Thera Discussion

M. H. Wiener's Reply to the Papers by Manning et al. and Friedrich et al.

The papers by Manning et al. and Friedrich et al. in this volume incorporate a number of optimistic assumptions concerning the accuracy and precision of radiocarbon measurements and of the calibration curve, and provide incomplete citations to the relevant scientific literature regarding reservoir effects, root intake of CO_2, and other matters.

Manning et al. state that "[a] change of −10 to −30 ^{14}C years makes almost no substantive impact. Overall, a −10 to −70 ^{14}C year change, even at the limits of the 2σ range, still requires a date for the Akrotiri volcanic destruction level before 1550 BC" (page 312). This assertion applies to the proffered Bayesian Sequence Analysis as a whole. In order to test this conclusion, it is appropriate to consider each major constituent of the sequence individually, namely 1) the Akrotiri Volcanic Destruction Level (VDL) material; 2) the accuracy and precision of the calibration curve measuring rod; 3) the nature of potential data-distorting factors such as regional/seasonal effects and, in particular, reservoir effects from the uptake of ^{14}C-deficient carbon from the earth, streams/springs, or the sea; 4) the Thera olive branch measurements; and 5) the radiocarbon dates from other Aegean sites. It is important to recall, moreover, that the flatness of the radiocarbon calibration curve between c. 1600 and 1525 BC makes the interpretation of measurements very sensitive to small changes in ^{14}C measurement, with the result that a 20-year difference in measurement may allow an 80-year difference in date.

I. Theran-Seed Sample Measurements

Consider first the radiocarbon measurements from short-lived samples from the VDL on Thera itself. The data is presented in Manning et al. Figure 6. The bottom of the Gaussian bell curve distribution of the measurements of the 13 samples (there depicted at the left as a spearpoint in red) intersects the top of the smoothed IntCal04 probability band c. 1530 BC, with a greater overlap, although still fairly small, if the unsmoothed IntCal98 curve is substituted. IntCal98 "preserve[s] more high-frequency variation compared to IntCal04" (Friedrich et al. 2006b: 2–3). A downward movement of only two to three per mil in measurement, equivalent to 17 to 25 years in the weighted average calibrated date, would point the center of the determinations close to the center of the probability band at c. 1530 BC. Douglas Keenan, a mathematician specializing in the verification of scientific and medical research including studies involving climatology, has kindly provided the following Figure 1 illustrating the effect of such a change of only two per mil (17 years), calibrated by the unsmoothed (and hence more appropriate in this context) 1998 curve. It is worth recalling in this regard that the IntCal04 committee recommended expanding the error bands of the 1998 calibration curve to reflect new estimates of the degree of uncertainty in the Belfast and Seattle laboratories' calibration curve measurements.

1525 BC is clearly within the range of reasonable possibility on these hypotheses, even without considering the potential impact of measurements of the same sample separated by more than one sigma, of uncertainties in the calibration curve, of regional/seasonal variation, or of ^{14}C-deficient carbon, discussed below. Figure 1 on page 566 of the article by Manning et al. in the 28 April 2006 issue of *Science*, reproduced here (Figure 2), presents perhaps a clearer picture, with at least part of each of the Akrotiri VDL dataset sample measurement ranges extending at one sigma below even 3300 ^{14}C age BP, and with central points near 3320 BP, entirely consistent with a calibrated eruption date around 1525 BC.

One should note especially the reference to one-sigma, 68%, probability ranges. Where all the potential sources of uncertainty discussed herein are added to the picture, the reader is perhaps justified in wondering whether the error bars depicted represent much more than a 50/50 bet. It is only a few of the pre-high-precision determinations from the 1970s and 1980s, rejected by Manning in earlier studies, that fail to intersect the 1530 BC portion of the calibration curve at the two-sigma range (Manning et al., page 302).

My thanks are due in particular to Douglas Keenan, Steven Soter, and Peter Warren for helpful comments and rapid responses to my queries.

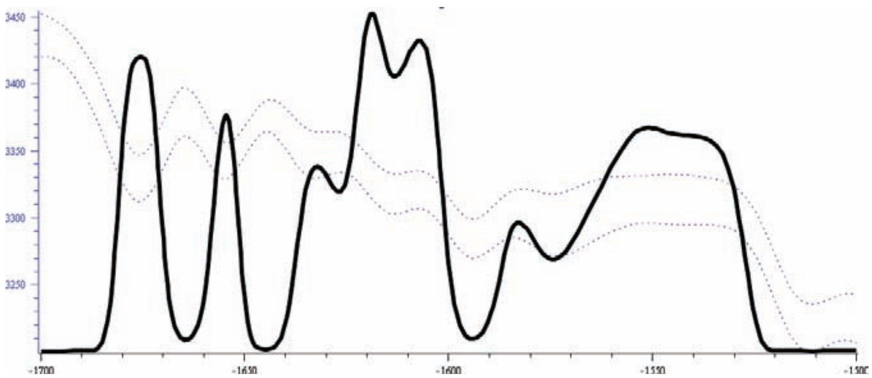

Figure 1: Calibrated radiocarbon determinations of the weighted average of 13 samples from the Akrotiri VDL, offset two per mil = 16.5 ^{14}C years, calibrated against unsmoothed, non-expanded IntCal98 calibration curve (Keenan, pers. comm. of 4 December 2008).

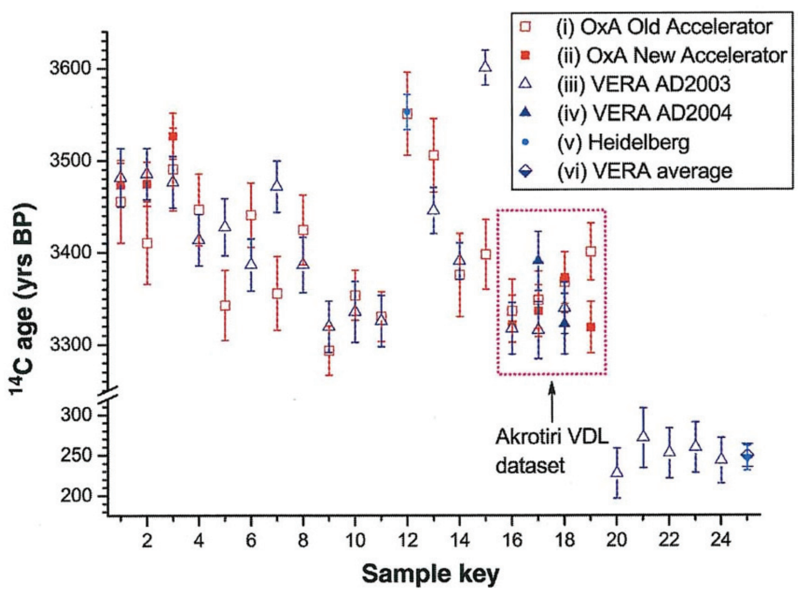

Figure 2: Comparison of ^{14}C age estimates for fractions of identical Aegean samples between (i) Oxford Old Accelerator (samples measured from AD 2000–2002), (ii) Oxford New Accelerator (measured in AD 2003), (iii) VERA (measured in AD 2003), (iv) VERA (measured in 2004), and (v) Heidelberg. The weighted average of the five VERA measurements on a sample of known-age wood are shown (vi) as compared to the Heidelberg measurement of the same sample. Sample key includes the following samples: 1, Trianda AE1024 rings 21 to 30; 2, Trianda AE1024 rings 11 to 20; 3, Trianda AE1024 rings 1 to 10; 4, Akrotiri M4N003 rings 6 to 8; 5, Akrotiri M4N003 rings 3 to 5; 6, Akrotiri M4N003 rings 7 and 8; 7, Akrotiri M4N003 rings 5 and 6; 8, Akrotiri M4N003 rings 3 and 4; 9, Akrotiri 65/N001/I2 ring 3; 10, Akrotiri 65/N001/I2 ring 2; 11, Akrotiri 65/N001/I2 ring 1; 12, Akrotiri M54/2/VII/60/SE>247; 13, Kommos K85A/62D/9:92; 14, Kommos K85A/66B/4:22+23; 15, Kommos K85A/62D/8:83; 16, Akrotiri M31/43 N047; 17, Akrotiri M2/76 N003; 18, Akrotiri M7/68A N004; 19, Akrotiri M10/23A N012; 20 to 24, Çatacık tree rings AD 1640 to 1649; and 25, weighted average VERA Laboratory data (samples 20 to 24) versus Heidelberg measurement of same sample. Samples 20 to 25 also offer a known-age test. All five VERA ^{14}C measurements included the correct calendar age range within their 1σ calibrated ranges (17, 22), as does the VERA weighted average and the high-precision Heidelberg measurement. Error bars indicate 1σ ranges (Manning et al. 2006a: Figure 1).

Here it is appropriate to recall the necessarily irregular nature of the underlying dataset, where decisions or protocols regarding inclusion/exclusion may sometimes produce significantly different results. Of the 13 Theran VDL determinations here at issue, two Oxford measurements of the same sample yielded determinations which did not overlap at the one-sigma range, and the same is true of a seed cluster divided between the Oxford and VERA labs. In each case, the higher (older) reading produced a measurement higher than any of the others in the series. Had a protocol of excluding measurements of the same sample whose one-sigma ranges did not overlap been adopted, the Theran VDL determinations again would appear visually to be consistent with a Theran eruption date of c. 1525 BC. The table of sample measurements provided in Manning et al. (2006b: 39–40) shows that two of the barley samples gave central ^{14}C ages 97 radiocarbon years apart (i.e., OxA-12175 and OxA-1556) and two of the grass pea samples 215 years apart (i.e., OxA-1549 and OxA-1555). Moreover, the one-sigma ranges of the grass pea measurements failed to overlap by 70 ^{14}C years (one sigma for these measurements). Three of these four sample measurements came from the 1989 Oxford study, when preparation protocols and measurement accuracy and precision may have been different. With regard to the post-AD 2000 determinations, two Oxford measurements of one barley sample, M10/23A N012, provided significantly different radiocarbon ages. The removal of humic acid apparently resulted in some shrinkage of an already small sample, to the point where the sample could not be shared with the VERA laboratory in Vienna as originally planned; the small size of the sample may have affected the measurement results. The decision was taken to include both measurements because they fit within the two-sigma range, if only barely (S. Manning, pers. comm. of 29 February 2004, for which I am most grateful). The exclusion of both measurements would lower the average age. The claim in Bronk Ramsey, Manning, and Galimberti (2004: 335) that the seeds in the jars from the Theran VDL "all give a perfectly consistent set of results" defies ready comprehension and requires explanation. To average such data to produce a "coherent weighted average" (Manning et al. 2006b: 6) and then calibrate only the average is statistically adventurous, to say the least. The Ward and Wilson "Case I" statistical method employed to compute the weighted average of these seeds by Manning et al. is explicitly stated in Ward and Wilson (1978: 20–21, 30) as applicable only to measurements known to be of the same date. The application of Case I weighted averaging to this dataset seems questionable. Further sensitivity testing is in order. It would be interesting to know the weighted and unweighted averages of all determinations of ^{14}C ages from seeds from the Theran VDL after all measurements which fail to overlap at one-sigma are removed from the dataset. Of course no amount of measurement and analysis can resolve problems resulting from the potential presence of ^{14}C-deficient carbon in a sample, discussed below.

Decisions as to what constitutes an outlier (sometimes made by laboratory personnel) may result, e.g., in the exclusion of measurements perceived as 250 years too early and the retention of determinations in fact 100 years too early, thereby giving a false sense of uniformity of result. Moreover, while inter-lab comparability of measurements has seen much improvement in recent years, some discrepancies naturally remain. Manning et al. (2006b: 5) report that:

> ... [o]verall, comparing the Oxford (OxA) versus Vienna (VERA) data on the same samples (using the pooled ages for each individual laboratory where they re-measured the same sample—thus n=17), 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. This indicates that there is an unknown error component of 54.5 ^{14}C years.

Subsequent work has reduced inter-lab discrepancies in general, but many of the calibration curve measurements were made prior to the advent of high-precision AMS laboratories.

Conversely, a calibrated date later than 1520 BC could not be encompassed by even an eight per mil change in reported radiocarbon measurements of samples or calibration curve segments (see below) or some combination of the two, given the slope of the calibration curve after that date (Keenan, pers. comm. of 26 November 2008, for which I am most grateful). Whether the calibration curve is in fact so sharply sloped after 1520 BC is open to question, however, as noted below. An eruption date somewhat after c. 1520 BC, arguably preferable on archaeological grounds, would require some factor other than measurement uncertainty affecting Aegean radiocarbon determinations in this period, such as major distortion in the calibration curve at this point, ongoing upwelling of seawater, or the presence of ^{14}C-deficient carbon in samples (see below).

II. The Accuracy and Precision of the Calibration Curve

The imperfect nature of the decadal calibration curve measurements from trees of known dendrochronological date is apparent, with determinations of adjacent

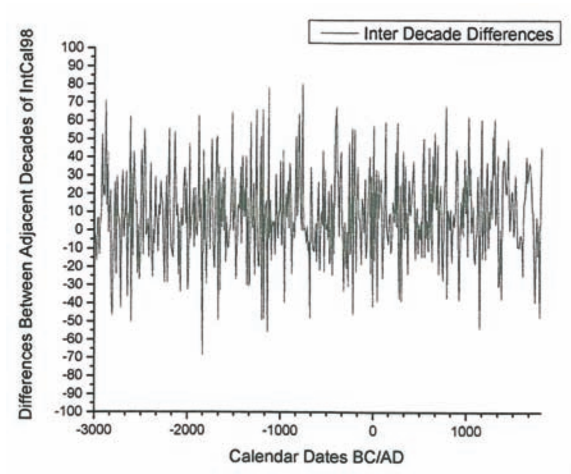

Figure 3: Inter-decade sample ^{14}C age differences (for adjacent decades) in the IntCal98 dataset.

decades regularly producing radiocarbon ages 30 to 70 years apart, as shown in Figure 3. (I am particularly grateful to Sturt Manning for making this unpublished figure available to me, in full knowledge of the use for which it was intended.)

When radiocarbon measurements of ten-year tree segment samples exhibit this amount of noise, notwithstanding the fact that they are far larger in sample size than seeds or seed clusters and are little affected by intra-annual variation in atmospheric radiocarbon (as well as some inter-annual variation, which may average out over a decade), caution concerning the precision of radiocarbon determinations in general is clearly indicated. When the 1998 calibration curve was issued, it was noted that a mean difference of 24.2 ±6 years existed between the Belfast measurements of Irish oaks and the Seattle measurements of German oaks for the critical period between 1700 and 1500 BC (Wiener 2007). Many of the calibration curve measurements still in use were made before the advent of high-precision measurements, and when less was known about best practice in such areas as the pretreatment of samples. Manning et al. Figure 4, reproduced here in Figure 4, is of special interest with respect to the critical decades centered on 1525 BC and 1515 BC. The Belfast laboratory measurements represented by the red circles are significantly above the upper boundary of the one-sigma smoothed 2004 band, and even further outside the unsmoothed IntCal98 band. With regard to the accuracy of the calibration curve in general, we may note as well the very large disparity at 1570 BC of the two measurements from the Seattle laboratory, and also the wayward measurement by the Belfast laboratory shown in Figure 4 at 1500 BC (but more likely the bi-decadal measurement centered on 1510 BC, subsequently identified as an erroneous measurement by the laboratory [Wiener 2003b: 391–392; Reimer, pers. comm. of 7 February 2003]). As an example of how one erroneous measurement may dramatically affect analysis of the date of the LM IB destructions in Crete and hence the date of the close of LM IA including the Theran eruption date, see Manning et al. 2002. In sum, the calibration curve is by no means a perfect measuring rod.

Finally, as my paper in this volume notes, conversion from radiocarbon measurements to calibrated dates provides additional problems. Manning et al. in their discussion of ^{14}C measurements of a three-year tamarisk tree twig preserved in the VDL on Thera which produced a date consistent with the historical chronology observe that the sample may "reflect short-term higher amplitude and/or frequency variation in atmospheric ^{14}C ages not seen in the IntCal04 record for the period, which is both based on 10-year growth samples and smoothed" (2006b: 12). A seed measurement 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 10-year oak tree segment may, for example, over-represent one or two years with wide rings (reflecting rapid growth from good weather) which do not include the year of the life of the seed. In addition, Theran seeds of course grow in a different climate from European oaks.

With respect to potential regional/seasonal effects on radiocarbon measurements, the authors of the comments are of course correct in noting that there are no marked ongoing differences between the calibration curve decadal measurements from trees from Western Europe and those from Gordion in the 17th and earlier 16th centuries BC. Thera in the Aegean, however, does not lie in the same meteorological zone as Gordion in the central plateau of Anatolia which is affected by strong winds from the northeast (Keenan 2002: 237, Figure 1; Reddaway and Bigg 1996: Figure 3). The effect of the intra-year difference between summer high and winter low radiocarbon measurements would tend to raise slightly the ranges of ^{14}C determinations from Aegean seeds in relation to European trees, but this factor alone is unlikely to affect radiocarbon measurements by more than a decade. As we have seen in the context of the question posed, however, even a shift of a decade or less may matter.

It is surely for these reasons, as well as those discussed below, that Manning wrote:

> [i]t is apparent from the parameters and data for the Thera "problem" reviewed in this paper, that a solution may well be unlikely from the volcanic destruction level radiocar-

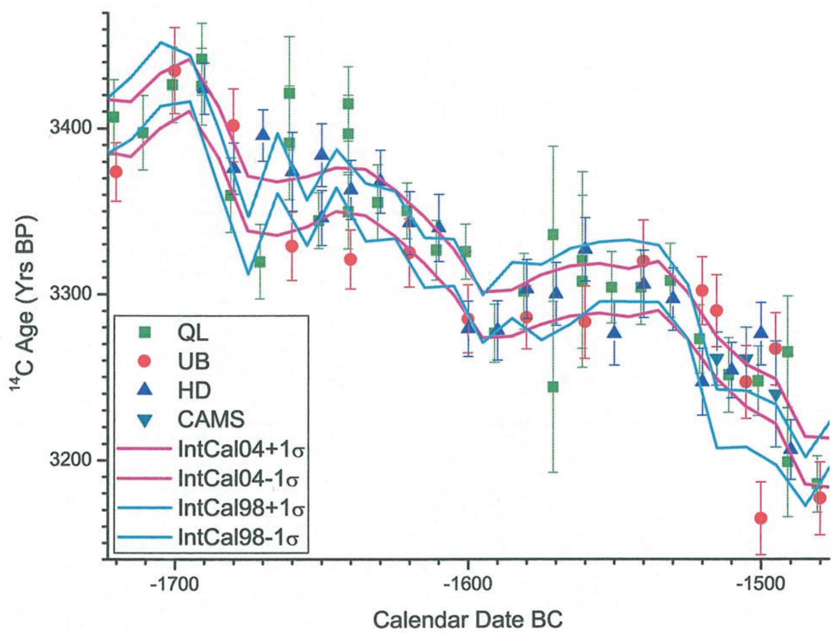

Figure 4: All ^{14}C data employed to develop the IntCal04 radiocarbon calibration curve for the period 1700–1500 BC shown against the IntCal04 and IntCal98 one-sigma envelopes (Manning et al. Figure 4).

> bon data alone. The data at hand either indicate strongly, or, in most cases, tend toward, a 17th century solution. However, it is undeniable that not all do, and that the radiocarbon "gap" between 17th century certainty, and 17th/16th century ambiguity, is all of about 20–30 radiocarbon years. This span is about the same as the best measurement precision available today for Accelerator Mass Spectrometry determinations—the source technology for nearly all the modern Thera radiocarbon ages. Hence one is operating on the limits of precision. And even small laboratory offsets, or variations caused in sample pre-treatment regimes, could become relevant in pushing data into, or out of, the ambiguity threshold. Hence we hit an impasse. And a skeptic is justified to be so (Manning 2005: 111–112).

I believe that the foregoing statement of 2005 remains true with respect to the VDL short-lived samples and that the addition of radiocarbon dates from Miletus, Trianda, and Chania in Crete, discussed below, adds little to the solution. The recent olive tree measurements discussed in Friedrich et al. are considered below.

III. Reservoir Effects of ^{14}C-Deficient Carbon

Finally with respect to the ^{14}C determinations from the VDL at Akrotiri, we turn to the potential for reservoir effects from the presence of ^{14}C-deficient carbon (henceforth 'old carbon') in the earth or water. Each one percent of such carbon in a sample would push mid-second millennium ^{14}C measurements back in time by c. 80 radiocarbon years. The authors of the two comments dismiss the problem by asserting that 1) such effects occur only in close proximity to a particular volcano or source of degassing; 2) such effects would necessarily vary by gross amounts; 3) trees and plants do not acquire/retain CO_2 by their roots; and 4) secure radiocarbon dates from the non-Theran sites of Miletus, Trianda, and Chania, not subject to any Theran volcanic field effects, strongly support a 17th-century BC date for the Theran eruption. None of these propositions finds general support in the radiocarbon evidence or the relevant scientific literature.

As to the first, the papers by Manning et al. and Friedrich et al. again cite the studies by Pasquier-Cardin et al. (1999) from the Azores and that by Shore, Cook, and Dugmore (1995) from Iceland of samples collected from the vicinity of volcanoes in areas of strong winds, plus a study on Thera by Bruns et al. (1980) to argue that volcanic effects are quickly dissipated after short distances. The article by Bruns et al. tells us that contemporary short-lived plant material near the volcanic vent gave ages 1300 and 900 years too early and that the effect dissipated after a distance of 250 meters, but does not state exactly what results were obtained beyond 250 meters. In wide areas of Italy non-volcanic CO_2 emissions have marked effects over many kilometers. Italian soil-gas surveys show "that the shape and spatial distribution of the gas anomalies are related to the structural pattern of the area, whereas the magnitude of gas leakage is controlled by the occurrence of deep gas-bearing traps (hydrocarbon reservoirs) and possibly triggered by seismic activity" (Guerra and Lombardi 2001). Radiocarbon dates now recognized by the appearance in the same contexts of datable Greek pottery as 100–300 years too old have long troubled Italian Bronze and Early Iron Age chronology.

Friedrich et al. also state that "Tauber (1983) has shown by ^{14}C measurements that even in extreme calcareous soil conditions the uptake of CO_2 from soil carbonate is indiscernible (0.12 ±0.3%)" (page 296). It is not, however, calcareous soil itself (which may be relatively insoluble, particularly if very old), but dissolved inorganic carbon (DIC, mainly CO_2 and bicarbonate ions) present in the soil of a degassing area which is relevant. Further, limestone decomposition resulting in the release of old carbon may be stimulated by heat and pressure changes at depth during the decades preceding an earthquake and/or eruption. In this regard, it is worth recalling that there is no way of knowing whether the VDL Theran seeds which form a centerpiece of the debate were collected before the earthquake and precursory phase of the eruption (McCoy and Heiken 2000) which drove away the population or during the brief period when work crews and scavengers returned to attempt repairs and recover belongings before the climactic phases of the eruption. If the latter, then the seeds may have been affected by the release of old carbon from the preliminary stage of the eruption. Moreover, an increase in old carbon emissions may have occurred even prior to the initial stages of the eruption. Finally, with respect to the citation in Friedrich et al. to the paper by Tauber on a group of beech trees in New Zealand, the example of unaffected beech trees growing in one particular edaphic situation does not refute all the research showing that other types of trees or plants growing in other edaphic conditions were affected by the presence of old carbon.

Even more surprising is the following assertion by Friedrich et al. (page 296): "From tree-physiology it is extremely unlikely that there could be any quantitative effect by CO_2 uptake through the roots." This simply ignores the substantial body of literature demonstrating the exact opposite. See in particular Cramer (2002) and Ford et al. (2007) and references therein. A good number of other studies have also shown that plants and trees receive some carbon through their roots as well as through their leaves (Stolwijk and Thimann 1957; Skok, Chorney, and Broecker 1962; Splittstoesser 1966; Arteca, Poovaia, and Smith 1979; Yurgalevitch and Janes 1988; Enoch and Olesen 1993; Cramer and Richards 1999. See also Saleska et al. 2007). I know of no study reporting to the contrary. Ford et al. (2007: 375) put the case succinctly with regard to pine trees: "plants can acquire carbon from sources other than atmospheric carbon dioxide (CO_2), including soil-dissolved inorganic carbon (DIC). Although the net flux of CO_2 is out of the root, soil DIC can be taken up by the root, transported within the plant, and fixed.... " Similar behavior has been proposed for willow and sycamore trees (Teskey and McGuire 2007; Vuorinen and Kaiser 1997). Oliver Rackham has noted that olive trees in particular spread massive roots in a search for water in dry climates (Rackham 1965–1966). Such groundwater, whether from streams or springs, is a potential source of ^{14}C-deficient carbon. N.-A. Mörner and G. Etiope note that in the "Tethyan belt [which includes the Mediterranean region], high CO_2 fluxes are related to important crustal formations of... carbonate rocks [causing a] high level of CO_2 concentration in ground and groundwater" (Mörner and Etiope 2002: 193. See also the work of Saurer et al. 2003 regarding groundwater effects discussed above). They further report that "the Precambrian bedrock includes stromatolites, marble and other carbonate bearing rocks [which]... may give rise to the escape of CO_2" (Mörner and Etiope 2002: 197). V. R. Switsur stated the general problem succinctly 25 years ago: "For reliable radiocarbon dating it is important to recognize when contamination with radioactive carbon from other sources is possible or probable—not only long rootlets from vegetation but also the percolation of carbon-rich ground water, or bicarbonates from the solution of limestone, even when from sources some distance from the site" (1984: 182). Moreover, it is necessary to consider the possibility that the uptake of soil carbon saturates at a fairly low value to protect the health of the tree or plant (unless the tree or plant is overwhelmed by proximity to a volcanic vent).

Friedrich et al. further state that:

> [t]he uptake could only occur as CO_2 in water which is transported via xylem (pipes) through the stem to the leaves where wood-cellulose is produced. Because of out-gassing of CO_2 in the xylem on the transport through stem and branches the contribution of CO_2 from root uptake compared to the CO_2 derived from the

air through the stomata in the leaves is negligible (page 296).

In fact, as described by Cramer (2002), plant roots themselves convert dissolved inorganic carbon (DIC) from the soil to organic molecules by the enzyme phosphoenolpyruvate carboxylase (PEPc). Cramer and Richards (1999) supplied DIC (labeled with ^{14}C as a tracer) to tomato roots and found that organic carbon in the xylem sap derived from the DIC was sufficient to deliver carbon to the shoot at rates equivalent to 1% and 10% of the photosynthetic rate for plants grown with ambient- and enriched-DIC, respectively. One percent retained in the roots from ambient-DIC lacking ^{14}C would result in an unwarranted increase of 80 radiocarbon years in the measurement of the seed or wood sample in question. Soter (forthcoming) further observes that dense crop canopies sometimes suppress ventilation, allowing CO_2 emitted from the soil to be taken up through leaves by photosynthesis.

In general, the Possible Sources of Error (PSE) in radiocarbon dating appear asymmetrical, with a tendency towards older dates.

IV. The Olive Tree Branch

Let us now turn again to the limb or branch of an olive tree found covered in Theran tephra and analyzed by Friedrich et al. This is indeed important new evidence, which the paper by Friedrich et al. regards as decisive.

Whether the branch was dead or alive at the time of the eruption remains an open question; long-dead branches are frequently observable on Aegean olive trees today (Wiener, this volume, page 288). An olive tree is a significant investment over generations. Olive growers today are conscious that removal of a major branch may damage the tree. In the early Late Bronze Age metal saws may have been less readily available to farmsteads (as distinguished from palaces such as Zakros in Crete where two were found) than they are today, which may have contributed to a reluctance to undertake the effort. Peter Warren, visiting Laconia in April 2008 after the devastating fire of 2007, observed new growth coming from parts of olive trees spared from the burning that had caused the death of other limbs and branches in the same trees (pers. comm. of 3 December 2008, for which I am most grateful). He further notes that the phenomenon of dead branches on living trees is not limited to olive trees, but rather is observable as well on trees such as stag's head oaks in the U.K. (Warren, pers. comm. of 30 November 2008).

The Friedrich et al. article also publishes a hypothetical reconstruction of the presumed location of the major volcanic fault lines prior to the great eruption. Of course no one can say with confidence what the topography of Thera was like prior to one of the greatest eruptions in human history; where streams or springs, under or above ground, may have been situated; where terrestrial sources of old carbon may have been located; or where underwater sources of old carbon capable of causing sea-water to boil (Bent 1966: 118) may have existed.

The article by Friedrich et al. lays major stress on the asserted robust nature of the radiocarbon data in response to potential significant alteration in the number of years represented by the ring count, which in olive trees is difficult; moreover, the rings counted may not represent annual markers. The proposed ring count does triple-duty: 1) as an indication of the number of years represented; 2) as a basis for the probabilistic analysis permitting the assertion of narrowly circumscribed error bars of ±13 years; and 3) as the underpinning for the proposed curve-fitting of the successive measurements from four parts of the tree to the IntCal98 calibration curve. Recent work on Thera by Dr. Paolo Cherubini (a senior scientist with the Swiss Federal Institute for Forest, Snow, and Landscape) involved taking specimens of olive trees and examining them by computer tomography. His preliminary conclusion is that even with respect to non-fire- or tephra-blackened specimens it is often difficult to obtain agreement in blind tests as to what constitutes a ring, let alone whether the rings are seasonal or whether some years produce no rings. (I am most grateful to Paolo Cherubini for sharing the preliminary results of his research with me in advance of publication.) Of course if the number of years represented by the Theran VDL olive branch is in major doubt, problems arise, not least with respect to the proposed wiggle-match. It has also been proposed (Manning et al., page 300; Friedrich et al., page 296; 2006a; 2006b) that reservoir effects of old carbon could not be a problem with respect to the olive branch inasmuch as the ^{14}C measurements from the branch produce a downward slope, i.e., they descend in order as they move toward the outermost rings. However, if the intake of old carbon saturates at around 1%, then the 99% of the radiocarbon content absorbed from the atmosphere would still cause the progression to descend in order. For the moment, the Theran olive branch remains that dreaded scientific datum, a singleton, and one providing measurements that are not reproducible. Radiocarbon determinations from more such olive branches could provide a major contribution to the dating controversy. All in all, the radiocarbon determinations obtained by Friedrich et al. from the first VDL olive tree branch constitute significant but certainly not conclusive evidence for the date of the Theran eruption.

V. Radiocarbon Dates from Other Aegean Sites Not Affected by Conditions on Thera

One further major claim is made in connection with the radiocarbon evidence, namely, that whatever special circumstances may exist on Thera, dates unaffected by such factors from Miletus on the Turkish coast, Trianda on Rhodes, and Chania in Crete strongly support an early date for the eruption. Is this claim valid? First, it is good to see in the paper by Manning et al. the straightforward acknowledgment that the radiocarbon dates from a piece of wood with 30 rings from Trianda (Manning et al. page 307) and the wood from a shrine area at Miletus constitute merely "irrelevantly high *termini post quos*," particularly in light of statements in prior articles suggesting the contrary (Manning et al. 2006a: 566). Two determinations from these sites remain. One from Miletus, consistent only with the traditional historical chronology, is dismissed on the grounds that the excavator, informed of the matter, reconsidered the area where the sample was collected and concluded that there was a significant possibility of later intrusion, and that accordingly the sample could not be firmly associated with the LM IA destruction horizon (Bronk Ramsey, Manning, and Galimberti 2004: 328). Certainly such things can happen, but so can what in archaeological parlance are sometimes called "kick-ups," when material from earlier horizons is carried higher—by leveling during rebuilding, or by digging of trenches for walls, or the creation of storage pits—and mistakenly used to date the higher stratum. The second sample consists of a twig found in a LM IA level at Trianda on Rhodes.

Two measurements of the twig were made at Oxford, yielding ^{14}C ages of 3367 ±39 BP and 3344 ±32 BP at one sigma. The bottom of the average of the one-sigma ranges is consistent with the historical chronology, and of course the two-sigma range is clearly so. Moreover, there is no evidence as to how the twig was used or whether it came to its final resting place as part of a tree harvested earlier. A branch from the Uluburun shipwreck is now thought to be significantly earlier in date than the shipwreck, and perhaps to have been used for many years as packing to cushion the ingots aboard the ship (Wiener 2003a).

Finally we come to the radiocarbon dates from Chania in Crete. The data bank here consists in its entirety of four seed samples, each measured twice. We are told that:

> [t]he set of eight data from four samples offers a rather wider spread and does not satisfactorily combine into single weighted average value, indicating either that more than one chronological horizon is included or one or more outlier measurements. Applying a minimum exclusion policy to obtain a satisfactory weighted average, then we find that if just OxA-10320 is excluded then a seven date weighted average is possible (3293 ±14 BP) (Manning et al., page 309).

Both of the highly experienced excavators of Chania, Dr. Maria Vlazaki and Prof. Erik Hallager, state that all the seeds came from the final LM IB destruction horizon at Chania (pers. comms. provided on short notice, for which I am most grateful), which suggests a problem in the radiocarbon measurements. Manning et al. (page 309) conclude:

> [t]reating all eight data as a Phase in OxCal (as was done in Manning et al. 2006a), then an event describing the Chania set spans 1590–1483 BC at 1σ and 1628–1447 BC at 2σ.

Even the one-sigma range utilizing all eight measurements of the four seed clusters does not necessarily contradict an end of LM IA Theran eruption at 1525 BC in accordance with the historical chronology, and the two-sigma range still less so. The same conclusion applies as well to the somewhat later ^{14}C measurements from Myrtos-Pyrgos on the south coast of Central Crete cited by Manning et al.

Omitted from the Manning et al. analysis of Cretan dates are the radiocarbon determinations from the LM IB destruction at Mochlos on the north coast in East Central Crete (Soles 2004: 147) where the four relevant measurements calibrate to 1500–1435 BC at one sigma and 1510–1425 BC at two sigma (subject to all the caveats stated above, plus the additional factor that the calibration curve is steeply sloped at this point, with the result that a small change in measurement may result in a large change in date). The published dates are consistent with the historical chronology, although potentially acceptable to a Long Chronology as well. Radiocarbon dates from the LM II destruction of the Unexplored Mansion at Knossos and from Kommos (Soles 2004: 148; Manning 1999: 220–223) fall into two sets which do not match, reflecting either problems in measurement or separate LM II destructions; indeed, the Unexplored Mansion is believed to have suffered two destructions in LM II. The earlier set permits a fairly wide span of dates because of an oscillation of the calibration curve here, but includes dates in the 1440–1430 BC range historically appropriate to an early LM II destruction. The later set of LM II destruction dates (3090 ±80 BP [Kommos] and 3070 ±70 BP [Knossos] [Soles 2004, 148]) are consistent with the historical chronology, and perhaps a little less comfortable with a final LM IB destruction

c. 1500 BC, but by the end of LM II, both chronologies essentially join, and the difference is inconsequential. Radiocarbon dates obtained from what the excavator believed were LH I contexts at Tsoungiza near Nemea in the Peloponnese are consistent only with the historical chronology. Manning et al. (2006b) say the context of the samples was "reasonably secure" and that the problem is "unexplained." Surely any attempt to alter dramatically Aegean Late Bronze Age chronology and history on the basis of radiocarbon dates should encompass all the relevant radiocarbon data and include a sensitivity analysis describing what effect small changes at the limit of measurement accuracy would have had on the radiocarbon dates proposed.

In sum, there is no probative radiocarbon evidence from the Aegean ex-Thera for a Theran eruption date earlier than the historical evidence would appear to allow.

VI. The Archaeological Evidence

The concluding paragraph of the Friedrich et al. comment states "[i]t would clearly be worth while to re-evaluate in detail the current interpretations of the archaeological evidence" (298). Such discussion is constant and ongoing. Twenty years ago I noted that much of the then-known Minoica in Egypt seemed to suffer from some Pharaonic curse, for when the chronological context of an object was clear, the potsherd or other object was somewhat enigmatic, whereas when the nature of the Minoan or Mycenaean piece was clear, the context was uncertain. Of course today we have many more archaeological interconnections, particularly with regard to Cypriot pottery. Every single such object has now been considered at length by numbers of scholars. For example, the critical and, if correct, practically dispositive identification of stone vessels found in the Shaft Graves of Mycenae as New Kingdom Egyptian by Warren and by Bietak has been questioned by Christine Lilyquist at the Metropolitan Museum of Art in New York, inquiring whether the calcite may be Near Eastern or even Aegean, and the inspiration for the shapes found in forms which predate the New Kingdom (Warren 2006: 305–308; Bietak and Höflmayer 2007: 17; Lilyquist 1996: 134–149; 1997: 225–227; pers. comm. of 4 November 2008). Warren notes, however, that large numbers of apparently similar alabaster calcite vessels are found in Egypt, that the Egyptian Nile Valley calcite quarries are well known, that texts show thousands of such alabaster calcite vessels were exported from Egypt, and that the stone of the vessels found at Mycenae seems visually undistinguishable from that used in the Egyptian examples (Warren 2006 and pers. comm.).

The problem is not that *some* of the proposed interconnections would require earlier dates to fit the radiocarbon-based interpretation of the Theran olive branch and perhaps other data, but that *all* the proposed interconnections would need to move. This would include everything from the Khyan lid at Knossos to the vessels in the Shaft Graves, the Aegean metal vessels and their depiction in Egyptian tombs, the Cypriot pottery sequence in the Near East and Egypt, and the similarities in the sequences, based on scarabs as well as pottery, between Tell el-Dab'a and the sites of Tell el-'Ajjul and Ashkelon in the Near East. It is worth noting in this regard that most recent work on Near Eastern chronology has favored the Low Chronology (Pfälzner 2004) or Ultra-Low Chronology (Gasche et al. 1998) for the entire area.

From the early Hyksos Period beginning around 1650–1640 BC onward until c. 1530 BC, there is a constant stream of MC III imports into Egypt, but no LC IA:2 material (Bietak 2000a; 2000b; pers. comm. of 30 November 2008; Eriksson 2003). The great majority of the pumice from the Tuthmoside New Kingdom strata at Tell el-Dab'a is of Theran origin, whereas all the pumice from the well-explored prior Hyksos levels analyzed to date comes from the earlier eruptions of Kos, Gyali or Nisyros in the Dodecanese (Bichler et al. 2003; Bietak 2004: 214–215). What is the likelihood that all of the apparent archaeological interconnections or contexts are erroneous, in comparison to the degree of uncertainty inherent in radiocarbon dating as described above?

Manning has attempted to contain the impact of the archaeological data by hypothesizing a division of Cyprus, whereby objects from the west of Cyprus did not reach Enkomi, the major seaport site in the southeast of Cyprus, for most of a century, and hence did not move on to Egypt (Manning 1999: 119–129; 2001: 80–84; Manning, Sewell, and Herscher 2002: 100–106; Manning and Bronk Ramsey 2003: 112). The hypothesis strikes most mainstream archaeologists as highly implausible, as indicated for example by the trenchant and detailed critique by Manfred Bietak (2004; see also Wiener 2001 with regard to the Cypriot pottery evidence). I have considered myself whether an eruption date of 1570 BC (where there is perhaps some evidence of an event in the tree-ring and ice-core record) could be defended archaeologically, but found even 1570 BC, let alone any earlier date, very difficult to square with the archaeological evidence. Moreover, given the oscillating nature of the calibration curve in these decades, an eruption date of 1570 BC is not significantly more compatible with the asserted radiocarbon evidence than a date of 1525 BC.

The article by Manning et al. cites works by Philip Betancourt and Robert Merrillees (page 313) as supporting an early date for the eruption. The articles cited are a decade or more out of date, however. In response to my query regarding his current view, Prof. Betancourt kindly provided the following reply for quotation:

> What I said 25 years ago was that I thought the evidence as we knew it then favored the early chronology. I no longer feel that way. I feel today that the strongest evidence favors the traditional chronology, though I remain open-minded to the possibility that new evidence we do not have yet may shift the balance (pers. comm. of 14 November 2008; I would add that I concur in all respects).

I have also corresponded with Robert Merrillees in this regard. Dr. Merrillees began by observing that in proposing initially a date of "c. 1650 BC" for the beginning of LC I, he meant to indicate that the date in his view should be earlier than the date of c. 1600 BC preferred by most Cypriot specialists then and now, and in particular earlier than dates in the 16th century BC preferred by some, but that c. 1630 BC would do just as well as c. 1650 BC from his standpoint. I replied that 1615–1610 BC would do just as well as the conventional "c. 1600 BC" for the beginning of LM I, particularly inasmuch as LM I must have begun before the transmission of LM I motifs to Mycenaean Greece at the beginning of LH I, and that in any event there is no reason why LC I could not have begun earlier than LM I, with which Dr. Merrillees concurred. Merrillees also does not believe that the White Slip I bowl from the Theran destruction is an early example of White Slip I. Thus there is no inconsistency between the Merrillees Cypriot chronology and the Aegean historical chronology.

Manning et al. further argue that:

> White Slip I supposedly cannot be found on Santorini/Thera before c. 1530 BC because that represents its earliest known secure find at Tell el-Dab'a (and hence the Akrotiri volcanic destruction level must postdate this date, as Wiener, this volume argues—but for why this viewpoint is not valid and indeed demonstrably incorrect, see Manning et al. 2002c; 2006a; 2006b; Manning 2007). Would this then make everything work "properly" and find the conventional chronology? No—all that then happens is that the analysis (if one adds a 1530 BC *terminus post quem* before the Akrotiri volcanic destruction level) finds *zero* possible agreement scores and *zero* analytical outcomes for the ^{14}C data sets in the Manning et al. (2006a) analysis (such is the non-compatibility) (Manning et al., page 310; and bibliographical references cited therein).

This observation holds true only if one assumes that a two per mil, 16.5-year difference in measurements of a thin data bank is not possible despite the various sources of uncertainty noted. Even without such an adjustment, the bottom of the two-sigma range of measurements when applied to the unsmoothed IntCal98 curve already overlaps the calibration curve at 1525 BC. (The olive branch radiocarbon measurements are a separate matter, considered above.)

It should be noted in this regard that Bietak would now place the earliest secure appearance of Cypriot WS I pottery in Egypt, and probably the Near East as well, in the Thutmosis III period beginning in 1479 BC, rather than in 1530 BC as in the Manning quotation. Bietak adds that he would be prepared to accept a one-generation, 30-year, delay between the creation of the WS I style in Cyprus and its first appearance abroad, or indeed a 50-year delay, but that a delay of 100–150 years (as required by an eruption date of 1613 BC, for example), when set within the context of a chronological series of Cypriot pottery styles present both in Cyprus and abroad, appears outside the bounds of reason (Bietak 2004: 206; pers. comm. of 30 November 2008, for which I am most grateful).

In any event, sherds from a WS I bowl *were* found below the volcanic eruption tephra on Thera. Both an excellent lithograph made from photos and drawings of the sherds exist independently, and these show that the bowl was used, broken and repaired in antiquity before it met its end in the eruption (Merrillees 2001; Manning 1999). In assessing the evidence for the date of the Theran eruption, both the radiocarbon determinations and the Cypriot White Slip I bowl are clearly relevant.

VII. Conclusion

Certainly if there existed only radiocarbon measurements, a substantial but not conclusive preference for an earlier eruption date would follow, particularly in light of the olive tree measurements presented by Friedrich et al. Similarly, if there existed only the historical evidence, a very strong preference for a later date (indeed, perhaps even later than 1525 BC) would follow. Rather than reject either body of evidence, I have sought to inquire whether, in light of the oscillating nature of the calibration curve in the critical century and the inherently problematic nature of radiocarbon dating, there is a point at which both categories of evidence can possibly meet, such as 1525

BC, and if not, which body of evidence is more likely to incorporate a systemic source of error. Surely this is the only path out of the chronological maze. The historical stakes are high, for the chronological solution will determine 1) whether Crete at the height of its pre-eruption florescence at the close of Late Minoan IA, Thera at the close of Late Cycladic I, and Mycenaean Greece at the close of Late Helladic I were in contact with Egypt at the beginning of the New Kingdom and 2) whether Crete in Late Minoan IB was in close contact with the assertive, expansive, and internationalist Egypt of Thutmosis III.

Submitted January 2009

Friedrich et al. Response to M. H. Wiener

In July 2007 a second olive tree was excavated that was, like the first found tree, buried alive *in situ* by the pumice of the eruption. The stem/branch has a length of 183 cm and a diameter of 12–15 cm. Samples of this olive tree are currently being investigated and tested for radiocarbon dating. Together with the man-made Bronze Age wall (Figures 1 and 2, pages 293 and 294) one gets the impression that the trees were part of an olive grove situated close to a settlement on a terrace of the caldera rim of that time. When comparing the new found olive stem/branch with modern olive trees growing less than one kilometer away from the locality we get the impression that the stem/branch could have 40–50 growth rings. Furthermore the finding of a piece of colored pottery of Late Cycladic IA style by the archeologist Nikos Sigalas connects the olive tree site directly to the destruction level of the Akrotiri excavation. Also the second olive tree gives us the impression that the trees were still alive when they were buried by the pumice of the eruption. We can clearly rule out the possibility that a dead branch was used for the radiocarbon dating. As one can learn from the ruins of the Akrotiri city, which is only a few kilometers away, strong earthquakes and precursory blast(s) hit the city, and also the site where the olive trees grew, prior to the main phases of the eruption. As a result all dead branches would have fallen off and be lying on the ground. Also the second olive tree was still standing upright in live position.

Comment on the paper by Malcolm Wiener: The finding of the olive trees is something quite unique and exceptional. As many others, we consider the radiocarbon date of 1627–1600 BC at present to be the most direct and precise for the Minoan eruption of Santorini. Concerning your criticism: We regard criticism as helpful since it sharpens our argumentation. However, we feel that the criticism should be more balanced and also show the weak points of the archaeological chronology.

Concerning the trees: Both olive trees are at present exhibited in a museum on Santorini and the locality where the olive trees grew is still accessible. However, this might change in near future, since houses are being built above the site. We therefore propose a meeting/workshop on Santorini as soon as possible to discuss all remaining issues there. It would also give other laboratories the opportunity to investigate the material.

Submitted November 2008

Manning et al. Response to M. H. Wiener

Malcolm Wiener's "Reply" suggests that "It would be interesting to know the weighted and unweighted averages of ^{14}C ages from seeds from the Theran VDL after all measurements which fail to overlap at one-sigma are removed from the dataset" (page 319).

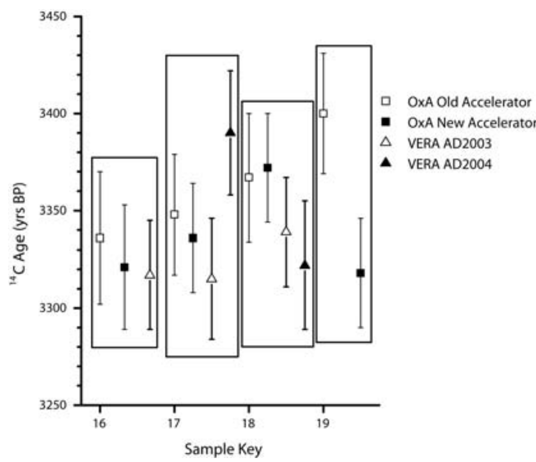

Figure 5: The Oxford (OxA) and Vienna (VERA) radiocarbon dates (in ^{14}C years BP) on short-lived samples from the final volcanic destruction level at Akrotiri (from Manning et al. 2006a: Figure 1). The figure shows in detail the samples indicated by the dotted box in Wiener "Reply" Figure 2. One-sigma error bars are shown. The samples within each box come from the same sample group: as listed in Wiener "Reply" Figure 2 caption, samples 16 to 19. Sample 16 = Akrotiri M31/43 N037; Sample 17 = Akrotiri M2/76 N003; Sample 18 = Akrotiri M7/68A N004; Sample 19 = Akrotiri M10/23A N012.

The Supplemental Figure (Figure 5) here shows the 13 radiocarbon dates (in radiocarbon years BP) on seeds from the final volcanic destruction level run at Oxford and VERA in the AD 2000s period with one-sigma error bars (this is a detailed version of the samples in the dotted box in Wiener "Reply" Figure 2 after Manning et al. 2006a: Figure 1). The samples within each box come from the same sample group. Sample group 19 (Akrotiri M10/23A N012 – OxA-11820, OxA-12175) has two measurements which do not overlap at one-sigma (they do at two-sigma). So let us discount these two data as Wiener proposes. Sample group 17 (Akrotiri M2/76 N003 – OxA-11817, OxA-12170, VERA-2757, VERA-2757r) shows the two Oxford samples overlapping but the two VERA samples do not overlap with each other, although they both overlap with the Oxford samples. We assume Wiener would also like to discount the two VERA samples. Let us do so. The other dates each overlap the other dates from their sample group at one-sigma.

We are left then with 9 dates on short-lived samples which are highly consistent and all from the same chronological horizon (and so applying a weighted average is entirely appropriate). The weighted average is 3340 ± 10 BP. The calibrated calendar ranges (from OxCal) are in Table 1.

IntCal04:	
One-sigma:	1664-1650 BC (17%)
	1642-1612 BC (51.2%)
Two-sigma:	1685-1606 BC (88.6%)
	1573-1558 BC (4.1%)
	1550-1538 BC (2.7%)
IntCal98:	
One-sigma:	1683-1667 BC (16.7%)
	1661-1648 BC (13.7%)
	1640-1604 BC (37.8%)
Two-sigma:	1685-1601 BC (72.8%)
	1584-1529 BC (22.6%)

Table 1: Calibrated calendar ranges for 3340 ± 10 BP.

Again there is no calendar range later than 1600 BC at one-sigma (68.2%) confidence. With IntCal04, there is only 6.8% probability for a date in the 16th century BC within the two-sigma (95.4%) range. With the previous IntCal98 calibration dataset, there is a slightly increased mid-16th century BC range (22.6% of the total 95.4% two-sigma range). In no case is a date c. 1525 BC or later within even the margins of the two-sigma range.

Wiener asks in addition after the non-weighted average. This is also 3340 BP. Clearly if one then chooses to ignore the standard and appropriate practice of reducing the error on an average from a consistent set of estimates on the same event (as above), and instead merely averages the errors of each of the constituent data, then one will get a wider date range—in this case 3340 ± 31 BP. Even so, the case for a 17th century BC date remains more likely as shown in Table 2.

In each case the most likely range is in the 17th century BC (the 58.8% or 51.3% parts of the one-sigma ranges above). A less likely alternative is in the mid-16th century BC. But by using such a larger error one can claim that dates into the 1520s BC are just possible at the extreme of the two-sigma range of c. 1691/89–1527/23BC—however, this is special pleading.

IntCal04:	
One-sigma:	1684-1607 BC (58.8%)
	1570-1560 BC (5.7%)
	1547-1540 BC (3.7%)
Two-sigma:	1729-1719 BC (1.9%)
	1691-1527 BC (93.5%)
IntCal98:	
One-sigma:	1684-1602 BC (51.3%)
	1562-1532 BC (16.9%)
Two-sigma:	1689-1523 BC (95.4%)

Table 2: As Table 1 but for 3340 ± 31 BP.

In conclusion:

(i) The radiocarbon evidence from the Akrotiri volcanic destruction level is very self consistent (see Manning et al. Figure 2; the chi-squared test is one obvious pointer to this). Given these data come from several laboratories and from different forms of analytical equipment and from different pretreatment regimes, this is a robust finding (and one cannot reasonably allege a measurement issue which somehow affected all the laboratories and their different equipment/methods).

(ii) Any explanation seeking to find a way to nonetheless allow for a later chronology would only work with some kind of a very uniform slight offset over the whole region. Even so, this would not make sense with the good correspondence (graphical fit) of the wiggle-match on the Theran olive branch to the standard northern hemisphere atmospheric radiocarbon record (whether as is, stretched or condensed); nor is there any other positive evidence. But this is at least a testable hypothesis, and one could look to e.g. marine sediment records as a test.

Submitted February 2009

M. H. Wiener's Response to the Friedrich et al. and Manning et al. Responses

Manning et al.'s Response (page 327) to the M. H. Wiener Reply (page 317) clarifies the nature of the data on which their conclusion is based. The heavy emphasis given to the average age of the nine radiocarbon determinations from the three remaining seed or seed-cluster samples serves to obscure the disparities in the underlying data set, both within the radiocarbon measurements of a single sample and between samples. Moreover, the statistical method employed assumes that radiocarbon-dating error is distributed randomly around the center of the dates obtained, whereas there is reason to believe that the error is asymmetrical with a bias toward higher determinations. The response again refers to "the standard and appropriate practice of reducing the error on an average from a consistent set of estimates on the same event..." (page 328). This practice is appropriate under the Ward and Wilson averaging criteria employed if it can be shown that the seeds and/or seed clusters from separate jars 1) were collected at the same time; 2) had lived under the same radiocarbon circumstances with respect to carbon reservoirs (volcanic vents, earth-gas emissions, CO_2 concentrations in groundwater or limestone); and 3) had the same exposure to precursor events, such as are known to have occurred in the period preceding the final eruption. In this connection, 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. The uncertainty in such matters is the cause of the disquiet with respect to the statistical method used in both Manning et al. Further Discussion (AD 2006–2007) (page 299) and the Response to Wiener, supra.

McCoy further comments that, in general, he finds it "surprising that the potential influence of magmatic CO_2 on ^{14}C dating is not more appreciated... especially on an active volcano such as Santorini" (pers. comm. of 16 April 2009). On this major issue, Manning et al. in their Further Discussion again refer to the Bruns et al. 1980 publication in *Radiocarbon* to support the proposition that no such effect could have been responsible for some of the small dating anomalies between archaeological/historical and radiocarbon dates. That study reported radiocarbon measurements from three contemporary plant samples. Two of the three, located 5 and 10 meters from an obvious source of volcanic CO_2 lacking ^{14}C, gave ages of 1390 and 1030 years, respectively. The third, located 100m away, provided an anomalous Δ ^{14}C measurement incompatible with the standard correction for the effects of CO_2 release resulting from industrialization (Suess effect) minus the ^{14}C addition resulting from nuclear testing. In any event, the age of the third plant was left blank in the column under "age" in the Bruns et al. study (1980: 535 Table 2). On this single aberrant measurement rests the argument that the radiocarbon dates of samples from Thera will only be affected by volcanic carbon if the sample tested is less than 100m from a major recognizable source. Abundant evidence from various places, such as Italy where large areas of gas emissions throughout the country frequently result in radiocarbon dates 1–300 years too old, indicate just the opposite. Much depends on whether the CO_2 source is a point, a line (fault), or a distributed source. Of course no one can know the topography of Thera prior to the great eruption. Whether because of reservoir effects or for some other reason, the radiocarbon measurements of Theran seeds gave a distance between the central values of two samples of barley found in jars in the same room in the volcanic destruction deposit of 97 ^{14}C years and a distance between two pea samples of 215 ^{14}C years. The claim that this evidence "is very self consistent" (Manning et al. page 328) will puzzle prehistorians unfamiliar with the special statistical vocabulary here employed. The very limited data bank—nine measurements from only three seeds or seed clusters in the latest iteration—may also be noted.

The questions raised concerning the adequacy and accuracy of the measurements are significant in view of the proximity of the Manning et al. average radiocarbon age of 3340 BP ±15 to the top of the one-sigma range of the calibration curve at the archaeologically appropriate data of 1530 BC (particularly given the statement of the IntCal04 Committee that the one-sigma error band required widening and smoothing, with data borrowed from surrounding decades for each decadal determination proposed, because of the imprecision and small number of the radiocarbon determinations available for each decadal segment). Moreover, all radiocarbon probability estimates rest on the implicit but insecure assumptions that the dates obtained are largely unaffected by 1) any reservoir effect, i.e., the effect of ^{14}C-depleted carbon whether from volcanic vents, terrestrial degassing, carbon retained by limestone or groundwater or the upwelling of seawater; and 2) regional and/or seasonal variation, or a combination of the two, notwithstanding the absence of any information regarding the possibility of regional variation in radiocarbon measurements between Aegean plants or trees and inland trees, such as the German oaks of the calibration curve or the Anatolian junipers of Gordion, either in general or at certain periods. A putative solar minima-induced cold period, causing German oaks which grow later in the season than Turkish pine or juniper to absorb less ^{14}C, has been offered as a possible explanation for the large discrepancies in the eighth century BC in radiocarbon dates of tree segments thought to be of the same date.

The foregoing comments apply as well to a new Figure 7 (see page 306) and accompanying paragraph that have been inserted into the Manning et al. Further Discussion, subsequent to my Reply (supra) to that response. The caption states that "over 80% of all probability lies before 1570 BC (13 date set) or 1560 BC (28 date set)." This statement is based on the implicit assumption that the ^{14}C concentration of seeds that grew during springtime on a volcanic island in the Aegean is directly comparable to the ^{14}C concentration of tree rings that grew partly during the summer in a forest in Germany. Again the term "probability" is used within the context of a particular statistical paradigm, whereas the concept of "probability" in general discourse implies that all relevant information, areas of uncertainty, and knowledge insufficiency have been considered.

The chemistry and biology of sky, land, and water is not easy to capture in ^{14}C measurements and statistical probability models. 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.

The Friedrich et al. Response to the M. H. Wiener Reply declares that "We can clearly rule out the possibility that a dead branch was used for radiocarbon dating" (page 327). The statement is based on the assertion that all dead branches on all olive trees in the vicinity in question would have been torn off by the precursor earthquake and blast(s) of hot air which struck Akrotiri 7km away, prior to the final stage of the eruption.

There is no basis for the assumption that every dead branch on every olive tree in the vicinity would necessarily have been torn loose prior to the final stage of the eruption. Dead olive tree branches are not easy to remove. Volcanologist Floyd McCoy states, "I doubt that earthquakes would have stripped leaves from trees or removed dead branches—I cannot recall any examples from historic seismic activity. It is unlikely that hot blasts accompanied the precursor eruption; there is no evidence at Akrotiri or in Theran field deposits for such an occurrence" (pers. comm. of 16 April 2009). In fact, Friedrich et al. state that the branch was "in life position" on page 293 of their Further Discussion. (In this case also, material has been added to the initial article subsequent to my Reply. The new material encompasses the whole of the explanatory texts accompanying Figures 1, 2, and 3, which depict the authors' hypothetical reconstruction of events.) With regard to the radiocarbon analyses of the branch, it should be noted that Figure 1 in Manning et al. Further Discussion is based on the stated assumption that the ring-count calendar spacing of the olive tree branch is accurate to within one year. There is no sound basis for this assumption, for as Cherubini and others have noted, olive trees generally produce irregular rings, sometimes seasonal in nature (Wiener supra).

A second branch or limb of an olive tree was found about two years ago. A photograph distributed at a 2007 conference at the University of Aarhus showed a piece of wood so large that it took four people to carry it, a size seemingly sufficient to permit samples to be sent to several laboratories not involved in the examination and publication of the first branch, in order to obtain independent dendrochronological analysis of olive-wood ring counts as well as independent radiocarbon measurements.

The call by Friedrich et al. for an examination of weak points in the archaeological chronology is certainly in order, the more so since few in the physical sciences have sufficient knowledge of texts and inscriptions from Egypt and the Near East, archaeological interconnections, and Egyptian astronomy to form any judgment as to the degree of confidence warranted. A major, decade-long research project under the aegis of the Austrian Academy, The Synchronization of Civilizations in the Eastern Mediterranean in the Second Millennium BC (SCIEM 2000), has brought together specialists from many countries for this precise purpose, with thousands of pages of analysis published. I personally have two articles in press questioning aspects of the textual/archaeological chronology. Indeed, all should remain open to new discoveries and scientific analyses, refrain from announcing definitive conclusions based on only a part of the data, and follow the evidence wherever it leads.

At the moment, the textual/archaeological chronology seems somewhat more solidly based than the radiometrically based chronology, given the uncertainties noted with regard to radiocarbon dates. Of course new evidence may shift the balance. Time will tell.

Submitted April 2009

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