MANNING, W. — A Test of Time. Oxbow Books, Oxford, 1999, (24 cm, XXXIII, 494). ISBN 1 900188 99 6.[1])

This book focuses primarily on the problems of dating the Minoan eruption of the Volcano of Thera. The date of this is still a matter of keen debate and, year on year, the research into resolving its chronological status reaps dramatic new aspects and results. Be that as it may, the issue has not been resolved, despite frequent premature claims to the contrary. It certainly will still take some or even many years before the long ongoing discussion can reach a conclusion. The date of this event is of critical importance for the synchronisation of the civilisations in the Eastern Mediterranean. A solution of this matter is the key to most of our present synchronisation problems. Any scholar who is able to present cogent evidence of this eruption date — evidence that will stand the *test of time* — will deserve an archaeological Nobel Prize.

This book is a courageous attempt to find a solution to the time gap between the date preferred by scientists which had been 1628 BC or at least late in the 17th century BC and the historical/archaeological date that lay somewhere between 1550 - 1500 BC. Sometimes a date even as late as 1460 BC has been suggested. The author is a Reader in Archaeology at the University of Reading, UK. He has a background in Aegean Studies, but also has a wide background in scientific methods used for archaeology, particularly in Carbon 14. He has written on the problem in many articles and contributions before (and also after) this book was published. Within this bulky book he not only deals with Aegean archaeology, but also with the scientific methods and results that focus on the pinpointing of the Thera eruption. His brief is to discuss matters of Egyptian archaeology and chronology, Levantine archaeology, Cypriot and Anatolian chronology in the Middle and Late Bronze Ages and he deals in all these fields single-handedly, confronting intricate problems in all those fields. That is why this book could be called a brave venture, although Manning himself has written (p.42) *"many archaeologists of one region/specialism have not critically and rigorously examined relevant archaeologist evidence from other regions. Hence collective mutual incomprehension, and incompatibility, reigns."* This, unfortunately and paradoxically, is one of the main shortcomings of this publication. Especially in the discussion of the archaeology of sites and regions unfamiliar to the author, there is — due to a strong bias of the author — a repetitive pattern of uncritical interpretations or quotes taken from other publications. The whole presentation deals with an enormous volume of material.

The contents of this book are arranged in the following way: An introduction of sorts with a glossary called "Maps, main ceramic types, and abbreviations", which does not show up in the contents of the book, followed by a Preface, an Introduction (I.), A brief history of the Thera debate (II.), Aims and preliminary issues for this study (III.), and as the first main chapter, Archaeology and historical evidence (IV.)

This section again includes a discussion on the relative date of the Thera eruption, a dating of the Middle Minoan III period in archaeological terms, a discussion of Tell el-Dab‘a and its frescoes in connection with LB Aegean chronology. There then follow accounts on linkages between the Late Minoan IA Period, the Late Cypriot I period, Egypt and the Levant; discussion of the problem of the Late Cypriot White Slip I pottery, its appearance on Thera and in Egypt — one of the most challenging pieces of evidence against high Aegean chronology — as well as a discussion on the "tempest stela" of Ahmose; archaeology and chronology of the LM IB and the LM II period; a discussion of the Keftiu representations in Theban tombs of the Tuthmoside Period and,

[1]) For consultation and criticism, I am indebted to Peter Warren and Malcom H. Wiener. All mistakes are mine.
Within this review following abbreviations are used:
BIHM=Bichrome Handmade Ware, BIWM = Bichrome Wheel Made Ware, BR I = Base Ring I Ware, BoR = Black-on-Red Ware, CoW = Chocolate on White Ware, LC = Late Cypriot, LM= Late Minoan, MC =Middle Cypriot, MK = Middle Kingdom, MM = Middle Minoan, NK = New Kingdom, PBR = Proto Base Ring Ware, PWS = Proto White Slip Ware, RLWM = Red Lustrous Wheelmade Ware, RoB = Red-on-Black Ware, TY = Tell el-Yahudiya Ware, WP II-IV, WP V, WP VI = White Painted II-IV, White Painted V, White Painted VI Wares, WS I = White Slip I Ware

finally, Amenhotep III and Aegean chronology. In this section the author displays his wide knowledge of the specific literature in this field. It is, however, one of the weakest parts of the book, permeated by a strong bias of the author and his failure to reach logical conclusions.

This is followed by the second main section on absolute dating evidence, including radiocarbon evidence; a northern hemisphere climate event in the late 17th century BC; a discussion on volcanic glass in the GISP1 ice core from Greenland as potential confirmation of a 17th century BC date for the Thera eruption; a discussion of the Aegean dendrochronology as further evidence for a 1628 BC climatic event consistent with the Thera eruption. This is the strongest section of this book and shows a high level of expertise by the author in this field.

The third main section contains a summary and conclusions (VI.) repeating, once again, the problem-issues of archaeology. It goes on to another discussion of Tell el-Dab'a; problem-issues of scientifically based dating; another discussion of the limited scope of a post-eruption LM IA phase and proposed conclusion. Section VII — with a discussion on problems of dating Alalakh VII — could be taken as an after-thought. This is followed by an actual appendix on Egyptian historical chronology and a second appendix on why standard chronologies are approximately correct and why radical re-dating is incorrect as a result. The book concludes with a very useful and bulky bibliography, albeit containing a few mistakes (which is uncommon these days). A few randomly chosen plates come at the end.

The way this book is structured leads to frequent and needless repetition under different headings of what are at times rather verbose commentaries. A streamlining would have rendered this publication far more user friendly. The author has tried, however, to help the reader by providing a detailed index.

A commentary on the mistakes and on points which are questionable will be presented below. Of course, the strong points of this study will also be outlined.

The very title page prepares the way for mistakes and misinformation. A representation of a girl from Xeste 3 on Thera is shown twice because she wears large ear-rings which on pp. 55-59 are compared to finds from Tell el-'Ajjûl Hoard 1450 and presented there as a later MB context. This should obviously be archaeological proof that the Theran paintings are from the same time-range as the Middle Bronze Age of the Levant. This hoard is, however, of later date and in any event disrupted by a scarab of the 7th to 8th century BC.[2]) On fig. 20, the author also points out close similarities between crocus- and papyrus-shape motives from a representation of a ship in the West House on Thera with beads of a pendant from grave 1073 at Tell el-'Ajjûl which is supposed to be of MB date. In reality, the tomb is from the LB and contained — in addition to a BR I jug — three scarabs of the 18th Dynasty, the earliest dating from the time of Tuthmosis III.[3]) If this evidence were to be taken as the author had in mind, this would mean that the Thera paintings would date approximately to the same time-range as the Tuthmosides (15th and not 17th century BC)!

[2]) Keel, *Corpus der Stempelsiegelamulette aus Palästina/Israel,* OBO 13, Fribourg/Göttingen 1997, 210-213, nos. 323-324. I would like to thank Christa Mlinar who pointed out this detail to me.

[3]) Ibidem: 184-185, nos. 242-244.

A worrying dialectic exercise is the terminology used for the different chronologies in dealing with the Aegean. He designates *"Early Chronology"*, around a high eruption date of Thera at 1628 BC — there is nothing wrong with that. Then comes the *"Compromise Early Chronology"* for a chronology tailored around a mid 16th century, with a date as late 1530 BC as for the eruption of Thera, which Manning considers still possible according to the radiocarbon data. This *"Compromise Early Chronology"* is nearly 100 years later than the *"Early Chronology"* which has since centred on an eruption date of ±1645/44 BC.[4]) The terminology is highly misleading as this is still to some extent the time-range preferred by adherents of a Low Chronology as Peter Warren and Vronwy Hankey. It may be described as a *"Medium Chronology"* or even a *"Low Chronology"*. This term is used, however, for eruption dates between 1520 and 1460 BC, but the differences between the *"Compromise Early"* and *"Low Chronology"* is much smaller than between *"Early"* and the *"Compromise Early"* which is *not* early at all.

P.26: Finds of MB II imports from the Levant within a sealed LM IA context from Thera are listed as a proof that LM IA started within the S.I.P. The theory that LM IA started within this period has broad support, but there are several shortcomings and mistakes to this reasoning according to the rules of logic.

1. The S.I.P. is a long time span (c. 1700-1550 BC when including only the later part of the 13th Dyn. otherwise 1795-1550 BC).[5]) It should be specified and explained which part of this period is meant when discussing specific points.
2. The identification of the amphorae imported from the Levant is highly questionable. Amphorae of the MB II and LB I look very similar. Until now there has been no study to differentiate the amphorae from MB II and LB I. The same amphorae have been explained by W. -D. Niemeier once as being MB and another time as being LB.
3. Even if imports from the Levant are MB, this proves nothing as this period is likely, according to the short and the high chronology of the MB, to continue regionally until the time of Tuthmosis III (1479-1425 BC). See Fig. 6 in Manning's publication.

P.28: At Kom er-Rabi'a at Memphis in a pre Tuthmosis III/early 18th Dynasty context a Minoan sherd was found "*and it seemed quite likely that it was LMIB in stylistic date*". Footnote 134 concedes that the sherd was originally considered Mycenean, but subsequent study showed that it was LM I, and *probably* LM IB.[6]) What was presented on this page, albeit with some caution becomes on p. 42 within the repetitions, so typical of this publication, solid "proof" that LM IA would have ended about the beginning of the 18th Dynasty: *"Bourriau and Eriksson (1997) present compelling evidence from conventional archaeology that imported LM IB and later LH IIA material was in fact being deposited in*

[4]) S.W. Manning & C.B. Ramsey, in: *The Synchronisation of Civilisations II*, ed. by M. Bietak, Vienna 2003, 111-133; C.U.Hammer, et al., *ibidem*, 87-94.

[5]) After K.A. Kitchen, in The Synchronisation of Civilisations in the Eastern Mediterranean, ed. by M. Bietak, Vienna 2000, 39-52..

[6]) J. Bourriau & K.O. Eriksson, in: *Ancient Egypt, the Aegean, and the Near East, Studies in Honour of M.R. Bell*, ed, by J. Phillips, San Antonio 1997, 95-120.

Egyptian contexts dating somewhere between the reigns of Ahmose I and Tuthmosis I." The change from uncertainty to certainty in the line of argument might be taken as a rhetoric aid to persuade the reader over to the high chronology. Such changes in register should not be used in an objective scholarly publication. This should set alarm bells ringing.

1. The sherd is not safely attributable to LM IB rather than LM IA.[7]) Manning made the identification from an inadequate illustration which he incorrectly copied.[8]) On the other hand, the author — when it suits his case — admits in other contexts that the differentiation between LM IA and LM IB pottery is not easy (p. 71): *"…the majority of LMIA and LMIB standard ceramic types are very similar, moreover, the classic LMIB styles known from the close of LMIB destructions on Crete did not appear immediately at the beginning of the phase. Hence determining whether one is late LMIA, or early LMIB is often not easy."*
2. The stratigraphic context is not published with its associated material. The dating between Ahmose and Tuthmosis I first has to be proved cogently. Until this is done, the so-called evidence has to be discounted. David Aston, one of the top experts of New Kingdom pottery, will shortly be presenting a range of evidence for a later date of this context.[9]) The reader has to be reminded of a Base Ring I jug from Memphis within a context attributed to the S.I.P. that in fact proves to be of an 18th Dynasty date (s. below).

At pp. 29-30, the author who is no expert in the MB/LB archaeology of the Levant presents as archaeological proof for the eruption date of Thera in the 17th century BC the destruction of the late MB II palace at Tel Kabri in North-Western Galilee. The uncritical reference is worth citing: *"W.-D. Niemeier (1990)… had been excavating a major MBA site at Tel Kabri in Israel, and particular a later MB destruction level dated around c. 1600 BC. He had just found LMIA-style fresco paintings in the Levant! He had also found Canaanite storage/transport jars very similar to an imported example found in the early 1970s by Marinatos at Akrotiri. The key Tel Kabri LM IA-style painting floor showed signs of wear, and had clearly been in use some time before the later MB II, c. 1600 BC, destruction at the site. Niemeier therefore argued that this evidence suggested that the LM IA style was clearly current before c. 1600 BC, contemporary with the later MB II period of Syria Palestine and that imports of this time had gone to Akrotiri. The implication was clear: this evidence supported the 'high' chronology, and the later 17th century BC date for the late LM IA eruption of Thera. It showed LM IA was contemporary with the later MB II pre-18th Dynasty horizon, in western Asia"*.

It must be said that this is a blunt, double-circular argument that is based on several grave mistakes in logic.

1. There is no independent absolute chronology for Palestine. How the date 1600 BC for the end of the palace and the MB II for it was obtained is not explained, but the result is used as a proof — which comes down to a flouting of all rules of logic.
2. Only small fragments from the paintings of the palace at Kabri are preserved. The restorations of the paintings by B. and W. -D. Niemeier were made in a professional way, but borrowing heavily on the Thera frescoes. This is the main reason why they share similarities with the Thera paintings. It would be, however, methodologically wrong to claim from the point of view of art history that the Kabri paintings must originate from the same time as the Thera paintings.
3. Now comes the second part of the circular argument. Because the Kabri paintings are so similar to the paintings of Thera, they must date before 1600 BC. This is the date W. -D. Niemeier — at the time an adherent of the high Aegean chronology — suggested, despite being at odds with the Palestinian chronology (s. below no. 4). Manning adopted this date wholesale without critically reviewing or explaining it, because it fitted into his predilection for the high chronology.
4. The end of the MB in Palestine is dated by most chronology researchers, if high or low, between the beginning of the New Kingdom and, regionally, up to the campaigns of Tuthmosis III (c. 1550, rather after the fall of Avaris c. 1530 BC until the campaigns of Tuthmosis III (c. 1457/6 ff. cf. Manning Fig. 6). It may have been that the end came earlier at T. Kabri because of local history. The claim by the late Aharon Kempinski that the palace dates to a phase before the introduction of the Bichrome ware is a weak *argumentum ex silentium*. The ceramic material goes, however, against such a supposition. The presence of Chocolate on White Ware (CoW) and of Cypriot White Painted VI (WP VI) Ware shows that this site must take us to the end of MB II. Both wares are markers for the late MB IIC *and* LB I.[10]) WP VI appears at Tell el-Dab'a at the end of the Hyksos Period and the first part of the 18th Dynasty (ph. D/2-1) and is present in Palestine until the Fosse Temple I-II in the Late Bronze Age (earliest Tuthmosis III). So, in theory, the Kabri paintings could even date to the first part of the 15th century and contemporaneously with the Tell el-Dab'a paintings. That is why the Kabri paintings cannot be used to support a date just before 1600 BC).

On the same subject p. 32 (see also p. 98) again the author confronts us with a dialectic exercise*: "In 1992 Bietak (1992b) announced dramatic findings of relevance to the Aegean. He too has found LM IA-style frescoes. These were said to be from a late Hyksos context (stratum D/2) at the site. This was in broad agreement with the Tel Kabri finds of Niemeier, and the old finds of Alalakh, and further supported a pre- 18th Dynasty, Hyksos date for the LMIA period. However, Bietak proposed a 16th century BC date for the Tell el-Dab'a context."* This statement does not make any sense, except if the absolute date of the Kabri frescoes before 1600 BC were self-evident. But again, where is the proof?

1. When Manning wrote his book he knew that the date of the Tell el-Dab'a-paintings had been revised since 1996 to the early 18th Dynasty (see Bietak 1996). Manning was invited to join the Haindorf Conference in 1996 and the Vienna Conference in 1998 when he received a full briefing on the contexts of the frescoes, besides the Bietak

[7]) P. Warren & V. Hankey, *Aegean Bronze Age Chronology* (Bristol 1989), 139.

[8]) Cf. n. 6.

[9]) D.A. Aston, in: The Synchronisation of Civilisations in the Eastern Mediterranean III. Euro-Conference of SCIEM 2000 in Vienna 28th June — 1st of June 2003.

[10]) P.M. Fischer, *BASOR* 313 (1999), 1-29.

1996, Bietak 1997 and other publications. So why does he return to the late Hyksos Period?
2. Even if the paintings dated from the late Hyksos Period, which ends at ±1530, it would be logical for the "late Hyksos" to be some time shortly before that. This is why Bietak has proposed a 16th century BC date. How such a context could possibly support a high date of the Thera eruption (late 17th century) which would be early Hyksos really goes entirely beyond comprehension, apart from the fact that the paintings date definitely to the time of the 18th Dynasty.

P. 30 Also in using the Alalakh VII paintings to support a high chronology, the author still makes the same basic methodological mistakes as with the Kabri excavation.

1. There is no art historical analysis linking the Alalakh paintings firmly to LM IA, although I for one think that this might be correct. From a methodological point of view, such considerations *per se* are of no relevance to the relative and absolute chronology.
2. The date of the end of Alalakh VII is linked to the Hittite incursion under Hattushili I (c. 2nd year of reign) to be dated, according to the Middle chronology, to c. 1628 BC and, according to the low chronology, to ca. 1564 BC plus, according to the even lower chronology of Gasche et al., to c. 1532 BC. This would mean that there was some likelihood of the paintings even dating to the 16th instead of the 17th century BC.

At p. 35 the author is under the impression that he could provide ammunition for his discussion on Aegean chronology with a dispute on the MB chronology between W. Dever, J. Weinstein and M. Bietak.[11]) Following the discovery of the Ashkelon seal impressions, the dispute has now been settled in favour of a low MB chronology.[12]) The previous differences between Dever, Weinstein and Bietak, however, never touched on the end of the Tell el-Dab‘a series where all three of them held virtually the same opinion. The series ends with the conquest and abandonment of Avaris between the 11th and the 18th year of Ahmose. Therefore the point of S. Manning, p.35, is a complete mystery: *"Finally, the excavator of Tell el-Dab‘a supported a low MBA chronology in a separate dispute within Syro-palestinian archaeology, and the dates offered for the D/2 stratum were the result of this and his arbitrary scheme allocating each of this MB phases at the site the same 30 year interval. Thus a little flexibility was possible"* .

Of course, a little flexibility is possible, but str. D/2 is fixed on a datum line of the abandonment of Avaris at the end of the Hyksos Period ±1530 BC. It is possible to make an earlier start with D/2, shortening the previous strata, but there is no way of moving into the 17th century as, in such a case, D/2 would almost cover the entire Hyksos Period, telescoping the other strata of late E/2, E/1, D/3 into less than a decade each — a highly unlikely proposition as each of the strata involved shows a distinct development in material culture (see below).

This *tour de force* is understandable only after the reader realises that the author still wants to stick to the date of the paintings and the first appearance of White Slip ware as being during the Hyksos Period, not necessarily the late Hyksos Period. This is his understanding of *flexibility*. Yet, even if the paintings and White Slip Ware were from the time of Str. D/2, it would not be possible to construct a date of 17th century, as D/2 is linked to the conquest of Avaris and shortly before, i.e. mid-16th century — which is still ca. 100 years too late to endorse a high chronology. This part of the discussion is in any case futile, as the paintings and the White Slip pottery were found only in 18th Dynasty contexts.

Pp. 80 ff. The author asks the reader at various stages of this book at least three times to study the data and facts without bias and objectively. His discussion of the excavations at Tell el-Dab‘a, however, seems to be a contrary exercise. This section can be seen as the weakest part of this book. The author can only be understood as an ardent adherent of the high/or early Aegean chronology. Whatever the evidence that goes against this idea, he casts doubt on the evidence that presents in a way that, especially for an insider, is impossible to accept. The excavations at Tell el-Dab‘a have turned up some results that are difficult to explain by a unilateral rise of the Aegean chronology. One such result is that the White Slip I Ware did not make its first appearance before the 18th Dynasty, another being some specific iconographic features of Minoan Wall paintings which have close parallels in Thera. On the other hand, the dating of strata of old excavations, like the one of Petrie at Tell el-‘Ajjûl, or what is termed archaeological high date of 1600 BC for the end of the palace of the broadly unpublished excavation at Tel Kabri (see above), are taken for granted without any explanation or critical review. Is it because they seem to fit into the chronological scheme of the author?

In commenting on the excavations at Tell el-Dab‘a, the author often throws scientific caution and fairness to the wind and is influenced in his interpretations by wishful thinking. Unfortunately, this detracts from the credibility of the scholarship of this book which, in other sections, has its merits and strong parts. He still tries hard to make a case for a Hyksos date of the frescoes at Tell el-Dab‘a, although the date has been settled since 1996 as pointing to an early 18th Dynasty chronology. It is also most unwise without the expertise and inside knowledge of a complex research project to try to reinterpret the results.

The site revealed in successive strata a citadel of the late Hyksos Period (ph. D/2) and a palace district of the first half of the 18th Dynasty (ph. C/3-2).[13]) The latter includes thousands of fragments of wall plaster with Minoan wall paintings dumped mainly in front of the smaller palace F (area H/I), lying over gardens of the late Hyksos Period. However,

[11]) W.D. Dever, *BASOR* 281 (1991), 73-79; id., *BASOR* 288 (1992)1-25; J.M. Weinstein, *BASOR* 288 (1992), 27-46; last, M. Bietak in: *The Hyksos, New Archaeological and Historical Perspectives*, ed. by. E.D. Oren, Philadelphia 1997, 87-139; idem, in: *The Middle Bronze Age in the Levant*, ed. by M. Bietak, CCEM III, Vienna 2002, 30-42.

[12]) L. Stager, in: *The Middle Bronze Age in the Levant*, ed. by M. Bietak, CCEM III, Vienna 2002, 353-362. The Leon Levy Expedition under L. Stager discovered between gate phases 14 and 13 over 40 seal impressions of the early 13th Dynasty. According to a joint comparative study with the T. el-Dab‘a team this corresponds to phase G/4 at Tell el-Dab‘a and firmly endorsed the date of this phase at the beginning of the 13th Dynasty. Thus the stratigraphy of Tell el-Dab‘a with the phases G/4 till D/2 is besides a datum line at the beginning of ph. K (Temple at ‘Ezbet Rushdi) firmly fixed into the time of the 13th Dynasty and the Hyksos Period (c. 1770-1530 BC).

[13]) M. Bietak, *Avaris, The Capital of the Hyksos*, London 1996; id., *BSA* 95 (2000), 95 (2000), 185-205; M. Bietak, J. Dorner, P. Janosi, *E&L* 11 (2001), 27-120.

the circumstances surrounding the first finds were unclear after the first two 1992 and 1993 seasons. The paintings and the small platform, that turned out to be the substructure of the small palace, were dated at the time to the late Hyksos Period.[14]) After discovery of the second large palace G (area H/II-III) with paintings of the same kind that had fallen off the walls of a gateway, though with some paintings still in situ, and following a re-examination of the stratigraphy of the small palace, a revision of the date to the early 18th Dynasty became an inevitability. Both palaces belong together and were constructed by the same planning body. Their distance from each other is precisely 150 ancient Egyptian cubits. The stratigraphic reasons for this re-dating were given in 1996, 1997 and, in more detailed fashion, in 2000 and 2001.[15])

In the meantime, two more strata of the early 18th Dynasty have been found under the palace district. The date was obtained by pottery analysis and the typology of a bronze mould of an axe.[16]) The earlier stratum (ph. D/1.2) consisted of storage facilities such as silos, magazines and a small palace, all behind an enormous enclosure wall. The second stratum (ph. D/1.1) was composed of soldier graves, horse burials and remains of a military camp. These two new strata took the palace district away from the beginning of the 18th Dynasty period into the Tuthmoside time (15th century BC). A clear account with stratigraphy and sections has been published.[17]) The details need not be repeated here. It is, however, important for the position of the frescoes which were found in secondary dump heaps to be perfectly understood in the meantime. They were indeed found within a stratigraphy. They were collected in two dumps, one at the base of the ramp and the other at the foot of the landing of the ramp of platform H/I (Palace F) on top of the former gardens of the Hyksos Period, but in association with New Kingdom pottery. Similar find positions were also found besides the paintings in the gateway, with wall plaster of the big palace H/II-III (Palace G). Due to the shrinkage of mudbrick walls on alluvial ground which could, according to sound experience, last at least 15 years, the paintings on inelastic hard plaster must have come off quickly. They were obviously collected, carried down the ramp (first heap) or were thrown down the landing (second heap).

The 18th Dynasty date of the paintings had already been clear after the 1996 Bietak publication, the author of the book under review having received pre-publication more detailed information about the interim strata ph. D/1.1-2. He himself cited the grain silo stratum (p. 92). Nevertheless he continues throughout his book to angle for a date during the Hyksos period, although it had been made quite clear that not a single fragment of the Minoan paintings on lime plaster had appeared in the Hyksos Period or even in early 18th Dynasty contexts. The Hyksos Palace was not found until 2001, more than 150 m towards the south without any link to the Minoan frescoes on lime plaster. The only paintings to be found in connection with the Hyksos palace were on mud coating and with only linear patterns and remains of hieroglyphic inscriptions.

However, Manning makes this attempt to bring the paintings of Tell el-Dab‘a showing iconographic features found in LM IA into the 17th century BC, as this would bring them near the supposed 1628 BC date for the eruption of Thera and paintings at Akrotiri, Tel Kabri (see above) and at Alalakh. It is an attempt to make the high eruption dating fit the historical chronology. In doing so, he even fantasises about dating the small platform H/I (Palace F) to specific Hyksos rulers, either Khayan or Awoserre‘ Apophis (p. 92). He does this without reviewing the archaeological material, though for this purpose a thorough knowledge of the ceramic material of the S.I.P. and the NK in Egypt would be indispensable. He also misrepresents the archaeological evidence (p. 94) that *“some of the fresco fragments were found stratified in the foundations associated with the modifications of the H/I platform”* or p. 95 that *“He* (Bietak) *admits some fragments* (of the frescoes) *were found in late Hyksos contexts but argues that it is impossible to show they are definitely of this date…”* These are misstatements, probably due to linguistic problems. The opposite is true. No fragment of paintings have ever been found in a Hyksos layer or in the two layers of the early 18th Dynasty. He seems to have mixed up a statement about a filling wall added later with fragment of paintings, found albeit together with blue painted pottery of the late 18th Dynasty.

The author goes on to give the wrong information (p. 96) that the platform H/I was destroyed soon afterwards. None of this was ever written by the excavators, but it is the cue for Manning to tie up this non-existent “destruction” with context of the Ahmose conquest.

He also shows lack of understanding Egyptian palatial architecture in failing to recognise an obvious ramp with a landing leading up to platform H/I and presents this structure as a later extension of the palace in order to claim two phases, one in the Hyksos Period and the other a later addition at the time of the New Kingdom (p. 95). These are just some of the numerous and quite deplorable mistakes which show an utmost lack of care in dealing with archaeological sources.

Summing up, the review of the archaeology of Tell el-Dab‘a is the weakest part of this book and shows lack of a proper and sound assessment of such an important site. There really has to be more communication with the excavators to help better understand the results of a current fieldwork project.

At pp. 100-107, the author attempts a stylistic analysis of some features of the paintings and argues that there are two strata of paintings, for which there is no basis other than to salvage the hope that some of the paintings might be from the Hyksos Period. He dates some of the features to LM IB rather than LM IA — which is entirely possible, but not conclusive as he admits at p. 100 that *“we are rather ignorant of LM IB frescoes”*. Worthy of consideration when evaluating the frescoes is Paul Rehak’s idea that *“the spread of the bull iconography outside of Knossos is a feature of the end of the Neopalatial period.”* Is it possible that the bull iconography at Tell el-Dab‘a is comparable to the spread of this kind of ideology to the Mycenean palaces? Art at Tell el-Dab‘a belongs from a stylistic point of view to Minoan art and bears little resemblance to Mycenean mural art. We do not know, however, whether the earlier Mycenean palaces did not also have mural art which may surface one day and add to our understanding of the Tell el-Dab‘a paintings.

To soften the critique, a ray of light appears at p. 97 where Manning suggests the alternative to putting the Minoan paintings into a Tuthmoside setting when close ties with the

[14]) M. Bietak, J. Dorner, I. Hein, P. Jánosi, *E&L* 4 (1994). 9-80.
[15]) Cf. ns. 13-14.
[16]) M. Bietak, J. Dorner, I. Hein, P. Jánosi, *E&L* 4 (1994). 73, fig. 32.
[17]) Cf. n. 13: Bietak, *BSA* 95, 187-195, fig. 5; Bietak, Dorner, Janosi *E&L* 11, 36-45, figs. 4-7.

Minoan world are known to have existed. Representations of Keftiu are known in a number of Theban tombs from the reign of Hatshepsut/early Tuthmosis III onwards. This option has indeed proved correct from recent excavation results.

In his assessment of the relative chronology of the Aegean towards Egypt (pp. 109 ff.), the author deplores the fact that nearly no MM III and LM IA exports have been found in Egypt. This is certainly correct to some extent, but it has to be taken into consideration that we have only few settlement excavations of the S.I.P. and early 18th Dynasty with the required volume for assessment. It is, however, more dangerous to turn the argument back to front and claim that, because we have no LM IA imports in undisputed 18th Dynasty contexts, LM IA must have ended before the 18th Dynasty. The question should therefore be asked: where are the LM IA imports in Avaris in the Hyksos Period after a heavy volume of graves and settlement had been dug there? On the other hand, there is some evidence of LM IA influence in the early Tuthmoside Period.

Fragments of an amphoriskos with a representation of a leopard in flying gallop and chasing an ungulate have been found. The amphoriskos has twin handles, outlined with a black and red stripe at their join to the body of the vessel, done in the fashion of an 8-shaped Aegean shield.[18]) Manning identifies this amphoriskos as Cycladic, coming definitely from pre-eruption Thera (pp. 114-115). He cites a similar vessel from Thera[19]) and also refers to the catalogue of Cline 1994 without specifically citing figures from there in the text. Thera was according to Manning *"the only Cycladic centre with regional and extra — Aegean connections. Hence the export of any Cycladic product outside the Aegean, or evidence of a stylistic linkage (whether import or export) with an area outside the Aegean, is likely only via an active Akrotiri. In support, we may observe that the only other LC I Cycladic exports into east Mediterranean are usually thought to be Theran,"* citing G. Cadogan and Vermeule & Wolsky (n. 514). Manning also deplores the fact that this amphoriskos was found out of context. But this is not the case any more. After several more fragments of this piece retrieved from a filled-up channel (L1182),[20]) undoubtedly of str. d-c = (phases C/3-2), had been found, its position in the stratigraphy is now very clear. It is highly unlikely that we are dealing here with redundant sherds, as the state of preservation was excellent and several fragments are involved. There is also a context of about 1500 sherds, which are largely from the Tuthmoside period. They were waste, thrown into the empty channel that was dug in the early phase of the palace (C/3) and was later filled in (most probably in ph. C/2: late Tuthmosis III and Amenophis II).

So, even according to Manning, there would be a connection between the LM IA and the Tuthmosis III period. The amphoriskos in question seems, however, to be a hybrid product related to the BIWM Ware, produced in the Levant and painted with influence from LM I A (or according to Manning Late Cycladic IA) art.

From the same context there were also fragments of locally made rhyta. Another such complete rhyton was also found in one of the palace magazines from the same period (ph. C/3-2=) Tuthmoside Period).[21]) According to a study by Robert Koehl (Hunter College, New York University) these rhyta of the early 18th Dynasty, produced in Egypt, are of LM IA typology.[22]) They must have started being produced at a time when LM IA was still the dominant cultural phase on Crete. The same is true of the representations of the Vapheio cups (Manning p. 210, fig. 38) from the Keftiu representations in the Theban tombs at the time of Hatshepsut and early Tuthmosis III (tomb of Senenmut, TT 71). They represent the LM IA time level — as even acknowledged by Manning (p. 213) who suggests that they were prestige objects and anachronistic as they had probably been in treasure chambers for some considerable time. Their disproportionate size does, however, not necessarily mean they were an unknown quantity. It was particularly around this time that specific objects were used as icons and represented in disproportionate size like, for instance, sandals topping representations of sandalmakers in the tomb of Rekhmire (TT 100) at Thebes. Even if the Vapheio cups had already gone out of fashion during the early reign of Tuthmosis III, they are proof that the time link must have still been alive and within a single generation.

The raft of links between the Tuthmoside Period and the Aegean concludes with the discovery of the Minoan paintings associated with the early phase of the palace (C/3),[23]) and the two bags with more than 150 Late Helladic arrowheads found near workshop W1 of the late phase (C/2) of the palace.

The author goes on to discuss Egyptian stone vessels found in LM IA or LM IAB contexts (pp. 115-117). Some of them have been dated to the time of the 18th Dynasty by Peter Warren, Eric Cline and Jacke Phillips. In the case of contexts that were specified as LM I, the author always takes the latest possible date for granted. Where this is not possible, as in the case of an alabaster jar from Knossos[24]) in an undisputed LM IA context, the author dismisses the New Kingdom date *"which could as easily be from a SIP vessel instead."* The reasons for this change are not discussed. The subject is just dismissed. No effort is put into assessing either the relative dating of the contexts more precisely or to assessing the typology. In such methodological premises the conclusion is not that credible: "*As noted, it is the mature LM IB period which witnesses the arrival of clear early 18th Dynasty imports"*. This is followed by a selection of 18th Dynasty objects from LM IB contexts from the catalogue of Cline. One wonders whether, in such a case, all the fragments of

18) I. Hein, in: Bietak Hein et al., *Pharaonen und Fremde, Dynastien im Dunkel* (Catalogue of an Exhibition in the City Hall of Vienna), Vienna 1994, 261, no. 359

19) L. Renaudin, Vases préhelléniques de Thera à l'école française d'Athènes, *Bulletin correspondance héllenique* 66 (1922), fig. 29, pl. 13.

20) Bietak, *BSA* 95 (2000), 192; Bietak, Dorner, Jánosi, *E&L* 11 (2001), 37, 41, section drawing fig. 6: no. 7. — The amphoriskos and its context was presented in detail by Irmgard Hein during the Third Synchronisation conference in Vienna from Mai 28 — June 1, 2003 and will be published in the proceedings of this conference

21) Hein in catalogue (cf. n.18), 245, no. 314, there still a date of early 18th Dynasty is given. This rhyton belongs, however, to a whole group of small vessels with parallels from the time of Hatshepsut and Tuthmosis III: op.cit. 244; ead. (n. 18), 550-552, figs. 1-2. Full publication of this context by I. Hein in preparation in the Proceedings of the Synchronisation conference 2003 in Vienna.

22) R. Koehl, Minoan Rhyta in Egypt, in: *Κρητη-Αιγυπτος*, ed. by A. Karetsou (Catalogue for an Exhibition in the Herakleion Museum 2000), Athens, 94-100

23) On the context: M.Bietak et al. *E&L* 11 (2001), 38-45; M. Bietak & N. Marinatos, in: *The Seventy Great Mysteries of Ancient Egypt*, ed. by B. Manley, London 2003, 166-169.

24) E. Cline, *Sailing the Wine-dark Sea,* BAR (IS) 591, Oxford 1994, no. 269.

calcite vessels can be dated that categorically, but the author is certainly right in thinking that either the calcite vessels are difficult to date because of their state of preservation or the circumstances surrounding their finds leave much to doubt.

An important cornerstone in Manning's synchronisation between the late Cypriot Bronze Age ph. LC IA:2/IB is a BR I jug found in what is called a "*Secure SIP context at Memphis.*" It was therefore repeatedly cited and discussed in this book to back up the high chronology (p. 120f., 167, 206, 254, 325). It was mentioned in articles by Janine Bourriau and attributed to the S.I.P. (lit. n. 552). Her early date of the BR I ware in Memphis was used again by Manning and Merrillees in favour of a high chronology.[25]) The context is still not published, but an examination kindly offered by Janine Bourriau to Dorothea Arnold, David Aston, Irmgard Hein, Perla Fuscaldo, Bettina Bader and Manfred Bietak showed that associated finds included sherds of the 18th Dynasty. The latest sherds date the context. The vessel was found according to a section wall within sharp-sloping New Kingdom deposits and therefore had come to rest near a S.I.P. wall. Nowhere in Egypt has BR I ware appeared in S.I.P. contexts. The major quantitative investigation at 'Ezbet Helmy leaves no doubt that the earliest the BR I Ware appears in Egypt is in the Tuthmoside Period (i.e. after 1500 BC). As a result, this single context has neither been attributed any credibility among ceramic specialists working in Egypt. The author was aware of the revision of this dating (p. 120). As the original date favoured his theory, he tries to dismiss the new evidence that leading Egyptology ceramologists were involved in producing.

P. 35 and pp. 150-191: One of the key-issues of the book is the debate on the chronology of the Late Cypriot WS I Ware. Space does not within this review permit citation of the numerous and repeated misrepresentations on those pages. It will suffice to concentrate on the main issue.

WS I ware did not make an appearance in Egypt before the conquest of Avaris (±1530 BC), or anywhere before the Tuthmoside Period (from c. 1500 BC onwards). This ware was, however, present on Thera before the eruption (high chronology 1628 BC at that time, now c. 1645 BC), leaving a time difference of over 100 years. Cutting this down and allowing the WS I ware to appear in the late Hyksos Period (str. D/2) could contract the time difference, especially with a "flexible" stratum D/2-chronology. The author tries to explain the time difference by the time-lag of Late Cypriot ceramic production in the southeast of the island. It was from there that Cypriot exports arrived in Egypt.[26]) But LC styles originated in North-western Cyprus where WS I and BR I developed first.

However, the development of Cypriot imports within the stratigraphy of Tell el-Dab'a does not simply follow the pattern of Eastern Cyprus which was Egypt's major contact zone on the island where typical Middle Cypriot (MC) III productions like WP III-IV were prevalent until Late Cypriot (LC) IA with only a few imports from the northwest of the island. Yet the forms and shapes did adopt some of the new morphological conventions, such as flat bases and the production of softer wares. Early Late Cypriot forms from the northwest such as Proto White Slip (PWS) and Proto Base Ring (PBR) are only rarely to be seen in the East.[27]) The adaptation to the LC forms, first produced in the northwest, follows abruptly with LC I B in the sudden adoption of BR I, WS I and Mo Wares.

There is no trace of any such development at Tell el-Dab'a where the main Middle Cypriot products, such as WP III-IV, are prevalent as imports between ph. G-E/1 and early D/3. In D/3 and D/2, WP III-IV is uncommon and can be regarded as redundant. RoB — which is a product of the Karpas, located in the northeast of the island — is much rarer but does show up from phases F till D/2 at Tell el-Dab'a. WP V, typical of the late MC Bronze Age, appears in ph. E/1 and flourished in ph. D/3-2 (i.e. nearly throughout the Hyksos Period). Plain White Ware is present only in ph. D/2; WP VI starts with ph. D/2 and continues till the first half of the 18th Dynasty at that site. With D/2, the late Hyksos Period, the LC forms and shapes appear as BIHM and BIWM with linear design and an array of PWS.[28]) In Egyptian terms, this reflects the age of LC IA:1. There is, however, no PBR. BIWM, now with complex or with figural design, WP VI and PWS continue in the early 18th Dynasty (ph. D/1.1-2). It is only in the Tuthmoside Period (ph. C/3-2, 15th cent. BC) that WS I, BR I and RLWM make an appearance in the stratigraphy. At that time, with the sole exception of WP VI, Middle Cypriot pottery was a thing of the past. It had disappeared completely with D/2.

Whilst the Cypriot assemblage is not complete, it nevertheless reflects a chronological development that was going on in Cyprus. Although, as R. Merrillees and, following suit, S. Manning has pointed out, what Tell el-Dab'a shows is mainly the development of the south-eastern part of the island; it is not merely a mirror of this region. Only terminal MC products like WP V and especially the VI overlap with the earliest LC forms and shapes. The presence of PWS — which is rather uncommon in eastern Cyprus — could be seen as a sign that there were also contacts either with the north or the south at a period when WS I and BR I were not yet produced. The PWS + WP VI succession, followed by WS I + BR I from north-western Cyprus, is repeated in southern Cyprus, as can be shown in Maroni and Kalavassos and in North-Eastern Egypt, most probably with some time-lag. Yet this duplication of the same sequence as in northern Cyprus could have survived only a short time span, perhaps one or two decades afterwards, certainly not seventy years or more than a hundred. After such a prolonged period, the chronological differentiation between a succession of first appearances of specific Late Cypriot wares would not have repeated itself. Manning's model of explanation is not-feasible, the more so because he, among others, has lifted the eruption date to c. 1645 ±7 BC, whilst the date of first appearance of WS I ware in Egypt moved to the Tuthmoside era. This pushed up the time difference between his model chronology of northwest Cyprus and Egypt up to 125-150 years.

Neither is the utter lack of contact between northwestern Cyprus and Egypt credible in the light of the special links

[25]) J.Bourriau, in: Studien Altägyptischer Keramik, ed. by D. Arnold, Mainz 1981, 32; R. Merrillees, in: *Proceedings of a Conference of the Swedish Academy of Letters in Stockholm*, ed. by P.Aström, KVHA Konferenser 54.Stockholm 2001, 26, quoting as support Manning, p. 92

[26]) R. Merrillees, *Levant* 3 (1971), 72-77..

[27]) Ibidem, 72-74.

[28]) M. Bietak & I. Hein, in: *The White Slip Ware of Late Bronze Age Cyprus*, ed. by V. Karageorghis, CCEM II, Vienna 1990: 171-194; idem, in: *Proceedings Stockholm*, ed. by P.Aström, (cf. n.25), 175-201, 231-247.

between Tell el-'Ajjûl and northwestern Cyprus.[29]) This site is the first harbour town between Egypt and the Levant and was controlled by Egypt during the Hyksos Period and the 18th Dynasty. It is unthinkable that the relationship between northwestern Cyprus and Tell el-'Ajjûl was not passed down to Egypt. Secondly, late northwestern Middle Cypriot and Late Cypriot pottery were also found, at least in small quantities, at Tell el-Dab'a.[30]) The author himself (p.162-163) suggests that the collections of Tell el-Dab'a/'Ezbet Helmy include material which, in his estimation, seems to be of the early WS I production in northwestern Cyprus. None of those sherds were found in Hyksos contexts. Most of the sherds were from post-NK contexts. The biggest mistake methodologically, however, and therefore unacceptable, is the fact that, in every case, he expresses the opinion that they were likely to be redundant sherds (which is correct) originating from SIP contexts (which is unproven), especially sherd no. 8894 F that is from an 18th Dynasty pit. Such wishful ruminations take up over one page. The point has to be made in this connection that, as a result of discovery of the two early 18th Dynasty phases D/1.1 and D/1.2 (strata e/1.1-2), the first appearance of WS I pottery was moved to the Tuthmoside Period. Not a single WS I sherd has ever been found, even in those early 18th Dynasty strata.

Neither are the origins of the Thera bowl from northwestern Cyprus proven, nor is there substantiation for its date at the beginning of the WS I series. Except for Celia Bergoffen, leading experts such as Merrillees, Karageorghis, Kathryn Eriksson (who has just completed a monographic study of WS pottery) and others have expressed a different opinion. In their view, the Theran as well as other Aegean bowls as that of Phylakopi may even be from southern Cyprus from where they have found good parallels to back up such a theory.[31]) Their opinion also differs terms of the chronological position of the Theran bowl.[32]) Little of Manning's theory explaining the time difference of the WS I ware between the Aegean and Egypt/Levant is supported by experts in the field, leaving us with the irresistible conclusion of failure of the archaeological part of the evidence of an Early Chronology of the Theran eruption.

Trying a test case against Manning's relative chronology, we only have to take a look at a context of a piriform 1 jug of the Tell el-Yahudiya ware/Lisht ware with incised figural design at Toumba tou Skourou (north-western Cyprus). It is a well-known type, mainly produced in Egypt that can be well dated to the time shortly before the Hyksos Period (Tell el Dab'a str. E/3). According to the chronology proposed by Manning, this jug should be from a period going back to the Late Cypriot Bronze Age (at least phase I A:2). It turned up, however, in a pure MC III context.[33]) This is the more remarkable as the northwestern part of this island only occasionally had dealings with Egypt. As a result, the claim cannot be made that there is a minimal difference between production and burial date. A burial date at least in the second half of the 17th century (Early Hyksos) has to be anticipated. This is proof that MC III lasted at least into this age which goes right against Manning's chronology.

P. 134 Some mistakes have also to be set straight with respect to the author's discussion of the Tell el-Yahudiya Ware. Again and again, there are inaccuracies. For example, the author writes that LC IA corresponds to str. D/3-2, instead of str. D/2 of Tell el-Dab'a onwards. More important is, however, the point made that, at Tell el-'Ajjûl, City 3, Palace I, there is no Tell el-Yahidiya Ware. According to the author, following a suggestion of E. Oren, this is a sign that there is no Tell el-Yahudiya Ware in circulation at the latest phase of the Middle Bronze Age. The author sees it as an important point to argue that this site — that he claims had exclusive trade dealings with northwestern Cyprus — had no contact with Egypt where this ware flourished till the end of the Hyksos Period. This is, however, illogical from many different viewpoints. There is Egyptian pottery in Palace I of Tell el-'Ajjûl. We should particularly mention a water jar (Zir, type VI) that definitely belongs to the time of the 18th Dynasty and should be seen as an indication that Palace I dates back as late as to the New Kingdom.[34]) This would explain why there is no Tell el-Yahudiya Ware around any more, whilst the presence of BR I and WS I Ware dovetails with the first appearance of those wares in Egypt in the 18th Dynasty. We still have to work on dating City 3, but not adopt without critical review the dates given by Flinders Petrie.

Pp.32, 145-140. Sturt Manning argues vehemently against the first appearance of pumice from Thera at 18th Dynasty and Late Bronze Age levels as evidence of a low chronology of the Thera eruption, scoring — as he does — unjustified controversial points in his line of argument. At p. 32 the author tries to explain away the relevance of numerous Theran pumice samples found at Tell el-Dab'a: *"…pumice from Thera (and subsequently positively identified as such in some cases) in what was said to be early 18th Dynasty context. Since this was the only time such pumice was found at the site, Bietak argued that this indicated an early 18th Dynasty date for the eruption of Thera. The fact that this really only set an undefined and unquantified terminus ante quem was ignored."* In actual fact, it was not ignored at all and the purpose of the author's distorted presentations has to be questioned again and again. On the contrary, the bulk of the pumice — not just *"some"* — was analysed and identified. See Bietak 1996: 78 "*Adherents of the high chronology for the explosion of Santorini have suggested that this pumice may have lain in the vicinity of Avaris for a long time and was only picked up during the time of the New Kingdom. This is possible as the materials retrieved in H/I and H/III were collected in workshops."*

Lumps of pumice have indeed been found at Tell el-Dab'a, mainly in workshops where they were used as abrasive material.[35]) As a result, they could have been gathered up or imported (according to Manning) in trading exchanges, depending on demand, even long after the eruption. Such an explanation could work for a single site, although it may be

[29]) C.Bergoffen, in: Karageorghis (ed.), (cf. n. 28), 154.

[30]) A WP V bowl from TD was identified by Merrillees as north-western. On the WS I sherds from TD, some of them, even according to Manning, are from the north-western part of the island (Manning, pp. 162-163.)

[31]) R.S. Merrillees, in*: The White Slip Ware of Late Bronze Age Cyprus*, ed. by V. Karageorghis, CCEM II, Vienna 1990, 93; cf. V. Karageorghis, *Tombs at Palaepaphos 1. Teratsoudhia 2. Elimylia*,27, Nicosia 1990, pls. VI; XV: E.11..

[32]) Merrillees, previous note; V. Karageorghis, *Tombs at Palaepaphos,* 57-60.

[33]) E.D.T.Vermeule & F.Z.Wolsky, *Toumba tou Skourou, A Bronze Age Potter's Quarter on Morphou Bay in Cyprus*, Cambridge/Ma. 1990, 296, T V.24, pls. 182-183

[34]) I am grateful to Karin Kopetzky for pointing out this context to me.

[35]) Jánosi, *E&L* 4 (1994), 35, pl. 10; Bietak et al. *E&L* 11 (2001), 89-96.

wondered why, after such a long time excavating at Tell el-Dab'a, no Theran pumice from the Minoan eruption has been found at Hyksos levels. Theran pumice suddenly makes an appearance in large quantities at 18th Dynasty levels from phase C/2 onwards. Thus it must be dated to the Tuthmosis III period. At the Hyksos and early 18th Dynasty levels, pumice is very rare and does not originate from Thera, but from older eruptions such as those of Kos, Gyali or Nisyros.[36])

Moreover, this pattern of first appearance repeats itself at other sites. There is Theran pumice from New Kingdom levels at Tell Hebwa in northern Sinai. Theran pumice was also found in larger quantities at level H 5 at Tell el-'Ajjûl together with the first appearance of WS I, BR I, RLWM, in combination with Egyptian Marl B pottery approximately of the time of Hatshepsut and Tuthmosis III.[37]) In the same time-range Theran pumice also appears at Tell el-Dab'a. The excavations at Ashkelon and Tel Na'ami have boths turned up Thera pumice only from the Late Bronze Age onwards, whilst all pumice from Middle Bronze Age strata is from other volcanoes.[38]) The appearance of Theran pumice at a number of sites near the seashore suggests that lumps of pumice were suddenly available in huge quantity. Lots of pumice can still be gathered up along an ancient seashore near Tell Hebwa. Investigation into the first appearance of Theran pumice in archaeological stratigraphies is not yet finished, but the trend seems to be hardening in favour of an eruption at the time of the early New Kingdom in Egypt.

Chapter V, "Absolute Dating Evidence", follows the author's stress on scientific dating that he obviously uses as his only serious guideline. In order to be provocative, historical dating and astrochronology are left out under this heading., This is a move the author uses to show his supreme confidence in his views, showing a distinct distrust of historical dating i.e. "dead reckoning" from fixed points in the middle of the first millennium BC backwards (for example the conquest of Egypt by Kambyses at 525 BC, or the beginning of the 26th Dynasty at 640 BC). It is true that historical dating has to rely on incomplete sources and inaccurate estimates. Nevertheless, the raft of regnal and genealogical data, together with synchronisms with the Mesopotamian, in particular the very accurate Assyrian chronology, makes for a reasonable degree of cross-checking to help produce a chronology of the New Kingdom that, according to all the experts involved, is accurate to within ±10 years.

It is a great illusion to believe that sciences are more reliable, at least just now, in obtaining absolute dates. Manning, who is undoubtedly very knowledgeable in this field, deals extensively with the problems of radiocarbon chronology, although not with all the problems. It is astonishing that he has so much confidence in proposing a 14C chronology for the Aegean, when the data available are so limited, and limited even more by the subjective selection process of the author's.

The major problem is that radiocarbon years do not correspond to calendar years due to unsteady cosmic radiation and uneven absorption of carbon 14. The discrepancy between radiocarbon years and calendar years is explored by calibration, i.e. by taking from specific series of well-dated trees, every decade or every second decade, a sample that is subjected to measurements. This process as such involves a lot of interpretation of the measurements obtained. In the graphic display of radiocarbon years moving towards calendar years, the curve is not steady but oscillates, sometimes more, sometimes less, depending on the fluctuations of radiocarbon absorption. A major problem is not only the conversion of the radiocarbon dates, but also the conversion of error thresholds to calendar dates. This can be particularly difficult in centuries when the oscillations of the calibration curve are strong. In such periods, standard errors increase very much and leave the evaluation to statistics and further interpretation. Unfortunately, the likely period of the Thera eruption — the 17th and the 16th centuries — belongs to such oscillation periods, leaving us to wonder why the author sets so much emphasis on radiocarbon results when the period in question is so problematic and open to error. We shall also see that the volume of suitable samples is insufficient and the way they are selected by the author could well be open to criticism (see below).

Wiggle-matching by itself is a highly interpretative evaluation process of 14C data and is likely to create an image of precision that, in its turn, could be deemed problematic. Other problems of 14C are the inconsistency of data caused by different benchmarks used over time and the discrepancies between different laboratory conditions. Any attempt to assemble a consistent set of dates rarely gleans from one site — or from one specific stratigraphic context — a sufficient number to clearly overcome random results. At the moment, the preferred sample is short-lived as fully carbonised seed, albeit at radiocarbon level, and is likely to deviate from the main trend considerably. Fragments of wood are likely be old and re-used when deposited. In the Orient, they may be more than a hundred years old by the time they were used. Such samples may produce dates lagging well behind. Small twigs are, however, suitable samples.

Another complication stems from redundant samples. As we have seen at multistratified sites, redundant sherds moved upwards by foundation trenches or storage/waste pits also make it feasible that organic substances are transported upwards from lower levels by human settlement activities. It is not every excavation that reveals *in situ* storage jars with grain inside. The number of samples has to increase considerably to help extrapolate such redundant carbonised matters.

Contamination and pre-treatment in laboratories can be looked upon as yet another complication which has not yet been sufficiently resolved. This is particularly true of uncarbonised or not sufficiently carbonised seeds that may be unfit for reading as a result. Another effect on samples may come from gases emitted from a volcano as the one from Thera, a complication known to research and acknowledged by the author but dismissed as negligible in the case of Akrotiri. A serious statistical problem is dismissal of results considered to be outliers. This human intervention spoils the degree of accuracy in the statistics of radiocarbon dating.

The author does not baulk at mentioning most of those dating problems. He also tells the sad story of the numerous samples collected from Akrotiri that are gauged by a variety of laboratories with highly inconsistent results. Controverting the dismissal mentioned above, he also asks the question whether the burial of the town of Akrotiri under metres of pumice did not create a soil chemistry different from normal contexts. P. 237: "*The samples could easily have exchanged carbon with permeated water*" . Anyway, all of the previous

[36]) M. Bichler et al. *E&L* 12, 66-67, Tab. 2 and 3.
[37]) P. Fischer & M. Sadeq, *E&L* 12 (2002), 125-129, 138-141 (tabs. 1,3).
[38]) Cf. n.37

samples were discarded. Only four samples that, according to the Copenhagen Laboratory, were up to standard as they were fully carbonised, ended up being considered. They range in carbon years between 3310±65 and 3430±90 (120 years apart). After conversion with the INTCAL 98 calibration with the Oxcal calibration programme, the evaluation shows three peaks, one in the late 18th, one filling the whole 17th and a final peak filling the period between 1590-1520 BC. The older Seattle 1993 calibration dataset produced five peaks, the last one in the 16th century BC being the strongest. Under such circumstances and premised on such a tenuous statistical basis, Manning says: *"We may therefore conclude that the good quality radiocarbon data presently available from Thera … cover both the Aegean 'early' and 'compromise early'* (by this he means the moderate low) *chronology. A date after c. 1530/1520 BC is impossible. The steep slope in the radiocarbon calibration curve after 1535 BC and especially 1525 BC, reflecting rapidly changing radiocarbon levels in the atmosphere at this time, takes the traditional calibration curve decisively away from the Thera age range. Thus the traditional Thera eruption c. 1500 BC is ruled out — and the recently proposed date for the eruption between 1515 and 1460 BC is simply not possible."*

Such a strong statement, based on four handpicked samples and dismissing samples which do not fit the high chronology as OxA-1557, seems to be propped up on weak and biased foundations, especially when considering the many problems encompassing the radiocarbon chronology in a place like Thera. Far more caution and patience are needed, especially as the drop of the calibration curve between 1525 and 1515 BC can be considered the result of an artificial construction.[39])

The dates at the end of LM IB at Chania and Myrtos Pyrgos are re-examined to help find more backup for the problematic Thera 14C data. Unfortunately, the average dates of the two sites that should be similar are about seventy radiocarbon years apart. Instead of considering individual site conditions, the author tries to bring the two median dates together by accommodating them at the time of the sharp drop in the calibration curve between 1526-1494/1487 BC. In view of the problem of this part of the curve (see previous paragraph) the conclusion of the author that LM IB must lie within the 16th century BC, not even in the end of the 16th century, has to be treated with caution. Manning then goes on to deal with three Oxford data from deposits of MM III, early LM IA and LM II from Kommos (pp. 250-252). Again, there is no objective treatment of this material. The three dates cover wide time-spans (from the early 18th until the middle of the 12th century in the one sigma reach). The theoretical time-range of the MM III sample until the 16th century BC is dismissed because of the Chania/Myrtos LM IB dates. As a result, an 18th and 17th century date is suggested. In the light of the problems of the Chania and Myrtos dates that are as such a result of interpretation, this is a doubtful approach from a methodological point of view. The LM IA sample is from a wooden log which might be old and re-used. Manning suggests a 17th century date, although both calibration curves allow a spectrum of up to the end of the 16th century.

[39]) M.H.Wiener, Time out: The Current Impasse in Bronze Age Archaeological Dating, in: Metron, Measuring the Aegean Bronze Age, ed. by K. Polinger Foster and R. Laffineur, *Aegaeum* 24 (2003) 392.

A similar subjective evaluation in dealing with results is noticeable in a set of three LM II destruction samples of charred barley from the "Unexplored Mansions" at Knossos. Two fall into the 15th century (1σ 1515-1436 BC, 2σ 1532-1394 BC). The third is of more recent date, making the author suggest that he should take it out of his final evaluation (OxA-2096). Leaving out one of only three does indeed amount to influencing the results, although the Kommos sample OxA-3674) of LM II with 3090±80 BP is very similar to the sample of 3070±70 BP. All in all, these samples would not be at odds with the low chronology of LM II of Hankey and Warren (1425-1390 BC).

In the meantime the author has collected more Aegean short-lived samples. The results continue to produce data of the Minoan eruption (more specifically pre-eruption layers either in the 17th or in the 16th century BC, latest 1530 BC). It is well known that 14C is about 50-100 years higher than the historical dates of the New Kingdom that, for the time being, leave only little scope for discussion. As a result, at the present stage of research, it seems wise not to mix historical with radiocarbon chronology, but to use them in opposition to each other until such time as the phenomenon of divergence can be better understood and explained. Such an approach on its own could be helpful in setting up historical dimensions in time. Unfortunately, this is not done by the author who tries an impossible balancing act by pushing historical dating upwards, as near as possible towards 14C — chronology in its higher time-range instead of trying to harmonise the two systems within the moderate low chronology with an eruption date mid-16th century, at the latest before 1530 BC. In this book he feels confident about doing so as a climatic event in the northern hemisphere at 1628/27 BC is clearly visible in the dendro-chronological data of North America and northern Europe. The author was one of the main advocates of tying this climatic signal to the aftermath of the eruption of Thera. He claims (pp. 268-272), contrary to Sigurdson et al., that such an eruption as the one at Thera must have had a sufficient sulphur emission to have impacted on the tree growth in the northern hemisphere. In this book he tries to make a link with the very distinct sulphur acid signal in Greenland ice, dating to ±1645/44 BC. This date was compatible at the time with the dendro signal at 1628/27 BC. Meantime, the Danish team under Claus Hammer has been able to convince everyone that the accuracy of dating is only ±7 and now only ±4 years, making this ice signal incompatible with the dendro date of 1627/28 BC. The Danish team also produced particles of volcanic glass shards from the ±1645/44 BC horizon. Examination by SIMS at first suggested the likeliest identification with the Theran eruption material.[40]) Is it a coincidence that Manning and others have recently abandoned the potential eruption date of 1628/27 BC for which he makes such a strong case in this book and, together with P.I. Kuniholm et al., changed to the ±1645/44 BC date like Hammer et al.?[41]) The reasons given are that the wiggle-matched floating Anatolian Dendrochronology would have fit better 22 years backwards than using the approach of identifying a strong Anatolian dendro-anomaly

[40]) Hammer et al. supra, n. 4.

[41]) S.W.Manning, B. Kromer, P.I. Kuniholm & M.W. Newton, *Science* 294 December 21 (2001), issue 5551, 2532-2535; Manning, S.W., C.B. Ramsey, C. Doumas, T. Marketou, G. Cadogan, C.L. Pearson, *Antiquity* 76 (2002), 733-744

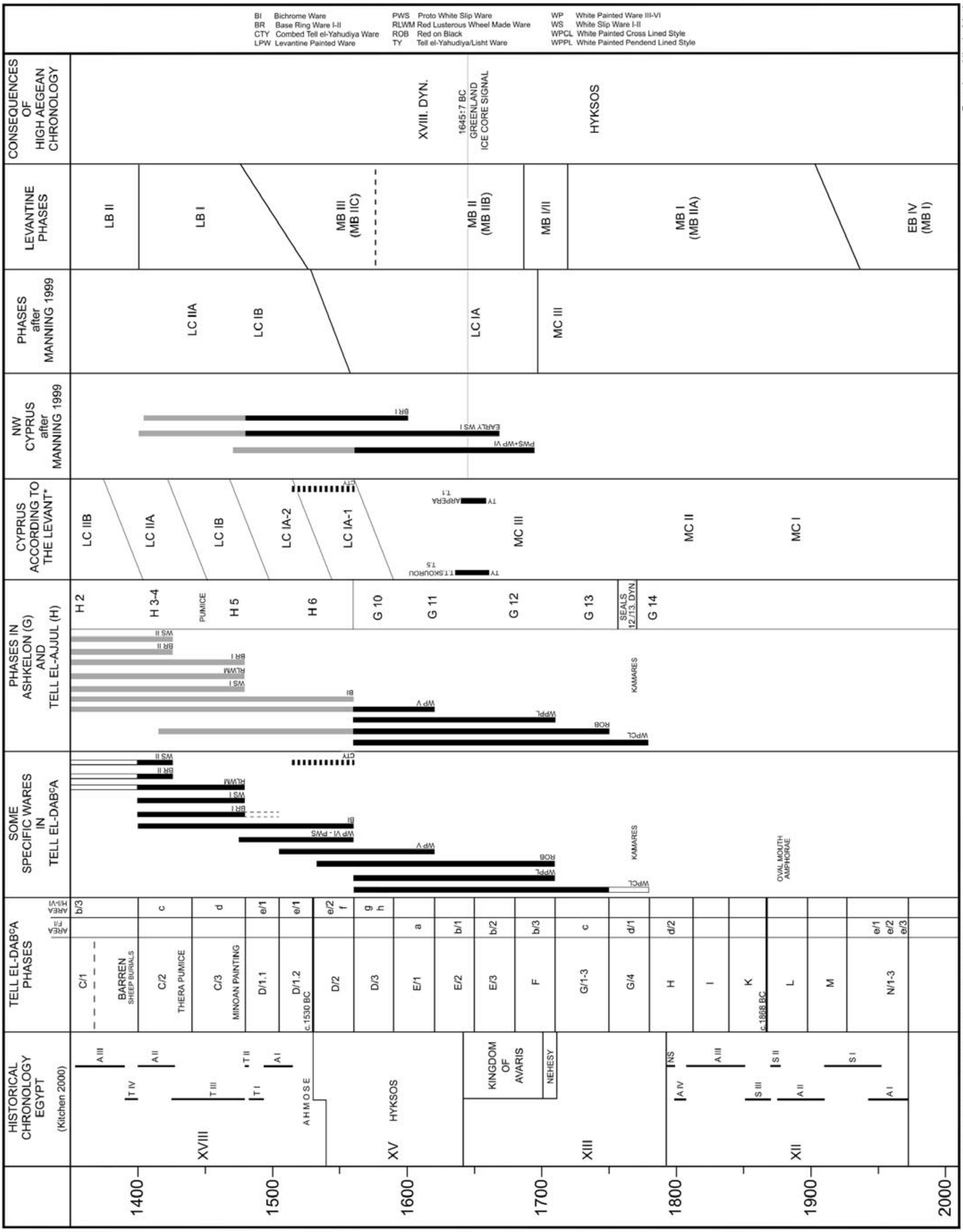

Fig. 1. Graph showing gap between historic-archaeological dating of complexes in the eastern Mediterranean and the dating preferred by Sturt Manning

with the 1628 BC frost anomaly of northern hemisphere. However, raising the supposed eruption date of Thera 22 years would make the author's attempted harmonisation between scientific and historical chronology even more untenable as it is already with the eruption date of 1628/27 BC in this book.

The move to ±1645/44 BC may well be premature, as it does make the whole scientific interlinking theories presented in this book more vulnerable to criticism and open to doubt. The eruption is no longer distinctly anchored in the dendrochronology. The new Greenland ice data is by no means established in fact and has since met with scepticism from scientists. New analyses make it more likely that the particles in question originate from the Aniakchak volcano in Alaska.[42])

The author's conclusions as commented on and criticised above are repeated in the chapter VI: Summary and Conclusions (pp. 321-340), making it unnecessary to comment on them again. This is followed by a chapter VII on the chronology of Alalakh, level VII. The logic is sometimes difficult to grasp. In particular, it is entirely beyond this reviewer's understanding why VI B level is supposed to support the high chronology with the first appearance of WS I and BR I ware. As in Egypt those wares do not appear before the end of the Middle Bronze Age at the end of Alalakh VII. They are not to be found in layer Alalakh VI A and do not start until Alalakh VI B. Even this attribution is questionable as the single deposition is considered doubtful. Nearly all other BR I and WS I sherds came from level IV together with RLWM ware.[43])

In his elaborate discussion on independent Anatolian chronology, the author discusses the date of the Ulun Burun shipwreck. This date was determined by comparison with floating Anatolian dendrochronology, considered at the time to have been firmly tied up with the Dendrochronology benchmark of northern Europe when a tree-ring anomaly was thought to be the one from 1628/27 BC besides a second minor anomaly reckoned to be the one from 1159 BC of northern European Dendrochronology (Manning fig. 63). Despite this seemingly cogent tie-up, it is no longer considered conclusive and, together with the integrated junipers from what is called the Midas Mound at Gordion, had to be moved back 22 years when the tree-ring anomaly would interface with the new ±1645/44 BC eruption date. Despite this lifting of the date not yet considered in the theory of this book, a critical note has to be added on the confident way the eruption date was originally constructed by dendrochronology and is now reconstructed. It might be deemed methodologically wrong to compare different varieties of wood originating from different regions, as in this case. The ecosystem could be expected to be different. Also, different kinds of trees react to climate in different ways. The juniper is from Central Anatolia, from an eco system far away from the one housing cedar. It is also by no means certain, nor even likely, that the cedar wood from the Ulun Burun wreck comes from inner Anatolia. It is likely to be from a region near the seashore. It could be Lebanese, Cypriot or Amanus cedar. Secondly, there is no possibility to verify the claim made for fitting the Ulun Burun ship with the Gordion tree-rings, either by the data nor in the graphs published. If S. Manning and P.I. Kuniholm want us to believe in those results, they will have to come up with far more detailed data that would stand up to the evidential test. Until such time, there is no evidence for an independent date of this shipwreck. We are still forced to use the artefacts from the ship as a way of dating.

Turning to Appendix 1 *"Egyptian Chronology"*, I refer to K.A. Kitchen's short answer.[44]) Appendix 2 *"Why standard chronologies are approximately correct and why radical re-datings are therefore incorrect"* is unnecessary and it may be wondered whether radical re-dating does not include the unilateral rise of Aegean chronology as the author attempts.

In fairness, it must be said that, at the moment, only few scholars would be able to write such an all-encompassing and knowledgeable account of the problems of the Bronze Age chronology surrounding the eruption of Thera. The author has assembled a tremendous amount of material for which he deserves our admiration. The over-biased treatment of the subject, however, invites much criticism, especially because alternative interpretations are mainly dealt with in a rhetorical way. Meanwhile, the author has moved the fictive eruption date of 1628/27 — the main pillar of his thesis — 22 years backwards, because the floating Anatolian Dendrochronology seems to fit better there (see above). This move may also have been encouraged by the date of the Greenland ice particles dating to ±1645/44 BC and obtained by the Claus Hammer group. In making this move, Manning has probably destroyed any possible link with the present historical chronology that forms a large part of the discussion in this book. He also runs the risk — just in case the Greenland ice particles are not from the eruption of Thera — of making his idea of high chronology rest solely on his interpretation of the ^{14}C data but which also allows a date from the 16th century to ±1530 BC. On the other hand, it may be said that no other book has stimulated to such an extent debate on the chronology of the Thera eruption and of Aegean Bronze Age chronology.

Austrian Academy of Sciences, Manfred BIETAK
October 2003

[42]) N. Pearce et al., Reinterpretation of Greenland Ice-core data recognises the presence of Late Holocene Aniakchak Tephra (Alaska), in print in the *Proceedings of the SCIEM 2000 Euro-conference*, May 28-June 1, 2003 in Vienna.

[43]) C.J. Berghoffen, *The Cypriot pottery from Sir Leonard Woolley's Excavations at Alalakh (Tell Atchana)*, CCEM V (Vienna in print)

[44]) K.A.Kitchen, Ancient Egyptian Chronology for Aegeanists, *Mediterranean Archaeology and Archaeometry* 2.2 (2002), 5-12.

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