Old Testament Introduction  
The Bible’s Buried Secrets  
Chapter 16, Jericho

<http://www.pbs.org/wgbh/nova/ancient/bibles-buried-secrets.html>

<https://www.youtube.com/watch?v=qalTJzk4kO0>

***About the Video***

What is for the most part an exact copy of the video script follows. There are a few places where individual speakers could neither be heard nor understood: for this we apologize. Every effort was made to be precise: there were just spots that defeated us. Since this is a quote in its entirety it seemed unnecessary to mark it with quotation marks. The notation for each speaker is tedious enough: Narrator, Reader, etc. If you discover bothersome errors, please reply to this website and point them out. You may verify the script more easily by starting to replay it where the “time” stamps indicate discussion begins. The second of the above links is free from advertising and thus easier to use.

***Overview***

The comment about “well-fortified city-states, each with its own king” is true, but irrelevant to Jericho: it does introduce the general nature of Canaanite government and society. It is true that Canaanites had alliances with Egypt:[[1]](#endnote-1) this is supported by the Table of Nations, as well as by the Amarna letters[[2]](#endnote-2). It is less true that the Canaanites worshiped many Gods: as a whole this may be true; but each individual city-state seemed more focused on a single deistic pair: the local deities.

*The Bible’s Buried Secrets* will develop the idea of Joshua’s so-called blitzkrieg[[3]](#endnote-3) later. In the process of contrasting Joshua with the Nazis, the idea gets greatly exaggerated. There is no blitzkrieg.

Our real complaint in this section is with the way that evidence and statistics are handled. We expect to follow two lines of evidence: provenance and archaeology: the video fails to explore these lines of evidence. Provenance claims that Jericho was razed, and burned; later a failed attempt was made to reclaim the city.[[4]](#endnote-4) From provenance we expect no evidence to remain. The Kenyon opinion, followed by later 14C archaeological and statistical evaluation appears to contain reporting errors. From the archaeological and statistical evidence, we found exactly what we expected to find according to provenance: nothing. We have no reason to accept or reject the claim that Joshua entered the Promised Land in 1364 BC; or that Yahweh caused the fall of Jericho. Questions have been raised, but no proofs are possible: Garstang’s work may not have been all that meticulous; yet, he doubtless found the correct general location. Both Garstang and Kenyon found Jericho’s basement: this only seems to be supported by the 14C evidence.

***Script***

Jericho (time 25:40)

Quote:

N: Following the Exodus the Bible says God finally delivered the Israelites to the Promised Land, Canaan. Archaeology and sources outside the Bible reveal that Canaan consisted of well-fortified city-states, each with its own king, who in turn served Egypt and its pharaoh.[[5]](#endnote-5) The Canaanites, a thriving near-eastern culture for thousands of years worshiped many gods in the form of idols. The Bible describes how a new leader, Joshua takes the Israelites into Canaan in a blitzkrieg[[6]](#endnote-6) military campaign.

R: “So the people shouted and the trumpets were blown. As soon as the people heard the sound of the trumpets, they raised a great shout, and the walls fell down flat.” — Joshua 6:20[[7]](#endnote-7)

N: But what does archaeology say? In the 1930s, British archaeologist John Garstang[[8]](#endnote-8) excavated at Jericho,[[9]](#endnote-9) the first Canaanite city in Joshua’s campaign. Garstang uncovered dramatic evidence of destruction, and declared he had found the very walls that Joshua had brought tumbling down.[[10]](#endnote-10)

Unquote.

***Provenance***

The evidence from provenance is fairly simple. The record claims that the city walls, where many of the residences were expected to be, experienced widespread structural collapse, without any evident physical attack by battering rams, catapults, sappers, or the like: the walls just collapsed, seemingly under their own weight (Joshua 6:20). This could indicate a hasty reconstruction, after previous destruction, using inferior materials[[11]](#endnote-11) and workmanship. After collapse, the Israelites entered the defenseless city, virtually unopposed; and, with the exception of a few individuals (Joshua 6:23, 25), proceeded to eradicate all life in the city (Joshua 6:21). Only precious articles were spared (Joshua 6:24): these, being metal, cannot be radiologically dated, once removed their provenance is also lost.

Finally, the city was burned. What we do not understand from such a fire is how a few seeds, swept into a corner, fallen into a crack; or wood splinters, cracked from breaking timbers, or old wooden implements, instruments, tools, or utensils; all of which had great age; would not give the same radiological date signature whether they were roasted in 1364 BC, or in 1570, 1650, 1623 BC, or at any other intermediate date. We do not understand that the radiological age has any necessary relationship to the date of any fire.

Many years later, a failed attempt was made to reclaim and rebuild the city (1 Kings 16:33-34). Provenance dates Ahab and the reclamation project to circa 871-852 BC, roughly half a century after Joshua. This reclamation project resulted in the deaths of two workmen, after which the project seems to have been abandoned. The first step in any such reclamation project is to remove all debris from the site; this step appears to have been completed: for rebuilding had reached the step of gate erection, the first step of rebuilding. The evidence, if any, would be found in a nearby dump site.

What we anticipate from this provenance, as well as any other destruction done by fortune hunters, scavengers, or erosion caused by weather elements: is that the site was swept clean and all evidence was removed, except for structures suitable for reuse as foundations.

This is comparable to the 911 disaster, where the site is well known; but, all of the evidence has been very carefully removed. If we conducted a trench dig in that area of New York City, we would expect to find nothing. If we did not know the provenance of the 911 monument there, we would not know that it was made from 911 debris; nor would we have any dating method to prove such a fact.

Artifacts disposed in a trash heap, would have lost all connections to provenance, and would not be datable for the most part: metals and rocks cannot be dated, while organic materials would rot quickly due to the heat of refuse. In other words, we don’t expect to find anything at Jericho. What was found?

***Kenyon***

Our date for the fall of Jericho is 1364 BC. This seems to fit perfectly with the claim that the city fell in the Late Bronze Age[[12]](#endnote-12) (1550-1200 BC) and remained unoccupied during the Iron Age.[[13]](#endnote-13) This would include the period from 1425 to1400 BC and afterward for centuries.[[14]](#endnote-14)

However, Kathleen Kenyon (1906-1978)[[15]](#endnote-15) dates the fall of the Bronze Age site to 1550 BC. Later (1995), this date failed to be confirmed as 1573 BC by 14C methods, as scientists examined evidence samples collected by Kenyon, and taken from her archives, after her death.[[16]](#endnote-16) These are the dates with which we shall have to contend. Since these dates are 186-209 years earlier than our date of 1364 BC, we cannot pass over this without comment.[[17]](#endnote-17)

If the Kenyon and the subsequent 14C dates stand, we are left with some ugly choices. a. We may have failed to find Joshua’s Jericho: it is most likely gone, beyond recovery. There is no remaining evidence for Joshua’s entry into the Promised Land. b. There may be a considerable dating error in the Bible: an error of roughly 186-209 years. c. There might not be any verifiable invasion by Joshua, and therefore no verifiable Exodus. d. There may be other possibilities.

Kenyon’s method of narrow trench excavation is instrumental in exposing and dating layering, for which it has proved to be a superior method. Its weakness is that it does not manage sufficiently large areas in an orderly and scientific way. So important datable artifacts may be missed, until much further area excavation can recover them. Broader excavation may provide a slightly different story. We are left with a puzzle, and in the words of Amnon Ben-Tor, “Who else could have done it?” Joshua (or rather Yahweh) is guilty of destroying Jericho, until he (He) is proved innocent.

***Wood***

The work of Bryant G. Wood[[18]](#endnote-18), seems to specifically contest and contradict the 1550 BC date held by Kenyon, and the 1573 BC date produced by 14C methods. Since this contest involves two distinct archaeological locations, data may reasonably survive at one location that does not survive at another location: numerous factors contribute to this sort of evidence loss. Since Wood’s views are generally rejected we will not pursue them farther; instead we shall be compelled to examine the post-Kenyon 14C results directly.[[19]](#endnote-19)

***Bruins, van der Plicht, et al.***

“In 1995 Hendrik J. Bruins and Johannes van der Plicht used a high precision radiocarbon dating test on 18 samples from Jericho, including six samples of carbonized cereal from the burnt stratum. The results of these tests gave the age of the strata as 1562 BC,[[20]](#endnote-20) with a margin of error of 38 years. These results therefore confirm Kenyon's estimate and cast doubt on the biblical story.”[[21]](#endnote-21)

In other words, after correction with the 2004 calibration scale:

“In 1995, Bruins and van der Plicht announced radiocarbon dating of the city destruction to between 1617 and 1530 BC, agreeing with Kenyon.[[22]](#endnote-22)”[[23]](#endnote-23)

And again:

“Bruins, HJ and van der Plicht, J (1995). Tell es-Sultan (Jericho): Radiocarbon results of short-lived cereal and multiyear charcoal samples from the end of the Middle Bronze Age, Radiocarbon Vol. 37, pp. 213-220.[[24]](#endnote-24) A radiocarbon date of 3306 ± 7 BP was obtained for grains probably remaining from the final few years. This corresponds to a date range (2 sigma) of 1617-1530 BC by the 2004 calibration scale.[[25]](#endnote-25)”[[26]](#endnote-26)

At first glance, these reports do not seem at all consistent to us. The first report claims a date of 1562 BC; the second and third reports claim 1573.5 BC. The margin of error is reported to be 38 years in the first instance, and 43.5 years in the second and third instances: however, this conflicts with a σ of ± 7, which seems to us to correspond to a margin of error of 14, and a range of 28.

What can we say? Neither reporters nor reports understand science very well. Because the whole world of technology has its own specialized lingo, Geek speak, few news media people really understand what is being said; consequently, popular scientific reports become confusing, garbled, oversimplified, and sometimes even dead wrong. This is what we are trying to sort out. So, we will seek to justify these numbers from the original data.

As we read the original science report of Bruins and van der Plicht, we discover that each of these popular news reports lied. They took what Bruins and van der Plicht said, ignored all their warnings, seized upon the one possible solution out of many others, that could justify their own biased opinions, and jumped on that false conclusion, turning it into pseudo-fact.

***Data***

This is more than a bit confusing, so let us hunt down and examine the original hard data if we are able. Unfortunately, these links do not always work: so, they can be very frustrating. If the following link fails, try stripping off the http:// or https:// prefix and enter the resulting code directly into your Google or other search engine. Alternative links are provided, should the first link fail to work. Google search for Bruins, HJ and van der Plicht, J (1995) also turned up the data, without a lot of hassle. Sorry, for any inconvenience encountered.

<http://www.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf>

<https://ww.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf>

[www.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf](http://www.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf)

<https://journals.uair.arizona.edu/index.php/radiocarbon/article/view/1666/1670>

<http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.477.2185&rep=rep1&type=pdf>

**14C**

The half-life of 14C is 5,730 ± 40 years.[[27]](#endnote-27) Using the standard of the Margin of Error[[28]](#endnote-28) at a 95% confidence interval, we would take this to be a Standard Error of the Mean (SEM) of 20 years: however, at least one authority assures us that the SEM for 14C is 40 years. We are looking for that statistic which tells us how much uncertainty is involved with the average value. This must be added to the measurement error by vector addition.[[29]](#endnote-29) We will use SEM = 20 for 14C half-life until relevant subject matter experts show us that this is incorrect: our decision is the conservative one: we don’t want to inflate error. We will attempt to incorporate this error from this point on. Other errors may also be involved.

What is important for the reader to see here, is that before we take the first measurement, the half-life curve for 14C decay already has an error for the SEM of at least 20 years.

Depending on the number of measurements taken, the scattering of individual measurements would be much larger than this: for example, if 10,000 measurements were taken, σ for the whole population would be in the neighborhood of 2,000 years, and we would need a range of ± 8,000 years to include 95% of all samples.

So, this ± 40 years is a very dangerous number if common reports do not explicitly define what it means and how it was calculated. Since, we do not have the sample base used for radiocarbon half-life, we don’t know how many samples were taken; so, we begin with a 20-year assumption of SEM for radiocarbon half-life.[[30]](#endnote-30)

**Specimens**

Some seeds and charcoal were found entombed in the structure. How and where these specimens were entombed becomes critical to our reasoned analysis. This information does not appear to be found in the evidence that Kenyon has bequeathed to us.

The migration problem. If specimens were discovered loose, they could have been moved there by a bird, insect, rodent, playing child, or careless adult: this would cast any idea of intimate relationship with the structure in doubt. If specimens were imbedded in the cement of the structure, we could be relatively sure of the intimate relationship.

The contamination problem. Specimens can also be contaminated by surrounding conditions, by air or water movement: this would cast doubt on our ability to take accurate measurements. If specimens were discovered in a sealed jar, preventing any outside contamination, we would be in better shape.

The aging problem. It is a fact of life that seeds can be stored for hundreds, even thousands of years.[[31]](#endnote-31) Specimens may have aged before they become entombed; generally, we assume that the specimen was new, when it was entombed: the seeds grew this year, the charcoal producing fire burned this year. An old specimen can become imbedded in a new wall: giving a false impression of age. The risk of this error depends on how rapidly exposed organic materials decay in the local environment, which is another half-life scenario.

Finally, we are measuring the age of seeds and charcoal, not of the structure itself. This means that our measurement is no better than its intimacy with the structure: for the structure below is at least as old as, or older than, any intimate, uncontaminated specimens found imbedded in it; while the structure above, will be younger: provided that we are not dealing with pre-aged specimens.

It is not at all clear from reports that Kenyon considered or guarded the migration, contamination, or age of the samples taken; that these were preserved in the chain of evidence; or that Bruins and van der Plicht were left in any position to evaluate such issues by the time that the specimens finally arrived in their hands years later.

**Structures**

The higher up a structure, the newer it gets. The top floor may be constructed years after the foundation. However, foundation repairs may be made still later, creating the impression that the foundation is newer than it looks. An older section of the wall could lie inches away from the archaeological trench surface. Moreover, new structures may be erected on old reused foundations, giving the impression, judging by the foundation that the new structure is older than it is; while judging by the new structure that the foundation is younger than it is. This means that any measurements of specimens must be accompanied by a detailed understanding of the nature of the structure, as well as by the exact location of the specimens in that structure. The exact spot of specimen imbedding cannot be older or younger than the specimen: unless the specimen is not new (pre-aged), migrated, or contaminated in some way. Depending on the structural issues discussed, the surrounding structure may be older or younger. The structure itself must be studied in great detail. Careful consideration must be given to any modifications or repairs. This is very important to the correctness of any conclusions drawn from the evidence.

It is not at all clear from reports that Kenyon made any such structural assessment. We do understand that she evaluated age related layering of structure, which is fundamental to trench archaeology. Did she also evaluate factors such as repair or remodeling? Even though the eighteen specimens[[32]](#endnote-32) were given a general location, was sufficient consideration given to establish a date relationship between the specimens and nearby structures? We readily assume such relationships: do they, in fact, exist?

**Goals**

We wish to reduce all the data to averages and Standard Errors of Means (SEM).[[33]](#endnote-33) Even if the data does not behave in a normal pattern, averages and SEMs will behave in a normal pattern. We will calculate or discover several distinct SEM measurements along the way. An SEM tells us how large the uncertainty of an average is thought to be. Any SEM impacting the average measurement must be added by vector addition. We have at least twenty-four averages with which to cope; each one is an error source with its own SEM.

**Measurements**

Modern radiocarbon dating is dependent on accelerator mass spectrometers (AMS). If the measurements were not taken with such a device, small sample sets of eighteen could not be considered reliable: we would reject all conclusions based on eighteen samples as statistical foolishness. The accelerator mass spectrometer not only gives faster results; not only uses less of the specimen for each measurement; but also, many measurements can be taken from the same sample. The longer the specimen remains in the machine, the more measurements are taken, while the more accurate the averaged results. This technique produces an average output of many measurements, not a report of single measurement. The ± statistic reported for each sample is the SEM for that sample; if our understanding of this is incorrect, then this ± statistic has no value: nor have we any way to calculate SEM from such a statistic.[[34]](#endnote-34) Other than that, we would only know that the SEM is equal to, or smaller than the ± statistic. The ± statistic is all we have to work with, so that is what we shall use as the SEM for individual samples.

**Analysis**

Analysis is broken down into three tables: Table 1 is an analysis of the six grain specimens; Table 2 is an analysis of the twelve charcoal specimens; and Table 3 is an analysis of all eighteen specimens together. All primary calculations will come from and be found in these three tables of the data. An average, ± average, and SEMs will be calculated for each chart. Then the half-life, individual sample average, and wiggle SEMs will be combined by vector addition. Next the BP average dates will be converted to BC dates. Finally, the mean wiggle offset will be added to the BC date.[[35]](#endnote-35) Since, both BP to BC conversion, and mean wiggle offset contain mammoth uncertainties, they will be discussed at length and no certain conclusions can be drawn from them: as soon as BP to BC conversion, and mean wiggle offset is attempted, any hope of finding a 95% confidence interval flies out of the window.

Our procedure, while using the Bruins and van der Plicht data, is obviously different than the Bruins and van der Plicht procedure.

* We used ordinary averages; B&P used weighted averages.
* We do not know the specific values of the B&P weighted scale.
* We assumed that the SEM for 14C half-life is 20.
* We assumed that the ± column provides the SEM for each sample. It makes little sense for it to mean anything else.
* We don’t know the SEM for calibration curves; we estimated one that gave reasonable results.

In spite of these defects in our analysis we were very impressed with the B&P results.

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| --- | --- | --- | --- | --- | --- |
| ***Table 1, Grain Samples*** | | | | | |
| ***Bruins, HJ and van der Plicht, J (1995)*** | | | | | |
|  | | | | BP  Base | Mean  Wiggle |
| No. | Lab no. | 14C date  (yr BP) | ± | 1950 | 210 |
| 1 | GrN-18539 | 3312 | 14 |  |  |
| 2 | GrN-18542 | 3288 | 20 |  |  |
| 3 | GrN-18543 | 3331 | 18 |  |  |
| 4 | GrN-18544 | 3312 | 15 |  |  |
| 5 | GrN-19063 | 3240 | 18 |  |  |
| 6 | GrN-19064 | 3375 | 25 |  |  |
|  | | | | | |
|  | | BP | SEM | BC  Date | BC  calibrated |
|  | Average | 3310 | 18 | 1360 | 1570 |
| σ | 45 |  |  |  |
| SEM | 18 | 7 |  |  |
|  | | | | | |
| Mean Error Analysis (SEM) | | | | | |
|  | | | | 1.000 |  |
|  | Half-life | SEM | ± | Wiggle | SOS |
| Value | 20 | 18 | 18 | 18 | 38 |
| Square | 400 | 335 | 336 | 335 | 1407 |
|  | | | | | |
| Calibrated BC Range | | 1495 | | 1645 | |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| ***Table 2, Charcoal Samples*** | | | | | |
| ***Bruins, HJ and van der Plicht, J (1995)*** | | | | | |
|  | | | | BP  Base | Mean  Wiggle |
| No. | Lab no. | 14C date  (yr BP) | ± | 1950 | 210 |
| 7 | GrN-18363 | 3365 | 25 |  |  |
| 8 | GrN-18365 | 3360 | 25 |  |  |
| 9 | GrN-18367 | 3350 | 20 |  |  |
| 10 | GrN-18368 | 3393 | 17 |  |  |
| 11 | GrN-18370 | 3380 | 25 |  |  |
| 12 | GrN-18536 | 3342 | 17 |  |  |
| 13 | GrN-18537 | 3384 | 15 |  |  |
| 14 | GrN-19068 | 3350 | 16 |  |  |
| 15 | GrN-19223 | 3388 | 16 |  |  |
| 16 | GrN-18538 | 3614 | 20 |  |  |
| 17 | GrN-18721 | 3385 | 20 |  |  |
| 18 | GrN-18722 | 3368 | 17 |  |  |
|  | | | | | |
|  | | BP | SEM | BC  Date | BC  calibrated |
|  | Average | 3390 | 19 | 1440 | 1650 |
| σ | 73 |  |  |  |
| SEM | 21 | 6 |  |  |
|  | | | | | |
| Mean Error Analysis (SEM) | | | | | |
|  | | | | 1.000 |  |
|  | Half-life | SEM | ± | Wiggle | SOS |
| Value | 20 | 21 | 19 | 21 | 41 |
| Square | 400 | 438 | 377 | 438 | 1654 |
|  | | | | | |
| Calibrated BC Range | | 1569 | | 1731 | |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| ***Table 3, All Samples*** | | | | | |
| ***Bruins, HJ and van der Plicht, J (1995)*** | | | | | |
|  | | | | BP  Base | Mean  Wiggle |
| No. | Lab no. | 14C date  (yr BP) | ± | 1950 | 210 |
| 1 | GrN-18539 | 3312 | 14 |  |  |
| 2 | GrN-18542 | 3288 | 20 |  |  |
| 3 | GrN-18543 | 3331 | 18 |  |  |
| 4 | GrN-18544 | 3312 | 15 |  |  |
| 5 | GrN-19063 | 3240 | 18 |  |  |
| 6 | GrN-19064 | 3375 | 25 |  |  |
| 7 | GrN-18363 | 3365 | 25 |  |  |
| 8 | GrN-18365 | 3360 | 25 |  |  |
| 9 | GrN-18367 | 3350 | 20 |  |  |
| 10 | GrN-18368 | 3393 | 17 |  |  |
| 11 | GrN-18370 | 3380 | 25 |  |  |
| 12 | GrN-18536 | 3342 | 17 |  |  |
| 13 | GrN-18537 | 3384 | 15 |  |  |
| 14 | GrN-19068 | 3350 | 16 |  |  |
| 15 | GrN-19223 | 3388 | 16 |  |  |
| 16 | GrN-18538 | 3614 | 20 |  |  |
| 17 | GrN-18721 | 3385 | 20 |  |  |
| 18 | GrN-18722 | 3368 | 17 |  |  |
|  | | | | | |
|  | | BP | SEM | BC  Date | BC  calibrated |
|  | Average | 3363 | 19 | 1413 | 1623 |
| σ | 74 |  |  |  |
| SEM | 17 | 4 |  |  |
|  | | | | | |
| Mean Error Analysis (SEM) | | | | | |
|  | | | | 1.000 |  |
|  | Half-life | SEM | ± | Wiggle | SOS |
| Value | 20 | 17 | 19 | 17 | 37 |
| Square | 400 | 306 | 363 | 306 | 1375 |
|  | | | | | |
| Calibrated BC Range | | 1549 | | 1697 | |
|  | | | | | |
| Alternate Mean Error Analysis (SEM) | | | | | |
|  | Half-life | SEM | ± | Wiggle | SOS |
| Value | 20 | 35 | 19 | 0 | 45 |
| Square | 400 | 1225 | 363 | 0 | 1988 |

We were not able to duplicate the Bruins and van der Plicht (1995) results for Tell es-Sultan exactly. This may be due to rounding errors in the data reports, the use of weighted averages instead of ordinary averages, or other misunderstandings concerning the meaning of the data. On the other hand, we got close enough to believe that the data reported, demonstrate a very accurate, precise, and reliable technological improvement over previous methods.

The most prominent exception is our No. 16, GrN-18538, which cannot be adequately explained: unfortunately, such data outliers call to question the reliability of the process as a whole. However, this is not a failure of measurement accuracy; it is a failure of specimen quality, exposed by the measurement accuracy: we will find other such discrepancies among the samples. No. 16 illustrates the problem of finding pre-aged material specimens: the outcome might have been much, much worse. Yet, we too easily fail to notice that the Table 1 results, differ from the Table 2 results by a full eighty years; this is an enormous difference: especially, when we’re attempting to establish the credibility of a 210-year mean wiggle offset: this is 38% of the problem. Nevertheless, we are not justified in removing No. 16 from the data set. Still, in all, we were very comfortable with the resulting BP averages of 3310, 3390, and 3363 BP.

On the other hand, if the full weight of the No.16 error is taken at face value, we have an error of 182 years or more; which, when compared to our estimate of mean wiggle offset of 210 years; approaches it within 28 years. This puts the age estimate right back to 1468 BC. Without any wiggle offset, this would return an age of 1258 BC, close to the time of Merneptah. Calibration corrections simply do not compensate for this or any other kind of specimen error.

The population, taken as a whole, is bi-modal; it represents two independent processes, that were assumed to be the same or at least similar. The two SEM curves fail to overlap at the 95% confidence interval. The specimen sets are not relevant to each other. If seed measurement is an accurate indicator of archaeological age; then, in this case, charcoal is not; the reverse is also true: the two sets are that far apart in their mean values. They do not represent the same evaluation of time.[[36]](#endnote-36)

Popular reports of the Bruins and van der Plicht results, do the very thing that B&P warn against: they jump to unwarranted conclusions about prematurely calibrated BC dates.

**Base**

To find the correct base year, we must know when the samples were taken; because, the advent of extensive nuclear testing has changed the ratios of nuclear particles in the atmosphere. Samples taken before the years of nuclear testing use a different base than samples taken after the years of nuclear testing. Our base year is 1950; so, we arrive at a BC date by subtracting 1950 from any BP date. This calculation is not commonly thought to add any error; however, it will turn out that 1950 is not a stable constant number: correcting the errors of this instability will prove to be a problem that has not yet been solved. Nevertheless, we will plunge ahead, as we must, as though 1950 were a constant, so all the errors stay the same: only the average and the ranges will be effected. An attempt will be made to address the 1950 errors at the calibration step; which is to say that the calibration step should include all 1950 base errors and compensate for them.

We are less comfortable with the 1950 BP Base calculation: it assumes a uniformity in the atmospheric concentrations related to 14C, which we now know does not exist. This is unfortunate; but, there is little to do about it: the conversion from BP to BC dates will be made by subtracting 1950 from the BP date. We calculated BC averages of 1360, 1440, and 1413 BC.

The assumption of these averages is that all eighteen samples are related: they were found close together or otherwise thought to measure the same thing (you can’t average apples and apricots). Since, we believe the date for Joshua’s crossing is most likely 1364 BC, these dates are delusively and seductively encouraging: just too good to be true.

**Wiggle Average**

Because of the risks involved with radiocarbon dating, it is now standard practice to apply a correction factor to the measured data, a calibration. This correction factor for dry land evaluations is taken from the measurement of tree rings; under water data employs a different method. To be valid, the tree ring data would best be taken from trees within the same climatological cycle system as the structure being analyzed. Bruins and van der Plicht specifically state in their abstract and elsewhere that such local calibration data was not available at the time their measurements were taken.[[37]](#endnote-37)

Finally, we looked at the various calibration curves available to us. We noted that Bruins and van der Plicht considered these calibration curves in far greater depth that we will be able to follow: they did not consider such calibration to be sufficiently accurate to draw precision dating conclusions from the data set. Any such conclusions would be premature.[[38]](#endnote-38)

We found the appropriate graph for INTCAL 04 on page 1057 of the following report. It is this graph that best fits the range of data, which data is found in the 3300-3400 area of the graph (neglecting the SEM errors for the moment).

<https://journals.uair.arizona.edu/index.php/radiocarbon/article/viewFile/4167/3592>

If we draw horizontal lines at 3300 and 3400 BR we see that there is no single point on the curve that crosses either of our lines: for 3400, the intersection takes place at least at two places; for 3300, the intersection coincides with a whole range of values. It is a basic principle of mathematics that multiple solutions to the same equation equal garbage.

Nevertheless, we measured each of these intersections or ranges with a ruler and dropped vertical lines to the bottom axis. We found differences between the vertical axis and the horizontal axis ranging between 150 and 270 years or more and less (mean wiggle offsets). So, instead of finding a single solution to mean wiggle for each of our three BP date averages, we have 120 (270 – 150), or more possibilities from which to choose.

If we look more closely at the graph we see hundreds of little gray lines; this sort of graph is used to display a range of data: very useful if we’re looking at ten or twenty data sets. Here, we may be looking at thousands, possibly millions of data sets, all crammed on top of each other, so that none can be distinguished from its neighbor.[[39]](#endnote-39)

Graphic presentation supposedly simplifies understanding and application, which may have been true in the slide-rule age. To handle this information, we need a super-computer and complex fuzzy logic algorithms to wade through all this data and find the best answer: if pigs could fly.[[40]](#endnote-40) The answer is staring us in the face. There is presently, no reliable way to predict the age offset, or calibrate the BP data known to man.[[41]](#endnote-41)

We get the same sort of results from the INTCAL 13 graph; except the results are off the page; so, we have to estimate by extrapolation to get a result. Here we found mean wiggle offsets around 200-210 years. Now you know why they are called wiggles. It makes no difference; no single number between 150-270 years (or even more or less can be selected with any real degree of certainty). We chose 210 years because we thought it was a conservative estimate. You may pick your own estimate and do the math over.

<https://en.wikipedia.org/wiki/Calibration_of_radiocarbon_dates#/media/File:Radiocarbon_calibration_error_and_measurement_error.png>

<https://upload.wikimedia.org/wikipedia/commons/1/1a/Radiocarbon_calibration_error_and_measurement_error.png>

<http://www.stat.cmu.edu/bayesworkshop/2007/BlackwellBuckCS9.pdf>

<https://journals.uair.arizona.edu/index.php/radiocarbon/article/viewFile/4002/3427>

**Wiggle SEM**

We still have SEM errors to consider. The laws of vector addition require that the wiggle error can only increase the overall accumulated SOS error. Because of the nature of the INTCAL 04 or INTCAL 13 graphs the wiggle calibration error is directly related to the SEM values for the three chart averages we calculated: 18 for grain, 21 for charcoal, and 17 overall.

What we need to know next is the slope of the INTCAL graph in the neighborhood of our average. It is impossible to detect a slope for INTCAL 04 at 3300 or 3400 BP; on average, the slope appears to be 0 degrees; but, the data wiggles around so much that it could be much larger. In between these two points the slope seems to approach 45 degrees. It is statistically impossible that the slope be greater than 45 degrees: for this would imply that the calibration is capable of removing error (SEM). It is statistically impossible that the slope be zero: for this would imply an infinite uncertainty. It is also statistically impossible that the slope be negative, as it is in many places on the calibration graphs: for this would imply that time was moving backward.[[42]](#endnote-42) On the INTCAL 13 graph, we measured the slope as approximately 1:2, roughly 30 degrees. We did two things with this information. For each of the three charts, we hypothesized that the SEM for the mean was the same size as the SEM for averages. We added all the errors by vector addition and arrived at SOS estimates of: 38, 41, and 37: the average BC calibrated date ± 2 times these numbers calculates the Calibrated BC Range for each chart.[[43]](#endnote-43)

For chart 3 we made an Alternate Mean Error Analysis (SEM). We set the wiggle SEM to zero, and did exactly what the INTCAL 13 graph suggests: we doubled the size (slope of 1:2) of the SEM for averages, which was a little less than 17.5 to get an SEM replacement number. This resulted in an accumulated SOS of 45 years, instead of the 37 years we previously calculated. These numbers and calculations are presented to help you play with the concepts. We are not suggesting that we have found the absolute final firm conclusion.

**Mathē´mata**

AMS. We are very pleased with the precision results attained by using accelerator mass spectrometers (AMS). We could not find in the report that this was the actual type of instrument used by Bruins and van der Plicht. We assumed this from the description of the test method.

Whatever instrument was used we are still pleased with the precision results attained; although, this could use further scrutiny.

Weighted Averaging. We took issue with weighted averaging when the weighting scale was not published; even so, the difference with our ordinary average calculation for grains was only 4 years; charcoal was 20 years: we are not overly concerned with these differences.

Error Statistic. Of slightly greater concern to us is the meaning of the ± statistic in the second column of B&P report Table 1. We hoped that these numbers are or reflect the value of SEM for each sample; this does not seem to be the case. To be on the conservative side, we used the full value as if it were SEM.

Uncertainty. We verified the reported ± figures for grain averages (± 7), and (± 6) for charcoal averages, elsewhere termed uncertainty, by dividing the average of the ± sample numbers by √6 for grains and √12 for charcoal. Such statistics report the precision, but not the accuracy of the measuring instrument. They do not report any precision or uncertainty associated with the samples: in fact, they expose it.

Specimens. We are not nearly as pleased with the specimen set. Ordinarily in statistics, we would expect a minimum of one-hundred samples to establish a baseline average; one-thousand samples would be better if we could get them. The archaeologist intrudes upon this environment of expectation with a handful of samples, collected from God knows where, and expects statistically valid results. AMS, in part, satisfies this junction by rapidly taking many small samples from one specimen: thus, generating many measurements (more than one-hundred, we hope).

Seed Aging. The precision of the measurements, made differences between test samples seem glaring. The range of grain values was 135 years. We expect that if six grains were dropped from the same crop year, given the claimed instrument accuracy that this range would be around 28 years, or less. The actual reported range of 135 years, seems to indicate that the seeds originated from a spread of 107 crop years. This destroys the initial assumption that the seeds were entombed from the recent crop year. So, the specimen set seems to be corrupted somehow.

Wood Aging. A similar approach cannot be applied to charcoal. Seeds are expected to grow completely in a single crop season, less than a year. Trees, as everyone knows, have the oldest wood at the center of the tree, the layer under the bark was formed during this growth season, so the age of the tree is calculated by counting the rings seen in a cross section of the trunk. We expect to find 70-year-old samples mixed with new samples. In this case the range of values is 272 years, subtracting 4 \* 6, we anticipate a spread of 248 crop years, not especially surprising for a tree.

Outlier. However, the outlier charcoal specimen is 224 years off the overall charcoal average, and 237.5 years off the average of its family samples. This seems to indicate that the bush or tree was at least 237/238 years old. Once again, the initial assumption of specimen “newness” is sorely tested.[[44]](#endnote-44)

Sampling Methods. The problem is not with the measurement technique; but rather with the selection of samples. When these ranges of 135 and 272 years are considered against our estimated mean wiggle offset of 210 years, the whole procedure appears to be futile.

Verification Tests for Seeds. We suggest, if not already accomplished, that a large sample of seeds be collected, from as wide a known age spread as possible. Several varieties should be chosen. “Placebo” seeds should be included in the set. The whole set should be randomized and blind, so that only the test designer knows the actual age of each seed. Then each seed would be AMS tested to determine the AMS ability to identify known ages correctly. To improve the robustness of such tests, they should be set in a full ANOVA design to increase the robustness of the outcome. Other variables, besides the seeds themselves could be two different temperatures, and two different humidities, which are easily varied in a laboratory. Two different pressures could be another variable; but would require a sizable pressure vessel. Any variations due to gravity could be incorporated by running replications at two different altitudes.[[45]](#endnote-45)

Verification Tests for Wood. To perform a similar confirmation test with wood requires that samples be isolated from cambium layers, and stored for several years, or that a method of isolating known age ring samples be used to prepare known growth year specimens.

Confirmation. If such tests confirmed the age of seeds and charcoal within a year or two, we would be very sure that there is a problem with the Jericho specimens. With or without such confirmation of the appropriateness of AMS dating of seeds or charcoal, the wide range of values casts considerable suspicion on the value of the Jericho seed and charcoal samples, as well as the means of their collection and preservation.

Validity. Another observation that derives from these observations of statistical ranges is that only the youngest specimen has validity, all of the older specimens show evidence of pre-aging, as much as 107 to 237/238 years of it, or more. From this line of logic, we would be prone to view the evidence as highly skewed: hence, we would estimate an average date from the eighteen samples of only 3277-3282 BP years (1327-1332 BC). Yet, now were back on top of the LXX date for Joshua (1364 BC). This predicts an age differing from the reported weighted averages of 24-29 years.

Calibration. We consider, once again, the problem with fuzzy data. We appreciate the hard work of presenting such data in detail. However, our goal is not simply the study of data itself: we want to predict archaeological age from it. The art of statistical prediction insists that we can only predict the motion of the herd, we cannot predict the motion of any single animal. The calibration graphs, by presenting so much data, tend to force prediction toward the behavior of individual animals, which is a worthless exercise for predicting archaeological age. What we need is a reduction to a single average calibration for each year and the slope of the curve (SEM) at that year, nothing more or less.

From the examination of the graphs with widest spans: at a first order of approximation this data is nearly a straight line taking the form y = mx + b; or rather from the way it is graphed x = y/m, since b is necessarily 0; where 1/m is always less than 1, and greater than 0; so, x always returns a larger value than y. At a second order of approximation this data may be slightly parabolic, or exponential. Handling wiggles, may require somewhat more finesse. You catch my drift here; we need to know where the herd is going; we do not need the life story of every tree. Two items are necessary: ordinary average, and SEM. SEM is also the slope of the defining curve, the first derivative of the defining curve. Can we please strive for greater simplicity of presentation?

**Findings**

* Kenyon’s dates are reportedly derived from links with Egyptian history and dates; such dates are notoriously unreliable. It is amazing, then, that her opinion should coincide so well with the very unstable wiggle data.
* As Bruins and van der Plicht report, there needs to be a tree ring study done in the Levant, preferably in the Jordan rift: “if only the logs could be found.”
* Kenyon’s 1550 BC date does fall within the range from Table 1 (1495-1645); does not fall within the range from Table 2 (1576-1724); barely makes it inside the range from Table 3 (1549-1697). Again, these corrections are premature. We can get an entirely new set of ranges: just by changing the mean wiggle offset (210 years); or by changing, especially widening (increasing) the error calculations. There is plenty of reason for doing either or both of these things: since, we strove to be conservative in our approach at all times. As an outcome, because these calibrations are premature, we cannot say that the experimental results either confirm or reject the Kenyon 1550 BC date. To read Kenyon’s work into our modern statistical ideas, we would conclude that Kenyon applied a mean wiggle offset of 137 years (1550 – 1413), which from our understanding of the INTCAL 04 and INTCAL 13 graphs, appears to be very conservative.
* We cannot say that the, as reported, 1573 BC date is either confirmed or rejected for all the same reasons: the application of mean wiggle offset and wiggle error is simply too arbitrary at this point in time.
* It’s a pretty safe bet that the application of some sort of mean wiggle offset and wiggle error will eventually be necessary. It seems unlikely, given the present conditions, that either the MT’s 1406 BC date, or the LXX’s 1364 BC date, will ever fall within the 95% confidence limits and thus be confirmed. That being said, reducing the mean wiggle offset from 210 to 150, would reduce all these ranges by 60 years: 1435-1585, 1509-1671, 1489-1637 for each table. The two biblical dates are still not confirmed.
* The wide divergences in the application of wiggle indicate that perfection of 14C dating, its statistical implications, and understanding still have a long way to go.[[46]](#endnote-46) The accelerator mass spectrometers (AMS) are nearly perfect; but, wiggle technology has a long way to go to catch up.
* What we usually call Joshua’s Jericho is most certainly at or very near the Tell es-Sultan location. This is the reason that Garstang and Kenyon both dug there. We’ve got the right spot. We just have no proof that Joshua was there, or knowledge of what sort of structures existed at that time.[[47]](#endnote-47) What we do see was certainly there as underlying foundation, if ever, and whenever we can substantiate the idea that Joshua was there.
* We have discovered a high probability that Jericho was under frequent attack and threat of destruction. Judging by its location it was probably a key check and control point for Egyptian immigration and taxation. Rebuilding may have been hastily done on reused old foundations, which could be an important factor in the structure’s sudden catastrophic collapse as reported in the Bible.
* A 1364 BC destruction under Joshua may have been so complete that any evidence of rebuilding circa 1570, 1650, or 1623 BC could have been completely obliterated, which opens up the possibility that both destructions could be historically accurate.
* The 1364 BC destruction under Joshua may have been removed by erosion or other factors.
* Stronger 1364 BC evidence could be located a few yards away.
* The Jericho evidence does not show that Joshua and the Israelites were present in 1364 BC; neither does it confirm their absence.[[48]](#endnote-48)
* Other errors could be significant and could bring 1346 BC within the confidence interval.
* Rocks cannot be dated. Any debris removed from the structure could not be associated with Joshua, even though he was there. Any such debris would have to be located in such a way that the wall could be puzzled back together.

***Conclusion***

The comment about “well-fortified city-states, each with its own king” is true, but fundamentally irrelevant to Jericho: it does introduce the general nature of Canaanite government and society. It is true that the Canaanites had alliances with Egypt: this is supported by the Table of Nations, as well as by the Amarna letters[[49]](#endnote-49). It is less true that the Canaanites worshiped many Gods: as a whole this may be true; but each individual city-state seemed more focused on a single deistic pair.

*The Bible’s Buried Secrets* will develop the idea of Joshua’s so-called blitzkrieg later. In the process of contrasting Joshua with the Nazis, the idea gets greatly exaggerated. There was no blitzkrieg.

Our real complaint in this section is with the way that evidence and statistics are handled. We followed two lines of evidence: provenance and archaeology: the video did not explore these lines of evidence. Provenance claims that Jericho was razed, and burned; later a failed attempt was made to rebuild the city.[[50]](#endnote-50) From provenance we expect no evidence to remain. The Kenyon and subsequent 14C archaeological and statistical information appears to contain reporting errors. From the archaeological and statistical evidence, we found exactly what we expected to find according to provenance: nothing. We have no reason to accept or reject the claim that Joshua entered the Promised Land in 1364 BC; that Yahweh caused the fall of Jericho. Garstang’s work may not have been all that meticulous; yet, he doubtless found the correct general location; but, the 1364 BC city seems to be gone. We conclude that there is at present no certain archaeological or statistical reason to accept or reject: either Garstang’s, or Kenyon’s hypothesis; or the biblical claim that Joshua entered the Promised Land in 1364 BC; or that Yahweh caused the fall of Jericho at that time. Many questions have been raised, but no proofs are possible.

[[51]](#endnote-51)

1. This fact seems to contradict any idea that the Israelites were not in Cisjordan as a distinct entity: for if the Israelites were nothing more than transitioned Canaanites we would expect them to be found among such alliances, and as writers of the Amarna letters, rather than being cast as consistently adversarial during this period. [↑](#endnote-ref-1)
2. It is intellectually dishonest to neglect the Amarna evidence all along, then suddenly insert a conclusion drawn from it. If we remove the Amarna evidence, then there is little evidence to suggests Egyptian-Canaanite alliances. If we remove the Table of Nations, there is no evidence of any such alliance at all. [↑](#endnote-ref-2)
3. Flash or lightening war [↑](#endnote-ref-3)
4. Joshua 6; 1 Kings 16:34 [↑](#endnote-ref-4)
5. One of the best sources of such information is the Amarna letters. Why were they not explored? [↑](#endnote-ref-5)
6. Neither Yahweh nor Joshua conducted a blitzkrieg military campaign in either Transjordan or Cisjordan. In the vast majority of cases Joshua was first attacked by his adversaries, not the other way around. The overall Divine plan seems to be to offer surrender and peace through repentance to each town: this would mean that they give up their local idols. In the vast majority of cases, the rival Canaanite tribes refuse to yield, and live at peace among the Israelites. Instead they chose war; when they were defeated at war, they gave their remaining energies to subverting Israelite morality, which seems to have worked surprisingly well, according to all accounts. There seems to have been ample room in Canaan for peaceful coexistence: so, it is doubtful that any form of armed takeover was ever in view. There is no blitzkrieg. A mythical summary of the Old Testament, “The Bible Walkthrough”, may have perpetuated such a violence model. The Canaanites started the fights; Joshua just finished them. Neither is any such picture painted in Judges: rather, a series of oppressions had to be overthrown. The synchrony of these oppressions may indicate that Egypt still had its toe in the water. [↑](#endnote-ref-6)
7. The evidence from this passage seems to show that the whole city had a chance to surrender; one woman did surrender with her family. The others chose to reject repentance, and welcomed the judgment of God. The city is not reported to have fallen to military attack, but to Divine condemnation. God was simply fed up with Canaanite behavior, giving them one last chance to change their tune. It is not as though this was the first incidence of Divine judgment in the area: for example, Genesis Chapters 14 and 19. [↑](#endnote-ref-7)
8. John Garstang (1876-1956), a British archaeologist. Works: various Roman sites (1897-1914), Ashkelon (1920), Tell es-Sultan (1930, Garstang’s Jericho). Garstang earned himself a reputation for less than meticulous work. In part, this is what subjects his dates to correction by Kenyon. Had the Garstang work been conducted in a more orderly manner, the evidence from his work may not have escaped us.

   <http://en.wikipedia.org/wiki/John_Garstang> [↑](#endnote-ref-8)
9. We are not even sure that Garstang’s Jericho is Joshua’s Jericho. No one disputes the idea that Garstang found the correct general location: every contributor digs at this site because there is general consensus that this is the right spot.

   “Archaeologists have unearthed the remains of more than 20 successive settlements in Jericho.”

   We will focus on the Bronze Age and Iron Age settlements.

   <http://en.wikipedia.org/wiki/Jericho>

   “Jericho … was destroyed in the Late Bronze, after which it no longer served as an urban [center].”

   <https://en.wikipedia.org/wiki/Jericho#Bronze_Age>

   “Tel es-Sultan remained unoccupied from the end of the 15th to the 10th-9th centuries BCE, when the city was rebuilt.”

   <https://en.wikipedia.org/wiki/Jericho#Bronze_Age_2>

   <https://en.wikipedia.org/wiki/Jericho#Iron_Age>

   These were all adduced by gainsayers to prove that there is no Exodus; that the roots of the Israelites are found in the indigenous Canaanite population. Any such conclusion is hastily drawn. We found what we expected to find at Jericho: nothing. The claim that Israelites are indigenous Canaanites is internally self-contradictory: they cannot be involved in wars, some of which would have been with themselves. So, we cannot reasonably put the words blitzkrieg and indigenous in the same sentence. [↑](#endnote-ref-9)
10. *The Bible’s Buried Secrets* reveals a conspicuous absence of intellectual honesty in limiting this discussion to Garstang. [↑](#endnote-ref-10)
11. Such as using sun-dried brick, rather than stone. [↑](#endnote-ref-11)
12. For those insistent on a Middle Bronze Age (MBA) date, note that Kenyon’s date falls right on the MBA/LBA division line. The term MBA, versus LBA inserts bias, in that it suggests centuries of difference; when, in fact, Kenyon’s opinion is a matter of a few years at most. Since we are attempting to work with great precision, introduction of heavily biased terms such as MBA or LBA is unwarranted: we need to work in years to be at all consistent. [↑](#endnote-ref-12)
13. <https://en.wikipedia.org/wiki/Bronze_Age#Age_sub-divisions>

    <https://en.wikipedia.org/wiki/Bronze_Age#Levant>

    <https://en.wikipedia.org/wiki/Bronze_Age> [↑](#endnote-ref-13)
14. <https://en.wikipedia.org/wiki/Jericho#Bronze_Age>

    <https://en.wikipedia.org/wiki/Jericho#Iron_Age> [↑](#endnote-ref-14)
15. Dame Kathleen Mary Kenyon (1906-1978), a British archaeologist. Works: Zimbabwe (1929), Verulamium (1930-1935), Samaria (1931-1934), Jewry Wall (1934), Southwark, The Wrekin, Shropshire, elsewhere in Britain, Sabratha, West Bank (1951), Jericho (1952-1958), Jerusalem (1961-1967). In terms of the volume of work, she may be the most impressive biblical archaeologist since Sir Flinders Petrie. That being said, as the article points out, her methods are not without weak points.

    <https://en.wikipedia.org/wiki/Kathleen_Kenyon> [↑](#endnote-ref-15)
16. Careful reading of the science report shows that the scientists themselves gave caveat, after caveat, after caveat, stating clearly and explicitly that their measurements could not be used to date any of the specimens in other than BP terms. The uncertainties concerning specimens, BC conversion, and calibration were far too great to allow anything more than premature, hasty generalizations. Popular reporting jumped on these wild guesses, in spite of the many caveats, and turned them into pseudo-facts. We will show, in agreement with Bruins and van der Plicht, that no such conclusion is possible. [↑](#endnote-ref-16)
17. <https://en.wikipedia.org/wiki/Jericho#Bronze_Age_2> [↑](#endnote-ref-17)
18. Bryant G. Wood (biographical dates were not found), an American mechanical engineer and archaeologist. Works: pottery dating, Kirbet el-Maqatir.

    <https://en.wikipedia.org/wiki/Bryant_G._Wood> [↑](#endnote-ref-18)
19. <https://challenge2biblicalexamination.wordpress.com/2011/08/13/canaanite-conquest-debate-second-exchange/> [↑](#endnote-ref-19)
20. This is a false statement! [↑](#endnote-ref-20)
21. This is a grossly inflated and unscientific claim. They seem to confirm Kenyon’s date. They do not cast doubt on the biblical story: they are an argument from silence. They do cast doubt on Garstang’s claim that he found Joshua’s Jericho: if he had such evidence, he lost it. The quote is found under, “The Walls of Jericho”, at:

    <https://en.wikipedia.org/wiki/Biblical_archaeology#Brief_summary_of_important_archaeological_sites_and_findings>

    <https://en.wikipedia.org/wiki/Biblical_archaeology> [↑](#endnote-ref-21)
22. This is a false statement! [↑](#endnote-ref-22)
23. 1573.5 ± 43.5 years. This source was inadvertently misplaced and can no longer be confirmed. [↑](#endnote-ref-23)
24. <http://www.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf>

    Link to publication in University of Groningen/UMCG research database, Citation for published version (APA). [↑](#endnote-ref-24)
25. This is a false statement! [↑](#endnote-ref-25)
26. This source was also inadvertently misplaced and cannot be found. We will correct this defect from new sources, and attempt to analyze the raw data from scratch. 14C methods are improved every year; better calibration curves and methods are developed; the data has been subjected to additional analysis and peer review. We are a long way from the last word in this technology.

    <https://www.researchgate.net/profile/Hendrik_Bruins/publication/30494975_Radiocarbon_Dating_in_Near-Eastern_Contexts_Confusion_and_Quality_Control/links/09e4150bb760d378c4000000/Radiocarbon-Dating-in-Near-Eastern-Contexts-Confusion-and-Quality-Control.pdf>

    <https://www.radiocarbon.com/archaeology.htm> [↑](#endnote-ref-26)
27. <https://www.illustrativemathematics.org/content-standards/tasks/782> [↑](#endnote-ref-27)
28. <https://en.wikipedia.org/wiki/Margin_of_error> [↑](#endnote-ref-28)
29. You can do this graphically. Lay out one error on the horizontal. Lay out the other error on the vertical, at perfect right angles to the first error. Measure the distance across the tips (the hypotenuse) of the triangle that is formed.

    Or you can do this on a calculator or computer spreadsheet. To calculate the square root of the sum of the squares (√SOS). Take the square of each error; add all the squares of the individual errors; take the square root of the sum of the squares of the individual errors. [↑](#endnote-ref-29)
30. <https://en.wikipedia.org/wiki/Radiocarbon_dating>

    <http://radiocarbon.ldeo.columbia.edu/research/radiocarbon.htm> [↑](#endnote-ref-30)
31. Who saves seeds? In an agrarian society, everybody saves seeds. Seeds are saved for tomorrow’s bread. More importantly, seeds are saved for the planting of next year’s crop. The wise farmer keeps as many years of backup as possible, which is how one averts famine in an agrarian society. In the case of exquisite exotic plants or special cultivars, seeds may be kept as long as they are viable, indefinitely. In an agrarian society, seed saving is a matter of life and death. [↑](#endnote-ref-31)
32. There were initially sixteen specimens; the largest specimen was divided into three parts to produce the other two specimens. [↑](#endnote-ref-32)
33. We calculated the ordinary arithmetic mean. Bruins and van der Plicht calculated a weighted arithmetic mean. This explains some minor differences. Since the weight mechanism is not explained, and does not seem intuitive to us, we stayed with the ordinary arithmetic mean. Even though Bruins and van der Plicht cited, “the formula by Mook and Waterbolk (1985)”, we were unable to locate that source. Such a weight process could be as simple as doubling the data for the seeds, making it appear as though there were twelve specimens each for both seeds and charcoal. Or, since three specimens were made from one, the weight of these three could be multiplied by one third to return an overall combined weight of one for the divided sample. Since, we don’t know, it is futile to guess. This turns out not to matter anyway. The disturbing sources of error will be found in the application of mean wiggle offset and wiggle error, or in specimen error, all of which are magnitudes greater than anything encountered with weighted averaging. On the other hand, we oppose the general use of weighted averaging on the grounds that it risks introducing additional bias and error. We prefer to deal with the evidence as found.

    <https://en.wikipedia.org/wiki/Weighted_arithmetic_mean>

    <https://en.wikipedia.org/wiki/Simpson%27s_paradox> [↑](#endnote-ref-33)
34. In the calculation of any average, as the number of specimens increases, the value of σ also tends to increase, becoming less precise. However, the SEM tends to decrease, becoming more precise. The reason for this is that SEM = σ / √n; so, the larger the n, the more that σ is divided. We note in our chart stages that the SEM for charcoal is slightly smaller than our SEM for seeds; when we consider that, for seeds SEM = σ / √6; while, for charcoal SEM = σ / √12: we understand why this is true. [↑](#endnote-ref-34)
35. Since the BP to BC conversion and the mean wiggle offset are simple additions, it makes no mathematical difference what sequence these operations are performed in. BP to BC conversion simply subtracts 1950 from the BP date: this yields the raw BC date uncorrected by calibration. We believe it is helpful to see this raw date and understand how we are adjusting it. The mean wiggle offset or calibration simply adds to the BC date increasing it. We get the mean wiggle offset from INTCAL 04, INTCAL 13; or other, somewhat arbitrarily selected, graphs. These graphs are hard to read and we don’t have the original data sets to sharpen them. The resulting mean wiggle offset could be any number from around 150 to 270, or even larger or smaller. [↑](#endnote-ref-35)
36. This is a roundabout way of saying that somebody lied, or was careless with the way that, and with the location where, specimens were collected. [↑](#endnote-ref-36)
37. <http://www.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf> [↑](#endnote-ref-37)
38. We agree with this point wholeheartedly. We have made some date calculations based on these tests. However, as far as the application of wiggle is concerned, these date calculations are little better than guesses. [↑](#endnote-ref-38)
39. This sort of statistical disruption has a cause. We have digressed from an assumption of linearity to a cure that is worse than the disease: we can only describe such a graph pattern as noise or static. If this were an analog electronics problem, we would find a way to filter out the noise. The problem with noise is that the true signal or sound cannot be heard above the roar of the noise.

    The first such disturbance that comes to mind is the Minoan eruption, also called the Thera eruption, or Santorini eruption (1642-1540 BC). The question arises, could Santorini have caused such a disruption in the atmospheric 14C chemical balance? We know that nuclear testing and solar flares cause such disruption. What happened circa 3300-3400 BP to cause such disruption? How can we improve upon the tree ring solution?

    <https://en.wikipedia.org/wiki/Minoan_eruption> [↑](#endnote-ref-39)
40. Modern curve fitting analysis seeks the least mean squared fit for complex data sets. Such curve fitting methods are vastly superior to the graphic methods used in the slide-rule era. Such curve fits can be linear regression analyses, or they can be more complex curves with varying degrees of freedom: so, the curve fit can be as simple or complex as the technician desires.

    What we are suggesting here is that with powerful enough curve fitting techniques the noise can be “filtered out” and a single curve should result. If the single curve does not provide two different answers to the same question: then, the intercept gives a single estimated result for the mean wiggle calibration; the tangent slope, the value of the first derivative (dy/dx), at that intercept point provides a rise/run ratio that can be applied immediately to the SEM, as we did in Table 3, Alternate Mean Error Analysis (SEM). Hopefully, smaller errors would be observed. [↑](#endnote-ref-40)
41. The statistical joke is: measure it with a micrometer (a very precision instrument), mark it with a crayon (a blunt instrument), and cut it with an ax (a rudely inaccurate instrument). Actually, this is an insult to nineteenth century axmen who needed to make smooth boards and tight joints using nothing more than an ax: alas, such skill is gone. You get the point: we have gone from measurements of 0.001 inches, to marking half an inch, to making three to six inch errors on the workpiece. This is exactly the same problem we face here; and, we haven’t yet considered the SEM error. [↑](#endnote-ref-41)
42. Note that INTCAL 04 is drawn graphically reversed to INTCAL 13; meaning that, the implications about positive and negative slopes must be reversed.

    The existence of such anomalies on the calibration graphs, drives us to ask, what perturbations caused them? To what influences are the trees responding? Understanding such causes and influences may help us understand why Joshua “observed” a long day, or why Hezekiah’s clock ran backwards (2 Kings 20:8-11), such instances, humanly speaking, may be written off as poetic language, or deceptions of the eye and mind. There is no reason to believe that trees can be so deceived, or that trees appreciate human poetry. So, what root causes so affect both the trees and the men? [↑](#endnote-ref-42)
43. Which, again, is not a serious number. The wiggle data is insufficiently clear to predict a date. All such predictions are guesses: they are premature. [↑](#endnote-ref-43)
44. We hope you see that such a pre-aging error obliterates any hope of finding a meaningful mean wiggle offset. The size of these numbers eats our estimated 210 year mean wiggle offset. This puts the credibility of the 1364 BC date right back on the table. [↑](#endnote-ref-44)
45. Thus, evaluating any effect due to the so-called space-time continuum. This would seemingly obviate any need for a pressure variable. [↑](#endnote-ref-45)
46. <https://www.rug.nl/research/portal/files/6807335/1995RadiocarbonBruins.pdf>

    <https://www.rug.nl/research/portal/publications/pub(6ea47cb4-6dc2-4d13-9b29-1ce09bb7a757).html>

    <http://www.rehov.org/Rehov/publications/Chapter14%20Quality%20Control%20Tel%20Rehov%20-vdPlicht%20&%20Bruins.pdf> [↑](#endnote-ref-46)
47. Duh, the city was destroyed, duh…. [↑](#endnote-ref-47)
48. <https://en.wikipedia.org/wiki/Battle_of_Jericho> [↑](#endnote-ref-48)
49. It is intellectually dishonest to neglect the Amarna evidence all along, then suddenly insert a conclusion drawn from it. If we remove the Amarna evidence, then there is little evidence to suggests Egyptian-Canaanite alliances. If we remove the Table of Nations, there is no evidence of any such alliance at all. [↑](#endnote-ref-49)
50. Joshua 6; 1 Kings 16:34 [↑](#endnote-ref-50)
51. If you have been blessed or helped by any of these meditations, please repost, share, or use any of them as you wish. No rights are reserved. They are designed and intended for your free participation. They were freely received, and are freely given. No other permission is required for their use. [↑](#endnote-ref-51)