36

A point in time

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Chronology is the spine of history.[1]

I. INTRODUCTION

The publication in 1989 of *Aegean Bronze Age Chronology* (henceforth *ABAC*), the monumental work by Peter Warren and Vronwy Hankey, marked a decisive moment in the study of Aegean prehistory. Subsequent discoveries have led to minor modifications to the chronology proposed, amounting to no more than 25 years at any point after 1450 BC in the Late Bronze Age.[2] I have contributed to this genre myself, contending that the Late Helladic (Mycenaean) IIIA2 period required a lengthening in light of a reconsideration of the Mycenaean pottery from the datable Amarna deposit and other Egyptian contexts. I also noted the large amount and wide distribution of LH IIIA2 pottery over the Mediterranean and the depth of LH IIIA2 deposits in various places,[3] only to discover that Peter Warren had already sensed a problem for, as he noted in the 'Postscript' to *ABAC*, 'recent discoveries of substantial LM IIIA2 levels in southern Crete, especially Aghia Triadha, may require this view [of a short IIIA2 period] to be modified'.[4]

ABAC and its progeny have had a wide geographic impact, sending ripples across the Mediterranean. For example, while mainland Italy, Sicily and Sardinia have no Aegyptiaca, considerable Mycenaean pottery, itself datable largely through Egyptian interconnections, is present and critically important for dating the phases of the Italian Bronze Age.[5] The basic framework proposed by Warren and Hankey has had a major impact on the eastern shores of the Mediterranean as well, at places ranging from Troy, Miletus and numerous other sites in Anatolia to the Dodecanese, Cyprus, the Levant and Canaan. In return, Egyptian and Near Eastern texts play a critical role in establishing the absolute chronology of these areas and hence the dating of Mycenaean and Minoan pottery found in them throughout the MBA and LBA.

II. TREE-RING AND ICE-CORE DATING

In recent years, the only major challenge to the *ABAC* general schema has come from studies in fields of science whose relevance is not limited to archaeology, namely tree-ring, ice-core and radiocarbon dating. Warren and Hankey in *ABAC* proposed a date of 1530–1525 BC for the Theran eruption. In 1984, an article in the journal *Nature*[6] reported that the long-lived bristlecone pines of California showed a serious frost year at 1626 BC, and suggested that this might be related to the eruption of Thera. Peter Warren promptly replied with an article showing that many major eruptions had left no record in tree rings.[7] In addition, tree rings sometimes indicate the possibility of an eruption when none was reported in that year.

The 1628–1626 BC proposition received an additional impetus when it was initially reported that tree rings at Porsuk, a Hittite site near the Cilician Gates in Turkey, 840 km downwind from Thera, also showed a major event within those years.[8] Anatolian junipers do not live for thousands of years as do California and Arizona bristlecone and foxtail pines or German and Irish oaks, but it has been possible nevertheless to construct an 'Anatolian Floating Chronology', now about 2009 years long from 2657 to 649 BC, through comparison of overlapping multi-century segments of trees, including logs with a total of 1028 annual rings from the so-called Midas Mound at Gordion.[9] The Anatolian Floating Chronology ends in historically dated approximate anchors at Gordion and in particular in the logs from the temple at Ayanis in Urartu built by Rusa II. An inscription on a single basalt block found near the Monumental Gateway at Ayanis lists this and other structures built by Rusa II, some in areas he

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[1] Noted Danish scholar Rudi Thomsen, cited in Hallager 1988, 11.

[2] Warren 2006; 2007.

[3] Wiener 1998; 2003*a*; 2003*b*.

[4] Warren and Hankey 1989, 214.

[5] Lo Schiavo 2002; Bettelli 2002; Jung 2005; 2006; 2007.

[6] LaMarche and Hirschboeck 1984.

[7] Warren 1984.

[8] The date proposed varies slightly because of the computer calculation addition of a non-existent year between 1 BC and AD 1 and the possibility that the effect of an eruption on tree growth above the 7,000 ft frost line would occur in the year following the eruption (which in turn might depend on the time of year of the eruption in relation to the months of maximum tree growth).

[9] Newton and Kuniholm 2004; Newton *et al.* 2007.

conquered, in what appears to be the correct historical order. The dates of Rusa II's reign, *c.* 685–645 BC, have a sound historical basis.[10]

The subsequent realisation that the Anatolian Floating Chronology in all likelihood required an upward adjustment of 22 +4/-7 years in light of a radiocarbon 'wiggle match' with the absolutely dated European oak chronology, together with additional information about the most likely historical date for the temple at Ayanis, negated the connection to the 1626 BC California bristlecone pine event previously posited.[11] The result of these changes is to put the possible volcanic event indicated by the Porsuk logs at 1642 BC, a date generally consistent with an acid spike in a closely datable annual lamination in a Greenland ice core, possibly caused by an Alaskan, but certainly non-Theran, volcanic eruption, for the reasons given below.

Former advocates of a 1628–1626 BC date for the Theran eruption had argued vigorously that an eruption of such magnitude would very likely have had an impact on the growth pattern of trees 840 km downwind of Thera at Porsuk.[12] There is no indication in the Porsuk tree rings, however, of a potential eruption between the *c.* 1642 event and 1573 +4/-7 BC, when the Porsuk sequence substantially ends. On the premise that a massive Theran eruption would necessarily be reflected somehow in Porsuk trees, the Theran eruption could not have occurred between *c.* 1642 and 1573 +4/-7 BC, a span which includes the dates 1627–1600 BC proposed on the basis of the radiocarbon measurements discussed below obtained from segments of an olive tree branch found on Thera. Of course, as Peter Warren noted, not all eruptions are recorded in trees, directly downwind or not. The response of trees to eruptions or other stressful events depends not only on proximity and wind direction at the time of the eruption, but also critically on whether the eruption occurs during the primary growing season of the trees, the age of the trees and their health, which in turn depends on such factors as preceding weather and soil conditions and on the general state of the climate system at the time of the eruption.[13] Of course the Porsuk tree-ring sequence sheds no light on the 1530–1525 BC date proposed by Warren for the Theran eruption in any event.

A second science-based challenge to the Warren and Hankey chronology (a.k.a. the 'standard chronology' and the 'Aegean Short Chronology') came from the dating and examination of laminations in a number of Greenland ice cores. An article in 1987 by Hammer *et al.* asserted that evidence of a Theran eruption date of *c.* 1645 BC had been found in the form of an acidity spike from one Greenland core.[14] In 2003 a second article appeared asserting that a comparison of chemical elements from a second Greenland source and from the Theran eruption shows that the *c.* 1645 BC Greenland event had been caused by the Theran eruption.[15] In response, advocates of an eruption date of 1628–1626 BC contended that the annual ice-core laminations must have split in places, or a late frost must have caused a second lamination in the ice in a number of years, leading to an unwarranted addition of years. The count had proven correct to within one year for an acidity spike thought to represent the eruption of Vesuvius in AD 79, however, thereby lending credence to the counting method. Rather it was the asserted similarity in chemical composition that posed the critical issue. Because the claim of similarities in chemical composition was based mainly on a comparison of rare-earth elements, I considered only these in concluding that the comparison was far from convincing.[16] Next, Douglas Keenan established that comparison of the composition of the bulk chemical elements in the ice core and the Theran tephra precluded any possibility that the Theran eruption was represented in the Greenland ice core.[17] Nick Pearce further stated that the chemical composition of the ejecta from the eruption of Aniakchak in Alaska, known on independent grounds to have occurred in the 17th century BC, was far closer than the Theran ejecta to the Greenland ice-core composition for the *c.* 1645 BC sample, and that in any event the composition of the Greenland ice core and the Theran tephra was sufficiently different to make the proposed identification invalid.[18] Alan Robock observed that there was no reason to assume that all or most great Northern Hemisphere volcanic eruptions would be represented by acid spikes in every square meter of Greenland ice.[19] Keenan noted that no trace of the great Krakatoa eruption of AD 1883 was present in either the Dye 3 or GRIP cores from Greenland.[20] Finally, Peter Fischer, employing more advanced equipment than had

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[10] Çilingiroğlu and Salvini 1995; 2001; Kuniholm 1996; Manning *et al.* 2001.

[11] Manning *et al.* 2001; Kromer *et al.* 2001.

[12] The proposed effect was the opposite of that observed elsewhere in trees affected by eruptions — instead of stunted growth due to frost damage, the Porsuk trees exhibit a 'growth spurt' indicated by wide rings in the year in question. The hypothetical explanation provided was that whereas trees growing above the frost line would suffer from extreme cold and damp caused by volcanic ejecta in the atmosphere blocking the sun's rays, trees in an arid climate such as prevails at Porsuk would benefit from the reduction in temperature and additional rain which an eruption might produce.

[13] Wiener 2006*a*.

[14] Hammer *et al.* 1987.

[15] Hammer *et al.* 2003.

[16] Wiener 2003*a*.

[17] Keenan 2003. For example, the measurements for silicon abundance (Theran tephra 73.2 ±0.26%; Greenland tephra 69.6 ±0.14%) do not overlap at eight standard deviations.

[18] Pearce *et al.* 2004; Denton and Pearce 2008; Vinther *et al.* 2008.

[19] Robock, pers. comm.; Robock 2000; Robock and Free 1995.

[20] Keenan forthcoming; Clausen *et al.* 1997, table 3.

been available in the initial analyses of Hammer *et al.*, examined the Greenland ice-core lamination of the year following the year examined by Hammer *et al.* with regard to Thera and found no trace of a volcanic eruption whatever, contrary to the expectation that some ejecta would have remained in the atmosphere.[21] Just as there is no viable tree-ring evidence to contradict the Warren chronology, so also there is no viable ice-core evidence (notwithstanding the frequent assertion in the literature that other scientific evidence supports the putative radiocarbon evidence favoring a high eruption date).

III. RADIOCARBON DATING — PROSPECTS AND PROBLEMS

Radiocarbon dating has of course provided the major challenge to the Warren and Hankey chronology. Various papers have proposed raising the date of the Theran eruption by 75–100 years from the 1530–1525 BC date proposed in *ABAC*. Radiocarbon dating is inherently problematic, however, and particularly so with regard to measurements from the volcanic island of Thera. Articles presenting Aegean Bronze Age radiocarbon dates typically remain silent about the major sources of uncertainty while providing dates within narrow ranges, based on sweeping but unstated, and often unwarranted, assumptions.

The first problem comes in the measurement of the amount of ^{14}C in a sample. A recent paper in *Radiocarbon* begins by reporting that '[t]he development of the accelerator mass spectrometry (AMS) technique in the 1980s caused a tremendous increase in the application of radiocarbon' but goes on to report that while the results have been impressive,‘4 severe problems kept occurring:

1. Increased and more variable background levels;
2. Mass-dependent ^{14}C/^{12}C ratios, and thus [the necessity of] normalization [to offset to the extent possible the differences in mass];
3. Lower precision of the ^{14}C measurement;
4. Considerable decrease in the success rate of measurements'.[22]

Significant differences are sometimes observed in successive measurements of a single seed cluster in a single laboratory, and inter-laboratory measurements of samples divided between them provide inconsistent dates with some frequency, if sometimes within broad measurement uncertainty bands. For example, Manning *et al.* in an article published in 2006, report that:

> [o]verall, comparing the Oxford versus Vienna data on the same samples…, we find an average offset of -11.4 ^{14}C years. The standard deviation is, however, rather larger than the stated errors on the data would imply at 68.1 [uncalibrated radiocarbon years]. This indicates that there is an unknown error component of 54.5 ^{14}C years'.[23]

Critical measurements of seeds from the Theran Volcanic Destruction Level furnish a good example, with central measurements for two seed clusters of barley 97 years apart and central measurements for two seed clusters of peas 215 years apart. These particular disparities were addressed by averaging the anomalous measurements and asserting a tight chronological range for the eruption based on the number of measurements, without regard to their inconsistency.[24] Unfortunately this logical flaw is currently embedded in the algorithms of the OxCal and other major radiocarbon computer calibration programs — the greater the number of measurements of any nature, the narrower the error range asserted.[25] While the contradiction is almost never acknowledged, it has not gone entirely unnoticed. In a recent study involving hundreds of radiocarbon measurements in Israel, Prof. T. Jull, the Director of the NSF-Arizona AMS Facility offered the following caution: 'Note, however, the fact that combining measurements always reduces the error estimate (regardless of how similar or dissimilar the actual measurements are!)'.[26] The extremely narrow one- and two-sigma error estimates often provided as a result of such averaging (sometimes as low as ±13 or ±16 years, for example) should not be taken literally, even apart from the other sources of uncertainty discussed below. Instead we should heed the advice of Aristotle to look for exactitude in each class of things only so far as the nature of the matter allows.[27]

The second major problem of radiocarbon dating is posed by the imperfections and uncertainties of the calibration curve. Because the production of ^{14}C in the atmosphere is irregular as a result of variations in solar activity and hence cosmic rays (whose interaction with nitrogen produces ^{14}C), it is necessary to calibrate the measurements of ^{14}C in archaeological material against the amounts of ^{14}C in long-lived trees of known dendrochronological date. Unfortunately the process is problematic. Some of the decadal or bi-decadal measurements were made a generation ago using now-outdated equipment, a few have been identified as

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[21] Fischer and Whitehouse 2004; Denton and Pearce 2008; Vinther *et al.* 2008.

[22] de Rooij *et al.* 2008, 413.

[23] Manning *et al.* 2006, 5.

[24] Manning *et al.* 2006; Wiener 2009*a*.

[25] For example, the recent measurement of two cattle bones found at Palaikastro in East Crete described as coming from a tsunami-disturbed level associated with the Theran eruption produced measurements of 3390 BP ±35 and 3310 BP ±35 (Bruins *et al.* 2008). The measurements do not overlap at the one-sigma error range. Nevertheless they were averaged, and the average date of 3350 BP was given an error range of ±25 because there were two measurements.

[26] Sharon *et al.* 2007, 9.

[27] *Eth. Nic.* 1094b 23–7.

wholly incorrect,[28] and in a good many cases adjacent decadal samples gave dates thirty to seventy years apart.[29] The INTCAL04 Calibration Committee accordingly recommended that the Gaussian bell-curve-derived estimates of measurement accuracies should be multiplied at the one-sigma range by 1.3 for the Seattle measurements and 1.76 for the Belfast measurements on German oak. The INTCAL04 Committee further decided to limit the impact of error in any particular decadal measurement by smoothing the calibration curve through incorporating into each decadal determination the measurements of the nearest 100 data points or observations, whether these observations came from repeated measurements of the same decade from the same piece of wood, the same decade from multiple pieces of wood, semi-decadal or decadal measurements, or measurements from individual annual rings within a decade. Accordingly the time span incorporated into each decadal determination can vary significantly depending on the density of the observations at a given point.[30] The concept of smoothing has proven controversial, however, and while retained in INTCAL09 is scheduled for reconsideration along with other statistical issues in the next two years.[31] Many additional measurements of five-year tree segments for the period 1700–1500 BC by Kromer *et al.*[32] will greatly improve the quality of the calibration curve for this period, without however alleviating the problems caused by the oscillation of the curve or the difficulty of comparing seed and tree measurements discussed below.

Radiocarbon measurement of a seed may represent a growing season of three weeks within a plant whose growing season is between six months and one year, whereas a measurement of a five- or ten-year oak tree segment may, for example, over-represent one or two years of abundant moisture and rapid growth resulting in wide rings which do not include the year of the life of the seed.[33] The 11-year sunspot cycle affects the production of radiocarbon in the atmosphere, which in turn affects single-season seeds and five-to-ten-year tree segments differently. In addition, the longer the calibration curve tree segment, the greater the likelihood that it will be affected by the 80- to 90-year Gleissberg sunspot cycle. The combination of intra- and inter-year variability in radiocarbon measurements means that the calibration of seeds against multi-year segments of trees is inherently problematic, even apart from the anomalies of calibration-curve measurement noted above and the regional and seasonal factors discussed below.

Some studies purport to account for such factors by increasing the standard deviations employed, but it is far from clear that the small increases proposed are sufficient in this regard. Perhaps the greatest challenge posed by the calibration curve, however, is its irregularity. For the period *c.* 800–400 BC, the 'curve' is flat; as a consequence, it is impossible to distinguish between dates in this period. At other times the curve is steeply sloped, so that a small change in measurement can result in a large change in the implied date. During the period between *c.* 1615 and 1530 BC the calibration curve oscillates, thus rendering problematic efforts to distinguish dates provided by radiocarbon measurements within this time period.

A third major challenge for radiocarbon dating comes in the form of seasonal and regional variation in dates, and in particular from the combination of the two. Seasonal variation refers to the intra-year difference in radiocarbon-age measurements between the summer high and winter low, which today varies significantly, generally in a range between eight and 32 radiocarbon years, but with occasional higher variations. Moreover, seasonal variation may have been greater in times past, prior to the effect of industrialisation on the atmosphere (the Suess Effect).[34] Regional variation in radiocarbon measurements of tree segments of the same known dendrochronological dates has been observed between trees in the Northern and Southern Hemispheres over the period of the past 900 years, with a difference in measurement of decadal segments of the same known date of between eight and 80 years and a mean difference of 41 ±14 years. The cause or causes of the variation are unknown. One possibility stems from the fact that more of the Southern than the Northern Hemisphere is covered by water, and water contains ^{14}C-deficient carbon which, when released into the atmosphere through periodic upwelling of deep-sea water and absorbed by trees and plants, makes calendar ages seem older than in fact they are (as noted below in the discussion of reservoir effects). Regional variation may exist within the Northern Hemisphere as well, in particular in the form of an island-coastal effect, which would of course be relevant to Mediterranean measurements. A regional offset has been suggested for the Japanese islands, either

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[28] Wiener 2003*a*, 382; 2007; 2009*a*.

[29] Manning, pers. comm.

[30] Buck and Blackwell 2004, 1100. I am most grateful to Dr. Paula Reimer, the chair of the INTCAL04 Committee and Director of the 14CHRONO Centre for Climate, the Environment, and Chronology at the Queen's University Belfast, for clarifying this matter for me.

[31] Reimer, pers. comm., 8 August 2009, for which I am most grateful.

[32] Kromer *et al.* 2009.

[33] A study of oak trees in northeastern Greece and northwestern Turkey covering the period from AD 1089 to the present indicated that May–June growing season precipitation was the critical factor in determining the size of annual rings, except for the period after AD 1960, when a North Atlantic Oscillation may have affected the data (Griggs *et al.* 2006).

[34] Keenan 2004, 102–3; Housley *et al.* 1999, 167; Levin *et al.* 1992, 503–18; Levin and Hesshaimer 2000, 69–80.

generally or for certain periods. A recent study of five-year Japanese tree-ring segments from 1060 BC to AD 400 found that

> [g]enerally, the obtained dates agreed well with INTCAL04. However, there are some periods when the dates differ significantly from INTCAL04. For example, from the 1st to 3rd century AD, systematical differences up to more than 50 ^{14}C years from INTCAL04 are observed in two independent Japanese tree-ring samples. Such large deviations would lead to invalid calibrated dates by an order of 100 calendar years.[35]

Reservoir effects provide a fourth and major challenge to radiocarbon dating in general and to dates from volcanic and/or gas emission zones in particular. Each one percent of ^{14}C-deficient CO_2 contained in a sample adds about 80 years to radiocarbon dates between 1600 and 1500 BC. Oceans and seas contain large amounts of old carbon. Rapp and Hill note that 'upwelling of deep water occurs near many coastlines' and that it 'is affected by the shape of the coastline and the bottom topography, local climate, and wind and current patterns'.[36] El Niño/ENSO episodes of upwelling in the Pacific Ocean have been advanced as a possible source of the Southern Hemisphere regional offset described above.[37] Periodic upwelling of old carbon has also been proposed for the Aegean, whether caused by the exchange of new cold deep water created annually in the northern Adriatic pushing up older water in the central Mediterranean, which then degasses as it depressurises, or by the exchange of water with the Black Sea, rich in old carbon,[38] or in the form of periodic release of old carbon from the underwater vents. The central Aegean is notable for the activity of its underwater volcanic vents. A 1992 event near the island of Melos was described as follows: 'Every fumarole on the shore blew out. And the sea boiled as the gas came out with such force. Stunned fish came to the surface'.[39] Another major underwater source exists 5 km north-north-east of Thera.

Soil containing radiocarbon-deficient CO_2 is the second major old carbon reservoir. Five types of sources have been identified: volcanic crater and ground emissions, non-volcanic gas vents, geothermal fields, and general soil degassing.[40] Statements by advocates of reliance on radiocarbon measurements for dating purposes that such reservoir effects disappear within 1–200 meters of a source rest on a few examples involving distance from a volcanic point source, as distinguished from a line (fault) source or distributed source, and ignore non-volcanic sources.[41] In Italy, the area of CO_2 emissions stretches from Florence to Naples and from the Tyrrhenian Sea to the Apennines.[42] Similar phenomena have been observed in areas of southern Italy.[43] On the volcanic island of Stromboli, diffuse deep magmatic CO_2 degassing structures have been identified over much of the island.[44] Reported radiocarbon dates from Italian sites whose historical contexts are clear are frequently 100–300 years too early.[45] Anomalies of similar magnitude have been reported elsewhere. For example, radiocarbon dates from wooden beams from two temple sites in Cambodia where inscriptions give foundation dates in the ninth century AD are 1–200 years earlier.[46] (Of course it is possible, if perhaps somewhat unlikely, that both temple sites used old wood.) At Sulphur Banks on Hawaii, three

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[35] Imamura *et al.* 2007. See also Ozaki *et al.* 2007. Sakamoto *et al.* (2009) note that with respect to radiocarbon dates from the Japanese archipelago 'possible local offsets of the curve cannot be ignored'. Stuiver and Braziunas (1993) describe how irregular water-circulation oscillations of ^{14}C-deficient water, some with a periodicity of 40 to 50 years, operate globally. They also consider whether a combination of low sunspot activity and resulting cold climate could cause a significant decrease in the plant intake of radiocarbon in certain periods and in particular places, resulting in radiocarbon measurements that are too old.

[36] Rapp and Hill 2006, 153.

[37] Stuiver and Braziunas 1993, 296.

[38] Keenan 2002.

[39] P. R. Dando, as quoted in Pain 1999, 41.

[40] Frezzotti *et al.* 2009, 108–20. I am grateful to Steven Soter for bringing this to my attention.

[41] Friedrich *et al.* 2009, 296; Manning *et al.* 2009, 301–04; Bruns *et al.* 1980; Pasquier-Cardin *et al.* 1999. The further claim that plants and trees do not acquire CO_2 through their roots (Friedrich *et al.* 2009, 296–7) has no scientific basis. Numerous studies provide clear evidence of root intake (Cramer 2002 and Ford *et al.* 2007, and references therein), while to my knowledge there is no evidence to the contrary.

[42] Frezzotti *et al.* 2009, 109.

[43] Rogie 1996; Chiodini *et al.* 1999; 2004; Rogie *et al.* 2000; Cardellini *et al.* 2003; Gambardella *et al.* 2004; Minissale *et al.* 1997.

[44] Carapezza *et al.* 2009. I am grateful to Floyd McCoy for informing me of this research.

[45] A forthcoming paper in *Radiocarbon* concludes that

> [t]he canopies of forests and cultivated fields can retard the ventilation of CO_2 emitted from the ground. This allows many plants to acquire a measurable fraction of their carbon from the recycling of CO_2 respired by the ecosystem. In seismically active areas, the flux of deep-source CO_2 can be comparable to that from soil respiration, and would therefore contribute accordingly to the CO_2 in the ambient canopy air. Such carbon has practically no ^{14}C, and its assimilation by photosynthesis in the canopy could lead to radiocarbon age increments on the order of a century or more. Some plants also acquire a small fraction of their carbon from the soil via root uptake, and in areas with a sufficient concentration of deep-source CO_2 this would further increase the apparent age of plant material. These effects may account for some of the radiocarbon age discrepancies found in Italy, Santorini and elsewhere (Soter forthcoming).

Of course, whether canopies of cultivated plants existed on pre-eruption Thera is unknowable.

[46] Uchida *et al.* 2008, 439.

living tree ferns and one Ohia leaf at distances of one to five miles from a fumarole gave radiocarbon ages of between 81 and 303 years.[47] There is every reason to believe that Theran measurements as well could be affected by ^{14}C-deficient carbon. The pre-eruption volcanic and non-volcanic gas emission landscape of Thera and its relation to areas of cultivation of various crops and olive trees is of course unknowable. Moreover, in addition to the ongoing sources of reservoir effects discussed in this paper, major eruptions are commonly preceded by a period of degassing of CO_2 (see below).

Lastly, freshwater may be a source of ^{14}C-deficient carbon, particularly where it has been in contact with thermal emissions or with limestone, a notorious source/conduit of such carbon.[48] Rapp and Hill state the matter succinctly:

> One potential influence is called the ***hard water effect***, where old or "dead" carbon containing no ^{14}C becomes mixed with the carbon in an organic substance, thus making the sample appear older than it is. This is a special problem in areas saturated with groundwater that has been influenced by bedrock limestone.[49]

Limestone may come into contact with soil directly as well, as a result of ploughing or by other means. The island of Crete is composed largely of limestone, which creates a risk that some radiocarbon measurements of Cretan samples may provide erroneous early dates. The foregoing caution may apply, for example, to radiocarbon dates obtained from seeds found in a Late Minoan IB destruction at Chania in western Crete.[50] The dates reported were earlier than any other measurements from LM IB strata elsewhere in Crete.[51] Farmland near Chania is watered by the Platanias river that runs from the White Mountains, which acquired their name because of their limestone composition, to the river mouth 13 km west of Chania. Homer tells us that the Kydonians lived around the streams of Iardanos, the ancient name of the river.[52] Radiocarbon measurements from the bones of animals such as cattle which consume plants dependent on freshwater may present the same problem at one remove.[53]

Finally, we encounter the problem of communication between the two cultures of the sciences and the humanities in general, and between statistic-speak and normal usage in particular. Only in statistic-speak could ^{14}C dates of two pea samples 215 years apart and two barley samples 97 years apart at the centres of their distributions be deemed to 'offer a very tight distribution of ages' and 'provide a highly similar set of ^{14}C ages'.[54] Statements with regard to Theran seed measurements such as 'over 80% of all probability lies before 1570 BC (13 date set) or 1560 BC (28 date set)'[55] rest on a number of major unstated premises. First, such statements presume that in constructing an absolute chronology the ^{14}C measurements of seeds which grew briefly during springtime on the volcanic island of Thera are directly comparable to the ^{14}C content of tree rings which grew partly in summer in a forest in Germany. Second, they take no account of the fact that the seeds grew on a small island days, weeks, or months prior to a paroxysmal eruption; such eruptions are often preceded by degassing of CO_2 which could easily have affected the radiocarbon measurements. Floyd McCoy, the volcanologist engaged in a long-term study of the Theran eruption, notes that ^{14}C-deficient CO_2 gas in the soil commonly leaks upward from a magma chamber prior to an eruption, to the point that such leakage is one of the major signals of an impending eruption used today.[56] Finally, such statements of probability assume in general that no problems of measurement, calibration, seasonal variation, regional

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[47] Chatters *et al.* 1969, table 2. Radiocarbon measurements from the 1960s are of course problematic.

[48] Mörner and Etiope 2002; Fischer and Heinemeier 2003.

[49] Rapp and Hill 2006, 149–50.

[50] Vlazaki, pers. comm., 24 November 2008; Hallager, pers. comm., 24 November 2008.

[51] Manning *et al.* 2009, 308–09; Housley *et al.* 1999.

[52] *Od.* 3.292. I am grateful to Erin Hayes for reminding me of this passage.

[53] Bruins *et al.* 2008 (198, 207) present two radiocarbon measurements from cattle bones found in the destruction at Palaikastro in East Crete caused by the tsunami which accompanied the Theran eruption. The two measurements do not overlap at the one-sigma range (3310 ±35 BP versus 3390 ±35 BP). The lower measurement is consistent with an eruption *c.* 1525 BC, in accordance with the Warren and Hankey Aegean Short Chronology; the higher measurement is not. The article averages the two measurements to produce a radiocarbon age of 3350 ±25 BP before calibration for the eruption. The averaging of two such determinations in the absence of evidence that the bones came from cattle which were of the same age, were subject to the same environmental conditions or necessarily died at the same time is questionable statistically (even apart from the fact that the determinations do not overlap at the one-sigma range). The statistical confidence that the two determinations do not represent the same date is 89% (Keenan, pers. comm.). Why, then, should the averaged error range be reduced (via the statistical package employed) to ±25? In any event, the difference between the lower portion of the one-sigma range for the average and the upper portion of the one-sigma range for the 1530 BC segment of the calibration curve is small, even without any reference to the hard-water effect or the general problems of obtaining radiocarbon dates from bones. Finally, the data bank is miniscule; in comparison to the analysis by Sharon *et al.* cited above and two other recent studies of organic material from Levantine sites which involved 300–400 measurements each (Finkelstein and Piasetzky 2009; Levy *et al.* 2008), here we have one measurement consistent with archaeological dating and one possibly not.

[54] Manning *et al.* 2009.

[55] Manning *et al.* 2009, 306, fig. 7.

[56] McCoy, pers. comm., 16 April 2009.

variation, or reservoir effect of ^{14}C-deficient CO_2 from any source affected the radiocarbon determinations. The same general assumptions underlie the conspicuously narrow boundaries which sometimes accompany stated radiocarbon dates, such as (e.g.) 1613 ±13 BC, or '1627–1600 BC at two sigma, or 1621–1605 at one sigma'.[57] In short, the term 'probability' is used within the context of a particular statistical paradigm, whereas the concept of 'probability' in general discourse implies that all areas of relevant information, uncertainty, and lack of knowledge or data have been considered.[58]

The chemistry and biology of sky, land and water is not easy to capture in ^{14}C measurements and statistical probability models. The chances of distortion may be asymmetric, both in general and with respect to Theran dates in particular, with greater likelihood of distortion toward older dates. The gaps in our knowledge, the sparseness of our observations in relation to the knowledge we seek, and the insufficiency of our explanations for the anomalies we observe in our measurements should induce caution in our conclusions.[59]

The most recent contribution to the radiocarbon debate comes in the form of a branch of a Theran olive tree found covered by tephra from the Bronze Age eruption. Olive trees do not generally produce annual rings, but rather irregular seasonal rings, so that the number of years represented by the 72 rings observed by X-ray tomography is uncertain. A University of Zürich Masterarbeit by T. Humbel[60] which examined living olive trees from Thera concluded that reliable ring counts were unobtainable and that the problems associated with the identification of individual growth rings were impossible to overcome. Tree rings could not be distinguished from intra-annual density fluctuations. A blind test involving several leading dendro labs produced widely divergent ring counts. The author notes that it is unfortunate that Friedrich *et al.* have made so little detailed information available (e.g., there are no images published of the 3-D X-ray computer tomography), which makes replication and evaluation of the work very difficult, and concludes that the results reported by Friedrich *et al.* have rightly been called into question.[61] The publications by Friedrich *et al.* state an allowance of ±25% of the 72 years suggested produces similar results, but it is far from clear that the ±18 years is sufficient.

Nevertheless, the four segments of the branch produced a series of sequential radiocarbon dates, with the final segment reportedly producing a radiocarbon date range of 1621–1605 BC at one sigma.[62] Accordingly, these measurements (together with rumors of two measurements from a second olive tree branch) appear to constitute the most significant current challenge to the Warren and Hankey chronology. The olive branch measurements, however, are inconclusive for three principal reasons. First, there is the possibility of distortion from any or all of the sources discussed above, particularly the admixture of ^{14}C-deficient carbon in the area of Thera. Second, it is possible that the olive tree, presumably alive at the time of the eruption, retained dead branches. Olive trees with long-dead branches are encountered with some frequency today,[63] since owners are reluctant to remove the branches for fear of damaging trees which represent a significant investment over time.[64] There is no reason to assume that great bronze saws such as those found in the Palace at Zakros in Crete were available to keepers of olive trees on Thera, while removing a large branch or limb of an olive tree with a standard copper dagger would have been a risk-laden enterprise. The claim that because the radiocarbon measurements of the successive segments of the branch decline in order there is no possibility of the presence of ^{14}C-deficient carbon in the branch is incorrect. If small amounts of such carbon are present (whether acquired through

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[57] Friedrich *et al.* 2006*a*; 2006*b*, table 2. In the case of the olive tree branch discussed below, the narrow range given also depends critically on the assumption that the segments have been 'wiggle matched' to the calibration curve, which in turn depends on the assumption that olive trees produce regular annual rings, rather than highly irregular, perhaps at times seasonal, rings (Cherubini *et al.* 2003; pers. comm., 19 April 2007; Humbel 2009).

[58] Curiously, even within statistic-speak itself the meaning of (e.g.) '80% probability' varies among the major calibration programmes. In the OxCal program '80% probability' means 80% of 100, whereas in the Calib program '80% probability' means 80% of the 95.4% two-sigma standard deviation (I am grateful to Douglas Keenan for clarifying this point). In both cases, '80% probability' refers to the putative accuracy of the calibrated ^{14}C measurement, not to the probability of calendar dates which may be affected by the various sources of uncertainty discussed in this paper. Note also that '13 date set' is based on 13 measurements of a total of four seeds (or seed clusters).

[59] The various issues have been discussed in numerous articles (e.g., Wiener 1998; 2001; 2003*a*; 2003*b*; 2006*a*; 2006*b*; 2007). The most recent exchange of scholarly salvos regarding the persuasiveness of radiocarbon dating in general and with regard to the Theran eruption in particular may be found in Wiener 2009*a*; 2009*b*; Manning *et al.* 2009; Friedrich *et al.* 2009.

[60] Humbel 2009.

[61] Friedrich *et al.* 2006*a*; 2006*b*. I thank Dr Paolo Cherubini for informing me of this research, and Mr Turi Humbel for permission to refer to his work.

[62] Friedrich *et al.* 2006*a*; 2006*b*, table 2.

[63] Rackham, pers. comm., 11 May 2008; Blitzer, pers. comm., 23 July 2008. See also Blitzer forthcoming.

[64] Elizabeth Warren (pers. comm., 30 November 2008) has kindly informed me that English stag's head oaks exhibit the same behaviour and that villagers have been known to protest when road crews attempt to remove trees with many dead branches rather than carefully removing the branches. Dead branches sometimes retain some bark; hence the presence of bark is no guarantee that a branch had not died.

roots or atmosphere) with an average intake of one percent of total carbon content, then the ages of the successive branch segments would still decline in order because the remaining 99% of the carbon would be normal atmospheric carbon with declining ^{14}C content. The result would be a series of declining dates about 80 years too early, all else being equal. Third, the putative 'wiggle match' between the ^{14}C measurements from four segments of the olive branch and the decadal measurements of the calibration curve, which in turn underlies the narrow error band (±13) asserted, is critically dependent on the bold assumption that olive trees make regular annual rings, as noted above.

IV. THE FIRST APPEARANCE OF THERAN VOLCANIC EJECTA IN THE EASTERN MEDITERRANEAN

In addition to ice-core, tree-ring and radiocarbon dating, the findings of a fourth area of scientific investigation require consideration. The pumice and tephra created by the Theran Bronze Age eruption is distinguishable scientifically from that of other eruptions. Investigation to date has produced the following result:

> 415 samples of archaeologically stratified pumices from excavations in Tel Nami, Tel Megadim, Ashkelon, Tel Gerisa, Tell el-ᶜAjjul, Tell el-Dabᶜa, Tell el-Herr, Tell el-Hebwa, North Sinai, Miletos, Çeşme, Maroni, Aegina-Kolonna, Palaikastro, and Knossos have been investigated since the beginning of the SFB [Spezialforschungsbereiche] and, with 19 exceptions, identified. For the unidentified samples it can be stated that they are clearly not related to the Minoan eruption. Up to now, no pumice from the Minoan eruption has been found in strata deposited before the 18th dynasty in Egypt. However, and oddly enough, only 27 pumice lumps have been located of such older contexts.[65]

Pumice is of course an extremely useful substance, with 14 known applications in antiquity.[66] It would be surprising indeed if pumice from the Theran eruption were available before the New Kingdom but unused. New Kingdom artefacts do not arrive in the Near East and Cyprus until after the expulsion of the Hyksos from Lower Egypt including the Nile Delta *c.* 1525 BC at the earliest. (Indeed, the pumice contexts reported to date appear to be Tuthmosid, hence not before *c.* 1500 BC.) Pumice from earlier contexts is mainly traceable to older eruptions of the volcanoes of Nisyros and Giali in the Dodecanese. For example, several small pieces of pumice from a Tell el-Dabᶜa stratum associated with Khyan, *c.* 1600–1580 BC, had a Dodecanese provenance.[67] A major deposit of waterborne Theran pumice comprising several hundred large pieces was found in what had presumably been a workshop area of the palace at Tell el-Dabᶜa of Tuthmosis III.[68] If the Aegean Long Chronology with an eruption date around 1613 BC is correct, then about 130 years separate the eruption from the deposit.[69] Of course absence of evidence is not conclusive evidence of absence, but the dramatic contrast, based on the large and growing dataset from numerous sites, between the pre- and post-18th Dynasty sources of pumice is clearly a strong scientific argument in favor of the Aegean Short Chronology of Warren.

V. DATING VIA INTERCONNECTIONS WITH EGYPT AND CYPRUS

We now move from the foregoing consideration of the uncertainties of scientific dating to the current state of, and various issues regarding, textual *cum* archaeological dating, concentrating on the MM III–LM I/LH I–II period and its interconnections with closely datable Egyptian material. (Of course Egyptian dating, while mainly based on texts of various kinds, also employs science, particularly with regard to astronomical observations.) Let us consider first the case of the alabaster lid of a jar bearing the cartouche of the Hyksos ruler Khyan found in the Palace of Minos at Knossos. At the time *ABAC* was written, Khyan was believed by some, following the work of the late Olga Tufnell on Hyksos scarabs, to be the first of the Hyksos rulers, with a reign *c.* 1648–1630 BC. This position was accepted in *ABAC*, although doubters existed.[70] Research during the intervening years has clearly moved Khyan to the third or fourth of the six Hyksos

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[65] Bietak, pers. comm., 3 July 2009. Tephra from four additional sites — Lachish, Megiddo, Quantir and Iasos — has also been examined with similar results. A breakdown by site of the numbers of samples of pumice and tephra tested follows: Tell el-Dabᶜa, Tell el-Herr and Tell el-Hebwa 154; Ashkelon (Stager) 83; Miletus (Niemeier) 29; Tel Megadim (Wolff) 7; Iasos (Berti/Momigliano) 15; North Sinai (Oren) 5; Quantir (Pusch) 4; Tel Nami (Artzy) 32; Tel Gerisa (Herzog) 15; Tell el-ᶜAjjul 39; Lachish (Ussishkin) 1; Knossos (Warren) 1; Aegina-Kolonna (Gauss) 3; Megiddo (Finkelstein) 1. I am extremely grateful to Manfred Bietak and Max Bichler for providing this information (pers. comm., 6 July 2009). See also Bietak and Höflmayer 2007, 17 and Sterba *et al.* 2009.

[66] Wiener and Allen 1998.

[67] Bichler *et al.* 2003; Bietak 2004, cols. 214–5.

[68] Bietak 2004, cols. 214–5; 1996, 77–8 for various uses of pumice including metallurgical work in antiquity.

[69] Of course pumice from an eruption may be used centuries later. Small amounts of Theran pumice have been reported from much later contexts (Wiener and Allen 1998, 25–7; Wiener 2003*a*, 371). That the pumice from the Tuthmosis III palatial workshop at Tell el-Dabᶜa shows evidence of being waterborne negates the possibility of purposeful direct import from Thera at a time long after the eruption, however.

[70] Tufnell 1984; Warren and Hankey 1989, 136.

rulers.[71] If 11 years at least are given to Khamudi (based on an inscription on the back of the Rhind Mathematical Papyrus), at least 40 years to his predecessor Apophis, an unknown number of years between zero and five to Yanassi, the eldest son of Khyan (whose separate reign was proposed by Bietak and accepted by Görg, Kempinski, Franke, Redford, Dautzenberg and Vandersleyen, but denied by Quack, Ryholt and Allen, notwithstanding the reference in the king list of Manetho to 'Iannas');[72] and if the conquest of Avaris and the expulsion of the Hyksos occurs in the 18th year of Ahmose between *c.* 1532–1522 BC (depending on whether the duration of the reign of Tuthmosis II is three or 13 years) on the 'currently traditional' chronology set forth below, then the reign of Khyan would likely fall between *c.* 1610 and 1580 BC, if his reign lasted between 16[73] and 19 years.[74] The account of Manetho, whose access to records and knowledge of the Hyksos may have been patchy, is here inconsistent between the various later renditions and difficult to reconstruct. A longer reign for Khyan may accordingly be possible.[75] The many scarabs and references to Khyan, some found in the Near East, suggest the possibility of a substantial reign, but 16 years may be sufficient.[76] Apart from the uncertainty of Hyksos reign lengths, it should be borne in mind that Egyptian absolute dates for this period come with an underlying general uncertainty of ±20 years.

Evans found the lid in a context he described as MM IIIA beneath a floor and an earthen substrate 'about a foot thick' (FIGS. 36.1–2).[77] A wall containing a cist reaches the substrate near the location of the lid. Colin Macdonald believes this construction to be typical of the MM IIIB reconstruction of the palace after an earthquake. A few meters to the north is a second cist filled with MM IIIB pottery, including pottery of a type which some would describe as 'late MM IIIB' or 'MM IIIB–LM IA transitional'.[78] Some reasonable length of time must be allowed for the alabaster lid to reach Crete and be separated from the jar it once covered. (Evans described the lid as 'absolutely fresh', notwithstanding breakages along the edge.[79]) *ABAC* allots roughly 50 years each to MM IIIA and MM IIIB, around 1700–1650 and 1650–1600 (or a little later), respectively. Analysis of material from recent excavations at Kommos and elsewhere have led to the suggestion that MM IIIA and IIIB may each represent not much more than a generation, or about sixty to seventy years in total.[80] The proposed reduction in the duration of MM III would permit MM II to end *c.* 1670–1660 BC instead of *c.* 1700 BC, or LM IA to begin around 1640–1630 BC. On the assumptions stated regarding the chronological limits to the reign of Khyan, an MM IIIA context for the lid would only be possible if Khyan had a very long reign and/or MM IIIA and B together spanned only about 50 years if the Aegean Short Chronology is correct, whereas MM IIIB (or IIIB transitional) would still be possible near the margin on the shorter Khyan reign assumption. The Aegean Long Chronology ends MM IIIB around 1700 BC, which would mean that the Khyan lid could only have arrived in any MM III context as an intrusion from a higher stratum. The contents of the Stratigraphic Museum lot in which Mackenzie grouped the Khyan lid unfortunately provide little assistance, for the lot contains potsherds of various phases including LM IIIA material (Mackenzie's notebooks give no indication of stratigraphic distinction between the Khyan lid and the pottery).[81] In sum, the evidence concerning the Khyan lid found at Knossos apparently fits more easily with the Aegean Short Chronology favoured by Warren, although not with a MM IIIA context, than with the Aegean Long Chronology, but the evidence is not conclusive.

The well-known Cypriot White Slip I bowl sherds from the eruption horizon at Thera are critical to the chronological debate and hence have received much chronological scrutiny.[82] The bowl travelled from Cyprus to Thera and was used and repaired prior to the eruption.[83] WS I appears in Egypt and Canaan entirely or almost entirely in post-1500 BC Tuthmosid

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[71] Ryholt 1997, 118–25, 201; Clayton 1994, 93–4; Krauss 1998; von Beckerath 1965; 1997; 1999; Schneider 1998; Bietak 2001, 139.

[72] Bietak 1980, 95; 1984, 474; 1997, 114; Görg 1981; 1993; 2003; Kempinski 1983, 58–78; 1985, 130–4; Franke 1988, 260–2; Redford 1992, 106–11; Dautzenberg 1993; Vandersleyen 1995, 171–8; Quack 2007, 39; Ryholt 1997, 120; Allen forthcoming. (I am grateful to Manfred Bietak for many of the citations.)

[73] Bietak, pers. comm., 27 May 2009.

[74] Schneider 1998, 74.

[75] An idiosyncratic position, thus far not adopted by others, is taken by Ryholt who believes a long reign, perhaps around 40 years, likely for Khyan, but perhaps only a one-year reign for Khamudi, thus placing Khyan at *c.* 1621–1581 BC, which would still be very difficult to reconcile with a MM IIIA context, but could fit MM IIIB. A 40-year reign could move the date of his accession back to *c.* 1630 BC (Ryholt 1997, 201).

[76] I am grateful to Manfred Bietak for discussions and information concerning the reign of Khyan. He is of course not responsible for my conclusions.

[77] Evans 1921, 418.

[78] Macdonald 2003, 40; Warren forthcoming.

[79] Evans 1921, 420.

[80] Rutter, pers. comm., 20 May 2009. The MM IIIA and IIIB pottery styles are described in the Kommos publications as 'MM III and LM IA early', respectively (Betancourt 1990, 37–48; see also Van de Moortel 1997, 24–8). See, however, Girella 2007; La Rosa 2002; Carinci 2001.

[81] Palmer 1969, 63–4; Pomerance 1984; see also Betancourt 1987.

[82] Merrillees 2001; Eriksson 2001; Manning 1999; Bietak 2004; Wiener 2001.

[83] Merrillees 2001, 93.

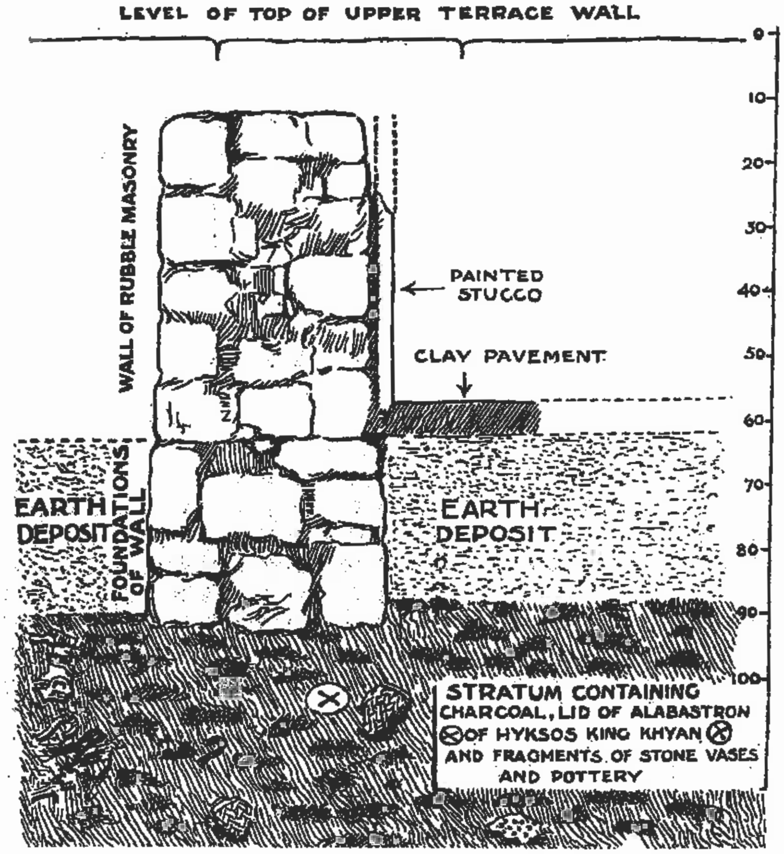

Fig. 36.1 (above). Section showing stratum containing the Khyan Lid (after Evans 1921, 418, fig. 303).

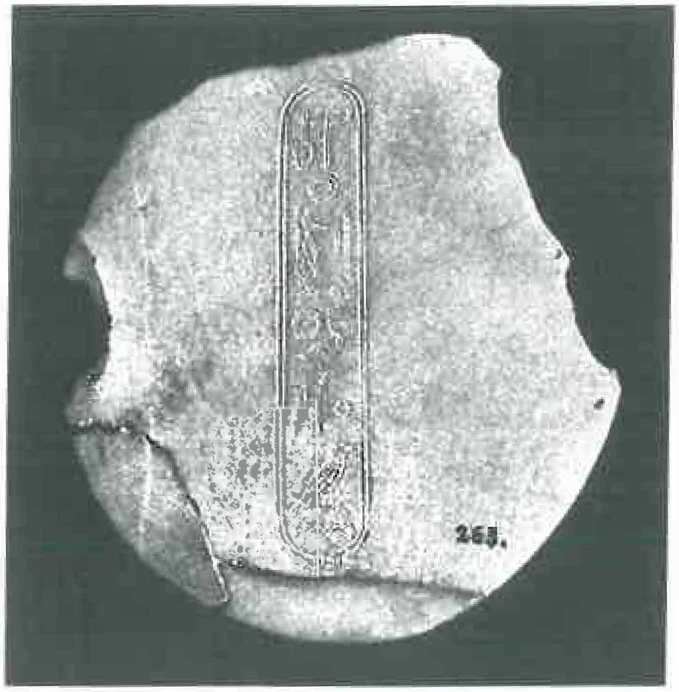

Fig. 36.2 (right). The Khyan Lid (adapted from Warren and Hankey 1989, 244, pl. 14A).

contexts,[84] with only a small number of sherds from Tell el-ᶜAjjul, near modern Gaza, arguably earlier.[85] Oren notes that as of AD 2000, WS I had appeared at 16 sites in Canaan, but nowhere earlier than '*c*. 1550–1470 BCE at the earliest', and that it is usually associated with other diagnostic Late Cypriot ceramics, especially Base Ring I, which also appears in large numbers in the Tuthmosid period but has not been securely documented in earlier strata.[86] If, as many Cypriot specialists believe,[87] the Late Cypriot IA:1 period begins *c*. 1600 BC (1650 BC at the earliest) and the LC IA:2, WS I pottery, period *c*. 1550 BC (1570 BC at the earliest), then the earliest possible date for the Theran eruption is *c*. 1560 BC (to allow time for transport, use, repair and destruction of the bowl before or during the eruption) with a date after 1540 BC more likely. Only if LC IA:1 begins before 1640 BC[88] would an eruption date around 1613 BC be feasible, and only then on the assumptions: 1) that the WS I bowl from Thera was one of the first of its type ever created; 2) that it was quickly exported, used, broken, repaired and reused, all just prior to the eruption; and 3) that all or almost all other examples, of which there are many, were somehow delayed by more than a century in arriving in Egypt and Canaan. Moreover, WS I falls into a defined chronological sequence after White Painted III, IV, V and VI and Proto White Slip (or in some places perhaps partly overlapping with WP VI and/or PWS).[89] A similar succession is found at Tell el-Dabᶜa in Egypt and at Ashkelon and Tell el-ᶜAjjul in the Levant.[90] A certain number of pots or sherds may arrive and be destroyed in datable habitation contexts abroad with a delay of generations (after the Theran eruption as required by the Aegean Long Chronology), but not thousands of sherds. An attempt to explain the gap by proposing that a division of Cyprus, perhaps caused by warfare, led to the export of a WS I pot from the northwest of Cyprus to Thera long before such pots reached Enkomi on the southeast coast of Cyprus and travelled to Egypt[91] has encountered cogent dissent.[92]

At Palaepaphos–Teratsoudhia on the western coast of Cyprus one tomb contained not only sherds of WS I and LM IA pottery (the same association seen at Thera), but also a serpentine vessel bearing the nomen and prenomen of Ahmose, the first pharaoh of the 18th Dynasty in Egypt, who becomes pharaoh on the death of his brother Kamose between *c*. 1550 and 1525 BC.[93] The Aegean Long Chronology with a date for the Theran eruption between 1645 and 1600 BC requires LM IA to end *c*. 1590 BC at the latest, which in turn would require either that the LM IA vases placed in the tomb were all heirlooms and further that the WS I vases were of an earlier date than WS I vases known from anywhere else, or that the tomb had been reopened to deposit the Egyptian serpentine vessel about 50 years after the deposit of the LM IA vessels. At Trianda in Rhodes, Cypriot White Slip ware appears only above the tephra layer of the Theran eruption.[94] Lastly, at Tell el-ᶜAjjul WS I appears for the first time in a secure context in stratum H5 with other wares of types no earlier than Late Cypriot IA:2, in contexts believed by the excavator to be no earlier than the beginning of the New Kingdom in Egypt and perhaps later, in view of parallels with Tell el-Dabᶜa phase C/3 which is Tuthmosid.[95] Stratum H5 also contained significant amounts of pumice from the Minoan eruption of Thera, whereas no such pumice has been found in the earlier levels H8 through H6. Level H6 already contains pottery identified by Fischer and Bietak as New Kingdom. The opinion of Manning[96] that the beginning of the LC IA:2 pottery phase with the earliest WS I pottery should be placed 'between approximately 1660–1630 BC or even as high as 1660/1650 BC ("at a minimum")' lacks any archaeological/textual support; even the ultra-high Cypriot chronology of Merrillees places the beginning of LC IA:2 around 1600 BC.[97] Rather, the Manning view is based on the asserted results of the few Aegean radiocarbon determinations discussed above.

The problematic radiocarbon measurement-based Aegean Long Chronology faces other difficulties as well. That five to six generations could separate the administrative use of the same Minoan gold ring as evidenced by its impressions (originally attached to parchment documents) found in the LM IA Volcanic Destruction Level at Thera and in what appear to be several final LM IB destruction levels on Crete as required by the Aegean Long Chronology seems less likely than the three generations required by the Short Chronology.[98] Peter Warren has noted that in light of

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[84] Bietak 2004; Oren 2001.

[85] Bergoffen 2001; but see Eriksson 2007, 87; Fischer 2003; Oren 1969; 2001.

[86] Oren 2001, 142.

[87] Åström 2001, 50 (LC IA:1 begins 1600/1575 BC; LC IA:2 begins 1550/1540 BC); Eriksson 2007, table 1B (LC IA:1 begins 1590 BC; LC IA:2 begins 1550 BC).

[88] Merrillees 2001, 94.

[89] For example, at Maroni in Cyprus (Cadogan *et al.* 2001).

[90] Bietak and Höflmayer 2007, 17.

[91] Manning 1999, 119–29, esp. 125.

[92] Bietak 2004; Eriksson 2001; Wiener 2001; *pace* Bergoffen 2001.

[93] Eriksson 2001, 63; Karageorghis 1990, 95, fig. 1, pl. XX:L.1. For the dates of Ahmose, see below.

[94] Marketou, pers. comm., 1 April 2007; 1998, 61–2. For more detailed analysis of pottery finds at Trianda, including Cypriot imports, see Marketou *et al.* 2006.

[95] Bietak and Höflmayer 2007, fig. 4; Fischer 2003, 273–90; cf. Bergoffen 2001, placing at least three of the WS I sherds in question in an earlier context. Fischer believes it likely that these sherds come from a burial in a courtyard cemetery rather than from a lower stratum related to an earlier palace.

[96] Manning 2001, cited by Fischer 2003, 273.

[97] Merrillees, pers. comm., 4 February 2009.

[98] Doumas 1996, 54; 2000; Weingarten 1997, 784.

his analysis of the LM IA pottery from his excavations at Knossos, the recent discovery at Mochlos in Crete of major LM IB construction in the tephra layer from the Theran eruption, and the analysis by M. Marthari in the Volcanic Destruction Level at Akrotiri of a Mycenaean vessel bordering on LH IIA, there cannot be a long post-destruction phase of LM IA as required by the Aegean Long Chronology.[99] Further, paintings from closely datable Theban tombs of Egyptian nobles show Aegean-looking emissaries carrying objects generically similar to objects which appear in LM IB destructions in Crete, as well as metal vessels of LM IA–LH I appearance. The Theban tombs containing these depictions were built no earlier than *c.* 1460 BC, and perhaps as late as 1425 BC (see below), whereas depictions of metal vessels similar to vessels known from the Aegean in LM II/LH IIB and IIIA1 contexts are depicted in the Egyptian tombs beginning around 1440–1430 BC.[100] The Aegean Long Chronology would require one of several possible explanations: that the objects were heirlooms; that they remained in circulation as gifts exchanged between Near Eastern and Egyptian courts for decades; that emigré Aegean metalsmiths continued producing objects, shapes and decorative motifs no longer employed in the Aegean; or that old pattern books were used by the tomb painters. The appearance in early 18th Dynasty contexts of imitations in local clay of Minoan rhyta of LM IA shape is consistent with the Warren and Hankey chronology, but would require a delay of 70–120 years pursuant to the Aegean Long Chronology.[101]

Finally, Peter Warren has proposed that two calcite vases found in Late Helladic I contexts in Shaft Graves IV and V at Mycenae (FIGS. 36.3–4) are of Egyptian origin in both material and morphology, and were made no earlier than the 18th Dynasty (or in one case, no later than the end of the 17th Dynasty during the Second Intermediate Period).[102] If correct in all three aspects, the Warren analysis of the Shaft Grave vases would provide conclusive evidence in favour of the Warren chronology and a date no earlier than *c.* 1525 BC for the Theran eruption. Peter Warren's argument is strongly supported by Manfred Bietak, the excavator at Tell el-Dabᶜa (Avaris), the Hyksos capital and later site of 18th Dynasty palaces in the Nile Delta. This conclusion, however, has been challenged in a number of publications by Christine Lilyquist,[103] on the grounds that the source of the calcite, the place of manufacture, and the date of the vessels are all uncertain.[104] Dorothea Arnold[105] shares this view.[106]

First, with regard to the origin of the stone Warren believes that the type of calcite utilised in the Shaft Grave vases is visually similar in its combination of squiggly narrow and broad regular banding both to the calcite used in many Egyptian vases and to the calcite from the Egyptian quarries located at Hatnub and Wadi Assiut. Warren also adds that the vessel type is common and was exported in large numbers.[107] Lilyquist, however, cites geologists who contend that calcite alabaster can theoretically be found anywhere in the Mediterranean and at places in Anatolia wherever there are outcrops of limestones or marbles, and believes that it would be strange if other cultures had not attempted to exploit such attractive and easily worked stones. Lilyquist adds that only a small number of the alabaster vases found outside Egypt which she has examined have the look of classic Egyptian alabaster. Lilyquist further notes that while very few quarries outside Egypt have been sampled and analysed, a rock found recently in Turkey has a crinkly structure very close to Egyptian rock.[108] A contrary opinion is expressed by Annie Caubet,[109] who believes that true calcite alabaster is limited to Egypt and Iran and is not found in Anatolia, Syria, Cyprus, or Mesopotamia, whereas gypsum alabaster is quite frequent in these regions.[110] A number of stone vases found in Canaan have been shown to have been made locally from local

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[99] Warren 1999 and works cited therein.

[100] Mattäus 1995; Koehl 2000; 2006, 342–5, 358.

[101] The identification of the types of Aegean metal vessels depicted in the Egyptian tombs has been made by Matthäus (1995). See also Macdonald 2001, 531 (review of Manning 1999). The Macdonald review also considers a number of other archaeological problems with the Aegean Long Chronology. Moreover, the Aegean Long Chronology requires in addition the extension of the Shaft Grave period at Mycenae well beyond the approximately three generations proposed by studies of the material from them (Dickinson 1997, 45–9; Dietz 1991; Graziadio 1991, 403–40).

[102] Warren 2006.

[103] The Lila Acheson Wallace Research Curator at the Metropolitan Museum of Art in New York.

[104] Lilyquist 1996, 134–49; pers. comm., 3 November 2008; and, more generally, 1995 and 1997. See also n. 112, below, for citations with regard to sources of calcite; Arnold, pers. comm., 9 February 2009. See also Phillips 2008; Warren 1969; Ben-Dor 1945. I am most grateful to Manfed Bietak and Christine Lilyquist for permitting me to listen to a discussion of several hours between them concerning the vases in question.

[105] Curatorial Chairman of Egyptian Department of the Metropolitan Museum of Art.

[106] Arnold, pers. comm.

[107] Warren 2006. A text from the Amarna period *c.* 200 years later describes the export to Babylonia of more than 1000 alabaster jars filled with sweet oil plus additional unfilled jars (Amarna letter 14, iii, 46, cited in Warren 1995, 3), but of course far fewer may have been exported in the late Second Intermediate Period/beginning of the New Kingdom.

[108] Lilyquist, pers. comm., 29 June 2009.

[109] Conservateur Emerita of the Department of Ancient Near Eastern Antiquities at the Louvre.

[110] Caubet, pers. comm.,1 July 2001, for which I am most grateful. She cites Ahrens 2006; Aston *et al.* 2000; Casanova 1991; Caubet 1991*a*; 1991*b*; Matoïan 2008; Icart *et al.* 2008; Chanut 2008; Lagarce 2008; Rowan and Ebeling 2008; Sparks 2007.

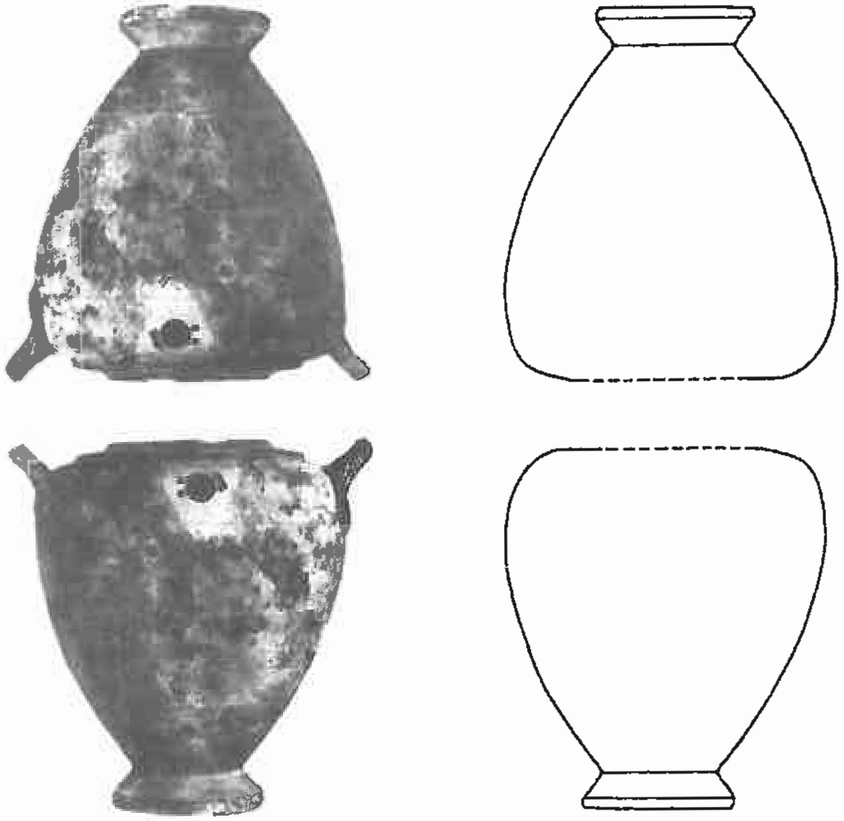

Fig. 36.3. Calcite jar from Shaft Grave V at Mycenae and drawings showing how it was converted from a baggy alabastron (adapted from Warren 2006, pl. 1C).

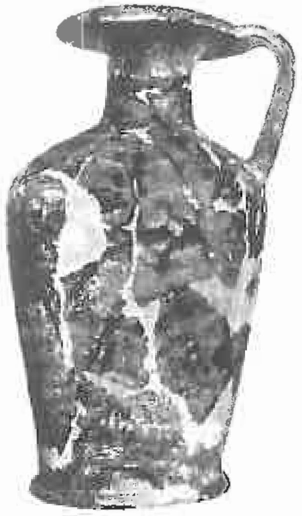

Fig. 36.4. Strap-handled jar from Shaft Grave IV at Mycenae (after Warren 2006, pl. 1A).

gypsum.[111] Attempts to determine the geographical source of alabaster through strontium isotope analysis have begun, with some success stated with regard to distinguishing Egyptian from Cretan alabaster.[112] As a consequence, a stone vase from the Minoan port of Katsambas initially thought by Warren to be Egyptian is now believed more likely to have its origin, in both material and manufacture, in Syria, the Levant, Cyprus or in particular Crete itself. In a number of cases scientific investigation has indicated sources different from those proposed through visual examination.[113]

Whether (and what) chronological conclusions can be inferred from the shapes of the stone vessels found in the Shaft Graves at Mycenae is the next issue. Both Warren and Bietak believe that the calcite vessel from

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[111] Ben-Dor 1945, 94–6.

[112] Barbieri *et al.* 2002; Barbieri, Lilyquist and Testa 2002; Testa and Lilyquist forthcoming; Ben-Dor 1945.

[113] Testa and Lilyquist forthcoming. Of course different quarries in the same region can produce stones with dissimilar isotopic signatures and the available data is quite limited.

Shaft Grave V was in origin an Egyptian baggy alabastron now inverted with the former rim serving as a base, with a typical Minoan spout added to turn the vessel into a Minoan bridge-spouted jar, and with gold foil added to the surface (FIG. 36.3). Such Minoan adaption of Egyptian vessels is known in numerous other cases as well.[114] Warren would allow for the possibility of a late Second Intermediate Period (17th Dynasty) date in view of the existence of a vase with a similar rim found in Level VII at Alalakh in Syria in what is called the Palace of Yarim Lim. Warren adds, however, that the rim may have been trimmed in Minoan Crete, and that in any event the general form and flat base are typical of vases of the 18th Dynasty. Phillips believes that the body shape of a baggy alabastron is 'not an infallible criterion for ... typological development', whereas there appears to be 'a systematic development of the rim, especially useful for dating Type C ("baggy") alabastra'.[115] The date of the Alalakh Level VII destruction is uncertain. The most recent analysis by Bergoffen suggests a date near 1560 BC, and of course the date of the destruction can only provide a *terminus ante quem* for the manufacture of the vase.[116] Lilyquist, however, maintains that the baggy shape begins in the Second Intermediate Period as indicated by a number of examples, that there is no sound reason for limiting the shape to the end of the Second Intermediate Period, that the Carter drawings of vessels from what may have been the tomb of Amenhotep I at Thebes to which Warren refers are not reliable, that shapes similar to Egyptian stone vase shapes were produced elsewhere through local invention or emulation, and that basing chronology on how a shape expands or contracts during what she believes was a period of rapid change is unsound.[117] Dorothea Arnold also believes that the calcite vessels found in the Shaft Graves at Mycenae have morphological features that are not strictly Egyptian and if Egyptian, that they cannot be precisely dated to the late 17th or early 18th Dynasty. The MBA to LBA transition in Syria needs clarification in general; for example, the relevant strata of the great trading emporium of Ugarit await future investigation in the main.

With respect to the strap-handled jug from Shaft Grave IV (FIG. 36.4), Bietak believes that the shape of the lower body is based initially on that of Cypriot Red Lustrous Wheel-Made (RLWM) terracotta vessels and that the strap handle and its lower end are typically Egyptian. (Bietak notes the existence of other Egyptian stone vase shapes which were derived from Cypriot pottery prototypes, in particular Cypriot Base Ring I.) The Cypriot RLWM vessel shape does not appear in Cyprus or Egypt in contexts earlier than the New Kingdom.[118] Lilyquist, however, argues that the Shaft Grave example with its large size, dark even bands, trumpet-like mouth, and heaviness of body differs from known Egyptian examples.[119] Lilyquist and Arnold believe that a 17th Dynasty date for the baggy vessel cannot be excluded, whether in Upper Egypt or elsewhere.

A question arises of whether and how stone vases made in Upper (southern) Egypt during the 17th Dynasty could have passed through Hyksos-controlled Lower Egypt (including the Nile Delta) to reach the Aegean. The periods of warfare between the 17th Dynasty Egyptians of Upper Egypt and the 15th Dynasty Hyksos in the Delta would in all likelihood have precluded normal trade at such times. At Avaris, the great Hyksos capital and port at the mouth of the Nile, no examples of pottery or fine flint from Upper Egypt have been found in late Hyksos contexts, although both were exported to the Delta before and after, during the Middle Kingdom and New Kingdom.[120] Similarly, coffins for important burials during the Middle and New Kingdoms are made of imported cedar or other conifers, whereas all the known coffins of the 17th Dynasty in Upper Egypt are made of local sycamore fig.[121] Conversely, Tell-el Yahudiya juglets from the Hyksos-controlled area appear in Nubia and Kerma (having presumably travelled along the oasis route), but not in Upper Egypt during the 17th Dynasty.[122] The Stele of Kamose, the last pharaoh of the 17th Dynasty (*c.* 1555–1540 BC) speaks of concern regarding the freedom of riverine traffic on the Nile.[123] This suggests, however, that even during the period of warfare toward the end of the Hyksos era there were periods of normal traffic. Further, the Stele mentions the argument of the elders opposed to Kamose's plan to attack the Hyksos that Egyptian cattle were able to

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[114] Warren 2006, 305–8; Bietak and Höflmayer 2007, 17. Peter Warren's fundamental work *Minoan Stone Vases* (1969, 105–15) discusses the work of Minoan lapidaries in transforming numbers of Egyptian stone vases in numerous ways to accommodate Minoan functional requirements and aesthetics. Phillips (2008, 80) reports a likely total of 34 Egyptian vessels reworked in Crete, of which 27 are certain and the remainder probable to varying degrees.

[115] Warren 2006, ~~2~~08; pers. comm.; Phillips 2008, 49. 3/

[116] Bergoffen 2005, 55–73; Gates 1987, 65, 76–9.

[117] Lilyquist 1995, 7–9; pers. comm., 12 June 2009; pers. comm., 3 November 2008.

[118] Bietak, pers. comm., 27 May 2009, citing Eriksson 1993; see also Merillees 1968. I am greatly indebted to Bietak for his clarification of the issues and restatement of his view in regard to the calcite alabaster vases from the Shaft Graves (conversations and pers. comm., 25 May 2009).

[119] Lilyquist, pers. comm., 3 November 2008.

[120] Bietak, pers. comm., for which I am most grateful.

[121] Davies 1995, 149.

[122] Bietak, pers. comms., 23 May 2009 and 27 May 2009, for which I am most grateful.

[123] Goedicke 1995, 37–42.

graze freely in the Delta (in the manner already documented in the Old Kingdom).[124] During earlier periods of Hyksos control of Upper Egypt, stone vases could clearly have travelled from Thebes to the Nile Delta and then on to Crete and Mycenae. Evidence of the existence of Egyptian stone vases of the types found in the Shaft Graves at this early a date is presently lacking, however.

Of course luxury products may travel to places in ways which differ from the path of other goods. Hyksos elites in the last half of their control of the Delta were busily Egyptianising in many respects, including the taking of Egyptian names and pharaonic or other titles; importing Egyptian scribes, bureaucratic practices, and craftspeople; creating shrines dedicated to some Egyptian deities; and supporting scientific investigations in the Egyptian tradition and language, as exemplified by the Rhind Mathematical Papyrus, dated in Year 33 of the Hyksos pharaoh Apophis *c.* 1540 BC and perhaps written or copied by a Theban scribe. Hyksos exchange of goods with the Levant and Cyprus is well documented.[125] Moreover, under Khyan around 1600 BC the Hyksos may have won control of much of Upper Egypt. Just as the Hyksos usurped and carried away stones from monuments, statues and stelae, so may they have taken stone vases, or for that matter stonecutters and craftspeople. An Egyptian alabaster vase bearing the name of Apophis was found in the tomb of Amenhotep I at Thebes.[126] Accordingly a stone vase made during the 17th Dynasty of the Second Intermediate Period could conceivably have found its way to Mycenae.

We are now in a position to address the chronological implications of the contending positions. Even 1) if the calcite vases were produced in Upper Egypt about 1600 BC (a century earlier than the beginning of their regular production according to Bietak), 2) then moved by one means or another (or via movement of their makers) to the Hyksos-controlled Delta, 3) next shipped to the Aegean and in one case to Crete for reworking into a bridge-spouted jar, 4) possibly used in ritual in Crete, and 5) finally sent on further to Mycenae for royal or chiefly use and display before burial in the two Shaft Graves which contained distinctive later LH I pottery, a reasonable allowance of time for these events would still place their deposition later than any Aegean Long Chronology radiocarbon-based date proposed for the end of LH I. The Theran eruption could not have occurred more than two decades at most before the end of LH I. Only if the eruption is placed at the very bottom end of the Aegean Long Chronology range (1650–1600 BC) at *c.* 1600 BC and the end of LH I at *c.* 1590–1580 BC could jars leaving Egypt *c.* 1600 BC appear in an LH I context.[127]

Thus, even if one accepts the hypothesis that the bridge-spouted calcite jar from Shaft Grave V was made a century earlier than the most clearly comparable and datable known examples, it is still very hard to reconcile its appearance with the proposed Aegean Long Chronology. Export from Egypt during the putative eleven-year reign of the last Hyksos ruler Khamudi, *c.* 1533–1522 BC (±20 at most), or even of Apophis, *c.* 1573–1533 BC (±20 at most), would also be consistent in the main only with the Aegean Short Chronology. The Aegean Long Chronology would require production of the Shaft Grave baggy calcite vessel about 150 years before the appearance in Egypt of the canonical 18th Dynasty form with its crisp detail, distinct neck, bagginess at the bottom and wide rim. The presence of the calcite vases in Shaft Graves IV and V at Mycenae, while not conclusive because of gaps in our knowledge of late MBA vase production in Egypt and Syria, on present evidence strengthens the case for the Warren chronology.

It is worth noting that while production of the vessels even as early as 1600 BC would be consistent with the Aegean Short Chronology of Peter Warren, production in 18th Dynasty Egypt as proposed by Warren and by Bietak and export after the conquest of Avaris and expulsion of the Hyksos *c.* 1535–1520 BC would involve a very rapid sequence of events en route to deposition in the Shaft Graves by the end of LH I, *c.* 1520–1505 BC, on the assumption of a Theran eruption *c.* 1525 BC. Indeed, the archaeological evidence standing alone, without regard to any consideration of radiocarbon measurements, would tend to suggest a date somewhat later than 1525 BC for the eruption, even apart from the Shaft Grave vases. Tephra and waterborne pumice from the Theran eruption have been found thus far only in Tuthmosid contexts not earlier than about 1500 BC, as noted above.

VI. EGYPTIAN CHRONOLOGY — THE HOREMHEB REVISION AND ITS AEGEAN IMPLICATIONS

One final conundrum requires consideration. A recent discovery in the Valley of the Kings at Thebes by Geoffrey Martin may require a reduction in the reign of Horemheb from 28 to 14 years, and hence a revision of the current most widely accepted Egyptian chronology with its easily recallable dates of 1479 BC for the accession of Tuthmosis III and 1279 BC for the

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[124] Goedicke 1995, 31–59.

[125] Bietak 1996, 55–63; Eriksson 2003, 419–20; McGovern 2000, 73–4, 77–9.

[126] Ryholt 1997, 385 (item 15/5); Lilyquist 1995, 3, 55–6, cat. 5; Dodson 2000, 27–8; Romer 1981, 238.

[127] Dickinson 1977; Graziadio 1991; Kilian-Dirlmeier 1986; Matthäus 1980.

accession of Ramses II.[128] Martin has re-excavated the Tomb of Horemheb (KV 57) in the Valley of the Kings at Thebes and discovered in the well shaft of the tomb a series of wine-jar dockets marked with reign years. The wine jars were originally found by the AD 1908 expedition somewhere in the interior of the tomb, and subsequently dumped by Theodore Davis's workmen into the well shaft.[129] The highest year attested by the jars is Year 14. The late Wolfgang Helck, also a distinguished Egyptologist, had already noted that it was strange there was so much evidence for the first 13 to 14 years of Horemheb's reign but no evidence after that point, either for Horemheb or any official who served under him; that the list of pharaohs compiled by the Egyptian priest Manetho in the third century BC gave Horemheb only 12 years and three months; and that the reliability of three inscriptions (one of doubtful authenticity) which have been interpreted to give Horemheb a reign of 27 to 28 years was questionable.[130]

The wine-jar argument raises a number of questions. Wine jars marked with differing regnal years and even the regnal years of predecessors are found in many Egyptian tombs, which suggests that wine jars may have been reused and/or that wine may have been deliberately aged. (A Ptolemaic text tells us that wine if properly stopped will keep many years;[131] and Near Eastern texts distinguish between old and new wine.[132]) The tomb of Horemheb contained the remains of at least three individuals, which raises a possible question as to whether the jars could have been placed in the tomb at the time of a different burial.

No other names are attested in the tomb, however, and no separate burial is traceable today (bearing in mind, however, that the tomb chamber was greatly disturbed by the AD 1908 excavation). Moreover, 254 inscribed sherds comprising at least 60 wine dockets were found, of which 46 had a year date: 22 of Year 13, eight of Year 14, and 16 incomplete, but of the incomplete inscriptions most appear to be of Year 13 and none later than Year 14.[133] If these wine dockets belong with another burial, where then are the wine jars which would surely have accompanied the burial of Horemheb? Other sites also contain wine dockets from various years of the reign of Horemheb — Years 2, 4, 6, 8, 13 and 14 at Deir el-Medina and Year 12 at Sedment — but again nothing after Year 14.[134] A 14-year rather than a 28-year reign better fits the unfinished state of the tomb.[135] Accordingly, the argument for a 14- (or at most 15-) year reign for Horemheb based on the wine dockets and absence of other evidence for his reign after the 14th year seems formidable indeed.

If the reign of Horemheb spanned only 14 years and not 28, what are the implications for Egyptian, and hence Aegean, archaeology? The answer depends initially on whether any, many or all of the remaining 14 years are to be added to the reigns of pharaohs who followed Horemheb or whether the dates of preceding reigns should be lowered. The major alternatives are set forth in tabular form in TABLE 36.1 *a* and *b*.

With regard to the chronology of the period following Horemheb until historical times, the reign of Shoshenq I, the first pharaoh of the 22nd Dynasty *c.* 945–925 BC, seems difficult to move very far because of the Biblical references to the invasion of Judah and Israel by Shishak in the fifth year of Rehoboam.[136] The reign of Rehoboam has been dated approximately by Biblical scholars through analysis of the contacts between, and sometimes overlapping reigns of, the Kings of Judah and Israel preceding Ahab.[137] The prominent reign and wealth of Ahab and his role at the Battle of Qarqar in 853 BC are recorded in the Assyrian annals, whose dating is generally accepted as secure.[138] Shoshenq's

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[128] Martin 2008. Kitchen has wittily described this as the 'currently traditional' chronology (Kitchen 2006, 303). Others have described it alternatively as the 'Middle' or 'Low' Egyptian Chronology, depending on whether the author in question believes still lower accession dates of 1468 BC and 1268 BC are possible in which case 'Middle', or whether the author believes 1468 BC and 1268 BC extremely unlikely, but accession dates higher than 1500 BC and 1300 BC possible, in which case 1479 BC and 1279 BC become the 'Low' and 1490 BC and 1290 BC the 'Middle'.

[129] Davis (1912) describes the earlier excavation. The tomb was excavated hurriedly by the 1908 mission, and the record of the excavation, prepared by Theodore Davis's assistant, Edward Ayrton, has not survived. (Note re small world: Martin served as an usher at the wedding of Peter and Elizabeth Warren in AD 1966.)

[130] Helck 1987; Krauss and Warburton 2009.

[131] Lesko 1996, 223.

[132] Dalley *et al.* 1976, nos. 252, 266.

[133] Van Dijk 2008*a*; 2008*b*.

[134] I am most grateful to Geoffrey Martin for this information, gathered by Jacobus Van Dijk (2008a), who has kindly consented to its use here.

[135] Van Dijk 2008*a*; 2008*b*.

[136] 1 Kgs 14: 25–6; 2 Chr 12: 2–9.

[137] Edwin Thiele, on whose work concerning the chronology of the Divided Monarchy all subsequent analyses to some extent rely, acknowledged the indeterminate and partly conjectural nature of his analysis, which posits an elaborate series of co-regencies and a complicated series of variations in calendars leading to the 926/5 BC date for the invasion (Thiele 1983, 39–49). Subsequent analogies have proposed slight modifications, placing the fifth year of Rehoboam in 922/921 BC (Hayes and Hooker 1988) or 918 BC (Miller and Hayes 1986). A small number of scholars known as Biblical 'minimalists' has denied all validity for dating purposes to Biblical accounts of this period (Cryer 1995; Barnes 1991; Tadmor 1979; Cogan 1992, 1007).

[138] Thiele 1983, 76; Kitchen 1996, 74–5; *contra* Tetley 2005, whose drastic proposal to raise the date of both Rehoboam and Shoshenq I by 40 years seems impossible in terms of Egyptian chronology, whereas her critique of Thiele's methodology may have merit and allow some upward movement of the Rehoboam date.

TABLE 36.1 *a*: **Reign dates of Egyptian pharaohs following Horemheb, if the reign of Horemheb is reduced to 14 years and the accession dates of Ramses II and succeeding pharaohs are raised 11 years.** The proposed revision would further require the addition of three years to reigns preceding Horemheb to reach the astronomically appropriate date of 1479 BC for the accession of Tuthmosis III and complete the closure of the putative 14-year Horemheb gap. (If Horemheb lives into his 15th year, then only two years need be added.) The two/three years are most likely to be found by reducing the co-regencies of Akhenaten/Ankh(et)kheperure and/or Tuthmosis III/Amenhotep II or by adding a year or two to the reign of Ay, or by some combination (I am grateful to Chris Bennett for these suggestions). The extension of the reign of Tuthmosis IV from 10 to 12/13 years is an alternative, but perhaps less likely, possibility. The last year documented for his reign is Year 8. David Aston has raised the question of whether the number of structures erected and officials known might not indicate a reign of more than eight years, but Betsy Bryan has noted that none of the structures in question is of large size, and regards their existence as consistent with a reign of eight to ten years (Aston 2005, discussed in Wiener 2006*b*; Bryan 1991; 2000).

Pharaoh	***Currently conventional dates****	***Revised dates if post-Horemheb reigns including Ramses II raised by 11 years***
HOREMHEB	*c.* 1323–1295 BC	*c.* 1320–1306 BC
Ramses I	1295–1294 BC	1306–1305 BC
Seti I	1294–1279 BC	1305–1290 BC
Ramses II	1279–1213 BC	1290–1224 BC
Merneptah	1213–1203 BC	1224–1214 BC
Amenmesses	1203–1200 BC	1214–1211 BC
Seti II	1200–1194 BC	1211–1205 BC
Siptah	1194–1188 BC	1205–1199 BC
Tawosret	1188–1186 BC	1199–1197 BC
Setnakht	1186–1184 BC	1197–1193 BC
Ramses III	1184–1153 BC	1193–1162 BC
Ramses IV	1153–1147 BC	1162–1156 BC
Ramses V	1147–1143 BC	1156–1152 BC
Ramses VI	1143–1136 BC	1152–1143 BC

TABLE 36.1 *b*: **Reign dates of Egyptian pharaohs preceding Horemheb, if the reign of Horemheb is reduced to 14 years, the accession of Tuthmosis III is lowered by 11 years, and three years are added by limiting the lengths of the putative co-regencies between the death of Akhenaten and the accession of Tutankhamun.**

Pharaoh	***Currently conventional dates****	***Revised dates if pre-Horemheb reigns including Tuthmosis III lowered by 11 years***
Ahmose	*c.* 1550/1540–1525/1515 BC	*c.* 1539/1529–1514/1504 BC
Amenhotep I	1525/1515–1504/1494 BC	1514/1504–1493/1483 BC
Tuthmosis I	1504/1494–1492/1482 BC	1493/1483–1481/1471 BC
Tuthmosis II	1492/1482–1479 BC	1481/1471–1468 BC
Hatshepsut	1479–1457 BC	1468–1446 BC
Tuthmosis III	1479–1425 BC	1468–1414 BC
Amenhotep II	1427–1401 BC	1416–1390 BC
Tuthmosis IV	1401–1391 BC	1390–1380 BC
Amenhotep III	1391–1353 BC	1380–1342 BC
Akhenaten	1353–1337 BC	1342–1326 BC
Ankh(et)kheperure (Smenkhare)	1338–1336 BC	1326–1322 BC
Tutankhamun	1336–1327 BC	1322–1313 BC
Ay	1327–1323 BC	1313–1309 BC
HOREMHEB	1323–1295 BC	1309–1295 BC

* The term 'currently conventional' refers to the fairly broad consensus on these dates as of AD 2007 prior to the proposed Horemheb adjustment, as set forth in Kitchen 2007, 2006 and 2000. The dates for Ahmose to Tuthmosis III vary depending on whether the Manethonian reference to the reign of Tuthmosis II should be read as three years or 13 years. Kitchen 2006, 303; 2007. Current opinion is divided. Compare von Beckerath 1997, 121 with Hornung 2006, 200–1, Krauss 2007, 182, and Gabolde 1987, 74–5.

invasion of Judah and Israel is described at length in a massive relief on a wall near a portal of the Karnak temple in Egypt.[139] An upward movement of the dates for Shoshenq I (in order to close the putative 14-year Horemheb gap) would require a change in Biblical chronology, which already strains to achieve an overlap between the reign of Shoshenq I and the fifth year of Rehoboam. (For example, *Chronologies of the Ancient World: Names, Dates and Dynasties* gives 926–910 BC as the reign dates of Rehoboam, which would place his fifth year at 922 or 921 BC, whereas the dates given in the same publication for the reign of Shoshenq I are 946/5–924 BC, thus placing his demise before his invasion, if the Biblical reference to the invasion in the fifth year of Rehoboam is correct.[140]) Krauss has proposed that an astronomical observation allows us to date the accession of Shoshenq I to 943 BC[141] instead of 946/945 BC, which would just fill the gap by lowering the date of Shoshenq's death. Conversely, raising the accession date of Shoshenq I by some years would fill at least part of the putative 14-year Horemheb gap but would require a corresponding adjustment to the reign of Ramses II (discussed below), and a dismissal of the astronomical date proposed provisionally by Krauss. (Of course all Egyptian astronomical dates are problematic to some extent.[142])

Between Horemheb and Shoshenq I, however, there are clearly places where years may be added to reigns. There is independent evidence that the two years previously allotted to Setnakht, the predecessor of Ramses III, must be increased to at least the beginning of a fourth year because of the discovery in 2007 of a stele of the High Priest Bakenkhunsu dated three years and one month into Setnakht's reign.[143] The Third Intermediate Period between 1100 BC and Shoshenq I remains an area of uncertainty (notwithstanding the monumental work of Kenneth Kitchen[144]), particularly with regard to the late 20th Dynasty and the 21st Dynasty.[145] Of course, filling the 'Horemheb gap' by extending the Third Intermediate Period would require raising the dates of all reigns between the Third Intermediate Period and Horemheb, including Siptah and Ramses III (TABLE 36.1 *a*), with major consequences for Aegean and Near Eastern chronology, and particularly with respect to the Late Helladic IIIC and late IIIB periods.

Two inscriptions from the temple of Ramses III at Medinet Habu, dated to the fifth and eighth years of his reign, describe the devastation caused by the attacks of the 'Sea Peoples' in Amurru (Syria) and Canaan, including the area of the great trading emporium of Ugarit. The inscription from the eighth year recounts how the Sea Peoples were finally defeated by the force of Ramses III in a battle at the mouth of the Nile. The fifth year of Ramses III is currently generally placed between 1183 and 1179/8 BC.[146] The destruction stratum at Ugarit contained a letter from Bay, the chancellor and effective ruler of Egypt under the pharaoh Siptah.[147] An Egyptian text states that Bay was executed as a traitor in the fifth year of Siptah's reign.[148] On conventional (i.e., pre-Horemheb adjustment) chronology, Bay's execution has been placed between 1193 and 1190/89 BC. If two/three years are added to the reign of Setnakht, the successor to Siptah/Queen Tawosret, as proposed above, then an additional eight/nine years would be required to raise the accession date of Ramses II by one lunar cycle from 1279 to 1290 BC and fill all but three years of the putative 14-year Horemheb gap.[149] On this hypothesis, the fifth year of Ramses III would be raised to 1192–1188/7 BC and the destruction of Ugarit would occur at some point before 1204–1200 BC. The effect of the proposed upward shift in post-Horemheb dates on Aegean chronology would be significant. LH IIIC Early pottery appears in the destruction levels associated with the Sea People's assaults at Ugarit and elsewhere.[150] It is unlikely that the first piece of IIIC

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[139] A monograph by Wilson (2005) questions many aspects of the account. While exaggeration of the extent and the degree of success achieved is of course always possible, it seems highly unlikely that the account of a major campaign could be wholly or largely fictitious.

[140] Quack 2007, 42; Liwak 2007, 56.

[141] Krauss 2007; Wiener 2006*b*. Krauss 2006, 411–2 places the accession date between December 944 and November 943 BC.

[142] Bennett 2008, 525–9.

[143] Schneider forthcoming; Boraik 2007; 2008/2009. See J. Baker post on The Ancient Near Eastern Chronology Forum, 13 June 2007, 7:39 am, http://disc.yourwebapps.com/discussion.cgi?id=177754; article=7295.

[144] Kitchen 1996.

[145] Bennett, pers. comm., 29 December 2008, for which I am most grateful; J. Baker, post on The Ancient Near Eastern Chronology Forum, 3 February 2009, 8:19 am, http://disc.yourwebapps.com/discussion.cgi?disc=177754;article=8992; Krauss 2006; Schneider forthcoming.

[146] Krauss 2007, 187; Kitchen 2000, 49; von Beckerath 1997, 106, 190. For an excellent summary, see Weninger and Jung 2009. I am most grateful to Reinhard Jung for making this important paper available to me in advance of publication.

[147] Freu 1988.

[148] Grandet 2000.

[149] The remaining three years would need to be added to reigns prior to Horemheb on this hypothesis. If the further condition of consistency with the lunar-cycle-constrained date of 1479 BC for the accession of Tuthmosis III is added, then a three-year extension through the elimination of a co-regency or co-regencies as proposed by Bennett (TABLE 36.1 *a* caption, above) appears to be the sole option. (If the Krauss proposal to lower the accession date of Shoshenq I from 945 to 943 BC on astronomical grounds is accepted, then an additional two years would need to be found among the uncertain reigns of the 20th and 21st Dynasties.)

[150] As Warren and Hankey (1989, 160–2) had already surmised, on the basis of far less information than is available today.

pottery ever made arrived in Ugarit in the year of the destruction; rather it seems more likely that IIIC pottery was in existence a decade earlier, i.e., by 1210 BC if post-Horemheb dates are raised as proposed. If at least a decade is allowed for the IIIB–C transitional phase which marks the destruction of the mainland Mycenaean palaces, then the LH IIIB period would end *c.* 1220 BC at the latest. If the proposal of Weninger and Jung[151] that recent dendrochronological, radiocarbon and textual/archaeological evidence indicates that the Sub-Mycenaean period falls within *c.* 1070–1040 BC is correct, then durations of a good two generations each for LH IIIC Early and LH IIIC Middle are indicated.[152]

Closing the Horemheb gap by raising subsequent reign dates of course requires moving the dates of Ramses II. The 66-year reign of Ramses II, presently 1279–1213 BC on the conventional chronology, may initially appear difficult to move, however, given the intensive documentation of his reign. The documentation includes correspondence with other rulers and with Egyptian vassals whose approximate dates are independently established via Assyrian and Babylonian chronologies; closely dated visits to Egypt by the Hittite king Hattusili III, and the analysis of astronomical dates, where current opinion somewhat prefers 1279 BC as the accession date, while acknowledging a considerable degree of uncertainty (see below). The next higher potential lunar calendar date for the accession of Ramses II is 1290 BC. A. A. Nemirovsky has recently published a series of six papers in Russian[153] arguing in part that the reinterpretation of one critical text in particular (KBo I 10) from the correspondence between Near Eastern rulers indicates that Kadashman-Turgu must have died after Ramses II's 21st year, that Tukulti-Ninurta must have assumed the throne several years after Ramses II's 43rd year, and that accordingly the reign of the Hittite ruler Hattusili III should be moved earlier by about a decade. Such a change would take Ramses II up a decade as well.[154] Joe Baker has also proposed that the accession of Ramses II should be raised to 1290 BC, following Nemirovsky, but adding additional arguments favoring an upward shift in the dates of Near Eastern rulers who intersect with Merneptah, Amenmesse and Seti II, the Egyptian pharaohs who succeed Ramses II, which of course would entail raising the dates of Ramses II as well.[155] The earliest reference to an attack by the Sea Peoples comes in an inscription dated to the fifth year of the reign of Merneptah, which the analyses of Nemirovsky and Baker would place at 1219 BC.

With regard to the astronomical evidence, Bennett has noted that all astronomical calculations are problematic to some extent, particularly in view of the uncertainty in many cases of whether the information recorded represents an observation or instead a prediction or estimate (e.g., of when a festival should begin). Other sources of uncertainty include the latitude of the place of observation; whether the observer was at ground level; whether the lunar crescent or rising of Sothis appeared at ground level or over a temple or geological feature; and the clarity of the atmosphere on a particular day. Bennett believes that the astronomical evidence somewhat favors 1279 BC over 1290 BC as the Ramses II accession date, whereas the textual evidence significantly favors 1290 BC.[156]

Recently, however, a number of scholars, including Rolf Krauss and David Warburton, have advocated the opposite solution, namely lowering the dates of earlier reigns by at least 11 years, also as suggested in 1987 by Helck (see TABLE 36.1 *b*). Krauss (perhaps surprisingly in view of his strongly expressed view at the SCIEM 2003 conference that the 1479 BC accession year for Tuthmosis III was firmly fixed astronomically, as well as his prior publication asserting the correctness of the 27- to 28-year reign then generally ascribed to Horemheb[157]) has proposed that astronomical evidence now supports lowering the accession year of Tuthmosis

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[151] Weninger and Jung 2009.

[152] The authors believe that contrary conclusions based on dendro and radiocarbon dates proposed for Protogeometric strata at Assiros and Kastanas in northeastern Greece are the result of measurements made on reused wood. See *contra*, Newton *et al.* 2005. In any event, the proposed raising of dates post-Horemheb would leave an additional decade for the developments in IIIC Early and Middle.

[153] See, e.g., Nemirovsky 1999; 2003; 2005*a*; 2005*b*; 2007; 2008.

[154] Nemirovsky 2003; 2007. I am most grateful to Chris Bennett for bringing these references to my attention and for providing Baker's English translation of one of these articles. The issue is discussed in greater detail in Wiener forthcoming.

[155] J. Baker post on The Ancient Near Eastern Chronology Forum, 26 January 2009, 8:15 am, http://disc.yourwebapps.com/discussion.cgi?disc=177754;article=8940. It should be noted that while the dates of these reigns may be moved, their duration cannot be lengthened significantly for, as Bierbrier (1975, 15) has shown, the documented careers of very long-lived senior officials under these pharaohs cannot reasonably be extended by more than five years.

[156] C. Bennett post on The Ancient Near Eastern Chronology Forum, 3 February 2009, 10:53 am, http://disc.yourwebapps.com/discussion.cgi?disc=177754;article=8997; pers. comm., 7 June 2009. Egyptologist Thomas Schneider has also concluded that all astronomical dates are problematic for a number of reasons and that the chronology of the Third Intermediate Period is particularly unsettled. He believes accordingly that raising dates after Horemheb provides the most probable means of filling the fourteen-year gap (Schneider forthcoming; pers. comm., 9 June 2009).

[157] Krauss 2007, 182; Krauss and Warburton 2006, 476–7; see also Krauss 1994.

III to 1468 BC (11 years, or half a lunar cycle, from 1479 BC), and that the dates for the beginning of the New Kingdom and the expulsion of the Hyksos should be lowered accordingly (again as previously suggested by Helck).[158]

Any such shift would require Near Eastern dates to shift as well in view of the correspondence between Burnaburiash II of Babylon and Amenophis III (EA 6), Akhenaten (EA 7–8), and Tutankhamun (EA 9) and of Assur-Uballit I of Assyria with Tutankhamun (EA 15) documented in the Amarna tablets.[159] For example, lowering the dates of pharaohs preceding Horemheb by 11 years absent a change in Babylonian dates would have Burnaburiash dying by the middle of Akhenaten's reign, whereas the evidence suggests that the correspondence between these rulers continued in all probability until the end of the reign, and would require that Horemheb become pharaoh early in the reign of the Hittite king Mursili II, whereas a new join of seven fragments of a historical text of Mursili II published in 2007 by Jared Miller indicates that in Year 7 of Mursili II, Horemheb was still only the general in command of the northern army.[160] Miller would propose lowering the accession date of Horemheb by at least three years, from *c.* 1323 to 1319 BC. Three to four years would still leave open the possibility of raising Ramses II by 11 years to fit the lunar calendar, but any further lowering would need to extend an additional 11 years to return to the currently conventional accession date for Ramses II of 1279 BC or the abandonment of the lunar calendar altogether.

Lowering the dates of the reigns of pharaohs preceding Horemheb would let loose a cat among the pigeons with respect to Aegean chronology. How fierce a cat depends on whether one allows three or 13 years for the reign of Tuthmosis II. If only three years, and 11 years are subtracted to fill the Horemheb gap, then the consequent lowering of the date of the beginning of the New Kingdom from *c.* 1539 BC to *c.* 1528 BC, and the expulsion of the Hyksos between the 18th and 22nd years of Ahmose to *c.* 1510–1506 BC, with the capture of Sharuhen (perhaps the site of Tell el-ᶜAjjul, whose Cypriot pottery is discussed above) coming three years later *c.* 1507–1505 BC, would close an already very narrow window between the beginning of the 18th Dynasty and the Theran eruption. Such a shift would require that the date of the mature-phase LM IA Theran eruption be placed significantly later than *c.* 1525 BC, which would move the date beyond the oscillating portion of the radiocarbon calibration curve between *c.* 1615 and 1530 BC. On the other hand, the lowering of the 18th Dynasty dates would lessen the putative challenge to the Warren/Hankey chronology posed by certain Aegean objects found in Egypt. LM IB/LH II to LH IIB pots or sherds from Saqqara Teti Pyramid Tomb NE I, Abydos and Kom Rabiᶜa appear in chronological contexts which, absent the proposed reduction in early 18th Dynasty dates, are close to the margin of the chronological span proposed by Warren and Hankey for LM IB/LH IIA.[161] Recent opinion as to the duration of the reign of Tuthmosis II is divided, with von Beckerath strongly in favour of a 13-year reign, supported by Schneider, but with Gabolde, Hornung and Krauss holding to around three years (Kitchen noted both possibilities without stating a clear preference, but used three years in his tables).[162] A 13-year reign of Tuthmosis II coupled with a lowering of the year of accession of Hatshepsut/Tuthmosis III to 1468 BC would allow the accession of Ahmose and the beginning of the New Kingdom to remain at *c.* 1540/1539 BC.[163] Of course if 1) the Horemheb gap is closed instead by

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[158] Krauss and Warburton 2009; Helck 1987. I am grateful to David Warburton for informing me of the position taken prior to publication of this article. Two recent reexaminations of the various versions of the third century BC text of the Egyptian priest Manetho and all other textual evidence support an accession date of 1479 BC for Tuthmosis III (Schneider forthcoming; Bietak forthcoming . I am grateful to Manfred Bietak for providing this information.) Bennett notes, however, that on purely astronomical grounds 1468 BC is preferable to 1479 BC, and moreover that there is an astronomical case to be made for 1465 BC as the year of accession of Hatshepsut/Tuthmosis III, which would fill completely the presumptive 14-year Horemheb gap with no requirement for any adjustment of later dates (pers. comm., 20 June 2009, for which I am most grateful).

[159] Moran 1992.

[160] Miller 2007; J. Baker post on The Ancient Near Eastern Chronology Forum, 31 January 2009, 5:50 am, http://disc.yourwebapps.com/discussion.cgi?disc=177754;article=8992.

[161] Bourriau and Eriksson 1997; Betancourt 1983, 28–30; Lilyquist, pers. comm., 21 May 1996. But see Warren 2009; forthcoming.

[162] von Beckerath 1997, 121; Schneider forthcoming; pers. comm., 9 June 2009; Gabolde 1987, 74–5; Hornung 2006, 200–1; Krauss 2007, 182; Kitchen 2000, 44.

[163] A 13-year reign would extend the life-spans of well-documented high officials who served under a succession of pharaohs. For example, the tomb inscription of Ahmose Pen-Nekhbet relates his military service under Ahmose in Canaan, Amenhotep I in Nubia, Tuthmosis I in Naharin and Tuthmosis II in Sinai, followed by his service as a nurse (perhaps meaning tutor) to the daughter of Hatshepsut, who heaped honours upon him. His tomb wall inscription states that Tuthmosis III was the pharaoh when it was carved. If he began his army career toward the end of the reign of Ahmose at age 18 (*c.* 1517 BC on this hypothesis) and served five years into the reign of Hatshepsut/Tuthmosis (*c.* 1463 BC on this hypothesis), he would still have been performing important duties at court at the age of 70. If his army service under Ahmose, which included the capture of a prisoner and 'one hand', began later than age 18, or lasted more than five

raising the dates of succeeding reigns including the reign of Ramses II as suggested above, 2) the accession date of Hatshepsut/Tuthmosis III remains at 1479 BC and 3) a 13- rather than a three-year reign for Tuthmosis II is accepted, then the New Kingdom would begin *c.* 1550 BC and the expulsion of the Hyksos would occur *c.* 1532 BC.

Lowering the accession date of Hatshepsut/Tuthmosis III from 1479 to 1468 BC with the consequent lowering of the dates of succeeding reigns would have significant consequences for Aegean chronology. For example, the accession date of Amenophis III would move from 1390 to 1379 BC. That accession date provides a critical linchpin for Aegean chronology, for it is the scarab of Amenophis III in Tomb 4 of the Sellopoulo cemetery at Knossos in a pure LM IIIA1 context that provides the *terminus post quem* for the period. The Amarna period and hence the LH IIIA2–IIIB transition would also shift downward. As a consequence, the transition from LM/LH IIIA1 to IIIA2 pottery would occur *c.* 1380–1365 BC rather than 1390–1375 BC and the transition from IIIA2 to IIIB would not begin before 1320 BC at the earliest, and probably not before *c.* 1310–1305 BC.[164] The date of the major LM IIIA2 destruction at Knossos and the Linear B tablets it contained would also move downward by about a decade. These consequences would only obtain, however, if 11 years of the putative 14-year 'Horemheb gap' were to be subtracted from the accession dates of the pharaohs between Tuthmosis III and Horemheb, rather than added to the dates of succeeding pharaohs. As of 1 July 2009, the evidence appears to favour somewhat raising the dates of reigns following Horemheb and leaving the conventional 1479 BC accession date for Tuthmosis III in place.

VII. CONCLUSION

Let us close by returning to the critical question of the date of the volcanic eruption of Thera. Tree-ring growth anomalies in two or more geographically separate regions and acid spikes in Greenland ice cores give indications of possible volcanic eruptions at 1571–1569 and especially at 1525 BC.[165] The earlier the proposed radiocarbon date, the harder it is to reconcile with the textual/archaeological plus Theran pumice evidence, which on its own may suggest a date later than 1525 BC, as noted above. Dates after 1525 BC, where the oscillating portion of the calibration curve ends, are significantly more difficult to reconcile with the radiocarbon evidence (subject to the various caveats set forth above with respect to problems of radiocarbon dating in general and Theran dates in particular). A Theran eruption date of 1525 BC, during a mature or final stage of LM IA, has the merit of possible compatibility at the margin with both the archaeological and radiocarbon data, plus the additional advantage that an event of that date, most likely a major eruption, is indicated in the tree rings in Siberia, California and Nevada and in a small acid spike in the Greenland ice core.[166] Thus we may have come full circle, at this point in time, to the date suggested 20 years ago in *ABAC* by Peter Warren.[167]

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years into the reign of Hatshepsut/Tuthmosis III, then his years of service and hence lifespan would expand accordingly. If the reference in the tomb inscription to Tuthmosis III is assumed to refer to the period of Tuthmosis III's sole rule after the end of the co-regency with Hatshepsut, then an additional minimum of 17 years must be added to the five previously assumed, bringing Ahmose Pen-Nekhbet to the age of at least 87 while still in service. If Tuthmosis II's reign lasts only three years, then the minimum lifespan for Pen-Nekhbet is 60 to 77, depending on the interpretation of the reference to Tuthmosis III. Pen-Nekhbet is one of a number of long-lived officials whose careers would be stretched by a 13-year reign for Tuthmosis II. The relatively small number of scarabs bearing the cartouche of Tuthmosis II compared to his predecessor Tuthmosis I or his successor Hatshepsut is another argument in favour of a shorter reign (Gabolde 1987, citing Hornung and Staehelin 1976).

[164] Wiener 2003*b*, esp. 250.

[165] It is also conceivable that a major eruption could occur and leave no readily observable trace in tree rings or ice cores for the reasons noted above. 1571 BC is at the outer limit of the range suggested by Friedrich *et al.* for the date of the eruption as indicated by the radiocarbon measurements of the Theran olive branch. The range proposed assumes 1) that the number of years represented by the rings on the olive branch is not less than half nor more than twice the length of time represented by annual rings, and 2) that no complicating factor relating to (e.g.) island-coastal effects or contact with ^{14}C-deficient carbon is present (see above). Here we may recall that in *ABAC* (p. 215, postscript to p. 142) Warren noted that the eruption could have occurred as early as 1560–1550 BC if a whole generation were allowed for a putative final phase of LM IA between the eruption and the beginning of LM IB.

[166] Wiener 2006*a*, 320–4. The Ahmose Tempest Stele describing a great storm may have been executed soon after 1525 BC on the currently traditional Egyptian chronology absent any down-dating of pre-Horemheb reigns (Foster and Ritner 1996; but see also Wiener and Allen 1998). The date of the Stele within the reign of Ahmose is uncertain, and the proposed connection between the Theran eruption and a storm at Thebes or elsewhere in Upper Egypt is more uncertain still.

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The 'Goddess of Myrtos'.
(ANM no. 7719. Photograph courtesy of the 24th Ephorate of Prehistoric and Classical Antiquities, Ayios Nikolaos).

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Contents

Abbreviations

GENERAL

BA	Bronze Age	H	Helladic
C	Cycladic	M	Minoan
E / M / L	Early / Middle / Late	PG	Protogeometric
FN	Final Neolithic		

ANM	Ayios Nikolaos Museum	L.	length
asl	above sea level	pres.	preserved
D.	diameter	SM	Stratigraphical Museum
FM	Furumark Motif	SMP	Stratigraphical Museum Pot no.
FS	Furumark Shape	T	tomb
FMA	Florence Museo Archeologico	Th.	thickness
H.	height	W.	width
HM	Herakleion Museum		

JOURNALS AND SERIES

AA	*Archäologischer Anzeiger*
AAA	*Αρχαιολογικά ανάλεκτα εξ Αθηνών. Athens Annals of Archaeology*
Aegaeum	Annales d'archéologie égéenne de l'Université de Liège
AJA	*American Journal of Archaeology*
ArchDelt	*Αρχαιολογικόν Δελτίον*
ArchEph	*Αρχαιολογική Εφημερίς*
ASAtene	*Annuario della Scuola archeologica di Atene e delle Missioni italiane in Oriente*
AthMitt	*Mitteilungen des Deutschen Archäologischen Instituts, Athenische Abteilung*
AR	*Archaeological Reports*
BAR–IS	British Archaeological Reports — International Series
BCH	*Bulletin de correspondance hellénique*
BdA	*Bollettino d'Arte*
BICS	*Bulletin of the Institute of Classical Studies*
BSA	*Annual of the British School at Athens*
CAJ	*Cambridge Archaeological Journal*
CMS	*Corpus der minoischen und mykenischen Siegel* (see list overleaf)
CR	*Classical Review*
Ist. Mitt	*Mitteilungen des Deutschen Archäologischen Instituts, Istanbule Abteilung*
JdI	*Jahrbuch des Deutschen Archäologischen Instituts*
JMA	*Journal of Mediterranean Archaeology*
KChron	*Κρητικά Χρονικά*
LIMC	*Lexicon Iconographicum Mythologiae Classicae*
OJA	*Oxford Journal of Archaeology*
ÖJh	*Jahreshefte des Österreichischen Archäologischen Instituts in Wien*
OpAth	*Opuscula Atheniensia*
PAE	*Πρακτικά της εν Αθήναις Αρχαιολογικής Εταιρείας*
PPS	*Proceedings of the Prehistoric Society*
PZ	*Praehistorische Zeitschrift*
RDAC	*Report of the Department of Antiquities Cyprus*
RA	*Revue Archéologique*
SIMA	Studies in Mediterranean Archaeology
SMEA	*Studi micenei ed egeo-anatolici*
TUAS	*Temple University Aegean Symposium*

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