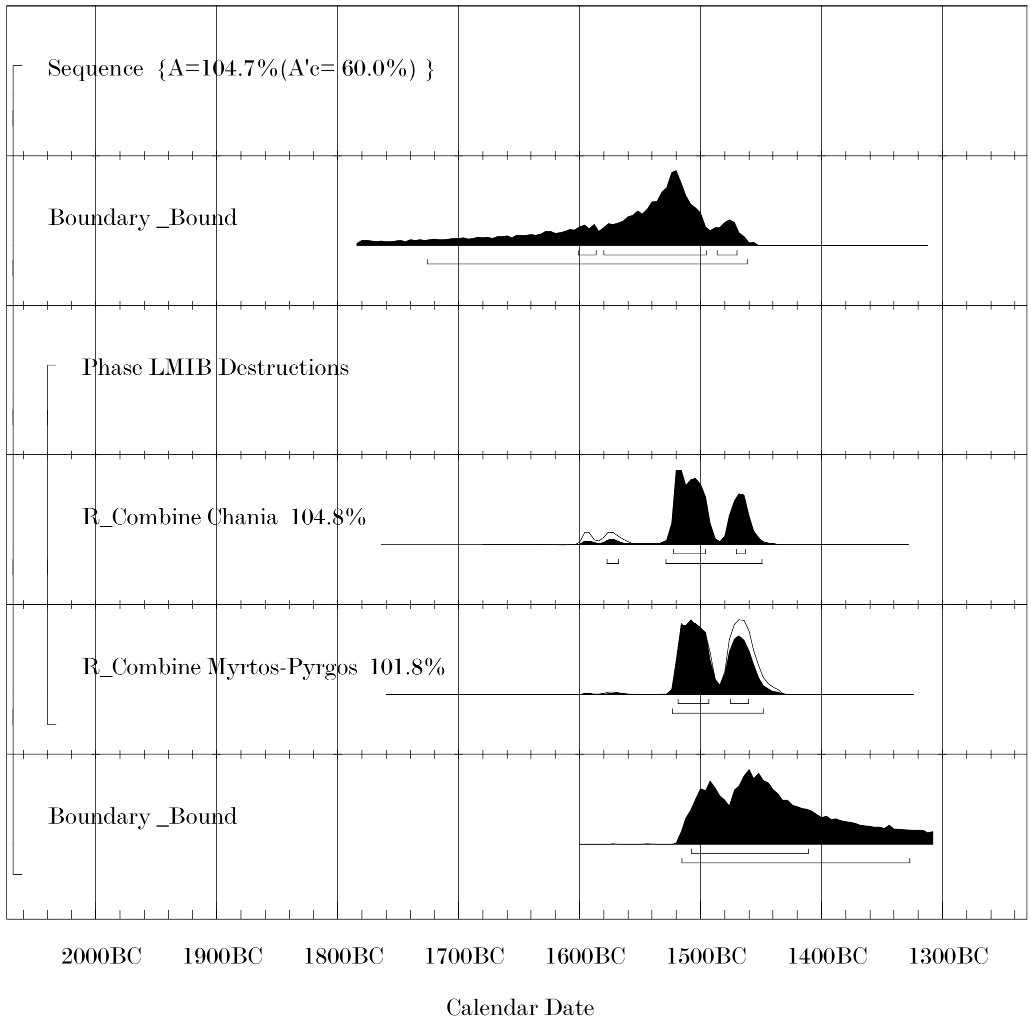

Fig. 3 Sequence-Phase analysis of the weighted average radiocarbon ages for the close of LMIB short-lived sample destruction datasets from Chania (excluding OxA-2517 and 10322 – see Figure 2) and Myrtos-Pyrgos. The calculated 1SD ranges for Chania are:1522–1496BC (56.3%), 1470–1463BC (11.9%), and for Myrtos-Pyrgos: 1518–1493BC (46.2%), 1475–1460BC (22.0%). The 2SD ranges end in 1449BC and 1448BC respectively. Thus both datasets clearly prefer a fit around or just before c.1500BC, but could date c.1475/70–1463/60 at rather lower probability. A date after c.1450BC seems ruled out. Data from OxCal 3.9 (Bronk Ramsey 1995; 2001; and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The lines under the distributions indicate the (upper lines) 1SD and (lower lines) 2SD calibrated ranges

likely tendency within the probability ranges). Mea culpa. The reader will note that for the quantified calibrated ages given above, I quote the 68.2% (1SD) or 95.4% (2SD) confidence total ranges. These are shown also in Figures 1–6.

The pattern in the radiocarbon evidence as a whole seems to be that radiocarbon and archaeological/historical dating work to give compatible information in the late 15th through 12th centuries BC (e.g. Manning *et al.* 2001b; 2002c; Manning and Weninger 1992). Acceptable match ups exist also in the Early Bronze Age (e.g. Kromer *et al.* 2003; Manning 1995; 1997) and into the first few centuries of the second millennium BC (e.g. Marcus 2003). The 'problem' is the 17th–16th centuries BC. Here radiocarbon dates from the Aegean have, for three decades, pointed towards a chronology rather 'higher' than the conventional archaeological interpretation. It is notable – and I think important – that this period is also 'thin' in plural (replicated) good, tight, archaeological synchronisms (as Hallager 1988 noted) – it lies between the multiple well-dated 14th–13th centuries BC synchronisms (Warren and Hankey 1989:146–162; Hankey and Aston 1995), and the pretty well-dated Kamares-Middle Kingdom synchronisms (Merrillees 2003). The limited or ambiguous to non-existent information available can plausibly be interpreted in different ways (either high

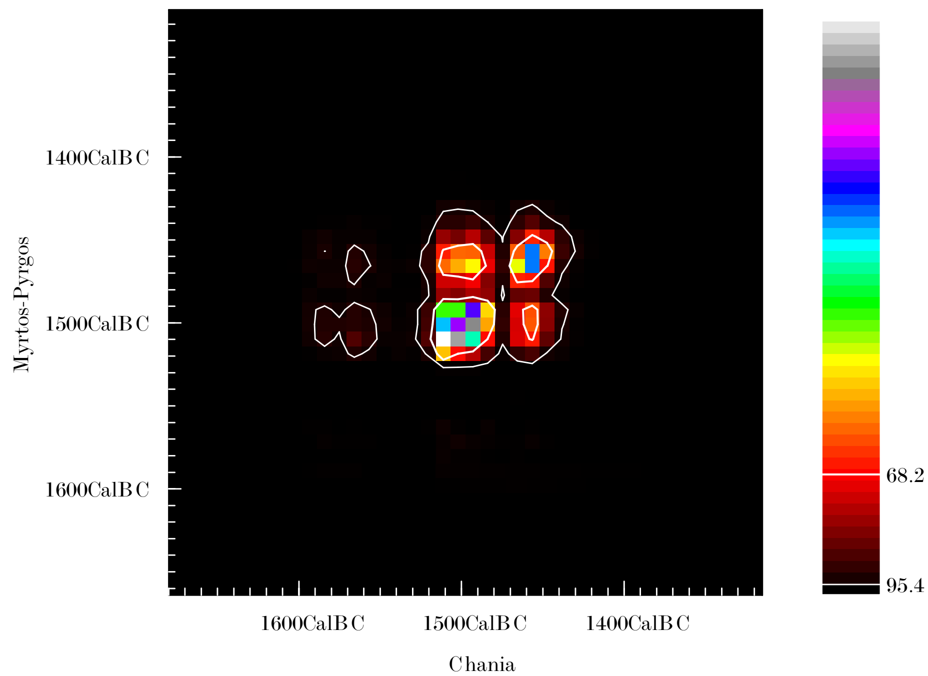

Fig. 4 Correlation plot from the analysis in Figure 3 showing the correlation of the calibrated calendar ages for the weighted average radiocarbon ages for Chania (excluding OxA-2517 and 10322) and Myrtos-Pyrgos for the LMIB destruction short-lived data. The inner (white) contour lines denote the limits of the 1SD ranges, the outer (white) contour lines denote the limits of the 2SD ranges. What we see is that the single most likely region for both sites to date is within the range c.1520–1480BC. This would also mean a relatively short gap between the respective destructions. However, it is also possible for (especially) the Myrtos-Pyrgos destruction to be later, around c.1470–1450BC. This would be most easily achieved if a gap of some 20–30+ years was permissible between the respective destructions. It would be possible at the lower margins of the 1SD limits to place both destructions here, but the Chania dataset is less consistent with this. Data from OxCal 3.9 (Bronk Ramsey 1995; 2001; and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The probability scale is shown on the right – from highest probability in white at the top to the lowest probability in black at the bottom

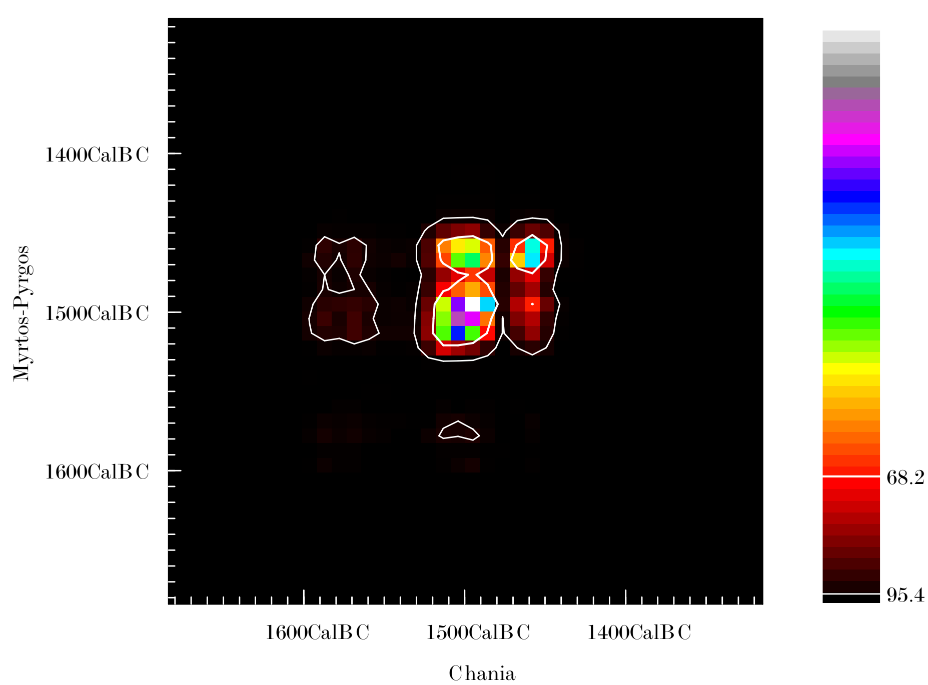

Fig. 5 As Figure 4, but using only the Seattle calibration dataset UWTEN98 (Stuiver *et al.* 1998b) (on German Oak for this period) and so avoiding the influence of the Belfast dataset (cf. Wiener 2003:392). The outcome is very similar. The probability scale is shown on the right – from highest probability in white at the top to the lowest probability in black at the bottom. The 1SD and 2SD limits are shown – these are the contours on the plot (inner contour = 1SD range limits, outer contour = 2SD range limits)

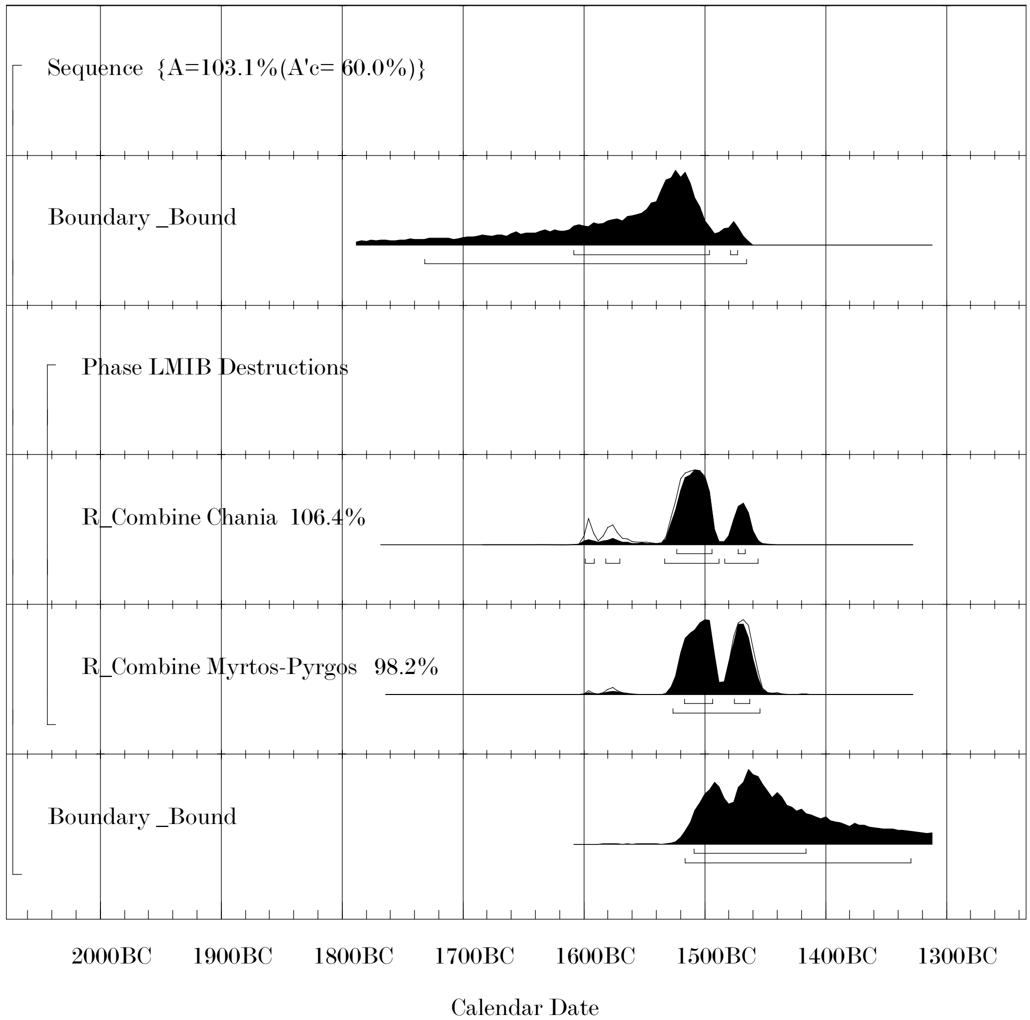

Fig. 6 As Figure 3, but using only the Seattle calibration dataset UWTEN98 (Stuiver *et al.* 1998b) (on German Oak for this period) and so avoiding the influence of the Belfast dataset (cf. Wiener 2003:392). The outcome is very similar. The calculated 1SD ranges for Chania are:1523–1494BC (60.9%), 1472–1466BC (7.3%), and for Myrtos-Pyrgos: 1517–1493BC (44.7%), 1476–1463BC (23.5%). The 2SD ranges end in 1456BC and 1454BC respectively. Thus both datasets clearly prefer a fit around or just before c.1500BC, but could date c.1476/72–1466/63 at rather lower probability (and especially for Chania). A date after c.1455BC seems ruled out. The lines under the distributions indicate the (upper lines) 1SD and (lower lines) 2SD calibrated ranges

or low dates or in between), or are vague, as highlighted by Merrillees (1972; 1977); Kemp and Merrillees (1980); Betancourt (1987; 1990); Manning (1988), etc.

Now one approach to the problem is to argue that for some unknown reason(s) radiocarbon dates in the Aegean are wrong (too old) just for the 17th and 16th centuries BC. And not just for one site or island, but for the whole central-southern Aegean region, and not just for a short interval of a few years (i.e. *not* just in the few years leading up to and around the Thera eruption), but for all of Late Minoan IA and right through to mature/late LMIB. And yet radiocarbon dates must also 'work' in the same region for the previous millennia, and for those following. Wiener (this volume; 2003:383) speculates along these lines, raising a variety of potential or possible suggestions – though none with any actual positive evidence of applicability to the Aegean data at issue – and with only some of actual potential significance when one follows through the data. The problem is that at present there seems no even vaguely satisfactory explanation that could plausibly account for such a small and consistent/systematic 'old' age error/contamination for radiocarbon dates for the whole region at this time (and only this time) (Manning *et al.* 2002c). The crunch is that cited instances of major effects creating anomalous radiocarbon ages (e.g. releases of volcanic ^{14}C depleted CO_2) are highly localised and/or time varying and would (i) leave a clear signal in a pattern of significantly affected data tailing away to non-affected data by distance and/or against time (and *not* the more or less consistent small, ca. 50 radiocarbon years or so, offset over a wide area and significant time span as in effect sought by Wiener, this volume; 2003), and (ii) cannot explain a consistent large area minor significant effect as the 'effect' is rapidly diffused and eradicated against both the scale of the atmosphere and the scale of other $^{14}CO_2$ inputs. The much smaller effects, or specific geology/soil speculations raised, could never explain a large area consistent effect on radiocarbon dates – and the former would indeed be unlikely to produce any detectable effect against the scale of other factors creating the consistent atmospheric ^{14}C signal recorded by plants growing across the southern Aaegean (Thera, Crete, Rhodes, Miletos in western Anatolia).

If we consider the possibilities of major effects, then no sapropel event is known at this time in the east Mediterranean (last one known is several thousand years earlier) and a significant up-welling event in the mid second millennium BC seems implausible (Manning *et al.* 2002c:744), and, since the situation must apply both in the 50–100 years before and c.100 years after the Thera eruption, localised and time-varying volcanic ^{14}C depleted (old) carbon dioxide from this eruption (or leaked in the lead up), or from other volcanic sources, cannot easily offer an explanation. Thera is not the type of volcano that continuously produces a very large diffuse CO_2 output (contrast Etna and some others: e.g. Allard *et al.* 1991) – and there is no evidence from modern comparanda for a significant volcanic effect that *consistently* covers many thousands of square kilometres at crop/tree leaf height (as required to account for consistent LMIA radiocarbon ages on plant and wood samples ranging from Thera to Rhodes and to Miletos: for full Oxford data, see Bronk Ramsey *et al.* 2004a; for partial preliminary data see Manning *et al.* 2002b; Manning and Bronk Ramsey 2003). Instances of volcanic or earthquake (etc.) related CO_2 emissions tend instead to be highly time variable (as clear from assorted examples cited by Wiener, this volume; 2003:383). Nor do the recent VDL or other Akrotiri samples exhibit the likely signs of such an effect operating (for which, see e.g. Bruns *et al.* 1980; Hubberten *et al.* 1990; Calderoni and Turi 1998; Pasquier-Cardin *et al.* 1999) – where very significant old-age biases can be observed close to a vent or hot spring source (and low to the ground only) and these then fall off quickly to more or less zero with distance (a few hundred meters), or are highly time and/or location variable (even close to a source). As Olsson (1987:22) concludes of work on Iceland: 'it is generally very difficult to see any effect except on the lava from 1973 on Heimaey and very close to some hot springs'. When we look at the more recent LMIA radiocarbon data from Thera from secure contexts with standard pre-treatment and processing and correction, we do not see any samples with massively old ages, and no pattern of fall-off from these to 'normal' ages. Instead, the data are pretty consistent over all samples, and across different crop types and a twig (see above and Figure 1 – the ability to be able satisfactorily to combine the whole set of 23 data on normally pretreated/processed short-lived samples from the very final VDL horizon demonstrates this point clearly). The palaeobotanical research of Sarpaki (1990) also indicates that the samples dated probably came from different fields, and so cannot all have been affected by a localised consistent effect (this, for Thera, and then the consistent LMIA data for Rhodes and Miletos, and data from Crete from LMIB also supporting a raising of the conventional low chronology, all indicate that no local ^{14}C depleted CO_2 source on or near Thera feeding either the atmosphere or root systems can satisfactorily account for the overall pattern of consistent data – suggestions to the contrary are special pleading with no positive evidence as of the present time). The usually (or on average) windy nature of Santorini (and so rapid atmospheric mixing), and the likelihood of some (most) crops growing on the non-caldera surfaces/slopes of the island (and so not in an imagined trapped reservoir of old carbon dioxide inside the caldera), further supports such a view. One may also note, for example, that radiocarbon satisfactorily dates the archaeology at Pompeii buried by the eruption of Vesuvius (Vogel *et al.* 1990; Nelson *et al*. 1990:202). Whatever 'effect' is proposed, it is also clear that no such 'effect' applied in the Levant (and so whole east Mediterranean?) generally, since, for example, good quality data from Tell Es-Sultan (Jericho) for the late MBA provide entirely satisfactory data (Bruins and Van der Plicht 1995). But, at the same time, it also seems that it is not only in the Aegean that we find earlier dates than expected from the conventional archaeological chronology in the 17th–16th century BC; it seems that a key site at the other end of some of the synchronisation discussions might also prove interesting in this regard. Kutschera *et al.* (2004) report radiocarbon data from the Second Intermediate Period (SIP) strata at Tell el-Dab'a, which, on average, suggest dates rather earlier than those assigned by the excavator, and quite compatible with some suggestions of a somewhat earlier dating of some of these strata and with the general high Aegean/ Cypriot chronology synthesis (e.g. Manning 1999). More data are needed to investigate the Tell el-Dabᶜa case.

The other approach is to accept that perhaps the radiocarbon data are more or less accurate (whatever minor problems and errors may apply). In the case of the dating of the Thera VDL, roughly the same age determination give or take a few decades has been found repeatedly over many years by several teams following standard pre-treatment procedures on relevant short-lived samples: see Figure 7 (and compare Figure 1) – with the recent work yielding much increased precision. All the data offer relatively similar outcomes. Thus the finding appears relatively robust within errors. If we regard these radiocarbon data as roughly accurate, then it is the conventional interpretation of the very limited archaeological-historical data for Aegean-Egyptian correlations c.1700–1500BC which needs some reconsideration. This would be along the lines proposed by the high chronology, or at least the compromise high chronology.

If we do follow this line and accept the radiocarbon evidence, then what scale of up-dating is at issue? The 'conventional' chronology has rather fragmented of late and has become a bit of a moving target. This alone almost demonstrates the value of the high chronology challenge, as new better positions are being developed; it also reveals the lack of evidential strength to the original standard consensus (much though it lasted for most of the 20th century AD). For example, when does the conventional chronology believe that the eruption of Thera occurred? I review just a handful of publications by prominent scholars:

Bietak (1997:125)	1515–1467BC
Bietak (2003)	early 18th Dynasty, before Tuthmosis III, so within c.1540–1479BC
Driessen and Macdonald (1997)	1550/1530BC
Eriksson (1992:219)	1460BC; c.1479BC in SCIEM2000 Euro-Conference 2 paper May 2003
Warren (1984)	1500BC
Warren (1999:900–902)	1520BC
Warren (2001:116)	1520/1500BC
Warren and Hankey (1989:215)	1535/25BC or 1560/50BC
Wiener (2003:363)	1560–1480BC, some preferring before 1530BC, perhaps 1560–1550BC (p.364 n.4)

A full 93 years is encompassed within the dates quoted, or 80 years if one takes Wiener's starting point. This is hardly a precise date. It is also important to note that some already seem prepared to concede one of the original starting points of the 'high' chronology challenge: that the eruption perhaps occurred before the 18th Dynasty began, in the SIP. A date c.1560–1530BC can just about be argued to be potentially compatible with the radiocarbon evidence – it is unlikely/less likely, but possible (see Figures 1, 7). A date from c.1520BC or later cannot be accommodated with the radiocarbon data – even at a considerable push.

A date of c.1560–50BC is some 30–60 years earlier than the Warren conventional position. It is just 38–48 years short of the lower limit of the 95% confidence most likely date range found for the Thera VDL in the study of Bronk Ramsey *et al.* (2004a). In other words, it is about halfway – and has to accept most of the high chronology synthesis concerning LMIA = SIP, LCIA (1 and 2) = SIP. Manning (1999) referred to this position as the compromise early chronology, Wiener (2003:364 and n. 4) instead claims it as the 'modified Aegean short chronology'. Critically, this position *must* adopt some version of the high chronology arguments about LCI regionalism and differences in trading partners in order to explain a pre 1560/50BC WSI bowl on Thera and yet no WSI at Tell el-Dab^ca (etc.) until early 18th Dynasty contexts (and generally Tuthmosid ones – and note that the most recent assessments of the material at Tell el-Dabᶜa tend to place the appearance of WSI, BRI, etc. *later*, no longer initial 18th Dynasty as in e.g. Bietak 2000:fig.1, but now down into the Tuthmosid and mainly Tuthmosis III period: Bietak 2003:24, fig.1). Please note. Thus the compromise early/modified short chronology will end up employing most of the arguments of the 'high' chronology synthesis, especially with regard to Cyprus (i.e. as proposed first in Manning 1999; developed in Manning 2001; Manning *et al.* 2002a). However, this position has the big advantage of offering a much easier explanation for the early WSI bowl on Thera and the early WSI at late MBA Tell el-ᶜAjjul, versus the general earlier 18th Dynasty finds of (mature) WSI, since only a few to several decades are at issue, and not about a century. As Bergoffen (2001a:146) notes, it of course seems inherently unlikely that early WSI pottery was in use at Tell el-ᶜAjjul decades before its appearance at Tell el-Dabᶜa. Bietak (2003:25–27) notes and dwells on this same 'gap' problem – the compromise early chronology seems to offer a way around – but from the perspective developed by the 'high' chronology synthesis.

The weakness of the compromise early chronology is that no actual suite of evidence as a whole really supports this specific date – it is just a compromise trying to best accommodate everything. In contrast, we do have some quite significant radiocarbon evidence more clearly supporting a date within the

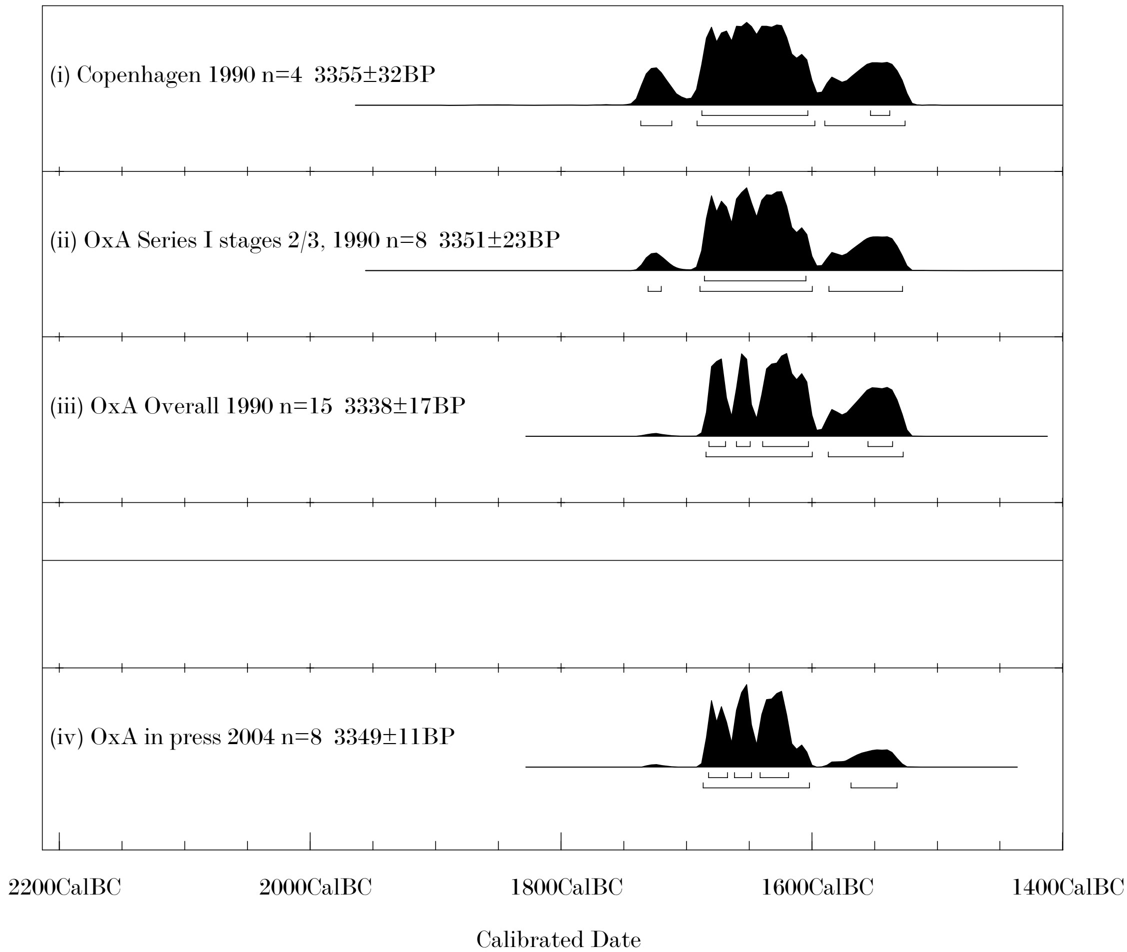

Fig. 7 Comparison of the calibrated age probabilities for each of the weighted averages on a variety of published or in press Akrotiri VDL data on short-lived samples from standard laboratory pre-treatment and correction regimes (except (iii) which includes the attempt by Oxford to replicate the 'novel' Simon Fraser protocol – which they could not – but nonetheless includes Oxford's Series II 'residue' data as the best overall estimate reached by Housley *et al.* 1990). For data see Friedrich *et al.* (1990), Housley *et al.* (1990); Bronk Ramsey *et al.* 2004). The in progress and not yet in press VERA data included in Figure 1 are not included here. Calibration using OxCal 3.9 (Bronk Ramsey 1995; 2001 and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The lines under the distributions indicate the (upper lines) 1SD and (lower lines) 2SD calibrated ranges

bounds 1663–1599BC. Note: The final published data in Bronk Ramsey *et al.* 2004a in fact yielded an average age of 3350±10 BP – versus the 3349±11 BP figure used above. There is no noticable difference.

The major gap in the radiocarbon data at present is the lack of any earlier LMIB datasets (just as correlation evidence from Egypt for LMIB is in fact for the classic *late* LMIB material known from the close of LMIB destructions on Crete). Such urgently needed earlier LMIB (or early to mid LHIIA) data now offer a critical test. Do they lie (also) in the late 16th century BC running into the current LMIB destruction data (and so indicating a relatively short, or later-dating, LMIB period), or do they lie in the earlier to mid 16th century BC, consistent with and requiring the high chronology? We need such data to find out: short-lived samples from closely defined contexts of early/earlier LMIB. Let us hope that excavators in the region can supply some suitable contexts and samples therefrom (recognising such material is a problem – what is needed are some overall LMIB stratigraphic sequences where samples may be taken from early through late to build up a chronological range for the overall period – one notes for example the reports of up to three building/stratigraphic phases within LMIB in House X (room 2) at Kommos, and similar reports of several building phases within LMIB at Palaikastro and Pseira (Manning 1999:334 with references).

5. Aegean Archaeological Evidence and Egypt for the LMIA–IB/LHIIA periods

Despite a lot of activity and 15 years of time, not that much has really changed in the archaeological situation since Warren and Hankey (1989); much of the potential ambiguity (e.g. Betancourt 1990) remains. It was subsequently suggested that finds of LHIIA or LMIB ceramics in earlier 18th Dynasty contexts in Egypt seem, plausibly, to require some revision of the Warren and Hankey chronology (and see already Warren and Hankey 1989 'postscript' p. 215). In particular: the LHIIA items from Tomb NE 1 near the Teti Pyramid at Saqqara were suggested to date from a burial during the reign of Amenhotep I *or* earlier (e.g. during the reign of Ahmose) (see for discussion Manning 1999:204; for correction of dating of the alabastron and cup to 'classic LH II A, contemporary with LM I B', see Macdonald 2001:530). Thus a date for deposition of classic LHIIA no later than c.1494BC and perhaps anywhere c.1550–1494BC was required. The LMI rim from Kom Rabiᶜa could be argued to offer similar evidence (Manning 1999:203–204 and references). A perhaps early LHIIA squat alabastron from Gurob from an early 18th Dynasty context might also be called into play (Manning 1999:206 with references; Macdonald 2001:529 for early LHIIA suggestion). Wiener (2003:364 n.4) notes this evidence and observes that these archaeological data are hard to reconcile with suggestions of a date for the Thera eruption c.1500BC, let alone one even later. Another relevant item is the early BRI jug from Saqqara (unnumbered intrusive burial in Mastaba 3507), for which Merrillees (2001a) argues that 'a date of around 1525 B.C. for the deposit cannot be too far wrong' – i.e. very early 18th Dynasty. Reviewing the jug and associated typology, Merrillees argues that this perhaps places the LCIA2–LCIB transition around 1525 BC *or before* – Merrillees (p. 27) notes, as he has many times, that the issue of unknowns in point of production for the type within its period of production (from LCIA2 to LCIIA1 – Merrillees suggests LCIA2 for this vessel) *and* possible time-lag in deposition, 'gives all such chronological calculations an intrinsically conservative character, which, if any adjustment were to be made, would require the figures to be raised, not lowered'. A few other finds of BR also seem to confirm that BRI began to be deposited in Egypt *no later than* the very early 18th Dynasty (Eriksson 2001:58 with references – in a paper mainly arguing for a low chronology). The much debated but unpublished (early) BRI juglet said to be from a SIP context at Kom Rabiʿa may offer even slightly earlier *terminus ante quem* evidence for a point in LCIA2 (at a minimum) in the SIP (Merrillees 2001a:27–28 and references; but cf. Eriksson 2001:56 and 58).

But against such evidence and analyses/interpretations, Aston (2003:141–143) finds little or less solid evidence to support the necessity of early dates for the Saqqara and Kom Rabiᶜa contexts, and indicates that such material could also date down to the reign of Tuthmosis III. Of course, the paper of Aston leaves one wondering what the contexts and ceramics of Ahmose to Tuthmosis II look like, as most contexts/material previously held to be early seem to be being pushed down into the Tuthmosid period – *some* material one feels must be early 18th Dynasty (which, as Aston notes p.140, 'is the least well known of the four defined pottery phases, since very few tombs can be unequivocally dated to this period'). Thus we are rather left with ambiguity – the evidence could seem to be potentially compatible with a higher chronology taken in one light (and the LMIB radiocarbon evidence); but it can also be argued to be compatible with the low chronology (subject to the observation, when reading Aston's study, that we do need to reflect on the fact that some early 18th Dynasty material has to be found for the c.50 odd years before the accession of Tuthmosis III!). Aston (2003:145) observes, as others have before, that the reign of Tuthmosis III marks the change from LMIB to LHII imports (and by no later than late in his reign to LHIIB imports: Warren and Hankey 1989:145–146; Macdonald 2001:530 who revises the Lachish stemmed cup to LHIIB) – where within his reign is of course important, but not clear on current data.[4]

[4] The interpretation of the wall paintings at Thebes showing Keftiu (Minoans) is problematic. Manning (1999:209–220) offered an attempt at a 'high' chronology interpretation, but Macdonald (2001:529) makes some fair criticisms. How to interpret the transmission of the item depictions and clothing styles is not clear. The early Tuthmosis III Senmut tomb undeniably has LMIA/LHI style representations (and broadly the Senmut and Useramun tombs depict types that could date from here to the end of LMIB/LHIIA). We then have later links for the late Tuthmosis III paintings in the Rekhmire and Menkheperraseneb tombs. Some see these as LHIIB/LMII to LMIIIA/ LHIIIA, others have pointed to LMII–IIIA1 design elements (and Warren 1998 was prepared to consider LMIIIA1 as starting by the end of the reign of Tuthmosis III) (in general for diagnosis of the objects depicted, see Matthäus 1995:esp. 184–186). Macdonald's cautions about transference from one medium to another are relevant, but we have little data to work with to control this one way or the other. The likely use of copybooks by the artists further introduces possible time delay (more plausible for the earlier paintings – whereas the re-painting in the Rekhmire

Late Minoan IA remains notably *not* dated by any finds in Egypt, and there has been little evidence recovered for any direct contacts between Crete and Egypt at this time. Some fairly general stylistic linkages have been noted, such as suggested comparisons of Egyptian 'imitations' of Minoan rhyta (KOEHL 2000), but these lack close chronological control. It is not actually known that they are imitations – and the decorations are not Aegean – nor, if they are imitations, what they were imitating or how (KOEHL 2000:97). Time-lags and other issues involved are unknown. Koehl proposes an early LMIA style for SIP/early 18th Dynasty examples and LMIA comparanda generally for examples through to the mid-18th Dynasty (p.96) – but we potentially see mature LHIIA and LMIB material current also in the early or earlier 18th Dynasty in Egypt (above); at present there appears to be limited useful/precise chronological value to be derived from the rhyta. The Tell el-Dabᶜa wall paintings are discussed below (see Section 9 below).

An excellent recent review of Kamares imports to Egypt and the associated chronological issues finds a chronology entirely compatible with, and rather in support of, the Aegean/Cypriot 'high' chronologies for the mid-second millennium BC (MERRILLEES 2003). It is wrong to imagine that the MBA evidence does not permit the Aegean/Cypriot 'high' LB1 dates (and important indeed to consider the evidence of the Middle Cypriot/Middle Minoan chronologies: a point made by MERRILLEES 2001a:29; 2003); similarly, the Aegean 'high' chronology rejoins the 'conventional' chronology around the close of the 15th century BC. The question and issue is the dating and cultural synchronisms for LMIA and IB, and Late Cypriot IA and IB, and how these relate to and inform the wider east Mediterranean picture. The evidence here is not clear-cut from the archaeology alone (contrast the 14th–13th centuries BC) – that is why we are now in the third decade of vigorous discussion surrounding the dating and synchronisms of the LMI and LCI periods.

For the high chronology to be possible, the LMIB period must be long.[5] The LCIA2 period must be likewise. An obvious problem is that earlier LMIB would be placed in the late Hyksos period through to the start of the 18th Dynasty (one can argue that the archaeological evidence could be compatible with mature LMIB and early to mature LHIIA and LCIB occurring in Egypt from early in the 18th Dynasty (and not just from about the reign of Tuthmosis III)) – but there is little positive evidence for this (the debatable Abydos sherd and tomb excepted – see MANNING 1999:204 and references). But, in reverse, as BETANCOURT (1998:292) reminds us, 'one must remember that the LMIB pottery we have as intact vases in Crete is from the very end of the period, at its destruction'. It is these very late LMIB assemblages which offer the comparanda for most of the known LMIB items from Egypt (and in support we may note that this late LMIB is in fact being replaced by mature LHIIA and LHIIB Mainland products in many cases) – thus one could argue that the LMIB items from Egypt be regarded as mature/late LMIB (MANNING 1995:220–221; a point agreed upon by MOUNTJOY 1999:16). If so, we are at present simply lacking material from the earlier part of the overall LMIB/LHIIA phases in the available synchronisa-

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tomb implies greater immediacy and need to revise the images of Keftiu – and the choice of the different clothing style, kilts, and their decoration, is perhaps relevant in this regard and might be linked to a real embassy or similar to the Egyptian court that informed both this and the Menkeherraseneb tomb paintings). When and how long to apply for such time-lags is of course unknown. The conclusion at present is probably that these data as they stand could be argued to conform to either high, compromise, or low chronologies.

[5] WIENER (2003:394 n.161) raises as a problem for the high Aegean chronology the issue of the length of the Shaft Grave period at Mycenae, suggesting that this covers 'approximately three generations' on the basis of studies then cited by him. I am afraid I fail to see the evidence which supports this view, and why it affects the high chronology case. The Shaft Graves at Mycenae cover the MHIIIA to LHIB periods (DIETZ 1997:fig. 1 usefully compares the two recent chronologies of the graves) – the latter runs to around and perhaps just after the eruption of Thera (it is possible to argue that LHIIA starts pretty much around or shortly after the eruption: MACDONALD 2001:527; MANNING 1999:17, 19, fig. 16). DIETZ (1991; 1997), for example, in detailed studies of the chronology of the Shaft Grave assemblages, has been happy enough to work with the high chronology. No total contradiction is evident from the Shaft Grave material itself. What *is* the issue, as noted in the main text, is the length of the LMIB and LHIIA phases. Can the earlier through mid parts of these phases stretch to the start of the 18th Dynasty? This is the issue or problem for the high chronology. Can the overall (start to end) LMIB/LHIIA periods be around or a little over 100 years in length? We must instigate research to try to find out. At present, I can merely quote a leading Minoan ceramic specialist who states that the length of the LMIB period 'must surely be well over a century' (BETANCOURT 1998:293). The compromise high chronology/modified Aegean short chronology of course avoids this issue/problem.

tion matrix (and again one may note the scarcity of agreed upon very early 18th Dynasty contexts in Egypt [see above], and the quite sharp change in the character of the Hyksos versus New Kingdom levels at Tell el-Dab^{c}a: Maguire 1995:54). On Crete, the evidence would seem to indicate that LMIB is a long phase (Betancourt 1998:293; Manning 1999: 330–335; only the classic/late LMIB material from the last part of the phase defines the short LMIB period of Popham, e.g. 1990). For Cyprus, we see classic/mature LCIB material deposited mainly from around the reign of Tuthmosis III, but perhaps as early as Amenhotep I (Aston 2003:140, 145 and references). Earlier 18th Dynasty imports of Black Lustrous and WPVI *could* be earlier LCIB (they die out on Cyprus during the course of LCIB) presumably from the east of Cyprus where WPVI lingers on longest, but they could also be LCIA (presumably LCIA2). The former with a decent length for the LCIA2 phase could be compatible with the high chronology, the latter favours the compromise high chronology or the low chronology.

6. The White Slip I Discussion

Here there has been some significant misunderstanding of the Cypriot data and literature, although progress has been made in recent years. The two key issues are:

1. The regional-temporal development processes of LCI as outlined by Merrillees (1971; see also discussions in Baurain 1984) and refined since (see further discussion in Manning *et al.* 2002a). This point is now increasingly noted and recognised (e.g. Bietak 2003) – debate is starting to move to the temporal scale and impact of this situation – versus ignoring it. This is positive. The regionalism of LCI Cyprus only comes to an end, and island-wide approximate homogeneity of (now all) mature LCI assemblages occurs (with, e.g., WSI of the framed wavy line style, as found all over Cyprus), by the end of the LCIB period – and not from the start of LCIB.

2. The tendency to ignore the actual range of production/occurrences of Cypriot wares (Åström 1972:675–705) and the application just of an initial date to foreign finds – where one might often expect them to in fact derive from later during the ware's lifetime of currency. Merrillees has noted this problem several times (e.g. Merrillees in Karageorghis 2001a:159, 217–218; Merrillees 2002:2, 5). Thus PWS appears in LCIA1 and then continues in LCIA2 and into LCIB (where it tails out). There is no reason for a given foreign export example or group to always be LCIA1; they could very well be LCIA2. WSI appears from the beginning of LCIA2 and then is dominant/standard in LCIB. There is some overlap with WSII. The length of this overlap and whether to see it as LCIB/IIA, or earlier LCIIA, is not clear. Appearances of earlier style WSI might thus be likely to be LCIA1/2 or LCIA2, and mature WSI most likely LCIB. PBR also occurs in both LCIA2 and into LCIB. Base Ring (BR) I occurs from LCIA2 and right through LCIIA and perhaps later for some classes like the jugs employed in funerary contexts (Manning and Monks 1998). There is significant overlap with BRII.

The value and interpretations of certain patterns observed in finds are of limited value without consideration of the appropriate Cypriot context, and the possible ranges involved.

^{c}Ezbet Helmi/Tell el-Dab^{c}a and the first appearance of White Slip (WS) I issue

On current evidence PWS makes its appearance in Stratum D/2 and continues through to the very start of Stratum C/2, and WSI makes its first stratigraphically securely attested appearance at 'Ezbet Helmi/Tell el-Dab^{c}a in the Tuthmosid period (Stratum C/3) and stops by the end of this phase (end of Tuthmosis III) Bietak (2003:24, fig. 1, 27). Very little PWS/WSI overlap is noted (c.5 years in Bietak 2003:24, fig. 1). But this should be the whole of the LCIA2 period and at least a significant part of the LCIB period! Five years cannot represent the overlap attested on Cyprus (LCIA2 to during LCIB). Even on the ultra-short Åström (1972:762) chronology, this represents a significant span within the total time estimated as 115 to 135 years for LCIA2 and LCIB together.

Thus it is immediately clear that the sequence at Tell el-Dab^{c}a does not represent the entirety of the relevant Cypriot record, or has specific biasing factors that lead to over-compression of the Cypriot import sequence. The quite sharp break between the late Hyksos levels with MC to LCIA1(–LCIA2) imports, versus the New Kingdom levels and imports, has been noted several times (e.g. Maguire 1995:54), and may have some bearing on this issue. Similarly-WPVI occurs in LCIA1, LCIA2, and LCIB on Cyprus; yet it too is shown with a c.5 year overlap with WSI and RLWM at Tell el-Dab^{c}a. Again we seem to miss almost the entirety of the LCIA2 and LCIB overlap in this record. Similar problems exist earlier also. White Painted (WP) Pendent Line Style (PLS) occurs in MCIII and right through LCIA. It occurs at Tell el-Dab^{c}a from Stratum F and ceases after the middle of Stratum D/3 (Merrillees

2002:3; Bietak 2003:24 Fig.1 shows it through to the end of Stratum D/3). Other evidence from Egypt suggests a similar range from the 18th century BC through to no later than the end of the Hyksos period (Merrillees 2002:4). This implies that LCIA2 was mainly if not all before the end of the Hyksos period also, with LCIA1 before this, and then MCIII. We are left with a general scheme of MCIII from the mid 18th through mid–17th century BC, and LCIA (1 and 2) from the mid-17th through mid-16th century BC (Merrillees 2002). Indeed this is exactly the consensus Cypriot chronology for the Cyprus Museum recently circulated by Dr. Sophocles Hadjisavvas: LCIA c.1650–1550BC, LCIB c.1550–1450BC. Bietak (2003:24, fig. 1), in contrast, has LCIA2 entirely in the 18th Dynasty and ending between about 1500 to 1460BC. But we lack a variety of expected LCIA2 evidence in the New Kingdom strata.

The situation with WPPLS and WP Cross Line Style (CLS) versus WPV at Tell el-Dab^{c}a further illustrates the problems. On Cyprus WPPLS appears at the MCII/III transition and WPCLS in MCIII, both then continue right through the LCIA2 period. WPV likewise appears no earlier than the MCII/III transition and is a MCIII and LCIA ware (Manning *et al.* 2002a:152–153 and references). WPPLS, WPCLS and WPV have more or less the same spans on Cyprus. But, at Tell el-Dab^{c}a, finds only partly overlap, with WPV appearing three or more strata *after* the others, and then continuing on for an additional stratum (Bietak 2003:24, fig. 1). We are clearly *not* seeing the entirety of Cypriot production attested. Roughly the first half of the WPPLS and WPCLS representation is plausibly MCIII and perhaps the second half is LCIA. In which case, more or less all the WPV must be assumed to be LCIA, since it only overlaps with the *final* ? to ¼ of WPPLS and WPCLS presence, with most of the MCIII production of this ware not (yet) attested at Tell el-Daba^{c}a (only recently did WPV even get extended back into (mid) Stratum E/1: Forstner-Müller 2003:170 and n. 23, fig. 6; cf. Manning *et al.* 2002a:153). This in turn suggests a close for the LCIA2 period around the end of the Hyksos period as represented by finds overseas. Allowing for even some time-lag effect, the real date on Cyprus can only be older (whether by a very short interval or possibly longer). Again the evidence is consistent with and/or supports the approximate Cypriot chronology of Merrillees (1977; 1992; 2002; 2003).

The implication of such observations is that the appearance of WSI at Tell el-Dab^{c}a does not date the first appearance of WSI in Cyprus (or elsewhere – and it seems to occur in the MBIII period at Tell el-^{c}Ajjul: Bergoffen 2001a).

Stylistic date of the Thera WSI bowl?

At present much of the 'high' versus 'low' chronology debate hinges on a now *lost* WSI bowl from Thera (Merrillees 2001b). The irony is impressive. Bietak (2003) argues that this bowl is not early style WSI but instead states that 'on the contrary, there are other assessments putting this bowl late in the WSI development' (p. 26). This claim shows a mistaken understanding of the evidence and previous analyses. Bietak correctly cites Merrillees (2001b:93) as the best recent study of the Theran bowl, but then significantly misrepresents Merrillees' views. Merrillees (2001b) suggests a stylistic date of LCIA–LCIB transition for the Thera bowl. Merrillees sees WSI production covering Late Cypriot (LC) IA2 and *all of* LCIB (compare Åström 1972:700 Chart). Thus an LCIA–LCIB transition date is in the *earlier* phase of WSI production (maybe broadly about ⅓ the way through). Merrillees' text (2001b:93) clearly confirms such an 'earlier' view when he commends Niemeier's (1990:122) observation of 'some Proto White Slip features, such as Popham's (1962, 283) 'rope' pattern with oblique cross lines, whereas for developed White Slip I Popham's 'ladder' pattern with cross lines at right angles is characteristic'. (The reader may indeed note that 30 years ago Merrillees 1974:6 and n. 16 in fact classified the Theran bowl as Proto White Slip – it is undeniably 'early' looking WSI.) Merrillees goes on to note that Niemeier therefore placed the bowl early in the WSI sequence and Merrillees writes 'Niemeier has identified the crucial element in the decoration which enables it to be placed ... in its proper chronological horizon …' (Merrillees 2001b:93).

Elsewhere (Manning *et al.* 2002a:98–106, 160–162; Manning 1999:153–157), I have argued that the Theran bowl is 'early' style WSI (so earlier WSI phase of production) – very much consistent with Merrillees (2001b). The analysis of Bergoffen (2002:34 n. 45, 36 n. 55) reaches a similar position. We can quickly review the salient issues and note a couple more with regard to the Thera WSI bowl:

(i) The decoration is early style WSI (Manning *et al.* 2002a:160–162) with the 'rope' pattern oblique cross lines in particular arguing for placement early in the WSI sequence (Merrillees 2001b:93);

(ii) The paired vertical lozenge chains are shown as *not* joining and with quite large irregular dots on either side (i.e. more PWS in inspiration than classic WSI) (Merrillees 2001b:fig. 2);

(iii) The decoration exhibits none of the features of later or mature WSI as typical of the LCIB period. It lacks the space and clean linear styling of classic/mature WSI and instead has early features only, including single pendant lozenge chains framed by single vertical lines. The best comparanda come from contexts most likely dated LCIA, such as *Toumba tou Skourou* Tomb VI (VERMEULE and WOLSKY 1990:317 TVI.25, fig. 46 – a context most likely to be LCIA since the rest of the assemblage is early LCI and there are no classic WSI items). And, in contrast, contexts of mainly LCIB (and onwards) date have lots of mature WSI but none of the 'rope' lattice early style WSI – with the tombs at Stephania and Ayia Irini not that far from *Toumba tou Skourou* nicely illustrating this clear distinction (noted first by PADGETT in VERMEULE and WOLSKY 1990:374). This distinct, later, material very often features the two parallel line styles (framed … styles, into metope style) (HENNESSY 1963; PECORELLA 1977). The association with LHIIA vessels at Ayia Irini (Tomb 3 where all WSI bowls are of the classic style – nos. 24, 37, 38, 61, 107, 110, 126, 127, 128 and 129 – and there are also two LHIIA imports – nos. 16 and 29: PECORELLA 1977) may also indicate a likely approximate LHIIA/LMIB–LCIB linkage, just as the LMIA and Late Cycladic finds at *Toumba tou Skourou* indicate a LMIA–LCIA linkage (VERMEULE and WOLSKY 1990:381–383; CADOGAN 1990:95).[6]

(vi) The Thera bowl is said to have had a brown fabric (MERRILLEES 2001b:90, 93). This is not distinctive with regard to provenance, as fabrics for WSI vary quite widely, but does run counter a likely provenance in southwest Cyprus, such as around Palaepaphos *Teratsoudhia*, where fabrics are typically whitish, yellowish, grey, dark grey and to reddish (KARAGEORGHIS 1990 – the few WSI sherds with 'brownish hard gritty clay', e.g. pl.VII (i), p.42, do not come from bowls offering stylistic comparisons to the Thera one). In contrast, among other possible loci, *Toumba tou Skourou* (VERMEULE and WOLSKY 1990) provides a number of brown or brownish fabric WSI bowls (e.g. T I.105, 505, 512, 522 532, 533, 544, 546, 547, 519, 537, 531, 521, 522, 543, Lo II.25).

(v) The Theran bowl is distinct in style and fabric from the mature WSI bowl (in special white fabric WSI) known from Phylakopi (contra BIETAK 2003:27–28). I argue it is also separate in time. One is LCIA, the other LCIB. The Phylakopi bowl quite possibly did come from southern and probably southwestern Cyprus (e.g. *Teratsoudhia* as KARAGEORGHIS 1990:57 n. 28 has suggested).

Archaeological date/context on Cyprus?

This is less clear-cut as we lack good LCIA to LCIB stratigraphic data from most sites, and rely mainly on evidence from tombs used often in both periods. Thus one can offer a 'higher' view by associating the early style WSI with only the other earlier material (i.e. LCIA), or a 'lower' view by suggesting association also with later LCIB material. One cannot on current evidence prove the former position; one can only argue that it makes sense in terms of the observed PWS-WSI evolution, and that early WSI occurs in contexts that can most plausibly be dated LCIA only, whereas classic/mature WSI occurs either in contexts covering LCIA and IB or just IB. There is of course overlap. Thus PWS occurs in LCIA1 through LCIB1, early style WSI in LCIA2 through IB, and classic/mature WSI mainly in LCIB.

The time problem?

BIETAK (2003:25–27) argues that, while he partly accepts the regional development of LCI Cyprus scenario, he cannot see how this provides more than a few years (10 or 20, maximum 25 years), and thus he argues that one cannot have WSI on Thera in the 17th century BC *and* first showing up in Egypt in the Tuthmosid period.

Moreover, in recent years, the Tell el-Dab^{c}a evidence has actually been *widening* this gap, or time problem, as the appearances of WSI and BRI have been if anything pushed later at the site. Thus whereas in BIETAK (2000:fig. 1; BIETAK 2001:fig. 1) WSI and BRI were shown as starting from the beginning of the 18th Dynasty and PWS was shown as stopping at the end of the Hyksos period, now in BIETAK (2003:24, fig. 1) PWS is shown as continuing well into the 18th Dynasty to the start of the reign of Tuthmosis III (into Stratum D/1: p. 27), and WSI and BRI have been pushed back to the reign of Tuthmosis III (BRI perhaps starting about Tuth-

[6] This picture is reinforced by early LCIA finds of Aegean material at Maroni: CADOGAN *et al.* (2001) – MACDONALD (2001:529) suggests that the CADOGAN *et al.* (2001:fig. 3) sherd could in fact be MMIIIB–LMIA transition or even LMIB but for context – point accepted, but the sherd still offers consonant data for a general LMIA–LCIA linkage – since the context is early LCIA and a MMIIIB–LMIA transition to earlier LMIA date would be perfectly acceptable.

mosis I: HEIN 2001:242–243). The 'problem' is therefore being made more and more acute. But the evidence is also very problematic as noted above. If the PWS represents the full range on Cyprus (as determined in ÅSTRÖM 1972:675–682, 700–701 chart), then earlier (to mid) LCIB ends (PWS declines) around the end of Stratum D/1 or around the accession of Tuthmosis III. In which case WSI and BRI of the LCIA2 and earlier LCIB phases is missing at the site – as is the substantial (LCIA2 and earlier LCIB) overlap of PWS and WSI. In turn, the LCIA2/IB transition is at an unknown earlier time. If the PWS is argued not to include material from the very end of the ware's history on Cyprus, then the problem is worse. It seems unlikely too large a time delay in transmission is involved in the later material, since the evidence comprises a reasonable number of samples (BIETAK 2003:27). Is there an answer? Did WSI come relatively late as a dominant fashion to the main export area to Dab^{c}a – only *during* (even later) LCIB? One might wonder about the relevance of the remarks about LCI Cyprus and its exports to Egypt made over 30 years ago by MERRILLEES (1971) – they seem to have some relevance in view of the fact that most of the relevant MC and earlier LC Cypriot imports to Tell el-Dab^{c}a seem to come from eastern Cyprus (MANNING *et al.* 2002a:103 and n. 15 and references there – odd possible exceptions notwithstanding: BIETAK 2003:27 and n. 43).

This apparent 'time gap' is probably the biggest problem at present, and divide, between the high and low chronologies. The 17th century BC date for at least initial early style WSI stems from the radiocarbon evidence (Section 4 above). *If* we did not have the radiocarbon data, then one could more easily accommodate the LCIA2 to earlier LCIB periods in the 16th century BC, consistent with the analyses of e.g. MERRILLEES (1977; 1992; 2002; 2003); KEMP and MERRILLEES (1980). And the lengths of apparent time involved, and gaps, etc., would be reduced.

But I submit that current review of both the archaeological and scientific evidence indicates that the time problem is less extreme than envisaged by some scholars (while still an issue).

BIETAK (2003) argues that the eruption of Thera occurred in the early 18th Dynasty, probably before the reign of Tuthmosis III – i.e. within the period c.1540–1479BC. And he notes the finds of Theran pumice from the Tuthmosid period and supports a linkage (BIETAK 2003:28), so by implication a date closer to c.1494–1479BC. One view of the archaeological evidence indicates that the subsequent LHIIA and LMIB periods were perhaps already underway before the Tuthmosid period and possibly as early as the reigns of Ahmose or Amenhotep I (see above Section 5). If this turned out to be correct, then there is no reason at all to regard these deposits in Egypt as dating the *start* of either LHIIA or LMIB in the Aegean. These data would instead offer *termini ante quos* – with the length of the *ant*e being unknown, but quite plausibly of several decades duration on any understanding (and potentially more). Thus an end for LMIA later than about the very start of the 18th Dynasty (18th Dynasty begins c.1550/1540BC) appears difficult from this view of the archaeological evidence, and this transition could well be several decades earlier (we simply have no robust archaeological evidence to constrain it in the upwards direction). But another view pushes several of these contexts/data down and probably also into the reign of Tuthmosis III, and so offers a synthesis entirely compatible with an early 18th Dynasty Thera eruption.

Is there any archaeological evidence to question the compact, short view? I return to the problem of the Cypriot sequence at Tell el-Dab^{c}a discussed above in this Section. We seem to have some gaps or compressions or biases; LCIA2 plausibly ends by the close of the Hyksos period, and there is thus the question of what happens in the 18th Dynasty until Tuthmosis III. Missing or unrecognised earlier LCIB and some LCIA2? Placing LCIA2 in the SIP as the evidence appears to support (also MERRILLEES 1992; 2002) would make such a date also required for the early WSI bowl on Thera (and the early WSI in a likely/possible MBIII context at Tell el-^{c}Ajjul (BERGOFFEN 2001a), and also BRI in a similar context at the same site (BERGOFFEN 2001b), can be seen as compatible). In turn, given LCIA–LMIA linkages, this would suggest a similar SIP date for the eruption. Whether it is late SIP (i.e. mid-16th century BC) as the compromise early chronology/modified Aegean short chronology allows, or earlier (late 17th century BC) as the radiocarbon evidence and Aegean 'high' chronology suggests, is then a matter for a choice: between (i) the easiest and – I agree – most plausible construction of the archaeological evidence given no other constraints, or (ii) a view regarding the significant body of radiocarbon data as requiring a way to be found to accommodate a late 17th century BC eruption date (and given that much of the archaeological evidence could be interpreted in a consistent light: e.g. BETANCOURT 1987; 1990; 1998; MANNING 1988; 1999; MERRILLEES 1977; 1992; 2002; 2003).

The important point is that *either* position means that early style WSI as found at Thera must *at a minimum* have been around before c.1540/1530BC – that is already some 50 or 60 years *before* WSI is reported from the C/3 Stratum at Tell el-Dab^ca which is dated to the reign of Tuthmosis III (reign commences c.1479BC). Thus some significant 'gap' is present with no reference at all to radiocarbon evidence (i.e. the 'divide' is *not* just between archaeology and science). This gap can only be explained in terms of, first, the Cypriot regional development process tied into predominant trade associations (MERRILLEES 1971), and, second, in terms of some gaps, biases, or other factors affecting certain periods of Cypriot-Tell el-Dab^ca linkages and artefact deposition. Therefore, even without the radiocarbon evidence, some model similar to the high chronology synthesis as in MANNING (1999); MANNING *et al.* (2002a) = MERRILLEES (1971) LCI Cyprus scenario, is required; with the radiocarbon evidence we definitely need a model like this.

At the other end, the suggested dates of 1628BC or 1645BC or 1650BC for the Thera eruption, based on hypothesises trying to associate tree-ring anomalies or ice-core signals (and the original compelling scenario where the ice-core and tree-ring data seemed able to be linked together in a package consistent with the radiocarbon evidence), have been set aside at present, since none can be demonstrated to have any firm link to Thera (see Sections 1–3 above). Yes, the eruption was very large, and yes its tephra and sulphur output may have been larger than some minimum estimates (MANNING *et al.* 2002a:156–157 and n. 240; Stuart Dunn and Floyd McCoy, pers. comms.), but at the time of writing there is no tie between the eruption of Thera and any given ice-core volcanic signal, nor other absolutely dated environmental proxy such as tree-rings (see Sections 1–3 above). This is a major change in the background mentalité of much 'high' chronology work – the present author very much included. Things change. This also creates more flexibility. The key and only directly relevant scientific dating evidence at present is the significant body of radiocarbon data (see Section 4 above). Previous work indicated a most likely date for the Thera eruption in the 17th century BC (MANNING 1999:232–246; MANNING and BRONK RAMSEY 2003), but with a lesser probability in the mid-16th century BC. This remains the case looking at the data for the Volcanic Destruction Level at Thera in isolation (e.g. see Figures 1 and 7). But, incorporating seriated sets of radiocarbon data from before, around, and after the eruption of Thera, new work in press (BRONK RAMSEY *et al.* 2004a) indicates that a most likely date range at 95% confidence level may be calculated as c.1663–1599BC for the eruption (and a further, future, paper will consider an even larger database of information and will compare this against the new INTCAL04 calibration curve and a range of other calibration datasets – so, as always, the statement just made in the text is provisional pending further information). A date in the upper part of this range clearly adds in quite a few decades; a date lower in this range (late 17th century BC) is not really that far away from one view of the archaeological evidence, or is even compatible with it. The latter would be the much easier situation to accommodate.

For the high chronology, early style WSI would thus have to begin being made in an evolution out of PWS (and with several PWS-style elements in the decoration) by around 1630–1600BC at the latest (and maybe a few decades earlier – earlier half of the radiocarbon range) and an example is exported to pre-eruption Thera (and compatible contemporary return imports from Crete, and Thera itself, have been found on Cyprus, including at a plausible home for early-style WSI at *Toumba tou Skourou*: see above). LCIA would begin (depending on the length of LCIA1) some additional few decades earlier. There are limits here, especially as indicated by the pattern of WPPLS abroad (MANNING 2001:78–80 [but note: now with the Middle or a low-Middle to Low Babylonian chronology applying, see MANNING *et al.* 2001a]; MERRILLEES 2002 [ditto note re-revision to Middle or low Middle to Low Babylonian chronology]) – so somewhere c.1700–1650BC depending on choices and lengths assigned. WSI is then produced through to the reign of Tuthmosis III. LCIA2 would run from the later/late 17th century BC through to the mid-16th century BC, LCIB from the early New Kingdom through into the reign of Tuthmosis III.

On Cyprus, WSI and the 'LCI' package starts in the northwest, it is then found in the west and the southcoast. The east, especially, sees the 'MC' styles linger and really only becomes fully 'LC' by the *end* of LCIB. If the vast majority of Cypriot exports to Egypt and other centres in the southeast Mediterranean derived from eastern Cyprus, as seems to be the case (and seems plausible), then WSI (and other 'LC' wares [except Bichrome Wheelmade Ware, which was an eastern Cypriot invention as first argued by ARTZY *et al.* 1973; ÅSTRÖM 2001:135 – it then spreads to other parts of Cyprus – and here I view the situation slightly differently to KARAGEORGHIS 2001b:144] would mainly not show up until *during* the LCIB period, even late in the LCIB period. This roughly matches the picture at Tell el-Dab^c^a (Manning *et al.* 2002a:148–154). There undoubtedly were exceptions; but we see a reflection of the main pattern. The northwest saw some contacts into the Levant (perhaps MBIII Tell el-^c^Ajjul: Bergoffen 2001a, 2001b), and in reverse there are some Hyksos/MBIII influences/contacts evident from the material at e.g. *Toumba tou Skourou* (see Manning *et al.* 2002a for discussion and references).

The compromise early/modified Aegean short chronology has the archaeological advantage that it does not require such a long LCIA2 phase. It must however either rely on low/very low radiocarbon probabilities (or considerable subjective selectivity in data accepted), or even largely ignore this evidence altogether. Unless some significant problem can be identified with the current radiocarbon evidence (see Section 4 above), this seems a problem at present for the mid-16th century BC date. Of course, the situation will have to be reassessed in 2005 when the new INTCAL04 calibration curve is available, and when all relevant data from the current round of radiocarbon research are available. I am keeping an open mind on a possible re-think here, depending on the final data/calibration situation.

WSII and BRII

Attention recently has concentrated on the appearances of WSI and BRI at Tell el-Dab^c^a and elsewhere in the region. Here the current view is that none appears before the earlier 18th Dynasty, and indeed the majority of finds occur from the reign of Tuthmosis III (so from 1479BC) (Bietak 2003:24 fig. 1; Aston 2003:143, 145; Hein 2001:242–243 noting earliest appearance perhaps from about Tuthmosis I but typically Tuthmosis III; Fuscaldo 2003:71–72). This evidence, by itself, clearly seems to support a relatively low chronology, even if all this evidence is regarded as LCIB (or later – BRI continues well into earlier LCII). But we might also think about the appearances of WSII and BRII. At Tell el-Dab^c^a these wares are indicated as appearing during Stratum C/2, just after the Thera pumice, at about the transition from the reigns of Tuthmosis III and Amenhotep II. This is therefore c. 50 years after WSI and BRI first occur. This implies a very short LCIB and LCIA2 period if Tell el-Dab^c^a is regarded as the arbiter of Cypriot chronology. The question of when LCIIA began and WSII first appears in the Levant/Egypt is also not totally clear. Merrillees (1977:42) made a good case that the LCIB/IIA transition occurs before the end of the reign of Tuthmosis III in Egypt, and thus a little earlier on Cyprus, while a reported WSII vessel was found (with a BRI vessel and a likely LHIIA jar [revising previous LMIB attribution: see Hankey and Leonard 1998:33 n.30]) in the LB1 cache at Tell Ta^c^annek near Megiddo often (if not totally securely) dated to Tuthmosis III year 23 (see Warren and Hankey 1989:142; Manning 1995:224–225 with references; 1999:206–207 and references). And, although disputed, some evidence may also indicate that BRII occurs before the end of Tuthmosis III's reign (Eriksson 2001:65 and references). Such indications might suggest that LCIB ends during the reign of Tuthmosis III – and not at its very end. Allowing for time-lags in transmission and then deposit, even if small, this could see LCIB ending on Cyprus perhaps around the middle of Tuthmosis III's reign – say c. 1450BC. This in turn requires that the LCIB and then LCIA2 periods be pushed 'up' at least somewhat from the very low dates determined by finds at Tell el-Dab^c^a.

7. Theran Pumice

Theran (Minoan eruption, Bo) pumice has been identified at Tell el-Dab^c^a and several other sites (for the latest on the identification of Theran pumice at Tell el-Dab^c^a and elsewhere, and for work towards establishing a robust approach to identifying the natural range of values for analyses of such finds of Theran Bo pumice/glass, see Huber *et al.* 2003). The Tuthmosid date for the appearance of this pumice at Tell el-Dab^c^a, and indeed its appearance mainly in the later part of the reign of Tuthmosis III in Stratum C/2 (based on Bietak 2003:28 and esp. 24, fig. 1), is held by Bietak to support an earlier 18th Dynasty date for the Thera eruption. The pattern of finds is indeed interesting. But the evidence as now understood, on solely archaeological grounds, in fact nicely disproves any relevance to the date of the Thera eruption. Stratum C/2 is dated to the later part of the reign of Tuthmosis III, and the words 'Thera pumice' appear *after* c. 1450BC in Bietak (2003:24, fig. 1). This is 50–110 years after even the mainstream 'conventional' chronology scholars place the Thera eruption (Driessen and Macdonald, Warren, Wiener cited above Section 4) – let alone returning to the discussions above in Sections 5 and 6. Thus this pumice most clearly does *not* show up in archaeological contexts at Tell el-Dab^c^a in Egypt until, at a minimum, many years after the eruption. The pumice thus merely sets a very loose *terminus ante quem*, with the length of the *ante* known to be at least 40–50 years and very possibly much more. There is no immediacy at all!

Bietak is right to note that Theran pumice suddenly appears and seems to be available from the mid-15th century BC. Why? This might be to do with either (i) new technology/practice in the region which now exploits a resource (perhaps linked with the Egyptian campaigns, building, and expansion into the Levant at this time), or (ii) the likelihood that Aegean traders (royal, or downwards) began supplying this 'special' resource as part of the well known Aegean (Keftiu) contacts evident from the reign of Tuthmosis III (especially).

8. Tell el-Yahudiyeh (TY) juglet at *Toumba tou Skourou*

Bietak (2003:29) argues that a TY juglet with lotus design from *Toumba tou Skourou* Tomb V chamber 1 disproves a high chronology. Let us therefore examine the situation. Bietak states that this type of TY was not made before Stratum E/3 at Tell el-Dab^c^a. Stratum E/3 is currently dated by Bietak at about 1685/80–1655/50BC (Bietak 1992; 1997; 2000; 2003:fig. 1). Somehow Bietak (2003:29) turns this into c. 1640BC. And it has to be remembered that these dates for Stratum E/3 depend on a variety of other interpretations – they are flexible, not fixed. Others have proposed dates a few decades or more earlier (Weinstein 1992; 1995; Dever 1997). Radiocarbon evidence from Tell el-Dab^c^a itself may also point in this direction (Kutschera *et al.* 2004).

This TY vessel is deposited in a late Middle Cypriot (MC) III tomb context. Thus this should be potentially at least a little before somewhere in the c.1700–1650BC range suggested for the start date for LCIA in the 'high' chronology (see above), if there is to be no problem. Merrillees (2002) has placed MCIII from c. 1750BC. Bietak argues that the TY juglet is early MBIIB. MBIIA overlapped into at least the start of MCIII, and ends about 1700BC give or take. Middle Minoan III on Crete starts also about 1750BC (Merrillees 2003). All these 'dates' are of course round numbers and approximate. We thus have a TY juglet perhaps of around the first few decades of the 17th century BC deposited on Cyprus close to the end of MCIII. Thus we might place the MCIII/LCIA1 border perhaps just after, c.1675–1650BC. We should remember also, of course, that the TY vessel date could go up by a few years given other interpretations of Stratum E/3's dating (just as, in reverse, it may not necessarily have been buried until a few years after production, nor be from the earliest phase of manufacture in Egypt).

There is thus no necessary problem (unless one tries to force the early MBIIB date down), nor are the 'dates' cited rock solid, but all are a little flexible. The synchronism can clearly work with a high Cypriot chronology; it certainly does not disprove it.

9. Tell el-Dab^c^a paintings and other LMIA linkage suggestions?

Bietak (2003:29) argues that the wall paintings from the Tuthmosid palace district at Tell el-Dab^c^a show some very close parallels to the Thera paintings, and he thus argues that Thera cannot have been too far away in time. Some similarities are indeed striking; but other elements of the Tell el-Dab^c^a paintings do not match so well with LMIA work and instead seem better dated to subsequent LMIB/LHIIA influences as a number of scholars have commented (e.g. Manning 1999:101–103 and references). There is also the problem that the wonderful and unique corpus of mature LMIA art at Thera leads scholarship to focus on it; we lack such full evidence from the LMIB period for example; ditto the Mainland which was increasingly important during the course of Tuthmosis III's reign. If we had more such evidence, then the Thera link might seem less definitive. We also do not know how chronologically stable representative traditions were with regard to wall painting, and certain key elements/motifs (and especially at élite centres) might vary from such normal rules even if we could provide such rules from the Aegean evidence (whether increased stability of tradition, or the reverse). It is notable in the Aegean that griffins, and bull-leaping, form a fairly stable tradition over quite a long period (much though we have only a few dispersed pieces of extant evidence). The range of possible dates and circumstances leads even a scholar prepared to accept a more LMIA stylistic link to end up stating that 'the value of the frescoes for Aegean chronology is very limited' (Macdonald 2001:529). And Macdonald wrote before it became evident that the frescoes date to Stratum C/3 and the earlier Tuthmosid period. The paintings could link the Tuthmosid palace with later LMI (i.e. mature LMIB) and derivatives and so be consistent with the high chronology, or the compromise high/modified Aegean short chronology, or they could fit into a low chronology.

Bietak (2003:29) raises the link made by Manning (1999) of some earrings shown in the Theran frescoes with examples found at Tell el-^c^Ajjul. Bietak states that Manning claimed these items were MB to fit his chronology; Bietak instead says they come from LB contexts. Bietak has not read the text of Manning carefully: he cites pages 55–59, and on p. 55 with reference to these items the text says in the parenthesis 'see discussion in Chapter IV' – in Chapter IV on pages 138–139 there is discussion and here the text says that 'in past literature, the Tell el-^{c}Ajjul examples have been dated LBI, but the correct date for these City II/Palace II, or earlier, finds (and the general tradition to which they belong) is later MBII in Syro-Palestinian terms'. There is then a footnote (668) citing references to the scholars who argue this, and a cross-reference to footnote 658, which also states the same thing with references and some related discussion. Of the scholarship cited I quote here Oren (1997:271):

Tell el-^{c}Ajjul has yielded some of the largest gold hoards ever discovered in the eastern Mediterranean … Although most of the hoards were assigned to Palace II and City II of the Late Bronze Age I, thus postdating the "1570 B.C.E. destruction", some objects were recorded in Palace I, under its destruction debris. Typological considerations suggest that the assemblages of gold objects were collected over a long time and *subsequently deposited* [my italics] in the Late Bronze Age. Analogous examples from Megiddo, Gezer, and Ugarit confirm the MBIII–LBI horizon of the Tell el-^{c}Ajjul hoards.

10. Egyptian Chronology

Here there is consensus among the standard range of scholarship, as Bietak (2003:23) states with references to the main studies (see also review of Egyptian and Mesopotamian dating by Wiener, this volume). All modern discussions of the last two decades place the beginning of the 18th Dynasty around 1550–1540BC. These studies all employ the same inscription/textual historical and genealogical data, and some make use also of the available astronomical records and their best retro-calculations to yield absolute calendar dates (e.g. recently Krauss 2003 with references, Krauss this volume). Given the information to hand, these studies are plausible and logical.

Contrary to claims by some critics, Manning (1999) employed this standard Egyptian chronology in his main text. In a couple of sentences of the main text and in Appendix 1 of the book, Manning explored whether perhaps there were any errors in Egyptian chronology that might allow another c. 11–25 years of time between the fixed point of 664/663BC and the start of the 18th Dynasty. It was thought that this might perhaps help synthesise science data and archaeological data. But there was no attempt to ignore nor undermine the worth of Egyptian chronology. Some criticism in print has been, to put it mildly, extreme and wrongly-based in most aspects: see the Appendix to this paper below.

So, is Egyptian chronology correct (for further details and references regarding the following, see the Appendix to the present paper)? This is a different question and one to which we cannot – at present – know the answer, precisely. However, it is unlikely to be far wrong, and some points may even be fixed (i.e. absolute). Both genealogy/history and astronomy seem to converge satisfactorily within the framework of conventional date ranges (see Rolf Krauss' paper in this volume). But it is also not unreasonable to argue for at least some flexibility at various points – pending general agreement on some astronomical absolute date fixes. The key thing to note is that one core element of current Egyptian chronology is built around highest attested reign years of a number of the pharaohs/kings – in few cases do we have a clear statement that so and so reigned a specific period, rather we have extant records up to a certain number of years. The question thus is whether there are unattested years (especially if there is little available data for a ruler, and thus more likelihood of missing information)? There is every reason to believe that there must be at least some: Kitchen himself has to accept that 'dead-reckoning' of such royal records leaves him a few years short between 664BC and 1279BC (an astronomically derived date). Therefore, in a simple logical progression, if we know that we have unknown information, then we cannot really quantify that unknown without other parameters being available. One could point to the astronomy, and yes, this seems to offer some key parameters, but there are at best a few precise data for the Middle Kingdom and then New Kingdom through Third Intermediate Period, and there remain disputes concerning the records (the interpretation and use of the texts), observation location and method, and various practicalities, and how best to analyse all these data and to which data one should give priority (see e.g. Wells 2002; Luft 2003; Wiener 2003:365 and n.7; O'Mara 2003). Taking a positive view we may be able to confirm the standard chronology (e.g. Tuthmosis III accession 1479BC) – taking a critical/sceptical view we are left with very few fixed points – for example Kitchen (2002:11) writes '…the lunar dates are all now to be discarded – see Wells 2002'. Wiener (2003:365) also reports such scepticism and concludes that such problems leave 'Egyptian texts as the sole chronological guide'. Such scepticism, and claims to reject for example all lunar data, have been shown to be based on incorrect or partial understanding of the data and their analysis (Krauss 1989; 2003; forthcoming; this volume). Nonetheless, such statements highlight the present lack of total consensus. Kitchen points to the genealogies of various priests recorded at temples as confirming that his chronology cannot be extended more than marginally. Again, yes, these data offer general support, but nothing precise; contrary Kitchen, they do not rule out some small amount of flexibility (up or down) (see the Appendix to this paper below). We thus have an *approximately* 'fixed' chronology.

The radiocarbon evidence from Egypt has been limited in literature published up to 2003. Nonetheless, one may observe that calibrated radiocarbon dating in general is compatible with standard Egyptian chronology – even some apparent problems in the Old Kingdom period seem not really to be so once allowance is made for both the shape of the radiocarbon calibration curve through this period, and for the clear/likely indications that old and recycled wood are involved in many cases (Manning n.d.).

Prediction is very difficult,
especially about the future. Niels Bohr

11. Conclusion

Wiener (2003) referred in his title to the 'current impasse' in Bronze Age chronology. Bietak (2003) saw a conflict between science and archaeology; he did not think this conflict could be bridged. In light of the foregoing discussion, I suggest that both views need some modification.

First, review of the evidence indicates that only two positions are now plausible, either:

(i) The compromise high chronology/modified Aegean short chronology with a Thera eruption date in the earlier to mid-16th century BC and LCIA ending by about the end of the Hyksos period. Here the WSI bowl from Thera lies in the LCIA2 period (even LCIA2–LCIB transition as Merrillees 2001b argued) and is dated a little before c.1560/40BC. This can easily be compatible with other indications of LCIA2 ending around the end of the Hyksos period, of WSI and BRI from late MBA contexts at Tell el-cAjjul, and of BRI first occurring no later than the very early 18th Dynasty in Egypt if not in fact in the SIP (see Sections 4, 5 and 6 above).

(ii) The high chronology with the eruption of Thera determined as within the most likely span indicated by the current radiocarbon dating evidence (at present this best-dated range at 95% confidence level is c.1663–1599BC – but future work will of course modify) and perhaps most conveniently (given archaeological issues) in the late 17th century BC. Here the WSI from Thera has to be interpreted as early LCIA2 (even LCIA1/2 border) – and Bergoffen (2001a:155) even raises the idea of such early style WSI being introduced on Cyprus in LCIA1. LCIA2 is then a relatively long phase lasting until the late Hyksos period/New Kingdom transition, ending c.1550BC give or take.

As discussed above, either position requires much of the high chronology synthesis and Cypriot regionalism model. Choice between them depends on the weight given to the radiocarbon evidence. It should be noted that the quality and precision of the radiocarbon evidence has improved very significantly in the last couple of years, as will be evident from reading Bronk Ramsey *et al.* (2004a) and a further publication in preparation. But the situation remains flexible, and further data and/or analysis might yet increase the prospects of the compromise high/modified Aegean short chronology. This author for one is paying close attention to this possibility. Should radiocarbon data and analysis change to give a reasonable (or better) probability to the mid-16th century BC, then the compromise high/modified Aegean short chronology becomes the obvious best prospect and route to explaining all the data we have – and will I imagine be rapidly agreed to by nearly all in the field in such a case. But, at present, the radiocarbon evidence more clearly favours the high chronology – leaving us with the two choices above. What is not possible is the 'low' chronology; it must reject all the radiocarbon data with no good reason, and it must also avoid some key archaeological evidence, especially as relates to LCI Cyprus.[7]

[7] The good quality modern (standard pre-treatment and processing and correction) radiocarbon data, as they stand, clearly favour the high chronology, and permit at lower probability the compromise high/modified short Aegean chronology (see Section 4, see Figures 1 and 7). It is difficult to justify simply ignoring these data. Unlike the situation in the 1970s (compare and contrast conclusions then of Betancourt and Weinstein 1976; Hood 1978), the accuracy and precision of these data (and the quality controls for the laboratories producing them, see e.g. Bronk Ramsey *et al.* 2002:1–4) are good and robust (see also discussion of Manning and Bronk Ramsey 2003:124–129).
Wiener (2003; and especially this volume) thus tries to move the agenda to a suggestion that there might be some offset effect leading to older radiocarbon ages for the period around the Thera eruption (see discussion in Section 4). The strategy is to note all sorts of possible sources of some form of offset inducing circumstance – without in a single case demonstrating that any such effect actually applies to the Aegean radiocarbon dates at issue (and note: the range of

Second, review of the evidence indicates that there is no real science versus archaeology split. Both plausible positions require the same kinds of cultural synthesis and explanatory scenarios (e.g. regarding LCI Cyprus); the high chronology just requires a more stretched out version. The concentration of key evidence at Tell el-Dab^{c}a into the Tuthmosid period in the latest assessments (WSI, BRI, RLWM, paintings, pumice), and downwards in time from original placements in the Late Hyksos period (in 1992), then early New Kingdom (1994–1995 onwards), requires significant distance from the Thera eruption – the question is just how much?

Overall, it is chastening to note with regard to East Mediterranean chronology both how little has changed in some regards over the last several decades (cf. Cadogan 1978), and at the same time how some other things have changed enormously (and sometimes then vanished). Today we await (as we have for 3+ decades!) the beginnings of a new relatively precise and robust radiocarbon framework as the best immediate hope outside of the artefactual evidence and stylistic comparisons (and of course traditional archaeology may yet produce fresh decisive evidence for several points which are currently ambiguous). And some day an ice-core layer that is soundly dated may also produce tephra closely comparable with Thera (and then preferably a second core, replicating this finding – noting the variability in ice-core records noted by Wiener 2003:376) – but not yet.

Finally, one might ask whether the present chronological debate is worthwhile? I would argue that the 'high' chronology challenge has significantly improved Bronze Age Mediterranean archaeological chronology by requiring careful scrutiny of previous loose assumptions, and by prompting much new research and data (even if often aimed at disproving the high chronology). It has also promoted the useful integration of science-dating methods into archaeological chronology. Already, it would seem that several scholars of the pre-existing conventional (or low) chronology are moving upwards to some extent to a 'modified Aegean short chronology' – just as the high chronology camp have noted for some years the plausible and basically identical 'compromise high chronology' position. Some form of paradigm shift is underway. Eventually, we will reach a consensus and an agreed best position. But even now we have much increased, and better, and more wide-ranging, data with which to disagree. This is good. And the plausible positions are narrowing. Even if finally disproved, the high chronology challenge will be able to claim some credit for the building of a new and better conventional position. This new, refined, chronology can then form the basis to a new generation of studies of the wider cultural relations and processes of the second millennium BC east Mediterranean.

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I am very grateful as always for the generous hospitality of the SCIEM 2000 project in Austria, this time in Vienna. The SCIEM 2000 venture under Manfred Bietak has come to lead the research agenda in its field, and its meetings offer the major international forum of the current time. This is a wonderful achievement and I congratulate Manfred warmly. I

fluxes in the literature cited is huge – some are so small as to be irrelevant – some do not distinguish volcanic and biogenic sources – and there is little data on how consistent and/or dispersed a signal is and at what distances and heights). One must also stress the time and location variability of such effects *even in* the localised areas where they apply – thus in the Azores case (Pasquier-Cardin *et al.* 1999) totally uncontaminated samples were also found *within* the caldera. As noted in the main text in Section 4, the recent and good quality radiocarbon data from the LMIA and LMIB periods do not seem to exhibit the types of evidence consistent with such suggestions – and in fact the reverse.

The situation in simple terms is that, before and after the LMIA–LMIB periods, radiocarbon dating seems to offer data consonant with the historical-archaeological chronology (i.e. expectations). But, in the LMIA–IB period, it does not – it is higher. The archaeological dating evidence for the LMIA period is also very thin. So, do you regard the LMIA–LMIB radiocarbon dates as wrong, or do you wonder about the conventional interpretation of scarce hard archaeological correlation evidence?

If one does, nonetheless, wish to wonder about the radiocarbon data, it is important to note that the 'effect' sought for the 17th–16th centuries BC is in fact small – and not really compatible with most of the significant, but highly localised, or time-varying, effects noted by Wiener (this volume). And it must apply consistently over a wide area, and for some significant time period, but only for this time period (LMIA–LMIB or thereabouts), and not it seems over the entire eastern Mediterranean either. Attempts to suggest, and, better to investigate and establish, other hypotheses of possible very minor radiocarbon age offset factors will I am sure continue, and are worthwhile and useful (and some of these effects are very interesting and have great potential relevance for studies in various fields outside archaeology). However, there is no positive evidence at present that any such effects apply to the relevant Aegean radiocarbon data. A positive, but critical, approach appears the correct position with regard to the radiocarbon evidence as of 2004.

am pleased to offer this on-going discussion document concerning how we best seek both to synchronise the civilisations of the east Mediterranean and to include science-dating evidence. I greatly appreciate and respect Manfred's attempts to include all views and evidence types, and his engagement with those interpretations and data which pose problems. This text offers my current perspective on some of the key issues, and thus contrasts with BIETAK (2003). Through the working out of problems and different interpretations progress will be made. Often neither side is completely right, nor completely wrong.

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APPENDIX: Some comments on 'Ancient Egyptian Chronology for Aegeanists' (KITCHEN 2002)

Introduction

KITCHEN (2002) criticises a portion (Appendix 1) of MANNING (1999). A review/response highlights a number of issues usefully and complements the discussion in Section 10 in the main text..

MANNING (1999) is a book about Aegean and east Mediterranean chronology and cultural relations in the mid-second millennium BC. In its main text (pp. 1–366), where pertinent, it uses (and clearly states this – see p. 66) the standard Egyptian chronologies of Kitchen and von Beckerath. All the main discussion and conclusions of the book as they relate to Egypt are so based. There are then two appendices to the book. Appendix 2 argues that various recent attempts radically to re-date (lower) Egyptian chronology are incorrect – i.e. it supports/defends the standard position. Rather than simply approve, Kitchen instead chooses to misrepresent Manning's position, as if to imply he supports radical chronological shortening (p. 7); whereas Manning has for a decade been a critic of such scholarship (e.g. MANNING and WENINGER 1992). Appendix 1 investigates just how secure is standard Egyptian chronology. It is this appendix that particularly vexes Kitchen.

Kitchen accuses Manning of 'hypercriticism' (p. 5), 'needless delusions' (p. 5), and 'unnecessary foul-mouthing of the proper state of Egyptian chronology' (p. 11). There is in fact only a small to non-existent chronological dispute between Manning and Kitchen. KITCHEN (2002:11) concludes that Egyptian chronology is '…within a decade back to c.1480 BC, and within some 20 years back to 1550/1530 BC' (which oddly becomes '1550/1520 BC' on p.12). All MANNING (1999:Appendix 1) sought to investigate was whether there might be a possible error range of 11 to 25 years at c.1550BC, and all I concluded was that 'the conventional [i.e. Kitchen/von Beckerath] chronology of second millennium BC Egypt is sound in general terms, but … is not known with total precision and accuracy'. This is hardly 'foul-mouthing'! Moreover, for the record, I am happy to note here (AD2004) – and superseding text written in the late 1990s – that based on current evidence and assessment I continue to regard standard (Kitchen, von Beckerath, Krauss, et al.) New Kingdom Egyptian chronology as closely dated within small errors given the evidence we currently have to hand. And, further to, and notwithstanding, a couple of possible questions (versus proposals) raised in MANNING (1999) – discussed below – I regard a start date for the New Kingdom c.1550/1540BC and an accession of Tuthmosis III c.1479BC as the most likely current positions. I used these dates in the chronological synthesis in Manning 1999 (p.339 Fig.62) and in more recent papers (e.g. Manning et al. 2002a). There is no attempt radically to change Egyptian chronology.

Kitchen's paper does two things. First, it makes a number of supposed criticisms of MANNING (1999), often misrepresenting what is actually written. Kitchen is in fact criticising various extreme views – none of which are Manning's and none of which Manning supports. I respond to these points below. Second, Kitchen's paper offers an up-dated summary (polemical) review from his perspective of Egyptian chronology. This is of course (as always) useful, and requires no comment here.

Responses

(1) KITCHEN p. 5 begins by alleging that Manning wishes to raise the start of the New Kingdom to 1575BC. Nowhere in the 494 pages + xxxiii of MAN-

NING (1999) does Manning say this. Manning merely wonders (p. 338) whether an 11 or 25 years adjustment to the current low chronology standard view is in any way possible (and refers readers to Appendix 1 for elaboration – where, as laid out on pp. 367–368, he 'seeks to review … (i) exactly how secure is the conventional chronology of the 18th Dynasty, and (ii) whether … a higher chronology is feasible?'). This is hardly odd when noted scholars, both Egyptologists and Aegeanists (KITCHEN, WIENER, WARD – cited p. 338), have acknowledged some slight movement being 'conceivable'. Manning's text nonetheless uses Kitchen's/von Beckerath's standard dates for his (then) proposed chronological synthesis (pp. 335–340 and esp. fig. 62). From the start, Kitchen is determinedly ascribing false views to Manning. Kitchen then enthusiastically attacks such asserted claims.

(2) KITCHEN pp. 5–6 is annoyed with MANNING (1999:373) which states that, although 664BC is usually agreed as the earliest fixed date for Egypt, in fact 'if one were strict' the earliest 'truly fixed' date is 525BC. Kitchen's own text proves Manning was correct. Kitchen writes 'There has never been any dispute over the beginning of the 26th Dynasty, except as to whether it began (accession of Psamtek I) in 664 or 663 BC, this turning on whether Amasis II reigned 43 or 44 full years. As there is good reason to accept 44 years, not 43 … 664 BC should be retained'. A 'truly fixed' (i.e. absolute) date means there cannot be any doubt at all. Hence there was nothing wrong with MANNING's (1999) statement p.373.

(3) KITCHEN p. 7 re- Shoshenq I states that 'Over this man, MANNING (1999:378) blunders horribly'. Kitchen tries to impute that Manning subscribes to the James et al. and Rohl (and others) revisionism whereby Ramesses II or III is referred to. MANNING (1999:378) says no such thing – he merely refers to their criticism of Kitchen and points out that Kitchen had not fully responded to all their points in a documented way. Continuing, Kitchen refers to 'MANNING (and the incompetents he chooses to cite)' (my italics). The scholars cited in the relevant section pp. 378–380 as expressing views which raise various uncertainties in the evidence, apart from James et al. and Rohl, are: Wente, Van Siclen, Cryer, Barnes, Hayes, Hooker, Tadmor and Cogan. Kitchen's definition of 'incompetents' is wide. (I ignore here the 'alternative' so-called New Chronology literature in places such as Journal of Ancient Chronology Forum or the book edited by VAN DER VEEN and ZERBST 2002). Finally, Kitchen complains that Manning 'cavalierly' dismisses the work of Thiele. Manning did no such thing. MANNING (p. 378) merely cites a scholarly critique of Thiele and the involved matters (Barnes), and points to the fact that several leading scholars have noted that 'an independent check on Biblical data for the reign lengths of Israelite kings is lacking' (Cryer, Tadmor, Cogan). I am happy to excise any reference to Shoshenq II.

(4) Kitchen p.8 complains that his date of 'either 1068, 1069 or 1070 BC for the death of Ramesses XI (and the New Kingdom) … must [be] treated more seriously than Manning does'. Not surprisingly, Kitchen fails to cite where Manning commits this sin. Why? Manning does not actually discuss the 21st Dynasty (and, as most readers will have noted: MANNING 1999 is not about the Third Intermediate Period – contra the apparent tone of KITCHEN 2002 – and issues regarding it occur at best on only a half dozen pages of 527!). But even so, Manning's basic point all through his 1999 Appendix 1 was that these sorts of 'Kitchen' dates are simply not truly absolute (as in fixed and with any associated uncertainty precisely quantified), and any stated error ranges are guesstimates and not solidly quantified. Kitchen himself confirms this point repeatedly in previous publications. Writing of 1069/1070BC, KITCHEN (1996a:3) admits this date is really to be seen as 'within some very narrow limits (not exceeding about 5 years)' (my italics).

MANNING's (1999:376–377) point in his discussion of Egyptian chronology from 525–1279BC is that KITCHEN (in publications of 1987 and 1996a) has to admit that he cannot find enough attested years in records to produce an acceptable date for Ramesses II simply from dead-reckoning. He had to make up 11–17 or 9–15 years between 664BC and 1279BC (accession of Ramesses II) (see also KITCHEN 2000:42, where Kitchen continues to note that 'clearly, the minimum and even "probable" dead-reckoning dates for Ramesses II are too low' – from his Table 4 by some 13 or 17 years). I note that KITCHEN (2002:9) now argues this discrepancy is reduced to 0/1 or 4/5 or 8/9 years, but at the same time Kitchen admits that 'one might smuggle in 11 [additional] years piecemeal'. And just one paragraph later, Kitchen says there might be a 14 year variation by the start of the reign of Ramesses I. Kitchen then writes: 'So, by accepting the higher date, Dr. Manning would gain more than his 11-year minimum gain!'. Fine. This was exactly the point MANNING (1999:Appendix 1) was making. I sought to highlight that there was some small flexibility in the dates.

(5) KITCHEN (2002), although very critical at

length about things mentioned in brief, or tangentially, or not at all in MANNING (1999), is then notably silent about the reign of Ramesses II and an instance of an actual discussion by MANNING (1999:380–388). Here Manning investigated the Egyptian links with Babylonia, Assyria and Egypt and argued that this evidence could be compatible with a 1290BC or even perhaps a 1304BC accession date. My point then was that 1279BC had not been established beyond doubt (versus a probable argument). Here is an instance for Kitchen to prove his case. He chooses to avoid the topic. He instead only (p. 10) reviews the Amarna period Near Eastern linkages. But MANNING (1999:390) had already concluded that 'whereas the evidence surrounding Ramesses II perhaps favours an earlier Egyptian chronology, the Amarna evidence is more consistent with the middle or lower Egyptian chronology (although there is greater uncertainty over the Babylonian kinglist and chronology at this time…)' (my italics). Kitchen thus discusses the example where both he and Manning reach the same position.

If we return to Ramesses II, we may observe that previously KITCHEN (2000:42–43) noted the lunar dates from Ramesses year 52 as the key evidence. Analysis of these could lead to an accession date in 1279BC or 1290BC (or in earlier literature 1304BC). KITCHEN's dead-reckoning had got him to 1262BC or 1266BC (2000:Table 4). Thus he happily accepts the need for (then) 13/17 extra (non-attested) years to make the lowest of these lunar options, and sets about distributing some of these about. In turn, Kitchen ignores the obvious logical point that if one has incomplete minimum knowledge with no constraints (an extra 13/17 non-attested years are necessary even for Kitchen), then one cannot constrain what one does not know without other linked information. But notwithstanding, Kitchen goes on to assert 'there is no warrant whatever to add over a decade back to 1290 BC', and criticises the 'scepticism of MANNING 1995, 16, n.5'. He writes 'so, 1279 B.C. alone will fit all the data'. Now I wish to state that 1279 BC is probably correct as a best fit, and that the Amarna period evidence would support this view against 1290BC and especially 1304 BC. But Kitchen's published arguments/logic re-Ramesses II fail to prove this point. Instead, KITCHEN (1987:39) more correctly described the situation when he said that the data and calculation of extra years beyond those attested 'do not suit 1290 BC so well …[and it] is rather less realistic … [or it is] unlikely but just possible'. Nothing substantive has changed since then.

(6) KITCHEN (2002:9) finds a total of 184 years between the accession of Sethos I and the accession of Tuthmosis III. He dates Sethos year 1 at 1295 (max) to 1281 (min). Hence we can have Tuthmosis III acceding in 1479 BC – the current standard date (NB. The '*1475 BC' of Kitchen 2002:9 should be *1465BC from 184 +1281). Adding the earlier 18th Dynasty rulers we might reach 1550 BC (KITCHEN 2002:9) or at a minimum 1540/1531BC. Fine. For the record, I am perfectly happy with c.1550/1540BC! I used this approximate date in MANNING (1999:1–366) and in work since (e.g. MANNING *et al.* 2002a). Similarly, I accept on evidence to hand that 1479BC represents the likely date for the accession of Tuthmosis III as argued by von Beckerath (1992; 1994; 1997) and KITCHEN (1996a:6) and KRAUSS (2003:195) (and e.g. WARBURTON 2000:56, 58; etc.). Again, this is the date I used in my 1999 chronological synthesis, and have used in work since then.

The point of MANNING (1999:Appendix 1) was to argue that, although this is the now standard view, there are some flexibilities in the evidence assembled and interpreted to yield these dates. Simply reading the work of Kitchen – even just his most recent 2002 publication – nicely demonstrates this. Manning sought to highlight that any dead-reckoned chronology based on what is undeniably only partial evidence must – at best – only establish a minimum chronology. Of course, this may be the correct chronology. But this will only be known when proven by either comprehensive evidence, or other independent means – e.g. science-dating such as dendrochronology. Already science-based dating has recently narrowly defined the range of second millennium BC Syro-Mesopotamian chronology (MANNING *et al.* 2001a), and hopefully progress will be made with regard to Egypt in the not too distant future. Indeed, Kitchen may wish to note that the latest work combining radiocarbon dating and dendrochronology supports a middle to low Old Babylonian chronology (MANNING *et al.* 2001a, revising KUNIHOLM *et al.* 1996) and that, therefore, Kitchen might consider returning again (cf. Kitchen 2000:46) to the potential linkage between Neferhotep I of Egypt, Yantin of Byblos and Zimri-Lim of Mari and a Yantin-Ammu of Byblos (KITCHEN 1967).

(7) KITCHEN pp. 10–11 rejects criticism of his (and Bierbrier's) generation counts as decisive evidence. MANNING (1999:377, 387–388) had cited the study of Henige. Here Kitchen believes in a rigidity of evidence that is not possible from such limited demographic data (one only needs to read KITCHEN 1996b and BIERBRIER 1975 to see various flexibilities and best reconstructions and interpretations in their

analyses of the genealogies of various families of dignitaries – and even some disagreements). Kitchen also ignores the basic rule that limited examples do not discount the possibilities of some exceptions in the total population of data. I stand by the paragraph in MANNING (1999:377). Indeed, KITCHEN's (2002:10–11) own examples more or less demonstrate the point. These selected examples offer average generations ranging from 23/24 to 27/28 years – over let us say even 6 generations this implies a sensible error range of ±15 years on the average – hardly insignificant. And of course the time-line against which the calculations are made is Kitchen's, and this itself has some small flexibilities. Compound errors should be calculated. But even from what Kitchen states, we see quite a bit of range. The real overall population will, by definition, exhibit at least a slightly larger range. And Kitchen still has no defence against the possibility (even likelihood over long time periods) of a few exceptional individuals who could throw his averages. BRYAN (1991:23) sums things up nicely: 'the average lifespan for ancient Egypt is unknown, and individual variations are great in all populations'. Thus there is some flexibility. As HENIGE (1981:184) wrote: 'the counting of generations in undated or partly dated genealogies, especially over a long period of time, cannot help in establishing exact dates'. Kitchen might argue his evidence can be compatible with such and such a view, but it in no way proves it, nor requires it. The genealogical data in no way rule out a 1290BC or even 1304BC date for the accession of Ramesses II (also HENIGE 1981:182). And note: I am not saying I believe in a 1290BC or 1304BC date (see above) – I am merely saying that the genealogical data by themselves do not rule out a very slightly longer chronology. (An interesting and slightly circular case of logic should be noted at this point. A key reason that Krauss proposed Elephantine as the observation point for the risings of Sothis (Sirius) was the chronological work of Bierbrier (KRAUSS 2003:184). But if this evidence were to be considered more critically within realistic errors, perhaps the case that needed to use the Elephantine observation location is slightly less strong.)

(8) KITCHEN p. 11 reviews Middle Kingdom and Second Intermediate chronology. He offers dates consistent with MANNING's review (1999:402–411). The sole issue Kitchen raises is that Manning 'overlooked' the evidence of the time-span provided by the number of rulers at Thebes regardless of who was 16^{th} or 17^{th} Dynasty (cf. KITCHEN 2000:44–46). OK. Kitchen himself nicely avoids discussion of the problems in his previous work introduced by the book of RYHOLT (1997 – cf. ALLEN *et al.* 1999). MANNING (1999:409) estimated from the Middle Kingdom and Second Intermediate Period evidence a likely range of 1564–1541BC for the accession of Ahmose – i.e. more or less the standard chronology. There is little to object to in the end result.

(9) KITCHEN p. 11 reaches the Thera eruption. He seems to regard dates from 1528BC to 1180BC as feasible. As noted above, any date after c.1530/1520BC must entirely ignore a considerable body of radiocarbon evidence (Kitchen of course has no interest in such matters). Kitchen correctly states – if the Aegean 'high' chronology is correct – that the eruption had nothing to do with the 18th Dynasty and was instead in the mid Second Intermediate Period. Yes: see text above, Manning et al. (2002a), etc. This is the high chronology synthesis.

Kitchen then accuses Manning of creating 'unnecessary fuss to try to insert 25 more years into the 18^{th} Dynasty', contra the text of MANNING (1999), which uses the standard Kitchen/von Beckerath chronology. Only in Appendix 1 did I investigate whether there was any flexibility to the standard chronology and whether dates 11 or 25 years earlier were conceivable. I argued (as above) that there clearly is a little potential flexibility. I did not claim that the higher dates were a fact, nor recommend them, nor use such dates in my text and analysis. And I do not now – see statements above.

KITCHEN (p. 11) then states that 'Science cannot solve the intricate problems of detailed Egyptian successions, and the cross-links with the neighbouring Near East'. Yes, but only to a point. Dendrochronology is starting to do exactly the opposite (e.g. see MANNING *et al.* 2001a). And Kitchen contradicts himself when on p. 12 he too admits that dendrochronology may prove useful. Sophisticated high-quality radiocarbon sequence analyses from Egypt may also in the future prove useful.

Finally, Kitchen admonishes Aegeanists for thinking pots can give absolute dates and reminds them that 'time-spans of pottery-style use are matters of (gu)es(s)timate'. If this is meant to be criticism of Manning, then Kitchen did not read the main text of MANNING (1999); this is after all more or less the point of many pages in the book! (and several previous studies by several scholars, including the present one over the last 15 years – e.g. MANNING 1988).

Conclusions

The basis of the Aegean 'high' chronology case is entirely independent of Egyptian chronology. And it need not have any direct impact on Egyptian chronology (compare also WARBURTON's 2000 reasoned assessment). MANNING (1999) employed standard Egyptian chronology. However, as part of a critical review of all evidence, Manning also reviewed just how secure the standard Egyptian chronology was (as of 1998–1999 evidence). He also noted, if conventional linkages between the Late Minoan IB period and Tuthmosis III are maintained, that a slightly higher chronology might be more convenient (pp. 338, 412). He asked whether this was even possible. But MANNING also stated (p. 412) that 'the middle (or even low) Egyptian chronologies can easily be considered compatible' (and this position was the one used in the figure 62 synthesis on p. 339). He also made clear that this 'issue' only arose if the conventional art-historical views were maintained (p. 412). In fact, as noted above in Sections 4 and 5, there seem to be reasons from both radiocarbon and archaeology to in fact revise and somewhat raise the conventional linkages – hence there is perhaps no problem at all and no conflict between the Aegean and Egyptian dates at this point. MANNING (1999:412) concluded by writing that 'Nor, as I seek to stress, is there any actual conflict between the … early Aegean LBA chronology, and the conventional (middle or low) Egyptian chronology'. Contra KITCHEN (p. 5), I do not 'wish to adjust Egyptian chronology'; I merely investigated its accuracy and precision.

Between when MANNING (1999) was written and now, some 5 years, a number of things have of course changed (and significantly): new evidence, new analyses, and so on. Issues with regard to science-based evidence have been briefly reviewed in the main text above. Some of the new evidence strongly enhances the case for the 'high' Aegean chronology, but other discussions (mainly archaeologically-derived) create new problems or complications. Some new scientific evidence from Egypt from Tell el-Dab'a itself raises potential issues about the chronology of that site especially in the SIP (KUTSCHERA *et al.* 2004), and further work is anticipated to investigate this situation (Bietak pers. comm. Jan. 2004). Egyptian chronology is separate so far to current debates. The chronology for Egypt painstakingly built up, and as represented by the corpus of work by scholars such as Kitchen and von Beckerath, is more or less correct. The (inadequate) radiocarbon evidence available from Egypt offers, broadly, support, or at least compatible evidence (MANNING n.d.). There is, nonetheless, some small element of flexibility in even the best existing analyses, because we do not have comprehensive and/or fully replicated data, nor evidence which is entirely contradiction-free.

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